THE

Psychological Review

EDITED BY

J. MARK BALDWIN, JOHNS HOPKINS UNIVERSITY AND HOWARD C. WARREN, PRINCETON UNIVERSITY

CHARLES H. JUDD, Yale University (*Editor of the Monograph Series*).

WITH THE CO-OPERATION FOR THIS SECTION OF

A. C. ARMSTRONG, WESLEYAN UNIVERSITY; ALFRED BINET, ÉCOLE DES HAUTES-ÉTUDES, PARIS; W. L. BRYAN, INDIANA UNIVERSITY; WILLIAM CALDWELL, McGILL UNIVERSITY; MARY W. CALKINS, WELLESLEY COLLEGE; JOHN DEWEY, COLUMBIA UNIVERSITY; J. R. ANGELL, UNIVERSITY OF CHICAGO; C. LADD FRANKLIN, BALTIMORE; H. N. GARDINER, SMITH COLLEGE; G. H. HOWISON, UNIVERSITY OF CALIFORNIA; P. JANET, COLLÈGE DE FRANCE; JOSEPH JASTROW, UNIVERSITY OF WISCONSIN; ADOLF MEYER, N. Y. PATHOL. INSTITUTE; C. LLOYD MORGAN, UNIVERSITY COLLEGE, BRISTOL; HUGO MÜNSTERBERG, HARVARD UNIVERSITY; E. A. PACE, CATHOLIC UNIVERSITY, WASHINGTON; G. T. W. PATRICK, UNIVERSITY OF IOWA; CARL STUMPF, UNIVERSITY, BERLIN; R. W. WENLEY, UNIVERSITY OF MICHIGAN.

Volume XI., 1904.

THE MACMILLAN COMPANY,

41 NORTH QUEEN ST., LANCASTER, PA.

66 FIFTH AVENUE, NEW YORK

AGENT: G. E. STECHERT, LONDON (2 Star Yard, Carey St., W. C.); LEIPZIG (Hospital St., 10); PARIS (76 rue de Rennes).

PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

CONTENTS OF VOLUME XI.

The Psychological Review,

MONOGRAPH SERIES.

The following *Monographs* have already appeared:

Vol. I.

1. * **On Sensations from Pressure and Impact**: HAROLD GRIFFING. Pp. 88.
2. **Association**: MARY WHITON CALKINS. Pp. vii+56.
3. * **Mental Development of a Child**: KATHLEEN MOORE. Pp. iv+150.
4. **A Study of Kant's Psychology**: EDWARD FRANKLIN BUCHNER. Pp. viii+208.

Vol. II.

5. **Problems in the Psychology of Reading**: J. O. QUANTZ. Pp. iv+51.
6. **The Fluctuation of Attention**: JOHN PERHAM HYLAN. Pp. ii+78.
7. * **Mental Imagery**: WILFRID LAY. Pp. ii+59.
8. **Animal Intelligence**: EDWARD L. THORNDIKE. Pp. ii+109.
9. **The Emotion of Joy**: GEORGE VAN NESS DEARBORN. Pp. ii+70.
10. **Conduct and the Weather**: EDWIN G. DEXTER. Pp. viii+105.

Vol. III.

11. **On Inhibition**: B. B. BREESE. Pp. iv+65.
12. **On After-images**: SHEPHERD IVORY FRANZ. Pp. iv+61.
13. * **The Accuracy of Voluntary Movement**: R. S. WOODWORTH. Pp. vi+114.
14. **A Study of Lapses**: H. HEATH BAWDEN. Pp. iv+122.
15. **The Mental Life of the Monkeys**: E. L. THORNDIKE. Pp. iv+57.
16. **The Correlation of Mental and Physical Tests**: C. WISSLER. Pp. iv+62.

Vol. IV.

17. **Harvard Psychological Studies, Vol. I.**; containing sixteen experimental investigations from the Harvard Psychological Laboratory: Edited by HUGO MÜNSTERBERG. Pp. viii + 654. $4.00.

Vol. V.

18. **Sociality and Sympathy**: J. W. L. JONES. Pp. iv+91. 75 cents.
19. **The Practice Curve**: J. H. BAIR. Pp. 70. 75 cents.
20. **The Psychology of Expectation**: CLARA M. HITCHCOCK. Pp. iv + 78. 75 cents.
21. **Motor, Visual and Applied Rhythms**: J. B. MINER. Pp. iv + 106. $1.00.
22. **The Perception of Number**: J. F. MESSENGER. Pp. iv + 44. 50 cents.
23. **A Study of Memory for Connected Trains of Thought**: E. N. HENDERSON. Pp. iv + 94. 75 cents.

Vol. VI. (To contain about 500 pages.)

24. **A Study in Reaction Time and Movement**: T. V. MOORE. Pp. iv+86. 75 cents.
25. **The Individual and his Relation to Society**: J. H. TUFTS. Pp. iv+58. 50 cents.
26. **Time and Reality**: J. E. BOODIN. Pp. v+119. $1.00.

* Monographs so marked are not sold separately. Vols. I–III are $7.50 each.

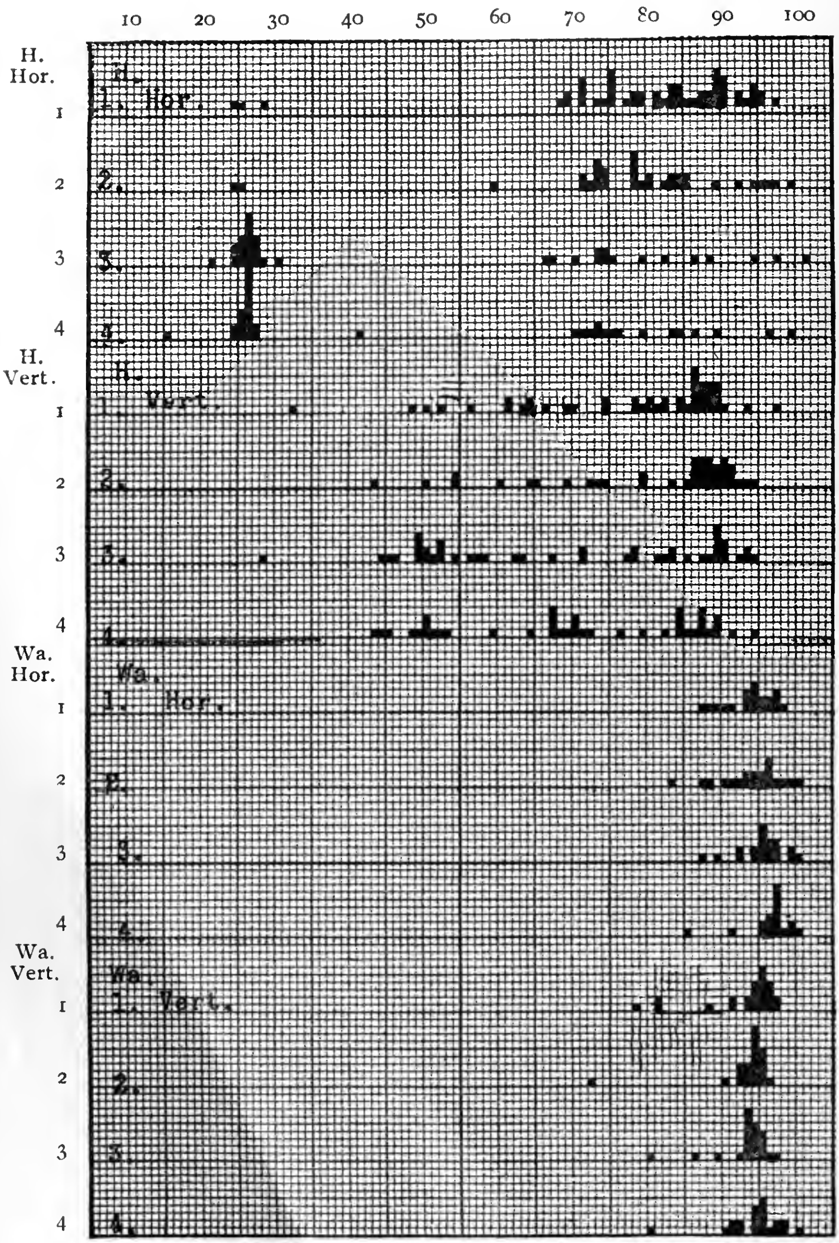


CHART 2.

NOTE. The uppermost row of figures should be *on* the base line, 1. Hor.

N. S. Vol. XI. No. 1 January, 1904

The Psychological Review.

THE PARTICIPATION OF THE EYE MOVEMENTS IN THE VISUAL PERCEPTION OF MOTION.[1]

BY PROFESSOR RAYMOND DODGE,
Wesleyan University.

Psychological tradition is practically unanimous in distinguishing two fundamentally different conditions under which the perception of motion may occur. The one presupposes a relatively motionless eye, and offers sensory data composed exclusively of the displacement and modification of the retinal image. The other condition is characterized by certain forms of eye movement which are supposed to furnish perceptual data quite independent of all modification of the retinal image.

Concerning the exact form of the sensory data which operate under the former circumstances, there is less unanimity. For the purposes of our discussion it is sufficient to recall four main varieties. The most important of them all is generally held to be the simple displacement of the retinal image, when the consequent successive excitation of different local signs is supposed to condition an immediate perception both of the direction and of the extent of motion. This main angular displacement of the retinal image must be distinguished from a secondary angular displacement which was especially emphasized by Hoppe. The latter consists of irregular movements of the retinal image following the accidental variations which occur in the direction or velocity of most perceivable forms of objective motion. This might seem to be merely a subgroup of the first

[1] Read in part before the New York Branch of the American Psychological Association in session with the Philosophical Club of Yale University.

variety, but it depends for its peculiar force, not directly on the successive stimulation of different local signs, but rather on certain temporal peculiarities of that succession, which experience has created one of the surest criteria of objective motion. A third variety was especially emphasized by Exner who paradoxically called it the 'visual sensation of motion,' and who regarded it as entirely independent of all spatial synthesis. It consists of a peculiar sensory datum, produced by every change of retinal stimulation and immediately apprehended as movement, even when it cannot be referred to any specific object or direction. Notwithstanding the psychological anomaly involved in this group, I believe it is destined to play a rôle of considerable importance in the theory of the visual perception of motion. The fourth variety also involves a consciousness of change, but it is characterized by a more or less definite comparison of the spatial relations within a given field of view with the immediately preceding spatial relations in the same field.

Obviously, any movement of the eye in pursuit of a moving object must more or less distort all four varieties of purely sensory data. The main angular displacement of the retinal image will be practically annihilated, while Exner's 'sensation of motion,' and even the comparison of successive conditions within the total field, would seem to be ambiguous unless supplemented and corrected by some factor concurrent with the eye movements. This factor in the visual perception of motion has been variously characterized as a feeling of innervation, sensations from the orbital muscles, either of activity or strain, and finally sensations of motion arising from contact between the sclerotic and various parts of the orbit, notably, in the most recent theory, between the sclerotic and the eyelid.

Extreme emphasis was given to this motor factor in Stricker's monument to the vagaries of pure introspection, while almost all recent discussions of the visual perception of motion hold it in one form or another to be a datum of fundamental importance. That its importance has been overestimated has been indicated by Fleischl, Aubert, James and Wundt, and most emphatically announced by Stern.

Recent experimental study of the eye movements has discovered a new and serious ground of suspicion against the traditional importance of the motor data by exposing the poverty and inaccuracy of our direct apprehension of the eye movements. The continuous involuntary eye movements, when the eye seems to the subject to maintain a constant fixation; the fixation pauses which interrupt every natural sweep of the eye across a complex field of view; the discreet corrective movements at the end of every considerable eye sweep; in fact, most of the known characteristics of the eye movements yield no introspective data at all, or only such as are ambiguous or absurdly inadequate. It scarcely seems probable that eye movements which we cannot even count, of whose amplitude we have no immediate subjective clue, of whose very existence we are often not aware even under the most rigid self-observation, could be very important factors in the perception of minute spatial changes. Not only, however, is there no independent consciousness of the eye movements, adequate to the refinement of the visual perception of motion, but the character of the eye movements which occur when we view a moving object furnishes evidence that, if our consciousness of them were complete and exact, it would be either useless or misleading as a datum in the visual perception of motion.

Characteristics of the Pursuit Movements of the Eye.

Photographic registration has disclosed two distinct types of eye movements, which are directly involved in the pursuit of a moving object. The one is the primary, reactive displacement of the line of regard towards an eccentric point of interest. The other is the true pursuit movement. The most important characteristics of movements of the first type are the relative constancy of their duration, under similar conditions of fatigue, of original orientation, and of the direction and angle of eye movement; and secondly, the fact that, under ordinary circumstances of illumination and complexity of the field of view, they are never moments of new effective retinal stimulation. The true pursuit movements of the eye, by which the constant fixation of a moving point of regard is maintained, differ in every respect from eye movements of the first type. Their chief characteristics may be summed up as follows: (1) The velocity of the pursuit movements has no fixed value, but varies with the apparent velocity of the object of regard as it moves across the field of view. (2) Unlike movements of the first type, the pursuit movements are moments of clear vision. Indeed, they are the essential condition for the clear perception of a moving object of regard. (3) While movements of the first type are fundamentally reactions to specific eccentric stimuli, pursuit movements sometimes assume the character of habitual movements, and may persist after the occasion for them has ceased. (4) Finally, whereas movements of the first type are always separated by relatively long intervals of rest, movements of the second type are separated chiefly if not entirely by movements of the first type.

The true pursuit movement, however, neither begins nor continues through any considerable angle of displacement, uncomplicated by movements of the first type. In the first place the line of regard naturally wanders over a moving object just as it does over a motionless object, but all displacements of the line of regard in response to a change in the point of interest are rapid movements of the first type. Moreover, the first phase of pursuit is never a pure pursuit movement. Photographic records show a well-marked group of irregular movements of the first type, separated by moments of complete rest, before there is any trace of the true pursuit movement. Finally, even after the pursuit movement proper has begun, it is always interrupted, more or less frequently, by eye movements of the first type. These interruptions occur when there is no conscious change of the point of regard within the moving object, and even in spite of the best endeavors of the subject to maintain the constant fixation of a single point. Every photograph that we have obtained of the pursuit movements shows these minute but characteristic irregularities, though they are less frequent in successive pursuit sweeps of the same rhythm than they are in the first sweep of a series; and even in any given sweep of considerable amplitude there is a marked tendency for the interruptions to decrease towards the end of the sweep. It appears that the true pursuit movement consistently lags behind the object of regard, and that the fixation is automatically renewed from time to time by the slight corrective movements of the first type.

A comparison of these two types of eye movements discloses a functional difference between the fovea and the periphery of the retina, with respect to the motor response to moving stimuli, that has hitherto been entirely neglected. The simple reactive displacements of the line of regard are apparently identical in general character, whether the eccentric object of interest is at rest or in motion. Under both conditions they function to bring the point of regard to a new object of interest. The only noticeable difference between the two cases is found not in the resulting eye movements, but in the stimuli which bring about the reaction. There is a certain liveliness in the moving stimulus which serves both to detach it from its background, even when the color differences would otherwise be imperceptible, and to compel our attention in a notable manner. The basis of this peculiar effectiveness of the moving stimulus would be an interesting problem, but it lies outside the field of our present discussion. The fact of especial significance seems to me to be that notwithstanding the real and apparent difference in the character of the stimuli, the immediate motor response is the same in both cases. One does not need a clearer indication of the relative unimportance of the motor reaction in the visual perception of motion. The initiation of the true pursuit movements, on the other hand, seems to depend in some way on foveal stimulation; since try as one may, they never begin until the initial phase of the pursuit has successively brought the retinal image of the moving object to the fovea.

This functional difference between the fovea and the periphery at once assumes considerable theoretical importance when we remember that not only is the relative efficiency of the periphery much higher in the case of moving than in the case of stationary stimuli, but that, in the overwhelming majority of cases, the initiative in the perception of motion must be taken by the periphery, even when the relatively constant fixation of the moving object is afterwards subserved by the true pursuit movements. Furthermore, the primary reaction of the eye to peripheral stimulation, if it ever furnished a factor in the general perception of motion, would be an utterly misleading measure of the objective motion, since it varies, not according to the apparent angle velocity of the moving object, but according to the angular distance of the peripheral stimulation from the fovea. Thus the movement of the peripheral stimulus through an arc of 2° at a distance of 40° from the fovea would be accompanied by a motor impulse corresponding to twice the angle that would accompany a similar movement at 20° from the fovea. It may be objected that the motor factor, under these circumstances, might not correspond to the actual eye movement, but to the difference between the impulses needed to fixate the peripheral stimulus at the beginning and at the end of a given arc of movement. This hypothesis is untenable. Exact registration shows that the average error in the eye movements by which we seek to fixate a peripheral stimulus at 40° from the fovea is from 20 to 30 times as great as the total arc through which a point of light must move in order to be apprehended as moving in a given direction. Obviously the graduation of the motor impulses in response to peripheral stimulation is altogether too inaccurate to account for the delicate sensitiveness of the periphery to moving stimuli.

We must, I think, conclude that, however much the traditional motor factor may enter into the foveal perception of motion, it cannot enter into the peripheral perception of motion as an immediate datum.

Pursuit movements of the eye seem to be regarded by those who maintain the existence of a kinæsthetic factor in the visual perception of motion, as though they were analogous to the passive movements of the hand, as it rests on a moving object. Naturally, such an analogy is altogether false. There are no passive movements of the eyeball, except the purely mechanical displacements resulting from pressure. All phases of the pursuit movement involve definite motor reactions to retinal stimulation.

Even if it were possible for the pursuit movement to begin in response to some central cue, as Holt seems to maintain, it is obvious that, until corrected by subsequent visual data, the eye movements could be no better clue either to the direction or to the velocity of the actual movement than the centrally conditioned expectation which occasioned them. While, unless the antecedent expectation were altogether correct, a kinæsthetic factor must be a source of error and confusion.

If, on the other hand, pursuit movements occur only in response to definite peripheral stimuli, as I believe, there are certain general characteristics of all reactions that render the value of kinæsthetic data in the visual perception of motion extremely problematic. Every reactive pursuit movement of the eyes must be conditioned both in direction and in velocity by certain definite characteristics of the sensory stimuli which occasion it. Not only can its accuracy never transcend the accuracy and completeness of the data on which the reaction occurs, but the two would be equal only in a perfect organism. It follows that no kinæsthetic factor from a reactive pursuit movement of the eyes could ever correct or materially augment the data furnished by the stimulus to reaction. That the antecedent data should be ignored in favor of a less accurate and delayed kinæsthetic factor seems to me a highly improbable hypothesis.

Not only, however, would the kinæsthetic data from a reactive pursuit be useless if it existed, but it would be a positive source of error and confusion, since, as a reaction, the pursuit sweep can follow the stimulus only after the elapse of a definite reaction interval. It might be supposed that, by reason of its hypothetical importance in the visual process, if not on purely anatomical grounds, the reaction time of the eye would be unusually short. The fact that it is in reality unusually long, 160–170°, indicates at once the relative unimportance of immediate pursuit and a considerable elaboration of the stimulus in what seems to introspection like a simple reaction. But any reaction interval at all means that, at the beginning of a pursuit sweep, neither the velocity nor the extent of the eye movements parallels the movement of the object of interest. Before the two could even approximate each other, the line of regard must first overtake the moving object. It is evident that kinæsthetic data from these pre-pursuit movements would not only be misleading in themselves, but that if they ever came into operation, general psychological law would tend ultimately to effect their elimination. Naturally this objection to the interjection of a kinæsthetic factor in the visual perception of motion holds only for the first phase of the pursuit sweep. But the recurring positive corrective movements, together with the lagging of the true pursuit movements in the second phase of the pursuit sweep, are also incapable of furnishing reliable kinæsthetic data, either for the perception of motion, or for the successive corrections of the pursuit. Moreover it is evident that, before the second phase of the pursuit sweep begins, the objective movement must have been already apprehended both as to its direction and its velocity.

Experimental Verification.

Any attempt to verify the theoretical deductions from the nature of the pursuit movements will be embarrassed by the practical impossibility of isolating the hypothetical kinæsthetic data. No natural pursuit movement, as we have already seen, can yield the motor data in pure form. It must always be contaminated by some displacement of the retinal image. And, unless the background be thoroughly homogeneous, all the usual forms of modification of the retinal image may occur. If all modification of the retinal image is to be avoided, an isolated stimulus must be produced at the exact time of a homogeneous eye-movement; it must have the same velocity as the eye-movement, and cease when the latter ceases. Since these conditions can never be satisfied by a reactive movement of the eye in response to a moving stimulus, the required homogeneous eye movement must be induced in some way independently of the moving stimulus, while they are, nevertheless, exactly concurrent. These requirements appeared at first sight utterly unrealizable. They are, however, fulfilled with striking fidelity in an experiment which was originally arranged as a test of the possibility of retinal stimulation during eye movement.[1]

A disk of black cardboard, perforated near the periphery by a concentric circle of small round holes made by a leather punch, was rotated by suitable clockwork between the eye and the clear sky. By purely empirical means, a critical velocity

[1] Psychological Review, Vol. VII., p. 458.

was found, such that, when a point just behind the perforated disk was fixated, the intermittent stimulation through the perforations fused to a circle; but when the line of regard was allowed to wander in the direction of the disk's rotation, from the primary fixation point to another about 4.7° distant, the fused circle broke up into bright, clean-cut perforations. The significant part of the experiment, in view of the present discussion, is the fact that, whenever the perforations were seen at all, they appeared to be standing still. They flashed out from the fused circle of light and disappeared again, apparently at the same point in space. The experiment is singularly exact. There was no opportunity for secondary corrective movements of the eyes, since the whole duration of the eye movement was less than 30σ, and corrective movements do not occur in movements of five degrees from the primary point of regard. The stimulus must have remained at approximately the same point of the retina from the time it differentiated itself from the fused circle of light until it disappeared. Finally, since, under ordinary circumstances, as the line of regard passes from one fixation point to another in a motionless complex field of view, there is no new effective stimulation of the retina; at exactly the same moment when the discreet stimuli appeared the entire background must have disappeared. Only one of the traditional data for the perception of motion is present, namely, the persistent stimulation of the same point of the retina throughout homogeneous eye movement, and that signally fails to effect a perception of motion.

One obvious objection detracts from the conclusiveness of the experiment. If we accept the differentiation of the two types of eye movements which I have been at some pains to establish, we must acknowledge that our experiment proves nothing for the true pursuit movements, but only for the eye movements of the first type. The change of the line of regard from the primary to the secondary fixation point was in no sense a pursuit movement. It occurred as a simple reaction to an eccentric stimulus, entirely independent of the moving perforations on the periphery of the disk. The question whether the slower normal pursuit movements yield sensory data for the perception of motion is consequently not answered by the experiment. One important step, however, is taken. Since the first phase of every pursuit sweep involves only movements of the first type, it is clear that the apprehension of movement must proceed entirely independently of data from the eye movements until the second or true pursuit phase begins. This means that kinæsthetic data are not available, if indeed they are available at all, until so late in the process that they would be useless if they ever existed.

There is no corresponding experiment for the true pursuit movements. The one prohibitive circumstance is the presence of the rapid corrective movements of the first type, and the persistent lagging of the line of regard which occasions them. There is, however, a form of eye movement which approximates the velocity of the true pursuit movements, which is conspicuously free from the minute corrective movements and the consequent displacement of the retinal image. This form I have elsewhere called the coördinate compensatory movements.[1] The type may be defined as those movements of the eyes by which the constant fixation of an unmoved object of regard is maintained during rotation of the head. Photographic registration of the coördinate compensatory eye movements proves conclusively that they are not preceded by any reaction interval after the head begins to move. They show no intercurrent corrective movements and no lagging of the pursuit unless the head movements are extremely rapid. The movements of the third type are, consequently, true pursuit movements in everything except in origin. Both approximate the apparent angle velocity of the object of interest; only in the third type the fixation is without interruption and without measurable error, so that there is no discoverable displacement of the retinal image.

Disregarding the matter of origin, which theoretically ought to have no influence in the matter, these characteristics conform admirably with the experimental requirements for isolating the kinæsthetic factor, if it exists. If an intermittently luminous point of light is fixated in an otherwise darkened room, while the head is rotated slowly from side to side, on a vertical axis,

[1] *American Journal of Physiology*, Vol. VIII., p. 322.

through from 10°–20°, it will be found that there is no apparent motion of the point of light so long as the intermittent flashes fuse completely to one undistorted point. If, subsequently, the velocity of the head movements is increased to the maximum, the coördinate compensatory movements will no longer be exact. The point of light will appear distorted or multiplied, and coincidently there will be a marked illusion of motion of the luminous point. This seems to me an almost perfect verification of our theoretical conclusions. We have produced an eye movement of the general characteristics of the pursuit type, in which an undisturbed fixation of the object of regard is maintained without any of the other cues of motion. As long as these conditions persist there is no appearance of motion, notwithstanding almost continuous eye movement. The moment a slight displacement of the retinal image occurs, however, there is a vivid illusion of motion, which there appear to be no kinæsthetic factors to correct.

A simpler though in some respects less satisfactory variation of the above experiment serves the double purpose of an easy test of the main point and an answer to a possible criticism. If a grating of fine wire is suspended about half way between the subject's eye and a smooth wall with a single conspicuous figure, it will be found that, when the grating is fixated during moderately rapid movements of the head on a vertical axis, the wall figure will appear to move back and forth behind the grating. Whenever, on the other hand, the wall figure is fixated during similar movements of the head the grating will appear to move. The principle is the same as in the dark-room experiment: coördinate compensatory movements of the eyes maintain the fixation, once established, whether of the grating or the wall. In either case the image of the fixated object remains practically motionless at the fovea. The eccentric position of the eyes with relation to the axis of the head, and the consequent lateral displacement of the eyes during each head movement causes an apparent displacement of the objects lying along the line of regard, which varies directly with the lateral displacement of the eye and inversely with the distance of the object. There is of course no real motion of the objects, and no reason outside the habitual interpretation of the sensory cues why the illusion of motion should attach itself to one object in the immediate foreground and not to another. That those objects appear motionless whose images remain motionless on the retina during head and eye movement, while the slightest displacement of the retinal image causes the illusion of motion, clearly indicates not only the utter irrelevance of the hypothetical kinæsthetic data, but also the real source of the relevent data. In view of the fact that the amplitude of the coördinate compensatory eye movements varies indirectly with the distance of the point of regard as well as directly with the amplitude of the head movement, it would be absurd to object that the kinæsthetic data from the eye movements were in some way counterbalanced by the kinæsthetic data from the head movements in the opposite direction. Finally, it would be indefensible to contend that, in this particular form of eye movement, experience had eliminated the false kinæsthetic data, since the same experience ought also to have eliminated the data which occasion the persistent illusion.

It is not improbable that eye movements of the third type compensate for other bodily movements besides those of the head; but the difficulties of registration have thus far prevented a demonstration of the hypothesis. There is, however, some indirect evidence in the fact that phenomena, similar to those described above, may be observed by myself when I walk and when I sway the trunk at the hips. The point fixated always seems to remain fixed; while adjacent objects which lie considerably nearer than the object fixated, or which lie considerably further away, seem to move up and down at every step. The fact that some of my students have not obtained self-consistent results from this last form of the experiment may be due to the general difficulty of maintaining a constant fixation for one point while steadily observing another; or it may be due, on the other hand, to faulty compensatory movements, such as are demonstrable in my own case, whenever I try to maintain a constant fixation while rotating the trunk on a vertical axis at the hips, when the neck is held stiff. Whenever the latter explanation is the true one, the eye movements will be of the second type, and will give evidence of the minute corrective movements which belong to that type. In my own case, just described, this results in illusions of motion covering the entire field of view, irrespective of the distance of the object fixated. If the bodily movements are continued long enough, dizziness develops. In the case of one of my pupils it was possible to demonstrate a faulty fixation, and this is my excuse for what might seem like an impertinent caution to those who repeat the experiment.

The familiar attempt to measure the importance of the motor factor in the visual perception of motion by the least perceptible motion of a point of light in a dark room needs no detailed critique in addition to what we have already said concerning the character of the pursuit movements. But besides the displacement of the retinal image, incident to the initial reaction interval, and to the more minute corrective movements of the pursuit, there are new complications introduced by the involuntary lapses of fixation, and the consequent persistent illusion of motion with which everyone is familiar who has worked with isolated visual stimuli. While the ordinary form of the dark-room experiment is thus rendered altogether equivocal and meaningless, a modification of it was accidentally hit upon, which constitutes a faultless experimental test of our conclusions. We have already called attention to the fact that the end of every pursuit sweep is freer from corrective movements than its beginning. This is conspicuously true of the pursuit sweeps by which the line of regard follows a swinging pendulum. Photographs of such sweeps give no indication of corrective movements either negative or positive within the last quarter of the swings studied. This ought perhaps in itself to have suggested the experiment. That the observation actually occurred without premeditation only made it the more striking. We were studying Exner's comparison of the apparent velocity of a moving object when pursued and when not pursued, and as a variant of his experiment we used the long counterbalanced pendulum which was previously used to furnish the stimulus for the above-mentioned photographs. Movable points of light were attached to the pendulum rod, one above the axis and one below. If the distance of both from the axis was equal, both would move through equal distances in the same time. The one fixated however always appeared to move much less than the one seen peripherally. It was found that if the two were to appear to move through equal arcs, the pursued must actually move through about three times the arc of the unpursued. This of course could be accurately measured by the relative distances of the two points from the axis.

This alone is good evidence that the hypothetical kinæsthetic factors in the perception of motion must be of less relative importance than the displacement of the retinal image. The objection might still be raised, however, that, if the fixated moving point be seen to move at all, some kinæsthetic data must be postulated. The force of such an objection has been already weakened by the preceding demonstration of the presence of corrective eye movements in all true pursuit movements. But while these corrective movements always involve some displacement of the retinal image, it is obviously difficult to demonstrate that in any one case the fortuitous displacement of the retinal image entirely accounts for the perception of motion. The most interesting and conclusive phase of the experiment I have never seen reported but it may be easily verified with the simplest kind of apparatus. When the point fixated approaches its extreme position in each oscillation, it seems to rest for an appreciable interval, while the other point seems to continue moving as though the two were connected by an elastic rod, which regularly gave the unfixated point a considerable additional oscillation after the fixated point had been arrested at the end of each swing. The illusion is persistent and striking, and is capable of only one explanation. It occurs at that part of the pursuit movement which photographic registration shows to be practically free from corrective movements. The fact that the point whose image remains motionless on the retina during an unbroken pursuit movement seems to stand still, while the other point, which is in reality moving no faster than its fixated companion, seems to make a little gratuitous whip-lash excursion, serves at once to show the utter inability of the pursuit movement either to subserve the perception of motion of the fixated point or to correct the exaggerated data from the displacement of the retinal image of the non-fixated point.

AN INQUIRY INTO THE NATURE OF HALLUCINATIONS. I.

BY BORIS SIDIS,

Director of the Psychopathic Hospital and Laboratory of the New York Infirmary for Women and Children.

The subject of hallucinations forms the stumbling block both of the psychologists and psychopathologists. The deeper one penetrates into the subject the greater confusion he encounters. Some regard hallucinations as being of peripheral origin, others regard them as central in character, while still others go to the extent of claiming that the most central hallucinations are of supernatural origin, being communications and messages from a transcendent world. In view of the great importance of the subject it may be well to make an attempt to throw some additional ray of light on this obscure matter from the standpoint of psychological and psychopathological analysis.

The usual definition of illusion and hallucination is with regard to the external object. Illusion is defined as fallacious perception of some actually existing object, while hallucination is perception of a non-existing object. This definition is good for practical purposes of the clinician, but it is not psychological. From a strictly psychological standpoint illusions and hallucinations cannot possibly be differentiated from other psychic states by the presence or absence of external objects. External objects can hardly be regarded as constituents or necessary ingredients of psychic states. Illusions and hallucinations should be defined in terms of psychic processes. As far as process is concerned it is quite possible that the same processes underlie both normal and fallacious or abnormal perception. In order to get a clearer insight into the nature of illusions and hallucinations it may be well to begin with a brief analysis of the process of perception.

Part I.

If we take a cross-section of a moment of consciousness and try to fixate it with our mental eye, we find a central psychic experience, or psychic element round which other psychic experiences or psychic elements are crystallized and organized. This psychic experience, or central element, is prominent, vivid and constitutes the vital point of all the other organized states, giving the tone to the rest, to a whole, to one organized experience. The psychic matter that surrounds the luminous central point does not stand in a free more or less disconnected relation to the latter, it is intimately related to the center and cannot be separated without destroying the moment as a whole and even the life existence of each particular constituent. The whole moment seems to form an organic network in which the other elements take their place according to a plan. The structure of the moment may in this respect be compared with that of the cell. In the cell we discriminate a nucleus round which cytoplasm is grouped. The protoplasm is connected with the nucleus by a network imbedded in the cytoplasm by a cytoreticulum. The destruction of the nucleus affects the cytoplasm and the destruction of the cytoplasm affects the nucleus. The two are intimately, organically interrelated by the common network, the general plan of their organization. If we closely examine the percept, we find in it a central sensory element surrounded by other elements. This central element stands out prominently in the given psychic state, while the other elements are subordinate. Not that those elements are unimportant for the percept, on the contrary they are of the highest consequence and moment, they only lie outside the focus of the mental state. Along with the focus those elements form one organized whole. All the elements of the percept form one texture having the central sensory element as its nucleus.

Integrated, however, as all these elements are they are not of equal value and importance to the life existence of the whole. The central sensory element is of the utmost consequence, it is the vital point of the total experience. While the change, or destruction of one or of some of the subordinate elements may still leave the total percept unchanged, or but slightly modified, a change of the central sensory element or of the nucleus will profoundly modify all the other elements and their interrelation.

The elements of the percept may be regarded as bound up in a 'chemical' compound, so to say, together giving rise to the qualitative aspect of the total combination, the difference being that in the psychic compound there is a central element that gives the keynote to the combination; in the chemical compound the elements are all equal in value and importance. To form water for instance, an atom of hydrogen to two of oxygen is required; the oxygen and the hydrogen are both equally requisite to the formation of the compound; one is not more important than the other. Not so is it in the psychic compound; there the elements are of unequal value. The most important of them is the nucleus; it determines the interrelation of the psychic elements and also the outcome of the whole combination.

From a biological standpoint we can well see why this should be so. A psychic compound is biological, not purely physical or chemical. In the physical components there is no higher and no lower, all are of equal value; in the psychic, as in all life existence, there is a higher and a lower in structure. In other words, the biological compound is peculiar and different from the chemical, inasmuch as the former is really not a compound, but an organization. The characteristic of organization is just this systemic combination of parts related in different grades and orders of importance for the total life existence of the whole.

Looked at from another standpoint we can further see the necessity of such a central element. We have pointed out in another place that one aspect of the biological process is that of purpose, and if that be granted, then psychic processes regarded as highly developed biological processes should present this general characteristic of purposiveness in its fully developed form. Now, where purpose is involved the end alone is the important thing, all the other elements are for that end, subordinate and determined by it. Not that the other elements are unimportant; they may be intimately related, but they, after all, are only means to accomplish the end. The elements that have for the time being the organizing power to aggregate round themselves the proper elements and lead towards the required end are predominant. For every psychic state is essentially for some reaction and that sensory element which gives the cue for the formation of the psychomotor elements leading to some given reaction is for the time being the center, the nucleus of the total state.

The flower before me attracts my attention. I see its color of a light violet tint, its rounded bell shape and its velvet-like softness; I stretch out my hand carrying the flower to my nose, to enjoy its fragrance. What I really see is the light violet tint; the rest of the elements are not given directly, they are largely inferred. The shape is largely an inference from previous muscular eye-movements and its softness is derived from previous tactual experiences. They are all, however, perceived by the eye, the cue being given by the prominent sight sensation. Although the flower as percept appears as an organized whole, still the sensation of sight forms the nucleus round which the others cluster; the perceptual tone is given and determined by the particular sensations of sight. The softness, though perceived, is still altogether different from the sensation softness as directly experienced through the organ of touch; it is a sensory *sight* softness. The same is true of all the other sensory elements; they are all essentially determined in their quality by the central sensation. In seeing a lump of ice we perceive its whiteness, its transparency, its hardness, its smoothness, etc. The hardness and smoothness are essential elements of the percept ice. These elements seem to be given directly in sensory experience. They seem to be directly perceived and still are qualitatively different from the hardness, smoothness and coldness as given directly by the experience, when the hand gets hold of a smooth lump of ice. The sensory elements are determined and colored by the central visual sensory elements. The sensory elements of coldness, hardness, smoothness are of a visual character.

It is usually claimed that such additional elements that are not given directly by the stimulated organ are elements representative in character, derived from memory. This statement is not quite correct. In seeing the piece of ice the hardness, smoothness are not represented, they are *presented* to the eye; we really seem to see, to experience these sensations going to make up the percept ice. It is not true that on catching sight of a whitish, transparent, glittering lump we remember that it is also hard, smooth and cold. The whole percept with all its sensory elements appears at once in the synthesis of the percept ice — we see, we perceive the hard, smooth, cold ice. The hardness simply remembered is altogether different from the hardness perceived in the seeing of the lump of ice. The hardness, smoothness as they appear in the ice are different to the eye and as different psychologically from the corresponding representations as the latter differ from the corresponding sensations directly experienced by the appropriate sense organs.

Pathological cases seem to confirm the same point of view. There are certain mental diseases, when the patient can perceive the object correctly, though he cannot represent it to himself. The patient can perceive all the elements on being confronted with the object, but by no means can he remember them. On the other hand, there are cases when the patient can easily represent to himself objects, but cannot recognize the object when directly confronted with it.

The subordinate elements in their turn play an important *rôle* in the total unity of psychic experience, in the percept, inasmuch as they give the content of the total moment, fermented, so to say, by the predominant nuclear element. The visual elements, the perception of play of light and shade would have been nothing but mere play of light and shade, if not for the subordinate tactual and motor elements that give rise to the perception of distance, dimension, size, body. The leading element gives the character to the content by having it appear under its own special sensory aspect, while the other elements give the content to the whole moment. Thus where space is perceived through the organ of sight mainly, the space is visual in character, though the content that gives rise to the perception of space itself is filled in by other psychic elements.

The central element with its content may be regarded as the nucleus of the cell surrounded by its cytoplasm and the total organic whole may be termed psychic compound. Now in the psychic compound the constitutent elements of the content can no longer be directly discriminated. In the lower forms of mental life the elements are firmly bound as we find it to be in the psychic compound — the percept. In the forms where the elements are more complex the synthesis results in fusion in which the elements can be more or less easily discriminated. Thus if one listens to the beats of a metronome and to the rhythmical vibrations of a pendulum the sensations blend and fuse, the sounds seem to proceed from the vibrating pendulum. The same case is well illustrated in the well-known amusement of having one hidden behind a screen and making a speech, while another one is watched who is gesticulating in accordance with the modulations of the speaker's voice. The two series of sensations blend and the voice seems to proceed from the gesticulating person. The synthetized elements here are fused or more or less 'mechanically' joined instead of being firmly combined in a sort of 'mental chemistry.'

In the higher and more complex mental states the constituents of the synthesis are neither 'chemically,' nor 'mechanically' fused. The constituents stand out free and distinct. While I am writing this page I see on my table at one glance the light, the lamp, the paper, the pen writing words and lines and hear and feel the pen move on the surface of the paper; at the same time I see the table, the chair in the room and hear the ticking of the clock. The multiplicity of all these experiences is simultaneously synthetized and at the same time discriminated in the unity of the total experience.

In the processes of succession of complexes of psychic elements, in the trains of ideas, the constituent elements attain their greatest freedom and independence. In the states of perceptual synthesis, on the contrary, the directly experienced sensory elements constituting the nucleus of the percept calls out immediately its appropriate associated perceptual elements and the compound, the percept, appears, as a whole that can be analyzed only under highly artificial conditions. Fixity is the essential characteristic of lower mental stages as well as of the percept.

The constituent psychic elements are so intimately united in the percept that they resist efforts at decomposition. If a percept *A* is composed of elements *a*, *b*, *c*, *d*, and if *a* be the nucleus, the stimulation of the nucleus brings out the rest — *b*, *c*, *d*. The central, or nuclear element is purely sensory, but the rest of the psychic elements *b*, *c*, *d*, are not sensory in the same sense as *a* is, since they are not derived from direct stimulations of the appropriated sense organs. Their character is not primarily, but only *secondarily* sensory. The retina gives only light sensations. The most differentiated and highly organized retinal structure of the highest vertebrates such as that of the higher mammals can only give rise to local signs, to highly differentiated light sensations varying with each retinal point or element, or cellular termination. The stimulated retinal elements with the neuron terminations of the optic nerve can give nothing else but light sensations and the image formed on the retina is in fact nothing else but a series of light sensations. If this be so, how then do we come to see that tangible, heavy, solid body yonder at a more or less definite distance? Solidity, bulk are not given in light sensations as such, how then are those spatial and physical characters perceived so distinctly as to assume a direct sensory character? It cannot be ascribed to the principle of association of ideas. For the object and its distance appear at once in one single glance before any idea comes to the mind. Furthermore, an idea from its very nature stands out distinct and definite; it is essentially free, but the psychic elements of object and distance are not discriminated. Again phylogenetically and ontogenetically sensation and perception precede ideation. The infant, the animal perceives objects and distance and certainly with little or no ideation present. In the visual perception of distance the subordinate psychic elements derived from other senses are not of an ideational character, they are of a sensory character. The eye *sees* the distance. The eye sees distance or volume directly, because of other elements involved in the process of perception, such as the kinæsthetic sensations coming from the movements of the eyes in their adjustment to the stimulations from the external environment, also tactual, muscular and kinæsthetic sensations derived from skin, muscles, joints and articular surfaces, all synthetized in the given percept. The subordinate psychic elements are neither of the character of pure sensations nor are they of the nature of pure ideas. What are they? They seem to be *intermediary* in character, intermediary between the nature of sensation and that of idea. Perception appears to be an intermediary process.

We may regard the same process from a hypothetical physiological standpoint which may possibly help us in picturing the mechanism. A specific physical stimulus produces in the peripheral sense organ a definite physiological process which is transmitted to groups of neuron systems stimulating them to activity and giving rise to specific physiological processes. Whenever these specific physiological processes are peripherally induced, the special sensory elements arise. If groups and systems of such psycho-physiological elements become associated and organized round a central nucleus, the result of the functioning activity of the total organic complex is a psychic compound, a percept. Whenever one of the groups is peripherally stimulated and is awakened to activity, the other elements become stimulated and the result is the organized activity of function of all the elements, thus giving rise to the synthesis of all the psychic elements, namely the percept.

Now we should postulate some difference in the pyschic state as to whether psychophysiological elements are stimulated directly through their own appropriate sense organ or whether they are awakened to activity indirectly through other sense organs. The *direct* peripheral stimulation gives rise to psychic elements characteristic of the particular sense organ and its nervous tracts and central systems of neurons, whilst the *indirect* peripheral stimulation gives rise to psychic elements whose pure and real sensory character is not clearly revealed in the total psychic state or moment. These indirectly induced sensory elements are so much colored and infused with the sensory qualities of the nuclear sensory elements that their character and origin are transformed and they appear not to differ in their nature from the nuclear elements. A closer inspection however fully reveals their real nature as sensory elements extraneous to the nuclear elements and derived from different sensory sources. The nuclear elements are *primarily* derived, in so far as they are directly initiated by the incoming peripheral stimulation, while the extra-nuclear sensory elements may be regarded as *secondarily* initiated by peripheral stimuli.

Let V be the sensory visual system, T and M tactual and sensory motor systems, A sensory auditory systems. Let V_1 be the visual sensations peripherally stimulated, T_1, M_1, A_1, the tactual motor and auditory sensations of the corresponding sensory systems. Let, further, V_2, T_2, M_2, A_2 be the psychic elements indirectly or secondarily initiated; then the percept when V_1 is the nucleus may be represented by $V_1T_2M_2A_2$.

Psychic elements primarily or secondarily peripherally initiated are not identical with ideational states. An idea differs qualitatively from a percept and its elements — an idea lacks sensory character. An idea is more generic, while a percept is more specific. I see that lamp-post yonder; it is a particular object rigidly limited in a particular space; not so is the idea, the idea of the lamp-post refers to lamp-posts in general. When I perceive an object and then try to represent it to myself, the object is not presented to consciousness in its sensory perceptual form — it is present to consciousness rather as a symbol ideally representing perceptual experience peripherally initiated.

From an anatomical and physiological standpoint it is quite probable that ideo-motor systems are different neuron organizations from those of the sensory-motor systems. Psychopathology with its rich store of facts seems to favor this view. As we have already pointed out there are pathological cases when the patient does not know the object on perceiving it, although he can represent it to himself and again there are other cases where the patient cannot represent to himself the object, but he knows the object on perceiving it. Flechsig's embryological studies go further to show that the sensory centers are different from the associative centers which do not stand in direct relation with the external environment and appear rather late in the course of ontogenetic development. The view often maintained that the same sensory structures underlie both sensory and ideational processes does not seem to be probable in the light of recent research. The activity of the sensory-motor neuron systems does not give rise to ideas, but to psychic states essentially sensory in character. In the case of the percept the subordinate psychic elements entering into the synthesis of perceptual psychic compounds are of a sensory nature; they only differ from pure sensations in so far as they are not directly periperally initiated, but centrally, or truer to say, *indirectly peripherally* initiated and as such occupy an intermediary state between sensation and ideation. In other words, the subordinate perceptual elements may be regarded as *reflex* in character, as being of the nature of *secondary sensations.*

The nature of illusions and hallucinations is more or less cleared up from this standpoint and the latter in its turn may be still further illustrated and confirmed by the facts coming from the domain of abnormal mental life. Let us take a series of cases of abnormal or fallacious perception. In looking through the stereoscope the two plane dissimilar views are combined and give the illusion of a solid object. Here the illusion is due to imitation of external conditions; the external stimulations that give rise to the perception of a solid object are here closely reproduced. The visual sensory elements are stimulated and the rest of the groups are reproduced, the rest of the sensory elements or secondary sensations emerge and the perceptual synthesis arises. The illusions to which in my student days I attracted Professor Münsterberg's attention are of similar character. If each eye looks through a separate tube and if the other ends of the tubes are brought together, the openings of the tubes coincide, appearing as one, and the eye appears to look through one tube only. If now only one tube is looked through and the other eye glides along the surface of the tube the opening of the tube appears outside, removed and raised higher than the real opening; the opening appearing to be directly seen not by the eye which looks through the tube, but by the other eye that does not look through. The illusion can be emphasized by putting the hand where the illusory opening appears and the hand appears to be pierced by a round hole. Here the conditions are such that the convergence of the eyes displaces the lighted-up opening towards the field of vision of the open eye not inclosed in the dark tube. Similarly when closing one eye and having the other wide open we press the closed eye sideways towards the nasal side the round phosphene seems to be projected into the field of vision of the other eye and the phosphene really appearing in the field of the closed eye as one can convince himself by closing the open eye, appears to be directly seen by the open eye. In all these experiments the arrangement is such as to imitate conditions under which other percepts normally arise and the result is the reproduction of those specific states of perception. To take another example, in a fog or in the darkness we may take a tree for a man or mistake a rope for a snake. Similarly, in the shape of clouds and blots we can often see different figures. The illusion here is rather due to the vagueness of the cue or of the sensory nucleus, the character of which may vary with distance or with the intensity of light.

In mental derangements such as in the different forms of insanity or of psychopathic functional diseases, in hypnotic, posthypnotic and hypnoidic states the object is perceived as different, independent of external conditions, such, for instance, as convergence, divergence, light, distance. A chair may be perceived as a tiger no matter how the visual axis is placed or what the distance be, or how intense the light is. Certain definite visual sensations may be correctly perceived, but on account of central dissociation in psychopathic states quite different than the customary associated secondary sensations are aroused which in turn arouse different secondary sensations in other sensory motor systems of neurons and the result is a different psychic compound, an illusion or a hallucination.

In the preceding cases the nuclear elements obscured in different ways by the subordinate elements are nevertheless present in consciousness and still form the nucleus of the percept into which other subordinate elements enter as organic constituents, and give rise to fallacious perception. Should now the nuclear elements themselves on account of inattention or of their minimal sensory intensity, or what is still more often the case, on account of states of dissociation, should such nuclear elements be left out of consciousness or remain in the subconsciousness as in dissociative states, then the fallacious percept stands out clear and distinct in the light of consciousness and a fully developed hallucination results. Sensory elements which themselves may remain unperceived stimulate other sensory elements that give rise to a perceptual compound which is entirely of a secondary sensory character. The hallucinatory percept does not contain the primary sensations aroused by the stimulus; it consists of secondary sensory elements and as such a hallucination may be regarded as a *secondary percept*. *Hallucinations are of the nature of secondary sensations.*

The simplest state of hallucination is possibly found in the phenomena of synæsthesia or in the phenomena of secondary sensations, such as light-phonisms, sound-photisms, etc., when one sensation, instead of giving rise to a subsequent idea, awakens instead a qualitatively different sensation derived from another sense organ — a color or letter arousing a certain sound, definite sounds arousing certain colors and so on. When a certain stimulus makes an impression on a peripheral sense organ and gives rise to secondary sensations, we really have a hallucination, but in its simplest form. He who on seeing the letter *A*, for instance, also hears a sound or feels a prick, or a touch may be regarded as having a hallucination. In this simple form we can possibly more clearly discriminate the character of hallucination. When on seeing letter *A*, we hear a sound, the indirectly aroused auditory sensory elements do not contain the primary sensory visual elements. In the secondary sensation or in the more complex state of perception of secondary character the primary elements are left out. A stimulus may arouse sensory elements in one sensory center, which in its turn may stimulate systems of sensory elements in other sensory centers, thus giving rise to a group of secondary sensations synthetized into a percept, while the original sensation with its nuclear sensory elements may remain in the background. Such a physiological stimulus may often be not an external physical stimulus, but a pathological process going on either in the peripheral sense organ from which the nuclear sensory elements arise or in the sense organs from which the secondary sensory elements originate.

Let S be the stimulus and V_1 the visual sensory elements and $A_2 T_2 M_2$ the secondary sensory elements, then V_1 may be dissociated while the secondary elements $A_2 T_2 M_2$ stand out alone in consciousness as a secondary percept or hallucination.

It may again be that not only the primary but also the appropriate system of secondary elements may be left out of consciousness, while *associated* systems of secondary elements may be awakened and stand out fully in the light of consciousness and thus give rise to a hallucination removed in its character from the original primary elements with their organized secondary elements.

The preparedness of remotely aroused secondary groups may often be determined by the type of mental structure. Hallucinations of visions, or of voices, or of movements will predominate, according as the type of mental structure is visual, audile, or motile. The mental type plays, no doubt, a very important part in the formation of illusions and hallucinations. In the insane auditory illusions and hallucinations predominate in the audiles; and while, on the one hand, paranoiacs are often audiles, on the other hand, audiles are inclined to paranoia. In hypnosis hallucinations become more easily realized, if they are adapted to the mental type of the subject.

Preparedness and subexcitement of ideo-motor groups with which the secondary sensory groups are associated also form an important factor in the final determination of the character of the illusion or a hallucination. This is rather of an indirect character. It is not that the ideo-motor groups themselves directly enter into the structure of fallacious perception, but they often may determine which of remote secondary sensory groups should be stimulated to activity. Groups of elements are more easily brought into active functioning the greater the activity of the elements with which they are associated, the course of group excitation being, so to say, in the direction of least resistance.

Pathological processes going on in one sense organ may sometimes give rise to secondary sensory elements belonging to other sense organs especially when favored by general states of dissociation; in fact we may say that from our point of view a state of dissociation is an indispensable condition to the formation of hallucination. The following cases may be taken as clear typical instances. Thus in one of the cases reported to me by one of my associates, Dr. Wm. A. White, the patient saw spirits and regarded them as ghosts of her deceased daughter. On examination her eyes were found to be normal in all respects. The patient saw the spirits even when her eyes were shut, and furthermore the hallucinations were not in the least affected even when her eyes were injected with atropine. When, however, the ears were examined a pathological process of old standing was discovered. Now when auditory stimuli were applied to the ear, the hallucinations were at once strongly affected, the spirits multiplied in number. This increase of spirits ceased as soon as the auditory stimuli were removed. A closer examination revealed the fact that the patient was greatly affected by the loss of a daughter. The pathological process served as the stimulus, while the excitability of the ideo-motor systems along with the general state of dissociation determined the nervous processes initiated in the ear in the direction of the sensory visual systems and gave rise to secondary sensory elements formed in the hallucinatory percept of ghosts and spirits resembling the patient's daughter. The aural pathological process itself remained in the background of consciousness and was unknown to the patient.

A similar case came under my notice in a paranoiac who had visual hallucinations of spirits, hobgoblins and saints. The organs of sight and hearing were found normal, but a pathological state was found in the skin of his scalp and especially in the muscular sensibility of the muscles of the neck. An inclination of his head in any direction caused him to see the spirits and hear their voices. In another case of mine definite auditory stimuli such as the singing of birds brought about hypnoidic states which are really complex states of hallucinations. In another case, in a female paranoiac with clearly defined auditory hallucinations, a similar state was revealed. The patient heard voices not through the ear, but through a spot located just over the region of the Fallopian tubes. Examination of the spot revealed tenderness and painfulness to pressure. The hallucinations, which were of a sexual character, became exacerbated during the menstrual period. Similarly in another case under investigation the auditory hallucinations were shown to be intimately connected with phenomena of unconscious phonation and with frequent earaches, with a limitation of the field of vision due to an error of refraction which, when corrected by eye glasses, modified the auditory hallucinations, the latter finally becoming dissolved. More cases of similar nature could be adduced, but the ones referred to are sufficient, and extreme as they are, they bring out clearly the secondary reflex character of hallucinations. *Hallucinations are essentially secondary percepts.*

Hallucinations are frequently due to peripheral processes, pathological or otherwise, occurring under conditions of dissociation, within the same sense organ, but the reflex hallucinations originating in other sense organs bring more clearly to light the secondary nature of hallucinations. The contention generally maintained that there are hallucinations independent of peripheral sources, or of 'purely central origin' which some even regard as supernormal experiences is highly dubious. As far as directly observed facts go, whether they be normal or abnormal, there is little to justify the central point of view. Like percepts hallucinations are peripheral in character, and are only in so far central as *peripherally* initiated secondary sensations are concerned. Hallucinations are of peripheral origin and may be regarded as complex cases of secondary sensations with the original primary sensation dissociated from or left in the background of consciousness. If, however, hallucination is abnornal perception, perception, on the other hand, is normal hallucination. If a hallucination is a secondary compound with the primary sensations ABSENT, a percept, in so far as it consists of secondary sensory elements, is a hallucination with the primary sensations PRESENT. Normal perception, illusion and hallucination have the same underlying process and as such may be arranged in a continuous series, according to presence or absence of the primary sensory elements.

(*To be concluded.*)

THE LIMITS OF PRAGMATISM.[1]

BY PROFESSOR J. MARK BALDWIN,
Johns Hopkins University.

Pragmatism — variously understood, variously approached, variously turned to account — is in the air. I shall not venture to define pragmatism, much less to ascribe any definite form of it to any particular writer; on the contrary, I shall merely state broadly the general idea involved in it, as a way of looking at things, and from the outcome try to reach an understanding as to what in fact it is.

Broadly speaking, enquiries are pragmatic which, with more or less thoroughness, make such conceptions as thought, existence, truth, reality, etc., relative to other terms in a movement, development, or evolution; relative to antecedents, consequences, modes of function, ends. All such determinations are not only ends reached in a movment, but also means to ends yet to be reached; and all of them, considered thus functionally, as terms of genetic organization, in so far forbid definition in a static, absolute, once-for-all-fixed system. Now whether or not all those who call themselves pragmatic — not to say pragmatists — admit that this fairly characterizes that feature of their thought, still that is what I now mean, and that is what this paper is about.

In the theory of knowledge, it is one of the main claims — and one of the prime advantages — of pragmatic theory, that it avoids and denies any dualism between reality and thought, in the sense that thought, or knowledge, somehow represents or reveals a system of realities which are already fixed, definite, and absolute, apart from the processes of cognition. It claims that reality is determined as truth, and truth is a mode of mental organization. It claims to be able to point out the adequate processes of knowledge and action, of selection and systematization, which determine truth; and thus to yield a theory of

[1] Paper read before the joint seminaries, Department of Philosophy, Princeton University, December 1, 1903.

reality which admits dualism only in the mechanism of the psychological processes themselves.

Now fully admitting both this claim and this advantage up to a certain point — I have myself worked out in recent publications such a view of truth, and of reality as cognized — I wish now to suggest and discuss certain limitations of this standpoint.[1]

The discussion may be brought on under suggestions made in reply to the general question whether there is anything in the conception and implications of reality considered reflectively and for the purposes of philosophical theory, which is not explained both as to its origin and mechanism, and also as to its validity, by this theory of pragmatically determined cognitions or truths. And this question may be resolved into certain more restricted ones:

1. Are there any realities apprehended apart from the cognitive function, or at least not adequately apprehended through it? If so, what is their relation to cognitive reality or truth?

2. Are there any realities not, or not yet, discovered at all; and if so what meaning do they have for us?

3. Are there any types of thought, or modes of treating reality generally whose meaning is not exhausted in the statement of their pragmatic origin?

I

These are large questions, and it is the outcome of centuries of effort that rationalism or rational idealism — the philosophy with which pragmatism comes most evidently into opposition — has worked out intelligible answers to them all. Its answer to

[1] This suggestion of the need of limitation, made in articles in the writer's *Dict. of Philosophy* (arts. 'Pragmatism,' and 'Truth in Psychology') is cited by Mr. Schiller in his book *Humanism*, p. 8, as involving an inconsistency. I wish to work it out here, fully maintaining this theory of the relation of truth to reality, however, as suggested there (art. 'Truth') and stated out more fully earlier in the address 'Selective Thinking' (now Chap. VII. in the work *Development and Evolution*). Mr. Schiller asks how on pragmatic principles of origin we can 'get at reality without knowing it'; I reply (see the end of this paper), by some other experience better qualified to report it exhaustively. His other question — how our estimation of what truth *is* can disregard and become independent of our modes of establishing it? — is also answered here: *i. e.*, they can not, but they (our estimations of truth) can include and revise the results of the theory of the establishment of it.

the first question is: 'No'—but with hesitation; for it has great difficulty in mediating the first contact of thought and reality. To the second it answers: "Yes; there is a system of real, but cognizable, relationships which are yet to be developed in the system of thought." To the third question its answer is an emphatic affirmative, for here is the region of its strength; it claims that there are two, at least two, marks of thought which must lift it in our estimation out of the empirical and pragmatic movement—its teleological character, on the one hand, and its normative character on the other hand. In these respects, say the logicist thinkers, thought not only reveals reality but it is a unique and most distinguished mode of reality *in its very self*.[1]

How then, we may ask, may the pragmatic thinker answer these three questions? Before taking that up, however, I think it would be profitable to inquire as to the presuppositions of a pragmatic theory as such: that is considered as an account not merely of knowledge but also of reality.

In the restricted sphere of knowledge any genetic or developmental account of thinking necessarily makes the thought function in some manner utilitarian, instrumental, adaptive. Thought proceeds by a series of constructions, discoveries, serviceable adjustments, etc. But unless we admit that the system is a self-developing one which advances under imminent rules of its own, which is just the assumption of the 'rationalists,' there must be a series of points of origin, stimulation—and to the onlooker, points of observation—with reference to which, and by the mediation of which, all the readaptations and new discoveries are accomplished. Of course this is what

[1] It is this claim with reference to thought which has had nothing like enough attention on the part of the pragmatic logicians, such as Dewey and his colleagues, in pressing home the refractory and persistent dualism of the idealistic theory of knowledge (Dewey, Miss Thompson, in *Studies in Logic*). The idealist, *e. g.*, Bosanquet, may reply that there is a fundamental identity of thought and reality, whereby the development of thought *is* a mode of reality which is fully realized only in the system of which thought, the individual's thought, is a part. For a criticism of the selective theory of thinking, as an exhaustive account of truth, see Bosanquet's strictures on the present writer's views in the PSYCHOLOGICAL REVIEW, July, 1903 (following up a discussion in earlier numbers).

in the biological and sociological sciences is called the *environment*. Even from the strictly psychic point of view, even for reflective thought itself, there is not entire autonomy within the movement of thinking. So true is this that the determinations of reality, not alone in the sphere of the external world, but also in that of the most abstract truth, now most current among psychologists, differ as to the place of such coefficients as 'resistance,' 'stubbornness,' 'limitation of activity,' 'experience of control'; but they do not ask whether they are there at all. The real, the fact, the truth, is in some sort or other, that which is accommodated to, that which *must be recognized* whatever else is refused recognition.[1] Putting this purely in psychic terms, sensational or other, we have to say that there are modes of conscious experience, entering essentially into the determination of truth, which are not determined entirely by earlier modes of experience; and these have their place and value just in virtue of their character as essentially determining future reality. Mr. Roosevelt's recognition of the Republic of Panama determines to me the truth or reality of that State. No one can view the pathological thought systems, with their pseudo-truths and realities, and refuse to admit that thinking is thus essentially conditioned upon what is both to the individual and to the onlooker extra-psychic.

Now what is this something, this environment, for pragmatism? Something itself constructed, selected, postulated by the thought processes? — something itself a part of that system of discovered truths and facts which it is the merit of this view to identify with reality? So the answer may read,

[1] It is an interesting instance of *Werthurtheile*, that pragmatic thinkers instinctively emphasize the manageable, selected, 'workable' aspects of external (including logical) reality. But the very question why some thoughts 'work,' while others do not, throws us back upon the environmental tests.

Professor Dewey seems genetically quite right (*loc. cit.*, p. 76) in making psychic objectivity an aspect of experience of control, rather than control a result of objectivity (externality); but before the rise of the dualism of which objectivity, in this sense, is a term, there is no subjectivity or 'inner' experience. If we are to have any explanation of the reason of the determination of both terms it must be one which does not assume one term — the sphere of subjectivity, the purely psychic — to account for the other (see the next point in the text).

when it is written: I know of no writer so far who has seriously proposed this question or who has seemed aware of the vital characters of the presupposition.[1] But such an answer would be open to certain grave objections. First, to maintain that all there is in the reality called the environment is what has already been through processes of thought and discovery and established as true, would mean that future thought processes should be entirely autonomous — entirely undisturbed by intrusions or stimulations which could disturb them and require new adjustments. Earlier truth would be *in* the thought system. Again, on this supposition, we might trace backwards the path of knowledge, and when we reached the first glimmer of cognition, ask what stimulated it, what were the necessary conditions of this sort of function in consciousness. Surely it could not be said that *its* environment is *its* truth-system. Further we might ask a similar question of the movement of thought as a whole in society or in history; if each step has been a tentative one, secured by the struggle and adaptation to which the thought function is ministrant and instrumental, with reference to what system, to what larger whole, are *these* adaptations and discoveries made? To deny that there is such a whole — such an environment progressively adjusted to — would be, it would seem, to give up the pragmatic method; for then there would be no recourse except to the idealistic position that thought is a self-sufficient and self-developing teleological system, not an instrument to anything.[2]

Another presupposition appears when we ask from what point of view pragmatism cites evidence of its truth: it must be

[1] Mr. Schiller dabbles in it (*Personal Idealism*, II.), I think to little profit Professor Moore intimates it in avowing a biological point of view (as does also Professor Angell, *Relations of Psychol. to Philos.*, p. 12 f.), but just then he stops! (*Studies in Logic*, p. 374.) Possibly the 'fragmentary' character of the pragmatic discussions is what we should expect from the truth of this theory of thought! Certain of these writers do seem to be urged on by what Royce calls a 'certain indefinite restlessness.'

[2] An alternative would be a form of 'radical empiricism' which actually accepts the postulate of various sorts of reality external to the individual as pragmatic development requires them. I suppose such a genetic realism, if it stopped there, might still by courtesy be called a philosophy! But on this see below.

replied —*from an objective point of view.* The adjustments, active adaptations, instrumental achievements, consequential sequences, etc., attendant upon an act of reasoning, for example, whereby the pragmatist defines it and validates it, are such *to him, a spectator*, not to the thinker himself. The psychic claim of thought is to be simply and only objective, *to terminate in the object which it constructs.* Granted the philosopher's 'will-to-believe,' even with it the believer claims to think exclusively on evidence, claims to free his mind from prepossession, prejudice, and voluntary bias. To be sure, in many reflective thought-processes, thinking is a conscious instrument, a means to a practical end; but to say that is far from saying that such pragmatic reference is a mark of thought, or constitutes its validity to the thinker. Such a general criterion can be claimed only from the psychological, in distinction from the psychic, point of view. I hold indeed that the instrumental character of thinking is marked, and that it is a function of utility in development and evolution: but that is my theory; I can not say that I am conscious of such features in my thought. It is just its own claim that thought is held to standards quite divorced from the individual's private volitions.

But once admitted that pragmatism takes an objective point of view of the thought function as a whole, and certain embarrassments at once follow. To assume this point of view is to accept the objective criteria of the whole process of thought: and that, when we find it in a *mélange* of phenomena of other sorts and classes — physical, social, inferential — each claiming objective value also by reason of its own set of objective coefficients. Once admit the validity of such an objective claim, and all sorts of truths follow, with the sorts of existence to which they belong: mental existence, physical existence, ideal existence, etc. In short, we have, when we assume that we can rely upon objective phenomena which pose as embodying a type of reality and claim to constitute it — we have by the same act to validate all such types as being what they claim. It suffices to make this point here; it comes up again below more forcefully where the dualistic implications of pragmatism are brought out. Here it may suffice to have made it clear that pragmatic philosophy involves this presupposition.

II.

Coming back now to the definite questions which we proposed to put to the pragmatist we may proceed to consider his answers to them one by one.

First, the question of *acognitive*, and what may be called *mixed modes of reality*.

In the literature, the development of pragmatic views has been largely in connection with the determination of cognitive reality as a system of accepted truths. Apart from the procedure of inferring from the results of the criticism of opposing views, the method characteristic of the writers of the Chicago publications, this has proceeded upon the psychological view of cognition which makes it a phase in a process of which action is another phase, the whole being the process of *the treatment of experience for practical purposes.*[1] If this be a valid way of looking at those modes of reality which are constituted by the exercise of the cognitive function, it would seem possible to pursue the same method in reference to those other aspects of mental function which also in some manner lay claim to real reference. If it be true, that is, that the criteria of truth upon which cognitive reality reposes and which serve as its coefficients, have their significance as being points of advantage in the life of active adaptation, the same may be true of the

[1] Such a view is explicitly carried out in my own development of the relation of thought and action in *Social and Ethical Interpretation*, Chap. III., VII. and *Mental Development*, Chap. XI. My treatment is to the last degree pragmatic (*cf.* the remarks of Caldwell, *Amer. Jour. of Sociology*, Sept., 1899), but being stated in terms of reaction it is objective in its point of view. Professor Dewey seems to prefer to speak in psychic or experiential terms, but gives us no account of the actual psychophysical factors entering into the concrete determinations of thought (such as kinæsthetic sensations, images, etc.) and thus, to my mind, loses the advantage of such psychophysical explanations as those given by the 'action,' 'synergy,' and other theories. Another result is his failure to work out the selective tests and checks upon thinking which are fully discovered only from an explicitly objective point of view. I have described the function as a self-repeating 'circular' (imitative) reaction, illustrating 'habit' and issuing in 'accommodation'; but I am quite ready to say, with the psychologist Dewey, that there is a conscious organization resolved into strains and tensions and reorganized in a new thought under 'stimulation' (but can not this term 'stimulation' be avoided?—it is shockingly objective!), or with the philosopher Bosanquet, 'that the thoughts which precede and follow, taken together, really illustrate 'identity in difference'!

coefficients of reality of other sorts — external, æsthetic, ethical, ideal, etc. It would be our task, as genetic psychologists, to work out the processes by which, in the active life, such and such marks of experience serve the life of adaptation in the sphere of reality respectively which this or that class of experience postulates. The objects of the external world, thus construed, are the experiences which mediate organic adjustments; æsthetic realities, those which mediate emotional adjustments; ethical, personal adjustments, etc. This would seem to be a legitimate and fruitful task — a larger problem of the genetic logic of reality — and one in whose accomplishment the pragmatist has a distinct advantage over the rationalist, by virtue of his doctrine of the concrete experiential determination of reality of whatever sort. So far, I think, the pragmatic method has great value, inasmuch as it recognizes the protests of heart and will — and anything else that can make good its claim — against an exclusively intellectualist theory of reality.[1]

But this is so far psychology, and, in a sense, logic. Can we stop there, and simply recognize the pluralistic real postulates of practical life? One of the fathers of pragmatism, Professor James, seems content to do so. Others again seem to have each his favorite among these different modes of reality. Some abhor pluralism, but rest content in dualism. Some say there are a lot of reals, but they are all ethical — thus deserting the pragmatic point of view. The question for the pragmatist here would seem to be: Is there any practical need for further adjustment of these realities to one another? — any issue of life which is subserved by the dominance of one mode of reality over the others? — or is there a further real construction which is realized by the subsumption of these varied realities? In connection with such an attempt at reduction the question as to the motive to it would arise. Is it a practical one — that is, is the demand for philosophy pragmatic and not purely theoretical? If so then are not the scales in any such weighing of claims loaded from the start with various personal, utilitarian and other practical preferences of individuals? — all of which are

[1] *Cf.* Dewey, *Studies in Logic*, p. 432, and the papers of Stuart in the same volume.

to this type of thought in themselves legitimate — or with social and conventional, types of belief, judgments of value, etc., which would hopelessly prevent any general unanimity or permanence of world-view? Possibly, then, we should have to appeal to the statistical logician, who deals with purely theoretical matter, after all, and asks him to establish a *pragmatic equation of error* whereby we might correct up the conclusions of those philosophers who ventured to think beyond the rankest pluralism!

But there is more than that in this question of the modes of reality. The objective standpoint itself issues in a dualism more sweeping than the various dualisms of the objective reality coefficients themselves. The prime and fundamental dualism is the old subjective-objective distinction. It is true it is now becoming, to present-day thought, a dualism for cognition, that is, a dualism between two objective constructions — *the me and the not-me.* Genetic and social psychology are making the 'me' the same as the 'other-me' of the social fellow, and both a part of the system of constructions determined as objective. But over and above this essentially objective dualism of 'me and not-me' there is, together with it and necessary to it, the dualism of the *subjective and the external*, of the 'inner' sphere of experience itself and, over against it, the 'trans-subjective.' This latter dualism would seem to be genetically earlier, at least earlier than the full form of that which embodies the distinction of mind and body; yet the self-not-self distinction, in the form in which it precedes the inner-outer distinction is largely one of organic or strictly presentative data. The dualism — to return to the last positive statement — which remains for reflection is that of 'inner' and 'outer'; it opposes *experience-as-such*, with its constructive principles of unity, continuity, identity, etc., to *outer-reality-as-such.* Here are two spheres, one of cognized realities *including selves as objects*, the other of *mental reality which functions as agent of the constructions of cognized reality.* Now what is pragmatism able to do with these great dualisms?

As to the me-not-me distinction there is no difficulty in securing full pragmatic justification for it. It has been a labor of the present writer in recent years to attempt to work out just that problem; the functional and active adjustments, principally social, under stress of which the me and the not-me are first distinguished in thought and furthered to maturity. If a person is to be a person, he must know that he is a person, and that others are persons; also that there are things which are not persons. So we may say: so much to the credit of a pragmatic method in psychology, so far as its attempt bears fruit.

But there is the other, the inner-outer dualism. This puts to pragmatism the different question: what is the practical origin and value of this distinction? And upon this the further question: is it possible to give a pragmatic account of this distinction without assuming it? This second question is put as it is in order to suggest the point of criticism now to be made — a criticism which, I think, lays bare one of the essential limitations of the pragmatic method.

The question raised in attempting to account for the inner-outer distinction is really that of a genetic account of reflection: for this dualism cuts through consciousness just where the spontaneous dualism of me-not-me passes over into a way of treating data of both these classes which recognizes the possibility that that which is objective may also be and perhaps only be 'inner' or peculiar to the individual's constructive processes, and not trans-subjective at all. The 'me' is objective but not external. Here the representative theory of knowledge at once springs up to interpret this naïve or common-sense reflection. The treatment of illusion, from the dictum that 'the senses deceive' of the Heraclitians and the Greek Sceptics, down to the elaborate theories of ideas and 'imagination' in modern times, witnesses to the real problem raised here: a world of 'inner' events and objects, ideas, images, etc., which may or may not be external or 'outer' also.

The ordinary idealistic solution is simply to take consciousness at its word, and concede that the representative series does not actually embody reality, but only copies or duplicates it. Then the problem is not so much to account for error in some representations as to account for truth in others: how can we be sure that reality really is in any case and anywhere what we in our 'truths' conceive. On the other hand, the pragmatist has on his hands the problem of error: if reality is that which the cognitive function normally reports, with no further reference to an outside system or series, how then can such a distinction ever arise as that between what is somehow referred to an external system, and what, though fully cognitive, is nevertheless only the product of the thinker's mind, while possibly to the thinker still making an erroneous claim to external value.[1] I see no great difficulty here, to the extent, at least, of finding pragmatic justification for the distinction of inner and outer, and with it of truth and error — provided we assume a thoroughgoing objective point of view. The pragmatist indeed must admit that such a point of view is, as we have found it to be, his necessary presupposition. The play of images necessitates mistakes and error; certainly so, if the imaging function is the vehicle of tentative, trial-and-error processes working in the interest of practical accommodation to an environment, physical, social, and other. So far so good: we have a genetic science, genetic psychology, depicting a development or evolution process in which truth and error are correlative aspects.

But in recognizing this necessity of the objective standpoint, we bring up again, and with increasing embarrassment, the problem of the environment. It is possible, as we saw above, to look upon the environment itself as a mode of pragmatically determined reality, but only on the assumption of the reality of mental function and development. But now we find that such recognition of mental function as a mode of real process is possible only in a dualism with the external — the dualism of inner and outer — of mind and that external reality or environment which we are to explain as a construction of the inner reality or mind. There is here a vicious circle. The only point of view (that is, the inner) from which the pragmatist can possibly explain the external environment is one from which *the very principle of his explanation*, a mental development by which the external is constituted as real, *is quite destroyed.*

[1] The difficulties of the representative theories are brought out with especial force by certain of the Chicago pragmatist writers: notably by Moore in *Existence, Meaning, and Reality* and by Dewey in *Studies in Logic.* But their treatment of the problem of error and illusion is correspondingly halting and inadequate, except so far as error is made to follow from a psychological process which is experimental (see, however, below).

It is a case of what I would fain call the *genetic fallacy*. In a recent paper[1] there are worked out the stages, called there 'progressions,' successively reached by consciousness in the development of the me-not-me dualism. And reason is there found for saying that a theory which would be consistent must not read into connection with a term on one side of this dualism at a given progression, a term from another stage or progression; the great case of it being, as there pointed out, the treatment of mind as real and body as subjective, when the very progression in which mind is found as real guarantees mind *only in a dualism with real body*. This is the 'genetic fallacy' — confusing the terms of different genetic progressions.[2]

We have here a case of it. The pragmatist must be a monist if he would explain the environment; but he can not account for error — nay, he can not be a pragmatist — without being a dualist. *He takes advantage of dualism to refute dualism.*

It is easy to see the necessity of insisting upon this requirement in comparative and child psychology, where it forbids us to construe the individual's mental constructions apart from the sort of function then and there possible; as in expecting any sort of moral insight in an individual which is not yet self-conscious. But it is harder to keep clear of the fallacy when we are theorizing about the mind. We fall to using the outcome of a later stage of reflection, which has its validity only as proceeding from an earlier, to criticise and invalidate the postulates or requirements of the earlier. Reflective idealism is possible only by refining one term of a crude dualism; it is then a logical circle to show that this refined self can not be a term in such a dualism. The proper method is to ask what becomes of both terms of the dualism in the process of further mental development. Pragmatism itself insists upon this in its protest against the extreme logicism which reads its developed 'universals' back into the crudest alogical states of mind.

The difficulty arising from this dualism is undoubtedly emphasized when we consider the question of time — a question

[1] Psychol. Rev., May, 1903.

[2] This general requirement of genetic consistencies is recognized in his comments on my paper by Professor Mead (*The Definition of the Psychical*). I could not wish a stronger enforcement of it than that of Dewey, *loc. cit.*, pp. 16 f.

which the idealist is not slow in urging upon the pragmatist. Time considered as a mode of pragmatically derived reality must be considered real in the sense that other abstract or conceptual modes are. The pragmatist has, from such a theory of time, a weapon of advantage in one respect, inasmuch as he can thus deny the reality of future, undiscovered, possibilities of existence, considering them as projections from realities already discovered and justified. But now the critic may say: you admit the reality of mind as a principle having a development in a time series, and recognize it as having constituted for itself a series of corresponding realities at its several stages of development; the reality of a time series considered as a mode of development of mental reality must be acknowledged, and with it the reality of time as a mode of other realities so constituted. The time mode is, in short, a mode common to the self postulated and to the realities in dualism with which the self all along knows itself. The independent time development of reality of whatever sort, therefore, must be admitted if the time development of the self be assumed. It is difficult to understand, indeed, why the reality of the future time-mode should not also be granted if we grant that of the past time-mode.

The advocate of some form of logical construction of reality avoids this dilemma by making time a mere cognitive mode — not real. But that has all the difficulties of a logical deduction of time, and also labors under the acute embarrassment of denying the progressive or dynamic character of reality. Here again the only solution possible to the genetic and pragmatic method would be one which actually depicted consciousness passing out of the dualism under which it is compelled to think both mental and external reality as in time.[1]

Writers of a pragmatic bent delight to press home the dilemma of dualism upon the representative theory of knowledge; and their indictment is, in psychology, unanswerable. And it is amusing how they instinctively fight shy of taking up

[1] An analogous question arises regarding space, except that it is a mode of external reality only; but pragmatic dualism would require us to recognize real space in the sense that physical existence in general is real. Only a mode of experience which not only satisfies but transcends spacial properties would be sufficient for a general theory of reality.

their own dualism. Yet withal they give signs of a certain apprehension of it. Notably is this the case in the able papers of Professor Moore. He concludes his examination of Professor Royce (*Studies in Logic*, XI.) by asking the question as to the tests or checks upon the action-knowledge process which the environment brings to bear; and makes the entire external and organic world as such environmental to the psychic function.[1] But such a dualism is just the last refinement of reflection in the distinction of self and not-self: it is the inner-outer antithesis which consciousness reaches only in its most extreme personal determinations. And it is this dualism of the reflective consciousness which is one with that of the philosophical theorist. Whether we consider the terms of the dualism as present to the actor or only to the philosopher, still the 'inner' and the 'outer' are in both cases alike cognitive constructions, and each is valid by the same right that the other is, and exactly to the same degree. If, however, we put the question to Professor Moore with reference to a stage of development at which the psychic dualism is not yet come, we must then take the objective point of view and treat that consciousness as *to us — not to it* — having such and such an environment. *It* has no ends,

[1] Yet he calls it all 'habit,' thus somewhat surreptitiously bringing the external world into the sphere of that which is already *adapted to*, neglecting that aspect of it which makes it that which is *still to be adapted to*. This is, of course, an easy way of 'begging' the pragmatist's dualism. In another place he explicitly speaks of 'habit somehow developing its own interruptions' in order to give thought a chance (*Existence, Meaning, and Reality*, p. 16). Remarking upon Professor Moore's treatment of my 'extra-organic' test (*Development and Evolution*, pp. 250–1) under that of the 'effectiveness of an idea to organize habit' (*ibid.*), I should say that it is just the intrusion of something that breaks up habit, and hence cannot itself be described as habit, that makes the reorganization by the idea necessary and possible. For example, I am in the habit of walking comfortably in a shoe with low heel; and when a new shoe with a higher heel strains my tendo achillis (as it just has!) my new idea of walking (on my toe!) arises and is effective as an accommodation to this new, in no sense habitual, experience of the high heel. In other words my old truthful idea of walking based on habit has broken down under the test of working in the extra-organic environment which includes the new shoe. This I have described (*loc. cit.*) as the 'test of fact,' supplementary to the 'test of habit.' So when Professor Moore says, quoting me, that a reorganization under stress of experience is entirely in "terms of '*activities already going on*'" (*Existence, Meaning, and Reality*, p. 18) he omits the new sensation experiences whose urgency compels the reconstruction.

no self-determinations; *it* makes no postulates; *it* simply acts straight-away as its stimulations bring it out. And any fruitful constructions of its 'purpose,' 'plan of action,' etc., are read into it by us. Much of the plausibility of the pragmatic philosophy comes from this playing between the two points of view.

In short—to sum up—the pragmatist must either frankly swallow the camel of a real environment which the knowledge function may then both truthfully and also erroneously reflect—a step which would involve him in all the epistemological litigation of the representative theories of knowledge—or he must find some guarantee for the reality of the mental principle which is not *rein pragmatisch.* This latter is his better course; the present writer adopts it as a limitation on his pragmatism. As psychological and logical method, the instrumental point of view is true and inevitable in present day evolution thinking; but there must be found a way to preserve it without expanding it into a one-sided metaphysics which then eats it up!

III.

The second general question proposed to advocates of pragmatism in philosophy is this: Are they undiscovered realities? What meaning can such a conception have for the pragmatist?

In order to give an adequate answer to this question, a detailed theory of the different modes of cognitive and logical process would have to be worked out. So far as I know, no one has yet attempted it from the pragmatic point of view. Especially would it be necessary to have an interpretation of the general, and universal, and normative aspects of experience, with accounts of the claims made by these modes, respectively, to report or embody reality. It would then be seen how successfully their claim to exhaustiveness was in truth made out. So far as these thinkers have gone they seem to be aware that if reality is to be consistently interpreted as a construction of experience, then there could be no realities which are not so made up in experience, in the way we call discovery. We are explicitly told that reality is actually made when it is discovered; that its development is, or proceeds *pari passu* with, the system of truths which is in process of progressive establishment. Of course in this matter the pragmatist is to be allowed all the possible reaches of cognition, and of other modes of experience, such as that found in social relationships, whereby all possible forms of apprehension are brought into play.

It seems to me, speaking tentatively, and subject to confirmation from the detailed study of the demands made by logical process generally,[1] that pragmatism is able to give a fairly consistent account of itself in this matter up to a certain point; a point which again marks the transition from psychology to metaphysics. The aspects of reality very clearly distinguishable, and with them the habits of mind which yield these aspects of reality, in complementary relation to each other and seeming when taken together to be exhaustive, are those called variously 'fact and value,' 'appreciation and description,' 'science and real life,' 'habit and accommodation,' 'prospective and retrospective reference,' and in the writings of pragmatic thinkers, though not so clearly expressing the same distinction, 'fact (or image) and meaning.' If we look at the distinction from the point of view of the psychic processes and attitudes involved, we may place on one side all that which is cognitively or actively apprehended, as the given, the established, the-now-and-here-existing; and, on the other side, those aspects of our mental determinations of all sorts which qualify the given or aim to establish it in any indefinite, hypothetical, or prospective way. Thus having divided the entire real predicate into that which to consciousness already real, and that which merely may be, seems desirable is not yet realized, we have to ask as to the pragmatic meaning and value of the latter member of this antithesis.

So far as pragmatic derivation and justification is concerned, they are not far to seek; the utility of hypothetical and normative motives to action is admitted on all sides. The reference to reality is, from the point of view of the pragmatic onlooker, that of a demand for progressive organization of the content already

[1] An attempt made in the volume on 'Genetic Logic' which is to be published in the near future, and of which certain of the developments are anticipated in this paper.

given and still subject to organization. Two questions arise, however, so soon as we press the problem of objective status of such hypothetical predications.

First, we have to ask: does the psychic movement postulate more than the reality already given in the datum? In reply to this it seems to be true that it does not. The real subject in any judgment of value is only that which just by being constituted as possibly real, already exists for action. So far as determined for action — that is, pragmatically — such judgments are true. The real predicates involved in the ideal and normative, no less than in the logically hypothetical states of mind, are constructed from the data of existence then and there at hand. So far we may go with a thoroughly pragmatic psychology.

The will to believe, for example, is effective, or enters into the determination of reality, only in so far as the belief postulates the result as already actually accomplished: the existence of the thing believed enters into the psychosis and determines the act of will. From the psychic point of view the will to believe is as much determined by real existence as is any other sort of intentional act (*e. g.*, that of moving my head to escape a blow); for psychically the reality or existence is taken for true in the act of belief. It is only to the onlooker that some new mode of existence is determined by the action in accordance with the belief. And even that is, to the onlooker, a mode of real determination only after it has happened. From neither point of view does it involve the postulation of unrealized reality.

How is it then that we, in real life, assume such a sphere of the undiscovered? Is not all ignorance failure to know what there is to know? True: but so far as this is reality at all, it is known reality in its warp and woof. The mental attitude here is essentially the same as that by which we depend upon the continued existence of what we have once known. Both are re-readings of the established order under familiar categories, and both are — when all is said in individual and in social psychology — through and through pragmatic; that is, both, determined by necessities and utilities of real life, are to the same extent that the originally acquired items of reality were.

But, all is not yet said. If we ask, second, for the relation between action and thought in the determination of the joint function which issues in reality, we find a consistent and attractive doctrine, as has already been said: thought — cognitive product in general — is a reflection of habits of action, an organization for future safe action and practical handling of experience. It is the static term in an active process. So far as static, its reference is trans-subjective; it *is reality*. So far as a mental organization of habits, it is dynamic, a 'meaning,' a 'plan of action,' a 'purpose,' an 'instrument,' an 'end' — to use current designations of different writers, all of whom aim broadly at expressing about the same thing.[1] The thought term is the term which is hypostatized as real for the self and for others. It stands as valid in experience until more effective handling of experience issues in a modified thought.

On this view the psychological and biological utility of thought or cognition in general *resides precisely in its static quality*. It is a definition, a specification, a determination. If it is a 'purpose,' it is a defined, usable, purpose; if it is an end, it is a presented, communicable, end; if it is a meaning, it is because it fits into a context of available meanings; if it is a 'plan of action,' it is as much a 'plan' as it is an 'action.' In minimizing the static aspect and reference of thought, pragmatic writer are depriving it of just those features by which its usable and effective character is to be established in mental development. Where would language be, and with it all the socially derived determinations of action, without this assumption. I am sure there will be a reaction on this point from the extreme view whose only justification is novelty.[2]

If this be allowed, we find it necessary to ask for each

[1] My own formulation, arrived at from a detailed exploration of the factors, individual, social, and other and stated from the objective point of view is: 'what we do is a function of what we think, and what we shall think is a function of what we have done' (*Social Interpretations*, pp. 106, 301).

[2] An extreme view which, in its emphasis on action, practically issues in a dualism of thought and action, is that of Mr. Schiller (Humanism, and 'Axioms as Postulates' in *Personal Idealism*). The 'Instrumental Logic' of the Chicago writers avoids this extreme (who for this reason, as I am informed by one of them, refuse to use the term pragmatism for their point of view); but it is still open to the limitation developed in the text.

thought determination not only what sort of action is served, what dynamic pragmatic meaning it has, but also what static, theoretical system of realities it finds its place in. Every true thought is true not only because it has active determinants but also because it reflects all the meaning for life which those active determinants have. So, to reverse the proposition, we may say that every successful active adaptation or line of conduct must, in its development, reflect itself in cognitions or thoughts which are the reflection of all its meaning. It thus becomes definite, socially available, and more than concrete. The categories of general, social, and communicable thought are the normal vehicle and embodiment of the utilities which are turned to account in development.

From this we see that a pragmatism which denies or discounts the validity of the logical point of view truncates its own assumed psychological process and becomes helpless. Thought becomes a by-product, a second-hand way of reaching reality, which is 'suspect' to those other and more valid intuitions given in feeling and action. And this becomes glaringly evident when such a view blows itself up into the dimensions of a philosophy. A philosophy is an attempt to think things: to reach a general and communicable theory of reality. Its characteristic feature is just its generality as opposed to concrete practical enterprises which, no matter how valuable, are still inarticulate. So the mere assertion that in its origin thought is a mode of action, and its revelations are possible because of its origin as serving the utilities of real life — even so much of a general statement as this is itself a logical reinterpretation of the bare reactions which it claims to interpret and generalize. To prove its own truth, indeed, pragmatism can not be content with its own formulation: for such a principle in action must itself issue in a theoretical or logical system.

We might indeed stop here; having a dualism of pragmatic and logical explanations, the logical being the naïve system of thought reflecting the adaptations of which pragmatism takes note. But another alternative would be to reach an interpretation which should reconcile the two essential phases of the action-thought process, and itself issue in the solution of the contrasts in our experience.

There is still lacking, I think, an attempt of the last-named sort; but in Dr. Peirce's and Professor James' formula for pragmatism, we find something of an attempt at the first named — an attempt to state logical meaning consistently with pragmatic origin. James' formula is: "The whole meaning of a conception expresses itself in practical consequences, consequences either in the shape of conduct to be recommended or in that of experience to be expected if the conception be true."[1] This would seem to be — disclaiming, however, the attribution of what follows to Professor James — a formula of the sort of logical systematization of meanings in which pragmatically determined thinking would be reflected. The meanings intended and accepted would all the while be subject to the selective, corrective, substitutive and other revisory processes of practical life, and the realities reflected in such a system of truths would be the stable system of meanings thus produced. The limit of the meaning to be preserved in any case would be, I suppose, its lack of inhibitory or 'interfering' effects, its negative fitness as not producing confusion, 'either in action to be recommended or in experience to be expected'; and the tests, final and conclusive, of logical meaning, would be such concrete practical fitness. This seems to me to be quite consistent, and, for

[1] In the writer's *Dictionary of Philosophy*, 'Pragmatism' (there also Peirce's formulation and criticism). I know of no other attempt to formulate a constructive principle of logical meaning on the pragmatic basis. Professor Dewey aims, it would seem, at justifying the logical processes from the point of view of genesis, rather than at treating of the structure or morphology of thought. And yet by defining thought explicitly in instrumental terms (*Studies in Logic*, 1 ff, 40, 76 ff, etc.) he really excludes the so-called analytic and deductive operations. James' formula would also seem to be limited to those 'conceptions' which *no* have some sort of practical consequences—a matter returned to below. Cf. James' address, *Philosophical Conceptions and Practical Results.*

A concise statement of the requirements of deductive and ratiocinative thinking, from the pragmatic point of view, is to be found in Professor Angell's *Relations of Psychol. to Philos.* (Univ. of Chicago Publications), p. 11. I do not deny his contention that experience is a "universe or system, in which truth is ultimately synonymous with the effective"; but this can not be used to deny the competency of the logical point of view within the system and the need of imminent logical criteria. The whole tendency of this way of thinking is to deny the validity of 'cross-section' or morphological principles, in favor of 'longitudinal section' or genetic principles—to use a figure whose meaning is enforced by the writers now under discussion. (Cf. Dewey, *loc. cit.*, p. 17.)

the sort of truths it really explains, to explain them! It is 'radical empiricism' in both its members, pragmatism of origin and pragmatism of meaning. The tests of utility are simply converted into criteria of logical meaning and value.

The questions, however, which it excites are those involved in certain of the idealist's most pregnant positions: those which assert the essentially teleological and universal character of thought. The criticisms[1] brought from such a point of view deserve more adequate notice and refutation than that which calls all universal and analytic judgments 'dead,' and 'no judgment at all.'[2] Not to account for such judgments is to fail to account for all deductive, mathematical, and subsumptive reasoning — or, indeed, to call it logically abortive and tautological! Instrumental or pragmatic logic must take up this problem with all its resources: the problem of the structural principles of thought, which are not in any evident way in their origin connected with experience at all. The theory of variations, with natural selection, bravely stated in James' 'back-door process' chapter, and repeated more feebly in Schiller's 'axioms as postulates,' goes a long way from the objective biological point of view; but that completely deserts the processes of knowledge, throws the epistemologist back upon native principles implicit in concrete thinking, and so sets a direct limit upon

[1] See Bosanquet's concise statement of points in his criticism of the present writer's theory of selective thinking (a theory directly in the line of the position required by a pragmatic logic), in the PSYCHOLOGICAL REVIEW, July, 1903, already referred to.

[2] Miss Thompson, in Dewey's *Studies in Logic*, p. 108. It is only on such a view that inference can be made a wider term than judgment (*ibid.*, p. 117). 'A judgment is an inference which is conscious of its ground' to this view; but if inference be a process of analysis or composition in higher reflection, involving a setting together of the elements of a thought-situation under certain rules of logical grounding, then it must be separately accounted for and not smuggled in as unconscious and prelogical. Inference is, or may be, preliminary to judgment just in so far as a logical thought-system is presupposed. Many judgments are thus determined. But to deny that they are judgments (as these writers do) makes it necessary to deny also that there is anything in inference not already in the preliminaries of the act of judgment. The real question is: can a genetic process of determination that of psychological conditioning found in the unreflective stage of mental development, be substituted for the logical determination, that of inference, found in the reflective stage?

pragmatism as a working theory even in the individual. We have — that is, he has — to work out individual pragmatism and then, to explain its limitations, assert pragmatism in a racial sense from an objective point of view; to explain the environment he must, as we saw above, take a subjective monistic point of view, which again subverts the pragmatic theory of reality with which he started out, by involving him in the genetic fallacy.

It is quite possible that a more or less successful defense against these criticisms is to be found in the line of a theory maintaining the social constitution of knowledge, with social embodiment and social transmission. I myself have found it necessary to hold that a strain of universality and generality is imparted to knowledge in the aspect which constitutes it 'public' to a social group. The utility upon which pragmatism may insist, therefore, as socially practical and concrete, may be, from the individual point of view, general and universal. A social practical value might be reflected in an individual's theoretical value. The psychological point of view might then be conserved in the continuity of social thinking, although lost in the biological constitution of the individual. Thus, as I have intimated elsewhere,[1] there might be a social derivation of the categories. I have great faith in future work along this line. But withal the limitation remains that such a theory would give a logic of a stage of cognitive process — that at which pragmatic tests are transferred to the social group — rather than a philosophy of the entire movement of reality. We should then have the formula that the individual's and society's common system

[1] *Social Interpretations*, Chap. III., where it is shown that the judgment of the individual, though privately competent, is the reflex, through organic and natural selection, of social life. Such a theory would repay working out in all the departments where the individual's norms of value seem independent of all experience — in ethics and æsthetics, as well as in logic. It promises a theory of the origin of the categories which would go far to reconcile the claims of *a priorism* (making it individual) and *empiricism* (making it social). It is, indeed, in this field that the battle has been joined by utilitarianism and hedonism in their substitution of 'general' happiness and utility for individual. It is not sufficiently understood that these schools have already worked the pragmatic hypothesis in their fields in ways which should instruct the novitiate in pragmatism in logic. Mr. Spencer's attempt at a physiological theory of the *a priori* should also be remembered with respect.

of logical meanings would be tested by private consent and social consequences jointly.[1]

It would remain, therefore, to take up the other alternative mentioned above and try to realize an actual reconciliation of the pragmatic and logical points of view in a synthesis in which they are equally essential members. This can not be entered upon here and now; it is the same need that we found under the earlier head where it was a question of finally escaping the dualism of inner and outer, with its necessary implication of an extra-mental order of realities. I think, however, that it is possible, as intimated at the end of this paper, and that it will take full account of the social aspects of logical determination.

IV.

It remains to take up the third of our general questions: that of the exhaustiveness, as measure of reality, of the modes of apprehension based upon empirical marks and coefficients. Are there modes or types of reality reached in experience for which there practical criteria are not sufficient?

It has been intimated, under the last heading, that a genetic account of the rise of all the modes of thinking — general, universal, normative, no less than particular and concrete — may be worked out successfully from the pragmatic standpoint; it must be done if evolution is to be a general theory.[2] But the

[1] A state of things analogous to the establishing of truth as to the external world by joint tests of the individual's habit *and* the external environment, as we saw above.

[2] The line of least resistance to the writer, worked out in earlier publications, leads to the view that the general aspects of our apprehension are supplied by our general habits of treating things and hence are motor in their seat; motor habit is thus a means of reducing and grouping the embarrassing details of sensory stimulation. M. Havard (*Revue de Métaph. et de Morale*, 1896, pp. 670 ff.) discusses this view as *le nouveau nominalisme;* and if no further logical account than this can be given of 'generals' his description and criticism would seem to hold. This is one of the points on which I do not find myself able to follow Professor Angell in his identification of logic with functional psychology — though disposed, indeed, to claim as much of 'the earth' as possible for psychology (Angell, *The Relations of Psychology to Philosophy*, University of Chicago publications). The authors of *Studies in Logic* seem to adopt the view (see pp. 113, 176, 198; and Moore, *Existence, Meaning, and Reality*, p. 24, quoting *Mental Development*, pp. 323 ff.) that such a psychophysical account of the rise of the general meaning attaching to concepts is sufficient without more ado as a logic of 'general' validity.

quite different question arises as to the meaning of certain of these modes of construing reality, together with the tests or criteria for their valid and successful application. The 'general' concept, for example, pretends to be valid as a vehicle of real apprehension of the world; but it would be impossible to make a conclusive test even of the workableness of such a concept by an appeal to a concrete or practical instance. In the absence of further standards it would be impossible to separate concrete consequences or other pragmatic marks from general consequences. On the contrary, it is just the meaning of a 'general' mode of thought that it stands for the particular case in the sense of organizing it with other experiences. It introduces organization, relationship, and systematization into experience just to the dropping off of those aspects which are individual. This is the sort of reality which such a concept claims to reflect; and its claim could be tested only by some principle which could span the system in which the particular case in question is organized. No appeal to a concrete situation can validate an aspect of reality which is *ipso facto* a systematization of various such situations or cases.[1] There must be, therefore, if such thinking is to have any control or positive validation, certain principles of organization of logically apprehended reality as such. This would throw us back upon the traditional 'laws of thought,' I suppose, or some analogous self-applying criteria of sound thinking.

This may be a way of saying, with many modern logicians, that only particular, not universal, judgments carry the affirmation of reality; if we limit ourselves to pragmatic tests, available only in concrete experience, I see no way of avoiding such a view. But such a position, it seems to me, allies pragmatism to extreme nominalism, and it stands or falls with that as logical

[1] Though from a logical point of view it might invalidate it. An application of this is at hand in the genetic account of the development of self and the dualism of self and not-self. These concepts are essentially general, and no single act of a single self, no matter what its consequences, could validate them as modes of reality, though by requiring a new psychological reconstruction of the material their logical meaning might be invalidated. It is interesting to note that the word 'general' does not occur in the rather full Index to the Chicago *Studies in Logic*.

doctrine. Moreover, it is a pragmatic psychology which most of all feels the need of some justification for universal truths and judgments, for their 'utility' is in some way to be reflected into reality, if reality is to be *no more nor less than the system of judged truths.* The difficulty arises, indeed, only when pragmatism aims to be a universal logic, and so essays the impossible.[1]

The case seems stronger still for the so-called normative or ideal aspects of experience. In their origin these are functions of the progressive organization of experience under pragmatic rules: they illustrate the 'prospective reference' of thought to the unfulfilled career and possibilities of reality. This we may concede and defend. But we do not find — it is a contradiction to conceive of finding — test cases, practical situations, which exhaust the meaning or establish the validity of these modes of reference *in futuro atque in eternitate.* How can we estimate the practical consequences of ideal virtue, whereby there would issue forth the 'highest good'![1] How can practical life adequately test the validity of modes which essentially claim to transcend the experiences of real life? If the normative modes of apprehension or thought are of pragmatic origin, then it is just the pragmatist himself who must give them validity as interpreters of real aspects of things and events; and he is the

[1] As to the position itself regarding universals, I think, it survives simply because nominalistic logic has not yet fully yielded to pragmatic psychology (which really requires instrumental and real logic). Psychologically the universal judgment refers to reality exactly as the particular does, *i. e.*, by the reference to the sort of real universe in which both judgments are made. It is said that particular judgments are experiential; but then universal judgments are never made except as holding true in some experience. Certainly pragmatism can not distinguish universal judgments from particular by that mark. The reader may see in Professor Royce's treatment of the two sorts of judgment with reference to their implication of reality ('outer meaning' of ideas) an attempt to make the real reference of universal judgments to reality negative only (*World and Individual,* I., Sect. VII.). This is a logical way of saying that a particular case may invalidate a general (which must be expressed in a universal judgment) but may not suffice to validate it.

[2] As intimated above utilitarian and hedonistic theories in ethics do attempt something of this sort; but they depart from their pragmatic formula in so far as they seek a logical justification of their conception of the highest good rather than an actual concrete experience, or situation, which would realize it. In either case the individual has no practical test of conduct which is adequate to establish its general or universal utility.

last person to be able, when the practical criteria break down, to throw over these categories and resort to subjectivism or pure nominalism. They have utility, as he says, as ways of interpreting experience; but they issue just by this interpretation in modes of reality. The outcome is that these modes of thought must carry in their exercise their own means of validating their claim to organize experience essentially beyond its actual realization.

An interesting turn may be given to this point by asking for the interpretation of the normative in logical values as such: what is ideal or perfect thinking, and what makes it ideal? Evidently it must be flawless thinking, as tested by adequate rules and criteria. The pragmatist would have to say that all thinking which fulfils the demand that it deal successfully with the concrete situation which stimulates it is in so far flawless: that is, that all thinking is flawless which does not issue in practical embarrassment and confusion. It can not say, in the particular case, that thought might have done better; for — to point again a foregoing criticism — there is for this view no way to test relative or alternative solutions: such a test would involve the application of general criteria of validity for which this view makes no provision. So soon, however, as we do admit, in the body of the logical processes themselves, certain criteria of the valid organization of thoughts, we then have standards whereby to determine a greater or less validity and conclusiveness with reference to an ideal of flawlessness in the logical process. As a matter of fact, we actually find many grades and modes of inferential process — the disjunctive, the hypothetical, the categorical — having varying degrees of psychological determinateness and of logical conclusiveness; and there are also various groundings of proof, as in universal or particular judgments, which actually do issue in varying degrees of logical cogency and validity. This is in so far confirmation of the presence of regulative or normative principles in the logical process, as such, *having this logical value no matter what their origin.*

It follows from these criticisms that in our final interpretation of reality as, in any sense general, universal, or normative pragmatism does not take us far. It omits all such modes of logical reality, although by its pragmatic account of their origin for utilities of life, it gives them some presumptive value. It must deny this presumption or — refuse to be a logic or philosophy! This latter is the only sensible course, if we are to remain pragmatists in our genetic psychology.[1]

V.

The requirements of a philosophy of reality based upon — or at least not invalidating — the results of mental development under the law of utility, are fairly plain; at least, in the negative sense of not incurring the criticisms urged in this discussion. They come out with some clearness in connection with the statement of the problem of error.

The problem of error as involved in that of truth has been a theme in many of the discussions in pragmatism. The possibility and meaning of error are somewhat more clear on the theory which holds that knowledge is a copy, an inadequate or defective copy, of a separate system of realities. The problem to this theory is not to account for the presence of error, but to reconcile it with the validity of thought. Otherwise all guarantee and reliability fail in respect to truth. If thought may report reality erroneously, how are we to know that it ever reports it truthfully? The positive constructive task therefore is to eliminate error, or to make it an incident only is a generally valid process. The limit and ideal of the apprehension of the

[1] This is explicitly the course taken lately by one of the fathers of pragmatism, Dr. Peirce; and the considerations he sketches in the *Dictionary* article already referred to are similar to those worked out here. He suggests a philosophy called 'Synechism' (see his art. of that title also in the *Dictionary*), in which he makes 'reasonableness' its own justification, and attempts to do justice to logical 'generals.'

The failure to explain these aspects of reality with constructive thoroughness stands out in such writings as Moore's detailed criticism of Royce already cited. Royce makes a constructive effort to overcome the dualism of thought and action. He reaches an absolute system of thought which may also be looked upon as an absolute purpose systematizing and completing finite purposes. Moore claims that this is no real reconciliation of the representative (logical) and practical aspects of the idea; but Moore, in his turn, goes no further than the re-assertion of the concrete action-thought process as genetic function.

real is an errorless experience: a system of logical values in which reality is completely and finally revealed; this is the ideal of intellectualist or logical theories as opposed to pragmatic theories.

To the pragmatist, on the contrary, error presents a different problem. It must arise by some variation or interference in the process whereby truth, and with it reality, are normally constituted. As a fact there is no difficulty, for the method of discovery, called in science that of 'trial and error,' is just that which is taken over and made the typical method of mental development. It is a matter of 'cases,' trials, efforts, only the *ex post facto* inspection of which reveals some — those which are unfruitful or embarrassing — as errors. Error then, to the pragmatist, is a normal aspect of the process of the discovery of truth.

To this theory also, the limit of the process of apprehending reality would appear to be the elimination of error. The development processes, at their limit, would issue in an errorless system of cognitions and judgments — logical processes — which would be the reflection into thought of a perfectly adapted and satisfied conation. Such system would be, within its own point of view, closed and self-maintaining. If pragmatism asks at all[1] for the meaning and limit of its process, this would be about the answer: a state of equilibrium, or an errorless experience.

The problem set to both theories, intellectualism and pragmatism alike, therefore, is the elimination of error, in the treatment of reality whether by thought or by action.

But to depict an errorless experience is to depict one in which the dualism of experience and reality is overcome. *Error is essentially a phenomenon of dualism.*

By no fair definition can error be attributed to a thought-system which is self-sustaining and has no reference to what is outside itself. But errorlessness is also the resolution of the active processes of adjustment to which the thought aspect of ideas is ancillary. Embarrassment, conflict and hesitation, restlessness, are the motor or practical results of cognitive error.

[1] Perhaps it doesn't! — see Moore, *Existence, Meaning, and Reality*, p. 25.

The problem therefore of philosophy—as it arises from the discussion of the demands of pragmatism—*is no longer that of the reconciliation of two logical categories*, being and becoming, identity and diversity, teleology and mechanism, thought and reality; it is, on the contrary, *that of the reconciliation of two opposed schemes of evaluation of experience in general*, that of logical systematization and that of practical manipulation, each, by the very terms of their relation claiming to be valid. It is possible that there may be no reconciliation; but the consistent development of personality as a whole by the activities in which these two evaluations arise, would lead us to expect that they reveal modes rather than diverse kinds of reality, and that there is some possible experience which, while enriched by this contrast, is not torn asunder by it.

Such an experience would be a deeper revelation of the nature of all the real than is either of the partial modes, and it would, at the same time, admit of the criteria by which each of the alternative points of view establishes the claim it makes.[1]

It would seem that this sort of requirement should commend itself to the Chicago thinkers who refuse to stop in the dualism of thought and action. Professor Dewey says (*loc. cit.*, pp. 80 f.): "Both material and tool [matter and thought] have been secured and determined with reference to * * * the maintenance of a harmonious experience * * * life proposes to maintain at all hazards the unity of its own process. Experience insists on * * * securing integrity even through and by means of conflict." Certainly then the final unity is one to be experienced or lived in, an experience which is the immediate unity of an autotelic whole; a whole which includes logical realities whose ends are practical and also practical realities whose ends are logical. This is what Moore's criticism of Royce points out,

[1] This, I take it, is essentially the reconciliation which Professor Royce attempts, though without, I think, the clear apprehension that it requires a category of experience not definable ultimately as either ideas or 'purposes' (conations), nor yet by saying that it is both. Professor Royce's recent work is however a notable advance upon a certain dualism of value and fact to be found in many recent writings (and also in his earlier papers; cf. his art. in *International Journal of Ethics*, July, 1895, and the present writer's examination of it in *Fragments in Philosophy and Science*, V., reprinted from the same journal, October, 1895).

I think (*cf.* his utterance as to dualism on p. 372, with his quite sentimental conclusion on p. 382, *Studies in Logic*). And in the matter of its treatment of dualism this view is not inconsistent with those of Mead (*The Definition of the Psychical*). Even James' penchant for pluralism is to Dewey a case of æsthetic unity in the thinker's contemplation!

In such an issue, reached from the previous criticism, I find one of the approaches to a type of philosophy to which other considerations, developed in various recent studies, have also pointed.[1] These 'approaches' converge upon a position which finds in æsthetic experience, at each grade in the development of the dualism of fact and values, truth and practice, inner and outer, just the union and reconciliation of the two sets of claims.[2]

Our conclusions may be summed up as follows:

1. Having successfully depicted the genetic processes by which consciousness reaches the dualism of the thinking principle and reality, it is the 'genetic fallacy' to treat one term of this dualism, the thinking principle, as valid in the sense it claims to be, and to deny that the other is.

2. If either of the terms of this dualism is to be made primary as a philosophical principle, it would seem to be the logical reality term; since it is genetically, at each stage of mental development, just the definite, general, and communicable term in which pragmatic gains are reflected. The pragmatic account of thought fully justifies its function of having general meaning as well as concrete. Pragmatism can not complete itself until it issues in a logical account of reality.

3. The universal and normative modes of thought do not get adequate logical justification in a theory which finds the ts and criteria of reality solely in concrete experiences of usefulness, workableness, etc. It is just the general and uni-

[1] See especially the article 'Mind and Body' in the PSYCHOLOGICAL REVIEW, May, 1903.

[2] It may be said (*cf.* Urban, PSYCHOLOGICAL REVIEW, January, 1896) that the self is the source of union of the two contrasting modes of experience; and that is of course true. But to use the *thought of self* is to resort to one of the categories in question, which involves the genetic fallacy of pragmatism; and to point out an experience in which the self finds its attitudes and values free from the dualism is just the question at issue.

versal aspects of such modes of thought whose meaning would not appear in any set of practical consequences. General tests of systematization or organization as such within the body of logical data would alone accomplish this. This throws us back upon such principles as consistency, contradiction, etc. — yet without prejudice to a thorough-going pragmatic account of the origin of the function of thinking.

4. The final demand is for a real reconciliation of the dualism of logical truth and experienced value; both making claim to interpret reality. This reconciliation must not deny the claim of logic wherever the material is logical, not that of value wherever a valuation is made; and no solution is possible except as itself an experience in which the dualism is actually outlived. Any other solution would be hypothetical only, and derive its support from one or other of the two modes of the dualism which is to be explained.

5. The thoroughgoing application of the genetic method, as illustrated in the foregoing point (4), requires that no member of a genetic dualism, or other contrast, be taken as explaining principle of the process in which that dualism or contrast arises. This is held to introduce a new philosophical point of view: that of finding the further genetic process by which the dualism is itself overcome, and of interpreting the nature of the reality which is then constituted.[1]

[1] This has been insisted upon, as necessary in science generally, and formulated in the theory of 'Genetic Modes,' in the work *Development and Evolution*, Chap. XIX.

DISCUSSION.

THE SEXUAL ELEMENT IN SENSIBILITY.

Any theory of socialization must give first-rate importance to the influence on the individual of the presence, behavior and opinion of others; and the quality of suggestibility to social influence, so important in the formation of the character of the individual and in the formation of society itself, seems to have two sources, one in the food process and the other in the process of reproduction.

The life of any highly organized species depends on the quickness, precision and adequacy of its reaction to stimuli. New and dangerous or advantageous situations are constantly presenting themselves and the species develops both the cognition and the emotional reactions suitable to accommodate to these. Every such species has, in consequence, a high degree of susceptibility. Perhaps the most remarkable expression of susceptibility in the human species is seen in the sensitiveness of man to the opinion in which he is held by others. Social life in every stage of society is characterized by an eagerness to make a striking effect. A bare reference to the ethnological facts in this connection will suffice: The Kite Indians have a society of young men so brave and so ostentatious of their bravery that they will not fight from cover nor turn aside to avoid running into an ambuscade or a hole in the ice. The African has the privilege of cutting a gash six inches long in his thigh for every man he has killed. The Melanesian who is planning revenge sets up a stick or stone where it can be seen; he refuses to eat, and stays away from the dance; he sits silent in the council and answers questions by whistling, and by other signs draws attention to himself and has it understood that he is a brave and dangerous man, and that he is biding his time.

This bidding for the good opinion of others has plainly a connection with food-getting, and with the conflict side of life. High courage is praised and valued by society, and a man of courage is less imposed on by others and comes in for substantial recognition and the favor of women. It is thus of advantage to act in such a way as to get public approval and some degree of appreciation; and a degree of sensibility on the score of the opinion of others, or at least a reckoning upon this, is involved in the process of personal adjustment.

But the problem of personal adjustment at this point would seem to call for more of intelligence than emotion; and we find, on the contrary, an excess of sensibility and a mania for being well thought of hardly to be explained as originating in the exigencies of tribal organization, nor yet on the score of its service to the individual in getting his food and living out his life. Why could not primitive man live in society, be of the war-parties, plan ambuscades, develop his fighting technique and gear, be a blood-brother to another man, show his trophies, set a high value on his personality and insist on recognition and respect, without this almost pathological dependence on the praise and blame of others?

Or if we approach the question from another standpoint and inspect our states of consciousness, we find signs that we have a greater fund of sensibility than is justified in immediate activity. We have the same mania to be well thought of; we are unduly interested when we hear that others have been talking about us, we are annoyed, even furious, at a slight criticism, and are childishly delighted by a compliment (without regard to our deserts); and children and adults alike understand how to put themselves forward and get notice, and equally well how to get notice by withdrawing themselves and staying away or out of a game. We have a tendency to show off which is not apparently genetically connected with exploit or organization, and we recognize that this form of vanity is not consistent with the ordinary run of our activities when we argue with ourselves that the opinion of this or that person is of no consequence and attempt to think ourselves into a state of indifference. Intellectually and deliberately our attitude toward criticism from others would often be, if we could choose, represented by Tweed's query, 'What are you going to do about it?' but actually it puts us to bed.

All of this seems to indicate that there is an element in sensibility not accounted for on the exploit or food side, and this element is, I believe, genetically connected with sexual life. Unlike the struggle for existence in the ordinary sense of the phrase, the courtship of the sexes presents a situation in which an appeal is made for the favor of another personality, and the success of this appeal has a survival value — not for the individual, but for the species through the individual. We have, in fact, a situation in which the good opinion of another is vitally important. On this account the means of attracting and interesting others are definitely and bountifully developed among all the higher species of animals. Voice, plumage, color, odor and movement are powerful excitants in wooing and aids both to the conquest of the female and the attraction of the male. In this connection we must also recognize the fact that reproductive life must be connected with violent stimulation, or it would be neglected and the species would become extinct; and, on the other hand, if the conquest of the female were too easy, sexual life would be in danger of becoming a play interest and a dissipation, destructive of energy and fatal to the species. Working, we may assume, by a process of selection and survival, nature has both secured and safeguarded reproduction. The female will not submit to seizure except in a high state of nervous excitation (as is seen especially well in the wooing of birds), while the male must conduct himself in such a way as to manipulate the female; and, as the more active agent, he develops a marvelous display of technique for this purpose. This is offset by the coyness and coquetry of the female, by which she equally attracts and fascinates the male and practices upon him to induce a corresponding state of nervous excitation.[1] This is the only situation in the life of the lower animals, at any rate, where the choice of another is vitally important; and corresponding with the elaborate technique to secure this choice we have in wooing pleasure-pain reactions of a violent character. In a word, extreme sensitiveness to the judgment of another answers on the subjective side to technique for the conquest of a member of the opposite sex. It seems, therefore, that we are justified in concluding that our vanity and susceptibility have their origin largely in sexual life, and that, in particular, our susceptibility to the opinion of others and our dependence on their good will are genetically referable to sexual life.

This view would be completely substantiated if we could show that the qualities of vanity and susceptibility in question are present in any species where it is impossible to assume that they were developed in connection with the struggle for food and as the result of the survival of types showing a tendency to combine and coöperate in the effort to get food. And we do, in fact, have cases of this kind among some of the lower animals. It cannot be said that the dog, for instance, has survived in the struggle for existence because of his sensitiveness to public opinion in his species nor on account of an interest in being well thought of by the community of dogs at large which would lead him to behave in a public-spirited or moral manner. At the same time, the dog in his relation to man shows as keen a sensitiveness to man's opinion and treatment as does man himself. The attention which the master pays to one dog will almost break the heart of a dog not receiving it. A neglected dog plainly suffers as much in

[1] See Groos, *The Play of Animals*, p. 283.

his way as the soldier who is sent to Coventry by his messmates; and if neglected and jealous dogs do not commit suicide, as they are reported to do, they are evidently in a state of mind to do so. This means that the dog has highly developed susceptibility to the appreciation of others, and that the species which he represents has had no history except a sexual history capable of developing this mental attitude. In connection with courtship he developed a fund of organic susceptibility, and this condition is involved in his more general relation to man: the machinery set up in sexual relations is played on by stimuli in general. A condition favorable to stimuli of a particular kind is favorable to stimuli in general; and it seems likely that this not very prominent fact of a state of excitation in a sexual connection is an important factor in the formation of the mind and of society.

There are also certain conditions in the development of the individual and of society where the sexual type of reaction is so near the surface that it shows through in connection with political, moral and other essentially non-sexual activities. Passing over the fact that the period of adolescence is noticeably a period of 'susceptibility' and personal vanity, we may take as an example of the intrusion or persistence of the sexual element in conditions of a non-sexual kind the frequent association of sexual with religious excitement.[1] The appeal made during a religious revival to an unconverted person has psychologically some resemblance to the attempt of the male to overcome the hesitancy of the female. In each case the will has to be set aside, and strong suggestive means are used; and in both cases the appeal is not of the conflict type, but of an intimate, sympathetic and pleading kind. In the effort to make a moral adjustment it consequently turns out that a technique is used which was derived originally from sexual life, and the use, so to speak, of the sexual machinery for a moral adjustment involves, in some cases, the carrying over into the general process of some sexual manifestations. The emotional forms used and the emotional states aroused are not entirely stripped of their sexual content. On the race side, also, there is a stage in development where the sexual pattern is transferred almost unmodified to public affairs. The following extracts from a lengthy description given by Mr. Bowdich of his reception by the king of Ashanti, in the year 1817, will illustrate sufficiently the employment of the turkey-cock pattern of activity in political relations:

[1] See *e. g.*, Krafft-Ebing, *Psychopathia Sexualis*, 3 Aufl., p. 10; Adams, 'Some Phases of Sexual Morality and Church Discipline in Colonial New England,' *Proceedings of the Mass. Hist. Soc.*, 2d Series, 1891, pp. 417–516.

"The sun was reflected with a glare scarcely more supportable than the heat from massive gold ornaments which glistened in every direction. More then a hundred bands burst at once on our arrival, with the peculiar airs of their several chiefs; the horns flourished their defiances, with the beating of innumerable drums and metal instruments, and then yielded for a while to the soft breathings of their long flutes. * * * At least a hundred large umbrellas or canopies, which could shelter thirty persons, were sprung up and down by the bearers with brilliant effect, being made of scarlet, yellow, and the most showy cloths and silks, and crowned on the top with crescents, pelicans, elephants, barrels, and arms and swords of gold. * * * The caboceers, as did their superior officers and attendants, wore Ashanti cloths of extravagant price, from the costly foreign silks which had been unravelled to weave them in all the varieties of color as well as pattern; they were of incredible size and weight, and thrown over the shoulder exactly like the Roman toga; a small silk fillet generally encircled their temples, and many gold necklaces, intricately wrought, suspended Moorish charms, dearly purchased, and enclosed in small square cases of gold, silver and curious embroidery. Some wore necklaces reaching to the waist, entirely of aggry beads; a band of gold and beads encircled the knee, from which several strings of the same depended; small circlets of gold, like guineas, rings and casts of animals were strung round their ankles; their sandals were of green, red, and delicate white leather; manillas, and rude lumps of rock gold hung from their left wrists, which were so heavily laden as to be supported on the head of one of their handsomest boys. * * * [The king] wore a fillet of aggry beads round his temples, a necklace of gold cockspur shells strung by their larger ends, and over his right shoulder a red silk cord, suspending three sapphires cased in gold; his bracelets were of the richest mixtures of beads and gold, and his fingers covered with rings; his cloth was of a dark green silk, a pointed diadem was elegantly painted in white on his forehead; also a pattern resembling an epaulette on each shoulder, and an ornament like a full blown rose, one leaf rising above another until it covered his whole breast. * * * The belts of the guards behind his chair were cased in gold, and covered with small jaw-bones of the same metal; the elephants' tails, waving like a small cloud before him, were spangled with gold, and large plumes of feathers were flourished among them. His eunuch presided over these attendants, wearing only one massive piece of gold about his neck; the royal stool, entirely cased in gold was displayed under a splendid umbrella, with drums, sankos, horns, and various musical instruments, cased in gold, about the thickness of cartridge paper; large circles of gold hung by scarlet cloth from the swords of state; * * * hatchets of the same were intermixed with them; the breasts of the Ochras and various attendants were adorned with large stars, stools, crescents, and gossamer wings of solid gold- * * * ."[1]

It is not surprising that the characteristically sexual method of display and emotional appeal should be associated with the earlier efforts at adjustment, both in the individual and in the state. This method is based on the instincts, and just as inhibition and brain integration follow the instincts in point ofdevelopment, a rational mode of control, individual and public, is developed later than the emotional form, or, at any rate, is not at first independent of it.

The origin of mental impressionability seems to lie then not in one but in the two general regions of activity — that connected with the struggle for food and that connected with reproduction. The strain on the attention in the food and conflict side of life involves the development of mental impressionability, particularly of an impressionability on the side of cognition. But in addition we have the impressionability growing out of sexual life which has been in question above, and which is more closely related to appreciation than to cognition. And of these two aspects of impressionability — the one growing out of conflict and the one growing out of reproduction, the latter has more social possibilities than the former, because it implies a sympathetic rather than an antagonistic organic attitude. It is certainly in virtue of susceptibility to the opinion of others that society works — through public opinion, fashion, tradition, reproof, encouragement, precept and doctrine — to bring the individual under control and make him a member of society; and it is doubtful whether this could have been accomplished if a peculiar attitude of responsiveness to opinion had not arisen in sexual relations, reinforcing the more general and cognitive impressionability. Without this capacity to be influenced the individual would be in the condition of the hardened criminal, and society would be impossible.

This sex-susceptibility which was originally developed as an accessory of reproduction and had no social meaning whatever, has thus, in the struggle of society to obtain a hold on the individual, become a social factor of great importance and together with another product at sexual life — the love of offspring — it is, I suspect, the most immediate source of our sympathetic attitudes in general, and an important

[1] Ellis, *The Tshi-speaking Peoples of the Gold Coast*, p. 249 ff.

force in the development of the ideal, moral and æsthetic sides of life. It is perhaps not pushing the matter too far to suggest also that the duality of motivation which characterizes our social system, and which we designate as *egoism* on the one hand and *altruism* on the other, is a natural result of the contrast in character between the states of consciousness originating in the struggle for food and those originating in courtship, and that the history of society on the moral and æsthetic sides is in great part the history of an attempt to make the more sympathetic attitude prevail over the more antagonistic.

W. I. THOMAS.

THE UNIVERSITY OF CHICAGO.

DR. MORTON PRINCE AND PANPSYCHISM.

In the last number of this REVIEW Dr. Morton Prince points out that the panpsychist doctrine of my *Why the Mind has a Body* was anticipated in his *Nature of Mind and Human Automatism* published in 1885. The interesting quotations which he makes from the latter and from an article in *Brain* for 1891 certainly bear him out in this assertion. With the book I regret to say that I was unacquainted. But I read his article some ten years ago, and I think that it contributed to turn my thoughts in a panpsychist direction. Its clear teaching that consciousness is the reality which appears as the brain-process helped to fix that hypothesis in my mind, and I have no doubt that the pages of Paulsen,[1] to which I have always felt myself mainly indebted, had a fuller meaning to me in consequence. I regret the more that, by the time when I came to write, my memory of its contents had lapsed, and I failed, rather inexcusably it now seems to me, to mention Dr. Prince among earlier expositors of the theory. Had I been acquainted with his book, I should have had a juster appreciation of his merits as a pioneer of panpsychism, and this would not have happened.

I have lately had the pleasure of reading the book, and I find that it contains an extremely clear and forcible statement of the panpsychist hypothesis. It differs from Clifford's essay on the 'Nature of Things-in-Themselves' in being not merely an exposition of panpsychism in the abstract, but a definite application of it to the problem of the connection of mind and body.[2] Readers who have difficulty in

[1] *Einleitung in die Philosophie* (1892), see esp. pp. 77–116, 137–149, 381–85.

[2] Towards the close of his essay on *Body and Mind* Clifford does make a rather definite application of it to the problem, as is shown by the sentence: "If mind is the reality or substance of that which appears to us as brain-action * * *" (*Lectures and Essays*, 2d ed., p. 269).

understanding my account of the matter would do well to consult it. In particular, the pointing out of the ambiguity of the term 'matter,' the rejection of non-empirical views of mind, the definite use of the conception of mental causality, the recognition that brain-events are not merely symbols but *effects* of the 'accompanying' mental states, indicate the clearness with which the conception is grasped in all its implications. That Dr. Prince should have worked this conception out for himself, without knowledge of Clifford, simply (as it appears) by way of criticism of the views of Huxley, Tyndall, Lewes, Spencer, and Bain, is an achievement to be proud of. Should the theory triumph, he will be entitled to an honorable place among its earliest discoverers and defenders.

There is one slight misapprehension in Dr. Prince's paper which I desire to set right, and that is as regards my attitude toward Clifford. Dr. Prince seems to think that I disapprove of Clifford's views, and regard them as somehow distinct from my own. I assure him that this is a mistake. The fact that I describe my theory, in my preface, as "that which is implied in the panpsychism of Fechner and Clifford" should, I think, have made any doubt as to the nature of my feeling impossible. I presume his error is due to my having referred to Clifford as a parallelist; whereas, according to Dr. Prince, parallelism is an erroneous theory, quite incompatible with panpsychism. This is really only a question of terms. Dr. Prince understands by parallelism the assertion of a second real series, running parallel with the psychical; and this, I agree with him, is a wholly erroneous conception. But I think it is not expedient, as a matter of terminology, to pin the parallelist down to the assertion of the independent reality of matter. If the interactionist may remain still an interactionist even though he conceive the matter on which the mind acts idealistically, then the parallelist may still remain a parallelist even though he conceive that the brain-process has no existence except when an external observer chances to perceive it.[1] The essence of parallelism is the denial of causal relations between mental and physical; and this denial, as I have shown in my book (p. 345), remains still valid on the panpsychist theory.

I want to take this opportunity of calling attention to a number of other panpsychist discussions of the relation of mind and body, with some of which I have only recently become acquainted. Professor

[1] German critics of panpsychism have fallen into this error, and are prevented by it from understanding the real meaning of the theory — see Heymans' review of Busse in *Zeitschrift für Psychologie*, Bd. 33, Heft 3, esp. pp. 217–219.

Lloyd Morgan, in his *Animal Life and Intelligence*, published in 1891, has a chapter on *Mental Evolution*, in which a panpsychist theory is sketched out (Ch. XII., pp. 464–503). The late Joseph LeConte hints at a panpsychist theory of the connection of mind and body in the remarks contributed by him to Professor Royce's *Conception of God* (1897; see pp. 67–68). Professor Royce himself, despite his early condemnation of 'mind-stuff' (see *Mind*, *O. S.*, Vol. VI., pp. 365 ff.), appears as a panpsychist in his essay on 'Self-consciousness, Social Consciousness and Nature' (published originally in *Philos. Rev.*, Vol. IV., pp. 465 ff., 577 ff., and reprinted in his *Studies of Good and Evil* — see especially pp. 229, 230 of the latter). Panpsychist principles seem to underlie Mr. Henry Rutgers Marshall's acute discussion of the parallelistic view in his *Instinct and Reason* (1898, pp. 19–67). Professor Walter Smith has criticized the notion of interaction very judiciously from the same point of view in *Philos. Rev.*, Vol. X., pp. 505–514, his little paper containing all the essential ideas of my book.

Professor Stout's chapter on 'Body and Mind' in his *Manual of Psychology* I have referred to in my preface. Professor Ebbinghaus's discussion of the subject will be familiar to readers of his *Grundzüge* (pp. 27–47). Both of course are panpsychist.

No person interested in the question should fail to read Professor Heymans' article 'Zur Parallelismusfrage,' in *Zeitschrift für Psychologie*, Bd. 17, pp. 62–105. In a capital review of Busse's recent book in the same journal (Bd. 33, pp. 216–222), Professor Heymans defends the panpsychist theory against misapprehensions, in a series of brief objections and replies which may be commended to the attention of those who think they see reasons for rejecting it.

C. A. STRONG.

COLUMBIA UNIVERSITY.

EDITORS' NOTE.

The customary sections devoted to 'Literature,' 'New Books,' 'Notes,' etc., are hereafter to be printed separately, appearing on the fifteenth of each month, in form considerably enlarged and comprising certain new features. For convenience of reference it will be known as *The Psychological Bulletin.* It is to constitute a separate volume, although still essentially part of the REVIEW.

Manuscripts for publication, books for review, and editorial matter generally should be addressed hereafter to Prof. J. Mark Baldwin, Johns Hopkins University, Baltimore, Md.; business communications and advertising matter to Prof. H. C. Warren, Princeton, N. J.

N. S. Vol. XI. No. 2 March, 1904

The Psychological Review.

THEORY AND PRACTICE.[1]

BY PRESIDENT WILLIAM LOWE BRYAN,

Indiana University.

I.

Two of my predecessors have discussed from this chair the application of psychological theory to practice. Upon the principal question considered by them, as you know, they did not agree. However, difference of opinion upon this point is less surprising than unanimity would be. For time out of mind there have been not simply many divergent opinions as to the relations of theory and practice, but several types of such opinions persisting side by side century after century in collision. In one case it is believed that there is a philosophy, which gives a finally valid account of all reality and which lays down the law for action in every field. In another case, it is not philosophy, but empirical science which, as it develops, is to free us from all the rules of thumb by which our ancestors groped and fumbled their way, and which is to show us with certainty and on rational grounds exactly what to do in every field. In a third case it is not philosophy and not science, not systematic learning of any sort, but intuition, tact, common sense, which alone enable us to achieve success in any field.

The mention of these typical opinions brings to mind at once many great names which could be cited for and against each of them. I have sometimes tried to make the historic conflict of

[1] President's address, American Psychological Association, St. Louis Meeting, December, 1903.

opinion upon this subject concrete for myself by imagining a committee selected from the great philosophers, scientists, poets, and men of affairs of history, the committee being directed, let us say, to act together as trustees of a village school. Plato, Cervantes, Comte, Prince Bismarck, Thomas Carlyle — whom you please, — it would be easy to make up an interesting committee. The debates of that committee, the hopeless reciprocal misunderstandings, the scorn or compassion of each man for all the others, it would take Shakespeare to imagine. And the scene would be worthy of Shakespeare for in a way, the most fundamental conflicts of the history of culture with all their humor and with all their gravity would be there. But even Shakespeare, I fear, could not imagine what the committee would decide to do. And yet decision as to what to do is the unavoidable task of most of us who profess psychology or indeed any science. For we are obliged to propose courses of study and to advise students who have in view one or another profession what courses they shall take. But what courses we propose and what advice we give depend over and over upon what we believe as to the practical usableness of our science. We are therefore each of us bound in conscience to face the question as well as we can, not as one of those questions which may wait upon the leisure of science, but as an always immediate question to which we can scarcely help giving daily some sort of answer to those who look to us for guidance. We are in the position of the conscientious physician who would like to wait for the instruction of another hundred years of experimental medicine but who must do as well as he can with the patient before him. For better or for worse, therefore, I shall give the conclusions which with time have come to me.

II.

The Failure of Theory.

When a theory will not work, as so often the most promising theory will not, I believe the difficulty lies simply in the fact that the theory is not true — not true, that is, with a sufficient degree of approximation. An action is always necessarily concrete, subject not only to certain known general laws and to certain known definite conditions, but subject to the whole of reality then and there effectively present. No theory completely embraces all the conditions determining any action. Some conditions are omitted unintentionally because of ignorance. Some conditions are excluded intentionally, on the one hand as disturbances which interfere with the accuracy of experimental results, on the other hand as complications which interfere with the possibility of mathematical or logical treatment. The intentional exclusion of disturbing or complicating conditions is not a procedure which requires defense. Its defense is found in the whole history of learning, and after that in the history of the practical applications of learning. To make any progress, we must focus for certain things and be temporarily blind to environing things.

It may be, however, that in arriving at a theoretical result, either because of my ignorance, or because of the very efforts to be exact or to be logical, I shall leave out of account conditions which are not in fact insignificant, which will not be absent when my bit of theory is tried, which will be there to upset all my previsions and to bring me to confusion. My air ship will not fly. In such a case, the best fortune is immediate and decisive practical trial. Decisive failure destroys our illusions, if we have them, and sets us looking for conditions which have been overlooked. Unhappily, however, decisive trial of theoretical results is often indefinitely postponed. In this case, the scholar must be of extraordinary constitution if he escape the historic disease of his kind, namely, blindness to realities which his method has not embraced.

I wish to consider two types of this illusion of the scholar. One of them, which may be called *the illusion of consistency*, is generally recognized. The other, not so generally recognized, I shall call *the illusion of precision*. I wish to show how in both cases these illusions spring directly out of the painstaking employment of methods which must be employed to discover the truth, and how, when they have risen, they render the scholar blind to certain aspects of truth which are not insignificant either in theory or in practice.

The Illusion of Consistency.

I am, let us suppose, a scholar who is impressed above all things with the necessary self-consistency of the truth. Accordingly, I have spent years in developing a system of greater or less extent, which, to my mind, has the quality of complete self-consistency. I have made its consistency explicit, by stating everything in exact logical or perhaps mathematical form. Every term, every proposition or equation, every syllogism or problem is perfectly defined and the whole stands, to my mind, flawless and self-evidential. Everything in it hangs together. Everything in it can be shown to be as certain as the most certain thing in it and that thing no sane man can doubt. Here is the truth, final and clear, and here, within the field concerned, is the law for action.

Whether such a system be finally credited with great value or with small, it is sure to have certain characteristics which limit its value. Its salient merit of exact logical or mathematical consistency was bought at a price. That price was the exclusion of conditions too complicated to be dealt with by the logical or mathematical methods employed. This price was paid by Spinoza in one field and by Newton in another. The procedure requires no defense. It is necessary. There is no definition without negation.

However, a life time spent in developing and contemplating such a system makes it easy to forget and ignore altogether what the method has excluded. Every clear idea, as we know experimentally, makes it harder to do justice to impressions just unlike those which belong with that idea. A system of such ideas is self-protecting somewhat after the analogy of a living organism. Every item in the system is felt to be proof of and proved by all the others. Everything in the system comes to the point of attack, makes me abnormally sensitive for faint experiences of the right sort and abnormally oblivious to salient facts of the wrong sort. In a word, there is perhaps no hypnotic agent more powerful to sharpen the sight or to dull it than a system of ideas which one has made for himself, and whose truth seems guaranteed at every turn by complete internal consistency.

Very likely this hypnotic illusion of consistency is strongest when the system concerned is believed to be all-embracing — a philosophy of God, the world, man, what not; and the illusion is the less likely to be broken because decisive trial is so difficult if not quite impossible. However, it is not simply the philosophers who along with their systems of beliefs, develop the illusion of consistency. No doubt every man does so in a degree and men of science along with the rest. The history of science is full of examples. It is seldom that a scientist is able to do justice to facts which controvert his most important theories. For this reason there is sober truth in the cynical remark that the progress of science requires the death of scientists.

The illusion of consistency as I have said, is very well known, for it springs out of conditions which have been legitimately and conspicuously present throughout the history of learning. And so for centuries this illusion has been notorious as a limitation of the scholar's knowledge and practical judgment.

I turn to an analogous illusion which is less generally recognized.

The Illusion of Precision.

To take a typical case, let us suppose that I am not a logician but an experimental scientist. I cultivate a distrust for philosophy. I am wary of all elaborate argumentation. Logic is a trap. I have studied facts pure and simple. I have lived in the laboratory. I do nothing except with instruments of precision. I have learned how to shut out disturbing conditions with the last degree of refinement. My results are strictly quantitative. Everything has been verified over and over and is verifiable by whom you please *ad libitum.* The outcome is not poetry, not a guess, not a speculation. It is science and within its field, it is the law for action.

It would be idle in this presence to insist upon the value of such procedures and such outcomes. The chief merit of our time lies doubtless in the fact that we have succeeded better along these lines than men ever did before. And yet directly out of the methods which science must employ, there rises over and over again an illusion which stands between the scholar and the truth and which may make him a failure in practice.

Those disturbing conditions which were with infinite pains shut out may be practically insignificant. Or the scientist may take adequate account of them in a separate study. But sometimes they are not insignificant and sometimes after having carefully shut them out of his laboratory the scientist forgets them altogether and does not dream that they are waiting outside his laboratory door ready to take revenge when his formulæ come to trial. Unhappily the necessary practical tests are often long delayed or indecisive. This is true in every field of science and there is no field of science where such delay does not permit the illusion of precision to survive.

But when the phenomena concerned are very complicated, when for example, we confront the complexities of human nature in the individual and in society, when we attack by exact scientific method the problems of psychology, ethics, political economy, or any science dealing with human life and thereupon undertake to tell men what to do, we have then the best possible conditions for the development of the illusion of precision.

For on the one hand it is possible in all these fields to be as precise as one will. There are methods from the older sciences to serve as analogical models. There are, if you like, instruments of the highest precision. One has only to be scrupulous, persistent, intolerant of errors. One will end by securing results, which whatever else may be true of them, are at any rate exact. All this tends to establish in the man who does it a faith which cannot be shaken. There is my machine. There is my mathematical method. There are my statistics. There is my sure concrete fact which no one can deny, which all the world may verify. There is a bit of *science* which will stand till the judgment day and take its place along with all the rest. How can there be any illusion in this? Is not this precisely the death of illusions? Is not this incoming of exact science the beginning of the end of every erroneous conception of human life?

So be it. There rises here nevertheless an illusion from which few of us altogether escape. If I would remember just what my scientific work has actually made known to me, namely, a fragment, which exists never in isolation but always in flux with innumerable other things which have not been scientifically determined, that would guard me against serious illusion — that would keep me, as a scientist, from believing or from advising or from prophesying except within the safe and narrow limits of my scientific knowledge.

But in fact it is fatally easy to forget how little I know, to forget the whole tangle of things which I have left out through ignorance or shut out in the interest of accuracy, to believe in a word that the whole complex affair from which I have painfully abstracted and defined a fragment goes on by the rules laid down in my monograph.

If one wishes to see the illusion of precision in an extreme and typically clear case he can find it sometimes in a young man just become a doctor. The young man has to his credit one dissertation upon some item of human experience. That has made him an initiate. He has passed from the outside world and is one of those who may speak to the outside world with the authority of science. His work touches great affairs in education, politics, ethics or religion. Time out of mind men have dealt with these affairs by rule of thumb, by their five wits, by what you please. The time for all that is past. This is the age of science. Let all concerned read this dissertation and govern themselves accordingly.

I wish I could say that this illusion were confined to a few unripe doctors of philosophy. In truth, the literature of the sciences dealing with human life overflows with examples, wherein men demand and expect a new education, a new politics, a new ethics, the revolution of institutions, each man assuming to speak with the authority of science, while yet no six of them could agree upon the programme which science requires.

In brief taking for granted that every sort of phenomena admits of and requires exact scientific investigation, I am of those who believe that every bit of knowledge, so far as it is true, is actually or potentially practical. I see no reason why any pulse of consciousness which reflects any aspect of reality may not really and usefully affect action.

It is an obvious fact, indeed, that in some cases the state of our knowledge permits us to formulate rules of procedure such that the results may be foreseen in highly accurate detail, while in other cases such precise prevision and prescription are quite impossible. Naturally the cases where this is possible lie in fields where the phenomena involved are simplest, most easily defined, most thoroughly studied and therefore already most completely understood. These conditions are doubtless found best in the phenomena dealt with in the simpler chapters of mechanics, chemistry, etc., and are most conspicuously absent when we confront the subtle complexities of human behavior. We know how to make soap but we do not know what Shakespeare will say next.

Reflection on this obvious contrast has given rise to the doctrine that there is a difference *per se* between nature and mind such that exact theoretical and practical science is possible in the one case but not in the other. In fact, however, the line between the simple well-understood phenomena where we have exact theoretical and practical knowledge on the one hand and the complex, little understood phenomena where we must guess and fumble and grope is not at all identical with the line which divides nature and mind. Our chemistry, mechanics, physics, biology, etc., confront fields within which nearly everything remains to be done and where we can still do nothing but guess and fumble and grope.

On the other hand we are not without a body of definite verified knowledge of human nature which gives us reliable practical guidance. I can think of no reason why this theoretical and practical knowledge of human nature should not continue to grow side by side with physical science both of them becoming with the years more comprehensive, more exact and more useful.

Nevertheless, the life of the scholar tends to unfit him to succeed practically in any field, tends to make his advice inadequate in every field, unless his work as scholar is tested, corrected and brought into due perspective with things outside his specialty by thoroughgoing practical experience. A lifetime spent in developing a system whose criterion of validity is its internal logical or mathematical consistency, may bring about a signal advance toward a finally valid view of all truth. In like manner a lifetime spent in intelligent scientific research makes its contribution to theoretical and in the long run to practical knowledge. But never, I believe, does either of these procedures or both of them combined determine all the conditions of any action. Always some of these conditions are shut out through ignorance or for the sake of consistency or for the sake of accuracy. From these excluded conditions the eye of the scholar is holden so that he cannot see them. And when from the height of his learning he tells the foolish multitude what to do, it is not simply the multitude which replies that he also is foolish. It is over and over again the greater reality which, speaking through the event, brings him to confusion.

III.

The Success of Theory.

I turn now briefly to the question, how may we mediate between abstract aspects or fragments of truth and the requirements of practice? There are two answers to this question which have weight beyond any individual opinion.

Concrete Science.

The first answer is given in one clear form by the higher schools of technology. The professors in these schools are in the best cases men who after thorough training in one or another fundamental science, devote themselves to the study of concrete problems for which a practical solution is required.

It is not to be overlooked that such studies have value as contributions to scientific theory. The technical sciences are not simply borrowers from the pure sciences. They exploit new aspects of reality. They establish new facts which 'stand in their own right, throw light upon the less and the more complicated aspects of reality and so do their share toward a future correlation of the sciences into science.'[1] What concerns us now, however, is not the contribution of such studies to scientific theory, but the fact that such studies must be made as bridges between abstract science and practice.

[1] Bryan and Harter, Psych. Rev., VI., 346.

If we inquire for analogous studies within the field of psychology, what showing can be made? We have for one thing a literature dealing with artificially isolated aspects of conscious life, such as will, attention, association and the like. We have another literature dealing experimentally with functions and processes which are in themselves concrete but which in the investigations are isolated from the complex stream of life in which alone they normally occur. We have finally pseudo-scientific literatures, phrenology, physiognomy, and the like, which are concrete enough, and which tell all men specifically what to do, but which science has disowned.

When we have told off these departments of our literature, comparatively little remains, and yet something remains. "Within the fields of comparative psychology, psychiatry, criminal and industrial psychology, we have pictures of the typical conduct of animals, children, melancholiacs, paranoiacs, etc., which instruct us better than unscientific popular psychology can, what to expect and what to do in dealing with individuals of these sorts." [1]

What the future will bring forth in the field of concrete psychology, whether in time the studies in this field will approach in importance the studies which now issue from the technical schools, only the future can show. For myself I have grown in the belief that in a great range of current psychological problems it is good strategy for the experimental psychologist to supplement his investigation of isolated activities and functions by the investigation of concrete activities and functions as they appear in everyday life. I believe that in this direction there lie new chapters in the history of psychology.

Before leaving this point I should like to say that a large part of the work of Professor James seems to me concrete and also practical in the sense which I have indicated. Professor James has not sought to develop a rigidly logical system within which everything should interlock with everything else. He thinks apparently that such a system sacrifices truth to logic. On the other hand his book is not a bare list of findings based upon laboratory statistics. No one has seen more clearly than

[1] *Loc. cit.*, 347.

be how the significant truth may evaporate through the finest mesh of statistics, leaving behind it the illusion of precision. Yet, notwithstanding his avoidance of an extremely systematic psychology on the one side and of an extremely experimental statistical psychology on the other side, his work is on all sides recognized as of quite first rate rank in the history of psychology. What he has done again and again is to satisfy men of many sorts, psychologists and laymen, that he has hit off a bit of life as it is. At the best his sentences are like Flaubert's *phrases viables*. A thing is at last once for all said. These happy achievements which are I believe, as practical as they are true, we owe to the fact that this author has the nearly unique advantage of being a scholar who is also an artist.

Experience with Affairs.

A second indispensable form of mediation between theory (whether abstract or concrete) and practice is found only in personal practical experience with affairs. Many scholars of course never enjoy this experience. Some never wish to enjoy it. It is easy as we know for a professor to become in effect a monk, living apart in his university monastery with cool and distant regard for the society from which himself and the university derive — often with childlike ignorance even of those practical affairs which his own specialty most nearly touches. If such a man does his own business well his social isolation is socially justified. He does one thing which he can do best and all men profit by it. The practical results of such work may, in the long run, prove to be incalculably great. Faraday, Kant or Darwin works fifty years upon a problem which seems remote. The busy public will believe anything of him except that he will ever accomplish anything practical. Yet we know very well that the ideas of such a scholar may in another fifty years quite transform both the ideas and the forms of business of the practical public.

It is equally certain, however, that learning does not effect such results except through scholars who are also men of affairs. It is the extraordinary good fortune of society to have had not a few such men. A Kelvin becomes counselor to engineers. A

Lecky or a Virchau serves in Parliament. A Lowell or a White enters the diplomatic service. An Eliot becomes a university president. In such a case the scholar does not confront society with remote academic advice. With all his learning, experience and will he grapples with men and affairs as they are. He is not there to announce principles. He is there to secure results. His principles are to be made flesh and dwell among us. His learning and his ideals throw their light about him as he works, but in the stubborn and tangled realities with which he works there is also light which in a life time may quite illuminate and transfigure his learning and his ideals. In a word the scholar may at a great price become a statesman. When this occurs, whether on a great scale or on a small one, whether at court or in a village school, we have at last a solution of the ancient problem of theory and practice.

ON THE ATTRIBUTES OF THE SENSATIONS.

BY PROFESSOR MAX MEYER,

University of Missouri.

When I was a small boy, I heard someone talk about sensations and their classification. He said that any sensation was a sensation of one of the five senses, and that each sensation had a quality and an intensity. This is the classification developed by the demand of everyday life. What is or should be the attitude of the psychologist to such a classification?

When I was a little older and received some instruction in psychology, I was told that it was not sufficient to distinguish merely quality and intensity of a sensation, but that there were two other *attributes* of a sensation, duration and extent, but that some sensations had no extent; besides I was told that there were probably more than five senses.

In recent years I read some books and magazine articles written by professional psychologists, criticising the theory of *attributes* and *elements* of consciousness. The criticism, however, was either purely negative or proceeded in a direction which reminded me too strongly of the psychology of a hundred years ago. It seems to me that the psychologists interested in this matter are not sufficiently aware of the fact that a scientific terminology can never mean anything but what we agree to mean by it; that a scientific term does not lose its usefulness by being used in daily life in a sense disagreeing with its scientific meaning.

Some principles of classification of psychological elements are rejected by some critics because the principle is said to be a physical or physiological or epistemological, not a psychological one. I am unable to appreciate in this connection an argument which is based on the distinction of different special sciences, *i. e.*, on a *terminology*. The terminology distinguishing special sciences is as far from being an *absolute* truth as any more particular terminology. The only criticism of a classification of elements of consciousness which I am willing to admit, is an inquiry into the *scientific usefulness* of the proposed classification of elements.

It is a very serious mistake, from the scientist's standpoint, first to adopt a certain terminology, and then to collect all those facts which fit into this terminology, but neglect all those facts of the same province of experience which do not fit into the adopted terminological system. And yet I am inclined to say that this mistake is almost without exception made by those who write about psychological terminology.

It cannot be enough emphasized, that the principle according to which we call a certain fact of experience either a single sensation or an attribute of a sensation, must permit a *modification* of our particular terminology whenever the progress of our knowledge of facts demands an adaptation of our terminology to these facts. However, some discussions of the terminological problem of sensations and their attributes give one the impression that the author was searching for such a thing as an absolute truth. The problem, if it is to be a scientific problem, is by no means this: *what are the attributes of sensation*, but this: *what facts of experience should we describe under the present conditions of our knowledge by the help of the term 'sensation' and what facts by the help of the term 'attribute'?* If the progress of our knowledge demands it, the terminology not only may change, but even must change. But, while the principle used for establishing a terminology must permit in the future the adaptation of our thought to those facts which will be discovered in the future, the terminology itself must be the means of describing our knowledge of actual facts of the present, not of merely possible experiences of the scientist of the future. We must not describe the known in terms of the absolutely unknown. Therefore the principle according to which we speak of sensations and their attributes must not make use of any hypothesis. A hypothesis is neither at present a fact of experience nor are we sure that it will ever be one in the future. What science needs, is not a possible terminology of the future, but an actual terminology adapted to the facts which we know at the present time. For this reason I reject Münsterberg's use of his hypothesis of a relation between a psychological atom and the function of a single nerve cell. This relation is at the present time perfectly hypothetical, indefinite, meaningless, unsupported by any particular physiological knowledge, and for this reason cannot be used as a principle of the *terminology* in the science of the present time.

The principle which I propose in the following agrees to some extent with Münsterberg's views concerning the 'elements and atoms of consciousness.' But only to some extent. I believe that the application of my principle to the facts brings about a terminology, which is more useful than Münsterberg's terminology, because it incites us to look first for the facts and then for the terms in which to describe them, whereas Münsterberg, it seems to me, offers a ready-made system of terms and merely invites us to find the facts which fit these terms.[1]

Among the principles proposed for the classification of elements of consciousness is one which is particularly favored by psychologists, namely the principle of independent variability; *e. g.*, a tone may retain pitch and duration, but vary its intensity. The reason why I am opposed to this seemingly very beautiful principle (of independent variability) is, that it does not possess scientific usefulness. I shall prove this at once by referring to a special case. Stumpf pointed out many years ago, that a single tone has the attribute of quality as well as of pitch. This means of course neither more nor less than that the tonal judgments which remain if the judgments of intensity and duration are neglected, should be divided into two distinct classes. For these two classes we must have names, scientific terms, and the best terms seem to be pitch and quality. This division is made for the single reason of its usefulness for the description of the facts. It is self-evident, that, the more superficial a psychologist's knowledge of the facts of hearing, the less he will see the scientific usefulness of the division of judgments into the two classes mentioned. That so few psychologists have paid any attention to Stumpf's division shows how

[1] I have in mind particularly his 'drei Qualitätenreihen, der Art, der Stärke und der Selbständigkeit.' *Grundzüge der Psych.*, p. 285 ff.

little interest the average psychologist takes in the facts of hearing. I am sure that Stumpf's division is exceedingly useful for a scientific description of the facts.

However, if we accept independent variability as the principle of distinguishing attributes of sensations, then this distinction between pitch and quality of a single tone is impossible since a stimulus of a given vibration frequency, producing a single tone, does not permit to vary the pitch independently of the quality, both being dependent on the vibration frequency. But what should determine our acceptance of a certain terminology, its merely formal beauty, or its scientific usefulness? I confess that I do not hesitate to decide in favor of the latter. Independent variability may be a very beautiful principle of classification, but it has ceased to be useful for the description of the facts. It is scientifically sterile.

I shall use in the following the terms 'single sensation' and 'attribute of a sensation,' and I shall call a single sensation a representative of an 'element' of consciousness, an attribute, an 'atom' of consciousness, selecting the latter two words as proposed by Münsterberg. Now, I cannot in advance say what I am going to mean by these four expressions; that would be unscientific. Their meaning can become clear only by their application to the facts. But I will tell at once some things which I *do not* mean by these expressions.

1. I do not mean, when I call a single sensation an element of consciousness, and an attribute of a sensation an atom of consciousness, that there are no other elements and atoms of consciousness but sensations and their attributes. But, these are the only elements and atoms of consciousness in which I am interested at this time. There is no law prohibiting the application of these terms, elements and atoms of consciousness, to other facts of psychological experience ('affectional' and 'transitional' elements and atoms), provided that this application is scientifically useful.

2. I shall limit the present discussion to *peripherally aroused sensations*. The reason why I do this is that otherwise I fear I could not make the matter perfectly clear in a brief paper. I do not deny that there are also centrally aroused sensations.

I doubt, however, if this fact could have any influence on the proposed terminology.

3. I do not admit at all as an argument for or against a scientific terminology its agreement or disagreement with the terminology of life. I do not believe that because 'the words element and attribute (or aspect) are vague and meaningless' in our daily life, they must for this reason be vague and meaningless to the scientist, the psychological theorist. If the theorist would only make up his mind to mean by these words definite facts and nothing suggested by these words in the affairs of the day, these words as psychological terms would be perfectly clear. I shall not begin, however, with a *definition* of these terms according to the rules of formal logic. I merely wish to say, that I regard an element as simpler *in a certain way* than a complex, and an atom as simpler *in a certain way* than an element. In what way simpler? This question cannot be answered in advance. We shall find out in what way *as soon as we use the terms for the description of the facts.* If we stated in advance what we mean by 'simpler in a certain way,' we should be compelled to adapt the facts to the terms. But, as scientists, we wish to adapt the terms to the facts and to mean by *these terms*, whenever we use them in this special science, nothing whatever but *these facts.*

I shall now proceed to an *analysis of a peripherally aroused state of consciousness* (affectionally neutral) and state the principles according to which I propose to classify the theoretical constituents of the actual complex state. I shall use the words 'simplification' and 'elimination' without meaning that eliminated constituents are annihilated, but merely that they are practically pushed beyond the threshold of psychological effectiveness. They may be entirely annihilated, but the word elimination shall not imply this. In the same manner I mean by simplification, that practically our given consciousness must be regarded as simpler, as less complex than another given consciousness, what we mean in life by speaking of concentration of attention contrasted with diffusion of attention. I have to state this in order to prevent any reader from applying arguments of formal logic to problems of scientific terminology.

A peripherally aroused complex state of consciousness can be practically simplified:

1. By either simplification of the objective conditions or concentration of attention, with similar results. If there is such a parallelism of effect, we speak *of single sensations*, *of elements of consciousness*, in accordance with the classes of judgments directly resulting from this simplification.

2. By concentration of attention only (not also by a simplification of the objective conditions). In this case we speak *of attributes of a sensation*, *of atoms of consciousness*, in accordance with the classes of judgments directly resulting from this simplification.

[3. By an alteration (but not simplification) of the objective conditions. This case is insignificant for psychological terminology.]

It is necessary to emphasize three points: (1) I did not speak of a simplification of a complex state of consciousness by a mere *alteration* of the objective conditions, but of simplification by a *simplification* of the objective conditions. I did not speak, however, of '*physical and physiological elements*,' because our views concerning such elements are too variable. No two scientists would probably be found in perfect agreement on this matter. But if of two facts, given in the simplest description possible the one is simpler than the other or not, on this question it is comparatively easy to agree. (2) When I speak of the objective conditions (the stimulus) of a fact of psychological experience, I mean those particular conditions which permit *the simplest and clearest definition of a stimulus producing the effect in question*. (3) When I speak of *objective* conditions, I mean here, temporarily, *physical or exterior chemical conditions*. Of course, physiological conditions of the nervous system (like Münsterberg's 'function of a single nerve-cell') are also in a certain sense objective, but we cannot use them, because we cannot define the known by the unknown, because physiological processes in the brain, to which we are accustomed to refer, are as yet almost unknown, chiefly hypothetical, not admitting any definite answer to the question, whether one of them is *simpler* than another one. However, when physiology shall have enabled us to answer this question, there will be no objection to using also the physiological simplicity for the classification of elements of consciousness.

I now have to apply the above terminological principle to all the sensory facts known (so far as this is possible within a limited number of pages). I have to show what sensations and what attributes we have to distinguish according to the principle. And in cases where I have reason to believe that the reader may hesitate to admit the scientific usefulness of the resulting classification, I have to point out wherein its usefulness consists.

Let us imagine the peripherally aroused state of consciousness of a very young infant, exposed to all the normal stimuli of the average day. There can be no doubt that in some respect this consciousness is much more complex than that of the average adult under similar objective conditions. Of course, the adult's consciousness is complicated by a great number of associated ideas, of memory images, of centrally aroused sensations. But these we leave out of consideration. The complexity of the infant's consciousness which I have in mind is referred to by some psychologists, when they call the functional aspect of the infant's consciousness by the name of 'fusion,' that of the adult's consciousness by the name of 'analysis.' We may express this fact also by saying that the infant has not yet learned to concentrate his attention.

Now let us see what judgments may directly result from either simplification of this assumed consciousness of ours by concentration of attention, or similarly from simplification of this consciousness by simplifying the objective conditions. We may imagine that all auditory stimuli are kept from the subject. Also all gustatory and olfactory stimuli. We may imagine the subject as floating in a medium so as to prevent all cutaneous stimulation. And so let us remove all stimuli but those which we call optical. The practical difficulty of such an experiment must not be offered as an objection. There is no doubt that we can *approach toward the limit* (this word is here used as it is used by the mathematicians) where only optical stimuli are active. Every physicist will admit that the description of this limit is very much simpler than the description of the condition of stimulation from which we started our discussion. And every psychologist will admit that the accompanying consciousness is simpler. But we have to continue our process of simplification. Let us imagine that the optical stimulation was that of a winter landscape, made up of nothing but dark trees, white snow between, and the blue sky above. Or let us imagine that our eye is exposed to nothing but the blue sky and an infinite snow-covered plane; or to the blue sky alone. Is not a complete mathematical description of the stimulus producing the latter experience simpler than the description of the stimulus in the other cases? And is not our consciousness simpler too? Nothing can directly result from it but a judgment concerning this experience of sky-blue. A similar simplification of consciousness can, of course, result from mere concentration of attention. Without simplifying at all the original complex stimulation, the adult mind may pay attention to the color of sky-blue alone and pronounce a judgment thereon.

The problem now before us is this: can this parallelism of the two methods of simplifying our consciousness be traced farther than to this point? Let us agree that the only impression which concerns us be the blue sky of the winter landscape. Can we not pronounce more than one class of judgments concerning this sky-blue? Can we not say that it is large (compared with the white and black remainder below), or that it is bright, or that it is blue? No doubt, we can simplify our consciousness by concentration of attention in such a way that only one of the judgments mentioned directly results. But can we also simplify the objective conditions so that the directly resulting judgment can be one of, say, brightness only? I shall show in detail that such a parallelism no longer exists here.

How can we determine by the simplest physical definition, under a given condition of sensitiveness of the eye, what we mean by blue, or violet, or a neutral gray, or black? In a more primitive stage of physical science the physicists used for this definition the infinite number of homogeneous lights of the common solar spectrum. We can use this primitive physical definition of a stimulus producing a certain visual experience, if we intentionally or unintentionally overlook its imperfection, for the application of the proposed principle of psychological classification. A difficulty arises in this case for the psychologist only with respect to the colors, which are not in the spectrum, *i. e.*, the purples which are more reddish than the violet of the spectrum. I shall actually show how we can use this definition to this extent. However, those who are familiar with the multitude of facts which we call physiological optics, will urge at once, that the only objective definition of any visual sensation, which is satisfactory to the physicist, is the definition by means of an equation containing as three constants three selected homogeneous lights. I copy a physical definition from Helmholtz's *Handbuch der Physiologischen Optik*, p. 341:

"Wenn wir die Grundfarben und ihre quantitativen Einheiten R, G, V gewählt haben, dann kann der physiologische Eindruck jeder andern Farbe F dadurch vollkommen beschrieben werden, dass wir sagen, sie sehe so aus, wie eine Vereinigung von so und so viel Einheiten R, G, und V. Also, wenn wir mit x, y, z Zahlen bezeichnen,

$$F = xR + yG + zV."$$

I shall now first use the physical definition of a stimulus (producing any visual sensation) by means of the infinite number of homogeneous lights. We have to answer the two questions: (1) Is the sensation produced by homogeneous light of a certain point of the spectrum (say, blue) simpler than the sensation produced by homogeneous light of another point of the spectrum (say, violet)? The answer to this question must be that the sensation blue is simpler in a certain way than the sensation violet. For we can judge violet with respect to its duration, its extent, its brightness, its bluishness, and its reddishness. Each of these judgments implies a simplification of our state of consciousness by concentration of attention. But we can judge the sensation blue only with respect to its duration, its extent, its brightness, and its bluishness. Blue therefore is simpler in this way just mentioned than violet. (2) But can we produce a simplification of our consciousness with a similar result also by simplifying the objective conditions? The answer to this second question must be negative. The homogeneous light producing the sensation blue is not simpler than the homogeneous light producing the sensation violet. We have to say, therefore, in accordance with our terminological principle, that violet is not a sum of sensations, but a single sensation in the same sense, in which blue is a single sensation. Does not the reader instinctively agree with this result?

We must now answer the same two questions with respect to 'blue' and 'gray': (1) That the sensation blue is in a certain way less simple than the sensation gray, cannot be doubted. For we can produce by concentration of attention each of the judgments concerning blue which we can produce concerning gray, and one in addition, that of bluishness. (2) The physical stimulus, however, which the physicist would use for the definition of blue, is simpler than the stimulus of gray. The stimulus of gray would have to be defined as the stimulus of blue plus a certain additional stimulus, or two additional stimuli, or more than two additional stimuli; *i. e.*, the stimulus of gray is the sum of two, three, or more homogeneous lights. We do not have, therefore, in this case, a physical simplification parallel to the simplification by concentration of attention. Therefore we must say, in accordance with our terminological principle, that the sensation blue is a single sensation in the same sense in which the sensation gray is a single sensation. I should think that the reader will agree with this result.

Let us further answer the same two questions with respect to white and black: There can be no doubt that the objective condition of white is less simple than the objective condition of black, the latter being merely negative. However, black does not permit any simplification by mere concentration of attention, which white would not permit also; black is not simpler than white. There is no parallelism of the two methods of simplifying our consciousness according to our principle.

The result of this application of our terminological principle to the facts which we know at present, is then this: The principle requires us to call any visual sensation which is uniform over a certain area of the field of vision, a single sensation, not a sum of sensations. We must speak of two or more visual sensations only in case we have two or more different areas within the field of vision.

The next question to answer is now this: Which classes of judgments are to be distinguished as directly resulting from a further simplification of single visual sensations by concentration of attention only? *I. e.*, which are the *attributes* of visual sensation? The classes of judgments which we have to distinguish are so far as my knowledge reaches, the following: duration, extent, brightness, bluishness, yellowishness, greenishness, and reddishness. These are the seven attributes of visual sensation, in accordance with the terminological principle and the present condition of knowledge.

I do not fear that anyone will raise the objection that the element of consciousness which we call visual sensation, cannot have *seven* attributes, since it is an *a priori* truth that any and each sensation has four attributes (with the exception of some which have only three). If a man pretending to be a scientist would argue in that way, I should simply leave him alone. I fear, however, that some psychologist might object to the above result, because *no particular sensation ever possesses all those seven attributes.* Yet this is not a scientific objection.

Does not the chemist call gold a chemical element, although it is sometimes solid, sometimes liquid, but never solid and liquid? Why should any particular experience of an element of consciousness be required to possess all the possible attributes of this element?

And further, this is not a singular fact, peculiar to visual sensation. We shall later see, that in the auditory element of consciousness too we find some particular auditory sensations which do not possess all the attributes of the auditory element.

I believe that it is one of the fundamental tasks of experimental psychology, to determine the laws governing the mutual relation between the attributes of a single sensation as well as the mutual influence of the attributes of several sensations. Some of the most important laws of the attributes of visual sensation are these:

1. The attributes yellowishness and bluishness do not coexist in a single sensation.

2. The attributes reddishness and greenishness do not coexist in a single sensation.

3. The attributes yellowishness, bluishness, greenishness, and reddishness may be absent altogether in a single sensation. The other three attributes are always with a certain vividness present in the conscious experience of a visual sensation.

Facts like these are not objections to the proposed terminology, but simply *the natural laws of the attributes of visual sensation.*

As children in school, we used to wonder why our teacher told us sometimes that a solution of an arithmetical problem was wrong in spite of the fact that the result was right. Now in the above classification of the attributes of visual sensation, I believe that the result is right, *i. e.*, scientifically useful, in spite of the fact that I made *two* mistakes: (1) I started from a physical definition which is too primitive, and (2) I used the trick of omitting from discussion the purples which are reddisher than the violet of the spectrum.[1] I did this because psychologists (and physicists) sometimes use and have to use this primitive physical definition in elementary instruction; when the student hears about this matter for the first time. I shall now apply my terminological principle to the facts while using the more perfect physical definition of Helmholtz, as above stated. We shall see that this results exactly in the above classification of attributes, so that the above classification is indeed the one to be accepted.

The equation $F = xR + yG + zV$ can be *simplified* in no other ways than by rendering either one or simultaneously two or all three of the numbers x, y and z equal to zero. Now, let us apply this fact to some particular experiences, the experiences of R, G, blue, purple, gray and black. R, G, and purple permit each five of the seven classes of judgments (above mentioned as attributes) by means of concentration of attention. *E. g.*, R permits the judgments of duration, extent, brightness,

[1] Someone might point out (justly) that a parallelism of simplification of consciousness does exist in the case of purple and blue ('Urblau'), since the latter can be defined by one homogeneous light, the former not by less than two. When we use the scientifically more perfect physical definition of Helmholtz, no such difficulty of an apparent contradiction arises.

reddishness and yellowishness. Blue permits only four. But the sum on the right side of our equation contains only one member in the case of R or G, at least two members ($F = xR + zV$; but it is improbable that the brightness should happen to be such, that y is equal to zero) in the case of purple, and at least two members (probably again all three) in the case of blue. It is impossible, therefore, to speak in these cases of a parallel simplification of our state of consciousness by either simplification of the objective conditions or concentration of attention.

Further, the experience of gray permits only three judgments resulting directly from a mere concentration of attention; the experience of blue permits four; the experience of V permits five. But the physical definition by Helmholtz's equation is simplest in the case of V; less simple in the case of blue; and least simple in the case of gray. There is no parallelism of the sort we are looking for.

Further, the experience of gray permits by concentration of attention three classes of judgments, concerning duration, extent and brightness. So does the experience of black. That the stimulus in the case of black is defined in the simple way $F = O$, does not establish any parallelism of the sort in question.

We must repeat therefore what we stated above:

Any visual sensation which is uniform over a certain area of the field of vision, must be called a single sensation, not a sum of sensations. The attributes of visual sensation are: duration, extent, brightness, bluishness, yellowishness, greenishness, and reddishness.[1]

Let us now consider a case in which the objective conditions of stimulation have approached the limit at which we need not speak of any but acoustical stimulation. Let us imagine that the stimulus consists of three sine waves of the frequencies 300, 400 and 500. We say that we hear several tones. No one

[1] I have been criticized by Mrs. Ladd-Franklin for saying: Die Heringsche und die Helmholtzsche Theorie ergänzen sich gegenseitig. I did not mean by this that both of them were *psychological theories of color-vision*. This name can be given only to the Hering theory. What I wished to express by the words above quoted is my conviction that we cannot get along in psychology without the Helmholtz theory. And I hope to have made clear now, in what sense this is true.

denies that a stimulus consisting of only one of these sine waves is physically simpler. We hear in this case one tone only. We can produce a similar result, without simplifying the stimulus, by merely concentrating our attention. We concentrate our attention on one of the several tones; and only judgments concerning this one tone result directly. A further simplification of our state of consciousness by simplifying the objective condition is impossible, since one sine wave is as simple as any other. We therefore say, that the tone we hear is a single tone, not a sum of sensations.

But a further simplification of our state of consciousness by concentration of attention only is entirely possible. Four classes of judgments may directly result: judgments concerning duration, intensity, quality,[1] and pitch. These are therefore the four attributes of auditory sensation.

Since the average psychologist takes so little interest in auditory sensation, I shall have to point out in some detail, in what respects it is useful to distinguish judgments of quality and judgments of pitch as two classes. I shall state a number of facts, most of which seem to be quite unrelated facts as long as we fail to make the above distinction but become interesting as particular cases of more general facts as soon as we refer them to the one or the other class of judgments.

There are two reasons why this distinction of the attributes of quality and pitch of auditory sensation is not generally made. One reason is that this distinction does not agree with the old-fashioned, but nevertheless absurd theory, that any sensation has either the four attributes of quality, intensity, extent, and duration, or the three attributes of quality, intensity, and duration. The other reason is that it does not agree with the principle of independent variability, quality as well as pitch being dependent on the vibration frequency; but psychologists should keep in mind that the terminological principle of independent

[1] I use the term 'quality,' because in English this is the term which is daily used by every maker of musical instruments when he is conscious of that sort of judgment. The reason why the psychologists do not like this term, is no other than its not fitting into their artificial systems. However, it is the best term I know. In German we should use Stumpf's term 'Tonfarbe' ('Klangfarbe' for a mixture of qualities).

variability is not a divine revelation. It has no claim for existence beyond its usefulness.

1. We can theoretically understand the difference between a 'pure noise' and a 'tone,' if we regard a tone as an auditory experience under such conditions material for the function of attention, that all four classes of judgments may directly result; a pure noise as an auditory experience made up of brief tone sensations under such conditions for the function of attention, that no judgments concerning pitch can result. *I. e.*, we may judge, that this noise is 'higher' (referring to the mixture of qualities, the mean quality) than another noise; but we cannot say that this noise is in unison with the other noise, or that it is its fifth, or a mistuned fifth. More details about the physical conditions of stimulation producing a pure noise are to be found in my paper 'Zur Theorie der Geräuschempfindungen.'[1]

2. In demonstrating to students very low and very high tones, say below 30 and above 8,000 vibrations, I have found that quite commonly my hearers refuse to call these sensations 'tones.' They incline to call them noises. This is not wonderful to the psychologist who is aware of the fact, that these tones do not possess the attribute of pitch (they cannot be musically employed), while they possess the attributes of intensity, duration and quality. Such a tone may be said to be lower or higher (referring to quality) than another tone, but it cannot be said to be its third or its fourth. The difference between the experience of such a tone and a pure noise consists merely in the fact that theoretically we regard the latter's quality as a mixture of qualities, the former's quality as a single quality.

3. The theoretical distinction between tone and noise is entirely different in kind from the theoretical distinction between, say, blue and yellow. We may compare, in some respects, a tone with a uniformly colored piece of paper, a noise with a painter's palette, or rather with a flickering color-wheel. The only respect I can see, however, in which the distinction of tone and noise can be identified with the distinction of blue and yellow, is its practical significance. To distinguish between tone and noise is *of equal practical importance in life* as to distin-

[1] *Zeitschrift für Psychol. u. Physiol. d. Sinnesorgane*, 31, p. 233.

guish between yellow and blue. But who ever thought of basing the terminology of pure science on such a consideration! Why not say, then, that the distinction between a horse and a steam engine is the same as the distinction between ice and glass? With respect to practical importance it certainly is. But scientifically?

4. The technical problem which the organ builder has to solve is this: He is required to construct single sources of sound which impress us as possessing the same pitch, but different quality. He solves this problem by making use of this *important law* of auditory attributes in a plurality of auditory sensations: We can easily pay attention simultaneously to several pitches so that several judgments directly result, but with great difficulty only to several qualities so that several judgments directly result, unless we pay attention to their corresponding pitches too. And we can easily concentrate our attention on a single pitch, but not on a single quality; if we try to pay attention to the corresponding quality, the resulting judgment of quality is as a rule a judgment determined by all the qualities of the several tone sensations present. This is the law which the organ builder uses in order to obtain the desired effect (but which seems to be unknown to most psychologists, who take only a slight interest in the fundamental laws of hearing). By using the physical fact of partial vibrations of elastic bodies, he combines several tone sensations of which one has a far greater intensity than the others. This intensity attracts our attention to this one sensation more than to the others, and we judge to hear one pitch because our attention is concentrated on one pitch. Our judgment of quality, however, is, we may say, *an auditory illusion*, entirely comparable with a certain class of geometric-optical illusions.[1] It is self-evident that the mellowest quality which an organ builder can give to a single source of sound is the quality of its fundamental tone (without overtones), since an elastic body may produce overtones, but does not produce undertones.

[1] Compare Schumann, *Zeitschrift f. Psychologie u. Physiol. d. S.* v. 24, p. 7, 1900. "Die eigentlich zu vergleichenden räumlichen Grössen bestimmen nicht allein das Urteil, sondern die Ausdehnungen benachbarter Eindrücke wirken mit."

5. Musical effects depend on the hearer's paying attention to the pitches. Our congenital ability in this respect differs individually. There are some individuals, however, who are not normally affected by musical relationships in spite of a maximum practice and effort to pay attention to whatever there may be in their auditive consciousness. We describe their condition by saying that they are 'pitch-deaf' (similarly as we call some individuals 'green-blind'). We are all of us pitch-deaf for the lowest and highest auditory sensations (similarly as we are all of us green-blind on the peripheral parts of our field of vision). For the individuals mentioned auditory sensation possesses only three attributes, duration, intensity and quality. To their class belong probably those rare cases, reported in psychological literature, of individuals who could sing a tune in the key in which they had learned it, but not in another key. They sang by a memory for quality, not by a memory for pitch. Melodic relationships are the relations (on a certain unknown physiological basis) of the pitches of the auditory sensations.

We need not wonder why the attribute of pitch in auditory sensation is sometimes lacking, while the attributes of intensity, duration and quality are always present. We have found analogies in visual sensation.

The terms 'high' and 'low' are ambiguous. When we speak of tones as being high and low, we refer sometimes to pitch, sometimes to quality. This ambiguity is the cause of some disagreements in experimental practice as well as in theoretical discussion.

6. The 'absolute memory for pitch' is actually much less a memory for pitch than a memory for quality. It is a well known fact that quite frequently individuals are able to name the tones of a certain musical instrument (*e. g.*, the piano), but not those of another musical instrument (*e. g.*, the human voice). If we call this memory an absolute memory for quality, as we have a right to do from other reasons too, the fact mentioned does not require any particular explanation at all, since the 'mean quality' of the sum of auditory sensations produced by singing *c* is very different from the mean quality of the sum of auditory sensations produced by striking *c* on the piano. The

pitch, if we pay attention to this attribute, of the strongest sensation in either case is the same, but the name *c* is absolutely associated not with this pitch, but with the quality of the piano *c*.

The physiological processes in our nervous system, which underlie the experiences of pitch and quality, are probably no less different than the physiological processes underlying the experiences of extent and brightness in visual sensation.

7. There has been a discussion in psychological literature (I will mention only the names of Stumpf and Ebbinghaus) whether we have a right to call a tone more *similar* to its Octave than to its Third, after we have agreed to call a tone more similar to another tone by three vibrations higher, less similar to another tone by six vibrations higher. It is here as in so many other cases in science, that a problem is discussed with many arguments in this or that direction, while there is no problem at all. It is clear that there is no scientific problem of this sort left for discussion as soon as we adopt the distinction between the two attributes of pitch and quality. Or should we not have the right to say, that a circle is more similar to an ellipse than to a rectangle, after we have agreed to call a circle of medium gray more similar to one of dark gray than to one of black?

The above facts seem to me to express the most important natural laws governing the attributes of auditory sensation.

Let us now consider a case in which the objective conditions of stimulation have approached the limit at which we need not speak of any but gustatory stimulation. We can physically define any given gustatory experience by saying that it is the same experience which we have when the stimulus is made up of certain quantities of four substances in standard solutions, as shown by the equation

$$G = xAc + yBi + zSa + uSw,$$

e. g., of tartaric acid, quinine, common salt, and sugar. By concentration of attention we can simplify such a state of consciousness so that only the judgment sweet, none of the judgments sour, bitter, or salt, directly results. The same is possible by simplifying the stimulus according to the equation.

Can we further simplify our state of consciousness which we call sweet by concentration of attention? We can; and two classes of judgments may directly result, judgments concerning more or less sweetishness and judgments concerning duration. But is also a parallel simplification possible by simplifying the stimulus according to our equation? This is impossible, for the equation does not become simpler by a variation of u. We must say, therefore, that the state of consciousness produced by a certain solution of sugar is a single sensation, and that its duration and its intensity are its two attributes.

The sensation of sweetness, then, has the attributes of intensity (sweetish, sweet, luscious) and duration, but not of 'quality.' Sweetness is not an attribute of sweetness. Else, we should have to call visuality an attribute of visual sensation, auditiveness an attribute of auditory sensation, acidity an attribute of sourness. We need not say that what distinguishes a certain single tone from the visual sensation of the blue sky, is their different 'quality,' pitch and blueness. For we have just as much right to say, that what distinguishes them, is the tone intensity on the one hand and the brightness on the other hand. This consideration seems to have been the basis of Münsterberg's distinction of 'drei Qualitätenreihen, der Art, der Stärke, und der Selbständigkeit.' These 'Qualitätenreihen' seem to be a very simple expression of the laws of sensation. However, Münsterberg's terminology, as well as the terminology which speaks of the two attributes of quality and intensity of any and each sensation, makes us believe that simplicity of psychological law reigns over a province of facts, where actually there is a great diversity of laws. Simplicity of description is the aim of science. But the pretension of simplicity, where there is no simplicity, by means of adapting the facts to the terms is worse than no science. I do not see the necessity of having a general term ('quality') for sweetness, visuality, auditiveness, etc. Let us simply speak of different departments into which our sensations as such, without respect to their attributes, may be classified. And let us say, that some four of these sense departments, namely sweetness, sourness, bitterness and saltness, are in some respects (with respect to certain reactions of ours) so closely related, that we may call these four departments by the common name of a sense of taste. If we do this, we do not make the mistake of asserting simplicity where there is none and using complicated description where the facts are simple.

One fact concerning the sensations of sweet, sour, bitter and salt deserves to be mentioned in this connection. Those who speak of 'the quality of taste' and 'the quality of color' and other 'qualities' have to admit the peculiar fact that most of their 'qualities' are of the form of a continuous series, while 'the quality of taste' consists of four discrete points only. Those who are accustomed to mathematical thought must suspect under such circumstances, that the distinction of these 'qualities' of the psychologists is not the result of the consistent application of a definite scientific principle. And indeed it is not. Those 'qualities' are the outcome of practical necessities, not of scientific thought. They are mainly those attributes of the several sensations, which happen to be particularly important in man's struggle for life. Of course, I do not deny that some one attribute of a sensation is practically more important than others. But I do not see why such a fact should be expressed by *the terminology of a pure science.* Unfortunately the number of psychologists is still too great who regard psychology as an art rather than as a science.

I might go on and apply the terminological principle to all the other sense experiences. I merely fear to tire the reader. The following table shows the result of the application of the terminological principle to the facts, so far as the facts are at present generally agreed on by the psychologists. If our knowledge of the facts progresses, the application of the principle to the facts may lead to the addition of elements and atoms or to changes in what I now propose to call in each case a particular sense department. However, a principle which did not permit such modifications of our thought, would not be a scientific principle. By 'element,' of course, I do not mean the special sensation of a special individual at a special time, but an abstraction. Similarly as the 'element gold' of the chemist does not mean the liquid gold in a certain smelting furnace or the gold in a gold bath of a certain photographer, but an abstraction.

TABLE OF THE SENSATIONS.

Sense Departments Distinguishable at Present. (Peripherally Aroused Elements of Consciousness.)	Groups into which some Sense Departments are Collected at Present. (Senses).	Attributes Known at Present. (Possible Atoms of Each of the Peripherally Aroused Elements of Consciousness. Their Number Differs in the Several Elements.)
1. Visual sensation.	Vision.	Duration, extent, brightness, bluishness, yellowishness, reddishness, greenishness.
2. Auditory sensation.	Hearing.	Duration, intensity, quality, pitch.
3. Sweet sensation.	Taste-sense.	Duration, intensity.
4. Sour sensation.		Duration, intensity.
5. Bitter sensation.		Duration, intensity.
6. Salt sensation.		Duration, intensity.
7. Warmth sensation.	Cutaneous sense.	Duration, extent, intensity.
8. Cold sensation.		Duration, extent, intensity.
9. Pressure sensation.		Duration, extent, intensity.
10. Pain sensation.		Duration, extent, intensity.
11. Muscular sensation.	Organic sense.	Duration, intensity.
12. Tendinous sensation.		Duration, intensity.
13. Articular sensation.		Duration, intensity.
14. Sexual sensation.		Duration, intensity.
15. Static sensation.		Duration, intensity.
16. Olfactory sensation *x*.	Sense of smell.	Duration, intensity. Further attributes?
17. Olfactory sensation *y*.		Duration, intensity. Further attributes?
18. Olfactory sensation *z*.		Duration, intensity. Further attributes?
19. Other olfactory sensations?		Duration, intensity. Further attributes?
? Other sense departments?	?	?

Some one may object to speaking of 'visual sensation' as an 'element' of consciousness, because 'blue' is as 'elementary' as 'yellow.' To him I have to say, that a chemist speaks of mercury as an element, although it may be hard or soft or liquid or gaseous. He calls mercury an element nevertheless. Why should the psychologist not call visual sensation an element in spite of the fact that it is sometimes blue and sometimes not blue? The chemist justifies his terminology by pointing out the scientific usefulness of this particular usage of language. And I believe that it is equally justifiable to speak of visual sensation, without reference to any of its particular attributes, as an element of consciousness. That the natural laws of chemical elements and atoms should be identical with the natural laws of the elements and atoms of consciousness, is an arbitrary requirement. For scientific terms need never mean anything beyond what we agree to mean by them.

AN INQUIRY INTO THE NATURE OF HALLUCINATION.

II.

BY BORIS SIDIS, M.A., PH.D.,

Director of the Psychopathic Hospital and Laboratory of the New York Infirmary for Women and Children.

I.

A peripheral process often of a pathological nature, a state of dissociation and a subexcitement of secondary sensory and ideomotor elements constitute the main conditions of hallucinations. The peripheral pathological process and the state of dissociation are requisite to the formation of the hallucinatory percept, while the content of such percepts are given by the systems of sensory-motor and ideomotor elements. A peripheral process alone even if it be pathological in character does not give rise to hallucinations. Similarly a state of dissociation by itself or a state of subexcitement of secondary and representative elements cannot give rise to hallucinations. It is only when these conditions coöperate, it is only then that hallucinations arise. The state of dissociation and that of subexcitement of 'central' systems may be regarded as the 'central' conditions of hallucinations, while the peripheral process is the factor that supplies to the systems the primary sensory nuclear elements round which the secondary elements crystallize and form a hallucination.

States of dissociation, provided the other conditions are present, are preëminently favorable to the formation of hallucinatory percepts. In sleep, when the mind is immersed in darkness, isolated isles of systems may stand out of this general night of consciousness and give rise to dreams of various degrees of intensity. Dreams are sleep hallucinations, while hallucinations are waking dreams. Both hallucinations and dreams develop under the same conditions of dissociation. The nature of dreams and hallucinations are essentially the same. An isolated dissociated system of secondary sensory and representative elements predisposed to function become awakened by a special peripheral stimulus or by a summation of series of stimulations and gives rise to hallucinations or dreams according to the general state of consciousness, waking or sleeping. The hallucination of the comparatively waking state stands out alone, it remains more or less isolated and becomes obliterated by the general inrushing flood of peripheral sensations and perceptions of the waking consciousness. The dream is made up of a series of hallucinations going sometimes to form a complicated hallucination expanded into a whole life history. From this standpoint we may say a hallucination is an abbreviated dream, while a dream is an expanded hallucination.

In sleep the primary sensory nucleus of the dream hallucination is supplied by the peripheral processes coming either from external stimuli or from internal stimulations, from changes taking place in the organism. The psychophysiological threshold is raised in sleep, the resistance to the entrance of sense impressions is increased, the rise being proportionate to the depth of the sleep state. The peripheral sensory channels are closed to external stimulations. External stimuli, however, assail the peripheral sense organs from all sides and now and then, whether on account of the intensity of the stimulus or of the summation of a series of stimulations or of the temporary rise of the sleep level and consequent fall of the threshold and decrease of resistance to the influence of external stimuli, sense impressions force an entrance and awaken to activity some slightly slumbering systems thus giving rise to the dream hallucination. Under such conditions the sense impressions have but small chance to awaken its appropriate systems and hence become incorporated into any chance system they happen to awaken thus giving rise to the phantastic combinations characteristic of dream life. The sense impressions form the nucleus around which cluster systems of secondary sensations and representations all tinged with the sensory color derived from the original primary nuclear sense impressions. The systems of secondary sensory and representative elements once awakened may go on expanding and developing, awakening other groups and systems, assimilating them or being assimilated by them as much as the nature of their content permits and being further reinforced by incoming stimulations. During the whole course of its expansion the aroused groups and systems maintain their sensory or rather their perceptual character. For, if a system is once awakened to activity, the threshold, the resistance to incoming stimulations is lowered and many more sense impressions gain access to the functioning systems and become incorporated and assimilated. This assimilation of chance systems and sense-impressions often give birth to highly elaborated phantastic dreams and visions.

II.

Systems awakened by stimuli must have some relation of familiarity to the nuclear sense impressions. If perception is to take place, there must be some congruence between the sense impressions and the stimulated systems. Only on such conditions can assimilation take place. Similarly the awakened systems in sleep assimilate congruent sense impressions, the latter becoming so transformed as to fit the system and the system is modified by the incoming impressions. This congruence in the dream state is often strained and remote and consequently often of a phantastic and irrelevant character. Thus the taking off a plaster may give rise to a dream of being skinned alive, or of being scalped by an Indian. A change to an easier position and a freer respiration may generate a dream of flying. In one of my experiments of dream hallucination the uncovering of the feet in a cold room gave rise to the dream of walking on the frozen surface of a river and the impeded respiration awakened the feeling of fear of falling into the water.

The internal sensations such as arise from the different functions of the bodily organs are very important factors in the generation of dream hallucinations. Every one knows the fact that indigestion often gives rise to nightmares and unpleasant dreams, but not many realize the fact that cœnæsthetic sensations, sensations that come from our internal organs play a very important rôle in the production of dream hallucinations. The circulation of the blood, the secretion of the various glands, the peristaltic movement of the small intestines, the action of the stomach, the changes in the muscles, the metabolism going on in the various organs of the body, in the cells of the organism, all these give rise to sensations which, though obscure and confused, go to make up the general sense of organic life activity. The sense of cœnæsthesis may in fact be regarded as the basis of our physical being or of our physical personality. A change of this sense is frequently an important factor in the formation of delusions, when mental life becomes dissociated and disaggregated. Hypoæsthesia or anæsthesia of the leg, for instance, may form the nucleus for the formation of the delusion that the leg is made of glass or of putty or is totally gone. Anæsthesia of the body or of the internal organs may develop the delusion of being dead, the patient asking to be buried. Similar conditions are also present in dream life. Changes of cœnæsthesis play no doubt an important rôle in the activity of the dream consciousness. Changes in the various component elements that go to make up the obscure but highly complex life of organic sensibility affect profoundly the rich exuberant play of the dream consciousness. Since the channels to external stimulations are closed, the cœnæsthetic sensations that form the obscure basis of waking consciousness become the sole possessors and guides of whatever mental activity is present in sleep. These internal sensations are woven by the dream consciousness into phantastic images of all shapes and forms.

The dream consciousness presents many characteristics found in states of mental dissociation and disintegration. Moral tone is lowered, attention is greatly reduced, logical thought is enfeebled and the sensory-motor and ideomotor elements are thrown out of gear, often resulting in the formation of illusions, hallucinations and delusions. In the dream state there is present the mental degradation of dementia, the sordid delusions of hypochondria and melancholia, the delirious states of mania, the delusions of grandeur of general paralysis, and even the persistent systematized delusions of paranoia. The dream consciousness is extremely unstable, it forms no definite type of mental disintegration and has no determinate course, it is extremely fluctuating in its states and its background is usually shifting ceaselessly. From this standpoint it may be said that the dream consciousness is a normal form of mental alienation and that mental alienation is an abnormal form of dream consciousness. A very characteristic diary brought to my notice in which a retrospective and introspective account is given by a patient in the normal condition of the experiences lived through in the state of mental aberration opens with the suggestive title: *Memories of my Dream Life* and with the following interesting introductory remarks:

"Where shall I commence? How shall I begin to recall and record this to me mysterious life I have been living? So beautiful, so strange, and in some way so terrible. Yet I would not forget, for it seems as though I must have been in communication with intelligences above — spirits of the air, if it were possible.

"When did it commence? How long has it been with me? are questions I cannot solve. For weeks before coming to the hospital I must have been living this 'ideal life' as in an 'ideal world.' I have jotted down what I have thought, though they are not one hundreth part of the thoughts which passed through my mind during this strange time of dreaming." In one of my cases of katatonia the frightful dreams of the year preceding the disease became hallucinations of the maniacal stages and appeared again as dreams during convalescence. The dreamer dreams with his eyes closed, the insane dream with their eyes open.

In both the dreamers and the insane the disaggregated states under the influence of external and especially of internal stimuli give rise to illusions, hallucinations and delusions. Dissociated states grouped round nuclei of primary sensations form the internal organizations of hallucinations and delusions so often characteristic of dream life and insanity. Cœnæsthetic sensations are important agents in the formation of insane delusions and hallucinations, there are so many fermentation nuclei among masses of dissociated states. Irritation of the ovaries may in the insane awaken hallucinations and delusions of a sexual character; constipation and heaviness in the intestinal tract may generate delusions and hallucinations of rats and pigs in the stomach; rumbling in the stomach and the intestines may give rise to the delusions and hallucinations of devils in the body or of electric discharges of powerful batteries placed in the abdomen. The hallucinatory delusional dream consciousness works on similar lines — thus the first stages of migraine with a heaviness of the head may in sleep give rise to the dream hallucination of the head being opened, the brain swept away and chalk substituted; pain in the abdomen may form the hallucination of mice gaining an entrance into the abdominal cavity and gnawing at the intestines.

The difference between the walking life of the insane and that of dream consciousness is the mode of activity, the dream consciousness works in images, in sensory percepts, while in the insane mind the activity is largely representative. This difference is due to greater dissociation present in dream consciousness. The awakened dissociated systems in dream life become tinged with a perceptual sensory color by the process of absorption and assimilation of all the incoming sense impressions. Pathological states of rapid mental dissociation, such as the acute states of maniacal excitement or in states of psychopathic functional dissociation, such as the 'Dämmerzustande' of psychic epilepsy and other states of functional psychosis, closely approximate to the condition of dream consciousness, though the former are more stable and far more consistent, being narrowed to the active functioning of definite mental systems, conditions rarely to be met with in dream states.

The dream consciousness lacks unity of logical thought, certainly fails in critical judgment and is sometimes brutally indifferent to immoral situations and acts. The credulity of dream consciousness is well known to every active dreamer. Changes of time, place and of objects are often instantaneous and the most incongruent situations as well as transformations of personality are placidly and credulously accepted. The dream consciousness is entirely at the mercy of incoming sense impressions which spin the dream experience regardless of truth and reality and steadiness of logical purpose and moral ideals of the race. From this standpoint it may be claimed that the dream consciousness is to some extent a reversion to the earliest forms of mental life, when the race was as yet undisciplined by the accumulated experience of ages of social life.

The teleological aspect of the dream consciousness may possibly lie in the fact suggested by some that the many trains of thought started in the activity of waking life and arrested and suppressed by the selective thought and logic of things and events of waking life find their vent and completion in the activity of dream consciousness. This vent relieves us from the high pressure of suppressed thought and makes it easier to sustain the rigid selection of sequences of mental states required by the struggle of existence and social life in our adjustments to the conditions of external environment. This view, however, is not strictly correct. For the dream consciousness follows not only along the lines of thoughts started in waking life, but more often forms new lines of associations giving rise to highly dramatic situations and far from relieving waking thought impedes and depresses it, since the mind feels unrefreshed by the sleep and in many cases serious mental troubles arise due to the disturbing influence of active dreams on the course of waking thought. It is more likely that there is little teleology to dream life and if any teleology there be, it may consist in the freedom and ease in which the mind finds itself in the dream state, fettered as the mind is by the rigid relations of the external environment. In dream life the routine of waking life is interrupted and new associations are formed. This possibility of forming new associations and thus breaking through the routine of life, a possibility maintained and fostered by the dream consciousness, might have possibly proved of the highest consequence to the human race. The dream consciousness may thus be regarded as an important factor in the progress of human thought, as an agent in the breaking up of habits of thought due to the routine of life and calling the attention of man, absorbed as he is with the interests and requirements of the needs of his physical world, to another life existence and strange universe of reality.

III.

The sense of reality and belief in external existence of the hallucinatory objects are quite strong in hallucinations and in some of the more vivid and intense dream states. In order to explain this seemingly anomalous sense of reality, it may be well to revert to our general principle of subsuming both the normal and the abnormal under the same general laws and processes. Although the abnormal is of the highest importance in revealing new relations which the customary and habitual normal seems to hide, as it is found for instance in the growth and development of physiology largely due to pathological research, still we must clearly remember that from a strictly scientific standpoint the normal and abnormal are but teleological concepts which are of importance for the practical purposes of our habitual life activity and possibly for classification of various types of phenomena, but which science is to reduce to the same laws and processes. The abnormal is the normal out of place. In mental life as in the phenomena of life in general the atypical, or the variation, helps to explain the typical, the normal and the latter in its turn explains the atypical, the abnormal. We may therefore turn to the criterion of the normal sense of reality and validity of experience as explaining the same relations in abnormal mental life and the latter in its turn may throw light on the 'reality and validity' of 'normal' experience. A brief review will suffice for our purpose. It may look as if we attempt to make an excursion into a domain not belonging to normal or abnormal psychology proper, but to epistemology. This may be so, but the nature of our subject brings us so closely to this problem that a brief discussion may help us to see the facts in a clearer light. Abnormal psychology with its various forms of mental aberration, such as are to be found in the phenomena of insanity, functional psychosis, hallucination, delusion, somnambulic states, hypnoidic states, is so intimately connected with aberrations of the 'sense of reality and validity' of experience that not only the abnormal psychologist, but also the clinician must take it into account from a purely practical standpoint. We shall view the problem only in so far as it directly concerns and illustrates the general subject of our discussion, namely hallucination and illusion, or fallacious perception.

The objective reality of the physical world is sometimes defined, and with best of reasons, as social experience, as experience common to ourselves and our fellow men, as experience which men share in common seems in contradistinction to the psychic experience which is essentially of an individual character. The tree yonder can be seen by everyone who possesses eyes, but my perception of the tree, or my idea of it, can only be experienced by myself. It may be said that this difference between the physical object and psychic state is a valid and valuable one. It is, however, neither general enough, nor specific enough. For on the one hand it may be claimed that from a more general philosophical standpoint even the physical object belongs ultimately to the individual only and on the other hand it may be claimed that psychic experience is communicated to our fellow men not only in terms of the physical object, but far more often in terms drawn directly from our psychic experience. Neither the physicist nor the psychologist will be quite satisfied with this point of view as both physical objects and psychic objects are entirely emptied of their specific contents and must remain at best in the dubious regions of epistemology. Still this social aspect of the physical object is significant and valid and is even used by the psychiatric clinician as a practical standard in the valuation of abnormal mental life in general and of insanity in particular. It may, therefore, be of great value even if we do not agree with the extreme way in which this view is sometimes put.

It is true that at the first glance we cannot help being struck by the import of the common or social aspect of external reality. We are well assured of the existence and presence of an external object, if we have the assurance of our fellow-beings, and what is accepted by our fellow men assumes the dignity and authority of actuality. A fact is regarded as existing beyond the shadow of any dispute, if every one can verify it in his own experience. The categorical necessity of our modern science rests entirely on this principle of validity: The social object is the valid object. This criterion of validity of the external object stands out specially clear and distinct in our standard of abnormal mental life. A belief is regarded as insane and delusional, if it is in opposition to social beliefs and experience and is emphatically rejected by all other men. An object is regarded as illusory or hallucinatory, if it is treated as non-existent by other people; a desire, an action is considered immoral, if it is spurned by our neighbors. The real object is the social object, the valid belief is the social belief, and the social will is the moral will. The individual object, the individual belief, the individual will are treated as insane. One can not help noticing the semblance of truth in the assertions of those pathological anthropologists who put genius in the same category with insanity. What is social is alone true, valid and real, the individual is false, non-existent. The individual can buy the reality and truth of his being on condition of becoming social. Sociality is verity.

Let us now, however, try to break through, if for a moment only, the traditions of social *régime* with its criteria of social reality and validity. When being pricked or in getting a blow, or when cut or scratched, along with the experience of the sensation, the experience of the external stimulus is also given. In looking out of the window and seeing the tree with its green leaves moving in the wind, along with the perception of the sensory elements, primary and secondary, the external existence of the object tree is also given. Similarly in listening to the sounds of a familiar and dear voice and listening to the words as they form into phrases and sentences is not the sense of reality of the external object given along with the series of sound sensations? Sensation carries along with it the sense, the reality of its stimulus. It is not that the sense of reality is different from the sensation, it is given in the sensation itself. Similarly the percept and the sense of reality of the external object are not two different things; they are given together in the same process of perception and are identical. The percept tree is the perception of the reality of the objective tree yonder. The sensory process is also the process of the sense of reality. As Spinoza puts it in his *Ethics*: 'If the human body is affected in a manner which involves the nature of any external body, the human mind will regard the said external body as actually existing.' In seeing or perceiving the chair yonder we do not perceive it as real, because of its social or common aspect —the reality of its existence is given directly in the sensory processes of the percept itself. Sensory elements involve the reality and existence of their stimuli; the percept involves the existence of the perceived objective content.

The sense of reality of the external stimulus or object is strengthened by association of the original sensory systems with other sensory systems, and the intensity rises in proportion to the number of systems of sensory elements, brought into relation with the functioning sensory system. If on perceiving an object, we wish still further to assure ourselves of its reality, we verify it by means of other sense organs. If one sees an apple and wishes still further to assure himself of the real presence of the object, he goes to it and examines it with his other sense organs, he touches it, presses it, bites it, tastes it. Kinæsthetic elements, being the most important in adaptations and reactions to the stimuli coming from the external environment, are possibly of all sensory elements the ones that give the keenest and most intense form of sense reality. Facts warrant us to assert with some show of probability that the sense of reality is chiefly centered in the sensory motor or kinæsthetic elements which serve as nuclei for other sensory elements. Whether this be correct or not, it remains true that the sense of reality is given directly by sensory elements and their combinations and organizations. The more systems of sensory elements are pressed into service, the stronger is the sense of reality and the more assured is the reaction to the stimuli of the external environment. In the evolutionary process of man's adaptation to his environment he becomes extended in being and grows more developed because of his social relations with other men. Man presses into active service the systems of sensory elements of his fellow beings. Adaptations and hence successful reactions to the external environment are now more assured and the sense of reality is still further emphasized and intensified. Throughout the course of intensification of the sense of reality the principle remains unchanged in a nature. The sense of reality is given by and consists in nothing else but the sensory elements.

Social experience may be regarded as more real or as giving a more intense sense of reality, because of the greater number of sensory systems involved, but an object is not felt as external and real, because of its social aspect merely, the sensory aspect is by far the more fundamental. If one's perception of the house yonder is of a purely 'individual' character, not shared by his fellow men and even emphatically denied by them, the visual preception as such still directly perceives it as real, external and physical. Should furthermore this experience be intensified or confirmed by all the other senses — should he be able to touch it, to press it and feel its resistance, knock against it and feel concussion and pain, and have a series of tactual and muscular sensations by walking into the perceived house and around it, and should he further have this purely 'individual' experience of all the senses each time he comes to the same spot, the perceived object would then be a real, external, physical object and no amount of social contradication and lack of the ear marks of community could make it less real objective and physical. Epistemologically regarded, community may be sufficient for the purpose of reality; psychologically regarded, the real, existent physical object is essentially the perceived sensory object given by the 'community' of sensory elements. Sensory elements give the objective 'reals.'

IV.

From this long digression we may turn again to the question: "What is it that makes hallucinations in general and dream hallucinations in particular appear real, objective?" The solution is given in the question itself. We have shown in our analysis that hallucinations are essentially peripheral and sensory in character and do not differ in their make-up from sensation and perception in general which furnish the very foundations of our sense of reality. Hence hallucinations are real and objective, because of the constituent sensory elements. Strictly psychologically considered, percepts do not differ from hallucinations as far as process is concerned. Normal percepts differ from hallucinations mainly by the fact that the former are the habitual, the customary, confirmed by other systems of sensory elements and that in the struggle for life, they proved to call forth the fittest reaction.

Dream hallucinations, like hallucinations in general, are initiated by peripheral stimulations; even the so-called 'central' hallucinations are really peripheral in origin, the dream hallucinations naturally falling under the same category. The entrance of external peripheral stimulations being difficult in proportion to the depth of sleep and extent of hallucinatory dissociation, the internal sensations predominate in the functioning systems of dream life. For in sleep the activity of the internal organs, though depressed, still goes on uninterruptedly; the glands continue their function of secretion and excretion, the heart continues to contract and dilate, the blood goes on circulating through arteries and veins; the liver, the spleen, the stomach, the intestines, the lungs and other organs carry on their functions without a moment's arrest; the whole sympathetic nervous system, the vasomotor, the spinal cord, the medulla and other basal ganglia, all, contributing to the vast mass of internal sensations, can hardly be regarded as being asleep. All these peripheral internal sensations go to form nuclei of primary sensations around which secondary sensory elements become crystallized and organized and give rise to hallucinatory percepts — to dreams. To these must be added the external peripheral sensations coming from touch and pressure of bed clothes, from changes in the muscles, joints, ligaments, and synovial surfaces, from changes in the superficial temperature of the extremities from chemical changes in the olfactory and gustatory organs, from summation of minimal acoustic stimulations, and above all from changes in the visual apparatus and especially from the masses of light in the retina and macula lutea.

With the obscuration and dissociation of the mind the internal sensations along with the external peripheral minimal sensations come to the foreground of mental life. The dreaming consciousness stands in closer relation to the bodily functions than the waking consciousness, absorbed as the latter is with the intense stimulations coming from the external environment. The intense external peripheral sensations of waking consciousness obscures the weaker, but more constant internal sensations, as Hobbes puts it, much 'as the light of the sun obscureth the light of the stars.' This intimate relation between internal sensations was clearly seen and pointed out by Hobbes; 'and because' he says 'the brain and nerves which are the necessary organs of sense, are so benumbed in sleep as not easily to be moved by the action of external objects, there can happen in sleep no imagination and therefore no dream, but what proceeds from the agitation of the inward parts of man's body; which inward parts for the connection they have with the brain and other organs, when they be distempered do keep the same in motion.'

Dreams often reveal in a symbolic form and frequently in phantastic and grotesque images the conditions of bodily function, conditions which the waking consciousness cannot detect, because they lie in the subconsciousness and cannot overstep the threshold of waking consciousness. It is here in the deeper regions of cœnæsthesis, that we have to look for those 'prophetic' dreams which seem to foretell some future event, some future state of the organism. An incipient irritation of the nerve endings in the teeth, an irritation not yet felt in the waking consciousness, may become the nucleus of a dream and give rise to a dramatic vision of sitting in a dentist's chair and being operated upon, a prevision that may actually become fulfilled soon after. The growth of a malignant tumor may be represented in a dream under the form of a savage dog making an attack and setting his teeth into the place where the tumor is to develop. An incipient affection of the stomach may appear under the vision of being eviscerated alive, or of having swallowed a mouse which gnaws at the intestines. Incipient organic affections, not yet felt in the waking consciousness, may thus become the starting point of a highly dramatic prophetic dream. Dreams of such a 'veridical' character often appear highly mysterious and their fulfilled prophecy seems nothing short of the miraculous and supernatural. There are many such cases on record, but the following may be regarded as typical.

A lady, a relative of mine, had a very vivid vision which proved 'veridical' and seemingly could only be accounted for on supernatural grounds. One evening, on being left in a room all alone, she suddenly saw the apparitions of her deceased parents. The lady became very much frightened, but the parents quietened her and told her not to be afraid as they came to bring her good tidings. "You will give birth to twins, a girl and boy, name them after us, they will be strong and healthy." With this the apparitions vanished. The lady became very much agitated and, although she did not suspect to become a mother, still, being religious and a firm believer in spirits, she had implicit faith in the actual appearance of her parents, who appeared to her in order to bring her glad tidings from another world, and naturally she even began to prepare clothes for the promised twins. As this happened in a remote country place this prophetic vision soon circulated among all the neighbors and expectations were aroused as to the fulfillment of the prophecy. It soon became apparent that at least a portion of the prophecy was being fulfilled. The lady soon discovered that she was going to become a mother — the sceptics were somewhat confused, still they maintained their front, but they were completely silenced, when after a few months the lady gave birth to twins and that a boy and girl. The vision then did prove to be of supernatural origin.

If, however, we analyze the vision somewhat more closely, we find that it can easily be resolved into elements which admit of a perfectly natural explanation. The vision first of all occurred during the state of repose and was really a dream hallucination. Still this does not explain the fact that the hallucination appeared in such a dramatic form which turned out to be so strikingly prophetic. On further examination of the lady's history it was found that she lost both her parents but a few months before the occurrence of the hallucination and that this loss deeply affected her. This mental system was an important factor in shaping the course and development of the hallucination. At the same time there was another factor at work in the elaboration of the dramatically effective dream hallucination. The lady before she gave birth to the twins was already a mother of fourteen children. It is quite possible that, although in her waking state she did not suspect of being pregnant, still in her dream state, being cut off from the intense external stimulations, she could more easily realize her condition from symptoms and changes in the internal organic sensations which now alone reigned supreme in the dream consciousness. These symptoms and changes in the organic sensations during the incipient stages of pregnancy could all the more be easily appreciated by the lady as she had ample experience of them before. Some special changes in the organic sensations such as the arrest of the menses, changes in the circulation, in the metabolism of the generative organs and other changes of similar nature served as so many peripheral stimulations which, in states of dissociation such as occur in the light states of sleep, favored the occurrence of a dream hallucination that took the form of apparitions of the deceased parents, because of the subexcitement of this particular system and because for the time being the system played a dominant rôle in consciousness. Moreover, the organic changes differed greatly from the previous experiences of similar kind, and it was therefore quite natural that the dreaming consciousness should suspect the coming of twins, a circumstance which connected itself all the more closely with the formation of the dream and was no doubt a factor in the determination of the appearance of the apparitions of the parents, which in turn, helped her further to confirm the intuition that she was to be a mother of twins. All this was represented in the dramatic form characteristic of dissociated states in general.

V.

States of dissociation, light sleep and especially the intermediary states occurring in the course of falling into deep sleep or coming out of it are especially favorable to the formation of hallucinations. Such conditions occur in abnormal mental states in hypnosis, in somnambulism, in hypnoidal and hypnoidic states, in the so-called psychic equivalents of epilepsy, in pure psychic epilepsy, and, generally, in states of functional psychosis. In the intermediary states between waking and sleeping, dissociated systems awake and become accessible to the influence of external stimuli. This is clearly shown in the hypnagogic hallucinations, as well as in the frequent dreams often taking place in the lighter sleep states usually before waking.

I have often observed in myself, when being fatigued and becoming drowsy and closing my eyes, how fast phantoms and scenes flit before the mental gaze, most of them being formed by the flitting masses of light in the field of vision. Often in closing my eyes and keeping quiet, so as to become somewhat drowsy, and watching the field of vision, not directly, but, so to say, from the corner of the eye, animals, figures, faces, can be seen forming and dissolving into mist. These phantoms can be directly traced to specks of light and masses of color coming from the retina and especially from the macula lutea. In many psychopathic cases, not only vision, but also sounds and voices are experienced as in some of my cases that have hypnagogic auditory hallucinations of voices. The dimly lighted up regions that lie on the borderland of sleep and waking states are peopled with phantoms, ghosts and apparitions.

Statistics seem to confirm this point of view, since about 50 per cent. of cases of hallucinations may be classed as 'borderland hallucinations.' Some recent critics in this field of inquiry strongly favor the view that hallucinations occur in dream states, hallucinations being nothing else but vivid dreams, the percipient not being conscious of having fallen asleep. This view is not new, it is favored by Hobbes. 'The most difficult discerning,' Hobbes tells us, 'of a man's dream from his waking thoughts is then when by some accident we observe not that we have slept.' Many cases no doubt admit of such an explanation. I myself had an experience of such a character. While sitting and studying one evening, I felt myself suddenly transported into my father's house and looking out of the window, seeing the scenery characteristic of the locality and hearing the voices of my parents in the next room, but I could not discriminate the words. The vision was so real that I was surprised to find myself again at my book and in a place hundreds of miles away from home whither my hallucinatory state had so suddenly transported me. The hallucination was so strong and real that had I not critically analyzed the conditions of its occurrence I should have been fully certain that the hallucination appeared in the waking state. As a matter of fact, I was fatigued from my studies and dropped off. The actual surroundings, the room, the table, the book, the voices of my friends present, all disappeared from my view during the intermediary state, and when I came out of it I remember the start I gave in realizing once more the actual situation. According to records, hallucinations take place when the percipient is in bed, just after retiring, or about to wake up, or after waking. The percipient is really asleep, only he is not aware of it, so brief is the state and so intense and vivid is the hallucination. It may, therefore, be maintained with some show of truth that hallucinations are dreams and take place in sleep states.

We must guard, however, against carrying a generalization too far. This contention that hallucinations occur in dissociated dream states is somewhat overstated. It is true that hallucinations require states of dissociation, but this does not necessarily mean sleep states. Not all states of dissociation are dream states taking place in sleep, although it may be safely asserted that all dissociative states have many traits in common and are at bottom of the same nature. Hallucinations and dreams may be analogous, may be of the same structure requiring the same general conditions, but it does not for that reason follow that they occur in the same states, in sleep states. Dissociation with consequent hallucinations may also take place in waking states. Those who have studied hallucinations in different forms of mental diseases know that most of the hallucinations occur under widely different conditions and they further know that it is precisely in the waking states that hallucinations are most commonly present, while in the sleeping states they are more frequently absent. Insanity may be *compared* with dream states, but they are by no means *identical*. The important condition requisite for the occurrence of hallucination is dissociation and this condition often occurs in waking states, such as the hallucinations found in many forms of insanity, as for instance paranoia, hebephrenia, katatonia, general paralysis and other states of mental aberration. Even hypnotic and post-hypnotic hallucinations can hardly be claimed to have been really induced in dream states. They who have devoted time and labor to hypnosis know that the hypnotic state can by no means be identified with sleep and that in the very deepest stages of hypnosis the subject is to all intents and purposes fully awake; he is full of activity, his eyes are open, his senses are on the alert — he is far more awake to external stimuli than even in his normal state. The mind is very active and the subject carries on long trains of reasoning, argumentations and discussions with the people around him; in short, the subject in the deep somnambulic state is in a condition the very opposite from that of the sleeping state. Hallucinations occur both in the waking and sleeping states and require dissociation as an indispensable condition.

VI.

If we inspect more closely the relation of the stimulus to the hallucination, especially to the dream hallucination, we find that the intensity of the content is disproportionate to the intensity of the initiating stimulus, to the peripheral sense impression. A comparatively slight stimulation often gives rise to a dream of a highly dramatic character. This exaggerated character of the dream hallucination is well known. Thus a prick of a pin may give rise to a dream of being attacked by robbers and finally being run through by a thrust of a dagger. The application of a warm bottle to the feet may develop a dream of ascending a volcano and walking on molten lava, while a cold stimulus may give rise to a dream of participating in a dangerous expedition to the North Pole. Pain in the head, impeded respiration and pressure in the region of the neck may develop, as in the case of a friend of mine, the horrible dream of being dragged into a narrow dungeon and then beheaded.

To explain this dream exaggeration a theory is advanced based on dissociation. It is claimed that dissociation tends to convert the physiological 'ideational currents' into sensory 'currents' and intensify and exaggerate the psychic states. Before discussing the theory it may not be amiss to examine the facts which the theory is called to explain. It is questionable whether the general relation of dream stimulus is quite correctly stated. It appears that the generalization is stated somewhat in the form of the well-known question: Why do great rivers flow by great cities? It is by no means generally true that the characteristic of dream consciousness is to exaggerate stimuli received and work them up to a pitch so as to convert 'ideational into sensory currents.' The relation is far simpler. The dream does not necessarily as a rule exaggerate incoming stimuli and make of them exciting and sensational dream hallucinations. What happens is this: the commonplace non-exaggerated, unaffective dreams tend to fade away almost immediately on waking, while the impressive dreams are usually remembered. I have observed a number of dreams in my own case as well as in others and have found that the number of ordinary commonplace dreams far predominates over the striking and extraordinary dreams. Even in psychopathic cases in which subconscious dream life is often well developed, even in such states I have found in the cases which have been under my observation and experimentation that the commonplace dreams far predominate over the dramatic and extraordinary ones. The only way to convince oneself of it is to try to write down the dream immediately on waking. I find that the ordinary dream is very hard to hold in memory, it is elusive and is constantly slipping away from us, a special effort of attention is requisite to hold on to them; they are usually hazy, vague and confused. On the whole, the indifferent dreams really predominate, but it is only the impressive ones that remain in memory. Even the freshness and recency of the dream do not save it from falling into oblivion. Now while commonplace and indifferent dreams are forgotten older dreams, but more impressive, more awakening our emotions, especially emotions of fear, will be clearly and vividly remembered.

Still the fact that exaggeration and intensification of the sense impressions received by awakening a greater volume of secondary sensory elements and representations more often than in the waking state requires an explanation. This intensification may partly be due to the fact that in sleep sensory impressions often enter consciousness suddenly. This brings about a shock, awakening emotions which are conducive to a greater stimulation of a greater volume of secondary sensory elements and their accompanying representations. Even in the normal waking state sense impressions suddenly introduced into consciousness may cause a shock and give rise to an illusion, the object appearing as something strange and formidable. We can often observe it in ourselves, when falling into a drowsy condition, a slight stimulus which we otherwise ignore will give us a sudden start. I often observed in myself when in a drowsy state and 'dropping off' how an ordinary stimulus such as a cough, for instance, will produce a shock affecting the visceral organs, the feeling being somewhat similar to the condition commonly described as a 'sinking sensation in the pit of the stomach'; the shock seems to reverberate all over the organism.

To this must be added another important factor, namely, the emotion aroused. When an object is perceived under conditions that do not permit its recognition or its assimilation and consequently its customary reaction, an emotion of fear, or that of fright is produced. Such is the case, for instance, when some objects impress us in the dark or when we get hold in the dark of some slimy, slippery and especially of moving objects. These two factors often work together inasmuch as an object suddenly introduced into consciousness is also not speedily assimilated so that the shock and emotion due to non-recognition or non-assimilation go together. Now in sleep stimuli entering into consciousness effect it in a sudden way and from the very nature of the sleeping consciousness the external stimulation is but imperfectly assimilated; both factors, shock and emotion, due to non-assimilation are present and sometimes give rise to a highly wrought up emotional state which is so apt to transform objects by arousing different systems of elements and at the same time to impress the memory powerfully.

It is claimed that the very fact of dissociation brings about an intensification of ideational states converting them into sensory states. Physiologically, the assumption is made that the sensory nerve cells can be set into activity not only by peripheral stimuli, but also by central 'currents' going from center to periphery. The sensory centers are like a bucket with water, the upsetting of the bucket being likened to the upsetting of the sensory centers, giving rise to sensations. This upsetting can be affected by peripheral 'currents.' Small intracellular ideational currents flow freely through the centers without upsetting them. Now when an obstruction occurs in the sensory centers the ideational currents which otherwise flow out and disperse may accumulate, and aided by a chance activity of central character may upset the nerve cell in the same way as our bucket may be upset by the accumulation of water from the small incoming currents (like the ideational currents), when the holes and interstices through which they usually flow out are stopped up. The hypothesis as far as explanation goes is good enough, the drawback is that it explains too much. For it is hard to understand why intense dreams of this character do not occur more often.

Besides it is hard to realize how an idea can give rise to a sensation of any intensity by the mere agency of ideas, the sensation and its intensity being entirely a function of peripheral stimulation and consequent sense impressions. An idea, a representation, may be very vivid, but does not become a presentation or sensation. A sensation is not an 'intense' idea, nor is an idea a weak sensation. A series of sensations arranged in ascending or descending gradation of intensity may be likened to the continuous series of the spectrum in which there is a qualitative difference from line to line, a difference that admits of no substitution. A sensation the intensity of which is changed is a fallacious percept, a hallucination. A thunder clap perceived as a whisper, a whisper perceived as a thunder clap may be equally regarded as fallacious perception as any other change in the content of the percept. The rustling of leaves perceived as an explosion is as much of fallacious perception as when the paranoiac, for instance, hears in it curses and threats of his enemies. Sensations and percepts cannot change in content or intensity without giving rise to illusions or hallucinations. The changes that may occur in regard to sensations and percepts without their being qualitatively changed and becoming fallacious can only be in vividness belonging to the representative elements which cluster round the primary and secondary sensory elements. A less intense sensation may be more vividly represented than one of greater intensity. A weak sound, a pale color, a light pain may be more vividly represented than the ones the intensity of which is far greater. This vividness, however, is not at all a characteristic attribute of the sensory elements, it is rather an attribute belonging to the functioning system of representative elements into which the given sensory elements enter as constituent nuclei.

Keeping to facts as closely as possible we may venture without much risk on the following generalization which may be regarded in the light of a working hypothesis. Just as sensory primary or secondary sensory elements vary in intensity and can be arranged in a continuous series of gradations of intensities, so do the representative elements vary in vividness and may be arranged in a continuous gradated series of vividness. *Sensory elements have intensity, but no vividness, while representative elements have vividness, but no intensity.* Representative elements may refer to the same presentative content with different degrees of vividness. Vividness of representative elements like intensity of sensory elements may pass through all degrees of variation from *maximum* to *minimum* and finally reach a vanishing point. In this respect vividness is like sense intensity and as a matter of fact the two are usually interrelated.

Under ordinary conditions of psychic activity sensory intensity and representative vividness vary together. An intense sensation is vividly represented and a weak sensation less so, the vividness varying directly with increase or decrease of sensory intensity. This direct variation, however, is not always constant; there are conditions under which the two may part company such, for instance, as are found in states of distraction or in states of dissociation. Under such conditions a strong stimulation giving rise to sensory elements of great intensity may give rise to representative elements of but slight vividness. In states of distraction as well as in various states of mental dissociation sensations of great intensity may meet with so little vividness in the representative elements as to fall so to say below the threshold of consciousness, may be submerged into the twilight region of the subconscious and 'not be perceived at all.' From this standpoint we may say that the depth of dissociation varies inversely as the degree of vividness. When vividness is at its *minimum*, dissociation is at its *maximum*, and inversely. *Briefly stated, dissociation and vividness are inversely interrelated variables.*

VII.

Functional psychosis, the basis of which is dissociation, *may also psychologically be regarded*, according to the gravity of the psychopathic affection, *as a decrease or even loss of vividness of representative elements.* The diminution or total loss of vividness may be of different systems of representative elements and will thus give rise to various forms of psychopathic amnesias, which play such an important rôle in functional psychosis, which in the main is a disease of representative life consisting in a decrease of functional activity of representative elements and which from the present point of view may be regarded as *the tendency towards a minimum of the most important attribute of ideational elements, namely, vividness.*

From this standpoint, the degree of vividness of ideational elements can no more confer on them sensory intensity than the idea of riches, however vivid, can confer upon one the power of wealth. Dream hallucinations, like hallucinations in general, are sensory in character, not because of the intensive nature of the central elements or ideas, but because of the primary and secondary sensory elements present, directly and indirectly peripherally initiated, as it is in the case of all sensory and perceptive processes. Hallucinations are peripherally induced and are started either in the same sense organ, or indirectly in some other sense organ, the secondary sensory elements form so to say the hypertrophied portion of the hallucinatory percept, but *they are always sensory in character and peripherally initiated.* The more closely one investigates hallucinations, the more he learns to trace cases of supposed mysterious hallucinations to external peripheral sources. A pure central hallucination is as rare as the fabulous phœnix. A central hallucination means an unanalyzed psychic state. Whenever an analysis of such hallucinations is made, the peripheral sensory character, primary and secondary, stands out distinctly in the foreground. In the so-called 'purely central hallucination' the nuclear primary sensory elements remaining in the background of consciousness cannot easily be traced to their appropriate peripheral sense organs and their external stimuli and are on that account regarded as 'centrally initiated.' Dream hallucinations, hypnotic, hypnagogic and pseudo-hallucinations, if closely analyzed, can be clearly traced to peripheral origin, — to peripheral stimuli that give rise to primary sensory elements that form nuclei round which secondary sensory elements become organized as cytoplasm.

These so-called central hallucinations form the stumbling block of the psychologist and the psychopathologist. To account for them the theory is commonly advanced that the irritability of the ideational centers may reach such a pitch as to give rise to such intense ideational states as to amount to a full-fledged sensation or perception and thus bring about a pure central hallucination. It is strange that such a theory should be maintained at all and that it should gain currency. The theory does not accord with the facts, and its very principle disregards facts. For no matter what strength an idea may attain it is still far from becoming a sensation. An idea of a bell does not sound and an idea of a blow does not strike. The fact is, as we have pointed out before, ideas or representations are qualitatively different from sensations; an idea can as little be converted into a sensation as the sour taste of vinegar can be turned into violet color of the spectrum. Ideas and sensations differ fundamentally, they differ in kind and no amount of ideational activity can ever be made to become sensory in nature. A higher pitch of ideational activity will make an idea more vivid, but can nowise confer upon it sensory qualities, just as all the immensity of space and infinity or eternity of time can not make them weigh as much as a grain.

A further modification of the same theory is given by those who maintain that central hallucinations are due to the irritability of the higher ideational centers from which 'ideational' currents are propagated to the lower sensory centers. In other words, it is not the idea that becomes by its intensity or by its vividness directly transformed into a sensation, but an intense or vivid idea may give rise to a corresponding sensation without the presence of an external stimulus, or of a peripheral sensory process. Psychologically as well as biologically regarded, the theory is untenable. For it is not in accordance with observed facts that an idea, however vivid, should give rise to a corresponding sensation or percept. Were that the case the course of internal and external worlds would have become confused and confounded, man would have become the dupe of his own ideas, the world a gigantic madhouse, and the process of ideational activity would have long ago become eliminated in the struggle for existence.

From a physiological standpoint, the theory can hardly be considered, inasmuch as it is in direct opposition to the known physiological laws. Sensory excitation, ideational processes and motor reaction form, so to say, a sensory-ideo motor arc, — the excitation going from peripheral sense organs to central systems and thence to the muscles. Now the conditions postulated by the central theory are such as to have the processes reversed· Sensory processes work upward, from periphery to center, while motor processes work downward, from center to periphery. On the modified central theory, the sensory process in hallucinations is reversed, it goes downward instead of upward. There is not a particle of evidence for such reversal, the assumption being in contradiction to the principles of physiology. The claim of special structures for effecting such a reversal is entirely unfounded. As far as can be ascertained, the neuron works 'cellulipetally' in the direction of the sensory ganglia and central neuron systems, while the neuro-axon works 'cellulifugally' that is from sensory ganglia and central neuron systems to the periphery to the muscular apparatus. There is on the other hand not the least bit of evidence that the functions of neuron systems can be reversed in their course.

The central theory then cannot stand the test of critical examination as it is neither in accord with the facts it is called to explain, nor does it fall in line with the facts and principles of physiology. We are therefore forced to fall back on the peripheral origin of hallucinations under the condition of central dissociation. According to the theory advanced in this paper, the origin, and structure of hallucinations, of dream hallucinations as well as of pseudo-hallucinations and hypnotic hallucinations do not differ in the least from those of normal perception, a difference unwarrantly claimed by the theories of central origin of hallucinations. *Hallucinations are peripherally* *initiated, hallucinations are abnormal percepts occurring under the conditions of central dissociation with primary and secondary elements as their central nuclei.*

VIII.

The phenomena of so-called 'double thinking' are extremely interesting from our point of view. The patient hears his own thoughts uttered aloud. He has the hallucination of his thoughts uttered when engaged in writing or in reading, though loud reading may check the hallucinatory voices. These hallucinatory voices may be of an imitative character and simply repeat what is spoken or read by the patient; or they may be of an anticipatory character and utter the patient's thoughts before he himself utters them. The usual explanation of such cases is found in the theory of the so-called 'overcharged centers.' Where the voices follow and repeat the patient's words and phrases, it is assumed that the auditory centers are highly irritable and overcharged so that stimulations from other centers bring about a discharge into the 'ideational' auditory centers and auditory hallucinations result. In the case of reading, for instance, the visual image of the word awakens also an auditory image, but when the auditory centers are overcharged the visual images awaken directly an auditory image before the spoken word takes place. Now this auditory image is so intense, on account of overcharge, that it becomes an auditory hallucination and the patient hears his own thoughts uttered aloud. This reflex action from one 'ideational' center into another occurs while the patient reads or writes, and that is why he has the experience, the hallucination that there is a voice often regarded as 'inner' which repeats his own words and phrases. Cases where the voice utters the words and phrases before they are written are explained on the hypothesis that the central discharge into the overwrought auditory centers occurs before the words are written down or before the motor discharge takes place. When, however, the patient hears the voice repeat the phrases soon after he has uttered them, the phenomena are explained on the supposition that the centripetal currents from the speech centers into the auditory centers give rise to the voices, the patient hearing his own words shortly after he has uttered them, the efferent discharge from the graphic centers into the auditory centers will give rise to an auditory hallucination of hearing the words and phrases he has just written. In the phenomena of 'double hearing' the patient has the hallucination of hearing his own voice while talking or reading aloud, and then again another voice due to the centripetal discharge from the speech centers to the overcharged auditory centers. Thus in some patients these hallucinations of hearing are brought about by the voluntary suppression of speech, the patient then hears a voice uttering his own thoughts. This is claimed as confirming centra initiation — the currents from the word images in the speech centers not having a free outlet run into the overcharged 'ideational auditory centers' and give rise to inner speech heard by the patient.

In opposition to this central theory of double thinking or of 'inner speech' held in various forms by psychologists and psychopathologists, there are some who maintain the view that these 'double thoughts' hallucinations are not of central, but of peripheral origin, being due to hyperæsthesia of the centripetal paths. The apparatus employed in speech carries out not only the requisite delicate movements, but also forms the sensitive apparatus for information of the movements executed. The sense of movement may be regarded as originating in the muscles, especially in the joints and articular surfaces. Sensory stimulations coming from these structures to their appropriate central systems give rise to kinæsthetic sensations and motor ideas. Now if the peripheral sensory tracts of the muscle sense or of kinæsthetic sensations become hyperæsthetic, kinæsthetic sensations and motor ideas are aroused automatically and may give rise to hallucinations of positions, movements and acts; movements which have not been performed are thus experienced. If now the centripetal sensory tracts of the speech centers are hyperæsthetic then involuntary kinæsthetic sensations and respective ideas arise which go to form the hallucinations known as 'double thought.' The patient experiences 'inner' speech, a voice repeats after him his own thoughts, his own words and phrases. When the speech centers are overcharged and give rise to automatic centrifugal discharges, then the hyperæsthetic centripetal paths bring it back in the form of spoken words and the patient experiences his own thoughts uttered by an inner voice which is foreign to him. In speaking the inner voice comes after the speech and reverberates like an echo and persists as an 'after image' of the spoken word or phrase. When the patient is engaged in writing, the voice usually precedes the written phrase, because the spoken word image precedes the written word image, the inner voice thus anticipates the patient's writing by uttering his thoughts. This theory seems further to be confirmed by cases in which such hallucinations take place. If one observes closely cases of 'double thinking' or of 'inner speech,' he often finds 'involuntary whispering' present — the patient whispers to himself. These whispers come back to him, on account of the hyperæsthesia of the peripheral paths he hears it as speech of some inner voice.

A close examination of the two theories, of the central and of the peripheral, reveals their inadequacy. The central theory, as it is generally put forth and commonly accepted, may possibly be regarded as the more inadequate. For the central theory rests on the psychological fallacy, so prevalent in psychopathology that it may be regarded as the psychopathologist's fallacy, namely that an idea may reach such a high pitch of intensity as to become sensory in nature and give rise to a percept. The percepts formed by the visual perception of reading awaken, according to this theory, also accompanying ideas of sound intimately related associated with visual word reading, and it is these ideas that reach such a high intensity as to give rise to hallucinations of hearing, the words are read aloud, as if by a strange voice. This explanation, as we have already pointed out, is psychologically incorrect and rests on the fallacy that ideas have intensity and that an intense idea becomes a sensation, or that a sensation is but an intense idea and an idea is a weak, a faint sensation. To modify this view and assume that an intense idea stimulates and gives rise to the formation of a percept is to assume a supposition not warranted by facts that an idea is equivalent to the action of external stimuli or objects with their requisite physical structures and processes. In either case, the central theory as it stands is not in accord with psychological and physiological data and, as such, cannot possibly be accepted at least in the shape as it is usually put forth.

Furthermore there is an inherent difficulty in the central theory itself. For if it be correct, as the theory claims, that the visual image calls forth an intense auditory image amounting to a hallucination, the hallucinatory voice should precede and not follow the patient's reading. In order to explain the hallucinations of double thinking or of double hearing in the case when the voice follows the reading, it would have to be assumed first that the visual image of the written or printed word stimulates the speech centers, which, innervating the muscular apparatus of speech, give rise to reading, which in turn stimulates the peripheral auditory apparatus, awakening activity in the auditory centers, giving rise to the hearing of the read words, and that then only do the indirect stimulations of the visual image coming from the visual centers awaken once more the same central connections, thus bringing about a repetition of the self-same words heard. We have to assume that the action of the visual centers in stimulating the motor speech centers with the resulting acoustic stimulations and functioning activity of the auditory centers are enacted before the direct central stimulations from visual center to auditory center take place; in other words we must *assume central retardation.* Now what does this central retardation mean? It means that the phenomena of double thinking or of double hearing are brought about by some form of central inhibition, of central blocking of pathways as it is usually put: in other words, the requisite condition of double thinking is reduced to the psychopathological state of central dissociation.

The inadequate side of the central theory as it is commonly advanced lies in the supposition of its referring auditory hallucinations in the phenomena of double thinking or hearing to the intensification of the auditory image or idea, but no straining of an auditory image can get a sound out of it. Moreover, were the central theory correct it would really involve a double auditory hallucination, one preceding and the other succeeding the reading. For in the process of reading the visual image of the word awakens the auditory image along with its kinæsthetic image, stimulating the centrifugal motor apparatus and giving rise to the spoken word. Now this awakened auditory image preceding the spoken word, on account of the assumed irritability of the auditory centers and the consequent 'intensification' of the stimulated auditory images, should necessarily give rise to a full-fledged hallucination. When the voice also follows the reading, a dissociation of the visual from the auditory centers is assumed, a dissociation that gives rise to a secondary succeeding hallucination of the words and phrases read and spoken. The fact that the central theory requires the presence in all phenomena of double thinking that the voice should necessarily precede the reading; that when the voice follows the reading, another hallucinatory voice must have also preceded and that there is also a double stimulation from the visual into the auditory centers, that the hallucination first appears under conditions of association of visual and auditory centers, while the succeeding hallucination occurs immediately under the opposite conditions, namely dissociation, — all these assumptions make the central theory wholly unsatisfactory and unacceptable.

IX.

The peripheral theory of double thinking is on general grounds more acceptable as it falls more in line with psychological and physiological principles and facts. Unfortunately the special facts which the theory is called for to explain do not exactly tally with it and may even be said to contradict the hypothesis. For if the hallucinations of double thought are due to hyperæsthesia of the centripetal sensory-motor tracts, then reading aloud should intensify the hallucination, but the case is quite the reverse, — reading aloud makes the hallucinatory voice to disappear altogether. On this theory again, the voice should follow the reading. We are thus confronted with the opposite difficulty met with in the central theory. On the central theory the hallucination should precede, while on the peripheral theory the hallucination should follow the reading. The central theory cannot account well for succeeding hallucinations, while the peripheral theory does not account well for preceding hallucinations. On the central theory there should be double hallucinations in cases where the voice follows reading, while on the peripheral theory there should be double hallucinations, when the voice precedes the reading. Besides 'hyperæsthesia' alone should rob the perception of its hallucinatory character, the patient should be the more conscious of his own utterance.

A closer examination of the peripheral theory discloses a fundamental fallacy which it primarily involves, a kind of *ignoratio elenchi*. The theory is probably correct in principle, but it misses the essential point of the whole problem; it may be an adequate explanation for motor, but not for auditory hallucinations. Hyperæsthesia of the central motor speech tracts would at most give rise to pure kinæsthetic hallucinations. The patient may have hallucinations of action, tension, or of movements in his peripheral speech organs, but he will have no hallucinations of hearing. To have an auditory hallucination, as to have an auditory perception in general, the auditory peripheral and central apparatus should be stimulated. No other organ but the acoustic apparatus can possibly supply sensations and percepts of an auditory quality, unless the hallucination be of a reflex secondary character, but then it may be induced through any other peripheral source than that of kinæsthesis of the speech organs.

Although each theory taken by itself proves to be inadequate and leads to contradictions and puts us out of accord with facts, still the two may be regarded in a certain sense as supplementing each other, if modified by supplementary conditions. Now the central theory emphasizes the aspect of the central character of the phenomena, while the peripheral theory lays stress on centripetal factors; both, however, can be brought in line with facts, if assuming centripetal factors of kinæsthesic and specially auditory hyperæsthesia we also refer to the central conditions of dissociation. The patient in double thinking is subject to subconscious states, to states of dissociation; this dissociation is of central character and specially affects the visual and kinæsthetic systems. Impressions, on account of dissociation and peripheral hyperæsthesia, are subconsciously received and subconsciously reacted upon. The visual impressions of the written and printed characters are subconsciously perceived and subconsciously uttered in a whisper and sometimes quite loud, as I had occasion to observe in a case of mine. This subconscious utterance, unperceived by the patient, comes back to him as a strange external voice proclaiming the patient's thoughts or repeating his words and phrases. The hallucinations of 'double hearing' are due to subconscious whispering which comes back to the patient as an auditory hallucination. I had the occasion to verify this phenomenon of subconscious whispering in a case in which functional dissociation was quite marked and in which auditory hallucinations and double thought were quite persistent.

In cases where the auditory hallucinations precede the reading or writing it is the subconscious whispering along with kinæsthetic and auditory hyperæsthesia that give directly rise to the phenomena of 'double thought,' or of 'double hearing.' The dissociation being in the kinæsthetic systems the patient does not experience consciously the peripheral incoming sensations due to his subconscious whispering. More often the patient continues to whisper subconsciously what he has just read consciously. Such a habit is common with many people in the normal state and is due to the result of the persistence of the peripheral sensory impression, to a kind of verbal after-image. The absence, however, in the normal condition of dissociative states prevents the formation of subconscious whispering with its consequent auditory hallucinations partly due to hyperæsthesia of the auditory tracts.

If cases of 'double thinking' are closely examined one finds in them the presence of subconscious states with their psychomotor reactions, the patient in walking on the street, for instance, may hear a voice telling him words and phrases that can be traced to signs and advertisements which he has read subconsciously though he himself has not been aware of it. What happens in such cases is this, the patient whispers or even utters aloud the words he subconsciously sees on the signs. It is this subconscious whispering that comes to him back as an auditory hallucination of a voice. In one of my cases in which the patients suffered from auditory hallucinations I found on close examination the phenomenon of unconscious or subconscious whispering, which became very much aggravated in proportion to the state of distraction in which the patient was, ranging from an almost inaudible whisper to a loud talk, the patient being entirely ignorant of it and could not be made aware of it, even when the attention was fully called to his talking. One of my patients suffering from pronounced auditory hallucinations, but in whom the dissociation is not deep, aptly describes his experiences as 'autovocalization.'

Similar conditions can be induced in hypnosis thus confirming our point of view by experiment on otherwise normal people. If a post-hypnotic suggestion of subconscious whispering is given, the subject experiences an hallucination analogous to that of 'double thought,'—the subject hears a voice telling him the words and phrases which he himself whispers, but of which he personally is entirely ignorant. The peripheral character of the 'double thought' or hallucination under condition of central dissociation may thus be regarded as an efficient working hypothesis in accord with facts.

From the whole course of our discussion it appears that we remain more closely in touch with facts, if we accept the view that hallucinations require states of dissociation as central condition and that they are primarily peripherally initiated having secondary sensory elements as their main content; in other words, hallucinations are *dissociated secondary percepts.*

DISCUSSION.

THE MECHANISM OF IMITATION.

The object of this paper is to explain the psycho-physical process by which acts of imitation are performed. We often hear imitation spoken of as an instinct, and of course, in the loose popular sense of the term, there may be no objection to speaking of the tendency to imitate as instinctive in mankind. But if we use the term instinct in a technical sense as applying to those acts which are made possible by hereditary paths of discharge in the nervous system, it seems impossible to bring our acts of imitation under this head. We can understand how a definite reflex might become hereditary. The act of swallowing when something is placed in the mouth, is a perfectly definite sensori-motor reflex, and we can easily conceive that the nerve arrangement necessary for this act should be prenatally established. But in the case of imitation we have no single definite act, but an indefinite variety of actions. No one definite set of nerve adjustments could explain such different activities as the imitation of sound, the imitation of a movement of the hand, and an imitation of the shake of the head. The very use of the single term imitation for such a large variety of actions, has a tendency to mislead. Many writers speak of imitation as if it were a sort of faculty, and this leads to the usual error of faculty psychology — the individual act is thought to be sufficiently explained when it is shown to be an instance of the general activity of the faculty. As James says there is no such thing as memory, but only memories, so we might say there is no such thing as imitation, but only imitative actions.

Our instincts are sensi-motor, our imitations ideo-motor. Since our ideas are not hereditary, but are always individual acquisitions, it seems necessary to regard the actions especially associated with these acquired ideas as also acquired. A movement which depends upon the perception of another's doings cannot be more original than the act of perception itself. In speaking of perception, Professor James says, "Nature is frugal in her operations, and will not be at the expense of a particular instinct to give us that knowledge which experience and habit will soon produce. Every perception," he says, "is an acquired perception."[1] Now this, it seems to me, applies to imita-

[1] *Principles of Psychology*, Vol. II., p. 78.

tion as well as to perception. Nature does not furnish us with particular instincts to give us those actions which experience and habit will soon produce. Every imitation is, I believe, an acquired imitation.

Many recent writers seem, however, to regard imitation as hereditary and instinctive. Thus Professor James himself includes imitation in his enumeration of the prominent instincts of man.[1] Yet he shows an apprehension of something other here than a single instinct by distinguishing 'the instinct to imitate sounds,' from 'the instinct to imitate gestures.' Had he carried this differentiation further he would soon have seen that the great variety of imitative acts precludes all thought of their reference to one or two hereditary instincts. Royce, in his recent *Outlines of Psychology*, speaks of the sources of the social interest, as instinctive and hereditary, and then goes on to say, "On the basis of the general social interests there appear more special instincts amongst which the most prominent is the complex of instincts suggested by the name imitation."[2] Tarde, in his *Laws of Imitation*, calls imitation the 'action at a distance of one brain upon another.'[3] He speaks of 'a will to imitate' as being handed down,[4] and he quotes from Maudsley with apparent approval the statement, "It cannot be too clearly apprehended that there is a sort of innate tendency to mimicry in the nervous system."[5] Baldwin's view is not entirely clear to me. He seems to regard imitation as an acquisition of the race, but as an instinct in the individual. At any rate he speaks of the 'imitative instinct,' and again refers to 'the instinct to imitate' as found in the child and in animals.[6]

In contrast with these writers who more or less definitely pronounce imitation an instinct, we may note this statement of Stout in his just published work, *The Groundwork of Psychology*. "Both spontaneous and deliberate imitation," he says, "presupposes a motor association between the perception or idea of the act to be imitated, and more or less similar movements which the child has already learned to perform."[7] Stout, it seems to me, is on the right track. He does not explain, however, the way in which these motor associations first arise. These motor associations, it seems to me, may be explained as follows: In same cases they are based on instinct. The

[1] *Ibid.*, p. 408.
[2] P. 275.
[3] G. Tarde, *Laws of Imitation*, trans. p. 199.
[4] *Ibid.*, p. 193.
[5] *Ibid.*, p. 88.
[6] Baldwin, *Mental Development*, p. 290.
[7] P. 82.

child instinctively cries at its own pain, and thus associates the sound of the cry with the cry movements. This association established, it becomes then perfectly natural, on the principle of ideo-motor action, for the child to cry when he hears another cry. In other cases the association between the idea and the movement may have been established by purely random activities. The spontaneous overflow of nervous energy makes the child wave his hands — he then sees the hand movement, and thus establishes a path of connection between the ideational process and the corresponding motor process. With this connection established, it is easy to understand how the child comes to wave his hand when he sees another perform the same act. In some cases probably the associations on which the imitative act depends may depend upon the imitations of others. Take the case of the imitative smile. The child cannot see his own smile and thus form a visual image of it, as he can of his hand movements. But the mother smiles when she sees the smiling face of the babe, and the child thus associates its own motor feelings of the smile with the visual idea of the smile as seen reflected in its mother's face. The necessary association is now established, and thereafter the child readily smiles when he sees others smile. In the case of vocal sounds while the imitation of the child by others may not be an absolutely indispensable factor, it is yet of great service in establishing the motor associations. To hear its own sound uttered by another, calls attention to it, makes its auditory image more definite and vivid and thus associates it more firmly with the movements of articulation. By means then of his original instinctive reflexes, his random movements, and the imitations of his movements, which he sees made by others, the child forms a sufficient fund of motor associations to explain his imitative acts, without the need of assuming any special instinct, endowment or faculty of imitation. Thus far we have considered only the first simple imitations of the child. But if these are not instinctive certainly none are. The more elaborate and complex imitations of later life are simply combinations and coördinations of the elemental movements already learned. As imagination can create nothing for which the ideational experience of the past does not supply material, so imitation can perform no act for which earlier motor experience does not furnish the elements.

Closely connected with imitation is sympathy. This connection is illustrated by an instance of my own son's babyhood. Whenever we repeated to him the nursery ditty — 'This little pig goes to market,' etc., he would always break out crying when we said the last line —

'This little pig cried Wee, Wee, Wee! I can't find my way home.' The 'wee, wee, wee' uttered in a somewhat pathetic tone of voice produced a cry which was certainly imitative, and perhaps in some measure sympathetic. One other example may be given — an incident related to me by a friend of his little two-year-old daughter. One day as the father was holding the child in his arms playing with her, she got quite a hard bump against his forehead and began to cry. To distract her attention, he took her hand, patted his forehead with it, and said, 'poor papa, poor papa!' The next day the child got a bump against a chair, and her father was much surprised to see her pat the chair and hear her say 'poor chair!' Certainly no one would claim that we inherit an instinct to sympathize with injured chairs and I believe there is as little ground for making sympathy an instinct in any case.

James again counts sympathy among the instincts,[1] and Baldwin regards organic sympathy as instinctive.[2] But much the same difficulties arise when we try to think how the vast variety of our sympathies can be accounted for by an inherited nervous mechanism as in the case of our imitations. In order to explain the first sympathetic feelings we need only to add to imitation the James-Lange theory of emotions. As already explained, the child cries when he hears another cry. He sees another in an attitude of pain or grief and imitatively assumes that attitude, but the assumption of the attitude excites the corresponding emotion and the child thus acquires the sympathetic feeling. But even if we do not accept the theory that the 'expression' makes the emotion, it is still unnecessary to regard sympathetic feelings as instinctive. The child does not feel the woes and joys of others until he has already experienced at least elemental feelings of the same sort in his own person. Having established then a certain fund of association between feelings and their outward expression, the imitation of the expression of feeling that he sees in others will awaken the kindred emotions in his own breast.

One objection to this experiential theory of imitation should be considered. It seems to be the social psychologists, and those who have approached the subject from the sociological side, who are especially inclined to regard imitation as an instinct, an endowment, or a quasi-faculty. When we think of the tremendous rôle which imitation plays in the development and education of the individual and in the entire organization and structure of society, can we regard it as any-

[1] *Principles of Psychology*, Vol. II., p. 410.
[2] *Social and Ethical Interpretations*, p. 222.

thing less than a native endowment? Or, to put the question more concretely, if imitation is only one type of ideo-motor action, why should it acquire such a preponderant influence? Why should the ideas enacted before us in the doings of our fellows take precedence in their influence on conduct over the numberless ideas of all sorts that crowd the mind? In answering this question we may note first that when a number of motor ideas are in the mind it is the one which is most interesting, the one which is most vividly apprehended, or which most forcibly strikes the attention, that is sure to be executed. From the first experience of parental care, to the last days of life, our social relations are of the utmost importance to us. There is therefore a constant and accumulative development of social interests. Where our interest is there our attention is. The child might wave its arms in imitation of the waving branches of a tree were it as interested in the tree as in the movements of its parents, brothers and sisters. Then too the movements actually enacted are much more clearly and vividly apprehended than those that are suggested by mere words or ideas that arise in mind. In fact many activities that can readily be understood when seen, can hardly be described at all. The gymnasium instructor may be quite unable to tell in words how he performs a certain feat, but by performing the feat in the sight of his pupils, they get a far clearer apprehension of it than words can possibly give. Moreover the very infertility of our minds as to ideas of conduct, makes us prone to follow the examples presented before us. We are endowed with a store of energy, we are bound to act, and given the proper motor associations the doings depicted before us offer the line of least resistance to our conduct. Lack of originality is as manifest in actions as in ideas, and it is therefore not to be wondered at that the doings made vivid by the acts of others, and enforced by the growing power of our social interests should have a dominant influence on our conduct. There is nothing then, it seems to me, in the importance of the rôle which imitation plays in individual and social life to invalidate this simple experiential theory of its workings.

F. C. French.

University of Nebraska.

N. S. Vol. XI. No. 3 May, 1904

The Psychological Review.

THE LAW OF ATTRACTION IN RELATION TO SOME VISUAL AND TACTUAL ILLUSIONS.

BY PROFESSOR HAYWOOD J. PEARCE, PH.D.,
From the Psychological Laboratory of Brenau College.

In working over the results of some experiments which were published under the title, 'Ueber den Einfluss von Nebenreizen auf die Raumwahrnehmung,'[1] in which was demonstrated a tactual illusion similar in nature to the Müller-Lyer visual illusion, I observed that the influence of a secondary stimulus (Nebenreiz) in producing an elongation of a primary linear stimulus was directly proportional to the intensity of the secondary stimulus and inversely proportional to the square of the distance between the two stimuli. The number of specific instances upon which this observation was based was somewhat small and the number of variations in distance and in intensity of secondary stimuli were very limited. Moreover, data for determining the influence upon the result which might be occasioned by the variation in intensity of the primary stimulus were almost wholly lacking. On account of these and other similar deficiencies, it was not thought wise at the time of the former publication to propose a hypothesis of such apparently far reaching significance.

Starting, however, with this hypothesis in mind I have directed numerous other experiments, designed to reveal the exact relation between primary and secondary stimuli and the effect in perception of the one upon the other. Inasmuch as it was so clearly shown[2] that the tactual illusion was similar in

[1] *Archiv fur die gesamte Psychologie*, Vol. I., pp. 31–109.
[2] *Ibid.*

almost every detail to the visual illusion, I selected the latter for the investigation. The visual figure lends itself more readily to small and numerous variations, the mean variation in any series of judgments is much smaller and it seems to me that the operation of a law such as that indicated would be more easily detected because more uniform in its manifestations.

The present paper, therefore, is a report of some experiments made for the purpose of determining quantitatively the influence exerted by secondary visual linear stimuli upon a primary visual linear stimulus, or perhaps more accurately stated, the attempt is to determine in mathematical terms the attractive force operative between two visual stimuli. It will be understood, of course, that the limitations of language confine us to the use of the term 'stimulus.' If there be, in truth, any attractive force operating between the actual objective stimuli, it is certainly not our present purpose to determine that. Stimulus, as I have used the term, represents an 'impression' made upon a sense organ. It is not yet, necessarily, sensation; it is certainly no longer stimulus. It is rather a middle state, viz., a state of the nervous system occasioned by objective stimulus and the conditioning element in sensation.

I began, first, a series of experiments with a figure of the Müller-Lyer type. The projecting arms were turned outward, and separated from the central line, or primary stimulus, by small open spaces as represented in Fig. 1 (Plate I.).

In general, the results were of the sort which I had expected, but it soon became manifest that another factor in addition to distance and sensation intensity was playing a part in the results. It is a well-known fact that the Müller-Lyer illusion varies with the cosine of the angle formed by the projecting arm and the central line.[1]

In order to eliminate this third variable factor, I abandoned the Müller-Lyer figure entirely and constructed a figure with one central linear stimulus and two other simple linear stimuli, which I have termed the secondary stimuli. The secondary stimuli were constructed exactly in the line of direction of the

[1] Heymans, *Quantitative Untersuchungen ueber das optische Paradox* in *Zeitschrift f. Psychol.*, Vol. IX., p. 221.

FIG. 1.

FIG. 2.

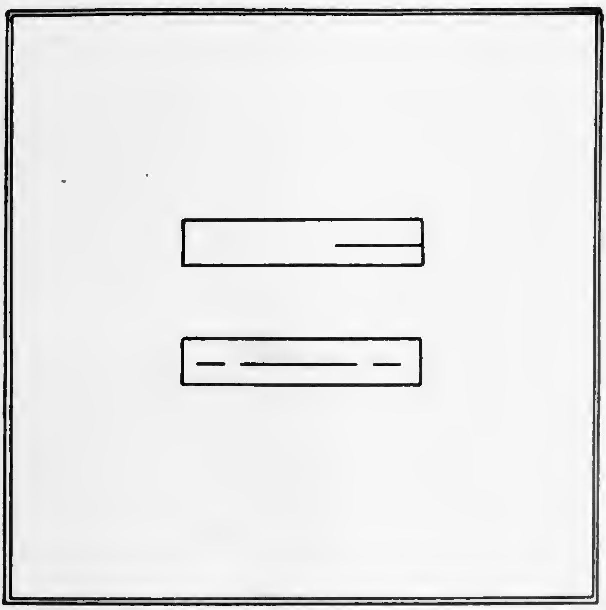


FIG. 3.

central, or primary, stimulus but separated from it by small open spaces.

The accompanying Fig. 2 makes clear this construction. AB is the primary stimulus, EC and DF are the secondary stimuli. The distance between primary and secondary stimuli is measured of course from center to center and is xy in the figure.

It will be observed that several radical variations of this figure may be made. We may vary (1) the distance xy, or (2) the stimulus AB, or (3) the stimuli EC and DF, retaining in each case all other factors constant. The results of our experiments, therefore, fall naturally into three groups: (1) The effect of secondary stimuli at different distances, (2) the effect of secondary stimuli when the primary stimulus is varied, (3) the effect of secondary stimuli of different intensities upon primary stimuli of a constant intensity and at the same relative distances.

Method of the Experiment.

Preliminary experiments were conducted in order to determine the best method. (1) A card upon which was drawn the figure to be judged was given the subject and, in addition, a series of cards containing each a single line but of different lengths. The subject was required to select from the series of cards the one containing the line apparently equal in length to the primary stimulus in the figure. (2) Instead of a series of cards, a single card upon which was drawn a series of lines of different lengths was used. The subject was required to designate in the series of lines the one which appeared equal to the primary stimulus. (3) Instead of a series of lines, a single long line was drawn upon a piece of cardboard, and the cardboard was adjusted to slide back and forth through a slit cut in another piece of cardboard. By this means it was possible for the subject to make the line longer or shorter until it seemed to him equal in length to the primary stimulus.

The method finally adopted and which I think will be recognized as the most convenient and accurate of the four methods which were tried, was as follows: A frame, three feet square, was hung upon two upright posts which projected three feet above a low table. In this frame was fixed a square of cardboard containing near the center two rectangular openings. On the rear side of the frame and parallel to the openings were tacked wooden runners or grooves, so adjusted that one could slide certain cards, containing the figures to be judged, into their proper positions filling the rectangular openings just referred to. When in position and ready for the experiment, the frame appeared to the subject as shown in Fig. 3. The upper single line could be lengthened or shortened by sliding the card back and forth. On the back of this card was a millimeter scale, so arranged that the experimenter could read immediately the length of the line as it appeared to the observer. The experimenter, seated behind the screen at the table, could move the card easily back and forth and record immediately the reading of the millimeter scale, which registered the judgment of the observer.

The observer was seated in front of the screen at a distance of 80 cm. His task was to observe the moving upper line and the lower stationary figure at the same time and to say 'stop' as soon as the *difference* between the upper line and the lower primary stimulus *ceased to exist*. This form of instruction to the observer was adopted because it was noted that if told to say stop when the two lines appeared to be equal, the subject adopted, somewhat irregularly, either of two courses: (1) She said 'stop' when the difference *ceased*, or (2) having allowed the variable to pass the point where the difference ceased, she said 'stop' not until she began to perceive a difference in the other direction. In order to secure relative constancy, the former type of reaction was insisted on. In half the experiments constituting a series, the moving, variable line was gradually lengthened and in the other half of the series this line was gradually shortened.

Method of Estimating the Influence of the Secondary Stimulus.

It seemed natural at first thought to estimate the influence of the secondary stimuli as equal to the difference between the length of the primary stimulus as given by objective physical measurement and the length of a second line which is judged by the subject to be of the same length.

Preliminary experiment, however, showed clearly that when a subject attempts to estimate the length of a single line (without secondary stimuli), using the method above described, the judgment is always too small, *i. e.*, the line is always judged to be shorter than it actually is. Consequently, in order that the secondary stimuli may produce subjectively an elongation of the line objectively given, the tendency to shorten just observed, must first be overcome. Inasmuch as the addition of a secondary stimulus accomplishes this, we must include this in our estimate of the influence of the secondary stimulus. Accordingly in every series of judgments of a line accompanied by secondary stimuli, I have required evenly distributed judgments of the same line without secondary stimuli. The results, therefore, which appear in the tables as 'influence of the secondary stimulus' always represent the difference measured in centimeters between the judgment of the line without secondary stimuli and the judgment of the length of the same line with secondary stimuli.[1]

The foregoing is in general the method of experimentation and computation of results employed in each of the three groups of experiments which follow. In connection with each group some further details of method must be pointed out.

[1] Professor Judd (*Genetic Psychology for Teachers*, p. 11) has attempted to explain the fact that if one tries to draw upon paper a line equal in length to a copy upon the blackboard, he invariably makes it too short, as due to the larger environment represented by the blackboard as compared with the smaller environment represented by the sheet of paper. The facts brought out in my experiments seem to throw serious doubt upon the adequacy of Professor Judd's explanation. In my experiments, the environment of the two lines judged to be equal was the same, and moreover, the error remained the same when the relative position of the two lines was reversed.

The explanation of the error is probably as follows: When I am comparing two lines, one standard and the other variable, the latter is the one which is kept most prominently in the foreground of attention. The eye wanders to the standard only to renew the memory of its length. What actually happens is a comparison of a present vivid, intense sensation with a fading memory image. I may fixate the standard but by the time my eye reaches again the line which I must make equal to the standard, the latter has become a memory image, or at best appears upon the periphery of vision and consequently has less sensation value than the same image upon the fovea centralis or in the focus of attention.

The Effect of Two Secondary Stimuli Upon a Primary Stimulus When the Distance is Varied.

I present first three tables, I., II. and III., showing results for a primary stimulus of 16.0 cm., 17.0 cm. and 18.0 cm., respectively, in length. The experiments with the three different primary stimuli, though recorded in different tables, were conducted simultaneously. For example, a card containing a primary stimulus 16.0 cm. and secondary stimuli 9.5 cm. distant was presented to the subject. Five successive judgments of this same line were required, the variable line being first lengthened and then shortened and so on alternately for the five judgments. Then a second card containing primary stimulus 17.0 cm. was presented and five successive judgments of this line in a similar order were required. Then primary stimulus 18.0 cm. was presented and the same judgments required. Now we return to primary stimulus 16.0 cm. but one in connection with which the distance of the secondary stimuli has been slightly increased, viz., 10.0 cm. Then the series 17.0 cm. and 18.0 cm. with similar increase in distance of secondary stimulus are taken, and then back again to primary stimulus 16.0 cm. with distance of secondary stimuli still further increased and so through the entire series of five variations in distance for each of the three primary stimuli. Including the three cards which contained only a single line each, to which reference has previously been made, there were eighteen different cards and five judgments of the stimulus on each were required. Such a series could be made in about a half hour, which was the length of a setting for each subject.

At the second sitting, the experiment was conducted in a similar manner, except that the detail in every particular was reversed. Two sittings afforded a series of ten judgments each for each of the eighteen primary stimuli. The tables show results for ten different subjects and each result given is the average of ten individual judgments made at two different sittings. There are two such series for each subject and the general average for the ten subjects represents in each case two hundred individual judgments.

The subjects used were of varied age and character. One is my colleague, Professor Essary, of the department of biology, to whom I am under especial obligation; another was a student in the department of psychology; a third was a special student of painting, and a fourth was a special student of music. The remaining six were taken indiscriminately from the preparatory school of Brenau College and vary in age from ten to fourteen years. All except the first mentioned are female.

TABLE I.

INFLUENCE OF TWO SECONDARY STIMULI, EACH 2.0 CM. LONG UPON A PRIMARY STIMULUS 16.0 CM. LONG AT DISTANCES 9.5, 10.0, 10.5, 11.0 AND 13.0 CM.

Distances.		9.5 cm.	10.0 cm.	10.5 cm.	11.0 cm.	13.0 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.49	1.18	1.11	.46	.04
	2	1.61	1.16	.81	.32	.08
Pa.	1	1.17	.81	.72	.18	—.01
	2	1.18	.71	.90	.06	—.18
B.	1	1.06	1.10	.78	.46	.35
	2	.86	.54	.31	.31	—.04
C.	1	1.49	1.46	1.35	.84	.47
	2	1.75	1.47	1.27	.92	.56
H.	1	2.16	1.59	1.51	1.00	.73
	2	1.92	1.90	1.81	1.56	1.08
Pi.	1	1.57	1.42	.92	.52	.29
	2	2.00	.82	.69	.98	.18
Pr.	1	3.30	3.07	2.57	1.78	1.10
	2	1.29	.87	.55	.09	—.22
G.	1	2.09	1.56	1.29	1.03	.24
	2	1.88	1.74	1.29	1.06	.77
Pp.	1	2.02	1.81	1.53	.95	.74
	2	1.97	1.64	1.16	.95	.71
Hn.	1	1.98	1.88	1.50	1.30	.89
	2	2.03	1.55	1.43	1.31	.56
Average.		1.74	1.41	1.18	.80	.44
$E \times D^2$		157.03	141.00	130.09	96.80	74.36

The first three mentioned had some knowledge of optical illusions and the first two were acquainted in part with the hypothesis upon which I was working. The others had no knowledge of the nature or object of the experiment except that which was gained as a result of their own observation in the progress of the same.

Turning to an examination of the results shown in the tables we find that, with a very few exceptions, there is a uniform decrease in the influence of the secondary stimuli corresponding to an increase in the distance between the primary and secondary stimuli. The majority of the exceptions to be noted will be found in Table II., in which are shown the results for primary stimulus 17.0 cm.

TABLE II.

INFLUENCE OF TWO SECONDARY STIMULI, EACH 2.0 CM. LONG, UPON A PRIMARY STIMULUS 17.0 CM. LONG AT DISTANCES 10.0, 10.5, 11.0, 11.5 AND 13.5 CM.

Distances.		10.0 cm.	10.5 cm.	11.0 cm.	11.5 cm.	13.5 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.15	.75	.93	.25	.44
	2	1.42	1.11	.53	.15	.00
Pa.	1	.73	.97	.57	.20	.19
	2	.96	.68	.72	.40	—.16
B.	1	1.06	.95	.46	.05	—.18
	2	1.13	.93	.61	.28	.27
C.	1	1.74	1.45	.64	.23	—.60
	2	1.54	1.31	1.42	1.12	.50
H.	1	1.70	1.60	1.18	1.04	.18
	2	1.68	1.49	1.60	1.28	.89
Pi.	1	1.05	.57	.56	.37	.18
	2	1.41	1.01	.44	.07	—.09
Pr.	1	1.75	1.61	1.49	1.00	.66
	2	1.67	1.08	1.11	.41	.46
G.	1	1.94	1.27	.90	.71	.20
	2	1.41	1.22	1.16	.81	.98
Pp.	1	1.83	1.58	1.06	.83	.46
	2	1.62	1.39	1.17	1.02	.40
Hn.	1	1.79	1.41	1.45	.99	.47
	2	1.76	1.26	1.17	.62	.45
Average.		1.47	1.18	.91	.59	.29
$E \times D^2$		147.00	130.09	110.11	78.03	52.85

The only explanation which I can offer for the greater irregularities manifest in Table II. is the fact that a stimulus 17.0 cm. cannot be distinguished with certainty from either 18.0 cm. or 16.0 cm. and inasmuch as the 17.0 cm. stimulus in the order of the experiment follows sometimes the 16.0 cm. and sometimes 18.0 cm. stimulus, the judgment when it related to the 17.0 cm. stimulus was unequally influenced by the preceding

TABLE III.

INFLUENCE OF TWO SECONDARY STIMULI, EACH 2.0 CM. LONG, UPON A PRIMARY STIMULUS 18.0 CM. LONG AT DISTANCES 10.5, 11.0, 11.5, 12.0 AND 14.0 CM.

Distances.		10.5 cm.	11.0 cm.	11.5 cm.	12.0 cm.	14.0 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Influence in cm.
E.	1	1.43	.71	.28	.19	—.17
	2	1.53	1.23	.77	.27	—.23
Pa.	1	.64	.45	.29	.41	.16
	2	.76	.62	.41	.29	—.28
B.	1	1.32	1.12	1.25	.60	.58
	2	1.50	1.19	.67	.50	.01
C.	1	1.76	1.58	1.27	.86	.01
	2	1.33	1.17	.91	.90	.45
H.	1	2.18	2.11	1.60	1.16	.18
	2	2.55	2.40	1.80	1.37	.98
Pi.	1	.82	.66	.35	.26	—.15
	2	1.99	1.78	1.19	.78	.02
Pr.	1	3.15	3.02	1.61	1.11	1.34
	2	1.23	.92	.49	.59	—.07
G.	1	2.40	1.96	1.66	1.37	.16
	2	2.06	1.41	.89	.51	.53
Pp.	1	2.30	2.46	1.98	2.01	1.60
	2	1.49	1.13	1.08	.46	.25
Hn.	1	1.38	1.19	1.51	.57	.63
	2	1.42	1.02	.68	.52	.30
Average.		1.66	1.41	1.07	.75	.32
$E \times D^2$		183.01	170.61	141.51	108.00	62.72

judgments relating to the 18.0 cm. and the 16.0 cm. stimuli. The subject was particularly liable to such confusion, because she was not informed as to the number of primary stimuli which were used, nor that a primary stimulus of different length was always introduced when the cards were changed.

Considering the general averages of all results for each distance, we find that the irregularities referred to have been eliminated and a consistent decrease in influence corresponding to each increase in distance is manifest. For example, the influence of two secondary stimuli, each 2.0 cm. in length, acting upon a primary stimulus 18.0 cm. in length at a distance of 10.5 cm. is found to be 1.66 cm.; at distance 11.0 cm. the influence of the same secondary stimuli is 1.41 cm.; at distance 11.5 this influence has diminished to 1.07 cm.; at 12.0 cm. distance the influence is 0.75 cm. and at 14.0 cm. it is 0.32 cm.

influence. This same degree of regularity is manifest in each of the other tables.

An attempt to establish anything like an exact proportion between the decrease in influence and the increase in the square of the distance was a failure. It became at once apparent that the decrease in influence was far more rapid than the increase in the square of the distance.

The foregoing fact directed attention to another principle, viz., the intensity of visual stimuli decreases as the stimulus is moved toward the periphery of the retina. There are not, within my knowledge, any recorded experimental data which directly confirm this last statement, and, indeed, the well known device of the astronomer of using the periphery of the retina in order to bring to view an otherwise indiscernible star seemed at first thought in direct contradiction to such a statement. This astronomical devise, however, only shows really that the periphery of the retina may under favorable conditions, be more sensitive to very *faint* stimuli than the fovea centralis and can be explained by the fact that the fovea centralis being constantly bombarded by intense stimuli becomes insensitive to very weak ones.

On the other hand, the common facts of every day experience that we see most distinctly when the stimulus falls upon the center and less distinctly when it is moved toward the periphery, together with the well known facts of nerve distribution upon the retina afford sufficient confirmation of the statement that the intensity of the same objective stimulus decreases as the stimulus is moved toward the periphery.

Applying this principle to the case under consideration, we see that when the secondary stimuli are removed to a greater distance from the primary stimulus, they are removed at the same time towards the periphery of the retina, inasmuch as the eye maintains the same position relative to the primary stimulus. We have, therefore, at the same time, an increase in distance and a decrease in intensity of the secondary stimulus, although it remains objectively the same length. Both of these factors thus entering into the conditions of our experiments call for a decrease in the influence of the secondary stimulus, according to our hypotheses, and the very rapid decrease to which attention was called is, so far, in confirmatory of rather than contradictory to this hypothesis.

If our hypothesis is valid, it follows that the influence exerted by a stimulus A multiplied by the square of its distance would equal the influence of a stimulus B multiplied by the square of its distance. In other words, $E \times D^2 = C$, in which E represents the influence of any secondary stimulus, D is the distance of that stimulus and C is a constant.

As has already been shown, the value of C in the results previously recorded is not constant. For example, in Table I. the value for the five distances decreases from 183.01 to 62.72. This rapid decrease was due to the very rapid diminution of the value of E, and this last we have attributed to the decreased intensity of the 2.0 cm. stimulus occasioned by its removal towards the periphery, in addition to the increased distance.

In order to compensate for this decrease in intensity of the secondary stimulus, I prepared a new series of figures in which the same primary stimulus and the same distances were employed as in the former experiments, but the length of the secondary stimulus was altered. The amount by which the secondary stimulus should be altered in length was determined as follows: I selected arbitrarily one value of C, viz., that shown in the second column of each table of results. I then determined for each distance what the value of E should be, using the value of C selected as a constant. I was thus enabled to determine what effect a secondary stimulus of the same subjective intensity should have at different distances.

Now at a given distance, we know by experiment the effect of a secondary stimulus 2.0 cm. in length; we also know for the same distance, by computation as above shown, what the effect of a secondary stimulus of a certain standard intensity ought to be. The problem is to determine how much the secondary stimulus shall be lengthened or shortened in order that it may have the same subjective intensity as the standard.[1]

For lack of a better, I adopted the purely objective method of solving this problem, using increase in objective length as

[1] Compare Weber's Law.

equivalent to increase in subjective intensity. For example, referring to Table I., we select the influence of a secondary stimulus of 2.0 cm. at a distance of 11.0 cm. as the standard. The value of C (see second column) in this case is 170.61. We have assumed that this value should be a constant, if the intensity of the secondary stimulus remained constant. But we find that the value of C when the distance of the 2.0 cm. stimulus is 11.5 cm. (see third column) is only 141.51, the actual influence of secondary stimulus being only 1.07 cm. Now if the value of C were constant the actual influence of secondary stimulus ought to be 1.29 cm., provided the intensity of our 2.0 cm. stimulus had remained the same. This conclusion is reached as follows: The value of C should be 170.61, but as a matter of fact it is only 141.51. This indicates that the influence of secondary stimulus (1.07 cm.) is less than is to be expected of a secondary stimulus equal in intensity to that one which we have selected as the standard (second column) and, indeed, 1.07 cm. is as much less than the influence of a secondary stimulus of standard intensity ought to have been, as 141.51 is less than 170.61. In other words $(170.61 \times 1.07 \text{ cm.}) \div 141.51 = 1.29 \text{ cm.}$ which is what the influence of a seconday stimulus of standard intensity ought to be at the distance 11.5 cm. Further, if a secondary stimulus 2.0 cm. in length has produced an effect of 1.07 cm., how long must the secondary stimulus be in order that it may produce an effect of 1.29 cm.? Proceeding according to the objective method, this question is answered by the following arithmetical operation: $(1.29 \times 2.0 \text{ cm.}) \div 1.07 \text{ cm.} = 2.41 \text{ cm.}$, which last is the length which our secondary stimulus must have at distance 11.5 cm., in order to be equal in intensity to the standard, which is a 2.0 cm. stimulus at distance of 11.0 cm. Proceeding according to this method, I calculated, upon the basis of results given in the three precding tables, what the length of the secondary stimulus should be in our new series of figures, in order that a standard intensity might be maintained throughout. The method of procedure is unquestionably crude, and is justified only on the ground that it was used merely as an empirical device. It is doubtless possible to determine definitely the relation between increase in subjective intensity and increase in objective length of visual stimuli. When this is done it will doubtless be possible to construct a series of figures, in which the secondary stimulus at different distances remains of the same subjective intensity. The time at my disposal did not admit of such a determination.

The validity of the objection just raised against the method of constructing the new series of figures was fully justified by the results of the experiments made with these figures. These results are shown in Tables IV., V. and VI. In the case of Table VI., primary stimulus 18.0 cm., the addition to length of secondary stimulus has produced a result which gives to C a practically constant value. But in Tables IV. and V. the value of C appears in a constantly diminishing ratio, showing that

TABLE IV.

INFLUENCE OF SECONDARY STIMULI OF VARIOUS LENGTHS, BUT ESTIMATED TO BE OF THE SAME SUBJECTIVE INTENSITY OR VALUE, UPON A LINE 16.0 CM. IN LENGTH AT THE SAME DISTANCES SHOWN IN TABLE I.

Lengths of the Secondary Stimuli.		1.79 cm.	2. 00cm.	2.15 cm.	2.90 cm.	3.70 cm.
Distances.		9.5 cm.	10.0 cm.	10.5 cm.	11.0 cm.	13.0 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.10	.71	.69	.68	—.06
	2	1.36	1.17	1.08	.95	.70
Pa.	1	1.15	.93	.88	.70	.36
	2	.88	.82	.48	.54	.07
B.	1	1.28	.93	.75	.67	.48
	2	1.21	.91	.62	.63	.35
C.	1	1.37	1.31	1.05	.77	.60
	2	1.36	1.14	.72	.69	.48
H.	1	1.27	1.04	.80	.67	—.09
	2	1.30	.96	.73	.43	.21
Pi.	1	1.40	1.34	1.20	1.06	.89
	2	1.60	1.61	1.42	1.22	1.08
Pr.	1	1.92	1.66	1.22	1.11	.50
	2	1.72	1.39	1.25	.99	.80
G.	1	2.00	1.64	1.55	1.43	.51
	2	1.29	.56	.69	.34	.39
Pp.	1	1.63	1.44	1.25	.88	.63
	2	1.40	1.34	1.20	1.06	.89
Hn.	1	1.55	1.36	1.05	1.01	.89
	2	1.79	1.80	1.67	1.34	.96
Average.		1.43	1.20	1.01	.86	.53
$E \times D^2$		129.06	120.00	113.52	104.06	89.57

the addition in length was not sufficiently large. In other words we have a somewhat new verification of Weber's Law, viz., equal increments in objective length of visual linear stimuli do not imply equal increase in subjective intensity of the visual stimulus.

Comparing Tables IV., V. and VI. with I., II. and III. respectively, we find that the increase in the length of the

Table V.

Influence of Secondary Stimuli of Various Lengths, but Estimated to be of the Same Subjective Intensity or Value upon a Line 17.0 cm. in Length, at the Same Distances Shown in Table II.

Lengths of the Secondary Stimuli.		1.77 cm.	2.00 cm.	2.35 cm.	3.32 cm.	4.90 cm.
Distances.		1.00 cm.	10.5 cm.	11.0 cm.	11.5 cm.	13.5 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.22	1.12	1.03	.97	.42
	2	1.23	.90	.81	.72	.43
Pa.	1	.80	.84	.41	.23	.02
	2	.89	.65	.40	.25	.15
B.	1	1.20	.85	.86	.58	.48
	2	1.24	1.08	.77	.57	.33
C.	1	1.47	1.28	1.02	.93	.30
	2	.77	.87	.62	.64	.20
H.	1	1.70	1.54	1.50	1.50	1.16
	2	.88	.75	.72	.71	.51
Pi	1	1.14	1.07	1.03	.97	.95
	2	1.00	1.01	.80	.45	.27
Pr.	1	1.61	1.39	1.35	1.04	.96
	2	1.00	.75	.86	.58	.50
G.	1	1.76	1.49	1.29	1.38	.84
	2	1.10	.79	.77	.48	.35
Pp.	1	1.77	1.46	1.28	1.35	.71
	2	1.53	1.02	1.06	.91	.34
Hn.	1	1.98	1.63	.93	.98	.68
	2	1.20	1.19	.94	.69	.58
Average.		1.28	1.08	.92	.80	.51
$E \times D^2$		128.00	119.07	111.32	105.80	92.95

secondary stimulus has greatly increased the constancy of C, in other words the effect of the secondary stimulus of increased length has been uniformly greater. It appears, therefore, more than probable that if the length of the secondary stimulus were increased according to subjective rather than objective standards, the value of C would become really constant as is demanded by our hypothesis.

TABLE VI.

INFLUENCE OF SECONDARY STIMULI OF VARIOUS LENGTHS, BUT ESTIMATED TO BE OF THE SAME SUBJECTIVE INTENSITY OR VALUE, UPON A LINE 18.0 CM. IN LENGTH, AT THE SAME DISTANCES SHOWN IN TABLE III.

Lengths of the Secondary Stimuli.		1.85 cm.	2.00 cm.	2.41 cm.	3.54 cm.	5.43 cm.
Distances.		10.5 cm.	11.0 cm.	11.5 cm.	12.0 cm.	14.0 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.41	1.35	1.19	1.29	.48
	2	1.39	1.25	1.22	1.30	1.18
Pa.	1	1.52	1.40	1.25	1.15	1.08
	2	1.34	1.23	1.06	.97	.72
B.	1	1.61	1.36	1.26	1.10	.95
	2	1.40	1.24	1.59	1.08	.98
C.	1	2.02	1.92	1.76	1.57	1.35
	2	1.39	1.30	1.14	.83	.84
H.	1	1.32	1.36	1.30	1.18	1.10
	2	.81	.64	.48	.40	.13
Pi.	1	1.28	1.34	1.27	1.18	1.02
	2	1.66	1.47	1.37	1.25	1.28
Pr.	1	1.93	1.64	1.72	1.65	1.47
	2	.99	.80	.58	.57	.64
G.	1	1.48	1.07	1.35	1.06	1.02
	2	1.11	1.18	.84	.68	.43
Pp.	1	1.28	1.40	1.36	1.34	1.07
	2	1.91	1.87	1.59	1.45	1.18
Hn.	1	2.02	1.55	1.21	1.02	1.18
	2	1.37	1.26	.94	1.12	.63
Average.		1.46	1.32	1.22	1.11	.94
$E \times D^2$		160.96	159.72	161.34	159.84	184.24

For purposes of comparison, I present three tables, VII., VIII. and IX., showing the results of some experiments conducted by Misses E. Dickson and B. Brock, students in my laboratory course. In these experiments only four subjects were used and ten tests of each was made with each figure. Thus each of the general averages represents forty judgments.

The primary stimulus in these experiments was 24.0 cm., 25.0 cm. and 26.0 cm., respectively. The secondary stimulus was objectively 2.0 cm. in length, the same as in the experiments previously recorded, but inasmuch as the increased length of the primary stimulus makes it necessary to remove the secondary stimulus towards the periphery, the subjective intensity of the secondary stimulus is materially decreased as compared with the secondary stimuli of the experiments previously discussed.

Comparing these results with those of Tables I., II. and III., we find the same general features, viz., decrease in inverse proportion to distance, but a decrease more rapid than is demanded by increase in square of the distance.

Detailed comparison of results in the several tables, bring out some interesting relations. For example, in Table I., where we have primary stimulus 18.0 cm., secondary stimulus 2.0 cm. and distance 14.0 cm., the influence of secondary stimulus is 0.32 cm. In Table VII. for the same distance, but a primary stimulus of 24.0 cm. the influence is 1.0 cm. Similarly, comparing the effect when primary stimulus is 25.0 cm., and the same distance, 14.0 cm., we find, in Table VIII. an effect of

Table VII.

Influence of Two Secondary Stimuli, each 2.0 cm. Long upon a Primary Stimulus 24.0 cm. Long at Distances 13.5, 14.0, 14.5, 15.0 and 16.0 cm.

Distances.	13.5 cm.	14.0 cm.	14.5 cm.	15.0 cm.	16.0 cm.
Subject.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.
A.	1.4	.80	1.0	.9	.7
Kg.	2.2	1.2	.8	—.2	—.7
Ch.	1.2	1.1	.9	.8	.6
Cl.	1.1	.9	.8	.6	.0
Average.	1.5	1.0	.9	.5	.15

Table VIII.

Influence of Two Secondary Stimuli, each 2.0 cm. Long upon a Primary Stimulus 25.0 cm. Long at Distances 14.0, 14.5, 15.0 and 15.5 cm.

Distances.	14.0 cm.	14.5 cm.	15.0 cm.	15.5 cm.
Subjects.	Inflnence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.
A.	.7	.8	—.2	—.7
Kg.	1.2	.8	.5	—.4
Ch.	1.6	1.3	1.0	.8
Cl.	1.7	1.4	1.0	.5
Average.	1.3	1.1	.7	.05

1.3 cm. Similar relations appear throughout, when the influence in the case of different primary stimuli with secondary stimuli at the same distances is observed. The effect of the secondary stimulus increases not only in proportion to its own intensity, but also in proportion to the intensity, or length, of the primary stimulus.

The Effect of Secondary Stimuli when the Primary Stimulus is Varied.

This relation to which reference was made in the preceding paragraph, was also brought out by a series of experiments especially designed for the purpose. A series of eight cards were prepared as follows: The primary stimuli were 8.0, 9.0, 10.0, 11.0, 12.0, 13.0, 14.0 and 15.0 cm. long; the secondary stimuli were in each case 2.0 cm., in length and the distance between primary and secondary stimuli was in each case 9.0 cm. The only variable factor, therefore in the conditions was the length of the primary stimulus.

Table IX.

Influence of Two Secondary Stimuli, each 2.0 cm. Long upon a Primary Stimulus 26.0 cm. Long at Distances 14.5, 15.0, 15.5 and 17.0 cm.

Distances.	14.5 cm.	15.0 cm.	15.5 cm.	17.0 cm.
Subjects.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.
A.	1.8	1.8	.6	.7
Kg.	1.7	1.6	1.2	.1
Ch.	1.5	1.3	1.1	.8
Cl.	1.5	1.3	.5	.4
Average.	1.6	1.5	.9	.5

The method of conducting the experiments was in general similar to that already detailed. The cards were presented in the order given above for the first five tests of each series and in the reverse order for the last five tests of each series — ten tests constituting a series. The experiments were conducted by Misses Newton, McConnell and Pauline Smith, three students in the department of psychology. The results are shown in Table X.

TABLE X.

INFLUENCE OF TWO SECONDARY STIMULI, 2.0 CM. LONG, UPON PRIMARY STIMULI OF VARIED LENGTHS, BUT SAME RELATIVE POSITION AND DISTANCE 9.0 CM.

Lengths of Primary Stimuli.		8.0 cm.	9.0 cm.	10.0 cm.	11.0 cm.	12.0 cm.	13.0 cm.	14.0 cm.	15.0 cm.
Subjects.	No. of Experiments.	Influence	Influence	Influence	Influence	Influence	Influence	Influence	Influence
Mc.	100	.24	.38	.40	.37	.51	.52	.61	.94
N.	100	.06	.17	.11	.24	.46	.03	.53	.28
K.	100	.28	.31	.06	.04	.43	.25	.36	.22
Average.		.15	.29	.19	.22	.47	.27	.50	.48

For purposes of comparison, I present also Table XI., which records the results of experiments conducted under exactly similar conditions by Misses Canning and Blalock. In this group, however, the lengths of the several primary stimuli were

TABLE XI.

INFLUENCE OF TWO SECONDARY STIMULI, 2.0 CM. LONG, UPON PRIMARY STIMULI OF VARIED LENGTHS, BUT SAME RELATIVE POSITION AND DISTANCE 14.0 CM.

Lengths of Primary Stimuli.		18.0 cm.	19.0 cm.	20.0 cm.	21.0 cm.	22.0 cm.	23.0 cm.	24.0 cm.	25.0 cm.
Subjects.	No. of Experiments.	Influence	Influence	Influence	Influence	Influence	Influence	Influence	Influence
Bl.	100	.00	.07	.01	.04	.21	.29	.64	.32
Cu.	100	.03	.12	.12	.16	.63	.36	.35	.78
Average.		.015	.10	.07	.10	.42	.33	.50	.55

18.0, 19.0, 20.0, 21.0, 22.0, 23.0, 24.0 and 25.0 cm., and the distance between primary and secondary stimuli was 14.0 cm.

Referring to the summary of results in both tables, it will be observed that in general the influence of secondary stimuli at the same distances increases as the length of the primary stimulus increases. Irregularities, however, in the rate of increase and actual exceptions to the rule are particularly noticeable. The fact that relatively speaking the same inconsistencies appear in both tables would seem to indicate that the order in which the cards were presented was a factor which affected the result and that the natural ebb and flow of attentive processes was involved. In addition, it should be observed that linear stimuli of the lengths here given cannot be distinguished from one another with any considerable degree of certainty, unless they differ by more than one cm. in length.

We may eliminate these inconsistencies in the two tables by taking an average of three different lengths of primary stimulus. For example: The results in the case of primary stimuli 8.0, 9.0, and 10.0 cm. were 0.15, 0.29, and 0.19 cm., respectively. The mean of 8, 9, and 10 is 9; the average of 15, 29, and 19, is 21. Therefore the corrected result for primary stimulus, 9.0 cm., would show an influence of secondary stimulus equal to 0.21 cm. Similarly the corrected result for primary stimulus 10.0 cm., shows an influence of 0.23 cm. When Tables X. and XI., are corrected according to the method just outlined the average appears as shown Table XII. Here we see a consistent increase in influence of secondary stimulus corresponding to increase in length of primary stimulus.

TABLE XII.

THE RESULTS SHOWN IN TABLES X. AND XI. WHEN REARRANGED AS DESCRIBED IN THE TEXT.

Lengths of Primary Stimuli.		9.0 cm.	10.0 cm.	11.0 cm.	12.0 cm.	13.0 cm.	14.0 cm.	15.0 cm.
Average.	Table X.	.21	.23	.29	.32	.41	.42	

Lengths of Primary Stimuli.		19.0 cm.	20.0 cm.	21.0 cm.	22.0 cm.	23.0 cm.	24.0 cm.
Average.	Table XI.	.06	.09	.19	.28	.42	.46

A closer examination of the two summaries of results shown in Table XII. reveals another fact which has entered as a disturbing element into my experiments and which I have not succeeded in satisfactorily isolating. I refer to the fact that the stretch of open space between the primary and secondary stimuli enters as an element in determining the influence of the secondary stimuli. When variations in the size of this open space are small its influence upon the result may perhaps be disregarded. But in cases of the sort now under discussion, these open spaces play a considerable part. For example, in

Table XII. we see that when length of primary stimulus is increased from 9.0 cm. to 14.0 cm. the influence is increased from 0.21 cm. to 0.42 cm. Now if we accept the hypothesis that this influence varies directly as the product of the intensities of the two sensations,[1] and inversely as the square of the distance between them, then the proportion of increase ought to be readily determined by use of the well known formula for the law of gravity. Using this formula ($f = C\,[(m \times m') \div D^2]$) as a basis of calculation, it will be found that the relation between the influence in the case of the 9.0 cm. and 14.0 cm. primary stimuli, will be as 0.22 is to 0.35. The relation as determined empirically was 0.21 to 0.42. The entire series as determined empirically is 0.21, 0.23, 0.29, 0.32, 0.41, 0.42. The entire series as determined by calculation based upon the formula is 0.22, 0.24, 0.27, 0.30, 0.32, 0.35.

It will be observed that in the latter half of the series there is an increase in the influence of the secondary stimuli, which is in excess of that which is warranted by our hypothesis as represented by the formula in question. It should also be noted that this unexpected increase in influence is coincident with a gradual lessening of the open spaces which separate the primary from the secondary stimuli. In the case of the 9.0 cm. primary stimulus this open space was 3.5 cm., whereas in the case of the 14.0 cm. primary stimulus this space was only 1.0 cm. This would seem to indicate that close proximity of the ends of the primary and secondary stimuli, increases the effect of the secondary stimulus.

The indication just referred to is further emphasized by reference to the other half of Table XII. Here we have primary stimuli increasing in length from 19.0 cm. to 24.0 cm. The series of figures showing the influence of secondary stimulus as determined empirically is 0.06, 0.09, 0.19, 0.28, 0.42, 0.46. A corresponding series calculated upon the basis of the gravity formula would be 0.19, 0.20, 0.21, 0.22, 0.23, 0.24. Here too there is a corresponding decrease in the size of the space which separates the two stimuli.

It is further evident from the foregoing that the disturbing effect of too close proximity of the ends of the two stimuli is in

[1] Intensity of sensation is equivalent to 'sensation mass.'

proportion to the length of the primary stimuli. This relation is manifest in a series of experiments recorded in Table XIII. The experiments were conducted by myself, the subjects being students of psychology. Three subjects were used and results for three series of ten judgments each are shown. In the first column are recorded the judgments of the length of a single line (4.0 cm.) without secondary stimuli; in the second column are shown judgments of the length of the same line when secondary stimuli (2.0 cm.) have been introduced at distance 3.5 cm. The distance between the end of the primary stimulus and the end of the secondary stimulus was only 0.5 cm. The third column shows judgments of a single line 6.0 cm. in length and the fourth column shows judgments of the same line when secondary stimuli have been added at a distance of 4.5 cm. The distance between end points is again only 0.5 cm. And so with each primary stimulus, viz., 8.0 cm., 10.0 cm. 12.0 cm., and 14.0 cm., the distance between primary and secondary stimuli is respectively 5.5 cm., 6.5 cm,, 7.5 cm. and 8.5 cm., but the distance between end points of primary and secondary stimuli is in every case only 0.5. cm.

Referring now to the average of results for all subjects, there is shown a marked increase in influence for each primary stimulus, despite the fact that the secondary stimulus was further removed and probably less intense. It is at once manifest that the formula under consideration cannot be used to determine the relative effect of secondary stimuli in the case of such a series as that which is represented in this table, unless another element can be introduced into the formula. It is not yet clear what this element should be. We can only say that close proximity of the ends of the two stimuli increases the effect of the secondary stimuli and that this increase in effect is itself increased in proportion to length of primary stimulus.

Influence of Secondary Stimuli of Varying Length upon a Primary Stimulus of a Constant Length and at a Constant Distance.

For showing this relation I have not conducted a separate series of experiments, but have rearranged the results shown in Tables I., II. and III. and represent them in Table XIV.

TABLE XIII.

INFLUENCE OF SECONDARY STIMULI 2.0 CM. LONG UPON PRIMARY STIMULI OF DIFFERENT LENGTHS AND AT DIFFERENT DISTANCES.

Lengths of Primary Stimuli.		4.0 cm.		6.0 cm.		8.0 cm.		10.0 cm.		12.0 cm.		14.0 cm.	
Distances.		3.5 cm.		4.5 cm.		5.5 cm.		6.5 cm.		7.5 cm.		8.5 cm.	
Subjects.	Series.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.
E. B.	1	4.04	4.46	5.98	6.52	7.84	8.70	9.85	10.85	12.05	13.16	13.89	15.33
	2	3.89	4.09	5.91	6.12	7.74	8.12	9.74	10.43	11.78	12.53	13.72	14.56
	3	3.86	4.04	5.76	6.35	7.62	8.27	9.57	10.36	11.46	12.47	13.43	14.42
N. B.	1	3.82	3.96	5.84	6.05	7.75	8.24	9.67	10.34	11.68	12.47	13.51	14.25
	2	3.88	4.01	5.92	6.26	7.88	8.36	10.13	10.73	12.24	12.84	14.08	14.82
	3	3.87	4.01	5.84	6.27	7.73	8.28	9.70	10.55	11.68	12.53	13.51	14.65
A. Mc.	1	3.95	4.24	5.85	6.35	7.79	8.44	9.70	10.57	11.67	12.61	13.86	14.50
	2	3.88	4.44	5.87	6.40	7.70	8.69	9.96	10.76	11.93	12.78	13.77	15.30
	3	3.73	3.85	5.61	5.96	7.70	8.30	9.65	10.30	11.75	12.41	13.76	14.69
Average.		3.88	4.12	5.84	6.25	7.75	8.38	9.77	10.54	11.80	12.64	13.72	14.72
Infl. of Sec. Stimuli.			.24		.41		.63		.77		.84		1.00

Unfortunately, the results which are comparable were not obtained in the same series of experiments, but all the results for constant secondary stimulus (2.0 cm.) were obtained first and then results for variable secondary stimulus were obtained in a subsequent series of experiments with the same subjects. Inasmuch as the magnitude of an illusion decreases with practice on the part of the subject[1], we find that in the second series of experiments the influence of the same secondary stimulus under similar conditions is less than in the first series. This is seen by comparing the instances in which the length of the secondary stimulus was the same. For example, when primary stimulus was 18.0 cm., distance 11.0 cm. and length of secondary stimulus 2.0 cm. in both series, the influence in Series 1 was 1.41 cm. and in Series 2 the influence was 1.32 cm., showing a decrease in influence due to practice of 0.09 cm.; similarly, when primary stimulus was 17.0 cm. and secondary stimuli 2.0 cm., influence in Series 1 was 1.18 cm. and in Series 2 it was 1.08 cm., showing a decrease in influence of 0.80 cm.; further, when primary stimulus is 16.0 cm., and secondary stimulus 2.0 cm., influence in first series was 1.41 and in second series 1.20 cm., showing a decrease of 0.21 cm. In comparing the results for the two series therefore we must either subtract these values from the first or add them to the second or, perhaps more accurately, subtract one half from the first and add one half to the second.

Comparisons of individual results of the two series are not satisfactory because of irregularities and we must resort to a comparison of averages in order to discover any consistent relations.

Taking first the results for primary stimulus 18.0 cm. we find the mean of all the distances used is 11.8 cm.; the length of the secondary stimulus used throughout the first series is 2.0 cm.; the mean length of secondary stimulus in the second series is 3.04 cm., the average influence in first series is 1.04 cm. and in second series is 1.21 cm., or if corrected as above suggested the influence in first series is 0.99 cm., and in second series is 1.26 cm. There is thus apparent a more or less direct ratio between the length of the secondary stimulus and its influence.

[1] Cf. Judd, *Genetic Psychology for Teachers*, p. 26.

The results shown in Table XIV. lend themselves, however, to a more comprehensive treatment and enable us to apply directly the formula implied by our hypothesis. By this hypothesis, $f = C(m \times m') \div D^2$ in which f is the force of attraction existing between primary and secondary stimuli, m is the mass or intensity of the primary stimulus, m' is the mass or intensity of the secondary stimulus, D is the distance between primary and secondary stimuli, and C is a constant which must be empirically determined.

From the results of Table XIV. this constant appears to be 0.339, determined as follows: In case of primary stimulus 18.0 cm. $(m \times m') \div D = 0.258$ and the influence as shown above was 0.99 cm. Hence we have $0.99 = C \times 0.258$, or $C = (0.99) \div 0.258 = 0.383$. Determining C for the six possible instances, I found the average to be 0.339 with a mean variation of 0.038.

Using the constant thus determined, it will be found by making proper substitutions that the formula given is an approximate expression for each of the results obtained by experiment, when the conditions are comparable with the foregoing and that the consolidation of individual results increases the perfection of such an approximation.

It should be expressly remarked, however, that the formula with constant above given, cannot be applied indiscriminately to all results in which widely varying distances involving varying intensities of secondary stimuli are included; nor can it be applied successfully to cases in which the ends of primary and secondary stimuli are less far removed from one another than 1.0 cm.

Mean Variations.

It will be noted that the mean variation does not appear in the tables. This is because the results shown in the tables always represent a calculated effect and not a judgment. This effect was determined by subtracting one series of judgments from another. A mean variation parallel to the results shown in the table would have no definite meaning. As regards the judgments made by the subjects I may make the following general statements: For all judgments the mean variation ranged from 0.2 to 0.8 cm. As a rule the mean variation is somewhat larger

Table XIV.

Comparison of Influence of Secondary Stimuli of Different Lengths upon Primary Stimuli of the Same Length and at the Same Distance.

Primary Stimulus 16.0 cm.				Primary Stimulus 17.0 cm.				Primary Stimulus 18.0 cm.			
Series.	Distance in cm. between Primary and Secondary Stimuli.	Length of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Series.	Distance in cm. between Primary and Secondary Stimuli.	Length of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Series.	Distance in cm. between Primary and Secondary Stimuli.	Length of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.
1	9.5	2.00	1.74	1	10.0	2.00	1.47	1	10.5	2.00	
2	"	1.79	1.43	2	"	1.77	1.28	2	"	1.85	
1	10.0	2.00	1.41	1	10.5	2.00	1.18	1	11.0	2.00	
2	"	2.00	1.20	2	"	2.00	1.08	2	"	2.00	
1	10.5	2.00	1.18	1	11.0	2.00	0.91	1	11.5	2.00	
2	"	2.15	1.01	2	"	2.35	0.92	2	"	2.41	
1	11.0	2.00	0.80	1	11.5	2.00	0.59	1	12.0	2.00	
2	"	2.90	0.86	2	"	3.32	0.80	2	"	3.58	
1	13.0	2.00	0.44	1	13.5	2.00	0.29	1	14.0	2.00	
2	"	3.70	0.53	2	"	4.90	0.51	2	"	5.43	

Primary Stimulus 16.0 cm.

Average Distance 10.8 cm.
Average Length of Secondary Stimulus in Series 1 = 2.00 cm.
Average Length of Secondary Stimulus in Series 2 = 2.31 cm.
In Series 1 $(m \times m') \div D^2$ = 0.253
" " 1 Average Influence of Secondary Stimuli = 1.01 cm.
In Series 2 $(m \times m') \div D^2$ = 0.314
" " 2 Average Influence of Secondary Stimuli = 1.10 cm.

Primary Stimulus 17.0 cm.

Average Distance 11.3 cm.
Average Length of Secondary Stimulus in Series 1 = 2.00 cm.
Average Length of Secondary Stimulus in Series 2 = 2.87 cm.
In Series 1 $(m \times m') \div D^2$ = 0.266
" " 1 Average Influence of Secondary Stimuli = 0.84 cm.
In Series 2 $(m \times m') \div D^2$ = 0.382
" " 2 Average Influence of Secondary Stimuli = 0.97 cm.

Primary Stimulus 18.0 cm.

Average Distance 11.8 cm.
Average Length of Secondary Stimulus in Series 1 = 2.00 cm.
Average Length of Secondary Stimulus in Series 2 = 3.04 cm.
In Series 1 $(m \times m') \div D^2$ = 0.258
" " 1 Average Influence of Secondary Stimuli = 0.99 cm.
In Series 2 $(m \times m') \div D^2$ = 0.393
" " 2 Average Influence of Secondary Stimuli = 1.26 cm.

when secondary stimuli are introduced and the variation is larger when the primary stimulus is increased in length.

Tactual Illusions.

In order to compare the visual and tactual illusions and to show the law of attraction as applied to the latter, I reproduce from the article 'Ueber den Einfluss von Nebenreizen'[1] to which reference has been made, the results of some experiments with a tactual illusion similar to the Müller-Lyer visual illusion. The line was produced by pressure of a thin strip of brass upon the skin of the forearm. The projections or arms were produced by pressure of short brass rods drawn to a point. An apparatus was so constructed that the pressure from line and all points could be given at the same time. For a more detailed description of the method and nature of these experiments, the original article must be consulted.

So far as these results are comparable with results of visual experiments previously detailed, they appear in Tables XV. and

Table XV.

Tactual Illusion. Figure Similar to the Müller-Lyer Figure with Projecting Arms Extending Outward.

Length of Primary Stimulus in cm.	Length of Secondary Stimuli in cm.	Angle Formed by Secondary Stimuli.	Subject Ke.		Subject W.		Subject M.		Average.		$\frac{m \times m'}{D^2}$
			Influence in cm.	M. V.	Influence in cm.	M. V.	Influence in cm.	M. V.	Influence in cm.	M. V.	
6.0	2.9	20°	2.9	0.9	2.5	0.5	3.4	0.4	2.9	0.6	0.90
6.0	5.0	20	3.1	1.0	2.9	0.7	4.3	0.4	3.4	0.7	0.99
7.0	2.9	20			2.5	0.4			2.5	0.4	0.84
7.0	5.0	20			3.4	1.0			3.4	1.0	0.97
8.0	2.9	20	2.2	0.7	2.0	0.2	3.4	0.5	2.5	0.5	0.76
8.0	5.0	20	2.4	0.6	2.5	0.4	4.0	0.3	3.0	0.4	0.94
6.0	2.9	30	2.8	0.5	1.7	0.4	2.7	0.3	2.3	0.4	0.90
6.0	5.0	30	2.3	1.0	1.8	0.2	2.8	0.7	2.3	0.6	0.99
7.0	2.9	30			2.2	0.3			2.2	0.3	0.84
7.0	5.0	30			1.5	0.3			1.5	0.3	0.97
8.0	2.9	30	2.1	0.2	1.2	0.6	3.1	0.9	2.1	0.6	0.76
8.0	5.0	30	2.1	0.7	1.8	0.5	3.4	0.7	2.4	0.6	0.94

XVI. The last column of each of these tables contains the value of $(m \times m') \div D^2$ arithmetically expressed. Here m equals

[1] *Archiv f. d. Gesamte Psychologe*, Vol. I., pp. 31–109.

Table XVI.

Tactual Illusion. Figure Similar to the Müller-Lyer Figure with Projecting Arms Extending Toward the Center.

Length of Primary Stimulus in cm.	Length of Secondary Stimuli in cm.	Angle Formed by Secondary Stimuli.	Subject Ke.		Subject W.		Subject M.		Average.		$\frac{m \times m'}{D^2}$
			Influence in cm.	M. V.	Influence in cm.	M. V.	Influence in cm.	M. V.	Influence in cm.	M. V.	
10.0	2.9	25°	1.9	0.7	1.4	0.2	1.0	0.6	1.4	0.5	0.71
10.0	5.0	25	2.2	0.3	1.5	0.3	1.6	0.3	1.8	0.3	0.89
12.0	2.9	25	1.8	0.4	1.9	0.7	1.3	0.4	1.7	0.5	0.62
12.0	5.0	25	2.5	0.2	1.5	0.0	1.6	0.4	1.9	0.2	0.83
10.0	2.9	45	1.8	0.3	0.9	0.3	0.5	0.3	1.1	0.3	0.71
10.0	5.0	45	1.6	0.3	1.1	0.5	0.6	0.2	1.1	0.3	0.89
12.0	2.9	45	1.6	0.5	1.2	0.2	0.6	0.3	1.1	0.3	0.62
12.0	5.0	45	2.0	0.3	1.3	0.2	0.5	0.2	1.3	0.2	0.83

the length in centimeters of the line or primary stimulus; m' equals the distance of the end point of the projecting arm from the end of the line or primary stimulus. The assumption that this last is the secondary stimulus is somewhat questionable. Its justification, so far as there is any, is based upon the following facts: (1) The introspective evidence of the subjects showed that the end points of the line were most prominent in consciousness, and consciousness of stimulation of the skin between the two end points of the line was very vague and sometimes altogether absent. Consequently the judgment really concerned a distance between two points (corresponding to the line) influenced by a consciousness of a *distance* between these two points and four other points. I have therefore considered these distances the secondary stimulus rather than the actual points stimulated.

D in the formula above given is the distance from center of primary to center of secondary stimulus, measured along the line which consciousness must inevitably follow. In other words D is here one half the primary plus one half the secondary stimulus.

A comparison of the average E (which here represents the average influence of the secondary stimulus or, more properly, the effect of the force of attraction between the primary and secondary stimuli) with the numerical equivalent of $(m \times m') \div D^2$

shows a fairly consistent proportional relation. The accuracy of the proportion is increased when averages of all comparable groups is taken.

It must be observed that comparisons of instances in which the angles are different cannot be made. The intensity of the secondary stimulus is decreased as its angle increases. We cannot compare satisfactorily the results of the two tables for a similar reason.

The figures which are used to represent the intensity of the secondary stimulus are at best only relative, not absolute, and hence the reason that we cannot at present complete the formula and assign a definite numerical value to C.

If, however, comparisons are made of instances in which the conditions upon which the intensity of m' depends are constant, the accuracy of the proportion existing between the attractive forces and the values of $(m \times m') \div D^2$ is very striking. For example, taking data from the first two lines of Table XV., we have the proportion 2.9 : 3.4 : : 0.90: 0.99 or 3.060 : : 2.871, there being a difference in the proportion of 0.189 cm. But inasmuch as there were four secondary stimuli in the experiments recorded, the actual difference in proportion for a single secondary stimulus would be only 0.047 cm.

The inaccuracy of the proportion in the second half of each table is largely increased. Here the results are from experiments in which the angle of the secondary stimulus was quite large. When this angle was large the magnitude of the illusion was considerably diminished. As a consequence the observer was more liable to be misled by other influences than the immediate objects of perception. Moreover the difficulties of accurately determining the numerical value of the illusion were increased for the experimenter. Hence the values given in the latter half of each table are less trustworthy than the corresponding values in the first half.

Individual Differences.

It is a notable fact that some individuals are more susceptible than others to an illusion of the kind under discussion. Binet has remarked[1] that young children are more susceptible than

[1] *L'Annee Psychologique*, 1894, 'L'illusion d'optique de Mueller-Lyer,' p. 330.

older persons. This fact is also very manifest in the results which are here reported. For example, referring to Tables I., II. and III. a very casual examination is sufficient to reveal the fact that the illusion values for the first four subjects are less than the corresponding values for the remaining six subjects. The first four subjects were adults, while the remaining six were children varying in age from twelve to fourteen years.

This difference may be accouuted for partly on physiological and partly on psychological grounds. In the case of children, the nervous organism is not so firmly 'fixed'; alterations among its parts may be more easily effected. Attraction between the elements of the organism, therefore, has a greater effect.

On the other hand, psychologically speaking, the judgment of the other person is more evenly balanced, which is perhaps equivalent to saying that experience furnishes to the older person a larger supply of data upon which a judgment may be based. A high degree of susceptibility to illusion, therefore, may indicate on the one hand, especially in children, a nervous organism which is plastic and impressible and therefore highly educable, and on the other hand, especially in adults, a weakness of judgment.

In the article 'Ueber den Einfluss von Nebenreizen' to which reference has already been made, I reported experiments by myself upon several groups of children taken from different classes of two elementary schools in Würzburg. The object of these experiments was to determine quantitatively the effect of a secondary stimulus upon the localization of a point stimulated upon the skin of the fore arm. It was found that a fairly consistent parallel existed between the amount of influence exerted by the secondary stimulus and the degree of mental ability attributed to the pupil by his teacher. This parallel was more striking when groups of dull children were compared with groups of bright children. The children used in these experiments varied in age from six to fourteen years. It is very questionable if the same relation would hold for older individuals.

The individual variations in the case of subjects of the same age are marked also in the case of the visual illusion, but the number of subjects for which results are reported is so small that comparison would be valueless. Future experiment must determine whether or not a relation, such as I have indicated, exists.

Geometric-Optical Illusions.

The literature of this subject is peculiarly rich and not unprofitable. I shall attempt to touch briefly upon such salient features only as are directly related to the phenomena which have been under my observation.

Heymans has shown[1] that the Müller-Lyer illusion (*a*) increases with the length of the projecting arms, is then (*b*) stationary, and finally (*c*) decreases as the length of the arms increases.

These facts which seem to me fatal to most of the theories which have been advanced to explain the illusion, are perfectly in accord with the law of attraction as developed in the foregoing pages. For, increase in length of projecting arm means, (*a*) increase in intensity of the secondary stimulus and (*b*) increase in the distance of the secondary from the primary stimulus. In the former case, we have *increase* in influence and in the latter case *decrease* in influence of the secondary stimulus. If we begin to increase the length of the arms when they are very short, each increment in length corresponds to a relatively large increase in intensity, but as a result of the operation of Weber's law, there comes a time when a very large increase in length of arm (or secondary stimulus) results in only a relatively small increase in intensity. On the other hand, each increase in square of the distance has diminished the influence of the secondary stimulus in proportion.

In the beginning therefore, (*a*) the increase in influence due to increase in intensity is greater than the decrease in influence due to increase in distance, later (*b*) the effect of the two factors is equal, the one counterbalancing the other, and finally (*c*) the decrease in influence due to increase in distance is greater than the increase in influence due to increase in intensity.

Heymans further shows in the same connection (p. 227), that there is a consistent proportional relation between the size

[1] *Zeitschrift für Psychologie und Physiologie*, Vol. IX., p. 236.

of the illusion and the cosine of the angle formed by the projecting arm and the central line (Schenkelwinkel). When the angle increases the illusion becomes less pronounced. This fact harmonizes with our law of attraction, inasmuch as it is to be expected that an attractive force will have greater effect when acting in a straight line than when acting at an angle upon a given object.

The application of the law of attraction to the other geometric-optical illusions with any degree of accuracy is difficult, if at all possible. In general, we may observe, however, that displacements take place in the direction of greater 'sensation masses.' In the Poggendorf figure, for example, the points where the diagonal joins the parallels are drawn, the one upward and the other downward, by the relatively large sensation masses represented by the sides of the two angles formed. The same may be said of the Zoellner figure.

In all such cases, where there is a displacement of a line from its true objective position, it will be found that certain points which mark the direction of said line are acted upon unequally by neighboring 'sensation masses.'

The most difficult factor to determine in all of these phenomena is the value of a particular 'sensation mass,' or, as previously termed, the intensity of a particular visual stimulus. Spatial measurement is a very inadequate expression of this intensity, as we had reason to observe in our discussion of the results of Tables IV., V. and VI. There are evidently several elements which go to determine the intensity of a given visual stimulus. The first of these is undoubtedly spatial size. The second is the proportionate part of all active sensory processes which the sensation in question represents. When an experience is already crowded with sensory elements, the addition of a new element has comparatively little sensory value. A third element is position in the visual field — the same stimulus being more intense upon the fovea centralis than on the periphery. A fourth element is the amount of central reinforcement which may be given the stimulus. Mach says,[1] for example: 'Der

[1] *Pflüger's Archiv*, Vol. 60, p. 509. Also *Zeitschrift für Psychologie und Physiologie*, Vol. 16, p. 298.

blosse Wille rechts zu blicken gibt den Netzhautbildern an bestimmten Netzhautstellen einen grësseren Rechtswerth.' An illustration of both the third and fourth elements just mentioned is furnished by a phenomenon which I have observed in making some experiments with the illusion to which Professor Loeb first called attention. One of the illustrations which Professor Loeb offered was as follows: If one places two pieces of money on a table so that they seem equally far removed to one's right and then places a third piece further towards the right so that the three pieces form a right angle triangle it will be found that the relative position of the first two has been so altered that the lower one which is on a horizontal line with the third, now appears further to the left than the upper one. In my own experience the phenomenon to which Professor Loeb calls attention does not always appear and in fact the reverse phenomenon sometimes appears, *i. e.*, the lower one of the two vertically arranged pieces appears further toward the right. On giving the matter closer attention I found that the change in result was brought about by a difference in the direction of attention. If, for example, the attention is directed to the two lower pieces the third is attracted by both and the phenomenon mentioned by Professor Loeb may be observed. On the other hand if attention is directed toward the one above and the one to the extreme right, or the two forming the hypoteneuse of the triangle, it will be found that the reverse phenomenon takes place, viz., the third is again attracted by the two to which attention is being given, the angle opposite becomes obtuse and the upper of the two pieces which were arranged vertically now appears to lie too far to the left. Here attention, or central reinforcement, and bringing of the two images nearer the fovea, both operate to increase the intensity, or sensation mass, of the two sensations, diminishing in a corresponding degree the intensity of the third sensation. The consequence is that the two stronger attract the third with a greater force than it attracts them and it is therefore displaced from its true relative position.

Finally, a fifth element in determining the intensity of a given sensation is the duration of the stimulus — the intensity of a stimulus diminishing as it grows older. This last element has perhaps not figured in the case of the illusions which have previously been discussed. But in the case of the so called illusion of reversible perspective it probably plays a large part. Such illusions are usually brought about by staring at a figure. The result of the staring is to diminish gradually the intensity of the sensations occasioned by the points which determine the form of the figure. The intensity of the neighboring points is relatively less affected. Consequently, these neighboring points finally have a larger 'sensation mass' or intensity than the others, and when this happens, they determine the form of the figure according to their own disposition. After this has happened a few times, one can so reinforce the intensity of these latter points from within that he is able to control the phenomenon at will.

Theories.

Heymans and Wundt[1] both agree that the Müller-Lyer illusion is due to an almost physical impulse (fast physische Zwang) to follow the direction of the projecting arms with eye movements. One might argue in support of this theory that the law of attraction governs in fixing the strength of the impulse to eye movement. The question still remains unsettled, however, as to whether the judgment is a by-product of this impulse to movement, or whether the impulse to movement is itself a product coördinate with the judgment.

Wundt, in criticism of Heymans' contrast theory, calls attention[2] to the fact that the illusion takes place when either figure is compared with a straight line and no contrast of eye movement is possible. This criticism is justified by my experiments which were concerned with only one type of figure.

It is manifest that such theories[3] as the confluence theory of Mueller-Lyer, the Auerbach indirect vision theory, the Brentano pseudoscopic angle theory, the Thiery perspective theory, the Einthoven dispersion image theory and all others which are based upon phenomena growing out of the extension of arms at an angle, are shown to be inadequate by the fact that the illusion is present when no such angles appear in the figure.

[1] *Physiological Psychology*, Vol. II., p. 149.
[2] *Die geometrisch-optischen Tauschungen*, p. 47.
[3] Titchener, *Experimental Psychology*, pp. 321–328.

General Conclusions.

The law of attraction as represented in the present paper is an attempt to state in definite form a principle which has been more or less prominent in the theories of several writers upon this subject.

Jastrow,[1] for example, has attempted to explain optical illusion in general on the principle that all objects are judged relatively to their environment. Our judgment of a thing is modified by the other things which surround it.

A great variety of facts which illustrate the principle may be drawn from every day experience as well as from experimental laboratories. For example, Lipps[2] calls attention to the fact that cows appear to be larger when they are in narrow, low stalls than they do when outside.

Professor Baldwin in an article upon the 'Effect of Size-contrast upon Judgments of Position in the retinal Field,'[3] reports that a point, in the field of vision, midway between two figures of unequal size, as two squares or two circles, will be attracted towards the larger figure. Further, the tendency to error increases with the relative increase of the side of the larger figure and the tendency is about twice as great when the figures are arranged vertically as when they are arranged horizontally.

In an article entitled 'Normal Motor Suggestibility,'[4] I have reported a series of experiments showing that the localization of a point stimulated upon the skin of the forearm is influenced by the stimulation of a second point, either above or below the one to be localized. It was also shown that the localization of a visual image in the peripheral field was similarly affected by the appearance in the same field of a second visual image, and, similarly, the localization of a sound was affected by a second sound.

The tendency to fuse together of two or more sensations which are simultaneously experienced has been frequently remarked and experimental psychology has shown conclusively

[1] *American Journal of Psychology*, Vol. IV., p. 381.

[2] *Raumaesthetik*, p. 65.

[3] Psychological Review, Vol. II., p. 244. Cf. also the further figures given in his *Fragments in Philosophy and Science*, pp. 275 ff.

[4] Psychological Review, Vol. IX., pp. 329–356.

that one of the chief defects of the older introspective psychology was its failure and inability to recognize in experience the elementary sensations which composed it.

All these and many other similar facts seem to point to a general law of relativity, which may be stated somewhat as follows: *Every sensation is influenced by every other sensation which may be present in any complex experience.* The nature of this influence seems to be a direct interaction of one upon the other, the resulting effect of this interaction being determined by the nature of the interacting sensations.

All of the sensations with which I have dealt experimentally are such as to make up 'extensive ideas';[1] they were either sensations defining position or form and magnitude, and the nature of the interaction seems to have been an attractive force, which I was able to measure. This attractive force is governed in its action by the same general law which governs the action of the attractive forces in nature, with which we are already familiar and which has been given mathematical expression in the well-known formula $f = C\,(m \times m') \div D^2$. In its application to the phenomena which have been under our observation, f, in the formula, equals the force exerted by two sensations, the one upon the other, m is the intensity or sensation mass represented by a primary stimulus, m' is the intensity or sensation mass represented by a secondary stimulus, and D is the distance between the primary and secondary stimuli, measured from center to center. The constant C must be determined empirically, and is not the same value in the case of visual and tactual sensations.

The apparently physical nature of the law leads to the suggestion that this attractive force operates between the nervous elements, electro-chemical in nature, which mediate sensation.

On the other hand, one might be justified in admiring that universality of the law, manifest in its consistent operation in two such widely separated spheres as the material and the spiritual.

The time is not opportune, I think for a discussion of the vexed question as to the nature of mind which is involved in the two possible theories here suggested.

[1] Titchener, *An Outline of Psychology*, p. 154.

Summary.

1. When a subject is required to judge the length of a single line, by comparing it with a second line which is variable in length, the single line is always underestimated.

2. Using the same method of comparison, if the subject is required to judge the length of the same line, now accompanied by shorter lines which represent extensions of the line of direction of the original line but which are separated from it by open spaces, the original line is judged to be longer than it was when unaccompanied by the shorter lines, and, generally, it is judged to be longer than objective measurement shows it to be.

3. When the results for several subjects are consolidated it is found that the influence attributable to the addition of the shorter lines, or secondary stimuli, is (*a*) increased when the length of the secondary stimuli is increased, also (*b*) that this influence is increased when the length of the line to be judged is increased, and finally (*c*) an increase in the distance of the short lines from the central lines, or primary stimulus, measured from center to center, is followed by a decrease in the influence of the short lines, or secondary stimuli.

These general relations obtain in the case of individuals as well as for groups of individuals, but the individual variation is somewhat large, and comparisons of individual results are not thoroughly satisfactory.

4. A fourth and a disturbing element in determining the amount of influence of the secondary stimuli was the space between the ends of the primary and the secondary stimuli. When the distance between the ends of the primary and the secondary stimuli was decreased, the amount of influence of the secondary stimuli was correspondingly increased. The exact proportionate relation was not determined.

5. When the disturbing factor just noticed could be disregarded by reason of a favorable arrangement of conditions, it was found that the well known formula, expressing the law of attraction in the material universe, can be applied to the results of the experiments here described.

6. The results of certain experiments in judging the length of lines stimulated by pressure upon the skin of the forearm, also yield to a similar statement.

THE RELATION BETWEEN THE VASO-MOTOR WAVES AND REACTION TIMES.

BY WM. R. WRIGHT,
University of Michigan.

The experiments herein recorded are a series of reaction experiments conducted with the view of ascertaining any possible relation existing between the vaso-motor wave and the reaction time of the subject; or, stated in the form of a question, does the reaction time of the subject vary in length in accordance with the rise and the fall of his vaso-motor, or 'Traube-Hering,' wave?

The subject was placed in a room separate from the recording apparatus so that all distractions of sight and of sound were reduced to the minimum. He was so seated facing a small table that both of his arms rested easily upon the top of the table. Within the palm of the subject's left hand was fastened a Hallion and Comte plethysmograph, while with the right hand he operated a telegraphic key. To the subject was attached also a Sumner pneumograph, the records of which were taken with the view that they might be of special value in the study of another problem in the future. Although no use was made of these records in this series of experiments, it was found that the markings of the pneumograph could be recorded along with the other records without interfering in any way with the subject's attention to the particular task assigned him.

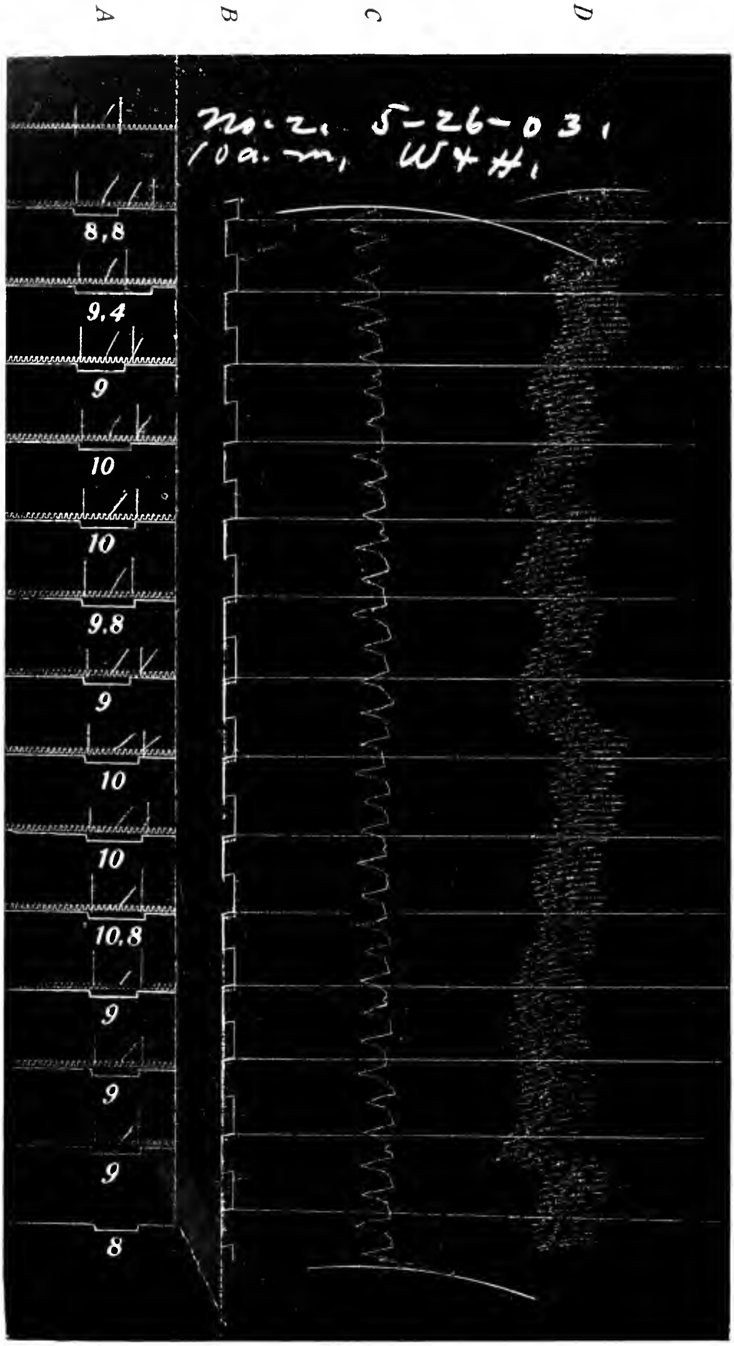
On the table in front of the subject was a telegraphic sounder. This was screened from his view, and furnished the auditory stimulus to which he reacted by pressing the telegraphic key mentioned above.

All the recording apparatus was placed in the experimenter's room, and was connected by air-tight rubber tubing and insulated wires with the apparatus in the subject's room. The records were taken on two kymographs, one of the horizontal type with a traveling carrier, and the other a Zimmermann, of the vertical pattern. The motive power for the revolving drums was furnished by an electric motor, the horizontal drum, 50 cm. in circumference, being so regulated in regard to speed that it made one revolution in 7.8 seconds. By means of graded pulleys the rate of speed of the vertical drum was so adjusted that the surface of the drum moved at a rate equaling the rate of the longitudinal movement of the markers connected with the horizontal drum; but it was found that complete reliance could not be placed upon this adjustment alone, as the least slip of one of the belts made a perceptible change in the rate of the speed of the drums.

The vertical drum received the markings of a Lombard-Pillsbury piston-recorder which was connected by a rubber tube with the plethysmograph, the markings of a Marey tambour connected by a rubber tube with the pneumograph, and also the records of an electric marker so connected with the reaction time-marker of the horizontal kymograph that the beginning of each reaction was written on the vertical drum. Care was taken to keep the three writing points in the same vertical line upon the drum.

On the carrier of the horizontal kymograph were fastened two electric markers, one, connected electrically with a vibrating tuning fork, marked fiftieths of a second, and the other by its deflections marked the reaction period of the subject. At first a tuning fork of 200 double vibrations was used, but this necessitated such rapid revolutions of the drum that the responses came in close rhythmical succession, and the subject responded when he expected them and not to the signal.

On the pulley of the horizontal drum was fastened a metal attachment that automatically closed and kept closed an electric circuit during one half of a revolution of the drum. The electric marker of the vertical drum, the reaction marker of the horizontal drum, the telegraphic sounder and the telegraphic key were so wired together and connected with the automatic circuit closer that the closing of the circuit gave the subject his signal and recorded on both drums the beginning of each reaction; and the pressing of the telegraphic key by the subject


A
B
C
D
No. 2. 5-26-03,
10 a.m., W + H.
8,8
9,4
9
10
10
9.8
9
10
10
10.8
9
9
9
8

released the reaction marker on the horizontal drum and marked the close of the reaction. For the early experiments the electric current was furnished by storage batteries; but, as these so often proved unsatisfactory, use was finally made of a small dyna-motor which gave a steady reliable current for the tuning fork and the markers.

The only instructions given the subject were that he should keep one position without moving his left hand, and that he should press the telegraphic key with his right hand each time he heard the signal.

Midway between two signals, the release of the telegraphic sounder could be faintly heard, and this became an equivalent for the experimenter's usual 'now.' The length of the time of the revolution of the horizontal drum giving the signals for the reactions was such that the subject was fully able to recover himself before the warning 'now,' and thus there was little fluctuation in the degree of his attention throughout a sitting. Thirteen and occasionally fourteen revolutions of the horizontal drum formed one series of experiments; and, after considerable experience, the experimenter was able to secure three series within an hour.

As the sheets of the kymographs were filled, each was fixed by the usual bath; and, for convenience in reading the records, the sheet containing the vaso-motor waves was pasted across the reaction sheet in such a manner that, for an ocular demonstration, the joining of the points marking the close of the reactions formed a series of curves under the vaso-motor curves (see Plate II.). The readings were taken by measuring in fiftieths of a second the length of each reaction. By means of lines drawn perpendicular to the line connecting the points marking the beginning of the reactions, the exact positions of the reactions in time with reference to the vaso-motor waves were found. The lengths of the reactions, or the reactions in seconds, were then grouped into four groups, as to whether the reaction occurred at the base of the vaso-motor wave, on the rise of the wave, at the crest of the wave, or during the fall of the wave. The reaction times of each group were then averaged according to the number of experiments in each group. A correction of .016

second was made to cover the latent period of the reaction marker.

Five persons served as subjects, Dr. Pillsbury (P.), Mr. Hayden (H.), Mr. Freund (F.), Miss Lee (L.) and Mr. Wright (W.). With the last named, Mr. Hayden conducted the experiments.

Experiments with P. were conducted between nine and ten o'clock, a. m., and the table below shows the results obtained.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	113	.170
Rise of vaso-motor wave,	65	.186
Crest of vaso-motor wave,	90	.194
Fall of vaso-motor wave,	82	.187

H.'s reactions were taken at ten o'clock, a. m., two o'clock, p. m., and four o'clock, p. m. on different days. His records, given below, are considered first as forming one series irrespective of time; then each hour is represented as making an independed series.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	53	.249
Rise of vaso-motor wave,	63	.261
Crest of vaso-motor wave,	42	.270
Fall of vaso-motor wave,	56	.262

Experiments Conducted at Ten O'clock, a. m.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	14	.241
Rise of vaso-motor wave,	24	.262
Crest of vaso-motor wave,	11	.283
Fall of vaso-motor wave,	15	.254

Experiments Conducted at Two O'clock, p. m.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	7	.249
Rise of vaso-motor wave,	10	.252
Crest of vaso-motor wave,	6	.248
Fall of vaso-motor wave,	17	.261

Experiments Conducted at Four O'clock, p. m.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	32	.249
Rise of vaso-motor wave,	29	.264
Crest of vaso-motor wave,	25	.269
Fall of vaso-motor wave,	24	.266

F.'s records, taken at 8 o'clock, a. m., are as follows:

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	9	.334
Rise of vaso-motor wave,	7	.342
Crest of vaso-motor wave,	11	.389
Fall of vaso-motor wave,	8	.299

L.'s record, taken at 2 o'clock, p. m., are as follows:

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	13	.273
Rise of vaso-motor wave,	10	.282
Crest of vaso-motor wave,	19	.293
Fall of vaso-motor wave,	17	.284

W.'s hours corresponded with H.'s and are similarly reported.

Entire Series of Experiments.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	112	.187
Rise of vaso-motor wave,	58	.189
Crest of vaso-motor wave,	110	.201
Fall of vaso-motor wave,	71	.191

Ten O'clock Series of Experiments.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	40	.189
Rise of vaso-motor wave,	27	.191
Crest of vaso-motor wave,	45	.208
Fall of vaso-motor wave,	23	.201

Two O'clock Series.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	47	.190
Rise of vaso-motor wave,	15	.199
Crest of vaso-motor wave,	30	.201
Fall of vaso-motor wave,	24	.186

Four O'clock Series.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	25	.182
Rise of vaso-motor wave,	16	.175
Crest of vaso-motor wave,	35	.190
Fall of vaso-motor wave,	24	.186

It is to be noted that the reactions followed one another in order at a uniform rate throughout a series of experiments regardless of the position of the vaso-motor wave, and that the subject at all times was ignorant of the relation existing between his reaction time and its relative position with reference to his vaso-motor wave, hence the number of experiments occurring in the different groups, *i. e.*, base, rise, crest and fall of wave, differed widely.

A single sheet of records showed little constancy in its results, and a slight variation may be noted in the breaking up of the whole number of both H.'s and W.'s experiments into the hour series; F.'s and L.'s experiments are too few to be of any special significance; yet even in these cases there is a decided tendency toward the results plainly seen in connection with the whole number of experiments of P., H. and W. The records of the three last named persons show clearly a difference in the times of the reactions that occur at the time of constricted vaso-motors, at the time of dilated vaso-motors or at points midway.

To review P.'s reactions, we find his reactions the shortest (.170 sec.) when the blood supply in his hand is at its lowest, and that his reactions are the longest (.194 sec.) when there is a full supply of blood. Between these two points, the reactions are slightly quicker if they occur at the time of the dilating (.186 sec.) than if they occur at the time of the contracting (.187 sec.) of the vaso-motors.

The results of H.'s and W.'s experiments, taken in their entirety as one series for each, agree with the facts already noted in connection with P.'s reaction times; *e. g.*, H.'s are .249, .261, .270 and .262 seconds, and W.'s are .187, .189, .201 and .191 seconds. These same records when broken up into hour series still show, with one or two slight variations, similar relations. The only real discrepancy in H.'s results is to be seen in the time of his reactions occurring at the crest of the wave, but with this it will be noted that the number of experiments (6) is too small to offset general results. With W.'s hour series but two variations occur, and neither one of these materially changes the general trend of the curve of reaction times established by the greater number of experiments.

In reply to our query stated in the beginning of our report, we would repeat that the results of the above experiments show that the subject's reactions form a curve, which, in shape, agrees close with the curve of his vaso-motor, or 'Traube-Hering,' wave.

L. M. Patrizi in 1896 (see *L'Année Psychologique*, Vol. 3, 1897, p. 359)[1] conducted a series of experiments similar to the above with the exception that he had the opportunity of taking the plethysmographic record from the brain of his subject and thus write the curve of cerebral volume. His reactions are recorded in only two groups, minimum and maximum volume, or base and crest of vaso-motor wave. In all he secured 244 reactions, 128 at the crest and 116 at the base of the wave, and his general average of reaction times shows .3325 seconds for the crest and .345 seconds for the base of the wave — just the opposite of those found in our experiments when the record was taken from the finger.

Notwithstanding the difference found by M. Patrizi in his experiments, he concludes that it is too slight to establish any relation whatever between blood supply and reaction time; whereas our results, conducted upon more subjects and in connection with the blood supply of the hand, do show differences great enough to warrant the assumption that the rate of reaction is related to the 'Traube-Hering' wave.

Were Patrizi more sanguine as to the correctness of his results, it would be interesting to discuss the question of the relation of direction between 'Traube-Hering' of brain and finger; but, as it is, no conclusions on this point can be drawn.

[1] Original article, *Archivi di Psichiatria*, 1896. We have seen only the summary.

ON THE HOROPTER.[1]

BY GEORGE T. STEVENS, M.D., PH.D.,
New York.

Of all the subjects in physiological optics none has been thrown into greater confusion by conflicting views of different investigators and none has been surrounded by greater mystery than that of the horopter. Helmholtz, after devoting about ninety pages of his monumental work on physiological optics to the horopter, pages replete with experiments and with abstruse mathematical formulæ, evolved a theory which no other investigator could verify even of the few who claimed to be able to understand it. With all this erudite labor and with all the enthusiastic interest of the great philosopher he worked out a single horopter of the infinite number which may exist and even that one, being based on false premises, was absolutely faulty for well adjusted eyes and entirely impracticable for any eyes.

It is, therefore, when all the divergent opinions are considered, not altogether without an appearance of justice that so astute a man as Giraud-Teulon should have characterized the horopter as a 'transcendental fancy.'

"When," he says, "all the labor of determining the surface curve (fulfilling the geodesical condition of the horopter) was ended it was discovered that this surface assumed the form of a *torus*. * * * It was not noticed that a table with four legs, a chair placed before us, were seen singly although they certainly had none of the attributes of a torus."[2]

Nevertheless the subject of the horopter or to put it better, of horopters, is one of great practical importance. We may emphasize the expression and say that it is one of preëminent importance.

[1] Read before the New York Branch of the American Psychological Association and the Section of Anthropology and Psychology of the New York Academy of Sciences, at Yale University, New Haven, October 20, 1903.

[2] *The Function of Vision*, translated by Owen.

A horopter may be defined as consisting collectively of all the points in space whose images, with a given adjustment of the eyes, fall upon corresponding points of the two retinas.

Notwithstanding the view I have expressed of the notable rank which should be accorded to this subject, the general definition as just given is almost the only point concerning the phenomena of horopters on which investigators, those who have conceded a horopter, have agreed.

By some it has been described as a line, by others as a surface and by Helmholtz especially as a most complex and quite incomprehensible combination of curves, planes and straight lines.

Without entering upon the merits of Helmholtz's propositions that the horopter is 'a line of double curvature produced by the intersection of surfaces of the second degree (hyperboloid to a nappe, cone or cylinder)' that 'it is a straight line and a curved plane of the second degree,' etc., we may for a moment, without accepting the doctrine, consider the position of the horopter according to this philosopher when the plane of regard is directed to the horizon.

"In a single case only," says Helmholtz, "is the horopter a surface, it is when the point of regard is situated in the horizontal and median planes and at an infinite distance. The plane of the horopter is then parallel to the plane of regard. * * * In the case of normal eyes thus directed toward the horizon the horopter coincides approximately with the ground on which the observer walks."

If we consider this proposition with care it will appear that if it were correct its accuracy would involve much ocular inconvenience. We do not look at the horizon when we walk. One who would hold the head erect and direct the eyes to the horizon would stumble often in his march. But, according to the proposition, if the eyes should be directed to the ground at a few feet in advance of the pedestrian he would bury his horopter beneath the soil and all the objects in his pathway would appear, so far as a horopter is concerned, confused and indistinct.

I have taken so much of your time with an introduction in order that we may at the outset form an idea of the present state of the doctrine. Recurring to our definition, if a horopter is the collection of the points in space whose images, with a given adjustment of the eyes, fall upon corresponding points of the two retinas, it follows that horopters succeed each other in endless variety and with amazing rapidity. With every glance of the eyes, with the passing of the line of regard from one part of the page of a book to another, in fact, with every change of the head, of the body or of the eyes themselves and with every degree of convergence a new horopter is developed. A horopter will be formed when the two eyes are so adjusted as to enable the image of the point fixed to be located exactly at the maculas of the two retinas.

The innate impulsion to form a practically complete horopter with any given fixation is so imperious that only insurmountable obstacles will serve as a restraint.

Two tenets or conceptions constitute the essential foundation for the doctrine of the horopter. They are, the theory of the position and direction of the meridians of the retinas and the theory of corresponding points.

In respect to both tenets Helmholtz and most modern searchers in this field have adopted views which have resulted in the confusion in which the subject has been involved.

Before we can proceed to the phenomena of the horopter then it is essential to obtain a correct idea of these two fundamental theories.

We speak of vertical and of horizontal meridians of the retina. They are, like the meridians of the globe, imaginary lines yet they have distinct relation to sight impressions. For example, let us suppose a horizontal meridian passing through the macula, the eye being directed straight forward and the head being in the primary position. The eye fixes a given point the image of which is impressed at the macula. Now if another point at one side of this point of fixation is situated on a higher plane than the point of fixation, its image will be impressed at one side of the macula and below the horizontal meridian. It is unnecessary to consider in detail this doctrine but we may at once assume an understanding of the general principle. Helmholtz, Volkmann, Hering and other investigators came to the more or less uniform conclusion that the horizontal meridians were all parallel with the external horizon but that the vertical meridians were only apparently vertical, and that they leaned out above and approached each other below. Helmholtz's experiments led him to the belief that the vertical meridians of each eye leaned out about 1¼°. A number of investigators immediately found that their vertical meridians in each and every instance leaned out exactly 1¼°. It remains for a society of psychologists to determine how it happened.

My own researches led me to devise the clinoscope which has now become one of the most important and essential of instruments in practical examinations of the eyes. One of the first things which the clinoscope did was to demonstrate that these leanings are natural defects — personal peculiarities — and that they vary with different individuals from one to a dozen degrees; that it is rare to find two persons in succession who record the same leaning. These leanings I have called *declinations*.

Abundant experience in the correction of these defects of declination have demonstrated beyond all reasonable doubt that the proper position for a vertical meridian is the vertical position.

That Helmholtz had what I have called a plus declination for each eye I am convinced. There is much reason however to believe that it was considerably in excess of 1¼°.

Thus, Helmholtz included in his most elaborate mathematical calculations his individual defects which he assumed were physiological features common to mankind.

This was one of the fundamental errors.

The second foundation tenet is the doctrine of corresponding points of the retinas. We may quote Helmholtz's proposition as the accepted view of the doctrine. "*The points which, in the retinal horizons are at equal distances from the point of fixation are corresponding points.*" He states the proposition similarly for the vertical meridians. In fixation with the two eyes the image of the precise point of fixation is impressed upon each retina exactly at the macula or fovea, and, according to the above proposition another point outside the point of fixation will be impressed upon corresponding retinal meridians which in the case of each eye will be equally removed from the macula. In respect to corresponding points in *the field of vision* we may quote again. "*Corresponding points in the two visual fields are those which are at equal distances and equal in direction from the corresponding horizontal and apparently vertical meridians.*"

While this proposition is not altogether clear it is evident from the context that according to it a series of points equally distant in the field of view and from which proceed lines of direction toward equally distant points in the retinas are corresponding points.

These propositions can not both be true except under circumstances entirely at variance with Helmholtz's illustrative experiments. These experiments are made, not with curved surfaces, hollow spheres, but with plane surfaces like the usual stereoscopic cards or the flat page of a book.

Accepting the experimental illustrations as the only practical tests, the two propositions are inconsistent.

Let us first suppose the distances between corresponding points on the horizontal meridian of the retina are exactly equal. Place a sheet of paper exactly in front of the eyes on which are several points in a straight horizontal line corresponding to the plane of regard (Fig. 1). Let the eyes be fixed on the central point. Then, according to the first proposition these points are not corresponding, for straight lines drawn from them through the two nodal points to the retinas will not form equal angles and will not meet equally removed points of the two retinas.

Suppose our points to be one half an inch distant from each other and fifteen inches in front of the eyes while the two eyes fix the central point, *A*. Then will the line of incidence passing from the point at the right of the central position, *B*, form with the line of incidence passing from the point of fixation through the nodal point to the macula of the left eye an angle of $1° 54' 5''$ while the angle formed by the line of incidence from this same secondary point will constitute with the line passing from the point of fixation to the right macula $1° 53' 26''$

Passing to the next succeeding point of the series, the incidental line from the new point will form with the line of inci-

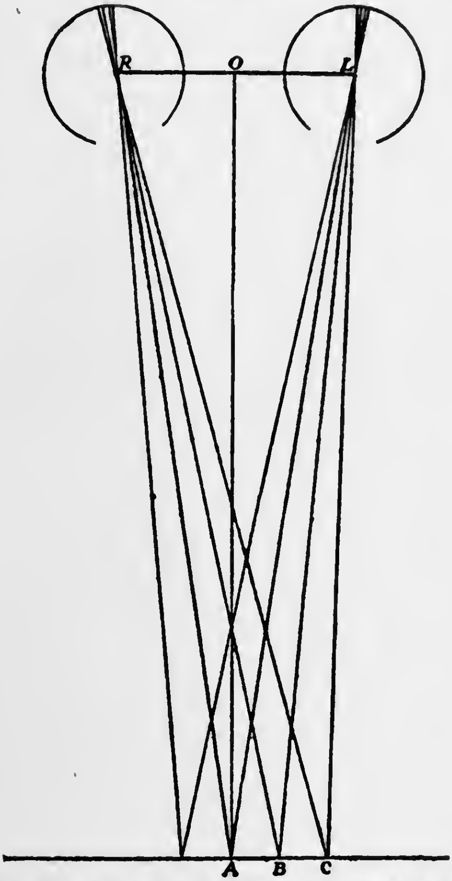


FIG. 1. Let R and L be the nodal points of the two eyes and A the point of fixation. The points B, C, etc., are outside the point of fixation. Suppose

$$RO = 1.25 \text{ in.}, \qquad OA = 15 \text{ in.}$$

$$\angle RAO = 4^\circ\, 45'\, 49''$$

$$\begin{cases} \angle ARB = 1^\circ\, 53'\, 26'' \\ \angle ALB = 1^\circ\, 54'\, 5'' \end{cases} \qquad \begin{cases} \angle ARC = 3^\circ\, 46'\, 1'' \\ \angle ALC = 4^\circ\, 39'\, 58''. \end{cases}$$

The points corresponding to the incidence of the lines CR and CL are not thus equally removed from the maculæ.

dence from the fixation point to the left macula an angle of 3° $46'$ $1''$. The incident line to the right eye will form with the original line an angle of $4^{\circ} 39' 58''$. It will thus appear that in this example, passing from the macula toward the periphery of the temporal side of the retina the angle increases while on the side of the retina medial to the macula it decreases.

Since these incidental lines cross at the nodal points it is evident that they must extend to unequal distances in the vault of the retina.

Müller, recognizing this, believed the horopter to consist of a circle passing through the nodal points of the eyes and the point of fixation and of a vertical line. This in fact amounts to no horopter. Time does not permit an examination of all these theories and it is sufficient to say that no theory based on equal distances for corresponding points of the retinas can serve as a satisfactory basis for a doctrine of the horopter.

These two tenets on which the whole structure has been erected being rejected the doctrine is to be abandoned or new basic theories must be found.

As already remarked, the clinoscope and practical work based upon its revelations have demonstrated that beyond all question the vertical and horizontal meridians of the retinas are, in typical cases, precisely what their names imply, exactly horizontal and exactly vertical. We may then substitute this fact for Helmholtz's theory and it will serve as our first basic principle.

In respect to corresponding points it is unnecessary to say that there is no such anatomical symmetry as to demand that equal extents on the retinal surfaces should represent equal extents in the field of vision. It is not the fact. It is however, true that there is an innate sense of the goniometrical value of motor impulses directed to the muscles of the eyes, and that the distances between retinal corresponding points need not be symmetrical for the conception of this muscular sense but that nevertheless they bear certain mathematical relations to each other.

We may define corresponding points of the retinas then as, *those points in the retinas which answer to proportional degrees of rotations of the eyes about their centers of rotation, and which, from given points in the plane of the point of fixation receive incident rays which must pass through the nodal points.*

They represent therefore the relation between the muscular and the retinal senses.

The definition is perhaps less easy to the average comprehension than it is to divide the retinas into squares of millimeters and point off so many to the temporal side of one and so many to the medial side of the other retina and call these corresponding points. There are various combined physical and psychical functions which are not to be measured by a pocket rule.

The actual movements of the eyes about the rotation centers are not always essential to an estimation of the relative positions of objects in space. In the absence of the objective movement there is the subjective conception of the impulse required to induce a given movement. We are all familiar with the experiment of Dove in which the observer, looking into a dark box until the eyes are supposed to have assumed parallel directions sees an electric spark within the box and it is seen singly. Of course the impression is made at the temporal side of each macula and there should be by rigid rule, when impressions are at these non-corresponding points, an impression of two sparks seen heteronymously. The rule in this case is not tenable.

If there were actually two sparks there would be two impressions on each retina whereas as a matter of fact there is but one. The consciousness of a single image for each retina and of its position external to the macula leads to the conclusion that a convergence of the eyes would be required to locate the image at the macula and the extent of the required motion would indicate the angle of convergence and therefore the distance of the spark. Of course there are other elements in this complex psychical phenomenon but that mentioned is enough to suggest the course of the psychical process. It is such processes of unconscious conclusions that bring many points within the field of vision into a *subjective* horopter.

Before proceeding directly to examine the principles of the horopter it is necessary to recall some of the changes in the directions of the meridians as the eyes pass from one point of fixation to another. When the point of fixation is at infinite distance and in the median plane all horizontal meridians are horizontal and all vertical meridians are vertical. So also if in the plane of the horizon the point of fixation is brought nearer, the meridians maintain their original relations and these relations will also continue if the two eyes are directed upward or downward provided the visual lines remain parallel. But if the point of fixation is at such distance as to demand convergence of the lines of regard and if it is above or below the horizon (the head being supposed to be in the primary position) all horizontal and all vertical lines assume new directions. The eyes rotate on their antero-posterior axes. This form of rotation is known as torsion. These torsional rotations are governed by fixed laws and the general principle is known as the law of Listing.

Should the visual lines of the two eyes converge at the same time that the plane of regard is depressed the horizontal meridians of each eye will tilt downward toward the temporal side and upward toward the medial side. The vertical meridians will also tilt with the upper part outward and the lower part inward. The tilting is in every case in proportion to the depression and the lateral direction of the line of vision.

Accepting the two basic principles as they have been stated and with an understanding of the laws of torsion we are in position to examine the phenomena of the horopter, eliminating the mathematical intricacies of Helmholtz and substituting only simple calculations in plane trigonometry. Time will not permit us to inquire in detail into its form in many positions, three will suffice to illustrate the principles and the details of only one of these need be given.

First the observer directs the gaze toward the horizon in the median plane at infinite distance, the head being in the primary position. A horopter is formed at the distance of the point of fixation and it will be a plane surface at right angles to the plane of regard. Objects within or beyond the distance of the point of fixation will not be in the objective horopter but may be in what we may term a subjective horopter. They may be impressed on the two retinas and they will appear, as in the case of the spark in the Dove experiment, as single, the principles controlling the psychical phenomena in that experiment being here modified to meet these different conditions.

Second, if the gaze is directed somewhat downward and to a point a few feet in advance, as in the case of a person walking, the horopter will still be very nearly at right angles to the plane of regard, tipping forward slightly since, although there is depression (a negative ascensional angle) of the plane of regard, the convergence (the lateral angle) is so slight as to induce small torsional action and the principle of objective and subjective horopter may be applied as in the first case.

There is also, at the lower part of the field of view a bending in of the horopter so that more of the pathway is in the horopter than would be were it through its whole extent a plane.

Coming to the third case we may proceed in more mathematical detail.

Let us suppose the case in which the eyes are directed to the page of a book in the ordinary position for reading.

Assume that the gaze is directed so that the point of fixation is in the median plane, and that the plane of regard is depressed 35°. Assume also that the distance between the nodal points of the two eyes is 2½ inches and that the convergence of the eyes (the lateral angle) is for each eye 5°. We have from these data to determine the distance of the horopter and its form and position relative to the place of regard.

To determine the distance of the point of fixation (which will be in the center of the horopteric field) we have the base, 2½ inches and the lateral angles 5°. Taking one half the base

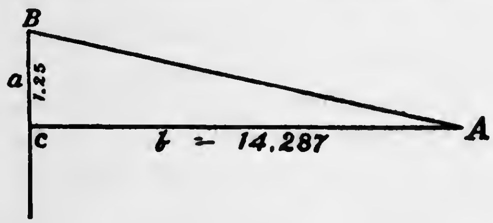


FIG. 2. Angle $A = 5°$, $\frac{b}{a} = \text{Cot } A$, $b = 14.287$.

and one lateral angle we have a base of 1¼ inches, a right angle and an angle of 5° to find the perpendicular or distance from the base line to the page of the book which is readily found to be 14.28 inches (Fig. 2).

The distance being ascertained by the formula $\frac{a}{b} = \cot A$, *a* being the base, 1.25 inches, *A* the angle opposite the base and *b* the distance sought. At this distance from the base line the image of the point of fixation will be exactly at the macula of each eye.

According to the law of torsions by this depression of the gaze and the convergence the meridians will have left the horizontal and vertical positions. Referring to the table of torsions found in Helmholtz's work[1] we find that for the ascensional angle of 35° and lateral angle of 5° the tilting of the horizontal (and of the vertical) meridians is 1° 35′. These conditions being given what will be the relation of a straight line passing horizontally through the point of fixation across the page to the horizontal meridians of the retinas now tilted 1° 35′ from the actual horizon. A series of points in a straight line thus passing through the point of fixation must impress themselves along the horizontal meridian of each eye otherwise the points will appear confused or double. But how can this series of points in a horizontal line be impressed upon the meridians which are tilted up toward the nasal side each 1° 35′.

It is a most interesting fact that the images of these points will in fact be thus impressed exactly along these tilted meridians of the retina and it is precisely because these meridians of the retina are thus tilted that it is possible for the impressions to be made along the proper meridians.

Too much space would be occupied were we to enter upon a mathematical demonstration of this statement but a little consideration by one familiar with the relation of lines and angles will show that in principle the statement is correct. A demonstration however would show that beyond a certain degree (10° to 20°) in the plane of regard a straight line actually appears to curve.

We come next to the more complicated question in respect to the position of a line running from the top to the bottom of the page. Will this line be at right angles to the plane of regard as the horizontal one is parallel with it or will it lean more or less toward or from the plane of regard?

We may select points above and below the point of fixation and determine their distance from the base line and thus obtain the angle of the surface of the book to the plane of regard.

[1] *Optique Physiologique*, p. 607.

Take, first, a point 5° above and one 5° below the point of fixation. The distance of the point of fixation from the base line connecting the nodal points has already been determined at 14.28 inches. In that case there was a lateral angle of 5° for each eye. Now, since the vertical meridian of the retina tilts out as it rises above the macula this lateral angle will increase as the image is impressed above the macula and it will decrease in proportion to the extent that the impression is made below the macula. Before we can proceed, therefore, it is necessary to find the exact amount of increase and decrease for the selected points 5° above and 5° below the point of fixation, since our angle of convergence will increase in proportion to the extent to which the vertical meridian leans out from its original position exactly at the selected distance and decrease in proportion as the meridian leans in below the macula at the selected distance.

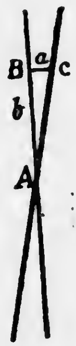


FIG. 3.
Angle $A = 1° - 35°$,
$\frac{a}{b} =$ Tan A,
Tan $A = .02764$,
$b = 5$,
$a = 5 \times .02764$
$= 0° \, 8' \, 52\frac{1}{2}''$.

We may find the extent of removal by the formula:

$a = 5 \times .02764 = 0°, 1382 = 8' 17\frac{1}{2}''$. (Fig. 3.)

$\frac{a}{b} = \tan A$; $\qquad \tan A = .02764$,

In which b is the selected distance above or below the macula, a the required increase (or decrease) in the lateral angle and A the angle of 1° 35′.

This gives .1382 of a degree which is to be added to our lateral angle (angle of convergence) when we can proceed as in the first case to find the distance from the base line to the selected point below the point of fixation (Fig. 4), $\frac{b}{a} = \cot A$, in which b is the distance sought, a the base line, 1.25 inches, A the angle opposite a, 5°.138. From this we find that $b = 13.904$ inches.

To obtain the distance of the point above the point of fixation we must subtract the 0°.138 (0° 8′ 17½″) from 5° when by the same formula we find the distance to be 14.6976 inches (Fig. 5.)

We have now the distances

5° above the point of fixation	14.6976
At the point of fixation	14.287
5° below the point of fixation	13.905

Forming from these distances two triangles by joining the three lines at their extremities we have a line joining them and

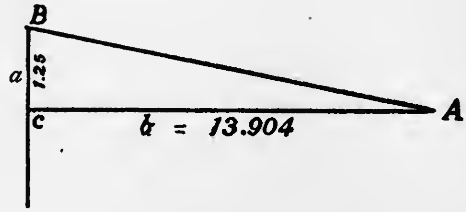


FIG. 4.
Angle $A = 5^\circ\, 8'\, 52\tfrac{1}{2}''$.

forming bases which represent a vertical line in the horopter at the level of the page of the book (Fig. 6).

The acute angle at this surface of the book for the upper

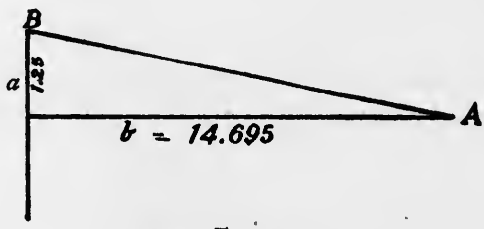


FIG. 5.
Angle $A = 4^\circ\, 51'\, 7\tfrac{1}{2}''$.

triangle of these two is, 69° 38′. That of the lower triangle is 70° 48′ 50.″

We have thus, in the space of 10° up and down the page, a curve of about 1° In other words the horopter in this direction is approximately a plane surface. If the calculation is carried to 10° each way, as in Fig. 6, equal to a space to about five inches on the page of the book the result is nearly the same but the curve is somewhat greater as we approach the periphery of the field of vision.

This gives us the position of the page in relation to the plane

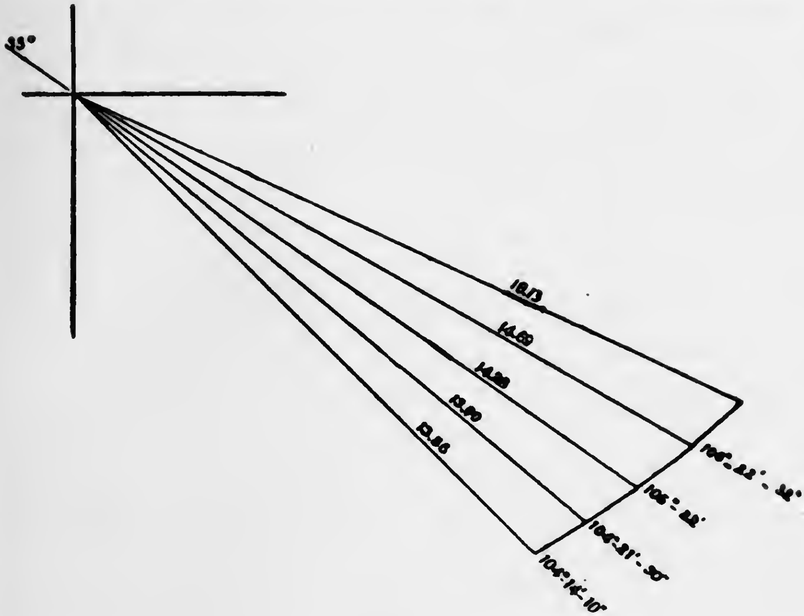


FIG. 6.

of regard in which the horopter is most completely formed and we find that the page is tilted about 15° beyond the right angle with the plane of regard, or at about 105°. We have found only

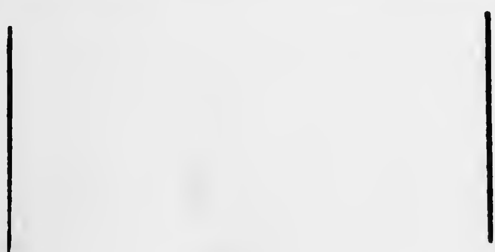


FIG. 7.

the direction of the horizontal and vertical meridians of the horopter but any other meridian may be found in a similar manner.

An interesting and very simple experiment for those who are able to unite stereoscopic figures by convergence without the aid of a stereoscope beautifully confirms the above calculation.

Draw two vertical lines parallel and at a distance of two and one half inches from each other on a card board (Fig. 7).

Hold the card board so that in fixing the center of the lines the gaze is directed downward 35°. Hold the card board twenty-eight inches from the eyes.

One who is expert with such exercises will be able to unite the two lines at the distance of fourteen inches from the eyes.

If, instead of permitting a perfect union of the lines in the stereoscopic image they are held at about one eighth of an inch asunder it will be easy to find at what angle the board must be held to render the two stereoscopic images exactly parallel.

In my own case I find by numerous experiments and careful measurements that the board must be tilted forward as nearly as can be ascertained exactly 15°.

I have 1° of declination of the right eye which would have little influence on the experiment.

Thus mathematical and experimental research lead to practically the same result in locating this horopter. By the formula given we may locate any horopter in the median plane. In other planes the formula will be modified.

Without discussing the application of these principles to space perception, a field of much interest and in which many empirically known facts in art and in architecture may be analytically tested, only brief time remains to allude to the more practical application of the horopter.

All the discussion which has preceded has been based upon the assumption that the adjustments of the eyes are typical in the sense of being the most favorable to the function of combining the images of the two eyes in a horopter.

In real life anomalous conditions of adjustments, conditions which interpose difficulties in forming perfect horopters, are of extreme frequency.

These anomalous conditions may act as slight hindrances or they may prevent any but an imperfect horopter from being formed.

Let us consider some of these.

It has been seen that with a given depression of the plane of regard and a given convergence a horopter is formed in a position which can be predicated when these two elements and the length of the base line between the nodal points are known. The depression of the plane of regard is controlled by an impulse which is not accidental or ephemeral, but which is automatic and uniform for different persons for the same depression under like circumstances.

Suppose a person whose eyes are so adjusted that with the minimum of impulse to the governing muscles they are directed 8° or 10° of arc above the plane of best adjustment. Among people of New England ancestry this is almost a characteristic as it is with some other groups of people. It is not a disease, it is the normal development from a certain form of cranium.

Suppose again that this person takes a book in hand to read. He holds it in the position and at the distance which we have assumed for our third horopter. Is it not plain that this person must not only depress the plane of regard the 35° assumed, but that he must also induce an additional depression of 8° or 10° as the case may be?

This extra depression at once automatically induces a greater tilting of the meridians. No horopter can then be formed. To remedy the difficulty in a measure the person may throw the head forward 10°, but in so doing there is some disturbance of the equilibrium of the muscles, hence even with this concession the horopter, which will be better than before, may still be somewhat imperfect.

In several papers I have shown that as a matter of fact people with this adjustment of the eyes do throw the head forward and the bending of the neck is, other things being equal, in proportion to the excess of the normal upward direction of the eyes. We will return to this presently.

A second condition which may interfere with the formation of a horopter in the appropriate position is in direct contrast to this. The eyes may be adjusted so that the plane of vision is normally directed low.

Suppose one whose eyes are 10° too low. By the same reasoning as before we see that because the dynamic depression of the gaze would be less than in the typical adjustment the tilting of the retinal meridians would be insufficient for the horopter and such a person must force the chin high in the air in order to be obliged to depress the gaze sufficiently to induce the necessary torsion.

I have written of these conditions and writers have interpreted the difficulty as a strain on the muscles of depression or elevation.

This is an entire misapprehension. It does not follow that there is any considerable strain on the muscles of adjustment but the head must be placed in position in which the automatic torsions shall in some measure correspond to the direction of the gaze.

A third form of hindrance to the constitution of the horopter is found in the condition which I have called declination.

This consists of an anomalous leaning of the meridians of one or both eyes. It is a very common defect and results in great perplexity to the adjusting muscles. Its practical importance is greater than those conditions already mentioned. It may induce, like the two conditions named, a throwing forward or a tipping backward of the head, depending on the direction, symmetry or degree of the declination defects in the two eyes. All that has been said about the forward and backward holding of the head in the other conditions may apply to these cases and in some instances the unnatural pose of the head and body from this cause are extreme.

What I have to add might perhaps better be addressed to a company of physicians than to psychologists yet in order to comprehend the importance of a subject we must know something of its practical application.

Recall the case of the person whose eyes are adjusted for too high a plane. The head is thrown forward as part of the automatic process of adjustment. The larynx is partly closed, the chest is sunken. Air passes less freely to the lungs than it would were the head held erect. It is among this class of people that consumption commits its ravages. There are few, if any consumptives who do not have a high adjustment of the eyes or a form of declination which induces a corresponding head position.

Then there is the person whose eyes are adjusted for too low a plane and whose head is thrown back.

It is with this class of persons and with those whose declinations induce a similar pose that the occipital neuralgias, pains in back of the head and neck and in the lumbar region are found. The number of such persons is enormous and the suffering from this cause infinite.

From declinations which do not induce false carriage of the head arise perplexities in adjusting for the horopter which result in headaches, dyspepsias and a long array of nervous ills.

A subject whose importance cannot well be overestimated has been presented in this brief outline in the hope that notwithstanding the necessarily incomplete nature of the presentation, some interest may be awakened among men whose special training peculiarly fits them for more elaborate investigations in this most difficult yet notably practical field of inquiry.

THE LOGICAL AND PSYCHOLOGICAL DISTINCTION BETWEEN THE TRUE AND THE REAL.

It was Mrs. Carlyle (was it not?) who said that 'mixing things is the Great Bad.' To the writer it seems that there is a peculiarly injurious variety of the 'Great Bad' in much of our recent psychological logic. It is because that sort of philosophy which the writer for over fifteen years has been calling 'dynamic' and which now seems to have come to its own under the name 'functional'—it is because, we say, that this kind of dynamic philosophy and functional psychology is peculiarly adapted to correct this 'mixing of things' that the writer offers a few words upon the distinction between the 'real' and the 'true.'

This sounds like a question of definition and a matter for logic to dispose of, but we submit that it is also a question of psychology, and that psychology has already made a distinction (also a matter of definition, to be sure, the facts having been understood from time immemorial) peculiarly adapted to explain the logical distinction here required.

It is remarkable that recent writers seem not to have been aware of the ambiguity arising from the identification of the real with the true. The present writer has elsewhere defined reality as 'affirmation of attribute' and this dynamic statement may usefully be contrasted to Lotze's descriptive definition that 'reality consists in standing in relation.' Upon critical analysis the two statements come to the same thing, but our present method in both metaphysic and psychology requires the dynamic form. Nothing can be real apart from a realizer. As Hoeffding says, 'The real is what we apprehend as real—which, in spite of all effort to the contrary, we must ultimately leave as it is—which we cannot but recognize,' though he at once goes on to confuse this real with what is true.

It may be assumed that all will agree with our definition of simple reality as a statement of metaphysical reality. Dewey says: 'The copula gives the statement of being, asserts the reality.' But he, too, goes on to discuss truth as relational. In our own extended discussion we endeavor to point out the union of subjective and objective in this identification of essence and attribute, which is only possible in an active percipient.

The logical abstraction of 'pure being' as the activity of the subject apart from the content (meaning — *i. e.*, attribute) is possible, but it involves, as Hegel abundantly showed, the loss of reality. Pure being and non-being were in this sense the same, both being all one to the subject who demands the *act* of asserting or identifying as well as the *mode* asserted.

Professor Baldwin has made, as we intimated above, the important distinction between psychic and psychological, and both Professor Bawden and the writer have shown that the psychic cannot become the subject of scientific analysis. Nevertheless it does afford the foundation on which science (the psychological) must rest. The predicate of reality pertains and can pertain only to the psychic. We do not construct reality but simply perceive (affirm) it. This ultimate fact in experience is reality. The opposite to real is not false, it is non-existent or unreal.

If it be objected that this limitation does violence to common usage it must be replied that any necessary logical distinction may do the same. The distinction between psychic and psychological traverses ordinary usage from end to end but if it expresses a true distinction it is well worth while to reconstruct terminology. In fact, it may well be that any further great advance in psychology must wait for a wholesale reformation of terminology.

The point is that we must have a word for this primary feeling-cognition which we have called reality. Reality is not something we say about experience but a *quale* of experience itself. We ascribe truth to *relations* of things or events among themselves, or ultimately as parts of a universe of things and events. Any reality would be no less real if it existed alone. If we must use Lotze's definition of reality as a 'standing in relation' we should say reality grows out of a relation to the subject alone, but this is a metaphysical after-thought.

When the naked fact of experience comes to be thought about or, in Baldwin's language, becomes psychological, we begin to develop relations which are true or false in so far as they do or do not cohere in an organized whole. The whole duty of science is so to cause the facts thought of to cohere in an organization. This is the sphere of truth.

There is a sense, however, in which reality escapes from the limitation of the psychic and sits enthroned over all thinking. In last analysis elements of our thinking have to be verified by reference to real experience. Sometimes we get a long way from such experience in abstract thinking. We keep building one set of relations upon another, trying with all our powers, meanwhile, to keep these relations true among themselves, much as one might work out the orbit of a comet, but at last the test is whether things in experience stand back of the true relations — whether the comet can be really found in this orbit.

When Höffding says: "The evidence of reality is given, then * * * in the firm connection of percepts. We can never be so strongly convinced of the reality of single things and occurrences, as of connected series of things and occurrences," he has confused reality and truth. Compared with the earlier statement quoted above, the incongruity appears grotesquely. He said that, 'in spite of all effort' we 'can't help' recognizing reality, and that there can be no question of any other than this subjective criterion, and now he proposes to add to this once-for-all reality greater reality by multiplying relations. But this is just the difference between truth and reality. Reality, once realized, can by no possibility be improved upon or made more real, while, on the contrary, truth grows more certain the more nearly all known relations are found to cohere with the given relation.

It is not meant by this, of course, that the truth increases with the number of instances, as in the common logical fallacy, but truth becomes more convincing the greater the scope of interaction discovered. The truth that all Felidæ are carnivorous is not greatly increased by observing one cat repeatedly to eat meat nor by seeing that one kind of cat always eats meat, but the finding that a different species of animal combines feline dentition with a carnivorous habit adds greatly to the evidence by proving that certain combinations are non-essential and throwing into prominence the organic or genetic relations.

Bosanquet seems to state the law of reality in the definition: 'Logic treats of the mental construction of reality,' 'the world which surround him is there only as an idea, *i. e.*, only in relation to something else, the consciousness which is himself.' But immediately and, indeed as in duty bound (his subject being logic) he proceeds to discuss the true. For him the objective world is 'what we are constrained to think in order to make our consciousness consistent with itself.' In other words, reality consists in consistence of relations, which is precisely truth. Logic might be defined as the science of truth.

Perhaps the discrimination of reality from truth may even help in the much discussed problem of the subjective and objective. For example, when we discover (by a round-about means) that a presentation has been made to consciousness we also get directly (subjectively) an affirmation of attribute. This is an ultimate of experience. It does not make an external world. 'Light is,' and that is all there is about it. But when I, psychologically, accumulate a lot of data and construct the concept of *substance*, this is a matter of relation. The brightness, heat, weight, etc. are made to cohere in the substance, 'candle,' a thing projected out of self and, by implication at least, contrasted to self, as an object. All these relations of activities are true to the extent that they cohere in one system or organism. When the question arises in our metaphysics as to the truth of the objective world as a whole, as it will when we become aware of the subjectivity of all knowledge, there is but one answer — the one already used. The objective world is true because it is in one organism with the subjective mind. Just as our partial judgments are true or false as they prove to be founded on relations in one whole, organically, not to say causally, connected, so the larger judgment 'there is a true external world apart from the mere act of perceiving it' is true only if the percipient or perceiving force be organically part of the same universe. No other criterion is possible.

The feeling of reality comes from the immediateness of the elements of experience.[1] It defies analysis and requires no definition and yet is implicit in all practical life. The judgment of truth, on the other hand, is a fluctuating evaluation based on relations which are known rather than felt. The weight of evidence forces me to believe what is true, I require no evidence to cause an experience to be real nor will any amount of evidence lessen its reality.

The old illustration of the inability of the blind to realize visual data though they may weave about them all sorts of relations, of the truth of which they are fully convinced, may not be realizable by the non-blind. To this end let us take another example.

A friend of mine who is expert in both physics and physiology, informs me seriously and in detail that he has discovered that by using the radium waves β, and passing them through a set of refracting appliances, he is able to produce a series of irritants which, when applied to the sensitive nerve plexus in the hollow of the human foot, give rise to sensations unlike any other. They possess a great keenness and penetrating force and seem to vibrate throughout the organism by a process of excessive irradiation. Each of these sensations has the

[1] Cf. Baldwin's explicit treatment of 'Reality-feeling' in distinction from 'Belief' in his *Feeling and Will*, Chap. VII.

peculiarity of localization in certain parts of the body. One 'wave-length' causes irritation at the root of the tongue and marked increase of blood supply. Each is also accompanied by its own emotional response, so that one kind of stimulus predisposes to religious fervor and exalted egoism and the other causes morose and turbulent passions. One even produces a violent desire for something of which no concept can be formed. Now I may believe all these as true statements of fact but they do not nor can they produce in me any sense of reality such as five minutes of actual experience might produce.

The writer believes that a consistent limitation of these words to the spheres respectively indicated will lighten the burden of the student of metaphysics as well as of psychology.

The loose use of the words real and true in psychology coupled with clear consciousness of the distinctions involved is encountered in James' Psychology. "The sense that anything we think of is unreal can only come when the thing is contradicted by some other thing of which we think. *Any object which remains uncontradicted is ipse facto believed and posited as absolute reality.*" But the only thing that can never be so contradicted is immediate experience. A subsequent experience may *explain*, it can never *annul* it. The only things that can so be contradicted are judgments of relations. The presentation 'rain-bow' is real, but the judgment 'rain-bow now in the sky' can be proven untrue.

If the word 'real' be considered to have too strongly intrenched itself in the wide sphere in which it has been used so carelessly, surely a new word is required for the primary affectation of consciousness called 'sense of reality' and 'reality-feeling.' The further characterization 'a sort of feeling more allied to the emotions than anything else' may, perhaps better apply to the recognition of truth. The reason for this relation to the feelings will be found in the nature of feelings. The writer in his inhibition-irradiation theory of pleasure-pain (which has recently received a psychological restatement by Fite[1] as 'resistance theory') has attempted to derive all emotional acts, physiologically considered, from resistance, obstructions, depletion, or other interference with the flow of nervous impulses, so that there is irradiation or inhibition respectively. If this derivation be correct it will follow that all acts of identification must share in this peculiarity. The new concept meets a barrier at the threshold of recognition which is finally thrown down and the wave of thought finds outlet in a path of least resistance, it is identified with previous acts. This release affords the

[1] PSYCHOLOGICAL REVIEW, X., 6.

condition for pleasure. Identification in one form or another, is back of nearly all intellectual pleasures. Discovery of a true relation is accompanied by pleasure, failure to identify is painful.

It is not without interest in this connection to observe how easily and satisfactorily the dynamic (functional) psychology disposes of the confusion expressed in the classical discussion between nominalism, realism, and conceptualism. So long as precepts, recepts, Anschaungen, concepts, and the like, are conceived as possessions or contents of the mind this discussion is inevitable, but when we become fully aware that these are names for acts or parts of processes the difficulty disappears.

When a mode is perceived there is a simple psychic act, even though the stimulus be of the most varied character. Here we have to draw a line as important as any in psychology. We, from the outside as observers, say that a stimulus has been perceived, but what we actually did was to affirm a mode (quality, attribute). Subsequent (psychological) activities consist in combinations of this material into relations. The act of perceiving does not posit any relation (unless the implicit relation to the subject be so considered, and this is thought back into the psychic and is a matter of metaphysic and not of psychology). Psychological work is all apperceptive; its processes are all synthetic (even its analyses). What Romanes calls a 'recept' is a thinking together of percepts. This unifying work of consciousness is a function of its unity which, as an equilibrium, is organically necessary. All organization must unify.

Here, for example, is a roll of paper passing through a ruling machine armed with many pens. I load one pen with blue ink and from that time forward a blue trace moves along the paper along with the red, green, and black traces. I may shift the adjustment here and there and these traces are brought into various relations, forming patterns, etc. The initial inking is the perceiving. This process adds to the activities in the mind a new one which may be shifted, combined, and modified in various ways but never thereafter will the mind-process-group be the same as it would have otherwise been. The psychic equilibrium has been changed. The relations between the several percepts is infinite but some of these are employed instead of others in our constructive thought. Out of activities, all of which cohere in an organism, our selection of part and our conception or thinking together is more or less an act of violence and must always so remain. In so far as a teleological nexus is formed the thinking together is true, in so far as the union is a purely arbitrary one or non-teleological, it is false.

For example, in our classification I have a concept, 'gopher-genus *Geomys*.' Another naturalist has another idea of the limits of the genus. Our concepts may be equally true but this truth consists in both cases in the recognition of a teleological bond. He perhaps includes more of the segment of evolution or career of the 'gopher movement' in nature than I do. The difference is nominal, the agreement is conceptual. We may not say in either case that *Geomys* is a *real* thing but it does in both these cases represent a *true* concept. Let the generic limits of *Geomys* once be set and agreed to, I then place in the genus an animal proving to belong to another line of descent, the reference is false. It is a question of relation.

It is wrong to say that a concept is only a name. It does exist in nature as the subjective expression of the truest thing we know and the most important. It is a 'genetic' verity. It is a career — a doing, in relation with all doing. It is a teleological verity.

But, it may be said, we are only holding a mirror up to nature and see the trajectory of a flying bird, for example, momentarily depicted thereon, or we are but exposing a sensitive plate in a telescope and get only a bright trace thereon. But these illustrations do not go far enough. In order that the plate may receive the star trace correctly, the mechanism of the telescope must follow the path of the star. There must be coördination. So our concept is a conceiving or following of the trajectory of nature. The proof of correctness is exactly the interaction. Our conceptualism has, therefore, a link to realism in that only upon the assumption that we are part of the organism from which the stimulus comes could these correspondences become intelligible. When we no longer find the trace on our photographic plate we adjust the movement. The feeling of reality and the conviction of truth have their justification in the monistic construction of organism.

C. L. Herrick.

Socorro, New Mexico.

THE PERIOD OF CONVERSION.

The recent scientific study of religious experience has led to many interesting and important results, not the least of which is the relation between conversion and the period of most rapid growth. But the difficulties attending the study of such phenomena are evident here as elsewhere. Among these difficulties may be mentioned that of dealing with a very complex group of mental phenomena without any adequate method of controlling their conditions. The usual method employed is the 'questionnaire,' supplemented more or less by hypnotic and other experiments. The chief reliance must be placed upon answers given at a distance, to questions which may or may not have been perfectly understood, by persons more or less accustomed to scientific observations and dependent upon their memories of events which transpired in the past. Several obvious sources of error lie in this method. First, the unreliability of memory and second, the unreliability of the observer's judgment as to the meaning of the question and as to its proper answer. But a third objection and this time a purely psychological one, arises from the complexity of the phenomena under investigation. Professor Wundt used to say in private conversation (but whether he has ever put it in print the writer is not able to say), that one can seldom be sure as to the meaning of answers secured by the questionnaire method, because of the complexity and consequent variability of the factors entering into the observer's judgment. Descriptive data of a more or less exact character are the most that can be hoped for.

But this matter of investigating religious experience is beset by two further difficulties which threaten its scientific value. To be thoroughly scientific, experiments and observations must be of such a nature as to be repeated by others; while in these matters, both the significance of most of the questions asked and the interpretation of the answers are alike beyond the control of other investigators. Consequently, the valid results of work in this field are fewer in number than might be expected considering the number of investigators and the extent of their efforts.

These difficulties are illustrated in the investigations as to the period of most frequent conversions. Different writers do not seem to take the term conversion in the same sense. To one author it means the change in human character by which *any* set of religious ideas and aims become the center of a man's life; to another it is that change in man's character by which Christian ideas and aims become central in his life. Now, for the psychologist, Christianity is a set of aims and motor attitudes belonging to religion as its genus; Christianity is a species of religion. Men may become religious without being Christians although they can not become Christians without becoming religious.

With this distinction in mind let us look briefly at this question of the period of conversion. By religion in general let us understand a natural consciousness of relations to a Totality of Existence of which we are a part and upon which we depend, together with the beliefs, types of action and institutions which have grown out of this consciousness. Lower animals and young children are not religious because they have no notion of themselves as subjects of experience, They do not think of themselves as personally identical, as capable of right and wrong conduct and of laying plans for the future. A man may think of himself as an animal and regulate his conduct accordingly; he may think of himself as a member of a certain church, family, club or political party; and in each case his thought of himself and his belief about himself are the most important factors of control in his conduct and character. If it were possible for me to determine your thought of yourself I could afford to let your conduct take care of itself; if I had absolute control over your idea of yourself, I could let your religion take care of itself. This reflective thought of self is a man's recognition of his peculiar place and function in the totality of life out of which he has arisen, to which he is organically related and upon which he depends. He may conceive himself, as Tolstoi says, as a spirit passing through a series of existences so related to each other that his conduct in one existence determines his position in the next. He may conceive himself to belong to a people chosen by God to receive protection and blessing, upon condition of perfect obedience to God's commands. He may think of himself as one of the thoughts and purposes of an Absolute Being who is perfectly rational and perfectly good. However he may conceive the Total Existence of which he is, in some sense, a part, and however he may define his position in it, this conception and definition together with the emotions and conduct growing out of them, constitute his religion, his faith. Conversion, in the naturalistic sense of the term, is that inner change by which some such conception and definition, constituting a set of religious ideas and aims, become central in a man's personality.

Now I suppose it is conversion in this sense which has been found to be one of the regular phenomena of adolescence, usually occurring somewhere between the ages of twelve and twenty-five, the year of greatest frequency being the seventeenth. Accordingly, the period of most frequent conversions seems to come just after the periods of greatest brain-weight and of greatest increase in body-weight. Professor Starbuck's conclusion is that the periods of conversion and the periods of most rapid bodily growth tend to coincide.

I suppose the most important result of these investigations is the conclusion that conversion, in some broad sense, is the normal experience of every man, marking the transition from his childhood to his youth, or that from youth to his manhood. It is the step by which an adequate sense of selfhood is approached and made possible. Conversion as a natural phenomenon is a deepening and broadening of one's ideas of himself which is at the same time a deepening and broadening of one's ideas of others; it marks the entrance into a new life based upon a profounder view of the kind of being one is, of the class of beings one belongs to. Conversion is an affair of the social consciousness; by it a youth comes to feel that he belongs to a noble company and to a divine order of things; by it he enters at last upon real life in a real world.

It has been objected that this period for the greatest frequency of conversions comes too early and that conversions frequently occur far on in life. Certain it is that a large part of the work of the church has been directed in vain to the converting of adult men, if the period mentioned above is the only period of possible conversions. Now with reference to this objection, aside from the doubt which attaches to the conclusions of Starbuck and Coe as a result of the methodological difficulties mentioned above and the relatively small number of cases examined, I should say that the word conversion is here used in our two different senses. As used in these investigations it seems to me to mean the natural coming to himself which every normal man, be he Hindoo or Hebrew, Latin or Greek, at some time or other experiences. To those who object to the results of these investigations it means the accepting of Christ and Christian truth as the central religious facts of the universe. Just what view of life and what attitude toward the world a youth is converted to will depend upon the training and surrounding influences of his life; that is, it will depend upon imitation and suggestion. He may by conversion become a Hindoo, a Mohammedan, or a Christian; he may be converted, like Tolstoi, to a simple faith which he understands to be the universal essence of all great religions; he may be converted to some country and make patriotism his religion; he may be converted even to some calling so that henceforth he defines his relation to the world as that of a miner or a teacher of English Literature, or a preacher of the gospel of Christ. I believe there are men in whose lives patriotism or devotion to some calling have, for a time at least, nearly all the essentials of a religion. That an individual should undergo conversion is insured by the laws of mental growth and the conditions of social intercourse; but that he shall become a Christian by conversion must be insured by teaching, preaching and living Christianity. Hence the significance of the church, of family worship and of all those noble agencies through which Christian truth is taught and Christian attitudes are trained.

That Starbuck and Coe must be nearly right as to the time of conversion in this general sense of the term, I think there can be but little doubt. For consider what the adolescent period is. It seems necessary to keep saying that puberty is not the whole of it, nor even its most important part. At about the tenth year girls begin to grow more rapidly than they have ever grown since infancy. Something like a year later, boys start, outstripping the girls about the fifteenth year and ultimately attaining the larger stature of the two. They first shoot up like iron-weeds, then broaden out and then fill up. At about fourteen in girls and fifteen in boys the brain weighs more than at any other time in life. During this period new organs develop, new instincts and acquired reactions show themselves, new centers in the sympathetic and central nervous systems develop and begin functioning. Moreover, growth is never proportional throughout the body. One organ after another and one nerve-center after another starts into activity and then subsides; and with these spasmodic developments, the youth's interests flash up and die away. Now he will be a great poet and artist, now a great orator and statesman, now a great adventurer and desperado or a great naturalist or a great inventor. And all the time he is living under an enormous blood-pressure and the demands of his growing organs are draining the energies of his central nervous system. If a youth does not discover himself at this time of disappointment and growth and trial, at what time in life is he more likely to do so? As a matter of fact the mental characteristics of the period are just what we should expect — a deepening awareness of self such that all experiences, especially in girls, come to have an intensely, sometimes a morbidly personal reference; an insatiable craving for sympathy and comprehension from those who are older; an extravagant passion for self-sacrifice, and a certain fickleness and fancifulness of interests, ambitions and tendencies. That reflective self-awareness develops especially at this time, is shown by the fact that when this craving for sympathy and this impulsive self-sacrifice are not satisfied by wholesome family and social relations, certain morbid types of self-consciousness are apt to result. Such morbid types are manifest in the desires to enter monasteries and nunneries, to become trained nurses and heroes of tragedy, and to commit suicide rather than endure the awfulness of living. Here also belong the feelings of many youths and maidens that they are different from other people, that they are hopelessly bad and alien, that no one understands them, and that God (if there be a God) has somehow left them out of His great plan for His world. The more one studies the mental states of adolescent boys and girls and the more one comprehends their fierce doubts, their titanic yearnings and their tremendous burdens of anxiety and fear, the more one is convinced that this is the natural time for the great awakening. At this time, and particularly just after the period of greatest bodily growth when the energies of the central nervous system are no longer drained to supply the demands of developing organs, one should come to realize one's place in the experience of God and in the institutions of His world.

But the term conversion as used by some recent writers means that change in man's religion by which Christ comes to be its center. If the foregoing view is true, every normal human being should undergo conversion some time within or near the adolescent period; every mature man is in this sense of the word religious; but to become religious in this sense is obviously not the same thing as to become a Christian. Two classes present themselves, viz., those who in their early conversion become Christians and those who in their early conversion become religious without becoming Christians. In the latter class I do not see why conversion to Christianity may not occur at any time after the adolescent period. In this second sense of the term, conversion may occur many times in the course of a life; and it is the almost universal experience of ministers and teachers that conversion does occur at any age from the twelfth year until death.

It is in the belief that the term conversion is used in these two different senses indiscriminately by different writers of the recent scientific movement in the study of religious phenomena, that the two ought to be kept distinct especially in dealing with the question as to the periods of conversion, and that their confusion has resulted from the complexity of the problem, the limited number of cases examined, and the necessary inadequacies of the method used, that this article is written. To the writer it seems idle to condemn *in toto* all efforts to attain exact knowledge as to the psychology of religious experiences, and we see no reason why recent efforts in that direction should not be welcomed and encouraged. But it seems to be a field in which results must finally be reached by deduction from general psychological hypotheses which are yet to be inductively established in other departments of the science. Meanwhile such deductive procedure is aided by such facts as Leuba, Starbuck, Coe and James are seeking accurate, systematic and exhaustive accounts of.

As to the time of conversion, two separate lines of investigation suggest themselves as extensions of the work already so faithfully done in a field where, both from the point of view of the science of psychology, from that of the practical worker in education, and from that of the conscientious individual seeking an intelligent control of his judgments in religious matters, light is so sorely needed. First, an elaborate series of investigations carried out in different lands among persons of different religious beliefs for the purpose of comparing the religious experiences of people in different countries, climates, races and civilizations. Secondly, a series of investigations carried out by teachers and ministers of different persuasions in Christian countries for the purpose of determining the times, the conditions and the nature of conversions to Christianity, and to other types of religious conviction. The difficulties of such investigations, from a scientific point of view, are enormous and results can only be contributory to a future consummation devoutly to be wished. The American investigator would be obliged to work through missionaries and college teachers in remote lands, and this circumstance, in addition to the inherent difficulties of our methods, necessitate the utmost judicial care in sifting results.

G. A. Tawney.

Beloit College.

THE GENETIC PROGRESSION OF PSYCHIC OBJECTS.

The recent relatively novel attempts in the literature to approach the logical processes from the genetic point of view, have made it clear that a good deal of close psychology is still needed in this field. What has impressed the present writer is the lack of an actual tracing out of the series of determinations of objects at the successive stages of cognitive development, and the motives in each such progression from one 'psychic object' to the next. This is the topic to which I am here applying the title of this short paper. The term 'progression' is one which I have used in a somewhat technical sense elsewhere[1]; it denotes a real genetic movement from one mode or stage of development to another.

In a series of university lectures, which are to be revised for chapters in a forthcoming work on the genetic treatment of the logical operations, I have worked out a tentative scheme of the sort; and as its points of emphasis are not altogether those hitherto familiar, I venture to present it here for preliminary criticism; hoping as well that it may incite to a renewed discussion of the general topic.

[1] Psychological Review, May, 1903. I use the term 'object' in the sense of whatever consciousness means or intends — that is whatever can be in any way, shape, or manner psychically set up, presented, or aimed at. Cf. the writer's *Dict. of Philosophy*, sub verbo.

In a preliminary demarcation of the field, we may ask two broad questions: first, what are the conditions determining the construction of objects at any given stage of mental development; and second, what are the psychic characters of the objects thus determined at any stage. Of course, the treatment of 'any stage' means the treatment of 'every stage,' and that involves the determination of the entire continuous movement of the cognitive function, with the ranging of all the objective determinations or specifications of psychic objects in a genetic series.

In the process of bandying this question about — making it every sort of psychic object! — the following types of enquiry have come to more or less definite shape. If we take the traditional outstanding distinctions of sorts of objects, such as sense objects, objects of memory, of thought, etc., as starting point, we may work out the more evident characters of such objects, range them in their apparent genetic order, and call them, as so arranged, the series of 'objective modes.'[1] We may then endeavor to work out the factors of determination for these modes in succession from the simpler to the more complex, in so doing recognizing any finer distinctions which appear, and rearranging the genetic order as we may find ourselves led to do so. This compels us — or has done so in my own case — to trace out certain relatively independent strands of genetic change, the transformations which certain great phases of psychic process undergo, along with the changes in the objects proper. These accompanying series, in so far as they are essential aspects of what we may call the 'object psychosis,' are indeed necessary to a full statement of the objective progressions. I find it at least interesting therefore — not to make dogmatic statements as to its possible value in each case for the main problem itself — to distinguish in the actual results to which I have been led, the following phases of consciousness,[2] traced in each case along with the objects, through a series of modes in turn: (1) the controlling conditions of the determination (that is, the 'control' of the object, a problem recently made much of in the writings of Professor Dewey), (2) the motive to the

[1] The term mode may be applied, I think, both to the sort of function whose progressions we are tracing out, and also to each characteristic stage in these progressions themselves, *e. g.*, the thought mode is a stage in the development of the cognitive mode.

[2] This description of these series was drawn up in answer to a question raised by one of my students after the table (Table I., herewith) in which the results were spread out, was presented on the blackboard; I say this to avoid the suggestion that the lines of enquiry were worked out under any prearranged scheme. On the contrary, the different modal series, as they may be called, resulted directly from the attempt to analyze and trace out the objective determinations in order.

determinations each in turn (the problem of 'interest,' which I find of extreme importance in the later discussions as to the determination of 'truth,' as 'practical,' or 'theoretical,' or both), (3) the function involved in each determination (the sort of attention in which the actual interest finds its vehicle), (4) the meaning of the object, over and above its actual objective marks (here the question of 'logical meaning' is of course uppermost, and with it what I call the problem of 'individuation,' or range of application — in logic, 'quantity' — and the question also of 'real reference,' or the psychic meaning of 'reality').[1]

With so much statement of the problem and the way of approaching it, the accompanying Table I. is I think fairly intelligible. The remarks upon it which follow are meant to explain the more unfamiliar features of the results which it exhibits, including the definition of certain new terms which it has been found necessary to use.[2]

As to the successive objective determinations themselves (1 to 8 in col. i.), they are largely explained by what is found in other columns of the same level, in each case. The principal innovations in the series consist in the essential progression from Memory objects (2) to Judged or Logical objects (7) through the stages represented by Fancy (3, in which arises the dualism of 'inner-outer'), Play (4, characterized by the constructions of 'semblance' or 'make-believe' — col. iii. — and 'experimental control' — col. vii. —) with the important transition, through the rise of psychic control and quasi-logical classification (col. iii.) to the dualism of 'mind-body,' called the 'Substantive mode' (5). In fact, I am prepared to insist that what is here called the 'Semblant' mode is an absolutely necessary term in the rise of the great dualisms which make the logical consciousness possible.[3]

Another point to be remarked is that the dualism of 'Self—not-self' (6) is made germinal to Judgment (7), and that the former carries with it as also preliminary, and so available to say the least, the

[1] Of course many other questions might be asked about the objective consciousness, as *e. g.*, what its emotional coloring, its conative accompaniments, etc., but these might just as well be asked in the reverse form — in tracing out the progressions of feeling or conation. Here we are dealing with what is found to be necessary in (1) the *determination* and (2) the *characterization* of the object *qua* object.

[2] As to the new terms, they are of minor importance, of course, and need not claim to be 'fittest'; the suggestions of others would be most welcome on this as on other features of the matter.

[3] Of course this and the other essential features of the progressions, so far as in any degree novel, are argued in detail in the full treatment, which is to appear in a volume from the press of Messrs. Swan, Sonnenschein & Co., London.

'social-personal' distinction. This means, it would seem, that a strain of social worth appears in all determinations of judgment. Further, as to judgment, *it* is found to be the criterion of the Logical, properly speaking (col. ii.), although the progressions up to it, through the preliminary dualisms (3, 5, 6), illustrate strikingly the fact of continuity. The modes 3 to 6 are in a very real sense 'quasi-logical.'[1]

Note also that 'Moral objects' (8) are 'extra-logical,' except when made matter of theoretical interest (col. v.), and that 'Æsthetic objects' (9) are hyper-logical, in the sense of having both practical and theoretical Individuation (col. iii.), and also as involving a higher form of interest and control.

Indeed, still speaking of the æsthetic, I may add that another of the points most in need of clearing up, and hence earnestly worked at here, is the relation of the two forms of 'Semblant' objects, those of Play and Art (col. iv., 4 and 9), considered in respect to their psychic meanings, to the other forms of objective construction. It is my conviction that in both of these — and it is part of the fact that *any possible psychic object may be determined as one of them* — we have the genetic resolution of the dualisms and pluralisms of the various cognitive modes as such. So I find it necessary to use terms which lack the partial connotations of those employed for such modes. The æsthetic is 'hyper-logical'[2] (as explained above); it is 'contemplative,' or as regards its end, 'pan-telic,' having both practical and theoretical interest; and it has the further extraordinary character that it is under what I venture to describe as 'syn-nomic' control: that is, it is a form of determination in which both the psychic and also the psychically-recognized-as-foreign conditions of determination are satisfied. There is here a higher psychic immediacy in which all the dualisms of the mental life, at the stage reached, may on occasion merge in an immediate contemplative value of real presence; the dualisms of 'theoretical and practical,' 'mind and body,' 'inner and outer,' 'freedom and necessity,' all merge to the vanishing point in the æsthetic.

It only remains to be added, in the consideration of the objective progressions as such, that the dualism of Self-notself is described as one of 'Content,' inasmuch as it arises only when the dualism of Mind-body, gives place to that distinction within the psychic sphere in which

[1] The progressions in the development of the Logical mode itself are matter for later statement, as are also those of the Self-social mode (cf. this REVIEW, May, 1903, pp. 226 ff.).

[2] Meta-logical, suggested to me and otherwise apt, has been preëmpted for the sense given to it by Schopenhauer (*Fourfold Root*, § 33).

part of the content is set off as 'self' over against the rest of the objective content or 'not-self.'

The progressions of the 'Individuation' mode (iii.) are at once most difficult to trace and most important in relation to the questions of logical value (ii.) and real reference (iv.). The considerations involved are so detailed that the catchwords given in the column iii. must suffice in this place. Yet attention may be drawn to the position that the recognition of 'class' is the term of transition from the Play mode to the Substantive mode, and that the recognition of 'general meaning or intent' is preliminary to the 'general concept' which alone is logical. Psychically there is reason also for maintaining that all individuation is a function of recognition.

The Reality progressions (iv.) are in familiar terms; though I may remark that by 'practical judgment,' I mean the sort of practical use of means to ends recently described by Hobhouse (*Mind in Evolution*) as probably occurring sometimes among animals. Genetically it seems to be closely associated — as regards the psychic elements involved — with the experimental treatment of objects so conspicuous in the Play mode (see col. vii., 4).

In the Interest progressions (col. v.), one should note the overlapping of the practical and theoretical interests, and the arrangement of the *psychic* stages with reference to the ends of the interests, namely, as 'a-telic,' (*e. g.*, without psychic end), 'auto-telic,'[1] practical (?-telic),[2] theoretical (?-telic),[2] and 'pan-telic' (inclusive of all sorts of ends). It may also be noted that the rise of theoretical interest is put in the transition from the Substantive to the Content mode — the interest which motives the distinction between self and not-self being both practical and theoretical.[3]

The progressions in the Attention mode are sufficiently explained by the explanations of the genetic formula for the attention given in the chapter of an earlier work where that formula is proposed (*Ment.*

[1] Cf. the *Dict. of Philos.*, sub verbo.

[2] Suggestions of proper compounds in these two cases are in order; possibly 'pragma-telic' and 'noö-telic' would do. It is advisable to confine the term 'practical' to the objective psychological point of view, and to use 'pragmatelic' for that psychic; for pragmatelic interest is not at all coëxtensive with practical interest. To make the same distinction general, as between 'telic' (psychic) and 'teleological' function, would aid in banishing the utter confusion which prevails in the use of the latter term. The teleological is the *end-attaining*, to an observer; the 'telic' is the *end-seeking* in psychic process.

[3] Theoretical in the germinal sense of being experimental — a necessary phase of theoretical interest, as I believe.

Devel., chap. X., § 3).[1] The arrows are explained by the terms theoretical and practical with which they are associated in col. v.

The Control modes are necessarily described as 'mixed' in respect to the contrast of psychic and psychological (or objective), inasmuch as the only possible variations in the description of the sorts of control are those characteristic of the two contrasted points of view. For instance, control is 'heteronomic' to the psychic, when it is described as external (*e. g.*, biological, organic, etc.); it is 'a-nomic' when it is or appears to be lawless from both points of view. Later on in the progressions we have terms in use for both sorts of control: 'belief' over against 'truth' (and 'fact'), 'duty' over against 'right,' 'æsthetic quality' over against 'beauty' (this last being a joint sort of control covered by the term 'syn-nomic'). A further point of interest to the writer is that suggested by the double brackets of different lengths between 5 and 7 (col. vii.); namely, the point that the theoretical form of control ('fact' and 'truth') extends from 5 to 7 — over a certain range of objects — while there are also other control-forms extending not only over the same range, but beyond it in both directions.

It may well be that such a schematic presentation as this has no value or suggestiveness; and I should not be surprised to hear this opinion expressed. But the tentative character of the results, and the absence of the detailed grounds which are to my mind reasonably strong, may be just the needed stimulus to some one to treat the topic more fruitfully.

J. MARK BALDWIN.

JOHNS HOPKINS UNIVERSITY.

NOTES.

I TRUST that the paper of Professor Max Meyer, in the REVIEW for March, on 'Attributes of Sensation,' may stimulate reflection, if not discussion, on that subject. Personally, I prefer the term 'element' for what Dr. Meyer calls 'attribute,' but that is mainly a verbal difference between us. The positive contribution of the paper seems to me to be the teaching that the existence of a psychologically simple 'tone-quality,' which varies with the pitch and yet is introspectively distinct from it, discredits independent variability as a principle of distinguishing 'attributes' of sensation.

[1] The genetic formula $Attention = A + a + \alpha$, in which A stands for the gross muscular and other sensational processes of attention, a the added contractions, etc., of recognizing a class (*e. g.*, visual objects), and α the finer adjustments of individual recognition.

The paper is marred by the uncritical adoption of duration as attribute of sensation in the sense in which qualities, intensities, and extensities are said to be attributes. But duration as content of consciousness is a complex, not an elemental, experience; and duration viewed as attribute is predicated of all events, physical as well as psychical, and is therefore not an attribute at all, in the psychological sense. Even a psychologist who does not admit the preceding statements, ought at least to recognize that the traditional treatment of duration has been challenged.[1]

MARY WHITON CALKINS.

WELLESLEY COLLEGE.

On account of the accumulation of material the REVIEW will issue a double number (July–September) on July 1. The PSYCHOLOGICAL BULLETIN of June 15 will also be a double number (June–July) devoted to Mental Pathology, of which Dr. A. Meyer, of the New York Pathological Institute, will be the 'effective editor.' It may be added also that no less than four Monograph Supplements are in our hands for immediate publication. The annual bibliography, THE PSYCHOLOGICAL INDEX, is to appear in a few days. It shows a marked falling off from the last year in the number of titles listed.

THE EDITORS.

[1]Cf. on the duration problem, a paper by Professor M. F. Washburn, this REVIEW, July, 1903, and a shorter discussion by the writer, *ibid.*, vol. VI., 1899, p. 506.

N. S. VOL. XI. No. 4–5 July–September, 1904

THE PSYCHOLOGICAL REVIEW.

AN EXPERIMENTAL STUDY OF THE PHYSIOLOGICAL ACCOMPANIMENTS OF FEELING.

BY L. PEARL BOGGS, PH.D.,
Urbana, Ill.

It had practically been taken for granted since the time of Kant that all feelings might be classified as pleasantness or unpleasantness until Wundt propounded his theory of the three directions of feeling. He names them pleasantness — unpleasantness, excitement — repose, and strain — relaxation. This psychologist believed that he had not only the evidence of his own introspection and that of others in favor of this view, but also the results of certain psycho-physiological experiments which showed that each of the six sorts of feeling had as its accompaniment certain changes in the vaso-motor system. These experiments were chiefly those of Mentz [1] and Lehman,[2] the former having made use of the sphygmograph and the latter the plethysmograph for recording the radial artery.

Not long since Dr. Max Brahn published an article [3] setting forth the conclusions reached from a long series of experiments made for the purpose of testing the tridimensional theory of feeling, and he finds six forms of pulse changes, each one of which is the persistent accompaniment of some one of the six kinds of feeling. It was also with the purpose of a test of this theory that the following experiments were undertaken and I find that in the main the results are a corroboration of those of Dr. Brahn.

[1] 'Die Wirkung akustischer Sinnesreize auf Puls u. Athmung,' *Phil. St.*, Vol. XI.

[2] *Hauptgesetze d. Gefühlsleben*, Leipzig, 1892.

[3] Brahn, 'Ex. Beiträge zur Gefühlslehre,' *Phil. St.*, Vol. XVIII.

It were perhaps useless to go into a lengthy account of the tridimensional theory. It is to be found in Wundt's[1] later psychological works and Brahn has given a short resume.[2] It is very probable that the differences in the one-dimensional and tridimensional theories arise from inaccurate introspection which does not clearly distinguish between sensation and feeling; from loose concepts as to the meaning of feeling; and from vague theories as to the origin of feelings and their physiological accompaniments. Wundt calls feeling the subjective element of psychical life and says that in seeking for the physiological accompaniments 'it follows from the subjective nature of the feelings, that we should not expect to find them among the processes produced in the organism directly by external agents, as the sensations are, but rather in *reactions* which arise indirectly from these first processes.'[3] Again Wundt does not allow that there is such a thing as a concrete state called pleasantness or excitement, but believes that these are only general names under which a multitude of feelings are brought which have a certain general resemblance. But putting theoretical questions aside we shall pass on to the account of the experiments.

Instruments and Methods of Procedure.

Preliminary experiments for this study were carried on at the University of Cornell during the fall of 1902 but nearly all of the results which are embodied in this report were obtained from experiments made at the University of Illinois in the spring semester and the summer of 1903. I used a Marey's sphygmograph for part of the experiments and for others a smaller and simpler one, which was very useful when the pulse lay very near the surface of the arm, as it did with some subjects. The record was always taken from the radial artery, usually the right one, with the arm in a supine position and resting comfortably on a high cushioned stool. For registering the pulse changes air transmission by means of a thick walled rubber tubing and Marey's tambours was used, the writing lever being laid lightly

[1] Especially *Outlines of Psych.*, 1897, p. 82, ff., *Phil. St.*, Vol. XV.

[2] *Op. cit.*, pp. 127–132. For a criticism see Titchener, *Zeit. f. Psy. und Phys.*, Vol. XIX.

[3] Wundt, *Outlines of Psych.*, p. 86.

on the smoked paper of a regularly revolving drum. The kymograph was rendered as noiseless as possible by placing a heavy felt mat under it. An electro-magnetic time marker connected with a second pendulum recorded the time while another electro-magnetic marker was used for recording the beginning and the close of the experiment, interruptions, or special signals by means of a double contact key which was inserted in the circuit and was sometimes in the hand of the operator or an assistant, and sometimes in that of the subject, as the circumstances of the experiment required. Later on I also used the pneumograph, but more by way of control for the pulse records. In nearly all cases the thoracial breathing was taken, and while the instrument was not very sensitive it seemed to be accurate within its limits. The breathing curve was registered in the same way as the pulse.

Much has been said for and against the sphygmograph as an instrument adapted to precise and accurate work. The criticism most often made is that it gives only the frequency of the heart pulsations since the plethysmographic effects are so great that the strength of the pulse cannot be accurately measured; that is to say, the volume of the arm changing, the pressure of the spring adjusted to the throbbing artery is changed, and therefore the excursion of the same is changed. In reply to this objection Brahn has spoken at some length,[1] citing von Frey as authority.[2] The conclusion is that such an error is present only where the change in volume is very great, for example, where the arm is tightly bound or in Valsalva's experiment, but that in ordinary experiments, especially of short duration, the error is neglible. Several curves were taken simultaneously with the plethysmograph on the left and the sphymograph on the right arm during the course of my experiments, and after careful scrutinizing of the two records I can find no traces of such an error. However, I do find the sphygmographic curve much plainer and more regular with more pronounced and uniform characteristics. Of course the sphymograph requires the most carefnl adjustment to begin with and I found several per-

[1] Brahn, *Phil. St.*, Vol. XVIII., pp. 143–144.

[2] Von Frey, *Die Untersuchung des Pulses*, Berlin, 1892.

sons who were unsuitable as subjects on account of the pulse lying too near the surface of the arm, or too deep, and each individual required a different amount of pressure to insure the most accurate results.

The experiments were conducted in a quiet room between nine and eleven o'clock in the morning and two and four in the afternoon, the subject sitting usually three quarters of an hour. My subjects were nearly always in a fresh condition but for the sake of observing the physiological effects and the variation in the results when one was tired or slightly indisposed, the subjects were occasionally taken in a slightly abnormal condition. One subject slept for twenty minutes during which time I took a number of records. Another subject who had just been smoking before he came in showed such a weak and irregular pulse that nothing much was to be gotten out of it for psychological purposes.

The introspections were given conscientiously and carefully and whenever there was reason to suspect anything wrong the whole experiment was thrown out. I did not especially train my subjects in introspection as Brahn did, but occasionally asked them to compare or contrast certain feelings. I sometimes cautioned them about distinguishing clearly between feelings and sensations. Occasionally an assistant was present and usually two persons were required to conduct the experiment if I was the subject. For this service so kindly performed I wish to thank Mrs. Alice Parks and Professor Colvin, as well as for their participation as subjects. My other subjects were unpracticed observers, or comparatively so, and were kept totally ignorant of the real purpose of the experiment in order that their introspection should not be influenced by any desire to make the proper introspection.

Measurement.

The measuring of the curves is a matter of great importance. It did not seem to me that the highest accuracy was to be obtained by measuring each pulse length as Brahn does. In the first place the variations are so small that the mechanical error in measuring may be considerable; and secondly, the variations of single pulse curves within a respiratory oscillation are so great that errors are likely to arise if due notice is not taken of this fact. Mentz adopted the plan of taking the mean averages of the shortest and longest curves of each oscillation. I modified this plan, simply measuring each oscillation and taking the mean average of the whole number of pulses, beginning with whichever pulse occurred first in the reaction as the starting point for all measurements for that particular instance. Lehman's method of following the natural groupings according to pulse lengths seems most unscientific, while some one has hit upon the ingenious plan of measuring the first half of the reaction and the second half separately and comparing the mean averages of the curves. The instrument used in measuring was a vernier recording tenths of millimeters.

Different rates of rapidity were used for the revolving drum but for paper 480 mm. long 93 and 80 seconds time was most frequently used. The measurement of the breathing I shall only give in words as also that of the height of the pulse.

No attempt will be made to reproduce the curves since it is impracticable to reproduce all and a few isolated curves would have little value since each curve should be read in comparison with the normal curves and other curves of the subject.

A Brief Account of the Results of Other Investigators.

As to the results of the experimental work previously carried on in this field a few words should be said, and first in regard to the parallel physical processes of attention.

Involuntary attention: Mentz[1] finds that acoustical stimuli received with involuntary attention caused a slowing of the pulse and usually of breathing. Lehman[2] says that an intense and sudden sensation causing attention usually does not affect the breathing; the first pulses are usually short but afterwards become longer and so the general characteristic of the curve is a lengthened pulse. Mosso's[3] statement that the sensations from

[1] *Op. cit.*

[2] *Körperliche Äusserungen psychischer Zustände*, Leipzig, 1899.

[3] *Diagnostik des Pulses*, Leipzig, 1879. *Die Temperatur des Gehirns*, Leipzig, 1893.

the various sense organs cause a slowing of the pulse while the breathing changes show no satisfactorily uniform result finds support in the two later experiments.

Zoneff and Meuman [1] find that there is no difference between involuntary and voluntary attention but say that a concentration of the attention causes a slowing of the pulse and an obstruction in the breathing. In respect to sense and intellectual attention the breathing is more obstructed in the former.

Voluntary attention, according to Mentz, is accompanied by a quickened pulse and usually more rapid breathing. Lehman's results shows a quickened pulse. If it is a case of long-continued intellectual work the breathing is usually more rapid and shallow.

Brahn discusses strain as the feeling side of attention, and prefers the expressions prepared and unprepared attention to that of voluntary and involuntary. Prepared attention gives the best form of strain while unprepared attention is likely to be ushered in by a feeling of excitement, which may be changed to that of strain only to be replaced again by excitement. Naturally some stimuli to which we attend do not cause a strain but pleasantness, unpleasantness or quietness according to their nature. This state of strain is accompanied by a quickened pulse and pronounced or low dicrotic while the opposite feeling of relaxation is accompanied by a slowed pulse and high dicrotic. In regard to the fluctuations of attention Neuman and Zoneff say that according to the degree of concentration of attention the breathing and pulse is obstructed, whereas so soon as the concentration is diminished it becomes faster. This statement is to be criticised later on.

As regards pleasantness and unpleasantness there is practically universal agreement. Mentz finds in the case of the former a slowing of both pulse and breathing and for the latter the reverse. While agreeing with this, Lehman says in addition that pleasantness is accompanied by a heightened pulse curve and unpleasantness by a lower.

Zoneff and Meuman find pleasantness accompanied by a less rapid pulse, but with more rapid and shallow breathing, while the reverse is true for unpleasantness.

[1] *Phil. St.*, Vol. XVIII.

Brahn agrees with Lehman is regard to pleasantness and unpleasantness. Excitement is accompanied by a heightening of the pulse without the lengthening, while repose is accompanied by a decrease in the height without change in length. Brahn follows Wundt's suggestions about the choice of stimuli, those for the different sense organs being accompanied by different feelings, such as taste stimuli by pleasantness and unpleasantness, color by excitement or repose. Also high and low tones were likely to produce one of the last named pair. This author believes that in the light of his knowledge of the feelings which certain stimuli arouse and their physiological accompaniment he is able to find many traces of feeling in the works of other investigators.

He finds that Féré[1] asserts that all feelings of depression show a diminution in the volume of the extremities while all feelings of excitement and pleasure show a volume increase. Strain and relaxation are also mentioned. In regard to Mentz,[2] he says: 'We need only to follow the meaning of sthenic and asthenic and of voluntary and involuntary attention without prejudice and take the matter in an elementary way in order to find the three directions of feeling.' Also in Dumas',[3] Binet-Courtier's,[4] and Binet-Vaschide's[5] works he finds hints of the tridimensional theory, and in Lehman's[6] latest work he believes that if the author had analyzed more thoroughly and adopted the tri-dimensional theory many of the results would coincide with his own. I shall speak of these works later in comparison with my own results.

[1] Féré, *Sensation et mouvement*, Paris, 1887.

[2] Mentz, *op. cit.*

[3] 'Recherches experimentales sur la joie et la tristesse,' *Revue philos.*, 1896.

[4] 'La circulation capillaire dans ses rapports avec respiration et les phenomenes psychiques,' *L'Année psychol.*, II.

'Influence de la vie emotionelle sur le coeur, la respiration et la circulation capillaire,' *L'Année psychol.*, III.

[5] 'Influence du travail intellectuel, des emotions, du travail physique sur la pression du sang,' *L'Année psychol.*, III.

[6] *Op. cit.*

Strain and Relaxation.

Strain and relaxation are the feeling side of attention. The feeling seems to be strongest when the subject is attending to stimuli about 8 seconds apart, the intervening time between the signals being free from any sensations or ideas, and all consciousness being intent on the recurring signal. However attention is sometimes accompanied by what the subject frequently designates as concentration. Here the feelings of strain and relaxation are not so pronounced, although my own introspection and that of others find them present. This is the case where one attends to a continuous succession of stimuli or ideas without an appreciable interval between. It is interesting to note that the physiological accompaniment of the two are somewhat similar. We find voluntary attention is accompanied by the purest forms of strain and relaxation, involuntary attention being often accompanied by excitement. In the following experiments we are to think of attention as being the former.

Experiment I.—P. P. 7/16/'03. This subject has on this occasion a marked respiratory rythm in the sphygmographic curve of about 11 pulses and his breathing was unusually slow and deep during a state of rest. The stimulus was the clicks of a double contact key which by means of the magnetic time marker were recorded on the smoked paper. The breathing at once became more shallow and about twice as rapid while the respiratory rhythm is much less pronounced, occurring every four or five pulses. The pulse is somewhat lower with a lower dicrotic during the period of strain while the pulse is higher with a higher dicrotic during the period of relaxation. The pulse rate is more rapid during strain than relaxation.

	Normal.	Strain.	Relaxation.	Strain.	Relaxation.	Strain.
No. of pulses,	4–5	4–4	4–4	4–4	4–5	5–5
Av. length,	5.2 5.1	5.1 4.8	5.1 5.1	4.8 4.8	5.5 5.1	4.8 4.9

At this point there seems to be a change. We have at relaxation 4.9 and 4.6 followed by 5.3, 5.1, 5.

Experiment 2.—P. B. 8/5/'03. Stimulus was clicks as before 5 seconds apart. The breathing was shallow and rapid while the respiratory oscillation almost entirely disappeared. Dicrotic changes as before.

	Strain.	Relaxation.	Strain.	Relaxation.	Strain.
No. of Pulses,	3–4	5	6	5	6
Av. length,	5.5 5.1	5.3	4.9	5.3	5

Here as in the former experiment a change is apparent. We find next 4.9 4.9 4.6 4.9, which according to the subjects' introspection is a sort of shifting or adjusting of the attention.

Experiment 3. — P. P. 7/14. The stimulus was again clicks about five seconds apart and the subject felt strain and relaxation.

	Strain.	Relaxation.	Strain.	Relaxation.
No. of pulses,	6	6–2	4–4	4–4
Av. length,	4.9	5.3 5.5	5.1 4.9	5 5.1

The dicrotic changes are as before.

Experiment 4. — A. P. 7/20. Stimulus is clicks eight seconds apart. The dicrotic changes are the same, low in strain, high in relaxation.

	Normal.	Strain.	Relaxation.	Strain.	Relax.	Strain.	Relax.
No. of pulses,	5–5	5–5	5–5	5–5	5–5	5–5	5–5
Av. length,	4.7 4.7	4.7 4.7	4.9 4.8	4.8 4.9	4.8 4.8	4.7 4.7	4.8 4.9

This subject shows very little change in pulse length in any reaction.

Experiment 5. — A. P. 7/16. Stimulus was clicks five seconds apart, which came too quickly for this subject, so that she was somewhat excited all the time.

Experiment 6, following a few minutes after with clicks ten seconds apart caused a feeling of depression although the strain and relaxation were also felt.

Experiment 7. — A few minutes after this clicks eight seconds apart seemed to give pure feeling of strain and relaxation, that is, the taps came just when expected, neither too soon nor too late.

	Normal.	Strain.	Relaxation.	Strain.	Relax.	Strain.	Relax.
Experiment 5.							
No. of pulses,	5–5	7	6	7	6	7	6
Av. length,	4.5 4.5	4.5	4.3	4.5	4.5	4.5	4.5
Experiment 6.							
No. of pulses,	5–5	6–6	6–7	6–7	6–7	6–7	6–7
Av. length,	4.5 4.5	4.5 4.5	4.5 4.4	4.4 4.4	4.4 4.4	4.3 4.3	4.3 4.3
Experiment 7.							
No. of pulses,	5–5	5–5	5–5	5–5	5–5	5–5	
Av. length,	4.3 4.3	4.4 4.6	4.4 4.4	4.4 4.6	4.4 4.4	4.4 4.3	

In all three of these the curve shows the dicrotic changes, the lack of variation in length is no doubt due to excitement in the first curve and depression in the second. The shortened pulse during the first period of relaxation of first curve is due to unpleasantness according to the subject's introspection. In the second curve we find the greater frequency of the pulse which so often accompanies prolonged attention.

In the third curve there is already present a low dicrotic and during the first period there is a stronger pulse with now a high, now a low dicrotic. The next shows a uniformly high dicrotic, while the last shows a lower dicrotic. The whole curve is very low and becomes almost threadlike, showing, I believe, the effects of fatigue.

Experiment 8. — P. B. 7/5/'03. Stimulus: clicks eight seconds apart.

	Normal.	Strain.	Relaxation.	Strain.	Relaxation.	Strain.
No. of pulses,	5	5–5	4–5	5–5	5–5	5–5
Av. length,	5	4.4 4.8	5 4.8	5 4.9	5 5.1	5 4.9

The breathing is rapid and shallow and the dicrotic during the first period of strain is low, increasing gradually in height until the middle of the next period, but for the remainder the dicrotic changes are not so marked. My introspection here was that the taps were too weak and far apart and so I became confused without any feeling of strain and relaxation.

In a word, we find in agreement with Brahn that strain is a feeling whose physiological accompaniment is a quickened pulse with a lower dicrotic while its opposite feeling, relaxation, is accompanied by a slower pulse with higher dicrotic. In regard to the breathing, of which Brahn does not take account, it is more rapid, regular, and shallow than in a state of rest. The respiratory rhythm disappears to a very large extent. This feeling, like every other feeling cannot exist in strength for any great length of time but gives way to a sort of confused state caused by the shifting of attention (Ex. 1, 2, 8). The physiological processes also lose their pronounced characteristics during this period. I find a difference in subjects in regard to the time of the appearance of strain and relaxation as it depends on the quickness of the subject in responding to stimuli.

There are a number of experiments which were undertaken in order to ascertain what effect the state of attention during psychical activity had upon the vaso-motor system. My own introspection and that of my subjects give a feeling of concentration as the accompaniment of attentive, psychic action, which is largely the feeling of strain, flash-like period of relaxation preventing its becoming too strong. Between the curves of attention with the feeling of concentration and strain and relaxation there is a great resemblance.

Experiments 9.—S. C. 5/12/'03. The subject looked at a figure of tumbling blocks. Each time the figure took a different form the subject pressed the electric contact key and recorded the fluctuation on the drum. The breathing was shallow, rapid and regular; the apex of the breathing curve usually coincided with the time of fluctuation. Sometimes the fluctuation did not occur at every breath but at the second breath. Normal average length of pulse is 5 mm.

Fluctuations,	1	2	3	4–5	6	7	8–9	10	11
No. of pulses,	5	5	5	5	6	6	6	4	7
Av. length,	4.3	4.4	4.1	4.5	4.2	4.3	4.6	4.6	4.5

Fluctuations,	12	13	14–15	16	17–18	19	20–21	22
No. of pulses,	7	5	5	5	5	5	5	5
Av. length,	4.5	4.1	4.2	4.3	4.3	4.4	4	4.7

The dicrotic changes correspond roughly to changes in length which occur with the fluctuations. It is lower on the whole than in the normal.

Experiment 10.—S. C. The subject looked at a very simple figure this time and the fluctuations were more regular, corresponding as before to the breathing apex. The concentration was not so great and the breathing is correspondingly slower, especially at first. The dicrotic is high during the first twenty pulses, after which it becomes lower and fluctuates with variations in length of the pulse curves.

Fluctuations,	1–2	3–4	5–6	7–8	9–10	11–12
No. of pulses,	4	4	4	4	4	4
Av. length,	4.8	5	4.8	4.7	4.7	4.6

Fluctuations,	13–14	15–16	17–18	19–20	21–22
No. of pulses,	4	6	8	7	7
Av. length,	4.6	4.8	4.6	4.8	4.6

Fluctuations,	23–24	25–26	27–28	29–30	31–32
No. of pulses,	8	8	6	7	8
Av. length,	4.5	4.4	4.4	4.3	4.4

The average length of these pulses is greater than in the preceding.

Experiment 11.—E. C. 5/22. The subject attended this time to the counting of a clock's ticking. He said he was obliged to concentrate his mind on it. The breathing is shallow and rapid. The respiratory rhythm tends to disappear although it is usually quite pronounced and is moreover a three pulse rythm. His normal pulse is regular as to height but irregular as to length, its average perhaps being 6.6.

No. of pulses, 3 throughout.

Av. length,	6.5	6.3	6.3	6.5	6.1	6	5.9	6.3	6	6.3

The dicrotic is lower excepting at 6.5, 6.1, with a slight rise at the last 6.3.

Experiment 12.—E. C. 5/22. After a short pause the subject listens to a music-box medley to which he attends without much effort and some pleasure. The breathing is deeper and slower and the rhythm in pulse more pronounced.

No. of pulses, 3 throughout

Av. length, $\underbrace{5 \quad 5 \quad 5 \quad 6.3}_{\text{Before.}}$ $\underbrace{6 \quad 6.8 \quad 6.8 \quad 6.4 \quad 6 \quad 6.6 \quad 6,}_{\text{Stimulus.}}$

During the four periods before the stimulus and the first after, the dicrotic is low. After that it is higher but fluctuates somewhat. The average length of pulse is greater than during the preceding experiment. The subject appeared to be on a strain before the stimulus began.

Experiment 13.—A. P. 5/21/'03. The subject is reading an interesting essay. Breathing characterized by long pause after expiration, fairly regular, and of medium depth.

No. of pulses, 5 throughout.

Av. length, $\underbrace{4.5 \quad 4.3 \quad 4.3 \quad 4.2}_{\text{Before.}}$ $\underbrace{4.2 \quad 4.1 \quad 4.2 \quad 4.3 \quad 4.3 \quad 4.2}_{\text{Reading.}}$

$\underbrace{4.4 \quad 4.1 \quad 4.4 \quad 4.3 \quad 4.1 \quad 4.1}_{\text{Reading continued.}}$

The dicrotic is lower generally but shows variations corresponding to the length of the pulses.

Experiment 14. — A few minutes afterwards another tracing is taken, the subject having read on without interruption. The breathing is more rapid and shallow. In both the respiratory rhythm almost disappears. The dicrotic changes are as before.

No. of pulses, 5 throughout.

Av. length,	4.3	4.3	4.2	4.2	4.4	4.3	4.4	4.2	4.3	4.2
Av. length,	4.5	4.4	4.8	4.6	4.6					

At this point a violet odor was given to the subject. It was pleasant but subject could not concentrate her mind on her reading again. The pulse shows a lengthening.

Experiment 15. — S. C. 5/12. Subject is reading an interesting selection.

No. of pulses, 5 throughout.

Av. length,	5	4.8	4.8	5.2	5

The dicrotic is rather high throughout, a little higher at 5.2 than elsewhere.

A little later another tracing is taken while the reading is still going on. The pulse is more rapid as is also the breathing while the dicrotic is lower.

No. of pulses, 5 throughout.

Av. length,	4.3	4.4	5	5	4.8	5	4.7	4.7	5

Several times I have mentioned the disappearance of the respiratory rhythm in the pulse during attention. This was very marked with two subjects whose respiratory rhythm was usually so pronounced that the tracings were once discarded so far as the question of emotional accompaniment was concerned. However with these subjects any sort of rhythmic action, bodily or psychical, tended to destroy the respiratory rhythm in a remarkable degree.

Experiment 16. — C. W. 4/24. The subject was a very athletic young woman, generally not responding emotionally to stimuli. A slow in and out movement of the free arm caused a greater rapidity of pulse with a little increase in height, the rhythm almost disappears, while the dicrotic is high in the outward and low in the inward movement curve.

	Normal.	Out.	In.	Out.	In.	Out.	In.
No. of pulses,	11	12	11	13	14	10	18
Av. length,	4.3	3.6	3.5	3.1	3.5	3.3	3.5

Another experiment with the same subject in which she opened and closed the hand resulted in practically the same tracing.

Experiment 17.—E. H. Another subject showed still greater variation of pulse within an oscillation, sometimes the highest and longest pulse being almost twice the dimensions of the smallest. The arm movement caused the rhythm to disappear to a great extent as did a ticking metronome which 'bothered' the subject. During a Beethoven selection on an organ which the subject pronounced 'sleepy,' 'monotonous,' the breathing is rapid and shallow with great regularity of the pulse.

	Before.			Music.			After.
No. of pulses,	10–10	10	10	10	10	10	10
Av. length,	4 3.8	3.6	3.5	3.6	3.5	3.5	4.4

The dicrotic was somewhat lower during reaction.

The most marked and regular respiratory rhythm was observed in the tracing of a subject who peacefully slept for twenty minutes while records were being taken. It was impossible to arouse the subject sufficiently to take part in the experiment with attention and in the tracings taken during a waking condition the rhythm persists strongly.

The results show a striking resemblance to the results of the previous series of experiments. Rapid, regular, shallow breathing, a more rapid pulse on the whole and dicrotic changes in the pulse corresponding to variations in length. Also the rhythm parallel with respiratory movements tends to disappear.

The results agree with those of most other experimenters as regards the greater frequency during attention, Binet and Courtier[1] having also called attention to the fact that the dicrotic is later and lower during psychical activity.

Meuman and Zoneff, however, find a slowing of the pulse during both sense and intellectual attention. I should be inclined to attribute this result to the choice of stimuli used. Nearly all require precise muscular accommodation of the sense organ. For example, a card containing five points was brought near until the subject was able to count them; another time a ticking watch is brought within hearing; again it is to determine the threshold for the sense of touch according to Weber. All

[1] 'Circulation Capillaire,' *L'Année psychol.*, Vol. III.

of these require or at least call forth an involuntary holding of the breath in order to further the sense accommodation.

The same is true in the case of the fluctuations of attention. I found myself 'holding my breath' when I tried to keep a wire moving along a certain line. A criticism might also be made upon the choice of stimuli in intellectual attention. Problems in mental arithmetic are likely to arouse excitement, confusion, pleasantness, or unpleasantness, according to the ease and success with which they are solved. Then again their conclusions do not seem to be justified by the results. In three out of ten experiments the pulse is more rapid. In five the pulse is more rapid during the first ten seconds and in another equal. In one experiment the rate is only given for the whole reaction so that there are only three cases where the pulse is first slower and in one of these the second ten minutes show a greater frequency. On the whole their results are not so much at variance with those of others as it seemed at first glance that they might be.

Pleasantness and Unpleasantness.

In regard to pleasantness and unpleasantness, little new has been found regarding their physiological changes in the vasomotor system. Pleasure is almost always accompanied by a slower pulse and unpleasantness by a quicker pulse, the height usually increasing in the former and decreasing in the latter. The following are a few examples:

Experiment 18.—A. P. 4/9/'03. Subject was amused at a high organ note.

No. of pulses,	5	5	5	curve slightly.
Av. length,	3.8	4.4	4	higher at 4.4.

Experiment 19.—S. C. 5/4/'03. White rose odor was pleasant.

No. of pulses,	5	5	5
Av. length,	4.5	5	4.8

Experiment 20.—L. B. 5/12. Clove oil was pleasant.

No. of pulses,	5	5	5	5	5
Av. length,	4.2	4.2	4.4	4.4	4.2.

Experiment 21. — S. C. 5/12/'03. A purple red colored glass held before the subject in front of a non-transparent window was pleasing.

No. of pulses,	5	5	5
Av. length,	4.8	5.3	5
		Higher.	

Experiment 22. — A. P. 5/7/'03. Turpentine odor called up a number of pleasant associations.

No. of pulses,	5	5	5	5	5
Av. length,	3.5	3.8	3.8	3.6	5.5

A very interesting fact about this subject is, that out of a large number of tracings taken when the subject gave the introspection of pleasant after an odor the pulse was seldom slower. By chance I discovered that the subject had a habitual dislike of all kinds of odors, which was stronger than the momentary pleasantness. This may account for the regular pulse variation being absent during these reactions. The feeling of unpleasantness is one which is comparatively easy to bring about but I shall give only a few of the reactions.

Experiment 23. — S. C. 4/20/'03. The stimulus was a disagreeable odor.

				Reaction.			
No. of pulses,	6	6	6	6	6	6	6
Av. length,	3.5	3.5	3.2	3.1	3.1	3.2	3.6
				Lower.			

Experiment 24. — S. C. 5/4/'03. The stimulus was a blue glass plate twice exposed but the subject was unable to focus it and hence had an unpleasant feeling.

				Reaction.				
No. of pulses,	6	6	6	6	6	6	6	6
Av. length,	4.6	4.7	4.5	4.2	4.2	4.5	4.2	4.2.

Occasionally a very disagreeable feeling caused a slowing of the pulse.

Experiment 25. — S. C. 4/13/'03. The stimulus was a painful pressure.

		Reaction.			
No. of pulses,	6	4	6	6	6
Av. length,	4.8	5.2	5.4	4	4.3
		Higher.			

Experiment 26. — A. P. 5/28/'03. The stimulus wa as few drops of lemon juice which was very disagreeable — 'terrible.'

No. of pulses,	5	5	5
Av. length,	4.9	4.4	4
		Lower.	

Experiment 27. — On another occasion the stimulus being again lemon juice the reaction was much the same.

No. of pulses,	5	5	5	5	5	5	5
Av. length,	4.5	4.5	3.9	3.6	3.8	4.2	3.7
			Lower.			Higher.	

In cases where the feeling of pleasantness or unpleasantness is mingled with other feelings the pulse changes show variations from the above as we shall see later.

Excitement and Repose.

Whether one is to consider excitement and repose as feeling is a matter of most accurate definition and careful introspection. But taking feeling in the sense of a reaction to sensations and ideas, we can perhaps call that consciousness of increased muscular or psychical power with a tendency to action a feeling to be designated by the terms excitement, or liveliness. The consciousness of weakened or inhibited psychical or physical force we may designate as a feeling of repose or depression. Either of these is less often found alone than other feelings and it is especially difficult to bring them about in the laboratory, Brahn having succeeded in finding five cases of pure excitement in two hundred curves. The stimuli best adapted for causing the feelings are bright-colored transparent plates, high or low tones, and the ticking of a metronome. Also certain odors bring about this feeling but it is more likely to be mixed with some other feeling. As I have said before, I measured the height of the curves only in terms of higher and lower. The following are examples of excitement.

Experiment 28. — L. B. 5/5. High chord of organ. Excitement.

No. of pulses,	5	5	5	5	5
Av. length,	4.6	4.6	4.6	4.4	4.6

Experiment 29. — L. B. 5/12. The stimulus was rasping files which first excited and then amused me.

No. of pulses,	5	5	5	5	5
Av. length,	4.4	4.4	4.4	4.5	4.2

Experiment 30. — L. B. 5/12. A shrill Galton whistle set my nerves on edge but I was obliged to smile to relieve the tension. I was on a strain in anticipation of what I suspected was coming.

No. of pulses,	5	5	5	5	5	5
Av. length,	3.7	3.8	4.2	4.3	4.2	4.2
			Higher.			

Experiment 31. — L. B. 5/18. Another day a high whistle excited, 'keyed me up.' I afterwards felt a thrill of pain behind the ear.

No. of pulses,	5	5	5	5	5
Av. length,	4.7	4.7	4.7	4.5	4.8
		Higher.			

Experiment 32. — L. B. 5/18. Ammonia caused a strong state of excitement.

No. of pulses,	5	5	5	5	5
Av. length,	4.7	4.8	4.7	4.7	4.7

Experiment 33. — L. B. 5/26. A bright yellow glass plate held between my eyes and the window caused a slight feeling of cheerfulness.

No. of pulses,	5	5	5	5	5
Av. length,	4.8	4.9	4.9	4.8	4.9

Experiment 34. — A. P. 4/16. A discord on the organ proved exciting.

No. of pulses,	5	5	5	5
Av. length,	3.9	3.9	3.9	3.9

Experiment 35. — A. P. 4/16. A red plate of glass caused a strong excitement; subject felt she must jump up from the chair; cold chills run up and down her spine. There was no doubt some unpleasantness present at the close of the experiment.

No. of pulses,	5	5	5	5	5
Av. length,	4.4	4.2	4.2	4	4.4
		Higher.			

Experiment 36. — A. P. 5/28. The same color proved 'something fierce' and subject felt cold chills after it was taken away.

No. of pulses,	5	5	5	5	5	5	5	5
Av. length,	4.4	4.6	4.4	4.7	4.7	4.4	4.6	4.8

Experiment 37. — A. P. 5/14. Red and yellow glass placed side by side was said to be 'awful, distracting.'

No. of pulses,	5	5	5	5	5
Av. length,	4	3.9	3.9	3.8	3.8
			Higher.		

Our conclusion then is that excitement is accompanied by a normal rate of the pulse but that the pulse is often stronger. In the cases where higher is not written there was no appreciable change in height. A number of curves were also recorded during a state of unpleasant excitement and pleasant excitement, while there are also cases of strain where excitement is present. The most striking examples of the last named combination are the reactions which took place during the ticking of a metronome which varied in rapidity from 48 to 90 strokes per minute. The following are a few examples:

Experiment 38. — L. B. 4/28/'03. The stimulus was a quick metronome and the reaction was a feeling of strained excitement. My usual expression was that it 'keyed me up.'

No. of pulses, 5 throughout.

Av. length,	4.6	4.4	4.8	4.5	4.5	4.5	4.7	4.7	4.7	4.6

The dicrotic was lower during the first three periods and almost disappeared they were so high during the last two.

Experiment 39. — L. B. 5/26/'03. The stimulus was a very quick metronome while the reaction was a 'keyed up feeling.' I seemed to stop breathing and my heart seemed to beat faster.

No. of pulses, 5 throughout.

Av. length,	4.8	4.8	5	5.3	5.3	4.8	4.8	4.8	4.9

The height of the pulse and the dicrotic were both greater during the first two periods.

Experiment 40. — A. P. 4/9/'03. The stimulus was a slow metronome and the subject felt very excited, 'it was a torture to sit still.' The subject was expecting a stimulus of this sort.

Reaction.

No. of pulses, 5 throughout.										
Av. length,	4.1	4	4	4.1	4	3.9	3.9	3.9	3.9	4

The curve was higher with a higher dicrotic during the third period before the stimulus, then both apex and dicrotic become lower during the next two periods but become higher very gradually after the stimulus until the fourth period when they become lower and vary slightly till the end when the dicrotic becomes higher.

Experiment 41. — A. P. 4/16. The same stimulus as the above caused practically the same reaction and same physiological accompaniment.

Reaction.

No. of pulses, 5 throughout.										
Av. length,	4.1	4.1	4.1	3.9	4	3.8	3.7	3.7	3.7	3.8

Experiment 42. — S. C. 4/20. The stimulus was a metronome at the rate of 48 strokes per minute. The subject declared it to be exasperating and unpleasant.

No. of pulses, 6 throughout.									
Av. length,	4.4	4.3	4.4	3.9	3.7	4	3.8	3.8	3.9

The pulse curve is at first high with high dicrotic. During the next two periods both become lower while at the fourth period the curve is very irregular, showing on the whole a higher dicrotic. It gradually becomes lower until the end of the stimulus when it tends to return to the normal height.

Other examples might be given but as we are concerned more with simple than complex phenomena these must suffice to show that a mixed feeling of strain and excitement has physiological accompaniments differing from those of either of the pure states. A few examples will also suffice for the other mixed states.

Experiment 43. — A. P. 5/21/'03. A crash on the organ was surprising and unpleasant, distracting.

No. of pulses,	5 throughout.				
Av. length,	4.5	4.3	4.6	4.8	No variation in height.

Experiment 44. — A. P. 5/28/'03. A strange odor caused a strong feeling of surprise and unpleasantness.

No. of pulses,	5 throughout.				
Av. length,	4.8	4.8	4.6	4.4	4.5
				Shallower.	

Experiment 45. — S. C. 5/12/'03. A high whistle was irritating.

No. of pulses,	6	6	
Av. length,	4.8	4.8	No change in height.

Thus there seems to be no consistency in these results, certain physiological effects predominating at one time, at another time others, probably according to the difference in the composition of the feeling.

Experiment 46. — L. B. 4/28/'03. A high tone caused a feeling of excitement but was at the same time amusing.

No. of pulses,	5	5	5	5
Av. length,	5	5.2	5.3	5.3

Experiment 47. — S. C. 4/20/'03. Anise oil odor was pleasant and exciting.

No. of pulses,	6	6	6	6	6	6	6
Av. length,	3.7	3.6	3.8	3.9	3.7	4	3.8
			A trifle higher.				

Experiment 48 — S. C. 5/18/'03. Menthol proved a pleasant and stimulating odor.

No. of pulses,	6	6	6	6	6
Av. length,	4.5	4.5	4.6	4.4	4.4

Experiment 49. — A. P. 5/14/'03. A rich red plate of glass gave the subject a pleasant exhilarated feeling, 'toned her up.'

No. of pulses,	5	5	5
Av. length,	4.2	4.2	4
			Higher.

The results here are more consistent because excitement and pleasantness have the heightened curve in common as a physiological accompaniment.

Repose or depression as a pure feeling occurred a few times and was usually not accompanied by any marked changes in the pulse either in the height or frequency. The following are examples.

Experiment 50. — L. B. 5/12/'03. A glass plate sage green in color was quieting.

No. of pulses,	5	5	5	5
Av. length,	5	4.9	4.9	4.8

Experiment 51. — L. B. 5/26/'03. Bergamot odor was heavy and depressing.

No. of pulses,	5	5	5	5	5	5
Av. length,	4.8	4.8	4.9	4.6	4.6	4.8

The after effect of this odor was unpleasant.

Experiment 52. — A. P. 5/14/'03. Sage green was depressing.

No. of pulses,	5	5	5	5
Av. length,	4.1	4.1	4.2	4.2

Experiments 53, 54, 55. — A. P. 5/7/'03. Low organ tones produced in these three cases a solemn quiet feeling.

No. of pulses,	5 throughout													
			Reaction.					Reaction.				Reaction.		
Av. length,	3.6	3.5	3.7	3.7	3.7	3.6	3.6	3.8	37	3.7	3.6	3.5	3.7	3.6

Experiment 56. — 4/30. A light blue glass plate caused a feeling of depression.

No. of pulses,	5	5	5	5	5
Av. length,	3.9	3.8	4.3	3.9	3.9

Experiment 57. — L. B. 5/5'03. A low organ tone was felt as earnest, rich, a somewhat peculiar feeling but fitting in here because it seemed to induce repose.

No. of pulses,	5	5	5	5
Av. length,	5	4.6	4.8	4.7

Experiment 58. — This was followed by the odor of castor oil which was depressing.

No. of pulses,	5	5	5	5
Av. length,	4.7	4.6	5	4.4

In all of the above cases there is very little variation in the length of the pulse curves and little or none in the height. There is not so far as I can see the uniformly lowered pulse which Brahn finds, however there is certainly not an increase in height in any of them. The failure to find the lowered pulse may be due to the fact that the cases are not those of pure repose, though this seems very unlikely. I am much more inclined to think that the feeling of excitement and repose is far more often accompanied by muscular than circulatory changes. A number of experiments with the automatograph and ergograph which I shall not describe here, as well as the introspection of my subjects led me to this supposition. In nearly all cases of pronounced excitement the subject speaks of a desire to move, while in the repose the quiet, restful effect is spoken of. Everyday observation teaches us that certain music has the effect of stimulating to action while other melodies have a soothing, sleepy effect, etc. and one is very averse to movement.

Just as in the case of excitement so here in repose we find mixed feelings; repose and pleasantness and repose and unpleasantness. The following are examples:

Experiment 59. — A. P. 4/16/'03. A low tone produced a feeling of unpleasant sadness.

No. of pulses,	5	5	5	5	5	5
Av. length,	3.8	4	3.8	4	3.8	4

Experiment 60. — L. B. 4/28/'03. A low organ tone was pleasant and quieting.

No. of pulses,	5	5	5
Av. length,	4.8	4.8	4.8

Experiment 61. — A. P. 5/21/'03. A low organ tone was pleasant and rich..

No. of pulses,	5	5	5
Av. length,	4.4	4.4	4.4

Experiment 62. — S. C. 4/5/'03. Light blue glass was quiet and pleasant.

No of pulses,	5	5	5	5	5
Av. length,	3.5	3.4	3.7	3.6	3.6

Experiment 63. — S. C. 5/4/'03. The subject described the reaction from the sight of a rich purple-colored glass as being very satisfying, rich, and the most esthetic of all feelings, *i. e.*, not organic.

No. of pulses,	5	5	5	5	5
Av. length,	4	4.2	3.9	3.9	4.1

In the above we find comparatively small changes though all except the last show no increase in the frequency of the pulse.

Experiment 64. — L. B. A very unpleasant depressed feeling was caused by inhaling a sickening odor of onion essence three times in succession. The curve is very much lower in height than it usually is and at the same is more rapid. It became stronger just at the time of the second and third applications.

No. of pulses,	5	5	5	5	5	5	5	5	5	5	con.
Av. length.	4.7	4.8	4.7	4.7	4.6	4.6	4.6	4.5	4.5	4.4	4.4
						Very Weak.		Strong.	Weaker.		

No. of pulse (con.),	5	5	5
Av. length,	4.8	5.1	4.8
		Strong.	Weaker.

Experiment 65. — L. B. 5/26/'03. The odor of bergamot was very depressing and unpleasant, the latter effect following the former.

No. of pulses,	5	5	5	5	5	5	5
Av. length,	4.8	4.8	4.8	4.9	4.6	4.6	4.8
					Lower.		

Experiment 66. 4/20/'03. The stimulus was an odor which produced a depressed disagreeable feeling.

No. of pulses, 6 throughout.

Av. length,	3.5	3.5	3.2	3.1	3.1	3.2	3.6
				Lower.			

Experiment 67. — S. C. 6/1/'03. Castor oil odor caused

the subject to feel very depressed 'brain congealed, life not worth living,' followed by unpleasantness.

No. of pulses, 6 throughout.
Av. length, 4 4 $\underbrace{3.9 \quad 3.8}_{\text{Lower.}}$ 3.6 3.9 3.7 4.1

The feeling of unpleasantness-repose causes in all cases a decided decrease in the height of the pulse with a somewhat greater frequency, and it is clearly noticeable that the feeling of unpleasantness follows that of repose here as pleasantness and unpleasantness have been seen elsewhere to follow that of excitement.

Summary.

1. The physiological changes in the vaso-motor system accompanying the states of strain and relaxation are as follows. Strain is accompanied by a decrease in the length of the pulse curve, while the dicrotic becomes lower. Relaxation is accompanied by an increase in the pulse length and a heightened dicrotic.

2. The state of attention is accompanied by a feeling of concentration partaking of the nature of strain and has parallel physiological process closely resembling those of strain and relaxation but with less marked and regular characteristics.

3. The breathing in both the feeling of strain and relaxation is more rapid, regular and shallow than in an indifferent state. This is also true of attention accompanied by a feeling of concentration.

4. Pleasantness is accompanied by a longer and higher pulse curve; unpleasantness is accompanied by the reverse.

5. Excitement and repose are accompanied by less marked changes in the vaso-motor system than the other feelings. There is practically no change in the pulse frequency and while the former is accompanied usually by a heightened pulse, the latter is accompanied less frequently by a lower pulse according to my results, but certainly never by a higher.

6. The excitement-strain feeling shows characteristics of the curves of both, while the excitement-pleasure is pretty constant in showing a lengthened and heightened curve. The

curves for excitement-unpleasantness have no constant characteristics. Repose-pleasantness is accompanied by no constant marked characteristics but unpleasantness-repose is accompanied usually by a shorter and decidedly lower pulse curve.

7. Where the feelings are mixed, pleasantness and unpleasantness do not appear until after those of excitement and repose. The time of the appearance of feeling of strain depends on the time it takes for the concentration of the attention to the stimulus. Sometimes it appears almost at once and again two or three pulses later. When it appears in mixed feeling it comes later than the others.

8. The rhythm in the pulse lengths which correspond roughly to the act of breathing, *i. e.*, an inspiration and an expiration, tends to disappear during states of attention and the more so the stronger the feeling of strain and relaxation or of concentration. [1]

[1] The MSS. of this article was received on February 8, 1904.—ED.

THE PSYCHOLOGY OF ÆSTHETIC REACTION TO RECTANGULAR FORMS.

BY THOMAS H. HAINES, AND ARTHUR ERNEST DAVIES.
Ohio State University.

Since the æsthetic value of the golden section was first developed by Zeising,[1] all questions in the psychology of the æsthetics of form have centered around this one, the psychology of the golden section. And to-day it is as much an open question, why we like the golden section, as it was when Fechner wrote,[2] "If you ask me, I simply say, I do not know." Fechner's researches showed that the golden section was not so nearly the universal choice as had been claimed. His measurements of many thousand paintings, and the results of choices, amongst ten cards of different proportions, made by several hundred men and women, show, however, that the preponderance of choices is near the golden section. Witmer's[3] investigations emphasize the importance of the relation in both the rectangle and the ellipse, and also in the division of lines, and in relations of separate lines to one another. Angier's[4] results likewise indicate its importance in the division of the horizontal line. But the evidence is not sufficient for suggesting an exact law; it is not as strong, *e. g.*, as it was in favor of Weber's law. In fact Fechner says that his 'results for every class of pictures show that the relation of the larger to the smaller dimension is on the average much less than that of the golden section.' ('Welche Bilderklasse man ins Auge fassen mag, das Verhältniss der grösseren zur kleineren Dimension durchschnittlich viel kleiner als das des goldenen Schnitter ist') (*i. e.*, to make the golden section, the large ought to be larger, or the small, smaller).

[1] *Æsthetische Forschungen*, 1855.
[2] *Vorschule der Aesthetik*, 1876.
[3] 'Der Experimentellen Aesthetik einfacher räumlicher Formverhältnisse,' *Philosophische Studien*, 1894.
[4] *Harvard Psychological Studies*, Vol. 1, pp. 541 ff.

So much for the facts. On the side of explanation we meet a much greater variety. It is therefore the least satisfactory aspect of the problem, and the one that calls most loudly for serious constructive work.

1. To pass proposed explanations of this class of phenomena briefly in review, we may mention, first the theory which proposes the *mathematical relations* of the lines, as the ground of preference. The rectangle whose sides are in the relation of extreme and mean ratio, is pleasing because of the simplicity of the process by which the variety here offered is grasped as a unity. The smaller being in the same relation to the larger as the larger is to the sum of the two, gives us the simplest possible concept of an organism. It presents a pleasing variety, which of itself, by virtue of the simplicity of its mathematical relations, leads the mind to the idea of unity. Hence Zeising saw in the golden section, 'the complete embodiment of an ultimate æsthetic principle — the combination of a complete diversity in an harmonious unity.' This theory is closely analogous to the one which explains musical harmony on the basis of an unconscious counting of vibrations. Both are open to the same objection. Psychology knows nothing of unconscious cerebration. But the mathematical explanation of the golden section, fails for another reason; considerable deviations from the golden section, are as pleasant as it itself is, to most people. Thus there is nothing analogous to the dissonance arising from beats, in tone.

2. Various attempts have been made at a physiological explanation of æsthetic problems. First suggested by Schiller,[1] it has been developed by Spencer,[2] Bain,[3] and Allen.[4] In general it holds that the æsthetic gratification in any form of activity, depends upon the condition of the whole nervous system, or at least of all parts more immediately concerned in the reaction. Surplus energy, consequently, is the essential physiological condition of an æsthetic reaction. But a discharge of the surplus energy of a given organ, *e. g.*, of the retina is,

[1] *Letters upon the Æsthetical Education of Man*, Letter 27.

[2] *Principles of Psy.*, Part IX., ch. 9.

[3] *Emotions and the Will*, 1859. *Mental and Moral Sciences*, 1868.

[4] *Physiological Æsthetics*, 1877.

in itself, insufficient to bring about an æsthetic feeling. The arousal of an æsthetic feeling connected with the visual processes, depends upon the relative amounts of stored energy or freedom to discharge, throughout the whole visual apparatus. It involves, at the minimum, retinal, eye-muscle and cortical processes. The main defect of this theory is, that it does not loan itself to concrete application. It is too general to be of service in actual explanation. It cannot be applied with profit to the explanation of pleasing rectangular forms.

3. A far more serviceable theory is that which may be called 'explanation through association.' Its adherents say that we like a given form because of its resemblance to forms to which we are already accustomed. Books, cards, envelopes, panels, windows, etc., are made in shapes which are determined by the purposes they are intended to serve. There are pretty definite upper and lower limits, beyond which these forms do not proceed. There is also a proportion of the sides, which is more convenient in these objects, and which is embodied in a very large number of them. We get accustomed to these forms; a facility in reacting to them is established; and the majority of people react most favorably to the proportions predominating in the environment in which they live. We like therefore what we have seen most frequently, because of the facility of its apperception. This makes the æsthetically pleasing the predominantly useful. Its use determines its prevalence; and this in turn, determines the facility of its apperception, and this again determines its power to engender æsthetic feeling. The possibilities are that the reaction will be only slight if it is agreeable, but if it be a disagreeable or unpleasant reaction, it will be much more positive and intense. The reason for this is that the nerve paths are less obstructed, and so lead to a ready apperception.

4. Again it has been proposed as a specific explanation of the pleasantness or æsthetic quality of a proportion, or asymmetrical division or relation, that it is in reality a symmetrical division or relation. In order to take pleasure in an asymmetrical division of a horizontal line, we have to supply mental material to the shorter end, in order that it shall come up in

weight, so to speak, to the longer. The weight is a weight of attention. From a meaning read into the shorter line, — say a purpose that it is subserving, — it is occupying an equal share of the attention with the longer line, and the two parts do really balance for the onlooker. It is the same explanation which sees in so many pictures a large object on one side, balanced and compensated for, by the direction of the attention of the onlooker to the other side, by leading lines, or the gaze of a human subject. The physically unsymmetrical is shown to be the psychically symmetrical.

5. Another theory accounts for the enjoyment of beautiful objects, by movements of attention and imagination, coupled with activity of comprehension and sympathy. Applied to our own problem, this means, that in lines there is a wealth of pleasurable association, of a kind that connects them with movements, which we admire in living things. These movements are those which suggest unconstrained activity. Besides this ethical motive, the intellectual motive of comprehension enters also as an explanatory feature of æsthetic pleasures. By comprehension is meant the possibility of bringing a form, *e. g.*, a line, under a single law throughout, so that it may be understood. We see very soon what its intention is. The successive parts have novelty, and yet in the end realize the expectation aroused. "The mental formula or conception gained early in our perception of a line, is found throughout to apply." Another factor is the organic reaction which the movement and life attributed to the figure, gently stimulate. Alteration in the breathing and circulation, rhythmic change in tension of groups of muscles of the limbs, neck, and trunk, accompany attention to the change in the imaginary point along the observed figure. Organic factors are however, only secondary; their function is to react upon the complex mental state, giving it 'body.' The object of our æsthetic enjoyment, is not given in sense, but in a spiritual construction out of material, which is often the very opposite of æsthetic. Elementary æsthetic pleasure has the same factors as our highest enjoyments and involves the awakening of intellectual, ethical and religious ideas. If graceful form does not

arise directly out of our ethical and religious nature, it does from something akin to it, namely, our sympathy with well ordered action and love of participation in such action.

Each of these theories possesses elements of value, but no one of them is sufficient. Such an array of contradictory theories, is surely warrant enough for making a new excursus into the field of the psychology of the æsthetic reaction. In order to work within a narrow and yet quite typical field, the present investigation confines itself to rectangular forms. This is a desirable form to work with, because it puts the investigation in line with what work has been already done in the same field, and thus provides a means for checking results. But it is an important figure for another reason. Limitation of material is important in questions of this kind. Witmer has lost some advantage that might have followed his method, by a lack of discrimination among the selected forms studied. Take, for example, the group in which the rectangle appears. It is put with the triangle and ellipse only because they are all 'enclosed figures.' It does not appear however, that they are all equally simple forms. Moreover, whether they are capable of evoking an equally primitive reaction æsthetically cannot be decided previous to investigation. We have taken one figure of this group, thinking it well to keep the investigation as individual as possible. We have chosen the rectangle in preference to the other figures, because it is, as an object, simpler than the other two; there are fewer distractions for perception in it, there is consequently, less hindrance to the emergence of a purely æsthetic response on its first presentation. The individuality of our problem lies in the fact that one of the simple space forms, and one that is constantly recurrent in experience, has been selected and submitted to experimental treatment, with an aim to define its æsthetic suggestibility.

Throughout the investigation, it is a matter of importance to ascertain what figure is selected out of the series of rectangles constructed on the same base, but varying in width, up to and including the square. Is it the rectangle whose given sides are as the golden section? In any given case, or number of cases, do the judgments cluster around the golden section, as they do with Fechner's and Witmer's observers? But more important still, is an insight into the mechanism by which the choice is made. This inquiry was couducted so as to lead into the field of explanation.

II.

The methods of Fechner and Witmer were neither of them satisfactory. Fechner used ten cards, each having a surface of six hundred and forty square centimeters. Beginning with the square, the relation of the sides was as expressed by the following fractions: $\frac{1}{1}$, $\frac{6}{5}$, $\frac{5}{4}$, $\frac{4}{3}$, $\frac{29}{20}$, $\frac{3}{2}$, $\frac{34}{21}$, $\frac{23}{13}$, $\frac{2}{1}$, $\frac{5}{2}$. These were placed at random on a blank surface, the cards being white, and the observer made a choice of one, if possible. Sometimes two or more were chosen as equally satisfactory. In such a case, 50 per cent., 33⅓ per cent., etc., was put down for each one so designated, and thus the individual's choice was distributed in making up the average. Now an objection to this method is, that that the rectangle which is pleasing in one position, often fails to satisfy when turned 90°. Another objection is that the judgments as to whether or not one likes a given figure, must be comparative under these circumstances. Judgments should be made as far as possible, in isolation from other objects of the same kind. The distraction also incident to having many shown at the same time, should be avoided, thus securing uniformity of attention to each.

Witmer met the first objection by placing his cards in serial order, and all in the same position relative to the same observer But the comparisons and distractions were still incident in his method. To avoid all three of these objections, we showed the cards, one at a time. We placed the white card on the black paper on the table before which the observer was sitting and asked him to accept or reject each one in turn by a definite act of pushing it away, or taking it into his hand. This action called forth definiteness of choice in many cases which was a surprise, even to the observer himself. This was perhaps, the most significant factor in our method, different from the other two mentioned.

The advantages from this method, over that of Fechner's and Witmer's, may be set forth in detail:

1. *The Advantage of a Clear Objective Field of Vision.* — The accepted and rejected figures are removed to one side and do not enter into competition with the last presented figure. The advantage of this is to reduce the element of distraction to a minimum, and to have attention free to react upon the presented form. Both of these are absent from the method of both Witmer and Fechner. We do not, however, exclude the comparison of one figure with another in the same group as an aid for judgment, but this is found necessary only when, for special reasons, the mind has wandered from the matter under determination.

2. *The Advantage of Serial Presentation.* — Under this condition, each figure makes its own impression — is presented to consciousness in an unprejudiced way — and yet does not enter consciousness as an isolated or stray fact. Continuity within the whole range of facts, covered by a series, is secured, thus making change an element of the objective facts, not of subjective attention, as is the case with Fechner and Witmer. And the relations of every new fact are already laid down by the mental disposition aroused by the serial order. In all this we have a marked approach to conditions of normal life where, for example, our interests tend toward groups, and the continuity of conscious experience lies within these larger areas.

3. *The Advantage of the Motor Element.* — The subject was allowed to take any card in his hand about which he might be in doubt, and frequently a decision was arrived at very quickly. Also in connection with each judgment, whether it was favorable or unfavorable, the motor factor was introduced with the removal of the card from the field of vision. We claim this had the advantage of naturalness. Our judgments are, for the most part, expressions of motor significance.

We used, in all, four series of cards. In each series the length remained constant, the width alone varying. The sizes follow:

Series 1, length 80 mm., width from 25 mm. to 75 mm. by 2.5 mm. steps.

Series 2, length 90 mm., width from 25 mm. to 85 mm. by 2.5 mm. steps.

Series 3, length 100 mm., width from 25 mm. to 95 mm. by 5 mm. steps.

Series 4, length 120 mm., width from 25 mm. to 115 mm. by 5 mm. steps.

The following tables (I. and II.) present results obtained from the use of the cards. The numbers 80, 100, etc., indicate the length of the cards. The word 'horizontal,' means the card was so placed before the observer that its longest dimension was parallel to the line joining his eyes. 'Vertical,' was at right angles to this position, and in the same plane. The figures in the column mm., give the widths of the cards chosen, in millimeters, and those in the percentage column, give percentage relations of these widths to the given length.

TABLE I.

Obs.	Length 80 mm.				Length 100 mm.				Length 120 mm.			
	Horizontal.		Vertical.		Horizontal.		Vertical.		Horizontal.		Vertical.	
	mm.	%	mm.	%	mm.	%	mm.	%	mm.	%	mm.	%
Cr.	25.0	31	27.5	34	25.0	25	40.0	40	30.0	25	30.0	25
	27.5	34	37.5	47	50.0	50	70.0	70	75.0	63	50.0	42
	60.0	75	60.0	75	90.0	90			110.0	92	60.0	50
											75.0	63
Da.	50.0	62	55.0	68	65.0	65	75.0	75	80.0	67	85.0	71
Ec.	30.0	37	67.5	84	25.0	25						
	55.0	68	70.0	87	35.0	35	65.0	65	35.0	29	105.0	88
	75.0	94	75.0	94	90.0	90	90.0	90	105.0	88	110.0	92
					95.0	95	95.0	95	115.0	98		
He.	35.0	43	35.0	43	50.0	50	95.0	95	65.0	54	115.0	98
	57.5	72	57.5	72	95.0	95			115.0	98		
Ho.	37.5	47	30.0	37	45.0	45	40.0	40	55.0	46	45.0	38
	42.5	53	25.0	34					50.0	42	50.0	42
Ke.	40.0	50	70.0	87	45.0	45					70.0	58
	45.0	56	40.0	50	60.0	60	60.0	60	80.0	67		
					85.0	85						
Mal.	45.0	56	75.0	94	55.0	55	95.0	95	60.0	50	115.0	98
	55.0	68			70.0	70			75.0	63		
	75.0	94			95.0	95			115.0	98		
Mas.	40.0	50	40.0	50	55.0	55	55.0	55	60.0	50	60.0	50
	62.5	76	62.5	76	80.0	80	80.0	80	70.0	58	70.0	58
My.	42.5	53	42.5	53	50.0	50	55.0	55	70.0	58	70.0	58
					60.0	60						
Pe.	27.5	34	37.5	47	45.0	45	45.0	45	45.0	38	45.0	38
	37.5	47	57.5	72	55.0	55	55.0	55	70.0	58	60.0	50
	75.0	94	60.0	75								
			75.0	94								
Re.	40.0	50	70.0	87	45.0	45	65.0	65	35.0	29	90.0	75
					50.0	50			50.0	42		
					65.0	65			80.0	67		
					85.0	85			90.0	75		

TABLE II.

Observer.	Length 80 mm.				Length 90 mm.			
	Horizontal.		Vertical.		Horizontal.		Vertical.	
	mm.	Per cent.	mm.	Per cent.	mm.	Per cent.	mm.	Per cent.
Ra.	35.0	43	35.0	43	40.0	44	40.0	44
Mc.	27.5	34	25.0	31	32.0	35	25.0	27
N.	30.0	37	27.5	34	27.5	30	30.0	33
Sh.	62.5	78	52.5	65	65.0	72	65.0	72
L.	35.0	43	60.0	75	65.0	72	50.0	55
Sc.	40.0	50	70.0	87	35.0	38	25.0	27
							80.0	88
T.	32.5	40	25.0	31	30.0	33	32.5	36
Ha.	50.0	62	47.0	58	55.0	61	60.0	66
	52.5	65	50.0	62	57.5	63		
	55.0	68	52.5	65				
Po.	42.5	53	50.0	62	57.5	63	57.5	63
Do.	70.0	87	70.0	87	80.0	88	80.0	88
Pa.	37.0	46	25.0	31	32.5	35	30.0	33
	40.0	50	27.5	34	37.5	41	32.0	35
	42.5	53	30.0	37	40.0	44		
	45.0	56	37.5	40	42.5	47		
					45.0	50		
Ko.	45.0	56	52.5	65	80.0	88	80.0	88
	67.5	84	55.0	68				
	75.0	93	60	75				
			62.5	78				

A superficial examination of these results, is sufficient to convince one of the absence of any single principle determining the choices of these various observers. Indeed the results given by a single observer, as Ec. or Mal. shows that very different types are liked equally well. This is what is to be expected, if we are controlled in making such choices, by the associations we connect with such forms. That association does play a conspicuous, if not an important part, is borne out by the introspections. The observer likes two or more very different forms because of the different purposes which they subserve, and different uses which they suggest.

Thus, we find that the introspective notes reveal quite distinctly, that the reason for liking any particular figure or group of figures is, either (1) because of the similarity of the proportions of the chosen figure to those of figures met with previously, or (2) because the chosen figure fits in with a group of interests already developed in the case of the individual observer. As examples of the former, we may cite cases in which there is a likeness to 'calling cards,' 'note books,' 'writing tablets,' 'posters,' 'looking glasses,' etc. As an example of the latter, one observer says, that a given proportion of sides is good 'because she would choose it for a canvass on which to paint a marine sketch.' This observer paints marine scenes. It is thus not merely likeness to other canvasses which have been seen, but the arousal of personal interests that determine particular choices. Another prefers given rectangles because they are the 'figures he likes to deal with in geometry.'

Beside these two general types of motive, we find also other influences, which, while not so widely distributed, are sufficiently prominent to deserve separate mention. (3) Solidity and completeness, give rise to certain of our æsthetic judgments. This is the case when a rectangle is spoken of as 'complete in itself,' or 'has wide enough base to hold it up,' or when it produces the feeling that it can be 'left alone.' (4) A complication of motives often determines such choices. If a rectangle suggests a panel, its selection is due to familiarity; but if, in addition, it is a panel suitable for the 'Gibson girl with a curl over her shoulder,' the limitation of use to which the panel is to be put, is imposed by the personal interests of the individual. These introspections indicate that the results here presented, are not wholly lawless, as might appear at first sight, and at the same time they point out the direction in which an explanation is to be sought. Two individuals may like different figures because they are controlled by different motives. In the same way, different grounds of choice, determine the same person to like two very different figures equally well. Looking at the tables, we readily see examples of what might be called type-selection, rather than individual-selection. *E.g.*, Ec. in Table I., shows clearly at least two types, in every series. So also do He., Mas. and Pe. Pa. of Table II., is a good case of many choices, but all conforming to a simple type. All four of these, in the first column, *e. g.*, ranging in width from 46 per cent. to 56 per cent. of the height, are equally satisfactory and for the same reason.

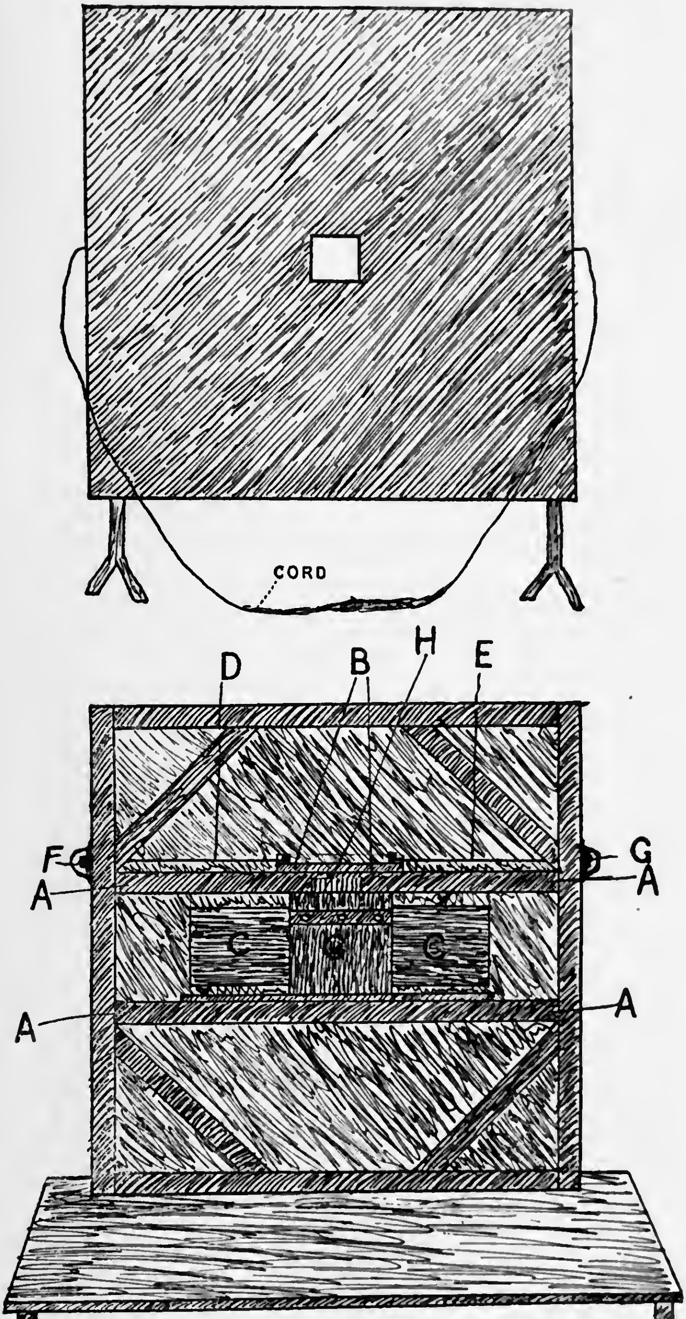
The evidence, however, for the existence of types, is yet stronger from the introspective notes. Some observers come with preferences ready formed, but not well defined, so that anything within certain fairly wide limits suits them. The figures conform more or less closely to what they demand. The observer says, *e. g.*, 'I always liked figures with even sides,' or 'These are exact proportions, in other words, a rectangle whose width is about half its length,' or 'a rectangle must be longer than twice its width, and shorter than three times its width.' It is often harder for the observer to select a given figure, than it is to say that five or six suit him. In other words, the group is easier selected than is a specific figure. But as you progress in a series, there comes a limit somewhere, that marks off this type from others; it is the limit of forms capable of being assimilated under the ruling concept. If choices are made beyond this, they are determined by other motives and belong to other types. Thus there are comparatively wide limits to a type, and yet types are clearly discriminated in given individual observers, and between individuals.

These facts in regard to types, show the improbability of the golden section being the æsthetic norm. They also point out the error of applying the method of averages to such results. To use the method of averages, in cases where such significance attaches to individual results, is indeed to kill the goose that lays the golden egg. So far from being a source of embarrassment, these variations themselves contain not only their own explanation, but in them is to be found the germ of the explanation of this whole class of æsthetic judgments. The variations then must be preserved. The method of averages, as applied by Fechner and Witmer, vitiates their results.

But the apparatus and the method of experimentation with cards, also come in for a share of criticism. The steps in the series may be too wide. *E. g.*, in Fechner's series of only ten cards, there are probably many cards left out which some observers would prefer to any that are given. The same holds of Witmer's series and our own. Clearly the ideal apparatus for this experiment, would permit the observer to make his own figure.

We used a piece of apparatus (see Fig. 1) which allowed of this possibility. It consisted simply of a screen of black card-board set up in a vertical position, 1.3 meters from the eyes of the observer, when he was sitting in a chair fixed to the floor. This screen was one meter square. In the center of the screen was an opening 10 centimeters square. The center of the opening was on a level with the observer's eyes. A simple mechanism provided a means of moving a black card across the opening, behind the screen. As a part of the frame to which the black card of the screen was fastened, there were two horizontal strips, *A* (Fig. 2), about 25 centimeters apart. Vertical saw slits were cut in these. A slide, *B*, rested upon the upper one of these pieces, and a card *C*, fastened to it, and depending through the slits, moved back and forth from side to side of the frame behind the window from the observer. Cords *D* and *E*, from either end of the slide, passed through pulleys, *F* and *G*, at the sides of the screen, and thence to the hands of the observer on the other side. The depending movable card was white, but in its center was pasted a black card, more than wide enough to cover the opening in the screen. The edges of this were exactly parallel to the opening of the screen, so that on whichever side of the opening it might be, right or left, some of the white card was exposed. Thus the figure presented to the observer, in white, was an exact rectangle. A millimeter scale, *H*, on the back of the screen, afforded a ready means of measuring exactly, from behind the screen, from right or left, any width of figure the observer might give. The work was done in a dark room, the only source of light being a sixteen candle power light held in a box directly above and behind the observer. Light was emitted through a three-quarter-inch hole, and even this was covered with tissue paper (Figs. 1 and 2).

By this simple apparatus it was possible to secure the motor reaction on the part of the observer, which had been found so useful with the cards. The observer had it in his power to modify the width of the figure within the limit of 10 cm., by any minutest step which he could perceive. Moreover, it was possible to go over uninteresting parts of the series with haste, thus avoiding fatigue, gaining time, and obtaining results with a more constant state of attention. The apparatus also provided a ready and efficient means of obviating two sources of



FIGS. 1, 2.

error which it itself might seem to introduce, namely, (1), by the direction of approach from right and left, and (2), direction in the series, *i. e.*, increasing or decreasing width. Each of these was obviated by the method of reversal. We approached the question always in the four ways thus made possible:

1. Closing in from a white square from the right.
2. Closing in from a white square from the left.
3. Opening out from an all black from the right.
4. Opening out from an all black from the left.

Tables III. and IV. present results given on this apparatus, with these four variations, by the same observers whose results with the cards are presented above. The results given are widths in millimeters for the four movements. The length was 100 mm. in all cases. The results therefore are also percentage values of widths in terms of length.

Table III.

Obs.	1	2	3	4
Cr.	62.5	55.0	56.0	45.7
Da.	65.5	61.0	54.0	55.0
Ec.	100.0	93.0	96.0	96.0
	57.0	100.0	53.5	67.0
He.	92.0	97.0	93.0	93.7
	53.0			
Ho.	54.7	50.5	49.5	53.6
		68.0		
Ke.	51.0	51.3	49.3	49.0
	49.5	47.5	45.0	45.0
Mal.	64.0	70.0	75.0	71.0
Mas.	56.0	70.0	22.0	45.0
	65.0	57.5	27.5	31.8
My.	62.0	68.5	66.0	64.5
Pe.	73.0	73.0	44.0	48.8
Re.	72.0	61.0	59.0	50.0
	63.0	61.0	38.0	47.0

Table IV.

Obs.	1	2	3	4
Ra.	41.0	36.0	21.0	28.0
	50.0	54.0	31.0	41.5
Mc.	39.0	43.5	27.0	26.0
	48.5	55.5	32.0	28.0
N.	62.5	46.5	26.0	25.0
	33.0	33.0	49.5	34.0
		23.0		47.0
	45.0	43.0	42.0	43.0
Sh.	76.5	64.0	81.5	76.5
L.	36.0	56.0	30.0	59.0
Sc.	51.5	56.0	52.0	48.0
	72.0	49.0	41.0	49.0
T.	42.0	56.0	45.0	44.0
	46.0	45.5	46.5	44.5
Ha.	64.5	65.5	70.0	70.0
	67.5	68.0	69.0	63.0
Po.	61.5	57.5	58.5	55.0
	41.5	39.0	56.0	52.0
Do.	87.7	85.5	84.0	83.0
	85.0	83.0	86.0	87.0
Pa.	55.0	45.5	28.5	22.5
	38.5	33.5	24.5	24.5
Ko.	57.5	81.0	91.0	80.5

Some advantages for this apparatus and method, already anticipated, come out in the results here presented. Greater facility in making a definite choice is secured. An observer who gave five cards near together as being equally desirable

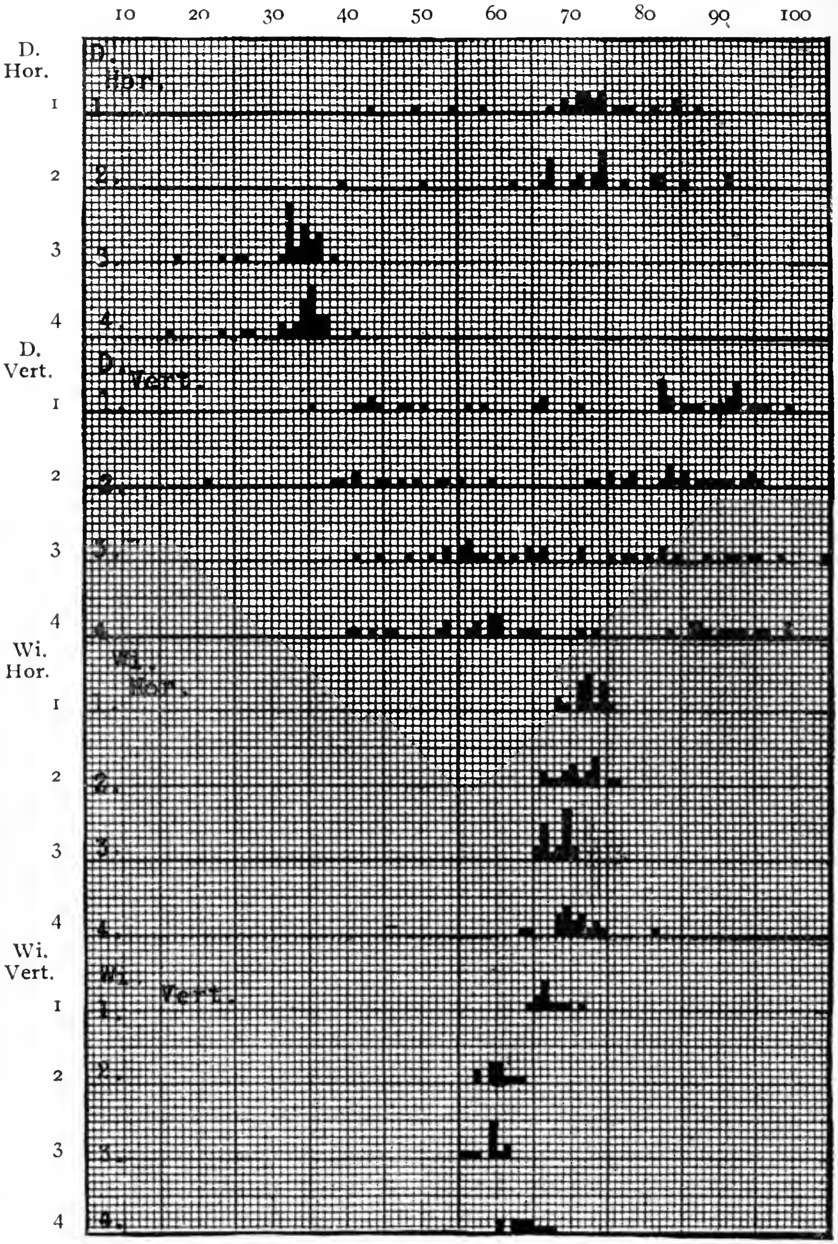

10 20 30 40 50 60 70 80 90 100
D.
Hor.
1
2
3
4
D.
Vert.
1
2
3
4
Wi.
Hor.
1
2
3
4
Wi.
Vert.
1
2
3
4

CHART 1.

would here give one as his choice: *i. e.*, one can more easily make his choice, here, among several that belong to the same type. It is evident, too, that sometimes a type is chosen very different from any given by the same observer with the cards. This is explained by the remarks of several observers. *E. g.*, one says he is 'less limited by material here.' A card is a card; but here he is interested in form, as such. Again Ec. selects two types: the square, and one about 67 by 100 mm. The square she says, she 'just likes,' but the other form she says 'would be a good form for a card if you dared have it.' She was able to see it more purely as form. On the whole, it is evident that the problem before us is more purely one of form. The observer feels less hampered by the conditions, and arrives at more definite judgments.

But the more serious and extended study of the problem is yet to be reported. In it, we confined ourselves to this apparatus. Altogether, there are seven observers whose results are reported on the apparatus as above described in the horizontal movement of the shutter. The screen was then turned 90°, making the slide move up and down, thus giving the four movements.

1. Closing from above. (Start with white square.)
2. Closing from below. (Start with white square.)
3. Opening from above. (Start with all black.)
4. Opening from below. (Start with all black.)

In this part of the work, there were in all, twelve observers. Some of these worked an hour a week for several months. Others worked only a few hours altogether. The amount of work done by each observer is readily seen from the charts, since each blackened square stands for a judgment. These blackened squares indicate by their position what was the width of the figure preferred, since there is a space for every millimeter of possible width. It has also seemed worth while to preserve the absolute results, even with regard to each one of the four ways of approaching the problem, since these are so strikingly different in some cases.

In Chart I., under D. (observer) Hor., we find the results presented as given by D. in the horizontal movement, for 1, 2, 3 and 4 as above explained. In (1) closing in from a white square from the right, *e. g.*, we find he stopped once at a figure 39 mm. × 100 mm. But he gave most of them between 63 mm. and 80 mm. wide, giving three each at 67 mm., 68 mm. and 70 mm., none at 71 mm. and one at 72 mm.

It is apparent at once in looking over the results indicated in these tables that all the observers had quite different choices. It is to be remarked that those observers who worked most with the apparatus give the most scattering results; and in the case of one, D., where the height of the figure was changed, 'vertical,' the choices are more widely distributed than in horizontal, indicating decreased definiteness of choice. This, however, is not to be taken as the natural effect of continued work. It is explained in large part by an illusion of which he found it very difficult to rid himself. The same holds of H., in a lesser degree. This also was due in some measure to the same illusion. In both these cases the *direction* of movement was responsible for the scattering only in so far as it was responsible for these illusions. Wi., Wa. and J. are the three other observers whose results give us good ground for comparison of results in vertical and horizontal movements. None of these show any considerable increase in scattering in the "vertical." All are clearly indicative of definiteness of choice, under a single motive. K. 'vert' is another example of scattering results. It was very noticeable in the case of this observer that, as the work proceeded from week to week, his limits of choice narrowed down. It is safe to say that, in the last three hours of work with him, all his choices fell between 38 and 48 mm. On the other hand, the increasing scattering with H. and D. are no doubt due to the increasing number of motives determining choices.

In general, the results show definiteness of choice. Where two motives rule, the ground between the two groups is pretty clear. See, D., 'Hor.,' and H., 'Hor.' Difference of types, where two occur in the same observer, and in different observers, comes out more significantly than in the card results given above. It is pertinent here again to ask the question, What would be gained by averaging the results of H., 'Hor.' to find how near his choice fell to the golden section? Or what would an average of the results of Wa. and J. indicate? Of what imaginable use could such results be?

The existence of two types in the same observer, for one position of the apparatus, is clearly shown by D., 'Hor.' and H., 'Hor.' Both come to a wide one which they like 'closing in,' in movements 1 and 2, and to a narrow which is equally satisfactory, 'opening out,' in movements 3 and 4. This interest in two types from the first in these observers is worth considering in relation to the later development of still other types. O. also has a clear leaning to wide ones for movements 1 and 2, and to narrow ones for movements 3 and 4. He said 'he would like the narrow ones better in 1 and 2' if he could 'content himself to come to them.' H. is an example of overcoming the interest in the narrow one, in about half the judgments in movements 3 and 4.

Another point of interest here is the relation between the ratios of the two dimensions of the figure when the movement is horizontal, *i. e.*, height is greater than the breadth, and when the movement is vertical, *i. e.*, breadth is greater than the height. Fechner found in his measurements of pictures that Genre pictures, whose height was greater than breadth, had a normal size of 1.202 by .992 Prussian feet, or ratio *1 : 82*. Genre pictures whose breadth was greater than height were 1.737 by 1.389; ratio *1 : 78*. Landscapes, height greater than breadth 1.890 by 1.330; ratio *1 : 70*. Landscapes whose breadth was greater than height, 2.271 by 1.571; ratio *1 : 68*. These results were the product of very extensive averaging, as already explained, and they were not very far apart. Our more intensive study of a few individuals presents no clear evidence for the one side or the other. Wi. and L. clearly prefer a narrower figure when it is turned on its side, *i. e.*, their figure whose height is greater than its breadth presents a smaller ratio of shorter side to longer than do their figures whose breadth is greater than height. The former for Wi. are about 65 by 100, and the latter about 57 by 100. This is in Fechner's direction and more so. But H. 'vertical,' compared with 'horizontal,' shows a clear widening of both types; J. also shows a clear broadening of the figure (one type) when the figure is turned 90°; D., even with this scattering, shows a clear broadening of both types; Wa. is practically the same in both series.

III.

We turn now to a classification of the motives leading to the choice. These are shown by the introspections of the observers themselves. They come in answer to questions both as to why they like those they do like, and why they do not like various unpleasant figures which they either make themselves or which the experimenter gives them. These are often very productive. Now these reactions of pleasant and unpleasant character are, of course, relatively simple likings and dislikings. They are simple reactions of an observer to a comparatively simple content. In any such case it is natural to find the reasons for liking, sometimes placed in the object and sometimes expressed in subjective terms. Thus the figure is now satisfactory in itself and now satisfactory because it fits in with a particular purpose of my own, or it conforms to an ideal proportion of mine.

This we make the basis of our first division of motives to choice. We find five classes, as follow. They form a convenient though not strictly a logical division of motives.

1. Motives found in the object. These are what may be called pure æsthetic judgments of simple type. The figures, so judged, seem good in themselves. We do not want to do anything with them. In the other classes the observer does want to do something with the figure, or it expresses what he wants to do.

2. The figure soon *grows* from the *immediate interest* of (1) to a definite suggested use. These we call *suggestive*. Emotion and apperception function in these cases.

3. This apperceptive element precedes the reaction. There is a preconceived ideal, as *e.g.*, in the ordinary association. Expectation is *definite*.

4. Simple sensory elements, sometimes illusions, make for pleasantness or unpleasantness of the figure.

5. A pure motor element comes into prominence.

The notes will explain these.

1. In giving 30.6 immediately after 70, D. says, "That's

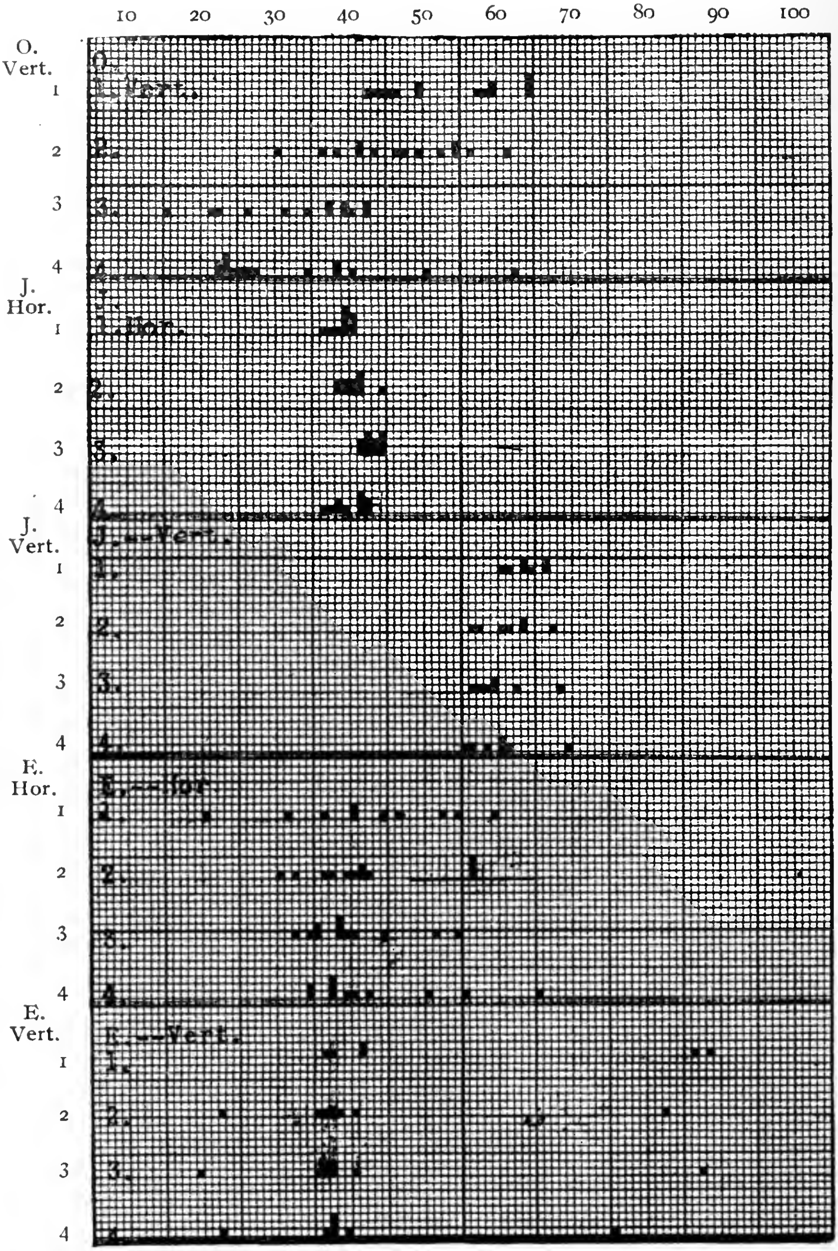

10
20
30
40
50
60
70
80
90
100
O.
Vert.
1
2
3
4
J.
Hor.
1
2
3
4
J.
Vert.
1
2
3
4
E.
Hor.
1
2
3
4
E.
Vert.
1
2
3
4

CHART 3.

easy. I like it for itself. I wonder now how I like the broader." Again he says, 'there is an absence of demands.' Again, 'there is here a permanence of mood.' He mentions pulses of feeling as he moves the shutter in. These, however, he finds occur at definite places in the scale, as he tries it with different rates of movement. Many observers use such expressions as the figure is 'easy,' it is 'comfortable,' 'it is neat,' it 'fits in,' it is 'complete in itself.' Again, 'It is open and has variety,' say D. and K. 'The lines differ, and are yet in harmony,' says L. 'It is substantial.' H. says of 67 'vertical' that 'it is substantial and complete in itself.' He says "that 75 as compared with 90 'horizontal' is pretty satisfactory"; 90 is 'too clumsy.' D. says of 70, 'It is too bulky, but I could spare very little.' L. says, 'The square is too bulky and has too much sameness about it.' Moving horizontally to 35, D. says 'all wider ones are too squatty,' by which he seems to mean they are too wide for their height.

2. This class is the direct outcome of 1. In fact it is often hard to assign a given case. These cases the observers describe as being 'sudden discoveries.' They are seized with a liking for a figure, which was totally unexpected. "It was an arrest, I had to do the stopping myself." They had failed to find anything in it before. An 'immediate liking' for it which is not coördinated with any preconceived ideal or associated use, seizes them. These interests may soon *develop*. They often do. But at the moment of its 'bursting in,' there are no interests or developed feelings. To K. and S. it comes with a 'thrill of satisfaction,' and Wi. likes it as he would 'like a $10 bill handed him as a present.' D. says of 42, it 'somehow fixed itself into shape.' Again it is described as 'causing no jar.' Another division of this subclass is differentiated by the emphasis placed upon the suggestions arising out of the figure which is at first liked in and for itself. *E. g.*, one observer says, 'I stopped for this' and the next moment 'it looks like a memorial tablet.' He 'finds an interest' and this develops. In other cases, a 'general idea of the place where a favorable judgment is coming,' develops as the shutter moves. The suggestion as to the goal of the process, seems to come out of the

process itself. Again an observer refuses to go on because of an unpleasant figure, to which the process seems to be leading. The case of O. fearing to go to the narrow one which he said he preferred to the broad one given, probably belongs here. The middle ground between the broad and narrow contains some such suggested bugbear. H. once refused 60, which had been the previous jugment, but went to 39, because he did not know 'what it (60) was useful for.' For him, suggestiveness is a positive factor, and distinct from associations, *i. e.*, from preconceived ideals and uses. He demanded a certain development.

The square, or what the observer called a square, was frequently chosen. Some of these seem to be cases of the kind we are here dealing with. The observer explores the whole series and finds nothing that develops an interest or proves suggestive. He takes the square because nothing else is interesting. Not that *it* is positively interesting or suggestive; but in a series failing to produce a development, the observer makes choice of a figure which is least objectionable. This is preferable to no figure at all. He finds no goal in *form* itself, and so sets himself to the more formal task of making the sides equal. This is the only thing he can do. This may be in part the explanation of the rising curve in favor of the square in Fechner's results. (See table S. 195, *Vorschule der Aesthetik.*) Another motive for the selection of the square is present no doubt. The square may be the preconceived ideal. But observers often said, 'I like this because I don't like anything else.' This is given also for the narrower rectangles. *E. g.*, 'This is not bad, though I can't get what I want,' or, 'I like this (38.7) because all others are unsatisfactory.' Again an observer tires of a type, and takes something different merely for the sake of a change. H., working on the motive of calling card, made a 'good envelope.' O., getting tired of 30, which he had been giving quite regularly, gave some in 60 and some in 10 and 20. Such a motive often explains K.'s changes from the norm which stands out so clearly in his results. Sometimes the 'possibility of developing in two directions' is a determinant factor. *E. g.*, 'I like 87.3, because it is partly in the square

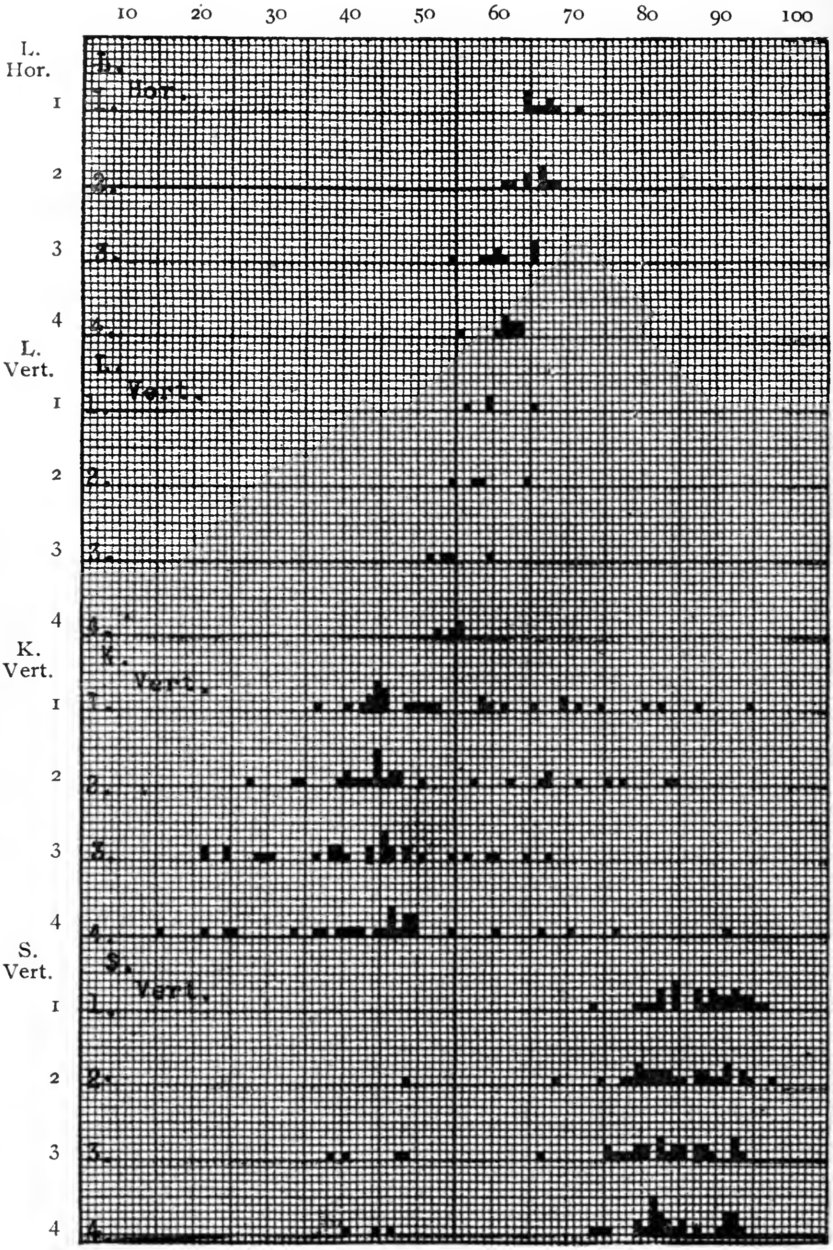

10 20 30 40 50 60 70 80 90 100
L. Hor.
1
2
3
4
L. Vert.
1
2
3
4
K. Vert.
1
2
3
4
S. Vert.
1
2
3
4

CHART 4.

series, and yet is not a square.' Again 45.3, is 'midway between the broad and narrow. Possibilities in these two directions bred the interest in this.'

3. In the last class of cases the suggestion grows. In contrast with it those to which we now turn, find the predominant motive in a preconceived idea. We may for convenience call these *associations*. We find a definite *expectation*, and the figure 'comes' to fit it. It is described as the '*developing* of a negative.' L. says 'I have always liked pictures and such things of this shape (54–62), *i. e.*, tall, high and narrow.' Another sees a window shade, and stops when it is the right distance up. D. says (60.5): 'This is what I have been trying for.' Sometimes these ideals cannot be realized. The observer is certain he knows what he wants, but he cannot find it. It eludes him. "I need something narrower, but I can't find it." Another says, 'I cannot get this (60) any time I want it.'

Among these preconceived notions which control choices are to be mentioned the things one knows about the golden section and its theoretical place in such work. The class embraces all notions of what ought to be the mathematical relations between the sides of a rectangle. *E.g.*, H. says: 'Mathematical associations as of the golden section assert themselves once in awhile.' This observer was giving, as a general thing, 28 and 90. This was also true of J. "General relation of golden section present." No other observer acknowledged this as a motive. Many of them had never heard of it. We made it a point to preserve their *naïvété*. Preconceived bad forms are also often determinative. D. says: 'I thought I was coming to something I would not like and so stopped.' In other cases it is said, 'to go on would be to destroy it.'

Associations are of course very numerous. A given form is good, because it is the shape of something which is familiar, or which one likes. Such objects as slabs, blocks of marble, step to entrance, coping over a doorway, sarcophagus, visiting cards, envelopes, photograph cards, window sills, entrance to a large house, open courtway, marble pillar, double door, church entrance with arch, bronze memorial tablets, transoms, window shades, loads of hay, and rising curtains, are found in the list.

These vary from individual to individual and with the same individual. H. was particularly fruitful in these. But all observers declare, many times, they have no associations.

4. The sensory elements indicated in this division are apparent chiefly in certain illusions. Three observers get illusions of movement. For W. (horizontal) 'with steady gaze, both sides seem to move out,' when in reality, only one moves. D. had a very lasting illusion of movement in the vertical series. He seemed persistently to desire figures of as much as ten centimeters width, but wanted more height than he could get. Of 78.2 he says, 'from here, the sides come in until I get the narrow one (47) and then the sides stand still.' Here there is an entire change of type, for, 'when he narrows still more the sides shoot out.' This in D. naturally leads to an illusion of form. He says, 35.2 "ought to get higher. But instead of doing this it gets broader." Of 55, he says, it needs to be higher and lower at the same instant. If he increases the height (vertical dimension) the sides come in, and if he decreases it the sides go out, and so, in both cases, his aim is defeated. Sometimes a whole series is given where this illusion does not operate. But usually some one or more in the four is affected. Where it is not operative, the wide figures, about 84–87, are given. The trouble with those wider than 49, under the illusion, is that the breadth leaves, or the horizontal lines are too short. Of 78, again he says, 'I like it when I take it turned 90°.' It is well supported. But moving along the horizontal line, as it is, it is flat. Again, 60 is good so long as he keeps his 'attention off the horizontal lines, and so resists the tendency to narrow it.' Control of attention can defeat the illusion. 17 is the first place in closing where he could 'defeat the horizontal cramp.' This illusory transfer of movement may be responsible for the greater spreading of D.'s results in the vertical series.

Another observer gets an illusion of the white card moving to meet the black, whereas in reality, it moves with it. Another sees the white as 'uncrumpling' or moving out both ways with the 'new substance welling up in the middle,' as the figure opens; and for him the white seems to be 'crushed in' as the figure closes. As to the form, again, the vertical lines are often seen converging toward the top, in the horizontal movement. The corners are too large; the advancing line is 'jagged' or 'bulged,' or 'wavy.' In his first work (horizontal 'moving in') D. says of 61, 'It is jagged up to this point; 61 assumes definite appearance; and smaller than this, is amorphous.' Another likes the figure whose sides are broken and not exactly at right angles to each other.

Another wants less light, 'to remove harshness of outline.' One says, 'the narrow ones get into a shadow,' when there is no objective shadow. Wi. says 64 is 'soft and restful to his eyes.' 'The glare of 95,' he said, 'was what made the sides slope in at the top.' He complains of 40 that 'it is a little dark, the corners do not show up well, and has to move to get it clear,' though no complaint of darkness was made about 28, immediately afterward, when 28 was preferred to 40. D. often wants something bigger than he can get. Of 44.2, 'I want something the size of the square and the proportion of this,' and of 49, 'I suppose this is what I would like if I could see it large.' These illusions and demands are all in a measure criticisms on the method, but more particularly are they cited here to be reckoned with in explanation of the results.

5. The motor element, it is true, was brought in by the experimenter in every case when considered. That is he carefully questioned the observers after considerable work had been done, as to the presence, especially of eye movements. Some follow the moving edge of the black card, but they find this does not give them a satisfactory view of the changing figure. Sometimes, they move the eyes alternately, first in one direction, and then in the other. Wa. says she probably knows when she has the right figure, by eye movement. D. says of 74.7, 'There is a balance of the two movements of the eyes.' There is a pain in the eye for further lateral movement. 'But the square (100) just before it,' is not explored by eye movements. "And 29.5, (horizontal) is explored up and down, only. The narrow top is easily measured without movement." He is very certain that the feeling, 'the figure is the right one,' does not come as a development of kinesthetic sensations from the eyes. All observers agree, that they really size up a figure with a fixed

gaze, taking in the whole. K. says, 'when you get a satisfactory one the eyes are still.' As one gets used to the apparatus, he invariably drops exploration, finding the best results come with steady gaze.

Of course, there is an important motor element in that the observer makes the figure for himself by pulling on the cords. One likes the narrow figures, 'because they are easy,' requiring only a little movement. Another likes it, because 'I know I am making it.' And wherever the observer has a ready-formed purpose, this gets fulfilled by his own production, if the quest does not prove futile, by reason of limitation of opportunity or otherwise. His *doing* is an important factor in the resultant satisfaction. By the balanced control of the pulls executed, through the two hands, he has made that which fits his purpose. It is his own, and he likes it. Some observers say, 'I like that now, since I made it or found it myself. I do not know that I should, if you had showed it to me.'

IV.

In the face of such a great variety of motives as above described, can we hope to find a single principle of explanation of the species of phenomena under consideration? There are very manifest differences in kind of motive. In fact, we find in this single study as great variety of motives, as there are theories occupying the field. In view of this variety, it is hardly to be expected that any one of the current theories can be adequate to a complete explanation of these æsthetic phenomena. Associations, for example, are clearly inadequate to explain the reactions of many of our observers. One observer never had associations with the figures she liked. Only when asked why she did not like the figures that were repulsive to her, did she find associations. She could never give a reason for liking the figure, except such as were descriptive of the figure itself. No more can motor sensations be made the basis of a complete explanation of æsthetic reactions. They are no doubt a factor, but we find they are only one, and comparatively unimportant.

In fact, Stratton (*Philosophiche Studien*, XX.), has shown very conclusively, that eye movements made in exploring a pleasing figure, of either right or curved lines, do not follow the graceful and easy lines of the figure. The movement is jerky, and is interrupted by frequent stops. This, of course, does not preclude obtaining a fairly accurate measure of the two dimensions of the figure, by the eyes; but it does warn us not to rest too much upon the kinæsthetic sensations from the eyes in the explanation of our choice of rectangles. The play of motor elements which are not actually functioning, is of course important. The figure is sized up by 'staring at it' say some observers. Of course, given retinal stimulations, may mean for the observer, certain definite movements. The local signs are fused, by means of the suggested movement, into space perception. The perception, as in all cases, grows only by the integration of motor with sensory elements, even though these motor elements are only latent movements.

There is though, more importance attaching to the sensory side of the process, so-called, than is sometimes allowed. In such an experiment as our own, the simultaneous stimulations of parts of the retina with light of greater intensity, brings out the figure independent of eye-movement sensations. Having given the adaptation of the eye to the dark screen, in our experiment, what happens when the shutter is drawn open? Clearly there is a relation between the required adaptation to the brighter surface now exposed, and the existing adaptation of the dark surface, which must prove a determining factor in the size of the chosen figure. The greater illumination in the centers of the retinæ, not only calls for an adaptation in these areas, but also to a new adaptation or a readjustment in the surrounding portions whose absolute stimulation has not changed. In other words, McDougall's[1] explanation of simultaneous contrast applies here, in part. This double process of readjustment, and the relations between the two processes, explain many of the illusions of form, such as jagged lines, sloping lines, and large angles. It may also explain many of the demands for size, as well as the expressions, 'it is soft,' 'comfortable,' and 'easy on the eyes.' There is of course, a motor element in these so-called sensory processes. Retinal adaptation clearly has a motor side. Mc-

[1] *Brain*, 1903, Pt. II., pp. 183 ff. See also *Mind*, N. S., XII., pp. 473-488.

Dougall shows that the after-image depends upon accommodation. And adaptation, doubtless, depends upon some motor processes. But for any definite statement of what it is, we are awaiting the future results of histological physiology.

Other motor sensations than those of ocular origin, no doubt play an important rôle. We have already mentioned the arm movements. One observer was very conscious of these. He often put his forefingers out on the cords and thus controlled the movements of the shutter through the more accurate articular sensations from the fingers. These sensations are a basis of choice, in an observer who says, 'I like that because I can make it easily.' But much more are they the means of doing what one wants done,— they are the media for the realization of our purposes. And this calls attention to a transition similar to that made above. As the eye movement may be the basis of the perception of dimensions, and so of the preferable figure, *and yet not become a movement*, so our motor tendencies as a whole, may be the basis of our choice of a rectangle, although these lie wholly dormant. This is the basis of many of our choises which show no definite motor factor. The motor side of these æsthetic processes, is highly important for the purposes of explanation, because it is so important a part of the process.

Another explanation of æsthetic judgments closely akin to that of balance of eye-muscle sensation, is that which endeavors to explain our likings by what may be called a balance between attention forces. It may be stated objectively as a balance of attention-drawing features in the object. They are sometimes spoken of as *forces* in the picture or the line. But they are forces of attention, so that it is ultimately and fundamentally a balance of attention and interest, which is made the basis of explanation. Thus, in the case of our rectangles, it would be said, that a given width is coördinated with 100 mm. of height, and a given height with 100 mm. of width, because this width or height has an interest or attention-drawing power equal to the interest in the 100 mm. height or breadth, respectively. This theory is valuable, in so far as it is an explanation of the wide departures from the golden section. One's interests change. The observer views the shorter (or the variable) dimension, in different lights at different times. It has now more of his attention, and now less, owing to the different ruling interests. So its affinities are different. Hence also the varying results, from observer to observer, as well as in the same observer from time to time. Our objection to this theory is, that it is not an explanation of the complex phenomena under investigation. It is very aptly descriptive of the facts in broad outline, but it leaves us with the problem of explanation, where we start. It seems on the face of it to go back to very simple elements. But attention is both the most baffling, and the most complex of psychological concepts. We cannot hold, therefore, that a theory is final which bases on attention, and says the asymmetrical elements are harmonious because of a psychical balance brought about by more attention or interest to the shorter. Any balance brought about by a heightening of the vividness of one element through attention, can by no possible means be ultimately simple; for this heightening is itself a complex process, not mere addition. It is a typical piece of organic mental growth. This being granted, it is the first business of the student of the phenonenon to gain insight into the mechanism of this growth. And when he has seen the component part processes of each, he will be in a position to understand the æsthetic balance between two given whole processes. What we have called *suggestiveness*, helps us, in some measure, to the desired insight.

In our experiment, under normal conditions, the observer was not consciously concerned with a balance. It was primarily a question of satisfactoriness of form. To be sure the form used is, to the analytic onlooker, only a composition of the two dimensions. To consider the relations of these two lines, each to the other, would be, *a priori*, a simple problem. But our experience confirms us in the view that this form may be considered — in fact is naturally considered — independent of, and prior to, the relation between any two contiguous sides. This is, in fact, a very primitive æsthetic experience, and insight into the reactions of the class called suggestive, really affords a basis for the explanation of the so-called balances of attention. For it does not require there should be two new experiences in order that we should have an æsthetic reaction — a balance between them. At least the restriction of the term, æsthetic, to such limit, precludes the cases, which alone, as it seems to us, give insight into the mechanism of the higher æsthetic emotions.

In the cases called suggestive reaction, the experience comes to a mind alert, but inactive,—attentive, but without any purpose, save to get the experience that comes from the presented stimulus, and to note the pleasantness or unpleasantness of this experience. In some of these cases, the experience seems very immediately pleasant. As contrasted with what we have called purely æsthetic, there is a reason here for the pleasantness, in the suggested *use* of the form. And they are different from cases of association, too, in that the use is suggested by, and comes out of, the experience itself. The emotional reaction and the suggested use seem to come simultaneously. On this point it is difficult to obtain definiteness by introspection. But *a priori* we would suppose that the suggested use preceeds and that the pleasantness is an indication of the mutual appropriateness of the experience and of the use, each to the other. This *a priori* view is supported by introspective evidence from another class of *suggestive* cases.

In these, as the observer changes the figure, a purpose arises which is not fulfilled. A goal is set for the process, as the process itself is perceived, member by member. By the use of this word goal, we do not wish to denote an end for action, consciously present in the observer's mind. It is rather the conative process set up by the sensory processes serially perceived. This conative process is too large and too vague for immediate execution, but it is that which determines the motor outgo of the moment. There thus arises or exists a disparity in the mind. Our clearest way of expressing this, is, that it is a disparity between the goal for which the motor outgo starts, and that which it realizes. It is, in fact, though, to the observer himself, at the moment, simply a baffled emotional state. The aroused emotional tendencies fail to get realized. The balance here is between the ideal, however vague, which always means conation, and the incoming experience.

The æsthetic moment is the moment of satisfied wants. In so far as there is satisfaction with the new experience, there is no further conative tendency. This is the case, in part, with what we have called purely æsthetic reactions. It is characteristic of the æsthetic mood. But right here in the sensi-motor circle[1] of the mental processes, at the point of impact of the sensory processes, is the place where emotional tendencies or ideals arise. The incoming experience either satisfies or it does not satisfy the previous emotional tendency or conative impulse, and in so far as it jars on it or fails to fit in with it, a new emotional tendency arises, which, expressed in terms of intellectualist theory, is an ideal. This in turn, leads to a new motor outgo, which brings a fresh sensory process, and so the circle repeats itself over and over; but it is mounting as a spiral, and growth is occurring, through the organization of this men-

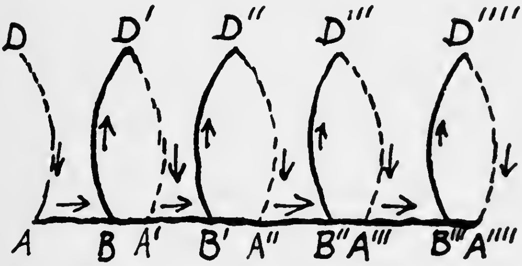


FIG. 3.

tal material called emotional tendencies. And the mechanism of this growth is through the impulses imparted by the incoming material jarring on the emotional incentive which led to the motor outgo. And right at the point of impact of the sensory income, as said before, is the place of the æsthetic feelings. These are simply the satisfactions with what we have called the *fit* of the *incoming* with the *emotional incentive* to the outgo, as the unpleasant are the dissatisfactions with the misfits. Thus we see the very fundamental place of these feelings in the growth of the mind. Our meaning may be brought out better by a diagram. Let A, A', etc. (in Fig. 3), represent successive sensory processes just arising through the action of stimuli

[1] We refer to the circular process in mental life so admirably developed in Professor J. M. Baldwin's *Mental Development in the Child and the Race*.

coming from objects D, D', etc. The psychic stream is represented by the line A–A'''', and B, B', etc., are incipient motor outgoes. A–A', etc., are then places in the circular reaction where the fit or the misfit of the new experience arises, this being the feeling of satisfaction or dissatisfaction. In the case of the latter, an emotional tendency to action organizes from this point and emerges in action at B. The return sensory process is at A', and this leads to a new motor outgo, B'. So the organization proceeds. This is simply to illustrate the place as we conceive it, of the elementary likes and dislikes in the sensi-motor circle before there is other psychic life.

Association and suggestion are such important elements in our theory, and they are so likely to be confused, that some added remarks are introduced at this point on their relation to one another. To start with the characteristic difference from the descriptive point of view, we may say that suggestion carries with it the specification of a definite use for a given figure, while association always affirms the likeness of a particular figure to a class or group of figures. Put into general terms, this means that suggestion is characteristically practical while association is theoretical or scientific, so far as motive is concerned. This has direct bearing upon the material and relative simplicity of the two classes. As to material, we may say that suggestion is predominantly emotive, but association intellective. That is to say, the wants that are contemplated by each class are different. Association seeks for a definite relation between the given object and the observing subject, in which case the æsthetic mood is held in abeyance, and is made to depend upon the solution of a problem. For illustration of this we refer to many cases where the square was chosen (see page 368). Suggestion subordinates the relation between observer and the thing observed, and allows free play for the feelings in determining the æsthetic object. In this way, the ideal is found, not made. It is discovercd, not anticipated. Thus we may see that suggestion is relatively much more simple than association. It takes us back nearer to the *elements* of the æsthetic experience.

This may be made clearer by laying bare the processes involved in suggestion and association. It is a commonplace of psychology to-day, that all psychic material is dynamogenic. When we are at the roots of mental life, this means that the question, 'What are you going to do?' is much more fundamental than, 'What do you perceive?' So that whether we perceive anything or not, the primary arousements of consciousness involve us in some kind or other of activity. It is not possible, on the basis of the present study, to go beyond this fact of motor connection. It is however, the point which is most

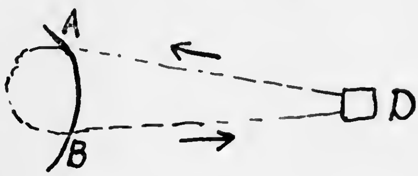


Fig. 4.

emphatically borne out by the results which we have presented above. This connection is the simplest form of psychic process with which we are acquainted. *What* the connection is and *where* it is, we are not able to say. These remoter questions are extremely interesting, but beyond our present work. All we can now affirm is, that impressions coming from the object *D* reach consciousness at the point *A*, and go out in motor discharge toward the object from the point *B* (see Fig. 4).

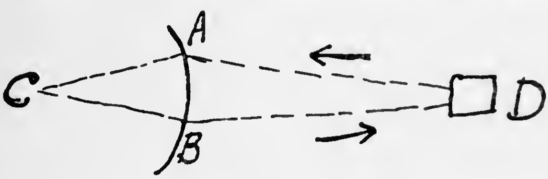


Fig. 5.

The importance of the motor reaction consists in its making secure the connection between *A* and *B*, and probably, in giving greater definiteness to *A* directly. In contrast with this, association is a more complicated process. Instead of defining *A* through *B* we have here the interpretative idea *C* (see Fig. 5), which is a go-between for *A* and *B*. The definiteness that comes to *A* from the motor outgo *B* in this case comes through *C*. *C* is also a factor determinative of the motor reaction. That is to say, action is probably both ways in this complex arc, as it is in the simpler one above.

Two other points of relation between association and suggestion should be mentioned in passing. One is the difference in the ways of their inception. The suggestive process starts at *A* of the above figure and the associative process starts with *C*. In the former, there is nothing until the sense experience suggests it, and this runs simply to its own fulfilment in the simplest cases, as a sort of psychological reflex. In the typical associative process, however, the preconceived idea *C* is the starting point and controlling factor of the whole. The other point of difference is already evident. The associative process is the more general of the two. This is consequent upon the complexity mentioned above. Suggestion leaves you with the particularity that belongs to the individual object or form — it makes it more concrete than at first; association takes away the individuality and substitutes the universality of the class. Association is essentially a process of classification, and this implies emphasizing common features. This is what was meant by saying that association comes from a scientific motive; it is fundamentally the process of classifying experience.

These suggestive and associative processes are not always pure and clearly distinguishable. The suggestive, being the more primitive, is always involved in the associative. There are also many gradations between the two. The suggestive process is often the starting point of an association. The process starts as a suggestion, but the psychic processes develop so far and so explicitly in advance of their realization that the idea, or ideal use, of this kind of experience serves at once as a guide to the motor outgo. Comparisons are prominent, and the whole has been raised into the higher psychic plane. Whereas, had the case remained simple we should have had a development without conscious guidance, on the level of simple feeling and mere conation. It is in rare instances, and usually under experimental conditions, that the human subject can know that he has had such an experience. As another example of mingled suggestion and association we mention those cases where an association helps on a suggestive process, but leaves it free to be controlled by the objective interests. You may have a limit imposed, as when you want a full length Madonna painted. The size is a definite, associated restriction upon the process, imposed by our ideas of use. But in many ways suggestion works unhampered.

We thus get a glimpse of the way in which that which is not perceptive or intellectual psychic material develops into that which is intellectual. It consists in seeing how that which we have called emotional tendency (*A–B* in Fig. 4) becomes ideal or interpretative idea (*C* or *A–C–B* in Fig. 5). The animal that can feel an incrongruity and stay by it, adjusting itself to its environment on the one hand, and adjusting the environment to itself on the other, is the animal that will survive; and the fittest to survive, has most of this accomodative power. Analytic power, which is the precursor of synthesis or generalization, which in turn is association, emerges at first very crudely in a being able to react on an aspect of an experience, neglecting unessential details. This kind of analysis at once has great survival value, and the animal that can translate his various impressions into an ordered experience will have begun the ascent of mental as distinguished from organic evolution. Thoughts are feelings objectified on the basis of wants through motor response to existing environment. Feelings and conation are directly connected, and when feeling becomes *generic*, *i. e.*, tends to react in response to the object for less than the object actually is, it is making possible that definite implication of the object in consciousness, which is the characteristic of intellectual life.[1]

[1] **The MSS. of this article was received on March 17, 1904.—Ed.**

CONCEPTIONS AND MISCONCEPTIONS OF CONSCIOUSNESS.[1]

BY RALPH BARTON PERRY.

Harvard University.

Were the use of the term consciousness to be forbidden for a season, contemporary thought would be set the wholesome task of discovering more definite terms with which to replace it, and a very considerable amount of convenient mystery would be dissipated. There is no philosophical term at once so popular and so devoid of standard meaning. How can a term mean anything when it is employed to connote anything and everything, including its own negation? One hears of the object of consciousness and the subject of consciousness, and the union of the two in self-consciousness; of the private consciousness, the social consciousness, and the transcendental consciousness; the inner and the outer, the higher and the lower, the temporal and the eternal consciousness; the activity and the state of consciousness. Then there is consciousness-stuff, and unconscious consciousness, called respectively mind-stuff for short, and unconscious psychical states or subconsciousness to avoid a verbal contradiction. This list is not complete, but sufficiently amazing. Consciousness comprises everything that is, and indefinitely much more. It is small wonder that the definition of it is little attempted. One of the most successful efforts is that of Professor Ladd, who regards consciousness as the difference (presumably from the sleeper's point of view) between waking and dreamless sleep. This is equivalent to the difference between more or less of something, and nothing at all; which is quite accurately true to current usage. Baldwin's *Dictionary of Philosophy* defines consciousness, on the one hand, as 'the distinctive character of whatever may be called mental life,' and mind, on the other hand, as 'the indi-

[1] Read before the American Philosophical Association, December 30, 1903.

vidual's conscious process, together with the dispositions and predispositions which condition it.' But it is more customary to say frankly that the term is indefinable. If it were taken for granted that it is therefore better left unemployed in exact thinking, there would be no occasion for objection. But its indefinability is more commonly attributed to the profoundness of its meaning. Indeed the definition of being in terms of consciousness is set down as the surviving and most illuminating truth of philosophy. The hope is expressed that we may now postulate it and proceed to more debatable matters.[1] And consciousness so regarded as the fundamental ontological truth, is called upon to carry and protect man's moral and religious interests. Especially in the nineteenth century has this term suffered the taint of eulogy, through being made the watch-word of non-materialism. The advocates of the spiritual man, never over-scrupulous in their choice of weapons, have in this case been willing to confound the enemy by confusing him. 'What are you going to do with consciousness?' asks the idealist. The materialist, not knowing precisely what it is, but convinced that it bears no resemblance to a motion or a secretion, does nothing with it. Whereupon the idealist shows him what he can do with matter, and the materialist, who is a stupid adversary at best, takes refuge in a general protest against metaphysics. The defeat of materialism is not to be regretted, but there must be no uncritical acceptance of the victor. The term consciousness as at present employed is too reminiscent of this controversy. It stands for a general propaganda, which runs some-

[1] "There are certain accepted doctrines of modern philosophy — *e. g.*, that knowledge is only of phenomena, not of anything unrelated to consciousness, and that object and subject are correlative — from which this conclusion seems to follow so inevitably, that anyone who has adopted it must enquire anxiously why it is not more generally recognised. If nothing can enter into knowledge that is unrelated to consciousness; if relation to a subject is necessary to make an object, so that an object which no consciousness presented to itself would not be an object at all; it is as difficult to see how the principle of unity, through which phenomena become the connected system called the world of experience, can be found elsewhere than in consciousness, as it is to see how the consciousness exercising such a function can be a part of the world which it thus at least coöperates in making." Green's *Prolegomena to Ethics*, pp. 14, 15. Compare the more recent statements of C. A. Strong, in *Why the Mind has a Body*, pp. 166, 183, 186. The criticism of such views as these is undertaken later in the present article.

what as follows: psychology and transcendental logic disprove materialism, seat God on his throne, and prove the immortality of the soul. When one ceases to look upon them from the standpoint of the counter-thesis of materialism, these are impossible allies. Consciousness cannot mean everything and yet mean anything. As a name for the psychological aspect of experience, it may be shown to mean something definite and important; but consciousness so interpreted is confused and misconceived when called upon to serve as a metaphysical account of being, and is no safeguard of man's spiritual interests. Consideration of morality and religion will be omitted from the present discussion, which will attempt first to account for and define a concept of consciousness, and second to criticise its metaphysical use.

Only a succssful analysis can justify the proposal to account for this concept in terms of psychological experiences, the more so since the term 'psychological' must be defined at the same time. But the arbitrariness of the procedure is at a minimum when we begin where the race and the individual have presumably begun to learn of these matters. Before a certain moment in the development of reflection the self is theoretically indistinguishable from body, and conceptions of it throw no light on the idea of consciousness; while after that moment the self is conceived with definite reference to a specific type of experience which has come to be noted and differentiated. That which makes this difference between the early cosmological, and the later radical or critical conceptions of the self, is the distinct employment of a set of ideas signifying seeming or appearance. While primitive experience is entirely free from any general idea of the dependence of objects upon the knowing of them, there are certain accepted cases in which an experience is definitely recognized as *my experience*, or certain facts which are regarded as deriving existence from a *for-me* relation. This is a very different idea from that of the functioning of the sense-organs. That I see and hear and taste is a commonplace of all experience, and I may study what I see, or the manner of my seeing, without effecting any discontinuity in my practical or scientific world of things. But to believe that what I see is constituted by my seeing of it, is to define a new realm, an anomalous science, and possibly a new philosophical method. Such a belief must arise very early in connection with discredited or illusory experiences. Illusions so vivid as dreams are doubtless in the beginning often regarded as unusually significant experiences of objects, but such can scarcely be the case with all dreams, with fever-deliriums, and with wanderings and inventings of the imagination. And these adventures are homogeneous with certain very familiar and normal happenings. Experience is constantly correcting itself and discrediting its earlier content. Observation and identification is a process of self-correction. The surviving judgment is the last of a series of discarded judgments which were once as living as itself. They are not the object *A*, but 'what I thought,' 'the way it seemed to me then,' my mistake, or confusion. To be sure, such retrospect is not demanded for the direct purpose of observation or identification, but they cannot altogether escape the notice even of the man of affairs. They tend, as in the case of the double images, to be neglected because not important. They become important, however, whenever the task of thinking becomes specialized, and interest is aroused in conditions that tend to determine its success or failure. Error and confusion come then to be attended to, and designated as a realm of idiosyncracy, to be corrected or repudiated by the wise man. The appearance of these ideas in early Greek philosophy is familiar history. They determine the common distinction between 'truth' and 'opinion'; and the Protagorean doctrine is an inference from them.[1] The aspect of experience recog-

[1] A special interest attaches to the earliest statements of this idea in philosophy. The following are representative:

"It is not meet to act and speak like men asleep." "The waking have one and the same world, but the sleeping turn aside each into a world of his own." *Heraclitus*, Fragments 94 and 95 in Burnet's *Early Greek Philosophers*.

"Welcome, noble youth, that comest to my abode on the car that bears thee tended by immortal charioteers. It is no ill chance, but justice and right that has sent thee forth to travel on this way. Far, indeed, does it lie from the beaten track of men! Meet it is thou shouldst learn all things, as well the unshaken heart of persuasive truth, as the opinions of mortals in which is no true belief at all. Yet none the less shalt thou learn of these things also, since thou must judge approvedly of the things that seem to men as thou goest through all things in thy journey." *Parmenides*, in Burnet, *op. cit.*, p. 184.

nized in this old epistemological criticism has played an important part in modern philosophy, where it appears notably in Spinoza's conception of modality and inadequate ideas, in Kant's manifold of the internal sense, and in Hegel's doctrine of subjective spirit. It furnishes the most likely definition of the field of psychology, and with reference to its bearing upon this problem, let us consider the analysis independently of its history.

The first intent or bearing of experience is objective,[1] as expressed in the judgment, *that is A*. But experience proves to be self-corrective. The content of *A* grows in the direction of its own completeness. *A* is in the first instance more or less problematical, and increases in articulateness. While the direction or interest remains the same, this experience is homogeneous, an experience, we say, *of the same thing, or context of things*. But an act of attention is possible whereby the direction is reversed. With this new interest there now appears a series of corrected experiences, to any degree of inadequacy. These specific limitations may be noted and attributed to specific conditions. In this wise the corrected and replaced experience, in contradiction to the corrective experience, is viewed as merely *my experience*, a term of my blindness and struggle. Since I have now apprehended the thing itself, I can define my more or less successful purpose with reference to the thing. In ordinary experience I have my face to reality and my back to such of the

"And the soul is like the eye: when resting upon that on which truth and being shine, the sou perceives and understands, and is radiant with intelligence; but when turned towards the twilight of becoming and perishing, then she has opinion only, and goes blinking about, and is first of one opinion and then another, and seems to have no intelligence." *Plato*, Republic 508 D, Jowett's translation. Cf. 510, 511.

"The senses are variously named hearing, seeing, smelling; there is the sense of heat, cold, pleasure, pain, desire, fear, and many more which are named, as well as innumerable others which have no name; with each of them there is born an object of sense,—all sorts of colours born with all sorts of sight and sounds in like manner with hearing, and other objects with the other senses." From *Plato's* exposition of *Protagoras* in Theaetetus 156 B, Jowett's translation. Cf. 157.

[1] I am at pains in this part of the analysis to avoid any verbal suggestion of the indispensableness of the subject-object relation. I shall, therefore, so far as possible, use the terms 'thing' and 'real' rather than the equivocal term 'object.'

cognitive process as I have passed by. But I may turn and behold the way I have come, together with its stages; and these latter I now denominate *points of view* in contradiction to that which may be so viewed. Such is the psychical fact and the reflection required for the identification of it. Let us turn to the consideration of examples.

The most unequivocal instance is the dream. This is a definite type of invalid experience, recognized as such from the standpoint of a valid corrective experience. Were there only dreaming, there would be no dreaming. Either I must myself awake or have my illusions observed by another, who both knows them and knows beyond them. The waking and the dreaming differ in that the former not only succeeds the latter, but includes and replaces it; while the latter on the other hand knows nothing of the former. The waking experience defines my dreaming, and in the presence of the real judges it to be unreal. When I wake up to the actual situation, my dreaming takes on the duller hues of a subjectivity and fancy which I significantly call my own.

There is a similar distinction between the narratives of the historian and the eye-witness. The historian corrects the experiences of the eye-witness by marshalling contemporaneous events and by eliminating the more accidental sequences and coincidences of observation. In view of the real order of events, the uncritical report of an individual may be circumscribed and identified as such. A continuous series of maps of the battle-ground, with the formations and movements of the combatants, would so include and transcend the order of occurrence in the experience of a soldier of the ranks.

Let us turn to those instances that are due to the deliberate psychological interest. The need both of an included and of a supervening experience is here determinative of a method, and is most clearly in evidence in the case of comparative and experimental research. The experience of the animal, child, savage, or abnormal subject, is viewed as within a valid world of experience, and interpreted in terms of specific and characteristic limiting conditions. In experiment these limiting conditions are in part artificially provided, and with them is coördinated the report of the subject, the whole being contained in the presumably or practically unlimited experience of the investigator.

But lastly let us consider the more crucial case of introspection, and in particular, introspective attention to perception. How is the psychological manifold differentiated from the thing-manifold where there are no social relations involved? The possibility of it is clear, the manner of it obscure. I can analyze my perceiving on the one hand and the object of my perceiving on the other with quite different results, and yet in the perception they are indistinguishable. The difference must lie in my interest, or in the direction of my attention, and it appears here also that one interest is fundamentally determinative. Indeed, the method is essentially identical with the judgment, 'I have been dreaming,' except that in this case the invalidity of the corrected experience is less radical. Introspection is retrospective attention to an experience which I now surround and surmount. That more or less complete apprehension which can now become a distinct manifold for me because I compare it with the occasion itself, I call *my state*. The actual method employed in this type of investigation is commonly hidden on account of the rapid alternation of interests. My objective experience is constantly awaking from new dreams. I must oscillate rapidly between the standpoints of experimenter and subject. From my standpoint as experimenter, my experience as subject is the relatively inadequate experience whose boundaries I may now view retrospectively and whose limiting conditions I endeavor to analyze. Consider the case of my perception of a house, which tends to reveal to me its true geometrical form, together with the totality of its exterior and interior. In ordinary experience I have it so present to me; practically, as is attested by my dealings with it, and theoretically, as is attested by my description of it to another. But I may compare with this valid experience the inadequacies which are contained and compensated for within it. My corrected spatial perspective would constitute such an inadequacy, and I may analyze this as respects its content, and as respects the manner and the means wherewith the correction is made. In such procedure the house has been regarded as the culminating event in a process of mind, and the factors determined by such an analysis are called states of mind. This interpretation of the method of introspection might be further and more readily illustrated with reference to imagination and memory. The same method holds in the case of feeling, this psychical factor appearing in the experience, *I want or like A*, in contradistinction to the experience, *A is good.* Feeling is an invalid judgment of worth. In each case the field of psychology comes into view only when an incomplete experience is recognized as such from the standpoint of an experience regarded as objective. The corrected or discredited experience so determined critically in an experience of things, is regarded as merely my experience, and may be analyzed as such. But we must have passed beyond the psychical to become aware of it. These psychical data cannot be called things or reals in the same sense as the standard objects, for they are completed and replaced by the latter. We therefore provide a radically different category for them, and recognize that their content is common to themselves and to things, while their specific character is given them by their limitations and context.

Accepting for the present this definition of consciousness in terms of relativity, let us examine the attempt to construe it as a philosophy. Such a theory might properly be designated as psychological idealism, and is known under the names of perceptual idealism, phenomenalism and sensationalism. This theory arises from the thought of the possibility of indefinitely extending the psychological manifold. Every corrective experience may, and tends to become in turn, a corrected experience. There is no experience of which one may not come to say, 'it is my state,' or, 'it is your state.' "At first sight," says Walter Pater, who styles himself a new Cyrenaic, "experience seems to bury us under a flood of external objects, pressing upon us with a sharp and importunate reality, calling us out of ourselves in a thousand forms of action. But when reflexion begins to act upon these objects they are dissipated under its influence; the cohesive force seems suspended like a trick of magic; each object is loosed into a group of impressions—colours, odour, texture—in the mind of the observer. * * * Experience, already reduced to a swarm of impressions, is ringed round for each one of us by that thick wall of personality through which no real voice has ever pierced on its way to us, or from us to that which we can only conjecture to be without. Every one of these impressions is the impression of the individual in his isolation, each mind keeping as a solitary prisoner its own dream of a world."[1] On such grounds one reaches the generalization that every knowable object is someone's perception, or the more radical persuasion that every knowable object is his own perception. The only definable being is seeming. In terms of the above analysis, this is equivalent to the proposition that everything so far as knowledge is concerned is invalid experience. To assert this proposition is, of course, to plead scepticism. But even as scepticism it is not tenable, since it is a criticism of experience according to a principle. There can be no experience of a world in which each mind keeps 'as a solitary prisoner its own dream of a world.' The Protagorean mind must itself have awakened and broken from its prison. Madame Ackermann is quoted as saying: "My last word will be: 'I have been dreaming.'"[2] But in that moment she will at last have ceased to dream. Relativism will not do as a doctrine, though it may serve as an apology for silence. And where the Protagorean principle has been asserted, it has almost invariably been associated with a deeper metaphysics calculated to make this principle itself a psychological one. In the case of Protagoras himself, the world was conceived with Heraclitus and Democritus as essentially motion. Perception is itself a type of motion, and so incapable of fixing upon permanent being. But motion itself is otherwise and distinctly conceived, so that perception is defined in terms of being, and as *within* a world. Such is clearly the case with all definitions of the perceptual realm in terms of so-called 'secondary qualities.' Where the motive of the physical sciences is the determining one, and this is very commonly the case, the world gets itself divided into the physical and the psychological realms, the former being

[1] *The Renaissance*, pp. 247–248.

[2] James, *Varieties of Religious Experience*, p. 63.

employed as the standard and defining world. And here a subsequent reduction of knowledge to psychological terms is evidently contradictory.

The perceptual idealism of Berkeley announces subjectivity as an ontological, and not merely an epistemological principle. The famous dictum, 'esse est percipi,' is the ontological counterpart of the more ancient dictum, '*Πάντων χρημάτων μέτρον ἄνθρωπος*.' But it appears shortly that to be is rather to perceive, or to cause to perceive. The soul and God are the real terms of the perceptual relation, and they are themselves revealed in another order of cognition. Berkeley's later tendency to abandon his perceptual idealism for one of the Platonic type is well-known, and emphasizes his inability to make an objective order out of the psychological realm. But he persisted in this course so far that he made content and subjectivity coextensive, and was then under the necessity of adding the objectivity all at once and abstractly. The same necessity is interestingly exhibited in the case of J. S. Mill, whose category of 'possible experience' functions similarly as objectivity conceptually and artificially superadded to a content that has been stripped of it.

Sensationalism in its other modern and contemporary phases scarcely warrants serious treatment. It commonly defines sensations as events within a physical world, and then gravely announces that these sensations, as the simplest terms of introspective analysis, are the ultimate beings. But the perfection of this contradiction is enlightening. The sensation is the quintessence of relativity. It signifies objectivity at a minimum and subjectivity at a maximum. Simple pressure, or the lonely and unrecognized sound, are the first dawning or the last waning of objects. But they are such vanishing points only in the light of their all but entire inadequacy. In themselves these pulses of experience are objective, and are remarkable only when we come to consider the great degree of their deficiency. Sensationalism means the attempt to define being in terms of what it is not. Indeed, such a plan is virtually announced in the language of all relativists. The Protagorean proposition stated ontologically would read: all things are the human measure of them, which contains the same substitution of a passive for an active or neuter verb that is remarkable in the Berkeleyan principle. But any account of being in terms of another than itself is as unprofitable as it is contradictory.

The *transcendental idealist* would doubtless regard the discussion up to this point as a stage in the development of his own argument, and he must now be reckoned with quite independently. His doctrine is established with direct critical reference to psychological idealism. The impossibility of defining objects in terms of relativity is allowed to conduct the thinker dialectically to the conception of the absolute. The sequel to my error or exclusiveness, is truth or inclusiveness. The outcome of this dialectic is determined by the symmetry of the antithesis. Corrected experience implies a last correcting experience; partial cognition, complete cognition; empirical subject, a transcendental subject; finite mind, an absolute mind. Hence being is definable as for a standard, complete, transcendental or absolute consciousness. Now it is evident that the validity of this reasoning depends upon the degree to which the limiting adjective determines the meaning of the substantive. If consciousness means limitation, then absolute consciousness is a phrase but not an idea. Where consciousness is recognized as relative, what does it mean apart from that relativity? This question has remained unanswered so far as transcendentalism is concerned. But if consciousness known as experience relative to a point of view, is not defined save in terms of that circumscription, then to retain the concept of consciousness for a realm defined as free from just that factor of circumscription, is sheer absurdity.

Let us consider briefly the Kantian foundation of transcendentalism. The *Critique of Pure Reason* taken as a whole, informs us that the object, so far at any rate as knowable, can be neither inside nor outside of my private consciousness. The dilemma is solved by defining the object as apperceived by a transcendental ego which is the ideal cognitive subject logically immanent in my consciousness. This subject remains for Kant a law of my consciousness and thus dwells in that logical realm which is neither soul nor nature, until it enters into the real world under the form of faith. But its relation to the realm of knowledge is such as to define nature as phenomenal on the ground that it falls between the unthought world on the one hand, and the completely thought world on the other; between the residual objectivity of the perceptual experience, and the ideal objectivity of the conceptual experience. There is too much or not enough of consciousness in the natural world to permit of its being a world of things; too much because space and time are merely subjective necessities, and too little because these forms of perception are such as prevent the realization of the ideal of subjectivity itself. The phenomenal realm is distinguished from such a realm as would have its being independently of thought, and such a realm as would have its being in the perfection of thought. So far as theory is in question Kant leaves us here. For the post-Kantian who wishes to define a metaphysical doctrine, there are two possibilities. He may conceive that successful thought ceases to be distinguishable as thought, and therefore realizes being as independent of thought; or he may conceive that successful thought is still essentially thought and therefore realizes only its own consummation. Kant's phrase 'intelligible intuition' permits either interpretation; the former is the way of realism of the Platonic type, the latter the way of post-Kantian idealism. But the only account of mind that is offered even by the idealist is an account in terms of its practical function with reference to the things which it seeks to evisage. As in Kant's delimitation of the realm of the internal sense as psychological, the realm of physical or external experience becomes for the moment a realm of things; so in any delimitation of the phenomenal world as a whole, the noumenal world becomes a realm of things. Now if I define my real world to escape my subjectivity why should I call it my transcendental self? Kant himself refused to do it when he maintained that the logical subject, or transcendental ego, was not a real. I might as well call it an objective subject, or an absolute relative. The contradiction is only thinly disguised in the common language of idealism. This theory finds no difficulty in an *absolute point of view* (or *Absolute's point of view*), as though anything absolute could be a point of view at all. To transcend my point of view, I am to employ a transcendent point of view. Since in knowledge I must escape subjectivity, I resort to a supreme subject. It is like defining riches as transcendental poverty, or satiety as transcendental hunger. Suppose an orifice through which light shines upon a wall: the disk is then due to the orifice. Remove the orifice, and the generally diffused light is due, according to the transcendental idealist, to a transcendental orifice.

But possibly we do this type of idealism an injustice through not advancing in its behalf the direct and positive argument for consciousness in consideration of its synthetic function. This argument is sufficiently obscure to make one fearful of stating it in behalf of another; but it seems to mean that truth is a gathering up, systematizing, or relating of terms, and that such is exclusively the property of thought. Now I may see the logical evidence for a connection without seeing any evidence for the dependence of that connection upon my seeing. My judgment does not attest its own indispensableness. Only a later judgment can so define my first judgment as a judgment at all. The judgment so discovered has, moreover, an individuality or numerical uniqueness that forbids the definition of its object in terms of it. Were the triangle constituted by the defining thought of it, there would be a triangle for every such judgment, but no such thing as a triangle. That truth is a synthetic activity of thought must be a psychological truth, *i. e.*, it has reference to my access to truth rather than to truth itself. It is biographically true that when I apprehend a law, or principle or definition, I comprehend a number of terms together and in relation. I reach the truth by combining, as, notably, in the case of my knowledge of similarity. But it would be folly to claim that therefore things are made similar by their combination in my experience. Things are not made similar by seeming similar. In seeming similarity there is doubtless a peculiar unity. Two similar seemings will not make a seeming similarity. But this has to do with the peculiar relational character of the psychical manifold, and not with the truth of similarity. It is true, of course, that a succession of feelings is not a feeling of succession, but this does not point at all to the dependence of the former upon the latter. It is the transcendentalist's favorite complaint against the empiricist that he confuses psychology with logic, but his own arguments for idealism turn upon this very confusion. His psychology of thought is an improvement upon the crude associational theory, but they are none the less psychology. And in the metaphysical use of his theory he identifies the object of knowledge with the knowing. He makes being out of the psychology of logic, and by a dialectic that is in this respect essentially indistinguishable from that of the sensationalist, he defines the real in terms of that activity, purposiveness, or category of objectivity which he regards as the most important factor of the knowing state. Indeed, he quite frankly acknowledges that metaphysics and psychology coincide in the conception of the self. There is space here for only a brief independent consideration of this conception, but sufficient to do justice to its serviceableness as a general ontological principle.

Self-consciousness is introduced to terminate the series of relativities defined by a perceptual idealism. If A be for B, B for C, and C for D, there must eventually be an M, such that while A, B, C and D are for M, M is for itself. The difficulty here centers in the proposition, 'A is for itself,' which for our critical purposes we may treat in a purely dialectical manner. If there be no difference between M and 'itself' there can be no relation between them except that of identity, M is M, which is the category of the thing. If M and 'itself' are not alike, then M_1 is for M_2, and the original perceptual series is prolonged interminably, or $M_1 - M_2$ must be regarded as a unique and organic relationship itself constitutive of a new thing N, which itself does not derive existence from relation to a mind. So we must either content ourselves with a world that is phenomenal and face the contradiction that is virtually contained in such a proposition, or consent sooner or later to regard the terminus of thought as a thing not constituted by that thought. And such a consent is in reality prior both temporally and logically to the conception of subjectivity. The error here is substantially the same as that which lies at the root of the other two transcendentalist arguments; the terms of psychology are misapplied to a totality of which by definition they signify only an abstracted aspect. The term consciousness has reference to relativity and exclusion within a world of reals, and therefore cannot signify a principle constitutive of that world itself.

It is the chief interest of faith that certain values shall survive and be consummated. If consciousness be either a specific and unique kind of thing, as certain so-called 'spiritualistic' philosophers would have us believe, or a general form of all being, it cannot be centrally important in such an issue. But if taken to signify selection within the realm of things, then, though it cannot be the ontological first principle, yet as the most general category defining a self it will apply either to psychology or the religious aspect of metaphysics. It must be admitted that error is an outstanding problem. But that circumstance is at least equally difficult for the subjective idealist. Grant him his absolute subject, and finite experiences with their relativity and exclusiveness are a totally new problem, which the general and innocuous pervasiveness of consciousness does nothing to solve.[1]

[1] The MSS. of this article was received on April 12, 1904.—Ed.

RETINAL LOCAL SIGNS.

BY DR. WALTER F. DEARBORN,
New York.

Retinal Local Signs.[1]

While the existence of some sort of local sign is generally regarded as almost self-evident, the hypothesis regarding their exact nature vary materially not only in their account of the constituent elements, but also in their estimation of the relative significance of these elements. Lotze, in his initial discussion presented, as is well known, three different hypotheses. Under all the various modifications, however, he conceived the local sign as a motor consciousness supplementary to the original retinal impression. His first two hypotheses are alike in their general nature in that each presupposes a physiological mechanism by which the stimulus from each point of the retina is transferred to the nerves of the orbital muscles. According to the first hypothesis the stimulation of each point through an 'interweaving' of the nerve fibers from the surface of the retina and the ocular motor nerves, causes an eye movement definite enough to bring the fovea immediately to the point of excitation. In the second hypothesis the retinal points are not supposed to have such an exact coefficient of movement, but with each point is associated only sufficient motor impulse to start a movement in the appropriate direction. Lotze regarded this latter hypothesis as the more probable. As a third hypothesis a theory of association is merely outlined and rejected without further elaboration.

Recent experiments have greatly increased the improbability of Lotze's second hypothesis, *i. e.*, of a continuous succession of motor impulses arising from a successive stimulation of the retinal points between the point of original excitation and the

[1] Read in part before the Section of Anthropology and Psychology of the New York Academy of Sciences.

fovea. It is now pretty well established that except for movements in the vertical and horizontal planes, the eye does not move in straight lines, and, in the second place, it has been shown[1] that during continuous eye movements from one point of regard to another there is no new effective stimulation of the retina which could occasion the successive motor impulses.[2]

The purpose of this article is to present some new experimental data as a contribution to the discussion of the first Lotzean hypothesis which still appears as a more or less important factor in practically all current accounts of retinal local signs. In a report of their experiments on the angle velocity of the eye, Dodge and Cline called attention to certain errors in the immediate fixation of eccentric visual stimuli. As these inaccuracies evidently concur the normal functioning of the motor impulse it seemed worth while to determine their extent and frequency.[3]

As a result of preliminary experiment the position of the stimulus to movement was arbitrarily fixed in the first series of measurements at forty degrees to the left of the primary fixation point. The stimulus itself was a bright point of light about eight tenths of a millimeter in diameter, made by exposing a ground-glass incandescent bulb behind a perforated screen. The observers *A* and *B* were respectively Professor Dodge and the writer. The ratio between the angular displacement of the eye and the displacement of the lines on the photographic negative was determined empirically. Since these measurements were concerned with errors occurring only in the vicinity of forty degrees to the left of the primary line of regard, it was necessary to determine the value on the negative of some unit of movement between thirty and forty degrees. Under the conditions maintained in the succeeding experiments the lines representing on the negative five-degree movement of the eye between 30°, 35°, 40° had the following values:

[1] Psych. Rev., Vol. III., pp. 454–465.

[2] *Ibid.*, Vol. VIII., pp. 145–157.

[3] The experiments were performed at the Psychological Laboratory of Wesleyan University and were undertaken at the suggestion and under the generous coöperation and supervision of Professor Dodge. The registering apparatus used was a modification of the Dodge-Cline photographic camera described in this Review, Vol. VIII., pp. 147–151, and in its recent form in the *American Journal of Physiology*, Vol. VIII., pp. 308–310.

For A the average of twelve five-degree movements was .54 mm., with a M.V. of .039 mm., *i. e.*,

$$1^\circ = .108 \text{ mm.}$$

For B the average of seven five-degree movements was .5 mm. with a M.V. of .06 mm., *i. e.*,

$$1^\circ = .10 \text{ mm.}$$

The following table, Table I., shows the frequency and extent of the corrective movements occurring in the movements of the right eye through forty degrees (*i. e.*, from approximately the primary position of the eye to a point of stimulation forty degrees to the left of the primary fixation point). They are in some cases positive, *i. e.*, the first movement of the eye fell short of the point of stimulation, making necessary a supplementary movement in the same direction, and in other cases they are negative. The positive movements are denoted by plus signs and the negative by minus signs on the right of each column.

TABLE I.

TABLES SHOWING THE EXTENT OF THE CORRECTIVE MOVEMENTS MADE BY THE EYE IN MOVING FROM THE PRIMARY FIXATION POINT TO A POINT OF STIMULATION 40° TO THE LEFT.

I. Corrective Movements of *A*.			II. Corrective Movements of *B*.		
1.	.20 mm.	+	1.	00 mm.	
2.	00 "		2.	00 "	
3.	.14 "	+	3.	.14 "	+
4.	.12 "	+	4.	.12 "	+
5.	00 "		5.	.19 "	+
6.	.13 "	—	6.	00 "	
7.	.10 "	—	7.	00 "	
8.	.27 "	+	8.	.13 "	+
9.	00 "		9.	.24 "	+
10.	.18 "	+	10.	.12 "	+
11.	.13 "	+	11.	.26 "	+
			12.	.22 "	+
Average,	.116 mm. (= 1° 42′)		Average	.118 mm. (= 1° 10′ 48″)	

Summary.[1]	No.	Per Cent. C. Mvts.	No. P. & N.
A	11.	72 $\frac{8}{11}$%	5 +, 3 —
B	12.	66 ⅔%	8 +

M.	M. V.
.116 mm. (= 1° 42′)	*A* .056 mm. (= 31′ 12″)
.118 " (= 1° 10′ 48″)	*B* .079 " (= 47′ 40″)

[1] No. = number of eye movements. Per cent. C. M vts. = percentage of corrective movements. No. P. & N. = the number of positive and negative movements. M. = The average or mean extent of corrective movments. M. V. = Mean variation in the extent of corrective movements.

One corrective movement is omitted from the above table of *B*. It is clearly an abnormal break and divides the whole movement into about two equal parts. Its extent is 1.98 mm., and thus represents an eye movement of about twenty degrees. If it entered into the computation, the average error for *B* would become .24 mm., *i. e.*, an average corrective movement of 2° 24′. Those instances in which the corrective movement is given in the table as zero must be interpreted in view of the limitations of the physical measurements of small differences, more or less exaggerated by certain pecularities of our photographic negatives. The cathetometer read only to .02 mm. making it impracticable to measure any movements of the eye half a degree or less. Moreover, with our present knowledge it is impossible to distinguish minute corrective movements from the slight eye movements found in all attempts to maintain fixation. And, finally, errors due to inaccuracies of definition on the negative which become evident only when the lines are magnified, lead to a probable error in all cases. While this would probably be evenly distributed positively and negatively in the cases actually measured, it makes it impossible to measure minute angles, and consequently renders the mean value that is given slightly too small.

In discussing further the above results, the question arose whether they might not in some measure be modified by short-lived motor habits which the eye seems to acquire with more or less facility. In order to determine the matter a second series of experiments was made in which the angle of movement was varied. In the process of refocusing the relative position of the camera and the source of light was somewhat changed, as was also the distance between the lens and the photographic plate. These changes necessitated a redetermination of the empirical ratio between the angular movement of the eyes and the displacement of the lines on the negative. The stimulus was in this second series exposed at one of three points — at 20°, 30° or 40° — instead of at only 40° (as in the first series). The ratio between the angular displacement of the eye and the lines on the negative was determined for the angle of 30°, 40°. In accord with the results thus obtained .106 mm. in the case of *A*, and .107 mm. in the case of *B*, is used as the equivalent of a movement of the eye of one degree at forty degrees from the primary fixation point.[1]

The results are given in Table II. Under *A* the measurements of the corrective movements of *A* are given in three columns, under *a* the 40°, under *b* the 30° and under *c* the 20° movements and similarly under *B* the measurements of *B* are given.

TABLE II.

A.

a (Stimulus at 40°).			*b* (Stimulus at 30°).			*c* (Stimulus at 20°).		
1.	.16 mm.	+	1.	.12 mm.	—	1.	.05 mm.	+
2.	.16 "	+	2.	00 "		2.	.30 "	+
3.	.19 "	+	3.	00 "		3.	00 "	
4.	.22 "	+	4.	.14 "	—	4.	.28 "	—
5.	.20 "	+	5.	00 "		5.	.14 "	+
6.	.22 "	+	6.	00 "				
Average .191 mm. = 1° 48″.			Average .043 mm.			Average .154 mm.		

B.

a (Stimulus at 40°).			*b* (Stimulus at 30°).			*c* (Stimulus at 20°).		
1.	.29 mm.	+	1.	.23 mm.	+	1.	00 mm.	+
2.	.34 "	+	2.	.08 "	—	(a)2.	.07 "	+
3.	.36 "	+	3.[1]	.21 "	+		.18 "	+
4.	.28 "	+		.10 "	+	3.	.14 "	+
5.	.36 "	+	4.	.13 "	+	(a)4.	.08 "	—
6.	.29 "	+	5.	.18 "	+		.10 "	+
			6.	.24 "	+	5.	.09 "	+
			7.	00 "				
			8.[2]	.14 "	+			
				.12 "	—			
Av. .32 mm. = 2°59′24″.			Av. .14 mm.			Av. .134 mm.		

Disregarding the error of movement when the stimulus was 20° and 30° from the primary fixation point, a summary of the 40° movements is given in Table III.

[1] The mean variations for both these units of measurements was (M.V.) .009 mm.

[2] In computing the total error of the double corrections, I have added their absolute values and determined the variation of this sum from the mean rather than the variations of the single corrections from the mean.

[3] For abbreviations see note on page 299.

TABLE III.

Subject.	No.	Per cent. C.	Mts.	M.	M.V.	No. of + or −.
A	6	100%	.191 mm.	(=1°48″)	.023 mm.	6+
B	6	100%	.32 "	(=2°59′24″)	.036 "	6+

These tables show, as was expected, a somewhat increased inaccuracy in fixation in the forty-degree movements and tend to substantiate the hypothesis that the earlier results were modified by the successive eye movements of the same angular displacement. At the same time they introduce some new factors into the problem. The unexpected accuracy of fixation in the thirty-degree movements — especially those of *A* in comparison with the 20° movements presents a phenomenon whose explanation is impossible without further investigation. Secondly, double corrections, *i. e.*, two separate corrections in one movement, appear twice among the twenty-degree movements, and twice among the thirty-degree movements. In two of these cases both movements are positive, but in the other two the correction is made by one positive and one negative movement. This is interesting as showing the great variability of eye movements.

In order to compare the inaccuracies of motor innervation with the threshold value of local discrimination, it was necessary to measure the latter for both subjects as exactly as possible. For the sake of more general comparison both the usual method of the discrimination of two points, and a second method, which will be described later, were used. The two points of light employed according to the first method were about 8/10 mm. in diameter, 60 cm. from the axis of rotation of the eye, and at a maximum of 40° to the left of the primary line of regard. They were exposed either simultaneously or but one at a time, and the subject was asked to state at each exposure whether he saw one or two points. The results were in brief that in a series of ten exposures when the points were 5 mm. (or 28′35″)[1] apart, they appeared as one to both observers. When 71/4 mm. or 41′ 27″ apart in a similar series of ten they could be distinguished

[1] As the distance from the axis of rotation of the eye to the point of fixation was 60 cm., the circumference of the circle of which this is a radius was 3771.12 mm. This gives 10.47 mm. as the equivalent of 1° on the circumference of the perimeter, or 1 mm. = — 5′43″.

as two by A but not by B. When 10 mm. or 57′ 10″ apart they were distinguishable by B. In some cases, before the threshold was reached, the subject reported that the single point of light seemed to appear broader than at other times. These cases of course, show the proximity of the threshold.

The thresholds therefore lie between 28′ 35″ and 41′ 27″ for one observer (A) and 41′ 27″ and 57′ 10″ for the other observer (B). These numerical limits might have been more exactly defined but the results of the second series of experiments seemed to make this unnecessary. The latter series depends upon what appears to the writer to be a more satisfactory method of measuring the fineness of retinal discrimination. Stern[1] determined the width of a just perceptible black line dividing an otherwise continuous whole surface, and regarded the result thus obtained 15″ as a measure of the threshold of space discrimination. But it seems very doubtful whether this is a measure of spacial discrimination at all; it seems rather to be a measure of the least intensity or smallest extent of the stimulus necessary to produce a sensation of blackness, and would doubtless vary for different colors. Gilbert[2] in repeating the experiments under somewhat modified conditions, reduced Stern's measurements to 2.5″. Stratton[3] has measured the threshold by means of exposing motionless points of light in immediate succession one above the other. The disturbing effect of irradiation was thus largely eliminated. His results for the angle of 30° are as follows:

Subject.	Angle.	No. of Cases.	Length of Arc Discriminated.	M. V.
A.	30°	5	29′	5.2′
Bd.	30°	3	18.3′	2.2′
P.	30°	4	63.7′	8.7′

In my experiments, a continuously moving point of light was employed and the smallest extent of movement which gave a definite clue to its direction indicated the threshold value. It has been considered by Stratton a possible objection to his experiments that although the points were actually motionless and only exposed in succession they produced the appearance of

[1] *Zeitsch. f. Psych. u. Phys. d. Sinnes.*, VII., 321.

[2] *Psych. Rev.*, IX., 435.

[3] *Ibid.*, IX., 436.

continuous motion. Since there was, actually, however, no objective motion and the effect was due purely to suggestion, he considered that the subjective illusion might be disregarded. The real justification of the method, however, seems to me not the fact that there was no motion involved, but rather that some form of local discrimination was distinctly included in the process. The important consideration is not the absence of movement, but the recognition of a definite change in position. Stratton has shown that there is no reason for considering these two processes, *i. e.*, the perception of movement and the discrimination of position, independent.[1] For our purpose, however, it is sufficient to point out that, whether the processes are dependent or independent, if observation is always made not only of the fact of movement but of the direction of movement, some element of local discrimination must be involved.

The stimulus used in these experiments with a moving point was, as before, the light of an incandescent lamp with ground-glass bulb which was exposed through a small aperture (8/10 mm. in diameter) cut in a moving slide. The extent of the movements of this point of light was limited by transverse slits, which were one, two, and three millimeters in breadth. By means of these a movement of the point of light of from one to three millimeters in length could be made at the option of the

[1] His conclusion, that the perception of motion is simply a perception of the fact that 'a sensation is changing its space relations,' has however hardly been established. The results clearly contradict the evidence upon which Exner's theory is based and leaves that the more improbable theory. It is recognized, however, that in all such experiments sensations of movement occur without any perception of direction. In the present experiments this was the case in several instances despite the fact that the observer was attempting to discover any clue that he might of the direction of the given movement. Observations of a similar character were made by Stanley Hall and Donaldson with relation to tactual discrimination (*Mind*, 10, p. 571): 'there is no doubt whatever that a distinct sensation of motion occurs without giving any impression of direction in many cases.' This may be due simply to a lapse in attention, or a return to the more habitual reactions, that is, as in the case of these experiments, we are much more accustomed to observe small movements without consciously noting their direction, or it may be due to other causes which may easily be suggested. But it has not been shown that any of these more evident explanations will cover all the facts of the case. The processes involved are evidently complex, and need further investigation and analysis before the relation of the sensations of motion to those of direction may be established.

operator. The contrivance also admitted of the movements being made in either the vertical or horizontal planes. It was found by this method that at forty degrees to the left of the primary fixation point the discrimination of the direction of a single point of light in motion is from seven to ten times greater than the discrimination of two discrete points.

The length of the first movement experimented with was 2 mm. (11′ 26″). The exposures were made in three series, each of eight to twelve exposures; in the first the subject was told that all movements would be in the horizontal plane, in the second, that all movements would be in the vertical plane, and in the third, in either the horizontal or vertical planes. The direction of all the movements in the first series was correctly perceived, in the second series three vertical movements were reported to be horizontal by *A* and one by *B*. But, even when the movement was recognized as being in the vertical plane, both subjects questioned in several cases the accuracy of their further judgment of the direction, *i. e.*, whether up or down. In the third series, all the horizontal movements were again correctly given by both subjects, but the vertical movements were reported horizontal by *A*, and in the case of *B* two out of four vertical exposures appeared as horizontal.

When the movement was decreased to 1 mm. (5′ 43″), the horizontal movements were still reported correctly, but the vertical were variously stated as movements from right to left or left to right, as oblique — *e. g.*, from upper right to lower left — although the subject knew that the apparatus in use did not admit of such movements being made, as a mere appearance of light without motion and as a movement whose direction was not perceived. Small changes in the velocity of the movement also affected the ability to perceive the vertical movements. As these movements did not, however, primarily concern this investigation, no attempt was made to determine the most favorable rate of movement. The appearance of these factors is evidence that the threshold for vertical movements has been reached more quickly than the threshold for the horizontal plane. The latter is obviously below 1 mm. (5′ 43″), but the form of apparatus did not admit of smaller movements in this plane.

Several peculiarities were also noted which were somewhat characteristic of these movements in either plane.

First, the movements, even when correctly interpreted, often seemed to both observers like the passing of some opaque object behind the aperture. Secondly, the movements appeared to be larger than they really were, and finally, *B* especially noted occasionally an act of judgment in which the direction of the movement seemed to be inferred from changes in the size of the stimulus as it came into view and disappeared. Obviously, however, all of these peculiarities must depend on some real differences in local coloring.

A similar series of experiments was made with a different form of stimulus. Instead of a point of light a piece of white carboard, 4 cm. square was used. It could be moved as the former stimulus with exactness in either the vertical or horizontal planes. With this method there were no marked peculiarities evident, such as have just been discussed, the former upper limit of value of local discrimination was somewhat reduced in both the horizontal and the vertical movements. Both observers perceived the direction of all movements over 0.9 mm. in extent. As this was the smallest movement experimented with, the threshold value must be even lower.

This refinement of special discrimination (5′ 8″) has been approached only by the questionable method of Stern. The results of Stratton, as stated above, show 18.3′ as the lowest perceptible area of movement at ten degrees nearer the primary point of regard that in our experiments.

The noticeable disparity between the results of our two methods, *i. e.*, as between the methods in which the threshold is determined with the aid of movement and that in which motionless points are used, does not admit of ready explanation. It might seem quite plausible *à priori* that a high degree of delicacy in the discrimination of two points might be brought about by means of repeated observations of changes in position. Undoubtedly the irradiation of light and the lack of sharp definition must be taken into account, but just how far this operates to lessen the discrimination of adjacent points is undetermined.[1]

[1] The similarly increased discriminative sensibility in the case of motion on the skin was first noticed, I believe, by Stanley Hall and Donaldson, *Mind*, X., 563.

A summary and comparison of the different parts of this investigation leads to the following conclusions. It has been shown in the first place that the mean extent of corrective movements of the eye in fixating a stimulus forty degrees from the primary fixation point was, when no effort was made to counteract the effect of short-lived motor habits, for *A* 1° 42′ and for *B* 1° 11′; when the attempt was made to prevent the formation of such habits, the mean extent of corrective movements was larger, for *A* 1° 48′, and for *B* 2° 59′. In the second place, it was found that the threshold value of local discrimination as determined by method I. was for *A* between 28′ 35″ and 41′ 27″, and for *B* between 41′ 27″ and 57′ 10″; as determined by method II., 5′ 43″ or less for each observer, and by method III., 5′ 8″ or less.

Even under the most unfavorable circumstances therefore (Method I.) the local discrimination at 40° from the fovea in the horizontal plane is much finer than the adjustment of motor impulses as shown by the mean extent of corrective movements made in fixating a point at the same distance (40°) from the primary point of regard. That the delicate local differences shown by methods II. and III. could result from such grossly inaccurate motor impulses seems out of the question.

These discrepancies between the accuracy of the motor impulse and the delicacy of local discrimination necessitate, as I believe, some modification of the traditional view in regard to the nature, or at least in regard to the relative importance of the motor factor. Moreover, the variability of the latter, as shown by the presence of corrective movements of varying magnitudes, is altogether in contrast with the relative constancy of special discrimination.[1]

[1] The Mss. of this article was received on March 4, 1904. — Ed.

STUDIES FROM THE CALIFORNIA PSYCHOLOGICAL LABORATORY.

VI. Some Peculiarities of Fluctuating and of Inaudible Sounds.

BY KNIGHT DUNLAP, Ph.D.

I. The Effect of Physical Interruptions in Subliminal Phases.

Eckener[1] states that 'subjective' fluctuations in a minimal sound may be readily distinguished from 'objective' interruptions, and in particular, that in phases where the sound is inaudible owing to the 'subjective' fluctuation of attention, an 'objective' or actual physical interruption may be perceived if it occurs. To test this phenomenon, I conducted some experiments in the spring of 1899, in the course of which some interesting developments appeared.

The sound employed in these experiments was that emitted by a telephone receiver in circuit with the secondary element of a DuBois-Reymond induction coil, the primary current of which was supplied by two gravity cells and interrupted by an electric diapason of one hundred double vibrations. The telephone receiver was placed close to the left ear of the subject, who sat in a 'silent room' from which external noises were excluded by padded double doors and specially constructed walls. By means of a noiseless key controlling an electric circuit the subject operated an indicator in the experimenter's room; either a sounder or a kymograph marker, as the case might be; and by a prearranged code of signals was thus able to give his reports. By means of a key the experimenter could at any time break the secondary circuit, and thus silence the telephone receiver without in any way interfering with the

[1] 'Untersuchungen über die Schwankungen der Auffassung minimaler Sinnesreize,' *Philos. Studien*, Bd. VIII., S. 365.

primary circuit. The diapason, induction coil, kymograph, and all accessory apparatus, were in the experimenter's room.

In the first set of experiments the subject's key was connected with a sounder, and he signalled the instants of appearence and disappearance of the telephone snarl by appropriate clicks. By varying the position of the secondary coil an intensity was found at which the fluctuations occurred well, and then in certain of the intervals indicated by the subject as 'empty,' (*i. e.*, in which the sound was inaudible), the sound was physically interrupted for a brief period. If the subject perceived this second death of the sound, he indicated it by a special signal.

Table A gives the numerical aggregates of the results obtained in this way. The first column of figures gives the total number of cases for each subject in which the sound was physically interrupted in an apparently 'empty' interval; the second column gives the total number of cases in which the subject indicated such interruption as having been perceived; and the third column gives the number of cases in which each subject indicated a physical interruption when really no such interruption had occurred. The third column is then a record of errors.

TABLE A.

PHYSICAL INTERRUPTIONS AFTER PSYCHOLOGICAL DISAPPEARANCE OF SOUND.

Subject.	No. Made.	No. Perceived.	No. Imagined.
S.	23	10	1
R.	9	3	0
G.	47	24	0
W.	25	17	3
A.	10	4	1
Bi.	75	34	10
Bo.	17	6	0

In the second set of experiments the conditions were the same as in the first set, except that the subject's key was connected with a stylus writing on the drum of a kymograph, and the operator's key for breaking the telephone circuit was connected with a second stylus writing directly over the first one. Thus the actual course of the experiment was recorded for leisurely examination. The results of this set are given in Table B.

TABLE B.

INTERRUPTIONS AS IN TABLE A.

Subject.	No. Made.	No. Perceived.	No. Imagined.
S.	55	32	7
R.	16	9	0
A.	69	5	0
C.	123	109	27
Bi.	27	10	3
G.	29	18	0

The length of the physical interruption was about 1½ sec., while the length of the average 'empty' interval was over four sec., so the apparent perception of the physical interruption was not due to a failure of the sound to reappear at the expected time. Such expectation, again, could hardly operate, owing to the great irregularity in the period of fluctuation; for so it would have produced a large percentage of reports of interruptions when none occurred.

That the perception of the interruption was genuine in almost every case is evidenced by the small number of false reports, in conjunction with the fact that in the majority of empty intervals no physical interuptions occurred, thus rendering large the opportunities for errors of imagination. Some of the errors were undoubtedly correct judgments delayed in the registration on account of hesitation of the subject to accept his experience as real. This delay occurred in several cases concerning which the subjects were questioned, but usually there was no opportunity of making inquiries until after the exact occurrence had passed from the subject's recollection. The recorded errors in almost every case occurred in empty intervals in which a physical interruption had been made a little earlier and not reported, but in obtaining the data of Table B. an arbitrary rule was adopted, according to which no report of an interruption was accepted unless recorded on the drum within one millimeter (three fifths of a second) after the break in the current. The majority of the accepted reports were however recorded within one half of a millimeter after the break. It should be noticed in regard to subject C. in Table B, that almost all of his errors were made in the early part of the work, and few after he became accustomed to the conditions of the experiment.

Three of the subjects made rather definite analysis of the conditions attending the perception of the second death of the sound. R. was so surprised upon receiving the interruption for the first time (having previously been given no information as to the nature of the phenomenon sought), that she stopped the experiment to describe it, and even after she had become familiar with it could not repress a feeling of astonishment at each occurrence. It finally became clear to her, however, that while just before the physical interruption she was sure that she was not hearing the sound, as soon as the interruption occurred she felt from her new point of view that she had been hearing it all the time, but had only now realized it.

Quite different was the report of C., who was just as sure after the interruption that he was not hearing the sound as he had been before. The occurrence was not for him the cessation of a sound, for that had absolutely disappeared previously. It was rather the 'bottom dropping out' of some indefinite element in consciousness; to be compared to nothing except the feeling attending the downward start in a rapid elevator. The experience of S. was similar, but of different characteristic. For him the occurrence was largely a feeling of relief, as though the sound had been absent previously, but that now he felt sure there was no further use in looking for it. Sometimes, however, his experience would incline towards that of R., and he would feel that he had really heard the sound all the time.[1]

This experiment shows conclusively that a minimal sound which has become imperceptible through the so-called fluctuations of attention, may yet be 'heard to stop.' But a yet more striking phenomenon was observed by subjects S., R., A., Bi. and C. The sound was in this case made so weak that it was not perceived at all, and was then physically interrupted at irregular intervals; the result being that in a large number of cases the interruptions would be correctly reported by the subject, who however, in the typical cases did not perceive anything corresponding to the commencement of the sound at all.

[1] See Stratton, *Experimental Psychology*, pp. 90-91.

That is, nothing was perceived except the stoppage of the imperceptible note. Two of the records of single tests on this part of the experiment are given in Figs. 1 and 2, in which the depressed portions of the upper line indicates periods of subliminal stimulation, and the elevated portions indicate no stimulation; while perceptions of any thing connected with the sound are indicated by breaks in the lower line, exclusive of the first

FIG. 1.

break, which was made with the drum at rest to indicate the relative positions of the two styli.

The relatively greater perceptibility of the stoppage, as compared with the commencement, of the note, can not be due to any instrumental peculiarity, since we are dealing here with an alternating current of 100 alternations per second, in consequence of which the interruptions by the key, which did not disturb the primary current, would produce no special effects upon the current at the moment of interruption. Moreover, a

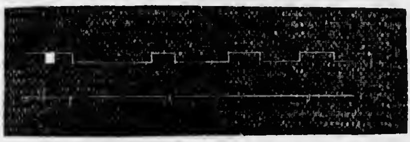


FIG. 2.

similar relation was observed in using distinctly perceptible sounds, in the following branch of the experiment.

Series were taken by commencing with a distinctly audible intensity of the telephone snarl, and decreasing the intensity slightly between each test and the following one, until it passed completely below the threshold. During each test, which lasted about twenty-five seconds, as in previous work, the sound was interrupted, sometimes regularly with three-second intervals, sometimes irregularly, the intervals in the latter case averaging in the long run about three seconds. The subject indicated these stoppages and commencements as accurately as possible, by depressing his key at each beginning, and releasing it at each ending, his reaction being recorded on the drum along with the physical series.

After determining by careful tests that the latency of the magnet markers was not measurably different for the two movements, *i. e.*, raising and lowering, careful measurements were made of the distances between each stoppage or commencement of the sound, and the registration of the same by the subject. The delay at the beginning was then subtracted from the delay

Fig. 3.
REGULAR INTERRUPTIONS

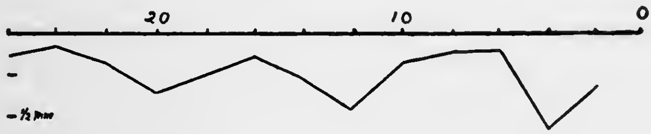
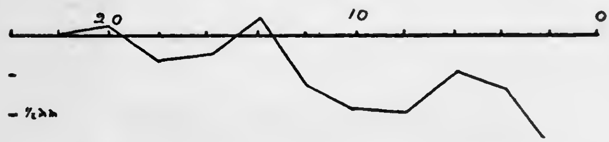


Fig. 4
IRREGULAR INTERRUPTIONS



at the ending, and the results for each interruption in a given test averaged together. Averages for corresponding tests from the other series for the same subject, reckoning backwards from the point at which the sound passed below the threshold, were then averaged together.

We have then for each subject, a series of numbers, positive or negative, or both, indicating the average excess in the delay in recording the ending of the sound over the delay in recording the beginning, through various intensities from perfect distinctness down to complete disappearance. Such a series we can represent by a graph, letting the ordinates represent the values of the successive numbers in the series, and the abscissas the points on the scale of diminishing intensities to which these numbers correspond. It is evident that a negative ordinate, or a negative value in the series of numbers represented by it, signifies that at that point the stoppage was recorded more quickly than the commencement (cf. figs. 3, 4).

Figs. 3 and 4 show graphs derived from the results of subject S., which are characteristic of the results for all. It is clearly evident from these that the stoppages are in general reported more quickly than the beginnings, and that the difference in this respect increases as the intensity of the sound diminishes.

Since there is no reason for supposing that the reaction time *per se* differs materially in the two cases, and since there is no known aural condition which could account for this peculiarity, the explanation is most probably to be referred to the general conscious conditions governing the superior discriminative advantages of a position in silence harking back to sound over a position in sound harking back to silence. The fact that contrast does play a large part in the perception of the weak sound is further shown by the fact that in all cases a continuous sound was found to be inaudible at a much higher intensity than an intermittent sound under the same general conditions.

The cases of the second death of a sound referred to above are most probably not different in character from the phenomenon just considered. A steady sound, inaudible continuously, or intermittently through the 'fluctuation of attention,' becomes perceptible at the instant of physical stoppage because of the sharp transition from feeble sound to silence. The introspection of the subjects referred to above favors this explanation decidedly. Of course the neural conditions of the phenomenon are yet to seek, but the psychological facts may be settled in advance; in fact they must be, before the neural facts may be safely inferred.

II. The Fluctuation of Diapason and Gas Flame Tones.

W. Heinrich[1] came to a remarkable conclusion that *pure* tones (*i. e.*, tones unmixed with noise) do not fluctuate, but are

[1] 'Zur Erklärung der Intensitätsschwankungen eben-merklicher optischer und akustischer Eindrücke,' *Bull. Internat. de l'Acad. des Sci. de Cracovie*, Nov., 1898, 365–381; also, 'De la constance de perception des tons purs a la limite d'audibilité,' *ibid.*, Jan., 1900, 37–45.

either heard continuously or not at all. This conclusion was based principally on observation of the tone produced by the singing gas flame. Titchener[1] repeated Henrich's observations briefly, and found that his results were apparently confirmed. H. O. Cook,[2] in Titchener's laboratory, had previously found that tones of diapasons show the ordinary fluctuations quite clearly.

The important bearing of these conclusions upon the whole matter of the fluctuation of attention, so called, as well as upon the special features of my own work, led me to investigate the behavior of both diapason and gas-flame notes under the most favorable conditions. This I did in the fall of 1903.

The subject was placed in the silent room mentioned above. A lead pipe has one termination within a recess in this silent room, and the recess is provided with a door by which it can be noiselessly closed, either partly or completely. From the silent room the pipe extends to an ordinary research room on the other side of the building. In this second room was installed the diapason, or other source of sound waves. The intensity of the sound in the silent room was regulated by varying the distance of the source from the one termination of the pipe, or by varying the position of the door over the other termination in the silent room, or by both. The subject, seated at a fixed distance from the orifice, indicated by a prearranged code of signals as before, the periods in which he heard the sound and the periods in which he heard nothing.

An electrically driven diapason of 500 d. v. was first used. The resulting note in the silent room, even when made very loud, was not only surprisingly pure, but was practically simple. The noise of the electric contact, as well as the overtones, seemed to be practically eliminated by the transmission through the long pipe.

The results on four subjects confirmed those obtained by previous investigators; the fluctuations clearly occurred. No experiments on the second death of the tone were made, because of the difficulty of bringing the note to an abrupt termination.

[1] 'Fluctuations of Attention to Musical Tones,' *Am. Jour. Psy.*, XII., 595.
[2] *Ibid.*, XI., 119–123.

Next, the tone of the singing gas flame was employed in the same way. The results were in direct opposition to those obtained by Heinrich and by Titchener, for under favorable conditions the fluctuations were unmistakably observed by each of the five subjects employed. Since the note could be abruptly terminated by bringing a card over the upper extremity of the tube, it was possible to investigate the second death in this case. This phenomenon was clearly observed by at least three subjects, and interruptions made in a subliminal tone were also clearly observable.

I said that the fluctuations occurred when the conditions were favorable. These conditions were somewhat difficult to secure, and demand first, that the tone shall be rather *pure*, second that it shall be *relatively simple*, and third, that it shall have a *fairly constant pitch*.

Let us consider the first condition. Heinrich claims that that gas flame tone is normally pure; much purer than the diapason tone. He argues that the latter is impure from the supposed fact that it will not set up standing waves in a room, while the gas flame tone will. Now the fact of the case is that the diapason will set up standing waves, as can be readily demonstrated in a room not too large. The nodes nearest to the wall may be easily observed, especially if the diapason be insulated from the table and other resonant material so that there may not be waves emanating from points widely separated. The only difference between the diapason note and gas flame note is due to the relative weakness of the former and its relative simplicity, which makes the nodes complete instead of partial as in the case of the highly complex gas flame note.

The diapason note, when the noise of the electric contact (provided one is used) is eliminated, is really very pure. The fourth is the only partial which is strong in comparison with the first and is so weak as to be indistinguishable when the intensity of the note is reduced to moderate intensities; being absolutely neglectable in the case of minimal tones. There is therefore nothing in the tone under these conditions which could generate 'noise.' Nor can it be reasonably claimed that the pipe transmission employed in my own experiment generated 'noise.' It might possibly have modified the pitch, and that is an inconsequential possibility. The only observable effect was a relative simplification of complex tones due to the disappearance of weaker partials, and a complete elimination of faint noises.

That the gas flame is the source of a very impure tone is readily observed. With certain pressures of the gas supply beats of great strength and as slow (with a four-foot tube) as one per second, may be obtained. Or, they may be increased in rapidity beyond the point of counting. In most cases there are several systems of beats in operation at once, and it is practically impossible to obtain by any adjustment a tone which is entirely free from the roughness (*i. e.*, noisiness), of rapid beats. But with certain adjustments this roughness was slight, and being further reduced by transmission through the pipe, did not prevent the fluctuation. The production of the beats is probably due to the great strength of the upper partials; which leads us to the second condition.

By *relative simplicity* is meant the condition that one note in the complex shall predominate considerably in intensity over the other partials. With a two-foot tube it was almost impossible to get an adjustment of gas and tube such that a note of about three-foot wave-length and one of nine inch length were not about equally strong. When reduced to extreme faintness this compound was heard continuously, if at all; but when made strong enough so that the pitch was perceptible, the note heard alternated between the two, showing a selective fluctuation of attention which would prevent the disappearance of the note completely.

As regards *constancy of pitch*, the gas flame tone is also apt to be deficient at times, as may be noticed by observing the variability in the number of beats which it makes with a steady tone from some other source. This inconstancy is at other times so small as to be neglectable, but there is always a possibility of its occurrence. It is perhaps due to variations in the temperature of the air column in the tube.

In addition to these factors which tend to prevent fluctuation, there is another which is not peculiar to the gas flame experiment, but yet seems to be especially strong therein. This is the persistence of after-images of the sound, which tends to bridge over gaps which would otherwise be caused by the psychological disappearance. At times, when the tone was shut off without abruptness, the subject would continue to hear it for a minute and a half after its physical disappearance. Why there should be more difficulty from this cause with gas flame tones than with other tones, I do not know; but such seems to be the case, so far as my observation goes.

Such inference regarding the influence of 'purity of tone' as can legitimately be made from my own and others' experiments, is that the advantage as regards case of fluctuation is on the side of the purer tone; and on the side of the simpler tone as against certain types of complex tones, as regards disappearance at least. The fluctuations of a certain type of complex tone of moderate strength may however be more marked than those of a simple tone, if we consider the *selective fluctuation* mentioned above.

The apparent contradiction of the first of these inferences by the easily obtainable fluctuation of such 'noisy' tones as the telephone snarl and watch ticks is not serious. The telephone snarl is highly complex, but not very 'noisy' after all, especially when reduced to its lowest terms; and the complex is not capable of ready resolution by selective attention. The ticks of a watch, on the other hand, vary physically so that they cannot be used as a criterion of any sort.

Noisiness, as distinguished from mere complexity of tone, can be considered as nothing else than *beat quality; i. e.*, shock, or jar, which is the characteristic thing about beats in an otherwise smooth tone. The function of beats in preventing fluctuation is closely connected with the 'second death' of the minimal tone. The sharp contrast between the minimal sound and the succeeding silence is perceptible although the sound was imperceptible up to that moment. Such contrasts occurring with sufficient frequency may prevent the pulsations of the thus marked off sound from ever clearly disappearing unless the intensity be weakened below the point at which the contrast is perceptible; in which case no sound will be heard at any time.[1]

[1] The MSS. of this article was received on April 2, 1904.—ED.

SOME OBSERVATIONS ON VISUAL IMAGERY.

BY DR. H. B. ALEXANDER.

With all that has been written on the subject of mental imagery, it yet seems certain that there is much to be done and said before we are to have a serviceable clearing up of the phenomena. It is possible that we have learned all that we may by the usual 'breakfast table' questionary, or like methods [1] relying upon uncritical introspection. There is too much danger of suggestion and fable and the trickery of language for the results to be perfectly reliable. When it is possible for one deaf and blind from childhood to use the language of the lost senses with the grace and truth shown by Miss Keller, more than ever must doubt be cast upon ordinary wordings as description of psychical events. The tact and deftness of such descriptions as 'white darkness' applied to dense fog, 'luminous shadows' to a river's reflections of trees, the most consummate literary skill well might envy and only an unusual visual sensitiveness would seem likely to suggest.[2] But while the need for caution is thus stressed, the desirability of careful observation is not at all gainsaid. Rather, now that the general characteristics of imagery are fairly determined, it is the more urgently called for; theories as to imagery cannot be on sure ground until we have fuller detail, and the detail is only to be acquired from trained introspection.

The nicety of discrimination demanded for just observation

[1] Such as that of M. Ribot: "J'ai toujours procédé de la même manière, en disant au sujet: 'Je vais prononcer plusieurs mots; je vous prie de me dire immédiatement et sans réflexion, si ce mot n'évoque rien dans votre esprit, ou s'il évoque quelque chose et quoi?' La réponse était notée aussitôt; si elle tardait plus de cinq à sept secondes, elle était considérée comme nulle ou douteuse."—*L'Évolution des idées générales*, p. 131. The present writer conducted in 1896 a series of similar experiments interestingly corroborative of M. Ribot's results.

[2] See *The Story of My Life*, by Helen Keller, pp. 21 and 39 (New York, 1903).

can only be known in the attempt, and at the best the results are far from indubitable; the mind too inevitably permutes and falsifies its own states, too constantly alters its phenomena under the influence of interest. I emphasize this merely that I may not seem to invite disregard of the personal equation nor to fail to reckon in the idiosyncratic distortion unavoidable to the study I undertake. This study is based upon memoranda covering a period of several years, noting the experiences as they occurred, and so having the advantage of representing psychical happenings induced in the mind's normal course, usually with no thought of special observation. I might add that the personalism necessary to the discussion appears to be permissible only in view of the general significance which I believe the facts to have. These facts are in themselves of an essentially tame and usual type,—which should constitute their chief value.

I. General Analysis.

I shall begin with a general analysis of grades and types of visual imagery. With reference to vividness, three grades or intensities are to be discriminated.

1. There are the fleeting images of common thinking. They are vague, fragile and ephemeral, only by chance to be observed, the moment they attract attention transforming themselves to grade 2. They may be pictures of things or typographical images.[1]

2. Whenever any image is fixed in attention, it becomes distinct in outline and seemingly filled in with substance. The liminal quality in this grade is of a tenuous, 'watery' sort, but if the thing imaged is naturally of distinctive color, the color is

[1] M. Ribot (*op. cit.*) recognized as a peculiar variety of his 'type visuel typographique' the imagery of compositors, finding his differentiæ in the facts that the compositors he examined (1) saw the words given in a particular typography, sometimes named, and (2) for semi-concrete words had accompanying picture images, for abstract words no images. I myself worked some years at the case, and I have many typographical images. But such images are certainly not traceable to the types, which seems to be M. Ribot's conception. The compositor always reads the letters *upside down*, and in handling the type depends quite as much on touch as on sight. As for the instances where the type face was named, I, as a typesetter, might very well describe my typographical images as in 'ten-point roman,' but such description would mean no more than technical familiarity with type sizes.

sure to appear. Thus *dog* calls forth only a colorless (gray) figure of a dog, but *Spot* summons the image of a particular, black and white, shaggy dog. The color element varies from the black and gray (pure white is difficult) of typographical images to a versimilitude hard to differentiate from reality. With the exception of the typographical, the images all appear small, *i. e.*, as miniature copies of the things they represent. Furthermore, they are commonly isolated; there is no background of non-related objects. A landscape may be presented, but it is seldom full or distinct unless enlarged to grade 3.

3. In the third grade images are richer in detail; there is illumination as of a stronger light thrown on, though the color scheme *qua* color is not altered; there is added substantiality and a filling in of background; and the images appear to be of the actual or approximate size of things as viewed from a normal standpoint.

It will be seen that the three grades pass perceptibly into one another. In general, the variation in distinctness of outline or figure is minimal; variation in definiteness of content (as smooth to shaggy) is greater; variation in substantiality is still more pronounced, and variation in color, luminosity and apparent size is greatest of all. In imagery subject to volitional retention the vividness is directly proportional to the attention bestowed. Of the imagery as a whole, the conspicuous characteristic is fragmentariness; seldom save by effort is a whole object seen, and within the given content there is usually one portion more striking, distinct and detailed than the remainder.

In addition to the three grades, there are to be distinguished two types or classes into which the images fall. These are:

α. Voluntary or memory images; all images that may be called to mind or retained by an act of will. Memory images, in the simplest sense, afford the typical instance, but I include along with simple reproductions all images consciously constructed from remembered elements, as, for example, a geometric figure, a landscape ideally composed in accordance with the elements furnished by a description, or a mechanical device illustrated in imagination.

β. Spontaneous and irrelevant images, the salient characteristic of which is that they seem to determine their own occurrence, coming and going of their own accord. Of course these images can be retained or reproduced in memory, but the retention or reproduction involves a change of quality: it removes that asset of surprise and perversity which gives so much of their forcefulness, and usually it projects them into new associational environments and new spatial contexts.

By 'spontaneous *and* irrelevant' I imply a partial disjunction. Spontaneity characterizes the whole group; irrelevance is a usual but not invariable feature. By irrelevance I do not mean necessary absence of suggestion, but its lack of rationality. Indeed, if the province were dominantly ratiocinative 'irrationality' would be the better word. To instance: If in reading, say, the description of Cleopatra journeying to Cydnus, I imaginatively build up the picture, though with all the royalty of Shakespearian imagery, the act differs not greatly in kind from an act of memory, for I can use only materials selected from the memory store.[1] But if, as I read, in place of Cleopatra

> O'erpicturing that Venus where we see
> The fancy outwork nature, —

I meet here, suddenly, exasperatingly, a grotesque negroid face, this I call an irrelevant image; it is a perverse and irrational intrusion, insulting alike to Shakespeare's lines and to the intelligence before which it presents itself. *A posteriori* there is traceable some associational suggestion,— Cleopatra being an African; but taken as a type, it is very like a prank of secondary personality. Indeed such images argue two planes of intelligence in the apprehension of meanings: there is, first, the plane of constructive coördination, or rational synthesis; there is, second, the plane of uncritical association, with an isolative intelligence incapable of just or proportionate assembling of imagery. Frequently, however, images of the β type show no discoverable connection with the conscious context; spontaneity and temporal independence are their sole differentiæ.

[1] I purposely discount the imaginative fervor which really gives such construction more vitality than is common to memory.

II. Projection and Size.

A satisfactory criterion of the externalization of images is difficult to obtain. In my own experience, all visual images appear in a field of vision, therefore, in that sense, as external; they never seem to be 'in the head.' They are not, however, all external in like sense; not all appear in the space which I sensibly perceive or am conscious of as real; there seems to be truth in the notion of a 'mental eye,' and it seems to perceive in space-relations of its own. Thus it is quite possible to compare a memory image with an after-image, although the two are assuredly not present in the same space. Nor is the discrimination of spaces dependent upon vividness; the memory-image is often more vivid than the after image, and it may, as at dusk, be more vivid than an actual perception. William Blake affirmed of his own imagery — from which his wonderful drawings were directly copied — that it was 'infinitely more perfect and more minutely organized' than ordinary perceptions. Further, Blake discriminated these images very explicitly from externalized or projected images — a 'ghost,' as he put it, he had never seen but on one occasion, when he beheld a repulsively spotted being on the steps of a house.[1]

I distinguish, then, as 'mental' images, those which have disjunct spatial contexts of their own; *bona fide* projections are images that intrude upon what one is conscious of as real space. This does not mean that the imaginary space is not external in the sense of being in a field of vision; but it is distinctively an imaginary space, related to the mind's eye. To talk about its internality or externality at all borders on absurdity; it is simply not included in the space system of which body and head form a part.

But it is not unusual for images in imaginary spaces to plunge into real space. The mark of their advent, and, I take it, the safest criterion of projection, is their modification of actual sense-perception. When a portion of the perceived surfaces of things is blotted out to give place to an image — as when a picture appears upon a printed page and hides or blurs the print — then projection takes place. Thus it is relation to objects of perception that is the determining factor.

[1] See *William Blake*, by Alfred Story (1893).

Effort has been made to distinguish very sharply between imagery that affects sensation and imagery that is purely ideational on the hypothesis of distinct cortical centers,[1] and this may very possibly be a just discrimination, but it certainly cannot sustain any very sharp cleavage of a subjective character. When I speak of images being projected, I mean that sensible ideas become sensation (that is modify sense-perception) without any material alteration of character and content. This is not a case of hallucination or of pseudo-hallucination (if this *means* hallucination), but it is the case of a visual mental image becoming a visual physical image, and being recognized as such. I imagine that images so projected are really what is meant by 'pseudo-hallucinations.' Professor James' account of these, characterizes very well images of the β type, when projected as I have described: 'From ordinary images of memory and fancy, pseudo-hallucinations differ in being much more vivid, minute, detailed, steady, abrupt, and spontaneous, in the sense that all feeling of our own activity in producing them is lacking'; further, they are 'projected outwards.'[2]

As a matter of fact, there is no hard and fast line between sensation and sensible imagery. Even the criterion suggested, of spatial interference, is not of indubitable application. I recall an instance in which an apparition-like image appeared directly over the shoulder of a person with whom I was conversing, so attracting my attention as to elicit query concerning what I was seeing. The image occupied a perfectly demarkable locality, toward which my eyes were visibly accommodated, yet I was not then aware that the real space was in any sense infringed upon and was perfectly aware that the image was of the fancy. As bearing upon the correlation of spaces, I note that many times projected images appear sequent to winking; I have

[1] See *The Nature of Hallucination*, Boris Sidis, PSYCH. REV., N. S., XI. 2. The statement (p. 128) that 'ideas and sensations differ fundamentally, they differ in kind and no amount of ideational activity can ever be made to become sensory in nature,' if it really means that mental imagery cannot be projected in the sense defined above, can hardly be accepted; the reverse is observably true in every respect except continuity of spatial context and even the ideational space may be considered as subject to projection when we consider the anomalous smallness of some projected images (see below).

[2] *Psychology*, II., 116–117.

repeatedly become aware of previously unnoticed imagery in this way. It would seem that a blanking of the retina to outer stimuli were necessary to the apprehension of these images, which, afterwards, are enabled to maintain themselves for an appreciable time in competition with sense-perceptions. Such images always appear in specific space,—on the wall, the page of a book, etc.

In this connection, it may be in place to offer demur to Professor James' categorical assertion that it may be stated 'as a universal proposition that after-images seem larger if we project them on a distant screen, and smaller if we project them on a near one, whilst no such change takes place in mental pictures.'[1] This is a usual, but not an invariable rule; some mental images follow precisely the law of after-images, indicating similar retinal excitation. For example, after an evening at chess or whist, I have observed, even at an interval of an hour (surely an impossible intermission for the advent of a positive after-image),[2] projections of chessmen or pips suffering the same distortion and alteration of size as ordinary after-images. Again, the occurrence of an image on the page of a book is plainly conditioned by the angle at which the book is held and is liable to distortion to suit this angle.

From the foregoing it will be seen that there is no universal distinction between 'mental' and projected images, and after-images. It is true that the latter are manifestly of sensory occasioning, but it seems also certain that all projected images involve retinal excitation. A difference of importance, though a relative one, is the ephemeralness of the evicted central imagery, — in my case, quite unable to stand up against incom-

[1] *Psychology*, II., 51.

[2] These images also differ from after-images in being wholly ephemeral, having not more than a second's duration, whereas an ordinary after-image may endure a minute or more. Professor James cites Meyer: "Most of these subjective appearances, especially when they were bright, left after-images behind them when the eyes were quickly opened during their presence. For example, I thought of a silver stirrup, and after I had looked at it a while I opened my eyes and for a long while afterwards saw its after-image." I do not see that this is not a case of simple projection, unless, as Professor James infers, negative after-images are meant. I have observed colored images to project themselves in black and white, but this I lay to their inability to compete with sensuous luminosity and to the fact that the color element is the weaker in my visualization.

ing stimuli. The various differences between after-images and imagination-images which Fechner has noted,[1] I find to hold either not at all or to only a partial extent. After-images are for me no more coercive or involuntary than images of the β type; they are neither brighter nor sharper in outline than other images may be; and though usually 'unsubstantial and vaporous,' this is not necessarily the case. On one occasion I placed a new silver coin on the palm of my hand, in bright sunlight, for the purpose of getting a negative of the design. I had no thought whatever of the hand. On closing the eyes it was with a shock of utter unpreparedness that I beheld a tiny hand as substantial, detailed, truly colored and formed as an actual perception. The surprise was so complete that for an instant the image was a veritable hallucination. With the eyes open it enlarged and dissolved in the usual fashion.

The tininess of this image is worth note. It brings up the whole interesting question of size in visual imagery. With me, and I presume with most persons, all after-images appear small when viewed with closed eyes or in pitch darkness. A window seen from a distance of seven or eight feet, as an after-image, contracts to a seeming height of two or three inches. The contraction is not instantaneous, though it must be particularly heeded to be observed *in transitu*. The size indicated remains nearly constant (so long as the eyes remain closed) during the changes attendant upon dissolution.

The small size characteristic of after-images with the eyes closed holds of most mental images where no effort is made to realize a just magnitude. Smallness, in fact, seems to be one feature of the spatial independence of these images. For the shrinking of after-images, the taking away of all the hints by which we commonly judge perspective might seem a sufficient account, and similarly abstractness an explanation of the smallness of centrally occasioned imagery; but *a priori* there seems to be no reason why an image should assume one size rather than another. Some help may be gained by reference to actual space perception. Chance juxtapositions often startle us by their emphatic contractions of the third dimension, and by

[1] Cited by James, *Psychology*, II., 67.

a sufficient effort one may very nearly free oneself from perception of this dimension. By attentively envisaging a house, for example, one may observe it gradually flatten out and shrink into a diminutive toy house.[1] But there is a limit to this contraction, a norm of diminutiveness beyond which it cannot go. Apparently the factors that determine this limit include besides narrowing of the attention, so as to eliminate the signs that ordinarily give perspective, some direct relation of physical and conceptual conditions.

I venture as a guess that the normal shallowness of space is a sort of sense generalization, a perceptual construct, determined by the focus of most distinct vision — that is, at about reading distance,— and that the size of images, relative to the area stimulated, is dependent upon the horopter at this focus.[2] An object at this distance stimulating an area equal to the area stimulated by a house at a hundred feet would then give the abstract or normal size of the visual idea of the house, which must hence appear very small if envisaged as a whole. But it is obvious that if all images are reduced to the same or approximately the same absolute size, they will not all be reduced in like proportion; objects naturally smaller will seem relatively less reduced. This is actually the case, the limiting instance being furnished by typographical images which appear undiminished because they are presented at the normal reading distance. We thus get an explanation of the usual lack of true proportionality of mental images.

It is not to be supposed that small images are universal. Many people either do not see images as small, or, if they do, they still reckon with space and judge the objects as if seen at a distance sufficient to account for the size.[3] Possibly it is only the

[1] Apropos, I vividly recall that on the first railway journey of which I have recollection (at about the age of four), I was amazed to see tiny people, tiny cattle and houses, all but a few inches high yet all as real as reality. There were whole fairylands through which I was whisked, but with imperishable glimpses retained. No scepticism of my elders could abash my conviction, for what I saw I saw with my mortal eyes.

[2] So far as I have been able to judge the process is independent of actual accommodation, which would rather discount the kinæsthetic element. In the reverse case, however, of imagining an image enlarged, kinæsthesis seems clearly present.

[3] So a friend testifies.

bookish who find the ideal world crowding so close; and in any case it is possible to enlarge all imagery subject to volitional retention either by a fiat extension of the third dimension or by imaginatively filling in the detail that creates perspective, though each process involves effort.[1]

III. The Imagery of Dreams.

The imagery of the state bordering on sleep is so distinctive in an empirical way that I have a somewhat coercive feeling that it ought to be placed in a class by itself. At the same time I can discover no certain characteristic differentiating it from the type β; the main differentia from other instances of this imagery is greater duration with an aptitude for evolving under the eyes from one form to another, but the evolution is so palpably a consequent of the duration, and the duration so plainly due to the freedom from sense or thought competition in the state of semi-sleep, that this differentia is of little account.

It is possible that these images arise in connection with idioretinal light or some internal disturbance of the eye,[2] after the manner of 'fortification' images, but in no case will this account for the intrinsic character of the images — that is, as representations of ideal things,—and in any case there remains a residue of manifest central origin; there is no ground to suppose that the retina can itself create imaginary portraits or

[1] Not to place too much weight on the mechanical features of vision, it is well to bear in mind the analogous smallness of auditory images which to many people, especially when overworked and nervous, take the form of imaginary *whispering*. Though weak in auditory imagery I have occasional snatches of music having a peculiarly phonographic diminutiveness. It should be noted, too, that there is some variation in the focus of imagery — if I may so call it. An after-image of a lighted lamp, with closed eyes, appears at about the distance mentioned; but if the image be taken with one eye only, the other being left open, it is appreciably nearer. Of course the contrast with the extension seen by the open eye might very well account for this.

[2] Compare the statement of Dr. Sidis, in the article cited: "Often in closing my eyes and keeping quiet, so as to become somewhat drowsy, and watching the field of vision, not directly, but, so to speak, from the corner of the eye, animals, figures, faces, can be seen forming and dissolving into mist. These phantoms can be directly traced to specks of light and masses of color coming from the retina and especially from the macula lutea." The certainty of this origin seems to me far from apparent.

evolve ideal representations consistent with, yet differing from, reality.

The persistently sensational feel of this imagery would, however, seem to warrant the inference of some physiological element, obscure though it may be. That there may be a kinæsthetic factor involved is at least suggestively hinted by an experience which I may cite: While reading, both hands beneath the book, I became drowsy, my eyes closed and my head seemed to fall forward. Just as, or just after, my eyes closed, a face appeared directly before them,—a man's face, thoroughly Mongoloid, excepting the eyes which were full, open, brown and very bright. As I saw the face, it seemed to fall toward mine, the eyes at the same time closing so that I saw the lids droop; there was also a vague image of a hand raised to catch the head. The image appeared with a vividness sufficient to rouse me completely. It is to be noted that my eyes did close, my head nodded, and although my hand did not rise up, my first feeling after the surprise was that this hand somehow seemed unsatisfactory, as if it ought to move. On several other occasions I have noted the same phenomenon of an image of closing eyes in connection with the drooping of my own lids. Such images seem to indicate the *translation* of an affective state into a form of cognition, of a subjective feeling into an objective representation. In a grade of conscious life where mental process has reference solely to near activities the like function might well possess vital significance.

The general character of borderland images is fairly described by Professor James' account of pseudo-hallucinations; they are relatively 'vivid, minute, detailed, steady, abrupt and spontaneous.' I find them also diminutive, projected to but a few inches, mutable or self-changing, usually fragmentary, and frequently grotesque or odd. To give instance: On one occasion I saw what seemed to be a living cartoon of the face of a friend, a microscopic exaggeration, yet perfectly recognizable. Again, a woman's head and shoulders, the head thrown back with strong light and shade on the features; the face of a primitive savage type, prominent cheekbones, wide zygomatic arch, prognathous jaws, the mouth open showing large irregular teeth; the eyes were in the shadow of heavy brows, the hair was low on the forehead, the neck very muscular. On another occasion, a Dantesque nose and brow which gradually filled out to full profile and lengthened into a vapid expression of mock piety, the whole irrelevantly giving place to a thick-lipped mouth with rows of teeth widely shown.

Preponderatingly these images represent faces of men or imaginary animals (the order of frequency I judge to be: eyes, mouths and noses, profiles, full faces, bodies), and it is not an uninteresting speculation — if the like experience is as common as I take it to be — whether such tiny personifications may not have had a deal to do with the development of belief in fairies [1] or diminutive night-folk who come, like Queen Mab,

> In shape no bigger than an agate stone
> On the forefinger of an alderman,
> Drawn with a team of little atomies
> Athwart men's noses as they lie asleep.

Between the imagery of semi-sleep and that of dreams proper there is one marked distinction — that in dreams objects appear to be of the size of real objects. Further, in dreams images do not appear to be images at all, but seem to be the real things themselves. But the latter distinction is subject to our very frequent consciousness of the unreality of the dream state, and even the distinction as to size must be received with caution. In the dream state there is no competing space relation (unless some vague sense of the body's extension), and hence no criterion by which to judge size outside the somewhat vague remembrance of dream consciousness. If a comparison of dream space and real space could be made, it might be found that dreamland is confined to Lilliput. I guess this from two confirmatory experiences. The first was an awakening in the midst of an early morning dream in which a horse drawing a sleigh seemed to be approaching, growing in size as he did so with the rapidity and a good deal of the effect of the moving picture produced by a kinetoscope. Wakened, I became aware of sleigh-bells outside, the approaching sound of which undoubtedly suggested the

[1] At least this seems no more improbable than the usual derivation from a race of palæolithic dwarfs. Compare, also, the conception of the human soul as diminutive and image-like: Tylor, *Primitive Culture*, I., p. 450.

dream; in addition, I became conscious of having heard the bells in the dream, and also that the visualization had been diminutive, enlarging as the eyes opened. The dream had, so to speak, telescoped, and was retrospectively given a proper extension through the influence of the continuous stimulus furnished by the bells. A second case was the projection of a dream image of a human figure upon sudden awaking. It was one of a group in the dream, and when my eyes suddenly opened, I was surprised to see the figure lengthen out exactly as does an after-image.

On more than one occasion I have had a distinct dream consciousness that the dream experience was one of imagery, not reality; but this I conceive to be a late development, partly due to the nature of my interests. Probably as fair a test of the relative real-seeming of dreams as can be gotten is the duration of dream impressions as compared with memories of real experiences.[1] My own most long-standing memories include at least two dreams which occurred when I was between three and four years old, as I know from the fact that both are concerned with the death of my mother at that time. Except from internal evidence (their inconsistency with reality as I have come to know it), I should not be able to distinguish these dreams from real memories, and of other memories of early origin I am not even now certain. Nowadays, however, I am frequently dream-conscious, and even find myself fully alive to the theoretic interest of a dream state, which I am yet unable to influence. A recent instance curiously illustrates. On January 7 I dreamed that I was being shown a collection of pen and ink drawings and instructed as to their merits. One of the drawings was of a mediæval landscape, and on examination the clouds in the sky proved to be swathed cherubim in horizontal attitude. In my dream this picture seemed very unique and impressive, and on waking I remembered it with great distinctness. On February 15 I dreamed that I was shown this same drawing again, that is to say, I recognized it in my dream; but on awak-

[1] This test is if anything unfair to the dream, since, especially in later life, it offers fewest points of attachment to that systematic apprehension of reality which experience generates and which in turn so largely selects those memories in which experience is preserved.

ing I remembered that what I had seen in this dream had in fact been a chalk drawing on slate of two vertical, or erect, swathed figures, with no landscape at all.

In these dreams and their memory images there is a curious interlocking of the dream world and the real world and an interconnection of dream interests somewhat bearing out the suggested conception of a secondary plane of intelligence. Although dreams are able to create their realities at whim, they yet reveal a rudimentary consistency which is the beginning of law and order. Linking together in memory, even if erroneous, is one step; discrimination of the dreamer's self from the dream world, though it be but a feeling of helplessness in the presence of dream mastery, is a greater step; and finally the advent of intelligent interest allies the dream state very closely with rational experience. In a few recorded instances dreams have actually solved problems which have baffled normal intelligence,[1] and it is worthy of note that they have done this in the form of sensible imagery and in what might be called the dramatic mood of reason.

IV. The Influencing of Imagery.

From foregoing discussions it should be apparent that lines of distinction between different classes of imagery are nowhere fixed and determinate. After-image and imagination-image, projected and non-projected, dream image and waking, all are united by liminal, indecisive cases. The distinction which maintains itself most steadfastly is that between images of the α and β types, between voluntary and spontaneous imagery, but even this distinction will be found to break down to a considerable degree when we carefully examine the influences to which imagery is subject.

In restricting the influence of volition to a certain type of imagery, I have allowed myself an accommodatingly indefinite use of the term. It is a question whether new combinations of elements are ever volitionally predetermined, while on the other hand it is certainly possible voluntarily to influence the occurrence, if not the content, of images other than memory images.

[1] See the cases reported by Professor Newbold, Psych. Rev., III., 132.

After-images, of course, are found by paying attention to them, and in the same way, if one keep on the look-out for imagination images one is sure to perceive them; indeed, I am not at all confident that their liveliness and frequency may not be directly dependent upon an attitude of expectancy toward them, — upon an assumption of incipient rapport with their plastic objectivity. This is not saying, however, that there is involved any predictive certitude as to character; the images always have some degree of surprise in store. Even in the most abjectly servile imagery there is always some spontaneity. It is sometimes held that it is impossible to will an indefinite end, that desire must be conceptualized before it can become an object of volitional effort. Were this really so, we should never will anything at all. The calling to mind of the simplest idea or the merest recollection always introduces some incommensurable element; there is always at least possibility of surprise in the summoned presentation. For example, I entertain the idea of an elephant, willing the image, and instead of the full figure which I have a right to expect, there appears only head and trunk. Further (I cite a specific occurrence), it appears at a certain distance and of a certain (life) size; the head is turned slightly to one side, the tusks are absent. It is perfectly plain that this experience represents a considerable degree of incommensurableness; the formal and final causes by no means square with one another. Volition is thus largely discredited at its inception, wholly unable to free itself from irrelevance and chance.

Moreover, even the general influence of volitional attention in assuming the attitude of expectancy must be guardedly exerted if it is to be effective. If too direct, attention really seems to defeat its object in many cases; often it must be oblique, sidelong, in order to catch that for which it waits. There is needed a peculiar kind of mental passivity — even in connection with the keenest curiosity — where images are concerned. If one too deliberately tries to manipulate them, they simply balk, and more than likely disappear altogether. Of course this does not apply to memory images, though even a memory image is apt to obtrude itself in unexpected garb if left unwatched.

If one may so put it, then, the most direct influencing of imagery must be indirect, and the further one proceeds from mere reproduction the more the indirectness grows. In ordinary thinking there is seldom a consistent rational nexus between thought and image, doubtless because the thought is too rapid for the imagery to keep pace. At the same time a reflective state of mind, one in which the ideas are chief concern, is decidedly conducive to vivid imagery, however inclined this may be to sport-like characteristics. If the interest be imaginative rather than abstruse the images are naturally more abundant and are also more apposite; and if the imaginative thinking is of sufficient intensity or lasts for some time, it alters the whole texture of thought, frequently for days at a time. On one or two occasions I have undertaken some line drawing, and on these occasions have found a large accession of images in black and white line-work. These images were characteristically spontaneous, with a strong element of surprise, and often they were projected. Usually the specific suggestion or association was quite untraceable, though in one case I recognized the nucleus of a striking presentation in a forgotten penciling of six weeks previous. The image had been subconsciously incubated during this period and at the end hatched forth with all the self-sufficiency of a lusty chick.

While interest is thus to a degree able to contract the range of imaginative experience and intensify its content, it is unable, so far as introspection shows, to furnish any account of actual variations. No element of suggestion, no law of association will quite explain the phenomena. Images frequently bear not even a hypothetical relation to their contextual consciousness. For example, I am quite at a loss for an explanation of the curious ethnological traits or yet of the grotesqueness prone to characterize my imagery. Again, even in the case of memory, images are seldom enough mere reproductions; neither are they mere erosions of former impressions; rather they are veritable transformations, growths, the spontaneity of which is sufficiently attested by the fact that they sometimes surprise to laughter. The problem of their origin and function may be worth a few paragraphs of speculation.

V. Imagery and Ratiocination.

Sumarizing the general phenomena of occurence, it may be stated: First, that the mind tends normally to inhibit a mass of inflowing imagery, either directly through refusal to entertain the presentations offered, or indirectly through ratiocinative or perceptual preoccupation. Second, that the mind is able to create, through interest or suggestion, an imaginative diathesis which, in a measure, overcomes the inhibitory function. Corollary to this is the tendency of the mind so predisposed to sink into an attitude of mere curiosity towards it images, which hence achieve relative independence and self-direction. Third, that a class of images (the β type) bear internal evidence of a process of subconscious synthesis,—that is, the elements of which they are composed in the relations in which these elements appear belong to no recognizable historical experience. This incommensurable element is present to some extent in memory images,[1] but *ex hypothesi* is there subject to limitations.

These facts to my mind make plausible the supposition of a primitive form of plastic intellection, intuitive rather than reflective, yet having the true characteristics of rational thinking—namely, dissociation and ideal synthesis. It is much the habit of writers on ratiocination to credit all mental power of abstraction and construction to the naming-function of language. Even if the generic image is possible, maintains Stout,[2] we have yet to show that it has not been rendered possible by preceding word analysis. It seems to me that such writers rely too strongly upon the supreme significance of language in their own, exact, thinking. Metaphysical abstrusities naturally demand a mobility of ideas which no form of thought limited to space-bound presentations could yield. But this is hardly true of thought in general. Galton found scientists to be nearly devoid of imagery, but women and children were freely endowed with it. The kind of ideas which one entertains makes all the difference in the kind of medium demanded, Language is undoubtedly the supremely facile agent of thought, but it is by no means the

[1] Consult the interesting discussion of *la mémoire pittoresque*, by M. Paul Souriau, in *l'Imagination de l'artiste* (Paris, 1901).

[2] *Analytic Psychology*, II., 176.

only efficient one. For my own part, I find that consciousness of language is keenest when I write; the words group themselves and fall in order far ahead of the pen; yet when endeavoring to master a difficult abstrusity, it is necessary to lay the pen aside; consciousness of language then sinks to a minimum and the conception if grasped at all is grasped in a sense of relationships, it is not mentally expressed. So far as I can observe such thinking quite transcends all use of language or other sensuous element. In an opposite direction, effort to understand geometrical or mechanical conceptions is vastly furthered by mental pictures; an illustration is much the most effectual definition of a machine, a diagram the aptest solution of a problem in space relationships.

It is certain enough that we do reason to some extent in pictures. Whether such reasoning actually precedes linguism may be doubted, but it seems to me that it presents a type of reasoning which might be fairly developed among the higher animals. Where the end of thought is proximate action — and no animal, we may be sure, reasons for any great future — thought should be as rapid and instinctive as possible; and for the saving of mental confusion and waste no surer device could arise in nature than subconscious specialization of thought-processes; reflective consciousness is decidedly the product of biological leisure. Moreover, since an animal's interests are practically all in the world of space, visual images answer his highest need for abstraction, especially since his thought's symbolism must needs be of an unconventionalized, pictographic order.

The mass of imagery, then, which so presses in upon our normal intellectual life may be no more than a ghostly reminder of what was once the sum and substance of thought. The fact that the function of the mental picture has been so largely taken over by language would go far toward accounting for the irrational, dreamlike texture and sequence of the images; they are mere residual mental organisms, pursuing a natural course of degeneration. Possibly the seeming degeneration may be accounted for as the natural inferiority of the mode of thought itself; the image failing to keep pace with the normal growth of mental life. Some such conception, in the case of dream consciousness, seems to be entertained by Dr. Sidis: "It may be claimed that the dream consciousness is to some extent a reversion to the earliest forms of mental life, when the race was as yet undisciplined by the accumulated experiences of ages of social life."[1]

As bearing upon the suggested view, I venture to cite, also, a suggestion of Dr. Bosanquet's: "It may be that in early soul life this reproduction [of ideas by other ideas] is unconscious, and that its results, the images which it brings before the mind, are not used as ideas, *i. e.*, are not distinguished from fact or known to be symbolic of a content other than themselves. The results of experience may be made available for the guidance of an animal through suggestion effected by reproduction, but not distinguished *as* suggestion from any presented reality. In this process we have something that does the work of judgment and inference, and that has the same fundamental nature with them."[2]

Apropos, it might be added that the better part of all our reasoning is unconscious. I, at least, have never been able to fixate with the mental eye an actual case of conscious disentanglement of puzzle or of clear-seeing conceptual synthesis. What I do find is a preliminary state of confused heterogeneity, mere puzzle, and then a succeeding state of illumination. Order appears from disorder, plan from chaos, but in response to no urgency or coaxing. Knowledge always comes as a precipitate from the psychic solution. Occasionally a significant phrase forms itself and presents itself, perhaps as a typographical image, coming as from nowhere and with no apparent incentive, but usually the process is merely the process of *seeing through*, and it happens with an utter spontaneity not to be distinguished from that of the apparition of imagery.[3]

[1] *Loc. cit.*, p. 110.
[2] *Logic*, II., p. 15.
[3] The MSS. of this article was received April 13, 1904.—ED.

INCIPIENT PSEUDOPIA.

BY REV. CHARLES CAVERNO, A.M., LL.D.

Lombard, Illinois.

One of the miseries of my life has been a supersensitive retina. The sunshine on snow or a bare road is often torture. Any cone of artificial light is unendurable. To sit in an electric-lighted room, even with eyelids closed and perhaps with a shade over the eyes, is to forfeit the efficiency of the next day. If I have congestion about the head from grip, fever or cold, then it is an affair of bed, black bandages and darkened rooms. Out of this experience have come some observations which are of interest to myself and may be to others.

The doctrine of the specific action of the senses is well understood. The nerves of the retina are made to respond to light and they answer even mechanical pressure in terms of light. I have been interested in watching, in my hours of darkness, the illumination which I get from the inflammation of the retina. The glory of light seems then turned on at full head. In the interest of clear psychological distinction I wish to say that this light is not subjective — a thing of imagination — it is objective; as it comes from the pressure, against the optic nerve-filaments, resultant from congestion.

Further on we shall see where psychic action comes in, as plainly distinguishable from this illumination, or any of its phenomena, as is such psychic action in any perception which is derived from light that comes from the external world. The field of vision in my case is confined to the optical apparatus itself.

I am satisfied that I see the march of the blood through the arteries of the retina itself. I feel my pulse by seeing it in the retina. It is with some timorousness that I make such a statement, but I make it. The statement rests on the perception of the same phenomena recurrent for many years. I have seen, under a microscope, the corpuscles of blood dart along the tissue in a frog's foot; and the motion there is similar to that which I detect in my eye; only the march is in open order in the frog's foot, whereas in my retina the order is close. I have tried to verify my conclusion in various ways. The steady onflow I cannot stop nor turn aside by volition. I can talk with friends or open my eyes, but when I settle back and the eyes are shut, there is the same procession of bright particles moving in the same way. This phenomenon comes in when the tension from inflammation is high enough and departs as that tension ceases. In this experience I am sure that psychically I am a passive percipient, I see what I have to.

But the show does not end with the sensation of light and the perception of the procession of the blood. I get an unlimited variety of pictures projected before this interior vision. They are the products of imagination I know, but I have no will power over them. They begin at the edge of this inward horizon, pass over the field and then depart. They come and go at their own sweet will. I can never anticipate what is coming. I can call up, alter or modify nothing. I am as helpless about what the exhibition shall be as one sitting in the midst of an auditory is over the movements of a panorama on which he is looking. I have tried again and again, when a face would begin to appear, to have it take the form of some one familiar or loved, and have never yet succeeded. I am compelled to watch the show that is brought up before me. While imagination furnishes some of the elements of the spectacle, I am sure inflammation furnishes others. For instance, something begins to move along before me and it develops into a full-blown iron-foundry. Everything in it takes on the glow that flashes out in a real foundry when the furnace doors are opened; only the color is uniformly distributed over everything. The tools, the walls, the iron rods, the cinders are of molten-iron color. A heap of cinders is simply a heap of gems, an iron rod is burnished gold. I charge the flame-tint up to motion which I interpret in terms of light. But the foundry and all its appurtenances move along and disappear. I cannot stop it. I cannot call it back. The glow may continue but it is formless. I have many sights savoring less of inflammation than that of the foundry. I am entertained with landscapes of all sorts; and here again I am helpless. If a landscape begins to move out before me I must take it as it is set. I have tried over and again to turn a starting picture into a vision of my old home, or of some familiar scenery, but it always develops in its own wilful way. There is a beautiful ravine twenty miles from Chicago with a magnificent elm at its head. I have supposed myself starting up that ravine many a time but I have never yet seen that elm in these compulsory imaginative excursions. Within three months I have been sure that I was at the lower end of that ravine and have had high hopes of reaching the elm, but the vision refused to go in that direction and turned me out, apparently on the edge of a wood in central Illinois that I had not called for, and I

"Was left lamenting."

Perhaps I had better give some of the conclusions I have formed. My helplessness in respect to imagination assimilates the phenomena of my visions to those of dreams. But then are we not in our waking moods more than a little helpless in regard to imagination? Do we not then and there have to take pretty much what volunteers in the field? It is a startling, disquieting thought that we have little control over imagination even in our normal conditions. It is the most elusive, evasive, the least tamed of our faculties, the one most inexplicable.

Throughout all these visions I find sensation and psychic action tethered together as in normal sight. The picture is carried forward because it is physically connected with the steady onflow of the circulation of the blood. In these visions I am on the open road to the pseudopia of all delirium. Between what I see and what a victim of *delirium tremens* sees is only a matter of degree. Carry the pressure by congestion on the filaments of the optic nerve far enough, and let some of the brain centers used in coördinating thought and act be involved in the inflammation, and it is easy from my point of experience to see that the vision of imagination might catch and hold the attention to the exclusion of the vision derived from the external world. I might then start to run up my fictitious vale at Brush

Hill or begin to set things to rights or wrongs in my mentally invented foundry. Out of my long experience I have no memory of unesthetic visions. Medically speaking they have been "benign." Landscapes predominate in my panoramas. Though I cannot call up the countenance of a friend the faces I do see are not ugly. Rarely does a human being figure before me. Though my foundry appears to be in working order to the last touch there is never a soul besides myself in it.

I am not fond of pain, but I have had my compensations in experiences of the above-described sort and in observation and reflection upon them. I have had my exhibitions when I have not used opiates, or rather they have had me. It would make no difference in the ultimate physical and psychic analysis however if opiates were used. They explain nothing. The question still is, what are the laws of such visions or why their lawlessness?

NOTE. — Since the above was written I have had another experience with pseudopia. I lay in bed a few days from a cold and congestion about the head. One evening the visions came on and I described them aloud to my wife for half an hour. This experience was mainly of a geological sort. Rock views passed before me such as one would get in riding over the mountains in Wyoming on the Union Pacific Railroad. The rocks were at close range so that I could see all the bands of stratification in the sedimentary rocks. Sometimes the rocks were divided perpendicularly as well as horizontally so as to suggest a wall laid up artificially. But I saw no tools and no workmen. In fact I did not see a single human being in the whole half hour show. The primitive rocks appeared in all their amorphous condition. I could not get vista or prospect among or over them though I tried with all my might. But view after view of rocks in endless succession came on and went off. The only modification of bare rock (no tree or shrub or grass was in sight) was once when the view slid off to a river — drift bank in which were tumbled large boulders as is usual. One boulder was broken across the face and plainly showed the mixed gray, pepper and salt mottling of granite. This did not long continue and then the vision went back to the lithological vision which was on before. Nothing was repeated. The variety was only equalled by the actuality as you climb the Rocky Mountains and go over the crest in Wyoming. All of a sudden the rock exhibition failed and there came out a beautiful landscape in which nothing of vegetation was wanting to completeness in effect. From embowering trees I looked down a long vista over a grain field filled with bunches of grain as farmers put them up before stacking. The field was apparently of acres in extent and the further edge faded out down a declivity with an intimation of a broad valley lower down beyond. This closed the show and I have seen nothing since and do not expect to till I have another season of congestion about the eyes.

I do not care about drawing conclusions; but I do not see why I might not have the visions of frenzied oracles, bacchants, hermits, devotees — any of the sights of sinners or saints, if my mind was bent in their particular directions.

N. S. Vol. XI. No. 6. November, 1904.

The Psychological Review.

THE CLASSIFICATION OF PSYCHO-PHYSIC METHODS.

By DR. EDWIN B. HOLT.

Every one who has undertaken experiments on the relation of stimulus to sensation, or even read considerably on the subject, must have become aware of the inadequacy of the historical and still current classification and designation of the psychophysic methods. For many cases arise in practice which have no immediate place in the classification, as for instance those in which it is necessary to recognize and more especially to evaluate judgments other than those of 'less,' 'equal' and 'greater' (as say 'much less' 'uncertain' and 'much greater'); and other cases arise whose place is doubtful because they have features of several of the methods but have not all the essentials of any one of them.

If for instance one were to wish to find the acuteness of a given sense in an interesting pathological patient, and if from independent reasons (as may well happen) one were obliged to adopt the so-called method of right and wrong cases, one might be brought to pause, if the subject were impatient or perhaps hysterical, by the appearance of adverse emotions and fatigue, due to the almost endless repetition of just two stimuli which this method requires. And yet perhaps one could get from the subject one fifth the required number of judgments on each of five different pairs of stimuli, or else one tenth of the number from ten pairs. Now clearly if one were careful to have one stimulus common to these five or ten pairs, one could calculate by the method of least squares from the five or ten resulting groups of judgments the coefficient of precision ('Präcisionsmass') with as much accuracy as otherwise from one group of ten times as many judgments, on a single pair of stimuli. Yet in such an instance one would not be using the method of right and wrong cases. For although the point of departure and the answer yielded conform to the descriptions of this method, yet the actual data gotten and used in calculating the result are precisely such as one would have gotten from the method of least differences, or minimal changes (Abstufungsmethode der kleinsten Unterschiede) if peculiarities of the sense-organ investigated or exigencies of apparatus available had forced one to use few and coarse gradations of stimuli and to present the different

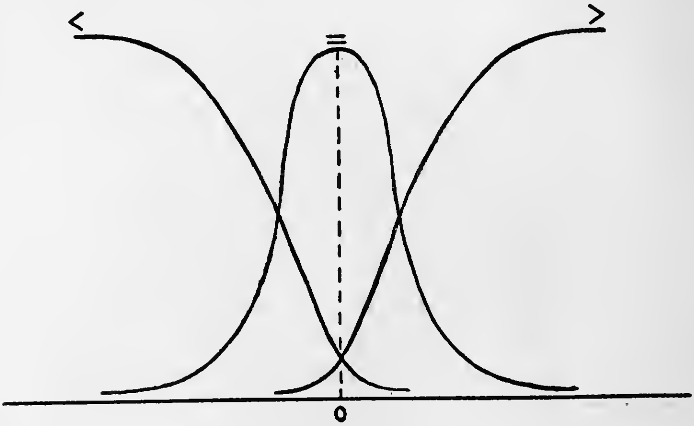


FIG. 1.

pairs at random. For in both cases the data would consist in judgments of 'less' (<), 'equal' (=) and 'greater' (>), which would be distributed as shown in Fig. 1, where the abscissæ represent the value of the stimuli greater or less than the standard stimulus (o) which is common to the ten pairs, and where the ordinates represent the number of judgments.

In short the experiment described does not come under any one of the traditional methods, since it has also features of at least one other method, nor can it properly be called a 'combination' of the methods, since it has not all the essential features of any one.

Apart from the confusion which every reader or experimenter in psychology has probably felt in assigning a given experiment to one of the four or six 'methods,' the unsatisfactory state of the whole matter is amply witnessed by the historic errors made by the most distinguished experimenters in designating the methods they have used, by the confused and overlapping definitions of the several methods invariably given in text-books, and by the amendments offered from time to time by various theorists. The errors of designation, although several were made by Fechner himself[1] are chiefly of historic interest. Of systematic interest, however, are the current definitions and descriptions of the psycho-physic methods, and the several emendations which their inconsistencies have elicited.

The case above cited of an experiment which, by having features of two methods and yet lacking the essentials of any one, belongs properly under none of the four or six methods, is not wholly unprovided for in the treatises. Thus Wundt,[2] in defining the method of mean gradations, describes, for the case that the middle stimulus varies irregularly, a certain simple way of deriving the mean value from the raw data: but this way need not be used, he says, since the raw data can also be treated by the method of right and wrong cases, in which case the whole procedure becomes 'a combination of the method of mean gradations with the method of right and wrong cases.' Now the simpler way of evaluating the data applies exclusively to the method of mean gradations: is it then essential to this method? Clearly not, since it may be abandoned in favor of the method of right and wrong cases. But then the method of mean gradations is not, as is generally supposed, a method for both getting and then evaluating raw data, but only for getting them. Conversely, too, the method of right and wrong cases is not at all a method for getting data but only for treating them when gotten. But if this is true, these two methods are not alternatives to be chosen between, but supplementaries to be used in combination; a conclusion which is at variance with the present theory and practice.

[1] Müller, G. E.; *Zur Grundlegung der Psychophysik*, Berlin, 1879, S. 56 ff.

[2] Wundt, W.; *Grundz. d. physiol. Psych.*, 5te Aufl., Leipzig, 1902, I Bd., S. 480.

This contradiction arises, of course, from the confused definitions of the methods, whereby these latter, instead of being four mutually-exclusive logical classes, as they should be, are defined as being now exclusive, now partially identical, now coördinate, and now not coördinate. And this confusion extends through the whole tissue of the methodology, at least the Wundtian. For this author, after dividing all methods in two (mutually-exclusive ?) groups, of gradation (Abstufung) and of telling-off (Abzählung), explains[1] that 'Among the telling-off methods the method of mean errors is most nearly related in its origin to the gradation methods or more particularly to the method of minimal changes.' Indeed,[2] 'The method of mean errors arises from the method of minimal changes in case one limits oneself to taking the just not-perceptible differences of stimuli.' By this is presumably meant that the raw data in the method of mean errors are identical with a part of the data in the method of mean gradations; and the fact is left out of account that the data are further quite differently treated in the two cases, in ways which by no means 'arise' the one from the other. In short Wundt's systematic treatment of psycho-physic method stops short of logically exact and consistent definition.

Yet these ambiguities are by no means peculiar to Wundt; firstly because most other modern treatises follow, or perhaps even copy, the veteran psychologist, secondly because the same ambiguities have prevailed from the very first. Thus while Wundt finds the method of minimal changes merging into that of mean errors, Fechner[3] found that it 'goes over' into the method of right and wrong cases! The key to the situation is this: the four psycho-physic methods are historical developments bearing the marks of their growth and of the accidents they have met. The methods have not been defined, they have been *used;* and where a user has had to modify a method he has generally not modified its name, so that there are several varieties under every method, and these bear the most diverse and unsystematic relations to one another. So great has been

[1] *Ibid.*, S. 473.
[2] *Ibid.*, S. 472.
[3] Fechner, G. T.: *Elemente d. Psychophysik*, Leipzig, 1860, I Th., S. 75.

the dignity of tradition in this matter of method, that a thorough-going revision and consistent systematization of the procedures has never been achieved.

To systematize the procedures is not necessarily to analyze or revise their mathematical details,— an ambitious performance indeed,— but the frame-work of methodology can be reconstructed in and for itself. This frame-work so recast will carry with it undisturbed, as tent-poles the canvas, the vast multitude of details.

To commence, one must first survey the methods in their confusion, then either discover or postulate some one feature as the sole essential of each method, and then deduce the logical consequences. The Wundtian account is the most characteristic and may well be made the starting point.

The methods fall in two groups, those of gradation and those of telling-off. The former group has two classes, the method of minimal changes and that of mean gradations; the latter also two, the method of mean errors and that of right and wrong cases.

1. The Method of Minimal Changes.[1]

"In this method one seeks to determine at different points on the scale of stimuli such a change in the intensity of stimulus as produces a barely perceptible change of sensation." And, in detail, one finds what interval between the standard stimulus and a lower, compared stimulus is needed to make the two feel different, and the same between the standard and a higher stimulus; and again the interval is found between standard and lower or higher stimulus which will just not make the two feel different. The average of these four intervals is the threshold wanted.

This is good as a scheme, but on coming to the practice one finds that there is no interval which just is or is not *always* felt as a difference. There are intervals which are generally felt as one or the other, but in order always to be so felt the interval must be either so large or so small that it tells nothing about the accuracy of discrimination which one is trying to find. The scheme appears to be applicable if the compared stimulus can

[1] Wundt, *op. cit.*, SS. 470, 476–479.

vary about the standard *continuously*, for then one commences with the compared stimulus plainly larger, say, than the standard and diminishes the former until it no longer seems larger; one calls this interval the upper threshold of difference, and finds the other three desired intervals similarly. But the continuously applied stimulus tires the sense, while habit retards or expectation hastens (presumably according to the temperament of the subject) the moment in which the relation of the stimuli is felt to change; and it has not been proved that these factors cancel themselves out. Thus the result yielded by such a procedure would be modified by the temperament of the subject and the capacity of his sense-organ to resist exhaustion, and would not be a pure measure of his discrimination.

If the compared stimulus is such as cannot be varied continuously then the procedure in question is, for all careful work, out of the question, for the final result will be found to be not a little dependent on the size of the step-wise gradations which one arbitrarily has adopted in the series of compared stimuli. And furthermore the case will often come up and have somehow to be taken into consideration that one interval will yield a perceptible difference, the next smaller will not, while the next smaller than that will again do so.

In short, whether the compared stimulus is to be varied continuously or step-wise, it is advisable (as Wundt himself admits; S. 478) not to present these stimuli in their orderly progression, but rather in a random succession. But 'this procedure has at the same time the character of a combined method, since it approaches the telling-off methods.' Whether 'combined' or not, it yields three kinds of judgments ('greater' 'equal' and 'less'), and they show the arrangement which we have met before in Fig. 1.

What is now essential to this method of minimal changes? It is not the use of a standard stimulus and compared stimuli, for the method of mean error also uses these; nor is it the 'minimal' gradations in the compared stimulus, for the methods of mean gradations and of mean error require these; nor is it yet the ascertainment of the intervals above and below the standard stimulus which barely do *not* give a difference in sensation, for the method of mean error involves also this. Essential to the 'method' of minimal changes is only the *project* of finding that difference between stimuli 'which produces a barely perceptible change of sensation.' This, however, is not a method, but a problem: furthermore the quantity so sought needs a precise definition (which the phrase quoted by no means gives) and gets this only from the method finally fixed on for solving the problem. Now the methods of evaluating data described by Wundt as peculiar to the 'method' of mean gradations have been shown to be inadequate for careful work; and in order to be precise, as Wundt himself advises, one must use a method which 'approaches' (to put it plainly *is*) the method of mean error. In a word, the 'method of minimal changes,' is not a method at all, but a problem which one sets oneself. It can be solved with precision only by such a procedure, and by any such, as yields at least three classes of judgments grouped as in Fig. 1. How from these a solution is to be gotten is not told by the 'method of minimal gradations.'

As if so much confusion were not enough, the account provides only for the case of finding that change of stimulus which produces a change of sensation, that is, the threshold of *discrimination;* whereas the case that one wishes to get the threshold of *sensation* may properly come under one of the methods, and certainly can come under no other than this of minimal changes. Finally this threshold of discrimination is an average or two thresholds, those of just perceptible and just imperceptible difference, each of which has often been made the basis of investigation, although in either case the method has generally been called minimal changes. Thus the 'method of minimal changes' not only is a problem and not a method; it is not even a definite, single problem.

2. The Method of Mean Gradations.[1]

This method, which Wundt substitutes for the older, somewhat more natural, and at least equally important 'method of over perceptible differences,' consists in finding a mean stimulus which shall seem to lie equally far from two chosen extremes.

[1] Wundt, *op. cit.*, SS. 471, 479-481.

Two intervals are to be made equal. 'But in order to get reliable results,' Wundt says, 'it is necessary to combine this method either with that of minimal changes or with one of the two telling-off methods about to be described.' In fact it appears that when the mean, variable stimulus is presented with the two standards, it will seem to lie nearer sometimes the lower, sometimes the higher, standard; sometimes just half-way between. This gives rise to three groups of values for the mean stimulus, those for which it is judged to lie more and less than half-way and just half-way, between the two extreme stimuli. And these three groups are arranged, once more, as in Fig. 1. If only a rough approximation to accuracy is wanted, the informal procedure described under minimal changes can be used in evaluating the data. But as has been shown, this procedure is not permissable if it is a question of careful work. Rather after the three groups of values shown in Fig. 1 have been found, these must then be in some so far unexplained way evaluated; in a way, presumably, which will 'approach,' 'resemble' or 'shade off' into one of the telling-off methods about to be described.

In short, mean gradations are no more a method than were minimal changes, but once more a problem. There the problem was to find the threshold of discrimination between sensations; here it is to find the threshold of discrimination between intervals; there the just perceptible difference, here the just equal over-perceptible differences (intervals), between sensations.

Wundt's account of this 'method,' which is doubtless designed to intone the importance of Merkel's 'law,' is peculiar inasmuch as it gives the impression that one must always take the extreme stimuli as fixed and vary the mean stimulus; whereas by far the most work on over-perceptible differences has been done by taking either the upper two or the lower two stimuli as fixed, and the variable stimulus below or above them. When this is done the data still consist in three groups of values for the variable stimulus, according as the variable interval seems greater or less than, or equal to, the standard interval: and these groups still shape themselves as in Fig. 1.

Thus both of the so-called gradation methods are no methods, but are problems; namely, how to find objective values (measuring the stimuli) which somehow correspond to just perceptible or to equal over-perceptible differences, respectively, between sensations. And either *problem* requires for its solution, firstly, a *procedure* which will yield *three classes of values of the variable stimulus* (corresponding to the judgments 'greater,' 'equal' and 'less') grouped as is shown in a general way by the curves of Fig. 1. Secondly, the problem requires a *procedure* for deriving from these three curves a measure of the discrimination, respectively of difference between sensations or of equality between over-perceptible intervals. The former procedure affords the raw data, the latter evaluates them. In these two processes will be found the real division of psycho-physic methods.

3. Telling-off Methods: the Method of Mean Error.[1]

This method 'arises from the method of minimal changes in case one limits oneself to taking the just not-perceptible differences of stimuli.' If this were all, then this method would be not an independent method at all, but one of the subdivisions of the so-called method of minimal changes. But it is not all. For while the raw data group once more as in the curves of Fig. 1, we are now for the first time told how to evaluate these data, and to get from the curves a measure of discrimination. The *procedure* becomes here for the first time precise; one finds by definite rules the raw mean error, the variable mean error, the constant mean error; not to mention, as Wundt does not, the probable error, the standard error or deviation, the coefficient of variability, and the relative variability.[2] The fineness of discrimination is usually taken as the reciprocal of the variable mean error; or, to put it accurately, the discrimination is *defined* as equal to the reciprocal of the variable mean error, called for brevity's sake mean error. By other conventions the other 'errors' are utilized more or less advantageously to give light on the workings of the sense organs under investigation.

[1] Wundt, *op. citat.*, SS. 472–473, 481–482.

[2] Cf. Yerkes, R. M.: Psychological Bulletin, 1904, Vol. 1, No. 5, p. 137 and Myers, C. S., *Report of the Cambridge Anthropological Expedition*, Vol. II., Pt. II., p. 212.

But this variable mean error measures the sense discrimination not only for the case that one is seeking the just not perceptible difference between stimuli, but also when one wants the just perceptible difference (the quest which is confusingly called the 'method' of minimal changes), or when one wants the just equal over perceptible differences or intervals (called the method of mean gradations). In both these cases also the variable mean error, with the method of finding it, is the precise definition of the terms 'just perceptible difference' and 'equal over-perceptible difference'—terms which were so far loosely used in stating the problems of minimal changes and mean gradations. In short the mathematical manner of evaluating the raw data, which always group as in Fig. 1, is the only exact definition of the measure of discrimination which is being sought.

The method of mean error is then both a problem and a method. Its problem is to find the just not perceptible difference between stimuli. Its method is a mathematical affair of averaging errors and of other details which we are not now concerned with going into; except to note that here at last is a true method. But this method has no intrinsic affiliation with the problem of not perceptible differences: it is equally necessary in evaluating the data on just perceptible differences and just equal over-perceptible differences or intervals. This is a fact which the traditional classification quite obscures, although the actual practice of psychologists will be found to accord with that fact and to belie the tradition.

The three 'methods' of minimal changes, mean gradations, and mean error have amounted so far to *three problems* in three kinds of discrimination, each calling for a special kind of data to be gotten by experiment; and then, *one universally applicable mathematical method* for evaluating these data. The three problems in discrimination may now conveniently be called—just perceptible differences, or *j.p.d.* (from the method of minimal changes), equal intervals that is equal over-perceptible differences, or *e.o.p.d.* (from the method of mean gradations), and not perceptible differences, or *n.p.d.* (from the method of mean error). The one mathematical method for evaluating any of these data, with its variations, will be called through the rest of this paper the method of mean error, proper. One more *problem* may well be added — that of the threshold of sensation, or *t. of s.* (in comparison with which, as will be recalled, the discriminations just mentioned are often named thresholds of difference).

4. The Method of Right and Wrong Cases.[1]

This method consists in the repeated presentation to a subject of two stimuli which are so nearly equal that they will frequently be judged to be quite equal, and the greater be sometimes judged even less than the lesser and conversely. Thus the judgments will be sometimes right and sometimes wrong — a fact which every author sapiently points out, as if the same were not true of judgments found under every one of the other

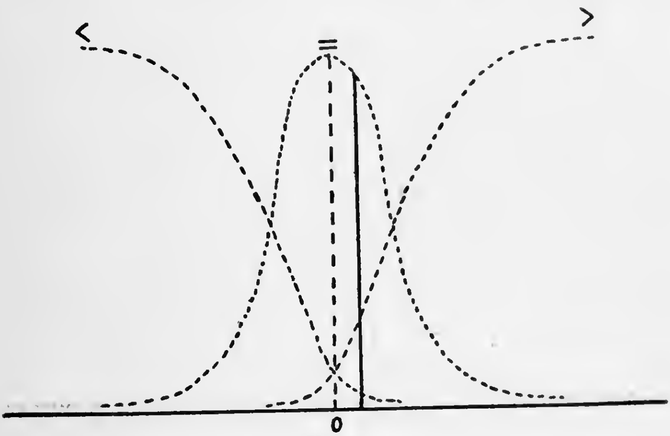


Fig. 2.

'methods.' But the essential fact is that, although these data cannot be grouped like those of Fig. 1, yet they are exactly such a part of Fig. 1 as would lie in an ideally narrow vertical section taken near the zero point of Fig. 1. This is shown in Fig. 2. That is, this method yields as much of the Fig. 1 as a standard stimulus compared with only *one other* stimulus can yield. The judgments gotten are that the compared stimulus

[1] Wundt, *op. cit.*, SS. 473-4, 482-90.

is equal to, greater or less than, the standard; and in order to complete these data into a figure like Fig. 1, it would be necessary only to bring in more compared stimuli, *i. e.*, to use more pairs. This was shown in the second paragraph of this paper. Why sometimes many pair of stimuli are used, and why sometimes only two, does not for the moment concern us.

Now we have seen that the data of Fig. 1 can be evaluated and made to yield a measure of discrimination, by means of a method which we have called the method of mean error. The method of right and wrong cases presents us, in fact, with a *second method* of evaluating data into a measure of discrimination, and relatively fragmentary data at that, *i. e.*, judgments on but two stimuli. It is not the purpose of this paper to discuss these mathematical methods of evaluation in their details; and it is sufficient to recall that a 'Präcisionsmass' is derived from the judgments on two stimuli by means of Gauss's formula for the error curve, or more readily by the use of Fechner's table of integrations derived from that formula. The method assumes that the theory of errors may be applied to the mistakes made in comparing two stimuli, that is, the 'wrong cases'; another fact which is invariably emphasized, although the averages taken in the method of mean error involve exactly the same assumption.

The method of right and wrong cases is seen, therefore, to be no new problem but in fact a new, real method of evaluation, to be used (one is told) when for any reason the stimuli compared have been only two. Right and wrong cases are generally used for finding a 'Präcisionsmass' of the just perceptible difference, but there is no intrinsic reason why the use of two stimuli and Fechner's integral table should not give a measure of equal over-perceptible differences, or even, with the standard stimulus made equal to zero, of the threshold itself.

We have so far analyzed the four so-called psycho-physic methods into four problems (not parallel with the original four 'methods') and two real methods. The problems are those of the *j.p.d.*, *e.o.p.d.*, *n.p.d.*, and that of the threshold of sensation, or *t. of s.* The methods are those of mean error, *m.* of *m.e.*, and right and wrong cases, *m.* of *r.w.c.* Let us now see what is the relation of these two methods to each other. The *m.* of *m.c.* is commonly recommended; except where the data are derived from the comparison of only two stimuli, in which case the more cumbersome *m.* of *r.w.c.* has to be resorted to. But this is not the real distinction between the two methods. It will be recalled by experimenters that the measure of precision yielded by the *m.* of *r.w.c.* is virtually neither more nor less than the steepness of the curves shown in Fig. 1, or to be more exact, it is a numerical representation of the steepness of the curve of 'greater,' or that of 'less,' judgments (for these two curves are assumed to be ideally similar) when one half of the 'equality' judgments at every point has been added on to this curve (Fig. 3). Now it is always in-

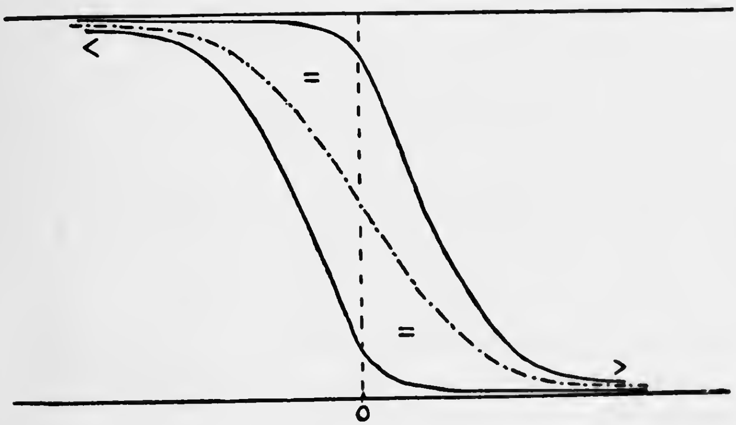


FIG. 3.

sisted that when the judgments are made on but one pair of stimuli, the number taken must be very large indeed if the measure of precision so derived is to be worth anything. But it is seldom if ever suggested, as was done in the second paragraph of this paper, that by the method of least squares the measure of precision may be calculated from more pairs of stimuli and proportionately fewer judgments on each pair. And the results in the two cases will be to all intents and purposes identical. Now this would be applying the *m.* of *r.w.c*, to the full data of Fig. 1: which shows that the distinction between the methods of *m.e.* and *r.w.c.* is not one of the meagreness or fullness of the data to be evaluated, that is, is not a question of the number of pairs of stimuli used. But it is a question of the *degree of accuracy* aimed at. The *m.* of *r.w.c.* gives a relatively fine measure of precision, of which the mean error, probable error, and other quantities of the *m.* of *m.e.* are the bares and roughest indications. But so far as these last signifiy anything, they suggest *approximately* the steepness of that same curve (Fig. 3) of which the 'Präcisionsmass' of the *m.* of *r.w.c.* is a relatively *accurate* measure. The difference between the two mathematical methods is thus purely one of degree of accuracy, and it is a mere accident of technique that the *m.* of *m.e.* cannot be used with judgments on only one pair of stimuli, nor the *m.* of *r.w.c.* for the determination of not perceptible differences. In fact the latter is possible if an experimenter should choose to neglect a part of the wrong cases, taking only those which form the curve of 'equal' judgments (Fig. 1), and deriving the steepness of this curve by means of Fechner's table. For this would be a permissible variation of the *m.* of *r.w.c.*, comparable to the several variations in the *m.* of *m.e.* which have been used and recommeded.

It may well be questioned whether the *m.* of *r.w.c.* is not a much more accurate procedure that the conditions of experimentation ever justify; or whether the application of least squares would not be a case of penny-wise after pound-foolish, in any sort of physiological work whatsoever. But we are not here concerned with the mathematical minutiæ of method,—only with the general classification. The new tent poles are to carry all the old canvas. Indeed if mathematical details were here in question, it would be our first duty to examine and if possible to justify the fundamental assumption of both methods, *i. e.*, that the theory of errors may be applied to curves which never are and by all psycho-physic laws never can be, truly symmetrical.

To survey our results once more, the four psycho-physic 'methods' resolve themselves into the four problems, of finding the threshold of sensation, and the thresholds of not perceptible, just perceptible, and equal over-perceptible, difference; and then the real two methods of mean error and right and wrong cases. The four original 'methods' are curiously illogical classes, which have come about in the course of the development of psycho-physics. They are historical relics. The first two 'methods' are only problems; the third is a problem and a method; while the fourth is a method but not a problem.

We have seen that in all psycho-physical experimentation there are two stages of the work — the getting of data, and the evaluating of them. We have already considered, so far as it lies in our purpose, the second, purely mathematical stage. It is instructive, and in part will account for the retention of the false methodology, to note how the experimenter has to approach the first stage of his work, the getting of data. Suppose that he wants to study by means of the ordinary olfactometer and one of the four 'methods' the discrimination for odors. He cannot use the 'method of mean gradations' because few if any subjects are able to identify and hold in mind an over-perceptible difference between odors. So the experimenter thinks that he must choose another 'method'; the fact it that he must choose another problem, that is, he must not hope to measure the olfactory discrimination for over-perceptible differences. Similarly he will believe himself deterred from the 'method of mean error,' because he has been taught that this 'method' involves the adjustment of the stimuli by the subject himself: and of course an odor-tube cannot be adjusted back and forth like a monochord. He may think of doing himself, as experimenter, the adjusting, continuing each time, although this is contrary to the school directions, until the subject declares a just not perceptible difference. This is possible to do, but hardly advisable; since it happens by an accident of physics that the olfactometer is more suitably and naturally adjusted from less olfactory stimulus to more, rather in the opposite direction. It is an accident, then, that the experimenter cannot well study just not perceptible differences, but must choose the remaining problem, the 'method' of just perceptible differences.

It may be said that this necessity of casting about for a problem (*n.p.d.*, *j.p.d.*, *e.o.p.d.*) suited to the accidental peculiarities of the sense-organ to be studied and the apparatus at one's disposal, has made the spurious distinction between 'methods' seem real and practical to experimenters who seem not to have noticed that the distinction is in problem and not in method.

Thus the first stage of psycho-physical experimentation, the getting of data, is practically not determined by the experimenter, but by relatively accidental circumstances, — peculiarities of the sense-organ chosen and of the apparatus available. And in determining what data can be gotten, these accidents determine at the same time what problem can be studied, that is, whether *n.p.d.*, *j.p.d*, or *e.o.p.d.* The word 'accident' may be objected to. Yet it is fair to call accidental the circumstance, for instance, that the *n.p.d.* cannot be studied in the active muscle-sense. Such seemingly chance and irrelevant factors are almost numberless. An important one of them, although it influences the general problem less directly, is the possibility of individual or massed stimulation of end-organs. By an anatomical accident the olfactory end-organs cannot be stimulated individually; so that what is an interesting problem of the dermal senses, the relative thresholds of different individual end-organs, cannot be studied in the sense of smell. It is apparently an accident, though an interesting one, that some sense-organs, as the olfactory, gustatory, or dermal when stimulated singly, do not afford us clear sensations of intervals, *i. e.*, of over-perceptible differences. Hence their power of discrimination must be studied in other respects; the problem of *e.o.p.d.* is debarred. Once again, if the experimenter happens to have two tuning-forks and nothing else, he will necessarily adapt his problem to the *m.* of *r.w.c.* with which he will have to evaluate his data. Whereas if he had a sonometer, he would more naturally let the subject adjust for himself and give judgments of *n.p.d.* The case in which the investigator has but two different stimuli to apply (and these must be nearly alike) is the one case in which truly the *method as well as* the problem is determined by irrelevant circumstances. Otherwise the *method* is chosen (*m.* of *m.e.* or *m.* of *r.w.c.*) according to the *degree of accuracy* which is desired.

Aside from the accidents which determine the problem, there is another kind which influences only the technique or at best bears but remotely on the problem. Such a factor, for instance, is the matter of simultaneity or succession of stimuli. Since the olfactory end-organs have to be stimulated all at once, any discrimination which is studied has to be a successive one (apart from the very doubtful case which some would claim, of simultaneous bilateral stimulation). This circumstance would still leave as possible any of the three problems; but it helps to restrict more precisely the technique to be adopted. There are countless other accidents of a similar sort.

Therefore in first approaching a psycho-physical problem, and in trying to get it realized in some arrangement of apparatus, the experimenter finds that very little is left to his free choice. Sometimes indeed he may choose no more than barely the sense which he studies. Two kinds of accidental circumstances, as we have seen, restrict his course, although in practice it is scarcely necessary to distinguish between them. In order to accept the inevitable, and set up his apparatus with the least waste of time and thought, the experimenter must run through the possible limitations and find out definitely what ones actually confront him. The possibilities are fairly well included under the following categories, although the list aims merely at being serviceable but not exhaustive.

1. Comparison — (*a*) simultaneous, (*b*) successive.

2. Comparison — (*a*) direct (immediate), (*b*) mediate.

3. Comparison between — (*a*) two stimuli, (*b*) more than two stimuli.

4. Variation — (*a*) random, (*b*) progressive.

5. Variation — (*a*) continuous, (*b*) discontinuous (step-wise).

6. Apparatus operated by — (*a*) the experimenter, (*b*) the observer.

7. Actual relation of the stimuli — (*a*) known to the observer, (*b*) not known to the observer.

8. (If the sense to be studied and the above enumerated conditions are such as still to leave the question open): Discrimination of — (*a*) threshold of stimulation, (*b*) not perceptible difference of stimulation, (*c*) just perceptible difference of stimulation, (*d*) equal over-perceptible differences of stimulation.

These headings are all familiar to the psychologist and need no elucidation. They are neither completely independent nor yet mutually exclusive. Class 1, for instance, is independent of 2; but 3, *a*, excludes 4 and 5. After learning what of these alternatives are open, the experimenter will see his way of proceeding rather precisely marked out. Herewith the first methodological stage is ended. Before coming to the second stage, of mathematical evaluation, he has only to get his data.

In regard to this second stage we have already seen that there are only two ways of evaluating data (although each method allows some minor variation), the methods of mean error and of right and wrong cases. It was not the purpose of this paper to discuss the methods in detail, but only to analyze the so-called four 'methods' and to classify the results. This we have now done. It need only be noted once more, that the choice between the two actual methods (except in the case of only two stimuli being used) depends on nothing but the degree of accuracy which is desired, that is, on the amount of labor which the experimenter thinks proper to devote to the inquiry. We may now pass to two of the other emendations of the traditional methodology, which have been offered.[1]

[1] The writer greatly regrets that before this article was actually set up, he had not seen the admirable work of G. E. Müller, in the *Ergebnisse der Physiologie*, 2ter Jahrgang, 1903, II Abth., SS. 266–516. Müller finds four cases (Fälle) in which psycho-physic methods may be used. These are our *t. of s.*, *j.p.d.*, *n.p.d.* and *e.o.p.d.* Of 'methods' he finds three; the first, in which the observer adjusts the variable stimulus in random sequence; the second, in which the experimenter does this, but in orderly increasing or decreasing progression; and the third, in which the stimuli are not adjusted (old method of right and wrong cases). There then remains the task (Aufgabe) of finding in general two values, a mean and its variability. This may be done immediately (our *m.* of *m.e.*), or by the mediation of formulæ (our *m.* of *r.w.c.*).

Of course this is in essentials far nearer to that which has been urged above than is any other classification hitherto offered. To the present writer it still seems, however, that the 'cases' are problems of which accidental circumstances largely determine the choice; that Müller's 'methods' are merely three among a large number of equally important such accidents; and lastly, that the actual methods are Müller's two Aufgaben, the treatment of the results either with or without formulæ. But only the second point is a difference in principle, while the first and last are merely nominal.

THE CLASSIFICATION OF KÜLPE.[1]

Külpe's analysis of the methods is not fundamentally different from that of Wundt, but there are superficial differences which are worthy of a brief consideration. For Külpe there are two groups of methods, that of minimal changes and that of errors; these two correspond to Wundt's gradation and telling-off methods. The method of minimal changes has four 'applications'; while the error methods are two — the method of right and wrong cases and the method of mean error.

The method of minimal changes may be 'applied' to the determination of threshold (Reizbestimmung), to the comparison of stimuli (Reizvergleichung, the *n.p.d.* mentioned above), to the determination of difference (Unterschiedsbestimmung or *j.p.d.* above), and to the comparison of differences (Unterschiedsvergleichung or *e.o.p.d.* above). This virtually admits, although Külpe seems unconscious of the fact, that these four groups are not methods but problems, as has been argued in this paper. Furthermore his classification is symmetrical and consistent: *i. e.*—

1. Threshold — (*a*) of sensation, (*b*) of interval between sensations.

2. Equality — (*a*) of sensations, (*b*) of intervals between sensations.

Külpe well says that there is one method, which he calls the 'method of minimal changes, that applies to these four classes. This 'method' is essentially like the procedure described by Wundt under the same name, and is subject to the objection which was noted in the early part of this paper. This is, as will be remembered, that whether the compared stimulus varies continuously or step-wise, there is no point at which the change in judgment from being always 'greater' or always 'less' to being for the first time 'equal,' or vice versa, is truly significant. For let us suppose the compared stimulus to be decreasing toward the standard, it may be considerably greater than this and be judged 'equal' while when further decreased it will be again judged 'greater.' Or if it is increasing to the standard, it will often be judged 'equal' when considerably less than the

[1] Külpe, O., *Grundriss der Psychologie*, Leipzig, 1893, SS. 55–81.

standard, but again 'less' when it has been increased nearly to equality with the standard. To ignore these inconsistencies and to interrupt the comparisons with the first judgment of equality which is given (or inequality, as the case may be) is, if the compared stimulus varies step-wise, to take a measurement of discrimination which depends materially on the size of the steps which have been arbitrarily chosen. If the compared stimulus varies continuously, the case is a trifle better but not much, since then the measure of discrimination is considerably vitiated by fatigue and expectation (see above). Furthermore it is often not possible to-make the compared stimulus vary continuously.

For these reasons Wundt virtually admits, as we have seen, that here is no method: and of his 'method of minimal changes' he leaves, although merely by implication, only the problem of the *j.p.d.* Külpe, however, accepts the informal procedure disparaged by Wundt, and insists that it is a method. It has been asserted elsewhere in this paper that every psycho-physical method must involve a procedure which yields at least a part of the data shown in Fig. 1, and must then mathematically evaluate from these some sort of a measure of discrimination. Now what part of Fig. 1 does this procedure yield which Külpe recommends? And how are these data evaluated? The 'method' is to present to the subject a pair of stimuli, a standard and a compared, for his judgment of 'less,' 'equal' or 'greater.' The compared stimulus varies in successive presentations not at random but so as to approach or depart from the size of the standard stimulus; and it may do both, either above or below this standard. Thus there are four modes in which the compared stimulus can vary. Whether in a given experiment some or all of these modes are used depends on the problem to which this 'method of minimal changes' is being applied.

Let us suppose, for example, that it is being applied to the problem of *j.p.d.* All four modes are used. The compared stimulus (*c.s.*) starts so much smaller than the standard stimulus (*s.s.*) that it is always judged smaller; and is gradually increased until the first judgment of equality is gotten. Here the progressive comparisons are interrupted, although if *c.s.* were further brought up toward *s.s.* it would very likely be judged less once or twice more. Here is of course the weakness of this method. Now *c.s.* is taken equal to *s.s.*, and made gradually to decrease until the first judgment of less is given. So far the experimenter has a series of judgments 'less' 'less' et cet. — 'equal,' for *c.s.* increasing; and a series 'equal' 'equal' et cet. — 'less,' for *c.s.* decreasing. In both cases *c.s.* is smaller than *s.s.* But it can increase and decrease while larger than *s.s.* Therefore two more similar series are gotten, for values of *c.s.* above *s.s.* Curiously enough, Külpe writes as if it were enough to take the average of only these four values of *c.s.* at which the first change in the judgment occurred.[1] But he can

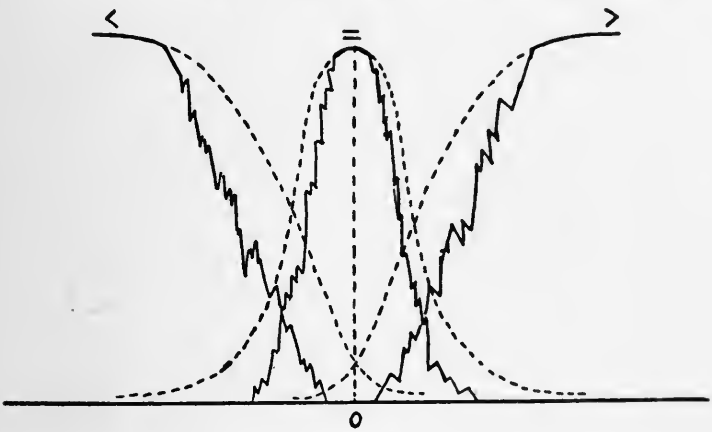


FIG. 4.

not mean so, for it appears later,[2] that a mean variation may be in question; so that of course, as one would expect, the four series are to be gotten many times, and an average of all taken for the actual measure of discrimination. The reader will see at once that if plotted these raw data would look like Fig. 4.

The relation to Fig. 1 (the dotted lines) is clear enough. Where the differences between *c.s.* and *s.s.* are so very small or so very large as always to be correctly told, the curves run smoothly. They become jagged where the chance errors interrupt the various series of 'less,' 'less,' et cet. —, 'equal,'

[1] Külpe, *op. cit.* SS. 60–62.
[2] *Ibid.*, SS. 66.

'equal' et cet.—, or 'greater,' 'greater' et cet.—, with a change in the judgment uttered. Now, as we have already seen, it is just because the first change in judgment depends more on the size of the step variations of *c.s.*, on fatigue or expectation, than on the fineness of discrimination in question that this method, as even Wundt grants, is not a proper method. It selects for further mathematical evaluation a certain portion of the data of Fig. 1; but the principle of selection is largely, though not utterly, independent of the power of discrimination which is being studied. And whereas in the *m.* of *r.w.c.* when only two stimuli are used, the selection although arbitrary yet leaves enough of the essential parts of the curves; the principle of selection here used effectually excludes just these indispensable parts, namely the parts which show the characteristic steepness of the curves.

Therefore in the earlier part of this paper the name of method was justly denied to this procedure, since it is absolutely inadmissible. And it was insisted that the only allowable and yet mathematically convenient method was to take data giving the complete curves of Fig. 1, and then to express approximately the steepness of those curves (transformed perhaps as in Fig. 3) by means of a mean error, probable error, or by some such readily obtainable quantity. Külpe's method of minimal changes is simply under no circumstances allowable. The merit of this part of his classification lies wholly in his four-fold 'application' of the 'method of minimal changes.'

The other part of his classification needs but brief consideration. As to his 'method of right and wrong cases,' he well says:[1] That it 'is capable of as manifold application as the method of minimal changes'; but then he goes on to describe the method so wholly in the traditional way that one has the impression that the use of only two stimuli is as essential to this method as is Gauss' equation itself.

Külpe's treatment of the 'method of mean error' adds nothing to that of Wundt, except the statement that while the method has been used only for the *n.p.d.*, it could be used for the *e.o.p.d.* as well. But he declares (S. 78) that it can be used

[1] *Ibid.*, S. 70.

only when experimenter and subject are one person, and when the stimulus can vary continuously. Both of these statements we have seen to be untrue.

The Classification of Ebbinghaus.[1]

· The other classification which will concern us in this paper is that of Ebbinghaus, given after his short and admirable account of the traditional four methods. Ebbinghaus says (S. 75) that, 'The goal of psycho-physic method is the determination of those stimulation values which are the outward causes of equal-seeming psychic values (that is, of equal intervals between sensations),' of course, then, the threshold of stimulation is not a problem of psycho-physics; and in fact the author makes no mention of it.

Now as to the size of the intervals between sensations, it can be either just perceptible or over-perceptible, and if the latter it can be as large as one chooses. Herewith our problems of *j.p.d.* and *e.o.p.d.* are recognized, although still called methods; while the problem of *n.p.d.* is ignored. Secondly, as to the way of judging, this may be so chosen that the subject (S. 75) 'has in mind the idea of an equal interval and varies the outward stimulus until the sensation which it gives corresponds to this idea: or, one can present repeatedly for judgment a given pair of stimuli and let the subject give his judgment in terms of certain replies previously agreed on,' such as 'less,' 'equal,' and 'greater.' The first procedure is called the 'finding a stimulus' to correspond to a given judgment; the second, the 'finding of judgment' to correspond to a given pair of stimuli. Lastly, in every case the final result must be the average of many observations; and since each observation will vary from this average, the mean of the variations must always be given (mean deviation), in order to show the reliability of the total average.

This scheme is very original and suggestive; but inadequate. It recognizes but one true method, the one which we have called above the *m.* of *m.e.*; and but two problems (which Ebbinghaus still calls methods) to which this one method can

[1] Ebbinghaus, H., *Grundzüge der Psychologie*, Leipzig, 1902, SS. 74–80.

be applied; those are our problems of *j.p.d.* and *e.o.p.d.* The problem of threshold of stimulation is dismissed as not lying within the field of psycho-physics. The expediency of so narrow a definition of psycho-physics is doubtful; for it should seem better to account the problem of psycho-physics the measured correlation in general of stimulus and sensation; and thus to include the problem of threshold, which after all must be studied if at all by the *m.* of *m.e.* or that of *r.w.c.* Also the problem of *n.p.d.* finds no recognition because, as the author says (S. 78): 'The third method, finally, that of mean error [that is the third traditional method, by which he refers to the problem of *n.p.d.*], affords nothing directly which can be utilized as a measure of sensation, since it operates not with a sensation interval, but with the disappearance of an interval'; *i. e.*, it operates with a not perceptible interval, which is seemingly not to the point. And again he says (S. 69): 'The determination of this mean error [made in judgments of *n.p.d.*] through the range of a given sense has of course its use, but clearly these values are something quite different from equal sensation intervals in the above mentioned sense [that is, of just perceptible, and equal over-perceptible, intervals], and the process of getting them is no true measuring of sensation but a process having some relation to such a measuring.' Ebbinghaus gives elsewhere his grounds for this view, in that the *n.p.d.* never had any value except on the assumption that it bears a definite and fixed relation to the *j.p.d.* from the same standard stimulus. This assumption has not been shown to be valid, and Ebbinghaus seems to account it very speculative if indeed not certainly false. He may be quite right. The point is one more of mathematical technique than of classification. If he once granted the validity of the *n.p.d.*, Ebbinghaus would doubtless class it with the *j.p.d.* and *e.o.p.d.*

The analysis given in this paper found the third so-called 'method' of tradition to consist in a problem and a method. Ebbinghaus discards the problem but retains the method, which is our *m.* of mean error. This is in fact his one method, and it must be used in every one of the four procedures into which he resolves the traditional 'methods.' These are, once more:

1. Just perceptible interval with finding of stimulus.
2. Just perceptible interval with finding of judgment.
3. Over-perceptible interval with finding of stimulus.
4. Over-perceptible interval with finding of judgment.

Thus in every psycho-physical procedure an average of the observations must be taken, with their mean variation or error. These two quantities are the measure of discrimination which is desired.

But what of the 'Präcisionsmass' as yielded by Gauss's equation and Fechner's table of integrals? Ebbinghaus minimizes in his system this method of evaluation because, it should seem, he accounts it a very laborious process to be used only when all the observations are based on but two stimuli: the more cumbersome mathematical evaluation being used solely in order to make up for the poverty of the material of observation. The author does not admit, apparently, that here is an evaluation method of relatively great accuracy, which can be applied as well to observations on many pairs of stimuli as to those on only one pair.

We have seen that from the first and second traditional methods Ebbinghaus analyzes out his two categories of just perceptible and over-perceptible, difference. Both of these 'methods' proceed by '*finding the stimulus*,' whereas the fourth 'method' (of right and wrong cases) proceeds by '*finding the judgment*'; hence these two new categories. Every psycho-physical procedure uses one of the last two, together with one of the first two categories. Thus arise the four methods of Ebbinghaus, given on the preceding page. But now all four of these methods have to use the little germ of truth which lay in the traditional third 'method' (of mean error): this was the use of the average and its mean error. Wherefore the final form of Ebbinghaus's four methods is as follows:

1. Just perceptible interval with finding of stimulus; average of all observations with *m.e.*

2. Just perceptible interval with finding of judgment; average of all observations with *m.e.*

3. Over-perceptible interval with finding of stimulus; average of all observations with *m.e.*

4. Over-perceptible interval with finding of judgment; average of all observations with *m.e.*

Thus this system, which is somewhat simpler than the traditional four, is gotten by the omission of two problems, those of the threshold of stimulation and of the *n.p.d.*; and by the omission of one method, that of right and wrong cases. It is curious that aside from the relatively special and insignificant expedient for evaluating judgments on only two stimuli, Ebbinghaus finds nothing in the traditional method of right and wrong cases save the suggestion to fit the judgment to the stimulus instead of the stimulus to the judgment, as in minimal changes.

Although this classification leaves out so much that it becomes inadequate to the subject, it probably has the distinguished merit of being the first radical and strictly logical recasting of the methodology. It is well worthy of study, and will be found to be possibly, the clearest and best form for teaching the methods of psycho-physics to elementary students.

Summary.

The four traditional methods of psycho-physics are found to be an illogical scheme of a subject which if analyzed resolves itself into the following system.

I. Four problems as to the relation between stimulus and sensation, which admit of quantitative treatment.

(*a*) The threshold of stimulation (*t. of s.*).

(*b*) The not perceptible difference of stimulation (*n.p.d.*).

(*c*) The just perceptible difference of stimulation (*j.p.d.*).

(*d*) The equal over-perceptible difference of stimulation (*e.o.p.d.*).

II. A great diversity of procedure by which data on these problems are gotten (see above), and of which the one thing essential is that judgments shall be obtained which group themselves like a part or like the whole of the judgments in Fig. 1, and so that the steepness of at least one of the curves of Fig. 1 is implicity contained in the judgments. This variety of procedure is not a variety of choice open to the experimenter. The procedure used in any case depends on relatively accidental characteristics of the sense-organ chosen for study; and for this reason they are not susceptible of rigorously logical classification. Furthermore it generally happens that these different accidents, so far from being a source of freedom, actually retrict the experimenter in his choice of problem, so that he is obliged to study a particular one of the four problems.

III. Two real methods by which the judgments can be evaluated into a measure of the discrimination of the sense studied: that is, by which the steepness of one or all of the curves in Fig. 1 can be approximately or accurately expressed. These may be called, out of respect to tradition.

(*a*) The method of mean error (*m.* of *m.e.*).

(*b*) The method of right and wrong cases (*m.* of *r.w.c.*).

The difference between these is one of accuracy. The former method measures the precision roughly by taking an average of the observed values and their mean error, or probable error, et cet. The latter method is more refined and uses the equation of Gauss and the integral tables of Fechner to obtain a 'Präcisionsmass.' The significance of either of these measures lies in its being an index of the steepness of one or all of the curves of Fig. 1.

Either of the two methods may be used with any of the four problems. There are thus eight alternatives. Having chosen the sense-organ which he will study, the experimenter finds his mode of procedure limited in many respects by accidental peculiarities of this sense and of the apparatus at his disposal. He may then choose what of the four problems he will study, or he may find even this determined by the accidental peculiarities. He is, however, free to choose his method of evaluation: and the basis of his choice is nothing but the degree of accuracy which he desires in his results or, what generally comes out to the same thing, the amount of labor which he is willing to spend on them. The one exception is the case in which some circumstance limits him to the use of only two stimuli, for then it limits him also to the *m.* of *r. w. c.*[1]

[1] The MSS. of this articlewas received April 17, 1904.—Ed.

STUDIES IN THE INFLUENCE OF ABNORMAL POSITION UPON THE MOTOR IMPULSE.

BY DR. CHARLES THEODORE BURNETT.

I. The Japanese Illusion and the Mirror Illusions.

The experiments that form the basis of the following report are the first of a series designed to open a new approach to the psycho-physics of the motor impulse, by way of the modifications that occur in the control of a limb when placed in unusual positions. The particular investigations of this paper are concerned with the direction of the impulse as shown in the control of the fingers when the hands are placed in unusual positions; concerned, in other words, with the ability to move a given finger at command.

We shall consider first the Japanese illusion. It occurs when, with arms crossed, the hands are clasped thumbs down, and are turned thumbward till they point up. If an onlooker, pointing to one of the fingers, asks the man thus situated to move it, the latter is frequently unable to do so, moving, if anything, some other finger. Of the experimental conditions, it need be said only that the hands were unclasped after every movement in many series; and that either the wrists and neighboring parts of the arms were concealed by a cloth wrapped about them, or the observer was covered with a sort of apron fastened about the neck and having an opening with edges drawn together by an elastic cord. Through this opening the clasped fingers could be thrust while most of the remainder of the hand was concealed. The purpose of these precautions was to preserve as long as necessary an illusion that yields pretty readily to experience of the situation. In the first form of this experiment the finger to be moved was indicated visually to the observer by pointing, whereupon the latter was to make the movement as quickly as possible. No attempt was made to eliminate the possible influence of the crossing of hands, whether right over left or left over right. But here and throughout, except in a single instance noted in its place, each finger (including thumbs) was as often required to move as is any other. The order of choice was irregular.

TABLE I.

JAPANESE ILLUSION.

	Date.	No. Exper.	Total Errors.	Symmet. Opposite.	Pairs.	Next Finger Opposite.	Next Finger Same.	Miscell. Opposite.	Miscell. Same.	Right.[2]	Left.[2]
Baldwin.	9 Oct.[1]	40	22	15	5	1	1				
	16 "	60	14	5	5		4			7	7
	23 "	20	1				1				1
Emerson.	28 "	60	29	16	2	4	5		2	14	15
	4 Nov.	80	35	19	1	8	7			8	27
Kleinknecht.	3 Dec.	40	19	15		4				8	11
Miller.	28 Oct.	60	23	14	1	6	2			3	20
	4 Nov.	60	15	12		1	2			2	13
Rouse.	16 Oct.	40	20	13		4	1	2		3	17
	23 "	40	19	16		3					19
	30 "	60	25	21		1	3			3	22
Rowland.	28 "	40	14	7		2	4		1	5	9
	4 Nov.	60	26	16		4	6			4	22
Totals.		620	240	154	9	37	35	2	3	57	183

RESULTS.

1. The disturbance in the direction of the motor impulse is rather large, as shown by the proportion of errors to the number of experiments.

2. In some cases adjustment to the abnormal position is not long in occurring. Miller and especially Baldwin show this. The latter, in a short test made after those recorded, showed entire readjustment.

3. The erroneous movements far more frequently occur in the finger symmetrically opposite than in any other. Following at a long distance are erroneous movements in the next fingers opposite and the next fingers on the same side. Errors of other types are few and scattering.

4. The errors occur far more frequently when the movement is to be made with the left hand than when it is to be made with the right. No observer shows a contrary tendency, though some exhibit none.

[1] Omitted from totals because fingers were not equally employed.

[2] In these columns throughout the tables are recorded the number of *failures* for the hand in question.

5. With hands in normal position, palms up, there are practically no errors.

6. When a finger is touched as well as pointed out, there is almost never an error.

7. When the eyes of the observers were closed and the finger to be moved was indicated by naming it, the following results were obtained:

	No. Exp.	Errors Symmet. Opp.	Errors Miscell.
Baldwin.	40	0	5
Emerson.	30	0	0
Kleinknecht.	30	0	4
Miller.	60	9	4
Rouse.	40	8	5
Rowland.	40	7	0

Here is a great reduction in the illusion. That this is not due in all cases merely to a growing familiarity with the situation is shown by the results of Emerson. Work in connection with this illusion previous to the present test had not occurred for four weeks. The day after this test the old conditions were restored and the illusion was back as strong as ever. The results of Miller show about as much illusion as in one of the sets of experiments recorded against him in Table I. Hence it seems possible that his confusion lay in his kinæsthetic knowledge of where his fingers were located. This confusion nearly disappeared for him when the hands were laid palm downward on the table pointing away from the body, while the other conditions of auditory stimulus and closed eyes were maintained. The presence of a weakened illusion with Rouse is perhaps connected with the fact that he visualized his hands. This Miller did not do.

Conclusions.

1. It appears from (6) and (7) above that the illusion lies in the visual, not in the kinæsthetic, experience of abnormal position, though one observer presents a possible exception.

2. The large excess of erroneous movements made with the finger symmetrically opposite shows how large a factor in the direction of the motor impulse is the visual peculiarity of a given finger. The motor current appropriate to that peculiarity is

started; but the element contributed by visual position diverts it to the wrong hand.

3. Why, it may be asked, does the visual experience of abnormal position divert the current far more frequently to a finger on the opposite hand than to another on the same hand? A glance at the hands in the position appropriate to the illusion will show that the roots of the fingers lie on the side opposite to the arm to which they belong; that the right-hand fingers point *from* the left *to* the right, and the left-hand fingers *from* the right *to* the left. This is just the reverse of what is true when the hands are clasped in the usual way. Going upon the basis of procedure in the normal situation, the observer in the unusual position moves the finger that *really* lies on the side on which the given finger *appears* visually to lie. This process of reasoning is, of course, wholly in the mind of the experimenter. For the observers the process is so mechanical that they are obliged to consider seriously when asked how they obey a given command. The usual reply is that they simply *see* what is wanted and then do it. The movement appears to follow directly upon the visual cue. It is not to be denied that the observers feel in some measure confused in this unusual position and occasionally feel almost unable to move any finger. The attitude of hasty attention that favors so many geometrical-optical illusions seems to be the best one in the present instance. The confusion soon yields far enough to permit a movement that is not merely spasmodic.

4. Why any correct movements at all? They become possible by a new adjustment to the new position—a recognition that the right-hand fingers point *from* the left and the left-hand fingers *from* the right. Some effort may be required to substitute the new visual cue for the old, and, when effort fails, habit steps into control. The new adjustment may be but partly successful and a wrong finger moved on the correct side. The mistakes of this sort give the second maximum of errors.

5. Is there any psychological account to be given of the second focus of errors in Table I., viz., in movements of the fingers *next* to the correct one whether on the same or the opposite hand? This is possible if in some way it could be shown that

the two fingers resembled each other. The middle and ring fingers resemble each other more than do any other two adjacent members, and the thumb and forefinger least of all. Here is the way in which the errors were distributed among adjacent pairs:

Thumb and fore-finger = 19	Middle and ring = 25
Fore- and middle-finger = 19	Ring and little = 10

There is no ground here for basing the error wholly on mutual resemblance, though to this it may at times be due. We seem driven to a purely physiological account.

6. That the second greatest tendency to error should involve moving a finger *next* to the correct one, while yet this tendency cannot be due in general to resemblances, suggests that what would be the habitual course of the motor impulse is preventing somehow a wider divergence in its actual course. It does not appear otherwise why the errors should not be more widely distributed.

7. The source of superiority in control of the right over that of the left hand does not at once appear. For movements so simple in the normal position such a difference does not exist.

Table II.

Japanese Illusion.

Left hand crossed over right.

	No. Exper.	Errors.	Symmet. Opposite.	Next Finger Opposite.	Next Finger Same.	Right.[1]	Left.[1]
Emerson.	40	23	15	8		14	9
Kleinknecht.	40	14	14			6	8
Rouse.	40	5	3	2			5
Rowland.	40	16	11	3	2	2	14
Totals.	160	58	43	13	2		
Right hand over left.							
Emerson.	40	19	14	5		6	13
Kleinknecht.	40	17	16	1		3	14
Rouse.	40	12	10	1	1	5	7
Rowland.	40	18	14	3	1	5	13
Totals.	160	66	54	10	2		

[1] Totals henceforth are not recorded in these columns because of the divergence among observers.

One objective factor not thus far controlled might be involved in this result. The abnormal position studied here can be obtained by crossing right hand over left or left over right. There is frequently a difference in strain in the two wrists; and the hand and wrist of more intense sensation might possibly be under better control. So much is at least suggested by the lessening of error when the control was of the auditory-kinæsthetic type. Or we might indeed find the reverse to be true. Table II. gives us the results of experiments similar to the preceding, but designed to show the effects, if any, of the method of crossing. Baldwin is omitted in this test because the illusion had nearly disappeared for him.

Results.

1. There are still many more failures in case of a commanded movement with the left hand than with the right. There is but one observer whose results suggest any influence of the method of crossing. No simple relation is apparent between the presence or absence of a feeling of strain, as reported by the observers, and this particular tendency to error. So the cause must still be sought.

2. The distribution of errors is like that in Table I, except that all scattering errors have disappeared and very few are found in the next finger on the same side.

Mirror Experiments.

In the following sets of experiments the abnormal position was attained by the use of a mirror, occasionally of two. The mirror space inverts the spaces of the real world in a direction perpendicular to the plane of the mirror; so that fingers in front appear to be in the rear, and those to the right lie apparently on the left; and *vice versa*. A direct view of the fingers was prevented by a broad collar of cardboard. After a few of these experiments had been made it was thought best, to the end of preserving the illusions in force, that the observers either close their eyes or look away after noting the finger to be moved, and then complete the movement. They were forbidden, however, to develop any new sources of information after closing the eyes. The general conduct of the experiments was as before except that the thumbs were not used, since in some positions they could not be conveniently interlocked with neighboring members.

The results are so arranged in the tables as to show the extent to which the errors follow the mirror reversal. To illustrate — when the hands are clasped palms up and the line of the interlocked fingers is perpendicular to the plane of the mirror, the forefingers which are really farthest from the body will in the mirror space be nearer the real body, while the little fingers, which are really nearer, will in the mirror space be farther away. If now the observer be directed by pointing to move a forefinger and he thereupon move the little finger or ring finger, that error would show that the movement followed upon the visual cue, the mirror space being regarded not otherwise than as real space. An erroneous movement of fore- or middle-finger for either ring- or little-finger will be classified thus; not so fore- for middle-finger or *vice versa*, nor ring- for little-finger. In other words, the eight fingers, being interlocked, are divided by a median line into two sets. The finger wrongly moved must not lie in the same half with the finger pointed out, if the error is to be classed as following the mirror reversal. If it does lie in the same half, one cannot say that the error is *not* due to the same cause. But by arbitrarily limiting the evidence to the more striking cases, a preponderance of these will make our conclusions much stronger.

By way of introduction we may notice here the character of the errors occurring in the attempt to trace with a pencil the outlines of figures that cannot be seen directly but only as reflected in a mirror. Henri [1] reports such experiments. The present results confirm his in all essential respects. (1) When asked to trace the outlines of a rectangle whose side was parallel to the plane of the mirror, all seven observers succeeded easily, though in four a false start in the opposite direction was noted when they began to trace the lines perpendicular to the mirror. This is the space relation that the mirror reverses, and

[1] 'Revue generale sur le sens musculaire.' V. Henri. *Année Psych.*, V., pp. 504–8.

the wrong movement thus conformed to the visual cue. (2) The tracing of the diagonals in this position was almost, if not quite, an impossibility for four observers, movements being made *at right angles* to the one desired, *i. e.*, *in a direction conforming to that of the reflected line*. For the other three observers the movement was easy enough except at high speeds, where an occasional error similar to the foregoing betrayed the tendency usually held in check by the successful adjustment to the new conditions. The reaction seems to involve the association of a new kinæsthetic complex with a given visual impression as soon as the reflected image shows that the movement is being made in the right direction. These two types of reaction suggest two types of brain function — the one where the organic paths already formed chiefly determine the direction of the motor impulse, and the presence of an element common to the new and the old is sufficient to draft the entire current into the old channels; while in the other type *all* the new elements contribute in determining the direction of the motor impulse. (3) When the corner of the rectangle was toward the mirror, the difficulty in drawing sides and diagonals respectively was reversed; but in kind was like the earlier error. (4) If a more complicated figure, such as a six-pointed star, be set for outlining, the difficulty increases, though in the case of one or two observers all

TABLE III.

MIRROR FRONT. SUPINATION. FINGERS CROSSED IN PALMS.

	No. Exper.	Errors.	Following Mirror.		Next Finger Same.	Miscell. Same.	Symmet. Opposite.	Next Finger Opposite	Miscell. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.							
Baldwin.	48	25	22	1			2			11	14
Emerson.	72[1]	33	19		10		4			17	16
Kleinknecht.	24	2			2					1	1
Miller.	72[1]	45	30	4	8		2	1		22	23
Rouse.	40	34	21	5	7		1			15	19
Rowland.	88[1]	26	7	1	4		10	4		16	10
Totals.	344	165	99	11	31		19	5			

[1] Results of several days combined. Tendency in the separate series the same as that in total except in Rowland's failures to right and left. The excess of right-hand failures is due to the results obtained at a single sitting.

the new adjustments desired throughout this experiment were made with ease.

TABLE IV.

MIRROR FRONT. PRONATION. FINGERS CROSSED OVER BACKS OF HANDS.

	No. Exper.	Errors.	Following Mirror.		Next Finger Same.	Symmet Opposite.	Next Finger Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.					
Baldwin.	40	9	6		2	1		4	5
Emerson.	40	2			1	1			2
Kleinknecht.	40	3	2		1			1	2
Rouse.	40	8	1		5	2		4	4
Rowland.	40	11	1	2	1		7	1	10
Totals.	200	33	10	2	10	4	7		

Of the special conditions governing the experiments of Tables III. and IV. it need only be said that in both cases the line of the fingers was kept perpendicular to the plane of the mirror, so far as comfort would allow. The fingers were so clasped in the work of Table IV. that the left forefinger always came next to the body; while for the experiments of Table III. the left little finger occupied that place, except in a part of the tests with Emerson and Rowland.

RESULTS.

1. The disturbance in the direction of the motor impulse is very markedly shown in Table III., though one observer is almost unaffected.

2. The influence of the visual factor appears in the fact that more than two thirds of the errors follow the mirror reversal.

3. There is no prominent tendency toward an excess of failures in one hand over the other. For most of the observers it is quite absent.

4. Nearly one fifth of the errors consists in a movement of a finger of the opposite hand. This is not due to any inversion effected by the mirror, so far as one can see.

5. The bulk of all the errors not directly accounted for by the mirror reversal consists in the wrong movement of the symmetrically opposite finger and of the next finger on the same side.

6. Under the conditions of Table IV. the illusion has greatly decreased. It is to be especially noted that the causes operative

in the former case to produce errors that the mirror reversal could not *directly* account for are now much more effective. The mirror errors are about one third the total, while in Table III. they are more than two thirds.

7. The errors, barring those of a single observer, show no tendency to concentration in either hand.

TABLE V.

TWO MIRRORS, IN FRONT AND BELOW. SUPINATION. FINGERS CLASPED OVER BACKS OF HANDS.

	No. Exper.	Errors.	Following Mirror.		Next Finger Same.	Next Finger Opposite.	Symmet. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.					
Baldwin.	40	8	6		1	1		3	5
Emerson.	40	16	15		1			10	6
Kleinknecht.	40	16	11		5			8	8
Rouse.	40	21	10	2	1	1	7	5	16
Rowland.	40	12	4	1	6		1	2	10
Totals.	200	73	46	3	14	2	8		

TABLE VI.

TWO MIRRORS, IN FRONT AND BELOW. PRONATION. FINGERS CLASPED IN PALMS.

	No. Exper.	Errors.	Following Mirror.		Next Finger Same.	Next Finger Opposite.	Symmet. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.					
Baldwin.	40	16	10	1	1	1	3	6	10
Emerson.	40	11	10				1	5	6
Kleinknecht.	40	8	5		1		2	6	2
Rouse.	40	26	6	7	3		10	7	19
Rowland.	40	9	4				5	4	5
Totals.	200	70	35	8	5	1	21		

8. What is the cause for the great difference in the amount of illusion between Tables III. and IV.? A suggestion readily occurring would attribute it to the greater ease of recognizing the fingers as individuals when they are clasped over the backs of the hands. In support of this view may be cited the results of some experiments performed on Rouse. The conditions differed from those of Table IV. in this, that the fingers were covered with paper rolls that largely concealed their individual

characteristics. In the same number of experiments his errors were three times as many; and more than two thirds of these followed the mirror reversal. And these results were obtained a week after the former, so that the former results do not appear to have been due to practice.

But we shall find in Tables V. and VI. evidence to show us that the positions of pronation or supination can importantly modify the illusion; and so to these factors in the present case we shall have to allow some influence.

For the experiments of Table V. two mirrors were used, at right angles to each other; one flat, the other perpendicular to the median plane of the observer. The fingers were clasped in the manner indicated by the tables and directed downward, so that the observer looking into the upright mirror could see a reflection of the image of the flat mirror. A cloth over the top of the upright mirror prevented a direct reflection of the hands in it. The image as seen by the observer reversed the real position of pronation or supination and also, as in the preceding experiments, the halves of each hand. In all essentials the conditions of Table V. resemble those of Table IV., the conditions of Table VI. those of Table III., except in pronation and supination.

Results.

1. The total amount of errors is greater in the position of supination than of pronation. In other words, the pronated hand appears to be under better control. The results of Miller have to be excluded from Table III. in order to make a justifiable comparison. Specifically stated, the errors for Table V., (supination) are more than double those in Table IV. (pronation). A comparison of Tables III. and VI. yields similar results.

2. Again the errors find a second center in Table V., in the fingers next to the one indicated and on the same side; while in Table VI., this second center is rather in the finger symmetrically opposite.

3. The tendency of the movement to follow the visual cue is still evident.

TABLE VII.

A. MIRROR FRONT. LEFT PALM UP. RIGHT DOWN.

	No. Exper.	Errors.	Following Mirror. Same Hand.	Following Mirror. Opposite Hand.	Following Mirror. Symmet. Opposite.	Next Finger Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
Baldwin.	48	28	16	1	5	5		1	13	15
Boswell.	72[1]	31	20		5	3	2	1	5	26
Emerson.	40	17	11		2	2			6	11
Holt.	24	13	12			1			2	11
Kleinknecht.	24	4	3			1			1	3
Rouse.	48	35	22		1	10	2		17	18
Rowland.	48	18	9			3	6		3	15
Miller.	48	26	16		4	4	2		9	17
Totals.	352	172	109	1	17	29	12	2		

B. AS ABOVE, EXCEPT RIGHT PALM UP, LEFT DOWN.

	No. Exper.	Errors.	Following Mirror. Same Hand.	Following Mirror. Opposite Hand.	Following Mirror. Symmet. Opposite.	Next Finger Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
Baldwin.	48	15	8	1	2	2	2		11	4
Boswell.	72[1]	37	20	1	5	11			11	7
Emerson.	40	13	11			2			9	4
Holt.	24	15	13		1	1			7	8
Kleinknecht.	24	5	1		2	2			4	1
Rouse.	48	31	21		4	4	2		19	12
Rowland.	48	29	11	1	9	3	3	2	14	15
Miller.	48	21	9		6	4	2		13	8
Totals.	352	166	94	3	29	29	9	2		

TABLE VIII.

A. MIRROR FRONT. DIRECTION OF FINGERS OPPOSITE. LEFT PALM UP, RIGHT DOWN.

	No. Exper.	Errors.	Following Mirror. Same Hand.	Following Mirror. Opposite Hand.	Following Mirror. Symmet. Opposite.	Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
Baldwin.	40	25	23	1	1					8	17
Emerson.	40	18	17					1		8	10
Kleinknecht.	40	7	4					3		7	
Rouse.	40	22	13	1	3	4		1		8	14
Rowland.	40	18	12	1	1	2		2		3	15
Totals.	200	90	69	3	5	6		7			

B. AS ABOVE, EXCEPT RIGHT PALM UP, LEFT DOWN.

	No. Exper.	Errors.	Following Mirror. Same Hand.	Following Mirror. Opposite Hand.	Following Mirror. Symmet. Opposite.	Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
Baldwin.	40	16	13		1			2		9	7
Emerson.	40	19	16		2			1		8	11
Kleinknecht.	40	16	14			2				2	14
Rouse.	40	26	11		12	3				11	15
Rowland.	40	21	15	2	3			1		11	10
Totals.	200	98	69	2	18	5		4			

[1] These results are combined from the work of two days, agreeing in tendency.

Table IX.

A. Mirror Front. Caps on Palm-down Fingers. Left Palm Up, Right Down.

	No Exper.	Errors.	Following Mirror. Same Hand.	Following Mirror. Opposite Hand.	Following Mirror. Symmet. Opposite.	Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
Baldwin.	40	22	15		2	3		2		8	14
Emerson.	40	8	8							1	7
Kleinknecht.	40	16	12			4				16	
Rouse.	40	24	19		3	2				13	11
Rowland.	40	9	8			1				5	4
Totals.	200	79	62		5	10		2			

B. As Above, Except Right Palm Up, Left Down.

	No Exper.	Errors.	Same Hand.	Opposite Hand.	Symmet. Opposite.	Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
Baldwin.	40[1]	22	18			3			1	12	10
Emerson.	40[1]	13	11			2				9	4
Kleinknecht.	40	13	7		3	3				4	9
Rouse.	40	24	18		1	4		1		8	16
Rowland.	40	23	20			1		2		9	14
Totals.	200	95	74		4	13		3	1		

Table X.

A. Mirror Front. Caps on All Fingers. Left Palm Up, Right Down.

	No. Exper.	Errors.	Following Mirror. Same Hand.	Following Mirror. Opposite Hand.	Following Mirror. Symmet. Opposite.	Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
Baldwin.	40	18	16		1	1				7	11
Emerson.	40	18	17			1				8	10
Kleinknecht.	40	13	13							11	2
Rouse.	40	18	15		2	1				3	15
Rowland.	40	14	9			2		3		1	13
Totals.	200	81	70		3	5		3			

B. As Above, Except Right Palm Up, Left Down.

	No. Exper.	Errors.	Same Hand.	Opposite Hand.	Symmet. Opposite.	Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
Baldwin.	40	20	17		2	1				9	11
Emerson.	40	19	19							15	4
Kleinknecht.	40	28	20			8				10	18
Rouse.	40	27	24		1	2				12	15
Rowland.	40	18	14	1	3					12	6
Totals.	200	112	94	1	6	11					

[1] Results obtained at two sittings but accordant.

A further test of the influence of pronation and supination, as well as of the visual position of the members, was devised in these new experiments.

The conditions belonging to them are the following: The line of the crossed fingers is again perpendicular to the mirror plane; but the clasped hands are one in the position of pronation, the other in that of supination. The differences among these four sets are the result of an attempt to eliminate the factors that might be responsible for the tendency to mass failures in a given hand. So in Table VIII. care was taken that the fingers of the supinated hand should not be allowed to curl up, as they are inclined to do; but should maintain their direction as steadily as do the fingers of the other hand. In Table IX. the attempt was made to check the one-sidedness that might well grow out of the greater ease in recognizing fingers whose backs are in view, by covering those fingers with caps made in the form of paper tubes. And these coverings were extended to the fingers of both hands in Table X., as equalizing most fairly the conditions for both. Here also the effort was made to maintain the opposition in direction of the fingers. Finally, the same number of experiments was performed with each hand in a given position.

RESULTS.

1. There appears at first sight to be no simple relation between the conditions studied and the tendency to mass failures in one hand. Looking further, however, we find that *while frequently there is no such tendency*, yet when it does occur, *the drift is to the supinated hand*. Cf. Table XI. One observer is a definite and consist exception.

2. This must be at least relatively independent of ease in recognizing the fingers, since it occurs even when the caps are on both hands.

3. Tables VII. and VIII. show a massing of erroneous movements on the symmetrically opposite finger, as well as on the next fingers of both the same and the opposite sides. This tendency to a confusion of hands cannot be accounted for as a case of mirror reversal. In Tables IX. and X., there is no such drift upon the symmetrically opposite finger, but the next fingers on the same side are chiefly favored.

4. In general, the distribution and the significance of the errors here agree with Tables III. and IV. A test of the fingers in this position under the condition of direct vision showed practically complete control.

Table XI.

Summary of Tables VII.–X.

	Table VII.			Table VIII.			Table IX.			Table X.		
	Right.	Left.	Comment.	Right.	Left.	Comment.	Right.	Left.	Comment.	Right.	Left.	Comment.
Baldwin.	13	15		8	17	l. up	8	14	l. up	7	11	
	11	4	r. up	9	7		12	10		9	11	
Emerson.	6	11	l. "	8	10		1	7	l. "	8	10	
	9	4	r. "	8	11		9	4	r. "	15	4	r. up
Kleinknecht.	1	3		7		r. down	16		r. down	11	2	r. down
	4	1		2	14	l. "	4	9	l. "	10	18	l. "
Rouse.	17	18		8	14	l. up	13	11		3	15	l. up
	19	12	r. "	11	15		8	16	l. down	12	15	
Rowland.	3	15	l. "	3	15	l. "	5	4		1	13	l. "
	14	15		11	10		9	14	l. "	12	6	r. "
Boswell.	5	26	l. "									
	21	16	r. "									
Holt.	2	11	l. "									
	7	8										
Miller.	9	17	l. "									
	13	8	r. "									

Against the significant differences in Table XI. is indicated the hand that was supinated. All the observers, except Kleinknecht agree in concentrating failures, if anywhere, in the hand whose palm is up. There are but two exceptions in the twenty-two cases. Kleinknecht is just as constant in the opposite direction and furnishes a larger number of significant cases than does any other observer. Why the cause operating in the other observers should produce intermittent effects does not so far appear.

The conditions prevailing in these new experiments were calculated to increase yet more the influence of the abnormal visual position of the fingers. Two mirrors were set together at an angle of about 90°. The observer sat over against the apex of the angle thus formed, and his clasped hands lay in the region embraced by the angle of the mirrors. The manner of clasping the hands is shown in the table. The fingers were

Table XII.

A. Two Mirrors, Right-left, Caps on all Fingers, Left Palm Up, Right Down.

	No. Exper.	Errors.	Following Mirror. Front-back and Right-left. Symmet. Opposite.	Following Mirror. Front-back and Right-left. Miscell.	Following Mirror. Front-back Only	Following Mirror. Right-left Only. Next Finger Opposite.	Following Mirror. Right-left Only. Miscell. Opposite.	Next Finger Same.	Right.	Left.
Baldwin.	40	32	11	1	3	16		1	13	19
Emerson.	40	38	20		1	15		2	18	20
Kleinkuecht.	40	40	9	4		20	7		20	20
Rouse.	40	36	16	5		14	1		16	20
Rowland.	40	32	10		3	15	1	3	12	20
Totals.	200	178	66	10	7	80	9	6		

B. As Above, Except Right Palm Up, Left Down.

	No. Exper.	Errors.	Symmet. Opposite.	Miscell.	Front-back Only	Next Finger Opposite.	Miscell. Opposite.	Next Finger Same.	Right.	Left.
Baldwin.	40	29	9	1	11	7		1	17	12
Emerson.	40	40	20	3	2	15			20	20
Kleinknecht.	40	39	18	3	1	16		1	20	19
Rouse.	40	39	22	3	3	9	2		19	20
Rowland.	40	34	17	1	11	4	1		15	19
Totals.	200	181	86	11	28	51	3	2		

disguised with the usual caps. The observer looked into the right mirror to see the reflection of the image as originally given in the left mirror. The second mirror gave a right-left as well as a front-back reversal of the real position of the fingers. The hands were so placed that but little could be seen of any primary images.

Results.

1. The very large proportion of errors shows the strength of the illusion.

2. This amount is so great that there is little chance to mass errors in either hand. The three cases where there is such a tendency conform to the chief type in Table XI.

3. The predominance of wrong movements is in the symmetrically opposite finger and those fingers in the opposite hand that lie next to the indicated finger. These are exactly the places where one would expect the wrong movement to be made. Where a new adjustment is made for the front-back reversal, the observer knows where on his hand the finger lies that he would move, but he mistakes the hand. Where a new

adjustment is affected for neither reversal, the observer knows where in a given half of the hand the movement should be made, but he confuses both halves and hands.

Table XIII.

A. Mirror Front. Back of Left Hand Against Palm of Right. Caps on All Fingers.

	No. Exper.	Errors.	Following Mirror.			Next Finger Same.	Miscell. Same.	Right.	Left.
			Next Finger Opposite.	Symmet. Opposite.	Miscell. Opposite.				
Baldwin.	40	8	3			4	1	2	6
Emerson.	40	9	5	1		3			9
Kleinknecht.	40	17	9	1	1	6		10	7
Rouse.	40	12	3	2	4	3		5	7
Rowland.	40	21	15	2	3	1		1	20
Totals.	200	67	35	6	8	17	1		

B. As Above, Except Reversed Relation of Hands.

	No. Exper.	Errors.	Next Finger Opposite.	Symmet. Opposite.	Miscell. Opposite.	Next Finger Same.	Miscell. Same.	Right.	Left.
Baldwin.	40	9	3			4	2	5	4
Emerson.	40	14	12			2		2	12
Kleinknecht.	40	19	12	4		3		5	14
Rouse.	40	16	5	4	2	5		8	8
Rowland.	40	15	8	6		1		3	12
Totals.	200	73	40	14	2	15	2		

Table XIV.

A. Mirror ∠ 20° Right.

	No. Exper.	Errors.	Following Mirror.			Next Finger Same.	Miscell. Same.	Right.	Left.
			Symmet. Opposite.	Next Finger Opposite.	Miscell. Opposite.				
Baldwin.	40	21	9	3		8	1	11	10
Emerson.	40	23	9	8		6		15	8
Kleinknecht.	40	22	18	2		2		17	5
Rouse.	40	37	34	1	1	1		19	18
Rowland.	40	23	14	5		4		5	18
Totals.	200	126	84	19	1	21	1		

B. Mirror ∠ 20° Left.

	No. Exper.	Errors.	Symmet. Opposite.	Next Finger Opposite.	Miscell. Opposite.	Next Finger Same.	Miscell. Same.	Right.	Left.
Baldwin.	40	24	20	3		1		10	14
Emerson.	40	21	18	1		2		12	9
Kleinknecht.	40	15	13			2		10	5
Rouse.	40	18	17			1		13	5
Rowland.	40	20	7	10	1	2		6	14
Totals.	200	98	75	14	1	8	1		

4. The difference in the amount of errors for the front-back and the right-left illusion indicates that adjustment to the former is much more easily effected. Our practical use of mirrors helps us to overcome the first illusion. The second sort of experience is relatively novel.

5. The number of errors that cannot be directly accounted for by the influence of the visual position is nearly negligible.

6. The right-left illusion is stronger when the left palm is up; the front-back illusion, when the right palm is up.

The conditions of the experiments in these tables were arranged to show the general principle of visual control, hitherto copiously illustrated, in yet further ways. For Table XIII., the hands are placed back against palm, the fingers interlocked, with the little fingers on the outside, and the line of the fingers parallel to the plane of the mirror set up in front. In the experiment of the other table the fingers were clasped palm up, the line parallel to the median plane of the body. The mirror was placed at an angle of about 20° with the median plane. This angle was made as small as possible consistent with a convenient view on the part of the observer. The arrangements in both these cases were to reverse in appearance the position of the hands with reference to each other.

It should be said of the first set of experiments that the position chosen was so difficult a one that it was nearly impossible to keep each set of fingers in lines parallel to each other and to the mirror. Such displacements tended to produce reversals among the fingers of a single hand. This probably accounts, in part at least, for the erroneous movements made with the correct hand, though these are certainly not in excess of similar errors in Table XIV., where such an explanation is not possible.

The caps were used in Table XIII. because the clasped fingers did not symmetrically correspond, and the resulting differentiation, if seen, might lessen the illusion. This reason did not hold in Table XIV.

Results.

1. The expected illusion occurs in both cases.

2. The heaping of errors in Table XIV. on the finger sym-

metrically opposite, and in the other table upon those fingers of the opposite hand that lie next to the indicated finger, is due in both cases to the same cause, viz., their occurrence in parts of the opposite hand spatially corresponding to the indicated finger.

3. The noticeable tendency in Table XIV. to a movement of the next fingers, either on the same or the opposite side, confirms earlier results; and we have already seen (conclusion 5 under Table I.) that the error cannot be set down wholly to resemblance.

Table XV.

Summary.

					Supination.								Pronation.			
	Table I.		Table II.		Table III.		Table V.		Table XIII.		Table XIV.		Table IV.		Table VI.	
	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
Baldwin.	7	8			11	14	3	5	2	6	11	10	4	5	6	10
Emerson.	22	42	14	9	17	16	10	6		9	15	8		2	5	6
Kleinknecht.	8	11	7	9	1	1	8	8	10	7	17	5	1	2	6	2
Rouse.	6	58		5	15	19	5	16	5	7	19	18	4	4	7	19
Rowland.	9	31	2	14	16	10	2	10	1	20	5	18	1	10	4	5
Miller.	5	53			22	23										
Baldwin.									5	4	10	14				
Emerson.			6	13					2	12	12	9				
Kleinknecht.			3	14					5	14	10	5				
Rouse.			5	7					8	8	13	5				
Rowland.			5	13					3	12	6	14				

Table XV. presents a summary view of the failures to make the correct movement as these appear in the right and the left hands. All the tables are included where both hands agree in pronation or supination. This particular condition, as it is found in the unsymmetrical relation of the hands, has already been discussed in connection with Table XI. Here the results for Tables I., II., XIII. and XIV. fall into one group, as being concerned with an illusion that tended to throw the movement over to the opposite hand; while the remaining results were obtained where the illusion tended to divert the movement to the opposite side of the same hand. Tables III., V., XIII. and XIV. are concerned with positions of supination, and Tables IV. and VI. with pronation. For Tables I. and II. the position is a combination of both.

RESULTS.

1. The tendency to mass errors, where it occurs at all, shows a drift toward the left. Of twenty-six instances, twenty are of this type and six of the opposite type. Four of the latter are confined to one table (Table XIV.).

2. The existence of such a tendency seems to be connected with the illusion that throws the erroneous movement over to the opposite hand. Twenty-one instances occur in Tables I., II., XIII. and XIV., where the illusion is of this type. The remaining five are scattered through the other four tables.

TABLE XVI.

COMPILED FROM EXPERIMENTS EMBODIED IN TABLES VII. AND X.

		Erroneous Move.				Failures.			
		2	3	4	5	2	3	4	5
Baldwin.	Right.	4	1	21	5	12	13	2	6
	Left.	6	11	24	7	15	17	8	6
Emerson.	R.	2	4	21	10	15	17	5	2
	L.			24	13	15	18	1	1
Kleinknecht.	R.	10	10	6	1	4	1	12	13
	L.	9	15	2	7	10	4	10	10
Rouse.	R.	10	8	11	13	12	5	10	7
	L.	18	16	7	10	16	5	19	19
Rowland.	R.	15	17	4	2	5		12	10
	L.	21	6	3	3	10	4	14	16

The numbers at the heads of the columns indicate the fingers in order, beginning with the forefinger.

The interesting questions naturally occur whether there is any tendency (1) to make more erroneous movements with one finger than with another; and (2) whether more failures occur similarly. To answer these questions the eight hundred experiments of Tables VII. and X. were worked over to discover the distribution of errors and failures among the fingers. Table XVI. presents the details.

RESULTS.

1. The several observers do show a preference among the fingers in erroneous movements and also a massing of failures but they disagree with each other.

2. The right and left hands show a somewhat remarkable agreement in distribution for any one observer. Out of the

forty cases, there appear to be but five where the relative distribution in the two hands is markedly different.

Table XVII.

To Show Drift of Errors Toward Thumb or Little Finger.

	Table III.		Table V.		Table VII.		Table VIII.		Table IX.		Table X.		Table XII.	
	I.	V.	I.	V.	I.	V.	I.	V.	I.	V.	I.	V.	I.	V.
Kleinknecht.	1	1	15	1	4	4	19	4	17	9	27	14	40	12
Rouse.	20	13	12	2	46	15	17	16	30	14	25	17	24	13
Rowland.	13	3	10	1	22	13	27	8	23	9	25	4	29	10
Totals.	34	17	37	4	72	32	63	28	70	32	77	35	93	35
Emerson.	12	17	5	11	10	16	1	34	4	17	5	32	16	22
Baldwin.	2	21	6	2	13	23	14	25	19	23	6	29	18	23
Totals.	14	38	11	13	23	39	15	59	23	40	11	61	34	45

Of the Roman numerals at the heads of the columns I. means thumb and V. little finger.

Table XVII. presents a new analysis of the results of the tables summarized therein. All the erroneous movements that were not made with the symmetrically opposite finger were classified on the principle of their occurrence either thumbward or toward the little finger from the indicated finger or its symmetrically opposite fellow. The observers were distributed so evenly between the two classes that they are separated in the table into two groups. Those of the former tables are included in this survey that showed the largest amount of errors falling elsewhere than on the finger symmetrically opposite.

Results.

1. The observers fall into two opposing groups, each showing a very consistent tendency of its special type, and one a very large one.

2. Considering each observer separately, we find that in one case only is there a direct contradiction of type, while in but three cases is neutrality almost or quite complete.

In what direction are we to look for an explanation of the facts that have come forward in the course of these experiments? In the first place, there is the fact of the existence of an illusion connected with an abnormal position of the members. We found this to be due in nearly every case to the abnormal visual factors, since their removal destroyed the illusion. A single observer in the Japanese illusion seemed to show that abnormal kinæsthetic factors were involved in producing it. We have to do here with a special case of neural habit. Visual cues and, more rarely, kinæsthetic cues have become in practice the well-defined guides of movement, to such an extent, indeed, that when these become untrustworthy through a change of conditions, it is only by effort, more or less, that the movement normally connected with them is prevented from occurring.

This principle seems to be illustrated yet further in our results. It appears that adjustments seeming equally easy to both hands in normal positions are less easy for the left than for the right when the positions are abnormal, as in our experiments, though the hands agree in position; and it appears further that for the supinated hand the adjustment is also more difficult. In other words, the neural habits underlying our practice in the control of our movements are primarily adjusted to a given space relationship of members; while plasticity is greater for the right hand than for the left, and for either hand pronated than supinated, though in the latter case we must not forget that for one observer just the reverse was true. The foregoing difference between the right and the left hands seems to be in line with the greater ease in control of the right that we find in many normal movements, though in the one we have investigated that difference had disappeared, yet only to reappear, as reversion to an earlier type, under the condition of abnormal position. This greater adjustibility in one half of the brain than in the other half we can view as related to practice. A similar account is possibly justifiable for the better control of the pronated hand, though we have still to dispose of our consistent exception. One is tempted to formulate a hypothesis along the familiar lines of the 'sensory' and 'motor' types, thereby saving our main principle in this case. For example, let us make the following suppositions: (1) Less vivid sensations represent our limbs in consciousness when they are normally than when abnormally disposed. (2) In the character of the motor discharge either the nature of the incoming currents or the situation of the centers may be prepotent. If the nature of the incoming current prevails, then the less familiar the situation the better the adjustment, and vice versa; but if the situation of the centers prevails, then the more familiar the outward situation, the more correct the response. The former is the 'sensory' type, in which must be classified the single observer whose control is best over the supinated hand; while the rest of the observers belong to the latter or 'motor' type.

The tendency of the erroneous movement to be drawn toward either the thumb or the little finger, according to the type, may be due to the more habitual employment of the members that lie on a given side. The difficulty with this view is that one would expect all erroneous movements to be drawn thumbward, since that side is probably in all but rare cases the stronger. Individual tendencies to favor or fail in a given finger have probably a share in the explanation accorded to the foregoing fact.

We have reason to believe that resemblance plays some part in the drift of erroneous movements toward the finger symmetrically opposite; but the amount of this error when the fingers are disguised with caps suggests the existence of an additional factor, perhaps purely physiological. In this direction points also the prominence of the fingers next to this and to the indicated finger in wrong movements; for we found in the discussion of Table I. that resemblance as a complete account of this case was out of the question. The precise nature of this additional factor is obscure to the writer.

There is further obscurity about the connection between the prevalence of failures in a given hand and the presence of an illusion that tends to throw the movement over to the opposite hand. The strength of the evidence for such a connection we saw in Table XV.

Summary.

1. The influence of abnormal position upon the motor impulse, under the conditions of these experiments, is to change its direction in certain well-defined ways (cf. all tables).

2. There is a strong tendency to move the finger that really is where the indicated finger appears to be (cf. all tables).

3. That visual factors control the movement is shown by the disappearance of the illusion when touch is added to vision, or where vision is excluded and the stimulus is auditory. Its failure to disappear in the latter case for one observer shows that occasionally abnormal kinæsthetic factors can rise to importance (cf. discussion under Table I.).

4. There is a greater tendency to a wrong direction of the impulse if the indicated movement is to be made (1) with the left hand (Table XV.), and (2) with the supinated hand (Table XI.). A single observer out of eight is pretty consistently of the opposite type in (2).

5. This tendency to mass failures in a given hand is not due to the greater difficulty of recognizing as individuals the fingers of that hand. Cf. Tables VII.–X.

6. In the case of the Japanese Illusion, it is not due to a greater strain on one wrist than on the other. Cf. Table II.

7. The prevalence of failures in the right or the left hand seems to depend upon the conditions favoring that form of the illusion that throws the movement over to the other hand (Table XV.).

8. Individual observers are inclined to favor particular fingers in erroneous movements and to fail more frequently in control of one finger than of another; but among themselves the observers are very divergent (Table XVI.).

9. There are subordinate tendencies to move: (1) The fingers next to the indicated finger on the same hand; (2) the symmetrically opposite finger, and (3) the fingers next to the latter (cf. all tables).

10. The tendencies described in (1) and (3) above are not due to the resemblances between the correct and the wrong finger. An examination of the results in Table I. showed that the middle and ring fingers, which resemble each other most of all, were not mistaken for each other with more significant frequency than the thumb and forefinger.

11. There is a further tendency for wrong movements to be drawn toward the thumb side of the hand, in the case of three observers, and toward the little finger in the other two (Table XVII.).

12. The existence of the illusion is based on the law of neural habit. Our habitual dependence upon the visual cue in controlling our movements leads us astray when that cue no longer truly represents the actual situation. Failures are more frequent in the left hand because finer adjustments are less habitual to it. For that reason they are more frequent in the position of supination. The condition of the centers is prepotent in determining the reaction. In the exceptional type in which failures occur more frequently in pronation, the reaction may be viewed as determined chiefly by the incoming currents Here the less familiar the situation, the more vivid the accompanying sensations and the better the adjustment. In the former, the more familiar the situation, the more correct the response. For the other facts in this summary, I can give no explanation.

The observers taking part in the work were students in the Harvard Psychological Laboratory, one being an instructor. Of the number, two were women and six were men. I acknowledge most heartily their coöperation, as well as that of Professor Münsterberg, to whom I owe the suggestion of the problem.[1]

[1] The MSS. of this article was received April 14, 1904.—Ed.

DISCUSSION.

MIND AND BODY—THE DYNAMIC VIEW.

It requires a certain temerity to reopen the perennial problem presented by the apparent dualism of mind and body. It might appear that the last word worth saying had long since been said. It is, however, indisputable that the point of view of psychology, and, to some extent, of philosophy also, is changing. At least its language is changing and this change is distinctly favorable to a new statement, if not a solution of this problem. Accordingly, a number of valuable contributions to the literature of this subject have appeared within the last few months and the evidence that a monistic construction is desired by nearly all is cumulative. As Professor Moore says: "'Life' experience is one inclusive activity of which consciousness and habit—the psychical and the physical—are, to the last analysis, constituent functions."[1]

The present tendency on the part of the physical sciences to escape from the shackles of a material hypothesis offers a 'psychological moment' for philosophy to capture the entire forces of both combatants.

In advance attention must be called to the fact that there is no dualism in any one science, neither can there be. Biology has no body-soul controversy; neither has psychology, as such. It is only when we attempt at the same time to use both sets of criteria that dualism arises. The psychological subjective-objective dualism is a polarizing of what is and always must be a single activity into two aspects, it does not create a pair of incommensurables. It follows that this inquiry very naturally assumes the form indicated in the article entitled 'Mind and Body,' by J. Mark Baldwin.[2]

"The distinction between phenomena of mind and body, considered as distinct types of presented phenomenal change, requires the use of two distinct categories of construction, the genetic and the agenetic. Physical science it is which interprets the agenetic. Its explaining concept of cause is illustrated only and always in transformations of energy. On the other hand, is the special realm denomi-

[1] *Univ. Chicago Contrib. to Philos.*, Vol. III., 1.

[2] *Princeton Contributions to Psychology*, III., 2.

nated 'subjective.'" (The author adds that 'life processes are really genetic,' an admission which will greatly influence our attitude toward the distinction between genetic and agenetic as here defined.)

The problem is formally set in the following inquiry: 'Can we hold each set of phenomena to its own legitimate construction, and at the same time, reach a comprehensive conception of the concomitance of mind and body under which the scientific formulas appropriate to each may be given full value?' (*Ibid.*, p. 38.) This question becomes more pertinent if this author is correct (as we believe him to be) in saying 'that the present forms of the interaction theory involve a confusion of categories, due to the failure to maintain a consistent level of mental development.' (*Ibid.*, p. 39.)

"Philosophy asks: How can we think reality in one thought? In terms of our present discussion, how can body and mind, being what we have come to think them to be, live hospitably housed together in one phenomenal group of facts?" These questions are such as to arrest our fullest attention and awaken our keenest interest. This statement of the problem is most helpful and necessary to further progress, but the answer given in this place is tentative and exploratory. That a single and simple solution is ultimately expected is indicated by the italicised phrase: 'All this means that the world is, after all, one and that the categories of mental construction, derived in a process of evolution by actual treatment of the world, *cannot finally reflect processes in essential contradiction with each other.*'

This is, in fact, the criterion of congruousness, which is the last appeal and unanswerable argument of monism. The universe is an organism and contradictory categories could not have developed under a law of evolution. It is quite disappointing, therefore, especially after an appeal to an '*all-comprehensive and completely full experience*' as the content of 'æsthonomic idealism' to learn that 'psychological parallelism then is, from the point of view of science, our positive catch,' even though there is 'hope for a theory of correlation of these characters which will yield a higher adaptation in the whole realm of science.' This is the more disappointing in that the one-sided and unsatisfactory nature of a simple scientific solution has just been insisted on. But Professor Baldwin modestly refuses to expose to view the statement of the metaphysical solution designated as Æsthonomic Idealism and we are left with one foot on biological foundations and the other on psychological conclusions but with the door of hope open before us. It was inevitable that others should take advantage of this fresh statement of the problem to attempt this next step which is to land us with both feet upon some monistic construction.

It is, at any rate, certain that the correlation sought cannot be in either of the partial realms. Neither biology nor psychology, as such, can hope to afford a solution which involves both of them. The unity must be sought in a field large enough to include both.

Nevertheless, it is important for our purpose that we should get the formulated results of both to be carried up into the higher sphere. In order to secure this material a brief survey of these contiguous fields will be necessary. It must be noted in advance that the net result in each of these cases is of one kind; there are no incommensurables or incompatibles in either sphere. These appear only when the ultimate data of biology on the one hand, and psychology on the other, are attempted to be compared (and this attempt is made in terms of one or the other of these sciences) that incompatibility appears. The suggestion is obvious that the incompatibility arises from the methods and not from the content — or, in other words, from the impossibility of attempting psychological structures with biological tools, and *vice versa.*

We may also anticipate our conclusion in so far as to call attention to the way in which the problem set for us by Professor Baldwin is disposed of by the so-called 'functional school' of psychologists who save us the trouble of further discussion by denying the existence of any problem. But it is notorious that, a quarrel once on, it is a work of supererogation to show that there is nothing to quarrel about. It is when the quarrel is over that the proof of its futility is balm to our wounds.

The most concise and intelligible statement of this functional solution which the writer now recalls is that given by Professor Bawden in THE PHILOSOPHICAL REVIEW, XIII., 3, May, 1903. "Mind, as here viewed, is the totality of the functioning of matter (in so far as function may be said to imply end or purpose). The psychical is the *meaning* of the physical." "Mind is simply a collective idea for all the psychic functions of an organism — and the psychic functions are coextensive with the growth of an organism. Mind is not an entity behind the process of consciousness, it is that process itself. Mind is just as truly a growth as any other living thing." "It can be a growth only if of the nature of a process. Mental life is a continual synthetic construction. It is simply a name for the orderly continuous functioning of an organism under conditions of tension in adaptation" (p. 308).

Professor Bawden uses for the theory thus stated the title 'Functional Theory of Parallelism," to which the present writer objects on several grounds, two of which may be mentioned. First, there is an implied recognition of a material substrate — of a something of which the mental activity is a 'function.' Second, the theory is not one of parallelism except as one returns to the artificial dualism of isolated sciences. Or, to make the criticism general, the view point is that of psychology while the subject is germain to metaphysics. That this writer has himself recognized and pointed out the remedy for these supposed defects may be gathered from his article in Vol. I., No. 3, of *The Journal of Philosophy, Psychology and Scientific Methods.* "Under the name of energy, motion is now regarded as itself the essence of reality, and the idea of brute, lump matter drops away. In place of a static we get a dynamic theory of the nature of reality" (p. 63). Professor Bawden also points out the paradox insisted on by Professor Baldwin. "The solution of this apparent paradox lies in seeing that consciousness, taken apart from the organism which is conscious, is not an entity or thing or even a process; it is simply a meaning or significance. * * * After abstracting the psychical by definition, from the physical, there still cling to our psychological statements of the nature of consciousness traces of our conceptions of material objects. * * * Any thinking or speaking is a polarizing into two aspects in thought of what is an undivided unity for action. This, of course, is a methodological not an ontological dualism; hence, it is paradoxical only for him who forgets its methodological origin."

But these are passages by the way, and we may return to our own survey. As we have already seen, the difficulties in the historic attempts are due, in a very large part, to the attempt to combine in one discussion the methods and data of two or more diverse methods of investigation. Usually the biologist, who essays to discuss the relation of mind and body, is unable to complete his analysis as a biologist simply; he cannot forget that he is also a person, with experiences of his own which he feels sure are also repeated in the lives of the objective units he is discussing. He cannot divorce his *biological* discussion from its *psychological* interpretation.

This is, of course, implied in the very nature of the topic, for any discussion of the relation of mind and body implies the use of the tools or methods, as well as the data of two sciences, and the question at issue is just the inquiry whether these data are commensurable and whether these methods and tools can be employed in the same discussion. As a biologist I cannot consistently inquire as to the relations between mind and body nor can I, as psychologist, properly discuss the body, except as an image presented to sense. The question reduces to this: Is it possible for the sciences of subjective and objective phenomena, respectively, to present to philosophy the results or interpretations of their research in common terms so that the unification (the real business of philosophy) can be completed.

First as to *biology*. One of its results is the recognition of living individuals. This is no easy matter nor can the discrimination be considered complete. Colonies and social groups imply lateral connection which appears in various forms throughout the series and the existence of which we must suspect in cases which by their nature prevent us from definitely recognizing it. Individual men are such units and biology busies itself in recording the complicated synthesis and coördinations of energy displayed therein. Reciprocal communication between part and part, mutual reaction of function upon function demonstrates a 'vital' relation of unity. No new force is discovered and, of course, no other than a physical force could be recognized if many existed. This may be claimed as matter of definition, for any phenomenon recognized by physical science would be *ipse facto* physical.

But there has been talk of a vital force. Such a term could only be a name for a coördination or a bond. Such a relation is a truth—a truth of the highest importance, and may well be worthy of a distinct name—but it is not a fact of the same order as heat, light or weight.

The recognition of a living unit is a fact of the same kind as the formation of the judgment of 'substance' or 'object.' 'A living object' is such a constant group of coördinated experiences as not only persists in established relations but proves adaptable to changes in the environment by reactions thereto without destroying the essential coherance of these experiences. A living thing is a construct similar to any other thing. One would not say that the inanimate object was created by cohesion, though that may be a name for a part of the observed coherence of attributes. Neither shall we gain by saying that the animate body is created or maintained by a vital force. Any given object, *e. g.*, any given man has his own individual formula descriptive of the totality of the reactions (or shall we say the trajectory or career). Not that we could express this formula by any means but such a formula could be conceived as possible.

Now our investigation of the individual man results in our determining certain partial elements in this all-inclusive formula. We get a little idea of the energic phases resulting in circulation, respiration, innervation, etc. Sometimes we are fortunate enough to be able to subsume several minor formulæ under one more general or more inclusive. We never doubt that the possibility exists of a synthesis which would show all these coördinated in one career. Of course it is soon discovered that many individuals are wrapped up in any one subject and that units of a higher order (species, etc.) can be formed — unities which are formulæ for a vastly more complex coördination yet presenting themselves to us in such wise that we are often able to approximate nearer to a total formula or statement of the career than is possible in case of the individual.

Now *as biologists* we observe the acts of the free individual and discover fundamentally no difference in kind between the secretion of bile, the peristalsis of the digestive organs and the most complicated free motions of prehension, locomotion, etc. There is biologically no difference between the act of the phagocytes preying on bacteria in the tissues and the Indian hunter in pursuit of bear and the Wall-Street broker preying on simple-minded citizens — each of these acts is beautifully adaptive. So far as we know, the image on the retina is as real an 'occasion' for the prehensile phenomenon that follows as the carbondioxide stimulus on the respiratory center is of the respiratory spasms which result.

We can biologically observe that the liver secretes bile; we can equally observe that action in the vicinity of the fissure of Rolando is followed by adaptive motions in the muscles of the limbs and that a stimulus in Broca's region is followed by reaction of the vocal organs. But it would be entirely incompetent for the biologist to say that brain action produces thought. Adaptive reaction is no proof of mentality as usually understood.

However, we are all born psychologists and, even though we deny the soft impeachment, we cannot escape this congenital peculiarity. We feel and sometimes we fancy that we think. We may now-a-days be a little afraid to admit volition but we still feel quite sure that other people are responsible for at least part of their actions.

These same physical phenomena, reported to our biological observation in terms of visual, tactual, auditory, and other reactions, are reported by the subject in terms of something which he alone can possess, viz., a subjective reaction, let us say a pain. But let us suppose that the subject of our study is also a trained observer. He might report to us as biologists the conditions of his own body as observed by him, that is, as he feels it, sees it, hears its vital movements, etc., and this information, if reliable, would become a part of our biological formula just as it would if we ourselves or some independent observer had recorded it. In addition, this subject might report data which we could by no means know anything about, *e. g.*, a pain, or peculiar sensation, and he might locate it with reference to the previous data. This is also valid biological material — this information is so important that frequently a surgeon will not hesitate in bringing a life into jeopardy by an operation upon such testimony alone. He, at least, has no doubt that that particular sense of tenderness and pain indicates a modification of the normal biological processess in, let us say, the appendix vermiformis. But he does not make the mistake of trying to excise the pain — he is a consistent biologist and to him the pain is diagnostic simply. Even the so-called empiricists in medicine do not commit that mistake (except verbally). That is the pet sin of current psychology alone. To the biologist the reported pain is as objective a phenomenon as the tympanic reaction to palpation or the cessation of peristalsis.

The reported 'mental' reactions of a higher type, with all the adaptive interrelations, fit into his formula for the life so long as they are descriptive data only. From his own experience (as psychologist) he may clothe these reports in a garment of reality, for he has felt the like, but, as a biologist, they are just other forms of reaction, like the contraction of a muscle. The experience of joy or a minor pleasure is connected with circulatory, muscular and nervous activities, and one is a fact to be catalogued like the others. So it appears that the whole field of descriptive physiological psychology is a purely biological science and is to be cultivated with the same tools as any other department of biology. A great deal of unrealized hope and of futile effort might, perhaps, have been saved by an adequate realization of this classification. Whatsoever a man (biologist) soweth, that shall he also reap.

But meanwhile we must give the psychic its due. None of these biological achievements would have been possible but for the subjective reaction which has not only made it possible to perceive and to assemble data, but on the accuracy and adequacy of whose forms the possibility of all classification depends. It is not merely that the objective world reveals itself to us, but we have created this objective world in accordance with forms inherent in our subjectivity. It is not merely that our personal experience has stamped each elementary reaction with the certificate of reality without which it would be valueless, but the very form of the apprehension of the external world has been the product of the form of our subjectivity.

It appears, therefore, that so long as we persistently abstracted the content of experience and the organization of it from the act of receiving and organizing the matter seemed simple, but when we ask ourselves, as sometimes we must, how it happens that we react as we do to the external world and not equally and indifferently otherwise, the difficulties of the problem appear.

Psychology may now examine the problem and attempt a solution from its own point of view. We now have to do with experiences as avowedly *ours*, *i. e.*, immediate realities. We have a multitude of presentations differing in *mode*. This difference we can never understand, we can only feel it. No Weber's law or periodic formula will explain why we feel light, taste, pain, etc. These are the data out of which all that we know is to be formed. There is nothing else. But a succession of different modes would never give us the contrasting perception of difference vs. identity on which all our psychological development rests. Here the old psychology demands its own, claiming that such recognition of difference (to put it simply) between presentations of sense in sequence implies a *tertium quid* — a soul — in which the comparison must be made. Just as, it is claimed, we cannot determine whether one figure is identical with another until it is measured by or in a third thing, so we cannot detect difference until the two compared elements are brought mutually into relations to another.

To this it may be replied that the ultimate test in geometry is *superposition*. In last analysis the demonstrations reduce to applications of this law of superposition. This analogy, if of any value, tends rather to the other conclusion that the perception of difference arises from the reaction between two presentations (or their several energic grounds) superposed in such wise that the overlapping or non-agreeing part forms a new percept. Yet here too we imply a continuum. It is not a conscious continuum. There must be a somewhat persisting through a greater or less span of time which not only somehow preserves some counterpart of one impression, but receives a new one in such wise that the new one is different from what it would have been but for its predecessor. Things are going on that are not reported in consciousness—things which determine the mode of consciousness at this moment, and which preserve the effects of the energy involved in some preceding form of consciousness.

We have the curious anomaly then of living in a sphere (psychic) the grounds of which are indubitably in something else. This something else has been called the soul. The little rivulet of consciousness on the wave of which rides present experience is all that is open to examination. We strive to ascertain whether relations (cause and effect, shall we say) can be discovered between elements in this wave of consciousness and others in other portions of the stream. But how do we now know anything even of the existence of these other events? Evidently the ground of their reproduction lies in the structure (*i. e.*, activities) of this *tertium quid* or soul. It appears entirely incorrect to speak of relations between successive acts of consciousness—the relations are between the total acts of which consciousness is one of the 'meanings' or modes. There is then no such thing, strictly speaking, as association of ideas. Is consciousness then but a feeble reflection of an inaccessible light and are such relations as we discover between successive flickers of the reflection dependent for their explanation on the reactions of the hidden light? Something like this, apparently.

This deeper light may be studied only through these imperfect, intermittent, one-sided, reflections — how imperfect only the trained psychologist can fully appreciate. And yet (lest we forget) these flickering reflections constitute our psychic life, *fide* current definitions. To say that they can by any means directly influence our inner light is absurd. No more could we kill our enemy by stabbing his shadow or feed our friend by offerings before his statue. Yet undoubtedly objective events do affect the psychic manifestations. This process might be illustrated by the actor who shoots the apple from the head of his unseen assistant by aiming with aid of a mirror, or by the Japanese fleet securing accurate aim at Port Arthur by wireless messages from vessels at a different angle.

We do not seek to communicate directly with our friend's thought but we strive to send our message through eye or ear to that somewhat from whence the thought arises. Here is undoubtedly a formal expression of some sort of parallelism but it can hardly be called a psycho-physical parallelism. Physically we did not find any reason for assuming anything psychic at all. Why should we say that this psychogenetic somewhat is physical?

But perhaps it is not wholly clear that the conscious process does not react on the body. Let us look at it in another way. I feel fear and because I feel fear I react in a certain way. Not at all. This statement is contradictory to all that we know of animal activity. I feel fear because certain activities are coördinated in a peculiar manner, or rather, certain coördinations or equilibriated forms having been induced, I feel fear. Fear may be but one of the expressions of that coördination, and there are others, some of which issue in running away, screaming, etc. Fear is the reflection, shall we say, of a conflagration having many phases? The fact that I feel fear is not the 'cause' of my running away.

I communicate the occasion for my fear, 'a burglar,' to my neighbor. Did I communicate my fear to him? Not in the least. Neither did I communicate running away to him. The great wave dashes upon a rock and passes onward in a hundred eddies, but the sound that is produced at the same time did not produce the eddies. (Let us not push this figure too far.)

Psychology may construct a geometry for the relations between the various experiences and rest content that the expression corresponds to valid relations existing in the unknown ground of consciousness. But these elementary experiences are only immediate data—our only way of knowing this 'ground'—the rest are only formulæ for arranging them. Judgment is such a formulating activity but is not it determined by something inhering in the same ground? Is there any external reason why we should formulate the concept 'substance,' for instance, or does such formulation express but a phase of the constitution of the 'ground'? It would appear that the mechanism for testing truth as much as that in which 'reality' inheres, is something back of consciousness or of which consciousness is only one expression. The form in which my judgments are cast is a fact to be dealt with as much as the existence of mode itself, and each act of comparison or identification has a certain mode or feeling tone which stamps it as 'ours' rather than another's, and thus adds 'reality' feeling to the fact of thought though it in no way vouches for the 'truth' of its content.

It becomes apparent then that both biology and psychology become conscious of limitations and so are aware that there are facts outside of their boundaries which are nevertheless necessary to the full understanding of the living individual. Biology assembles observations of the behavior of the individual. No one observer is able completely to observe and so part of the information is reported by others and among the others there may be even the subject of observation himself.

The facts assembled by his own effort and that of his fellow laborers and even, to a certain point, by the observed individual are of the same kind, but the last mentioned is able also to report phenomena inaccessible to the others, yet these unique data fall into congruous relations with the others and supplement or confirm data of the direct or objective sort. Their validity it is foolish to deny and they become part of the biologist's material (pain, animal behavior, etc.).

The method of securing this information does not trouble the biologist who remembers that all of his data without exception were derived by inference from psychic acts or modes of experience. Psychic and physiological data come to us over the same route. It is when we seek to interpret these that we find it necessary to resort to a most complicated contrivance in our own mental activities for outward projection in one case and inward reference in the other. We are informed by the genetic psychologists that there is a stage prior to this polarization of experience in the development of the individual. If this be so we have really encountered nothing so far justifying us in setting up so fundamental a distinction as that between mind and body. The most we can say is that we discover in ourselves a difference between simple psychic acts (*i. e.*, immediate experiences) and the arrangements, relations, and inferences we are forced to make of them apparently as a result of some orderly or organic mechanism underlying or including the power to experience. Two things remain unknown and unknowable from the standpoint of both biology and psychology, viz., the reason for the modes of simple experience and for the forms of judgment based on them.

The problem is now appealed by both parties to a higher court. The trouble has been lack of jurisdiction in each case. It cannot be said that either department has found justification for separating body and soul. Each has recognized its limitations and, at first blush has been inclined to lay all the blame for the 'other' it discovers or postulates upon the rival science.

The trouble all along has been that the judge is also *particeps criminis* and the biologist can no more divest himself of psychological infirmities than the psychologist can forget that he is also human and so biological.

Metaphysics is therefore called upon to reconcile the residual and unassimilated results of both. Biology asserts that its field is a unit and everything harmonious so long as it does not consider the source of its information, but the moment that question is raised, it is forced to admit that all it has in the way of data is a mass of inferences or judgments the form or validity of which it can in no wise explain, and that these judgment are based on immediate experience in various modes, the differences between which are as unexplained as is the nature of consciousness itself. Biology therefore relinquishes this problem to psychology with some asperity to make of as much as possible. (It may be confessed that it is not very much that is made of it.)

Psychology catalogues experiences and names the forms of judgments and diagrams the observed relations, polarizing them into subjective and objective without finding any inherent difference between them and discovers that there is no direct relation between one experience and the next. As one feeling does not cause another there must be some kind of organic nexus behind experience. One thought does not call up another any more that the secretion of bile to-day produces a similar act to-morrow, both sets of phenomena are 'explained' as related to some organism or continuum. Psychology is prone to suspect biology and to think that a brain is the thing back of thought in which all psychological manifestations are bound together. When convinced of the futility of this suggestion it gives up the quest, simply concluding that the bodily phenomena are 'parallel' to the mental. This is nothing but a polite way of confessing defeat, or of keeping out of the quarrel.

One common element may be recognized in the midst of the obscurity of this discussion, viz., *forms* of activity. It is not the *fact* of energy but its *mode* that presents to science its multifarious material.

So when asked to arbitrate this dispute metaphysics offers some such result as is briefly given in the sequel.

But first a word as to the nature of energy. Of energy, in the nature of the case, nothing can be known except as expressed in the form of activity. Nothing is to be gained, therefore, by postulating matter or other entity, different from or behind activity, as a *cause* or ground of activity. As stated above, to us energy is known and *can only be known by its form or mode.* Behavior is the thing. Energy is the term representing the fact (all facts known or possible) concerning behavior. Dynamic realism definitively abandons the search for the unknown *ground* of behavior and claims that for any human philosophy the activity itself is the ultimate. It especially declines to be deceived by any analogy requiring us to know what by nature and definition must ever remain unknown, viz., matter, a something itself incapable of action, but the ground of all action.

But energetic form may be viewed in two ways. Otherwise expressed, all activity in a world of reaction expresses itself in two classes of modes, one which we may call intrinsic, the other extrinsic. This is a direct result of a law, which is clear enough from the physical side but has hardly been sufficiently appreciated in philosophy; namely, that activity is meaningless without resistance. Any expression of energy *in a universe* is dual in its manifestation. We could perhaps imagine, or at least, speak about unimpeded energy or 'pure spontaneity,' which would possess only an intrinsic mode. Its meaning would be for itself alone. No such *manifestation* of energy is possible. Physically, action and reaction are constantly associated and equal. A single or isolated force is impossible. In metaphysics, reality is the reaction of objective and subjective — the 'affirmation of attribute.' Morally, the solution of the problem of good and evil, from this point of view, is that the real good is a doing or striving, and the evil is the condition of such strife; this is good in the making but evil if unvanquished. (See Paulsen's System of Ethics.) Metaphysically speaking, every being in every phase of its career has a double meaning — a meaning for itself and a meaning for the universe. Illustrations are apt to be misleading or unconvincing, but let us use a psycho-geometrical analogy. We may suppose that a certain type of being is represented by an elliptical orbit or trajectory. This activity will impress itself upon adjacent (in Lotze's sense) energic modes and the form, extent, and result of this activity will depend on the nature or mode of the activity in question (here represented by an elliptical trajectory). The resulting readjustment may be supposed to extend indefinitely. The universe as a whole is different from what it would have been but for this particular energetic manifestation. This is the extrinsic side. Now this being is known to the observer, not by what it is, but by its extrinsic effects, by the impress it makes on the universe, or, more particularly, on the immediate environment of the observer.

But there is another way in which our ellipse must be viewed. As a result of its activity upon the world, the world has reacted upon it. The trajectory is thereafter a different kind of ellipse for having reacted with the rest of the universe. Its intrinsic nature has altered. Its locus formula would have to be rewritten. The inner meaning is constantly changing. The next time a reaction takes place the effect will be different from that of the former activity.

Now suppose, as we must, that certain sorts of trajectories or modes (not to say all of them) express this intrinsic form in terms analogous to consciousness. This psychic mode is the intrinsic meaning corresponding to the given locus formula.

A still further suggestion could be hazarded: It might be supposed that a certain degree of complexity would be necessary in order to reach any particular type of conscious expression. Then, if there were complicated systems of equilibriated energy (say human bodies) which were subject to cyclical or rhythmical variations, it is possible for the equilibriated unit to drop from a state of extreme complexity, with an intrinsic mode of consciousness, into one not intrinsically capable of consciousness in any given form. Later on, in another phase, the activity could again rise above the 'dead-line' into that phase whose intrinsic form is psychic. In the interval below the 'dead-line' we say the subject sleeps. What the 'genetic modes' of the equilibriated unit might be no one can tell till he himself experiences them.[1]

But how does it happen that we feel our conscious life as a continuum? So far as our feeling it is concerned the question does not need to be asked, for we have no mechanism for recognizing the hiatus, but there is that behind which bridges the hiatus yet to be accounted for. It might be said that the intrinsic form varies sympathetically in response to every influence and retains such segments of past experience as serve to connect all in a present unity of experience.

The ground for our confidence in the general correctness of the data of mind is to be found, especially from the evolutionary point of view, in the belief that all these forms of energy have been evolved by interaction and that the influence of one part is justly and adequately expressed in every other part. This is what we mean in metaphysics by describing the universe as an organism. On this basis alone a monistic interpretation is possible.

The view just expressed cannot be called parallelistic except by doing violence to the usual form of statement of parallelism and,

[1] Perhaps the most apt physical illustration of the idea of psychical equilibrium advocated by the present writer may be gained by the study of the gyroscope. I am not aware that the mystery of what Foucault called the 'fixity of the plane of rotation' and what Tait and Thomson describe as 'gyroscopic domination' has ever been adequately explained but we may easily convince ourselves that composite motions of revolution may be so adjusted as to acquire a high degree of independence of external influences (such as gravitation) and to present great resistance to impacts from without. Such a system becomes gyrocentric.

The formula given for the estimation of the angular velocity, etc., of the gyroscope is sufficiently complex and we can only faintly imagine the difficulties in the way of constructing a formula covering all phases of gyroscopic interaction — of wheels within wheels. But when one contemplates the complexities which must characterize the gyrocentric activities coöperating to produce the type of equilibrium required to produce a thought imagination is quite at fault.

There can be no doubt that the concentric equilibrium produced is capable of offering a very high resistance to external impacts in some directions while being, like the gyroscope, exceedingly sensitive in its responses to influences in other directions. In other words, the nature of the response is directly a function of the form of the equilibriated forces.

similarly, it can be classed with 'identity' systems only at considerable hazard of misconception. We prefer to speak of it simply as dynamic.

In details it is very hard to present this view in such a way as to give to it the same pleasing objectivity which accompanies the idea of a material brain grinding out thought as a mill grinds out flour. If we admit that the complicated equilibriated organism of our being developed under the law of evolution it need not surprise us that the reaction corresponding to sensation of redness is an invariable counterpart of some particular orderly happening in what we call the objective world, nor yet need we consider it impossible that, under the same law, that peculiar conscious reaction which we call a judgment of 'substance' (always some particular substance) corresponds with coördination having a constant value as representing an objective thing. So on indefinitely. The most complicated coördinations of our mental life have a meaning which expresses a real (evolutionary) correspondence with other things in the universe (objective realities not otherwise known to us). Even the much discussed concept of 'freedom' must have its value — it is somehow true. However much its philosophical interpretation may trouble us, if we are consistent evolutionists and fully grasp the meaning of the word 'dynamic,' we must accept its practical implications as genuine.[1]

C. L. Herrick.

Socorro, New Mexico.

[1] The MSS. of this article was received March 28, 1904. — Ed.