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THE REFRACTORY PHASE OF THE PROTECTIVE-WINK REFLEX:

The Primary Fatigue of a Human Nervous Arc

By Raymond Dodge, Wesleyan University

Two considerations at least give psychological significance to the refractory phase of a human reflex. The first is its direct bearing on the problems of mental fatigue. A second is the remoter possibility of using it as a measure of recuperability.

To Professor Verworn [1] and his pupils we owe the proof that the refractory phase of nervous tissue is an elementary phenomenon of fatigue, and that in what we more commonly know as fatigue and exhaustion, the refractory phase is relatively prolonged.

Mental fatigue, if the phrase has any propriety, is a concept which implies a correlation between mental processes and processes of general physiology. But in the attempt to analyze out the psychophysical correlations of mental fatigue, psychologists have always been embarrassed by the apparent difficulty of finding anything in experience and conduct which corresponds directly with fatigue of nervous tissue. This difficulty is even more conspicuous with respect to the primary fatigue of nervous tissue, *i. e.,* its refractory phase. The immediate repetition of a simple mental process seems to be one of the easiest of mental tasks. There is certainly no obvious moment of paralysis when the immediate repetition of a simple

[1] M. Verworn, *Allgemeine, Physiologie,* Fifth Edition, 1909, 558 fol.; see also *Silliman Lectures* for 1911.

mental act is impossible. It is significant that no one has ever sought evidence of mental fatigue in repetitions of the same mathematical sum. On the contrary it is curiously customary in addition tests and other similar tests to repeat the same combinations as seldom as possible. What that means, and how constantly changing activities could ever give rise to true fatigue, are puzzling questions.

These difficulties, however, were my motive for desiring to produce and study the best possible records of the refractory phase of human reflexes. My first attempt was made on the refractory phase of the patellar reflex, demonstrated for the first time in my "Systematic exploration of a normal knee jerk."[2] Two technical difficulties showed themselves in preliminary experiments. It was found to be a mechanical difficulty to give a rapid succession of blows on the elastic patellar tendon of the same physical intensity. But there was still more serious physiological difficulty. In the knee jerk the chief reacting muscle is the quadriceps. It is probable that the stimulation occurs, not in the tendon, but in the sharp elongation of the muscle when the tendon is struck. To produce a refractory phase of the quadriceps the second stimulus must be applied to the muscle before it has entirely relaxed from the previous contraction. That means that the receptor in the two instances is not in the same condition. Finally, it is almost impossible to distinguish in the records between the mechanical effect of the blow and minute contractions of the muscle. These difficulties are probably not insurmountable, but I finally chose the protective-wink reflex because of them, and because of the simplicity and accuracy of the latter technique, in which reactor and receptor might be kept entirely separate.

The second interest attaching to the refractory phase of nervous tissue is as a test of the metabolic processes roughly designated as recuperability. In deep narcosis, extreme fatigue, and exhaustion, the refractory phase is indefinitely extended. In death its duration is infinite. In tests of neural efficiency as well as in the elaboration of a final formula for mental work it is important to have some direct technique to measure the recuperative processes in a reflex arc. From this second interest, however, we abstract entirely in the present paper.

The latent time and the duration of the several phases of the wink reaction have been investigated by a number of physiologists with a variety of different techniques. The first measurements of the latent time of the wink reflex were made

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[2] *Zeitschrift für allgemeine Physiologie,* XII. 1910. 53-55.

by Exner.[3] Subsequent measurements were made by Franck,[4] Mayhew,[5] and Garten.[6]

Garten's beautiful photographic technique gave the first accurate curves of the course of the lid contraction. The recent simple and convincing kinematographic records of O. Weiss[7] unfortunately refer only to the voluntary wink and not to the protective reflex.

Both Garten and Weiss used the general principle of serial, intermittent photographic records. To give comparable values for the extremely short latent time of the reflex wink (30-50σ), such records should have a frequency of from 500 to 1000 per second. The obvious difficulties in kinematographic records of this frequency, while they are not prohibitive, emphasize the relative simplicity of my photographic method, which gives continuous records of the shadows of the eyelashes on a rapidly moving photographic plate. I believe that this will be found not only the simplest but the most accurate available technique for time measurements of the wink reflex.

The only study of the refractory phase of the wink reaction with which I am acquainted, is the classical study of Zwaardemaker and Lans.[8] Experimenting with both rabbit and human subjects, they used two forms of stimuli, flashes of light and puffs of air blown against the cornea. Both are normal, adequate stimuli for the protective wink. But they have the common disadvantage that the receptors are affected by the reaction as well as by the stimulus. The wink itself produces a sudden change in the illumination of the retina, and a slight stimulation of the cornea. Even more serious in any study of the refractory phase is the fact that, for a considerable interval of time, both receptors, the cornea and the retina, are more or less completely inaccessible to stimuli during the reflex closure of the lids.

It is commonly known that a blow on the face or a sudden noise will also produce the reflex wink. The dermal stimulus was discarded by me because preliminary experiments showed relatively long after-effects. Even the noise stimulus is not without its possible difficulties. It would be useless in case of deafness. Subjects trained by participation in athletic sports or by other means to keep their eyes open would be in a class

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[3] *Pflüger's Archiv,* VIII, 1874, 526.

[4] Uber die zeitlichen Verhältnisse des ref. u. willk. Lidschlusses. Dissertation, Königsberg, 1889.

[5] *Jour. of exp. Medicine,* II, 1897, 35.

[6] *Pflüger's Archiv,* LXXI, 1898, 477.

[7] *Zeit. f. Sinnesphysiologie,* XLV, 1911, p. 307.

[8] *Centralblatt für Physiologie,* XIII, 1899, p. 325.

by themselves. With the deaf I have had no experience. The class would probably not be larger than that of visual defectives. Of those specially trained not to respond, I have investigated and reported one case.

The disadvantages of using the sound stimulus are not greater than those of other stimuli. The advantages of the sound stimulus are three: it permits of direct recording in the same shadow complex that includes the eye-lashes; successive stimuli are apprehended as discrete, well within the limits of completest refractory stage; the mechanism of reaction in no wise modifies the sense organ.

The Wesleyan technique for recording the reflex wink was devised by me in Verworn's Laboratory, and was published in my Systematic exploration of the normal knee jerk.[9] It has five essential factors: (1) A photographic recording device. (I naturally used my familiar falling plate recorder.) (2) A head rest to bring the shadows of the lashes perpendicularly across the slit that admits light to the photographic plate. (3) A sounding board against which a spring hammer knocks to produce the desired sound stimulus. When an offset from the sounding board casts its shadow across the aperture of the recorder, parallel to the shadow of the eye lashes, the moment of the blow is recorded with great exactness. (4) An arc light as source of illumination. (5) An oscillating marker, which is instigated by an electrically driven tuning fork, and is so placed between the arc light and the recording apparatus that the light is twice cut off in the middle of the arc of movement by every double vibration of the tuning fork.

The resulting records are shown in the figures 1-4. In reading these records one will bear in mind the following facts: The abscissae are shadows of fine silk threads. These are held before the falling plate parallel with the direction of its fall. They are spaced cr. 2mm. apart. The ordinates are the moments when the vibrating marker cuts off the light from the slit of the recording apparatus, 100 per second. All records are read from left to right as we read the printed page. The photographs are so taken that stimulus and positive reactions appear as elevations of the respective recording lines. Simultaneity of different parts of the record is guaranteed by the single source of the illumination. It is read directly by reference to the ordinates. The upper double line on each record shows the state of the sounding board. The stimulus noise, its temporal incidence, duration, and overtones are clearly recorded. The lower horizontal lines are shadows of the eye

[9] *Op. cit.*

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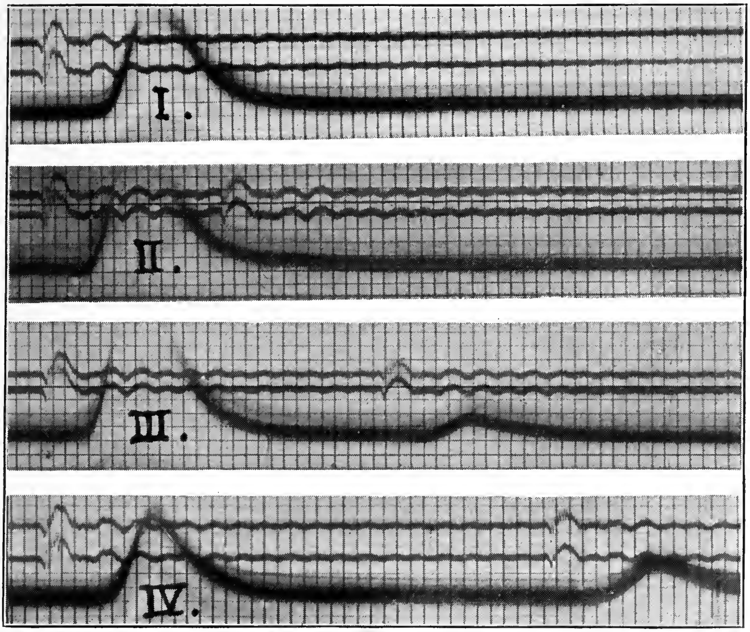

lashes. They show the incidence, duration, and extent of the reacting lid movements.

The whole arrangement is remarkably free from instrumental errors. The registration is entirely without friction, latent time, or other common imperfections. The largest constant error is the transmission of air waves from the sounding board to the ear. But that is less than the profitable unit of measurement, which in the present plates is 1σ. The supreme advantage of the technique is its frictionless direct action, and its sensitivity to even the smallest tremors of the lid. The slightest movements will be recorded with the same instrumental accuracy and freedom from delay as the largest.

In reading the plates two difficulties will present themselves. The first is to determine the exact moment when the shadow of the eye lashes leaves its abscissa. In actual practice this was determined by two methods: by direct magnification with a lens, and also by projection. The second difficulty will be to account for the downward preliminary stroke of the stimulus line. This downward stroke lasts approximately 5σ. Simultaneous control records of the hammer movements and the sounding board show that the two actually meet at the lowest point of the curve. The antecedent movement in the curve is produced by the "give" of the whole sounding board system, when the hammer was electrically released. The release of the hammers was noiseless. It was accomplished by breaking the circuit of the electro-magnets that held the hammers.

In my previous paper I published data only on the latent time of the protective-wink reflex. The present records do not materially modify those data. Five subjects with cr. 10 records each gave extreme records of 28σ and 47σ, and a mean of 30σ. It was significant that the latent time of the low records, within the relative refractory phase, averages only slightly longer than that of the primary reactions. It seems not improbable that the entire difference is a matter of reading, due to the slower initial movement away from the abscissa. This is an important matter but at present I see no way of determining it with entire satisfaction.

The present records confirm our earlier data. The protective-wink has a very low latency for a true reflex. The longer latent periods of earlier investigators may be due in part to differences in the reflex arc. Part of the difference is undoubtedly due to their use of less delicate recording devices.

Our records of the refractory phase of the protective-wink are very different from those given by Zwaardemaker and

Lans. They found an absolute refractory phase of about 500σ in duration.

Stimuli from 500σ to 700σ apart gave double reactions in 34% of the cases.

Stimuli from 750σ to 1000σ apart gave double reactions in 67 % of the cases.

Stimuli from 1000σ to 1250σ apart gave double reactions in 100% of the cases.

My records on the contrary show no absolute refractory phase. In all my subjects a second wink was elicited by a stimulus that both the subjective apprizement and the records show to have been no more intense than the first, within 300σ of the initial stimulus. In all cases within 2σ, however, the second reaction was lower than the first. Even when, as in Fig. 2, the second stimulus occurs before the lid has reached its original abscissa, after an interval of less than 160σ, the refractory phase is not absolute. To be sure the reaction consists only in a slight hesitation of the descending limb of the curve. But this hesitation clearly differentiates record 2 from all natural returns. A less delicate technique would have missed it entirely. By comparison with the normal returns in the other figures, the reaction is unmistakable, and its apparent latent time is only slightly longer than that of the primary reaction.

Such fundamental differences between our records and the classical work of Zwaardemaker and Lans demand critical scrutiny. Our results are neither more nor less reliable, merely because they are recent. The whole matter of credibility hinges on the reliability of the respective techniques. I believe my records speak for themselves.

This is no place to discuss in detail the bearing of these differences on the general problems of primary fatigue in mental life. But it is obvious that the implications of a prolonged relative decrease in sensitivity are entirely different from the implications of a complete refractory phase, a moment of complete paralysis.

In the light of our records, the refractory phase of the nervous arc does not lead us to expect an elementary mental fatigue phenomenon of a definite absolute incapacity to repeat an act. We have no right to expect that it will operate to close absolutely a single avenue of reaction, or necessarily to increase the time of any relatively simple process. We must rather expect it to appear as a more or less pronounced tendency not to repeat a mental act, requiring a constantly increasing stimulus for producing continued rapid repetitions of the same mental act, and tending to delay their succession with stimuli of the same intensity.

These tendencies certainly do exist in our mental life. They deserve to be traced with care, not merely in the matter of delay of repetition, but with respect to the relationship of all the various factors of the complex. They bear the marks of a phenomenon of real mental fatigue.

In much the same way that Sherrington indicated that fatigue operated to save the sense organs from hypertrophy, probably mental fatigue saves our mental life from monotony and the over-development of specific functions.

THE CURVE OF FORGETTING*

By E. O. Finkenbinder, A. M., Clark University

CONTENTS

I. Introduction

The aim of the investigation reported in the following pages is to trace the course of the curve of forgetting as determined by the lapse of time. Every act of memory has, of course, to do with some remembered content; and within this content it is possible to distinguish at least two factors: 1. A memory image of something which has been perceived; and 2. Certain associative bonds which connect the images with one another and thereby constitute trains and constellations and other more or less unitary groups of contents of consciousness. Both of these factors must be taken into account in any investigation of memory, because a complete sundering of the image factor and the association factor is impossible. In the present investigation we are concerned chiefly with the image. For the purpose of isolating this factor so

* The investigation which is here reported was conducted in the psychological laboratory of the University of Illinois. The experiments were carried through by Miss Alida C. Bowler and Mr. Erwin O. Finkenbinder, who were then students in the senior year. On the completion of the experiments, Miss Bowler turned over her data to Mr. Finkenbinder who has assumed the responsibility of preparing this paper for publication. J. W. Baird.

far as possible, we have chosen nonsense syllables as our material. We may study the image phenomena in still purer and more isolated form by the use of evidence furnished by the introspections of our observers. Therefore, our results and the inferences which they justify have to do with the processes of memorizing and recalling in which the association factor is reduced to a minimum and the image factor is raised to a maximum.

II. History of the Problem

The first experimental investigation of the rate of forgetting was undertaken by Ebbinghaus,[1] in the years 1879-80 and 1883-84. He attempted to discover the rate of forgetting meaningless material and also significant (meaningful) material during the first thirty days after it had been learned. His nonsense material consisted of meaningless single syllables, while his significant material was made up of selections of prose and poetry. Ebbinghaus himself served as both observer and experimenter in this investigation. He first prepared a great variety of nonsense syllables, each syllable being constructed by inserting a vowel or diphthong between an initial and a final consonant. Each of these syllables was written upon a small card and the cards were shuffled so that the syllables would occur in random order. Each series of twelve syllables was read at the rate of four-tenths of a second per syllable; and after a pause of fifteen seconds a second reading was begun. The successive readings were continued until the learner found that he was just able to recite the complete series correctly. Permanence of retention was tested by means of the "saving method," i. e., after an hour, a day, or a week had elapsed, a given series was relearned; a comparison of the length of time which was necessary for the original learning and for the relearning furnished a means of measuring how much of the original series had been forgotten during the hours, the days, or the weeks which had intervened between the original learning and the relearning. By this means he made a quantitative determination of the amounts that had been forgotten during the following intervals: 19 minutes, 65 minutes, 8 hours, 1 day, 2 days, 6 days, 30 days. His results show that the curve of forgetting assumes a most remarkable form. For-

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[1] H. Ebbinghaus. *Ueber das Gedächtnis: Untersuchungen zur experimentellen Psychologie,* Leipzig, Duncker und Humblot. 1885. 1-188.

getting proceeds very rapidly during the first twenty minutes after learning. And, indeed, more than one-half of the material learned is forgotten during the first hour. The subsequent deviation in the curve is gradually much less abrupt until, after a day has elapsed, its course becomes approximately parallel with the axis.

Other psychologists were instigated to investigate the problem of forgetting with different sorts of materials. Wolfe[2] employed tones and made determinations for intervals up to 120 seconds. Lewy[3] worked with visual extents and determined how well they could be recognized through intervals up to 60 seconds. Bigham[4] chose words, letters, colors, and numbers; and intervals of 2, 10, and 30 seconds. Baldwin and Shaw[5] used simple geometrical forms, and determined the impairment of memory at the end of 10, 20, and 40 minutes. In all these investigations and in numerous others which need not be cited here, the work was more limited, at least in regard to amount of material and lengths of intervals, than in the pioneer work of Ebbinghaus; but, so far as they go, they all confirm in general the findings of Ebbinghaus. Thus it appears that the temporal course of the process of forgetting is essentially similar for a great variety of memorial materials,—nonsense syllables, tonal pitches, visual lengths, geometrical areas, colors, numbers, letters and significant words. Yet it is to be noted that these later investigations deal with relatively brief intervals,—seconds and minutes; the determinations made by Ebbinghaus were for longer periods of time, ranging from nineteen minutes to thirty days.

A second investigation of the problem to determine the progress in learning and forgetting nonsense syllables was undertaken by Müller and Schumann.[6] In this investigation the observer was in no way concerned with the act of presenting the syllables. Moreover, the order of the syllables themselves within a given series was carefully arranged; by this means

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[2] H. K. Wolfe, Untersuchungen über das Tongedächtnis. *Philosophische Studien,* III, 1896, 534-571.

[3] W. Lewy, Experimentelle Untersuchungen über das Gedächtnis. *Zeitschr. f. Psychol.,* VIII, 1895, 231-292.

[4] J. Bigham, Memory. *Psychol. Rev.,* I, 1894, 34-38; 453-461.

[5] J. M. Baldwin and W. J. Shaw, Memory for Square Size. *Psychol. Rev.,* II, 1895, 236 ff.

[6] G. E. Müller und F. Schumann. Experimentelle Beiträge zur Untersuchung des Gedächtnisses. *Zeitschr. f. Psychol.,* VI., 1894, 81-190; 257-339.

it was possible to eliminate the variable influence of alliterations, consonances, and the like in successive syllables. The syllables were presented by means of a rotating drum which enabled the experimenter to keep the temporal conditions of presentation constant. The results of Müller and Schumann confirmed the findings of Ebbinghaus in so far as they bore directly on the curve of forgetting, but these were very limited. They found that the amount of forgetting was less than Ebbinghaus had reported.

Radossawljewitsch[7] attacked the problem anew in 1903-04. He employed more learners,—fourteen men and four women between the ages of twenty and forty, and six boys and five girls between the ages of five and thirteen years. His method was essentially identical with that employed by Müller and Schumann, excepting that he introduced the factor of accentuated rhythm into the act of learning the syllables. His findings differ from earlier investigators, 1, in that the rate of forgetting is slower, and 2, that it is not uniformly progressive. The most prominent irregularity in his curve consists in an enormous deviation at the end of the eight-hour interval; indeed, he found that more had been forgotten at the end of eight hours than at the end of a day or even two days. It was partially in the hope of finding an explanation of this remarkable and irregular state of affairs that the present investigation was undertaken.

III. Our Own Experiments

A. Materials, Apparatus, Procedure, Observers, Differences Between our Method and Those of our Predecessors

Our materials consisted of nonsense syllables which were constructed in the following fashion. We first wrote out lists of all of the possible combinations of three letters containing each vowel between an initial and a final consonant. Thus, one list began with *bac, bad, baf, bag,* etc., another list contained *bec, bed, bef, beg, beh, etc.* Each of the five vowels being thus combined with every possible pair of consonants gave a total of 2,205 syllables. This long list of syllables was now censored with great care, independently by several readers, and every one which strongly suggested a meaning was eliminated; for instance, of all the syllables which are cited in the above illustrations, only one, *beh,* escaped elimina-

[7] Paul R. Radossawljewitsch, *Das Behalten und Vergessen bei Kindern und Erwachsenen.* Leipzig. Nemnich, 1907. 197 pp.

tion, and was employed in the investigation. Approximately one thousand syllables were left after this process of elimination was completed; and these were combined into groups of twelve each, care being taken throughout to avoid alliterations, consonances, recurrence of the same initial or final consonant in successive syllables, and the like. An effort also was made so to constitute the groups that each series of twelve presented about the same degree of difficulty for the act of learning. It is, of course, impossible to find any combination of three letters which shall be wholly unmeaning to all learners. But it is possible, by the exercise of care, to construct groups of syllables which shall present an approximately uniform degree of difficulty of memorization throughout, and in which the factor of familiar association is reduced to a minumum. And the fulfilment of these two conditions seemed to be sufficient to provide the proper material for our investigation.

The letters which we employed were made of black paper, 3.3 cm. in height and 2.8 cm. in width, and were pasted, in groups of three, upon sheets of white cardboard 12 cm. wide and 18 cm. long. The twelve cardboard sheets which carried the syllables of each group were then bound into booklets by means of adhesive tape; and the pages of this booklet were then cut or "indexed" across the top in such fashion that the experimenter could readily turn the pages one at a time. A metronome in a sound-proof box, audible to the experimenter alone, marked off the time interval which should elapse between the successive presentations of the syllables of a series. The booklet stood in a convenient position upon a table; and the experimenter turned a page, thereby exposing a syllable, every two seconds,—each syllable appearing immediately after the disappearance of the preceeding syllable, and remaining in view for a period of two seconds. This procedure was continued without interruption until the observer found that he was just able to predict the syllable which was about to appear; this criterion had been adopted by the experimenters as an assurance that the learning of each series had been brought to a uniform level of completeness at the instant when the act of learning ceased and the process of forgetting presumably began. A record was kept of the number of presentations which was necessary to produce this memorial effect. This same series of syllables was relearned in like manner after a definite interval,—30 minutes, 1 hour, 2 hours, 4 hours, 8 hours, 12 hours, 16 hours, 24 hours, 36 hours, 48 hours, or 72 hours; and a comparison of the number of repetitions required in the two cases enabled us to determine the amount that had been forgotten. In order to discover and to make allowance for the diurnal variations in ability to learn, the learning times for each observer were distributed throughout the day, as follows: 8 A. M., 11 A. M., 2 P. M., 5 P. M., 8 P. M. (11 P. M. in a few cases of especial interest).

No attempt was made to impose any particular or uniform method of learning upon our observers. Each was permitted to employ his own natural method, but was warned not to change his procedure during the course of the investigation. After each act of learning, the observer gave a detailed introspective account of the act of learning; these introspections not only afforded an insight into the mental processes involved, which will be mentioned in a later section, but they also constituted a control in that they revealed any deviation from the original natural procedure in learning. The observers gave no further voluntary attention to the syllables until the desired interval of time had elapsed, when they made a brief recall to determine how many they could freely reproduce. They then relearned in the same manner as before, continuing the process of learning to the point where they just succeeded in predicting each syllable before it came into view.

The observers, four women and ten men, who served throughout the year, were: (1) S. Carolyn Fisher, graduate student in psychology; (2) Alida C. Bowler, senior, psychology; (3) Sarah Rogers, junior, languages; (4) Amy Overland, freshman, literature and arts; (5) Howard F. Swits, senior, psychology; (6) Oliver L. Herndon, senior, psychology; (7) Claude E. Burgener, senior, psychology; (8) Erno B. Pletcher, junior, literature and arts; (9) Erwin O. Finkenbinder, senior, psychology; (10) Jacob Sinclair, junior, science; (11) Lyle J. Pletcher, freshman, science; (12) J. Elmer Wiley, special, agriculture; (13) Royal R. Finkenbinder, special, agriculture; (14) Louis Seyster, academy.

Former investigations which deal with exactly the same problem of forgetting differed from our own in the following particulars:

1. Our observers were fourteen in number; Ebbinghaus had but a single observer, himself.

2. An experimenter presented the material; this avoided the distraction which necessarily occurs when the observer

presents his own material. Ebbinghaus presented his own material to himself as observer.

3. Our materials were nonsense syllables; and our groups of syllables were carefully prepared with a view to avoiding alliteration, consonance, rhyme, etc., which would serve as associative bonds to the learners. This precaution Ebbinghaus failed to observe, in that he shuffled his syllables in constructing a series.

4. Our observers were allowed to follow their own methods of learning; but they were cautioned to avoid grouping the syllables, to avoid looking for associations, and to avoid changing their method during the course of the investigation. Radossawljewitsch prescribed a method of learning,—which employed an accentuated rhythm throughout. Ebbinghaus employed an arbitrary but varying accent in reading.

5. We first gave series for preliminary practice in order to familiarize the observers with their part of the work, and to bring them to such a level of efficiency that the increase of efficiency[9] in learning would be slight as the experiment progressed. Radossawljewitsch did not take this into account, although he found that in the later sittings the act of learning was accomplished in about one-third of the length of time which was required in the earlier sittings. (See p. 22 of this paper.)

6. Our observers read and re-read the syllables continuously until all were learned,—each syllable being present to vision two seconds each time. Ebbinghaus, Müller and Schumann, and Radossawljewitsch introduced a pause after each series had been read through. They also required a recall or reproduction when the observer thought that he was able to repeat all the syllables in the series, and then a second recall as an assurance that the learning had been complete.

7. Eleven different intervals between learning and relearning were included in our investigation. Seven of these furnished determinations for points within the first twenty-four hours of the curve, while Ebbinghaus and Radossawljewitsch made determinations for only three and four points, respectively, within this same span.

8. In order to neutralize the results of practice, which may increase the efficiency of the learner as the experiment

[9] A curve of the gain in efficiency throughout the experiment is given on p. 22, which indicates that no marked change in efficiency occurred during the progress of those experiments whose results are included in our table.

progresses (i. e., even in the experiment proper, after the preliminary practice period) these intervals were investigated at sittings which were distributed regularly throughout the whole year's experimentation. Radossawljewitsch failed to regard this fact; he first tested the five-minute period, second the twenty-minute, third the one-hour period, etc.

9. Between successive determinations a period of eight hours or more was allowed to pass, so as to avoid confusion between syllables presented at different sittings. Radossawljewitsch gave three new series each day.

10. To prevent the effects of diurnal variations of bodily and mental condition,—fatigue and the like,—from falling at any one place in the curve, the periods for learning for each observer were distributed uniformly throughout the day. Ebbinghaus distributed his learning time throughout the day as follows: 10 A. M., 11 A. M., 1 P. M., 6 P. M. Radossawljewitsch did not take sufficient precautions to eliminate the variable influence of diurnal variations of general efficiency, with the result that in the case of the eight-hour interval the determination of amount forgotten was always made in the late afternoon, a feature which was characteristic of only this interval. A further discussion of this point appears on p 28.

B. Results, Quantitative and Qualitative

1. Methods of Learning, Recalling and Relearning

Various methods of procedure were followed by the different observers. It appears, however, that three observers, numbers (5), (8) and (12) of the above list, fall into one class. They gaze intently at the syllables while learning them,—as they express it, they "just look at them." But at times even these three report that they "think them over and over, saying them in imagination" while looking at them. One observer (8) prefers to take just a glance at the syllable and then turn away, picturing it in space and retaining it, continuously if possible, as a visual image. He recalls in terms of visual imagery, for at times he knows the general shape of the syllable, but is not sure whether the letter is H or K, or even B; he knows that it is not Q or C, since the shape is very different. He remembers a "full syllable" opposed to a "thin" one, as MUB opposed to YIL. Occasionally, when he tries to learn a difficult syllable, he probably vocalizes it, although he says that he "thinks it over and over visually." Vocalizing, however, for him is very rare, for he clings to one type of imagery more exclusively than does any other of our observers. A sheet of original data which shows the kind of work that is typical for him, follows:—

Observer No. (8).

Series No. 41. Initial learning at 8 P. M., Jan. 27.
Learning,—ten presentations of each syllable.
Relearning,—after 72 hours, six repetitions.

Introspection,—

"The syllables were not learned by grouping; they were learned individually. The first one 'stuck' on the first time through, then the next one was associated with it, and each to the one preceding. The last two or three came together after about the fifth time. The syllables seemed to 'stick' just by looking them over. I looked at them until I could see them in imagination before they came into view."

Introspections show that another of these three (5) at times articulates the syllable silently. Seldom does he move his lips while learning, and only when learning a difficult syllable. His procedure consists in staring at the syllable before him. After looking at each a few times, one or more are known; and from this time on he spends a part of the time in trying to bring up a visual picture of the forthcoming syllable. Near the end of the learning, the time is mostly spent in attempting to recall the syllable which is to follow. Finally he is able to image them all visually before they come into view.

The other of the three predominantly visual learners (12) says, "I just look at them," yet he, too, has at times repeated them, moving his lips, and thus can not be using visual imagery alone, as a method of learning. In recall, he has never been able to discover any but visual images.

Observers (11) and (13), on the contrary, prefer to repeat the syllables in a whisper or aloud. They say, for instance, QIJ, QIJ, QIJ, QIJ, while the syllable is before them; and when XAC appears, they say, XAC, XAC, QIJ, XAC, XAC; then while the next one is before them, they say, QIJ, XAC, SOH, SOH, SOH, then, SOH, VIG, SOH, VIG, VIG, etc., in varying time and emphasis. These two then employ mostly the vocal-motor image in learning. Their recall in many cases shows that the syllable comes again to them by means of vocal-motor images, but at times together with auditory and visual images, as the following shows. Introspection at recall, 36 hours after learning is: "Could get them better by pronouncing them. The associations of the sounds seemed to help somewhat." Another statement of the same observer (11) at recall after a 24-hour interval is: "Relearning is easy because I can almost recall the whole series, some syllables being on 'the tip of my tongue,' but I cannot write them down." The other of these two observers (13) "articulates" the syllable in recalling it. He is never sure whether C or S had appeared in the syllable, when they may be pronounced alike. For example, he reports: "It is SEV or CEV, but I cannot decide which one." This observer in recording his reproductions never prints the syllables in large capital letters, as do the more dominantly visual observers. The latter say that after seeing their own printed syllables their assurance is greater than when the syllables are present only in imaginal form.

The author (9) employs mostly motor but very often auditory images in recall as well as in learning. "I predicted visually the second syllable with uncertainty, and tested it quickly to ascertain whether the feeling of saying it would make me certain of the vowel. It did. First, I tried to say MIR, but that did not feel right, then the syllable came easily and I knew it was correct,—MUR." The high-pitched sound of YIK, NIQ, TIJ, is contrasted with the round and empty sound of YOG, BOQ, PUV, GUB, etc. However, M and H are usually visual images in recall, and they are remembered sooner than many other letters, and with greater certainty.

Observer (2) repeats or vocalizes the syllable, and vocalizing appears to be almost as prominent in each of the other women observers. One of the women (3) reports that she most often repeats (vocally) the syllables before her in order to learn them, yet she has often given emphasis to the visual image. In some cases she reports that she just looks at the syllables and learns them so that they all appear in visual imagery.

Another observer (1) has particularly associative methods of learning. She uses auditory and motor imagery chiefly, and the associations are built upon similarities of sound between nonsense syllables and familiar words. She reports many instances of associations which seem to be far-fetched, but which came to her in a most natural and involuntary manner; for instance, HOQ suggests HONESTY, and PUV becomes PULVERISE. There are plainly auditory associations, e.g., LOZ is associated with and remembered by the word LAWS, and YOG by JOG. As many as eight associations have been recorded in her introspection on learning and recall of a single series. The associations seem very effective to her as a means of recall and almost as effective in learning.[10]

Our learners may then be classified as follows: *1*. The silent visualizer, the learner who just gazes at the syllables; *2*. the active vocalizer, the learner who actually speaks the syllables in a whisper; *3*. the vocalizer and visualizer, the learner who uses the motor image and the visual image correlatively; (this type Radossawljewitsch did not find among his observers; but he rather encouraged the use of the auditory and motor image by the employment of rhythm in presentation); *4*. the vocalizer and auditor, the learner who images the sound of the syllable as well as its pronunciation; and *5*. the vocalizing auditory-associator, the learner who formed many associations with words, and then remembered these by auditory and motor images. The images which each observer employs are chiefly of one sort throughout, although no observer uses any one sort exclusively; and every observer has at times employed vocal motor imagery, and similarly, visual imagery. Several of our observers use such a variety of images that it is very difficult to determine which modality is most prominent, and it might be well to classify them as belonging to the "balanced" type.

Variations in amount and accuracy of recall have proved to be many times greater than variations in length of time necessary for relearning. At times when the observer was mentally alert and vigorous, many more of the syllables could be recalled than when he had become fatigued. The quantitative results of the recall experiments will not be included in this paper because of their great variability.

When incorrect syllables had been written during the recall test, there was, in many instances, a strong tendency to introduce them into the series during the subsequent relearning, which slightly hindered the process of learning. It sometimes happened that correct syllables were not recognized when they were reproduced during the recall test; they seemed to the observer to be imagined and not remembered. In a few cases, syllables were not recognized even when presented

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[10] Recourse to the factor of association which was so frequently reported by this observer was almost invariably absent in the case of every other observer. And it is interesting to note that this observer's process of retention follows, in general, the same temporal course as that of the other observers, although it occupied a slightly higher level throughout (3 to 5 per cent.).

for relearning, but this was exceptional; almost invariably they were more or less familiar.

We find that the order of the syllables in the series was recalled with greater certainty than the syllables themselves. The order was not always learned before the syllables; and this gave rise to a frequent error of anticipation. Often during the learning, a certain syllable, more clearly known than others near it in the series, was expected to appear several syllables before it did. Our findings regarding the order in which the twelve syllables of the series were learned agree with the results of other investigators: the initial and final syllables of the series were the first to be mastered; the central section of the series, usually the seventh or eighth syllable, was the last to be learned.

2. The Value of Various Methods, and Other Comparisons

The differences in method of learning and in ideational type, which have been noted above, do not appear to be responsible for much variation among the curves of forgetting for the various observers, except in the case of the observer who formed meaningful associations. This variation was only 3 to 5 per cent. Radossawljewitsch has shown that there is an average difference of about eight per cent. in the memory of nonsense and meaningful material; and from his results we should expect that this observer (1) would remember more than those observers who attached no meanings to the syllables. Only a few meaningful associations were noticed by the other observers; and, of course, the influence of a very limited number of associations is slight. If, however, the learner should make frequent use of associative connections, the influence of such a procedure would make itself felt upon the tenacity of retention. For this reason we have taken the liberty of discarding certain of our results, basing our selection upon the following principle. When it was found that more than three associations had appeared during the act of learning or relearning, the results obtained were not included in our computations.

Our data do not furnish a basis for the comparison of memory for associated nonsense syllables with memory for monosyllabic words; but if we compare the percentage of associated nonsense syllables remembered (our results) with Radossawljewitsch's curve for memory of monosyllabic words, we find that approximately the same percentage of significant words and of associated nonsense syllables is remembered. Nor are our data sufficiently extensive to warrant a conclusion regarding the relative memorial efficiency of fast and slow learners, or of men and women. Table I shows, however, that the four most rapid learners,—whose learning times, in seconds per series, averaged 207, 208, 225, and 229 respectively,—are a woman, a man, a woman, a man. The memorial efficiency of these four observers is approximately two per cent. greater than the memorial efficiency of the four slowest learners of our group, whose learning times were 375, 297, 282, and 264 seconds respectively, and whose ideational types are as non-uniform as those of the first four. We have classified the rapid and the slow learners, respectively, in the order given above, as vocalizer, visualizer, vocalizer, visualizer, and as auditor, vocalizer, visualizer, visual-vocalizer. These correlations do not justify the inference that the use of any type of imagery without modification is superior to another. The auditor, the slowest learner, has remembered quite as well as the vocalizer, the most rapid learner. The visualizers (3) and (5) who proved to be fast and slow learners respectively possess approximately the greatest and the least memorial efficiency among our observers. Memorial ability is not clearly correlative with rapidity of learning, with ideational type, or with sex.

3. The Effect of Time of Day Upon Rate of Learning

Time of day has a marked effect upon the learning of many of our observers. While three observers are about equally efficient at all times of day, others come with vim and vigor in the morning but become dulled before noon and do not regain their alacrity during the afternoon or evening. Two observers manifest two maxima of efficiency,—at eight o'clock in the morning and at eight in the evening. One observer does his most rapid work at two in the afternoon, but most of them do best at eight o'clock in the morning. The grand averages for all observers show that the most rapid work is done at eight o'clock in the morning, and the least rapid at five o'clock in the afternoon; an average of 13 per cent. more time is required to learn a series at five o'clock in the afternoon than at eight o'clock in the morning. The average length of time required by each observer for the act of learning a series at each of the five periods of day is given in Table I.

TABLE I. RATE OF LEARNING AT DIFFERENT TIMES OF DAY

THIS TABLE SHOWS THE AVERAGE TIME, EXPRESSED IN SECONDS, WHICH EACH OBSERVER REQUIRED FOR LEARNING, AT EACH OF THE FIVE DIFFERENT HOURS OF THE DAY. EACH NUMBER IN THE TABLE IS THE AVERAGE OF EIGHT INDIVIDUAL FINDINGS. THIS TABLE CONTAINS RESULTS FROM ONLY TWELVE OF OUR FOURTEEN OBSERVERS,—THE OTHER TWO DID NOT FURNISH THE FULL COMPLEMENT OF SITTINGS FOR EACH OF THE FIVE TIMES OF DAY. THE MOST FAVORABLE TIME OF DAY FOR EACH LEARNER IS INDICATED BY AN ASTERISK IN THE APPROPRIATE COLUMN.

Observer	*8 A.M.*	*11 A.M.*	*2 P.M.*	*5 P.M.*	*8 P.M.*	*Average*	*M.V.*
1	230	246	347	...	224*	261.7	42.7
2	173	192	192	312	170*	207.8	41.0
3	236	233	228	225*	229	230.2	3.6
4	200*	240	216	...	246	225.5	17.7
5	269	304	282	309	248*	282.4	19.3
6	301*	341	437	432	367	375.6	39.6
7	300	252*	336	300	298	297.2	18.1
8	228	222	220*	227	252	229.8	8.4
9	259	281	266	271	258*	267.0	9.2
11	280	259	234*	295	252	264.0	18.8
12	197	197	161*	229	259	208.6	28.6
13	264	266	244*	278	242	258.8	12.8
Average	244.8*	252.8	263.6	277.7	253.8	259.9	21.7

4. The Effect of Practice Upon Learning

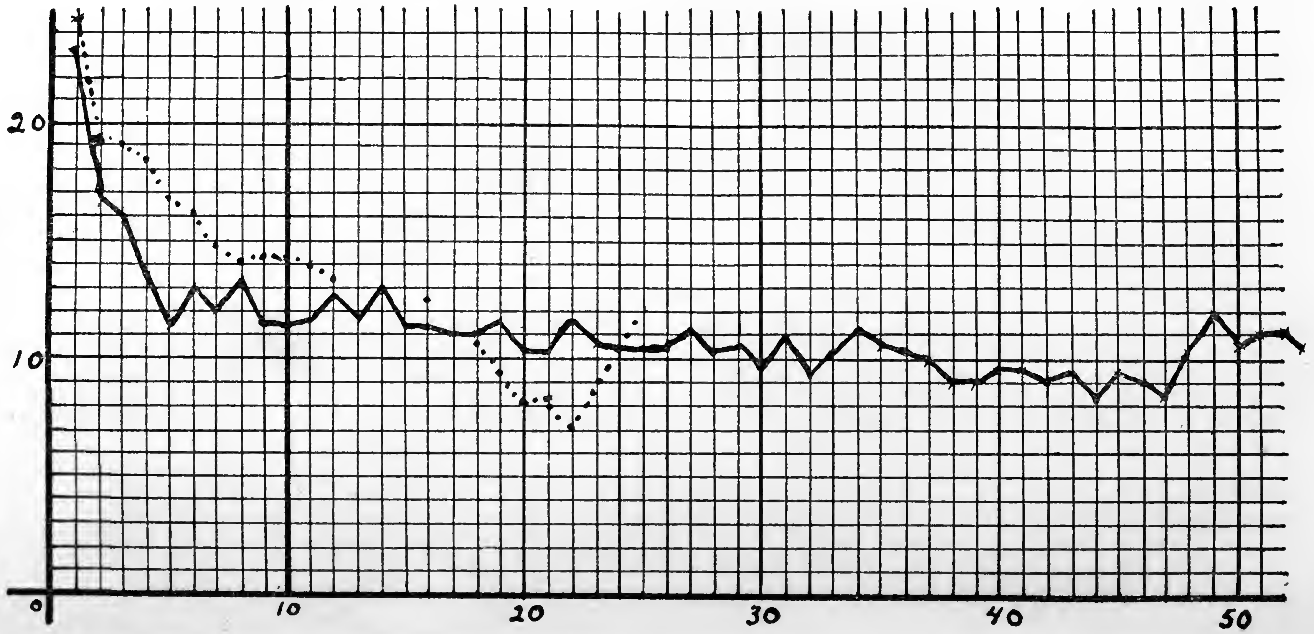
In Plate I, the continuous line shows the average number of presentations required for learning at each sitting throughout the present investigation. The extremes for the first sitting are 13 and 32 repetitions, the average 23.3. The extremes for all single readings after the tenth sitting are 7 and 16 presentations. Most of the gain in efficiency occurs during the first ten sittings. The writer feels that these initial experiments should be regarded as preliminary practice; accordingly these initial results are not included in our computed averages. The dotted line represents the curve of practice as plotted from the results of Radossawljewitsch.

5. The Final Quantitative Results, Showing the Curve of Forgetting

The table below shows the amount forgotten, the averages computed from the results of fourteen observers. The percentages express the ratios of the time spent in relearning to the time spent in first learning. When the observer

PLATE I.—EFFICIENCY IN LEARNING, SHOWING THE EFFECT OF PRACTICE

The continuous line is the curve of the present investigation (fourteen observers); the dotted line that of Radossawljewitsch (eight observers). The ordinate shows the average number of repetitions required for learning a series. The abscissa shows the numerical order of the series.



required twelve repetitions of the series for the first learning and four repetitions for relearning, the ratio of the time for relearning to the time for first learning is 33.3 per cent., which we assume to be approximately the percentage forgotten. If the observer had required twelve repetitions for learning and twelve for relearning, the percentage forgotten would be 100; but if six repetitions had been necessary for relearning, the percentage forgotten would be 50. This method of computation was employed by earlier experimenters and is well understood.

TABLE II. THE AMOUNT FORGOTTEN AFTER THE LAPSE OF DIFFERENT INTERVALS OF TIME

THIS TABLE SHOWS THE RESULTS OBTAINED BY RADOSSAWLJEWITSCH, BY EBBINGHAUS, AND BY THE PRESENT INVESTIGATORS. THE NUMBERS REPRESENT THE AMOUNTS FORGOTTEN, EXPRESSED IN PER CENTS.

Length of Interval	Results of the present Investigation			Results of Radossawljewitsch		Results of Ebbinghaus
	Nonsense Syllables			Nonsense Syllables	Meaningful Material	Nonsense Syllables
	Ave.[11]	P.E.	*10 (log t+1)*[12]	Ave.	Ave.	Ave.
5 Min....	....	...	16.9	2.5	....	....
20 "	....	...	23.0	11.4	3.9	41.8
30 "	25.0	1.3	24.7	....	....	....
1 Hour...	27.2	.6	27.7	29.3	21.7	55.8
2 Hours..	30.6	1.8	30.7	....	....	....
4 " ..	33.6	1.7	33.8	....	....	....
8 " ..	34.5	1.4	36.8	52.6	41.9	64.2
12 " ..	36.2	1.4	38.5	....	....	....
16 " ..	37.0	.9	39.8	....	....	....
24 " ..	42.2	1.8	41.5	31.1	20.3	66.3
36 " ..	41.2	1.9	43.3	....	....	....
2 Days...	44.5	1.8	44.5	39.1	33.2	72.2
3 " ...	47.9	1.7	46.3	....	43.5	....
4 " ...	....	...	47.6	....	45.5	....
5 " ...	....	...	48.5	....	43.5	....
6 " ...	....	...	49.3	50.7	57.6	74.6
7 " ...	....	...	50.0	....	50.0	....
14 " ...	....	...	53.0	59.0	70.0	....
21 " ...	....	...	54.8	62.2	52.4	....
30 " ...	....	...	56.3	79.8	76.1	78.9
120 " ...	....	...	62.3	97.2	....	....

[11] These numbers are the averages of our fourteen individual curves; and each point in the individual curves, from which these are computed, is the average of five tests, except in the cases of 8, 12, 16, and 36-hour intervals, for which the relearning time of one or more of the desired determinations fell between midnight and six o'clock in the morning. The mode, the median and the average almost coincide throughout.

[12] The general formula may be stated, using k and c as constants,

IV. Discussion

The curve which expresses the results of our investigation shows a greater percentage retained than the Ebbinghaus curve, but less than the curve of Radossawljewitsch. Under the conditions employed by Radossawljewitsch or by the experimenters at the University of Illinois no observer has shown so great an amount of forgetting as did Ebbinghaus. The fact that he was observer and experimenter at the same time is unfavorable for obtaining the best results. And the fact that he presented his material at an exceedingly rapid rate may, in view of his unusual experimental conditions, have constituted a second cause of variation between his findings and our own.

That the Radossawljewitsch curve shows much less forgetting than the curve yielded by the present investigation is

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t the time in minutes, and *log* the common logarithm: the amount forgotten equals $k(\log t + c)$. The Ebbinghaus formula is

$$100 - \frac{100\,k}{(\log t)^c + k}$$

Piéron (H. Piéron, Les courbes d'évanouissement des traces mnémoniques. *Compt. Rend. Acad. d. Sc. Par. CLII., 1911. 1115-1118*) has given a formula for the pond-snail,—

$$100 - \frac{k\,(\log t)^{\alpha}}{t^{\beta}}$$

But his curve can not be compared with ours because he has not allowed for the effects of practice; and in his brief test to see if this curve applies to humankind, employing 50 ciphers as his material, he found that even at the close of seven days no forgetting had taken place.

That our curve follows so closely the relatively simple theoretical formula seems rather remarkable. And upon presenting these results to Professor William E. Story, of the Department of Mathematics, Clark University, we immediately aroused his suspicion that an error had crept in. The present writer, because of lack of space and additional proof, reserves his criticism of the validity of the *Ersparnismethode* as a measure of the amount forgotten.

The above formulae prove to be rather absurd, at first glance, if a very brief time, a second, for example, is considered. And at the moment when the act of learning is completed, according to the above formulae, an infinitely large negative amount is forgotten. In our own formula, at the close of one-tenth of a minute after the close of the learning period, forgetting begins. This does not appear erroneous if we view it in comparison with the positive "after image" following the presentation of a visual stimulus; and we may probably justly compare this theoretical result with the experimental findings of Wolfe and others, cited above, that the period of greatest accuracy in recognition is not immediately after the stimulus has ceased but about two or three seconds later, after which forgetting immediately proceeds very rapidly.

to be explained from the fact that his observers made a recall when they thought they had learned the series, and then a second recall to establish greater certainty that they actually had it learned. These recalls, being somewhat valuable to the observer as a means of learning, make it very probable that his observers learned each series beyond the point where it was just learned or known, that is, that the series was over-learned in every instance. Now it is unquestionably true that if one learns a series of twelve syllables until it can be reproduced without hesitation in ten seconds, as did Radossawljewitsch's observers, the learning has been more thoroughly done than if one learns it so that he is just able to recall each succeeding syllable in two seconds, as was the case in our experiments.

The whole investigation of this problem is based upon the measurement of learning and relearning, since the ratio of these two measurements is the index of the amount forgotten. In both the learning and the relearning, it is very important that the process shall not be continued beyond the point where the prediction of the forthcoming syllable is barely possible. This secures a two-fold desideratum,—a constant and uniform degree of perfection of learning, and a complete absence of over-learning. Since the ratio of the relearning time to the learning time is the variable[13] which has been adopted as the unit of measurement, the rate of learning, both during the initial act of reading and during the subsequent act of recall (in the investigations of Ebbinghaus and Radossawljewitsch), must be made as uniform as possible; and the other factors must equally be kept as nearly constant as possible. Thus, we must take into account the whole time spent in the process of learning, i e., the time spent in recalling and trying to recall as well as the time spent in observation. Radossawljewitsch, in measuring the time spent in learning, did not consider at all the value of his double recall. The act of recalling introduces one or other of two causes of error into his method: 1. In recalling after the reading has continued

[13] Ebbinghaus introduced the plan of measuring the amount forgotten in terms of the time required for relearning, as compared with the time required for initial learning (*Ersparnismethode*). Müller and Pilzecker (Experimentelle Beiträge zur Lehre vom Gedächtnis. *Zeitschrift fur Psych. u. Physiol. d. Sinnesorgane. Ergänzungsband I.* Leipzig, Barth, 1900. 300 pp.) introduced the method of right associates (*Treffermethode*). This latter method measures primarily the strength of the associative bonds; while in the *Ersparnismethode* it is primarily the retention of non-associated syllables as such, that is measured.

to the point of the observer's ability to reproduce the material, the learning is carried beyond the point of just knowing,—what amount beyond we are unable to determine,—and this vitiates the experiment; 2. and if the observer has not learned the material to the point where he is able to reproduce it, and by a process of recalling and thinking about it is enabled to do so, he has learned by the process of recall. It is therefore erroneous to fail to regard the recall-time as learning-time, and include it in the computations. For this reason, we introduced no pause between successive readings of a series; nor did we allow a period for recall at the end of the act of learning. The reading was continued until each syllable was learned just to the point where it could be reproduced or anticipated during the two seconds before it came into view. Thus the recall factor[14] was at work, but under uni-

[14] In every act of difficult learning factors of observation and of recall are at work. In observing, the emphasis is placed upon a complete grasping, taking in or noticing what is presented; while in recalling, the emphasis is placed upon a holding or a bringing again to mind of images of the things observed. If only one nonsense syllable has been observed, it may be learned by simply glancing at it; and it may be remembered all day if only it be recalled occasionally. Here the recall factor is, in fact, equivalent to a relearning, and is similar in importance to a re-observing. This is most plainly seen in the case where, for instance, the syllable has apparently been forgotten. By pondering and thinking, by trying to recall the syllable, we may be able to reproduce it, which is just as clearly a relearning of the syllable as though it were observed again. And it may be remembered longer and more vividly if recalled than if re-observed. We have not found it possible to learn twelve syllables by taking a single glance at each. Presentations must recur several times, in fact, about a dozen times; and the recall factor is steadily and keenly active throughout, holding and bringing up again the images. During the later repetitions of a twelve-syllable series, many of the syllables are known,—are present to consciousness as a result of recall,—before they appear in the presentation. The observation factor is less prominent here than it was at the beginning of the learning; and the last repetition is of little value as an observation, because the time is mostly spent in recalling the more difficult syllables. Thus, by holding the attention uniformly upon the learning, we bring it about that the observation and the recall factors work together; and we can measure them together as learning-time. In this way we consider the whole time spent in learning. This same method we used in relearning, which affords us uniformity in learning and also furnishes a more satisfactory measure than either the observation-time alone or the amount recalled would give.

We are not able to correlate the results of the experiments of Bean (C. H. Bean. The Curve of Forgetting. *Archives of Psychol.* No. 21, Mar., 1912. 1-45.) with the present findings, because his work was done by the recall method alone. His observers learned only nine consonants, and made several recalls during the relearning, which as one might expect would secure a better and more thorough learning.

form conditions. It was measured in both the learning and the relearning. Since Radossawljewitsch granted to his observers both the pause and the recall, and did not count these as learning-time, and since these are of real value in learning, his results show a greater percentage remembered, as we see from a comparison of the curves. For example, the percentage remembered in his curve at the end of the twenty-four-hour period, is 67.8 as compared with our 57.8.

The effects of practice and familiarity must be taken into account in every memory experiment. It appears that Radossawljewitsch failed to do this. During the first sitting of his whole investigation he made a determination of the amount forgotten at the end of the five-minute period; and his results show that only three per cent. was forgotten. A re-determination, made just one month later, showed that nearly nine per cent. was now forgotten during this five-minute interval,—almost three times as much as the first determination showed. This great retentivity at the outset one would naturally expect, because it is well-known that unfamiliar occurrences are more vividly experienced and are longer remembered than ordinary occurrences, presumably on account of their novelty and enhanced interest. This we found to be true in our experiments. In fact, two months after learning the series which had been presented at the first sitting of our investigation, four of our observers succeeded in recalling it entirely without any difficulty. This

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His curve shows a higher degree of retention than any other investigator has found. In the introductory chapter of his paper he notes the interesting deviation in the curve plotted by Radossawljewitsch. But apparently he has omitted to investigate the eight-hour interval; and the main purpose of his investigation seems to be to find the curve of forgetting as it may be determined by the recall method, for intervals of one day or longer.

In the section of his work which deals with gain in speed and retention of speed in the act of typewriting, there is little relation to our problem. Those observations during the time which we allowed as practice,—preliminary to our experiment,—correspond to his work, since he is interested in measuring the losses during the period of rapid gain in rate of learning. (See the first ten tests in our practice curve on page 22.)

Bean did employ the *Ersparnismethode,* however, in a very brief series of experiments. Two of his observers learned and relearned certain brief exercises on the typewriter; but by this method only three tests were taken for each period by each observer, and in these tests the number of repetitions for learning was so few as three. His measurements, in terms of number of repetitions during relearning, must necessarily turn out to be either 33.3, 66.6, or 100.0 per cent.,—a very crudely graduated scale.

was not true of the later series, after the task and the method had become familiar to the observers. The inclusion of his initial determinations vitiates the computed curve published by Radossawljewitsch, notwithstanding the fact that he attempted to eliminate the error by an appeal to check experiments.

A more serious defect in method consists in his failure to distribute the learning period of each observer throughout the day. The work of Oehrn,[15] of Pillsbury,[16] Larguier des Bancels,[17] and particularly that of Ebbinghaus,[18] shows that observers are not able to do mental work equally well at all times of the day. Ebbinghaus found that in his own case the act of learning was accomplished in 12 per cent. less time in the morning than in the latter part of the day.

Table I shows that certain observers did most rapid learning in the morning at eight o'clock, others at eleven in the morning, others at three o'clock in the afternoon, and still others at eight o'clock in the evening. The average of the records of all observers shows that the most rapid work was done in the morning at eight o'clock, the least rapid at five o'clock in the afternoon. This afternoon hour is approximately the period when all of Radossawljewitsch's observers relearned for the eight-hour point in the curve. His plan of having several observers work in the morning, between seven and eleven o'clock, and several in the afternoon, between one and six o'clock, proved to be convenient for all his intervals up to twenty-four hours, excepting for the eight-hour period. Here he shifted the afternoon workers to the morning hours, when, as their learning-times show, they did more rapid work, lowering their time for the learning of a series from 8.2 to 6.6 repetitions (or one-fifth). Consequently, in the case of the eight-hour interval, the relearning was done in the later part of the day, when the observers did less rapid work (thus increasing their relearning-time by one-fourth). In a word, this variation in method, during the progress of his investigation, decreased the learning time by one-fifth, and increased the relearning time by one-fourth, in so far as these shifted

[15] A. Oehrn. Experimentelle Studien zur Individualpsychologie. (Kraepelin's) *Psychol. Arbeiten,* I, 1896, 92-151.

[16] W. B. Pillsbury. Attention Waves as a Means of Measuring Fatigue. *Am. Jour. of Psychol.* XIV, 1903, 277-288.

[17] J. Larguier des Bancels. Note sur les variations de la mêmoire au cours de la journée, *L'ann. psy.,* VIII, 1901, 204-213.

[18] *Op. cit.,* 95.

observers were concerned.[19] If we add to the learning-time this amount due to error (or substract it from the relearning-time), we find that Radossawljewitsch's observers then show a forgetting of 44 per cent. after the eight-hour interval instead of the 52 per cent. that he reported. This would smooth out his curve, and eliminate the enormous irregular deviation which is so characteristically present at this point.

If we select from our data those results which were obtained under conditions similar to those of Radossawljewitsch for the eight-hour period, we find that his curve and our modified curve are approximately identical at this point, but that the amount forgotten is slightly greater in the case of our observers. If we choose only those data (the length of time required for initial learning) obtained at the time of day when learning is most efficient for each observer, and if we base our measurement of forgetting upon data obtained eight hours after this most favorable time of day (see line *C* of Table III.), we find, as Radossawljewitsch did, that a relative excess of forgetting has occurred at the end of the eight-hour interval; but if, on the other hand, we arbitrarily select data dealing exclusively with times of day when learning is least rapid (see line *D* Table III.), we find that the curve plotted from these findings deviates in the opposite direction at the end of the eight-hour interval.

An appreciation of all of these considerations convinces one that the enormous deviation which characterizes the eight-hour point in the curve of Radossawljewitsch is not a product of purely memorial factors. It is clearly a product of variable extraneous conditions, chief among which is the variable time of day at which his observers learned and relearned their memorial material. It is a well-known fact that the influence of fatigue, of condition of nourishment, and of other purely physiological processes gives rise to a diurnal variation of mental efficiency. And there seems to be no doubt that the most characteristic finding which Radossawljewitsch obtained in his investigation is to be interpreted, not as a lapse in memorial efficiency indicating that a relative excess of forgetting normally occurs eight hours after learning, but is to be interpreted as a lapse of general mental efficiency which normally occurs toward the close of the day.

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[19] *Op cit.*, 44-47. These figures are obtained from the results of the twelve-syllable series, tables 19-22.

The results of Table III, together with the Radossawljewitsch curve (F) and the Ebbinghaus curve (G) are plotted in Plate II immediately following Table III.

TABLE III. EFFECTS OF THE MORE AND LESS FAVORABLE PERIODS FOR LEARNING, AS SHOWN IN THE RATIO OF RELEARNING-TIME TO LEARNING-TIME

THIS TABLE SHOWS THE EFFECT WHICH IS PRODUCED WHEN THE LEARNING-PERIOD FALLS AT A MORE FAVORABLE TIME OF DAY AND THE RELEARNING-PERIOD AT A LESS FAVORABLE TIME, AND VICE VERSA. THE NUMBERS REPRESENT THE RATIOS (COMPUTED FROM AVERAGES), EXPRESSED IN PER CENTS., OF RELEARNING-TIME TO LEARNING-TIME, WHEN:

A. The initial learnings were done at all times of day for the determinations of each interval, as in Table II.

B. The initial learning was done only at 8 A.M.

C. The initial learning was done at the period of most rapid learning for each observer.

D. The initial learning was done at the period of least rapid learning for each observer.

E. The relearning at the close of the eight-hour interval fell at the period of most rapid learning, for each observer.

Interval	*Half hr.*	*1 hr.*	*2 hr.*	*4 hr.*	*8 hr.*	*12 hr.*	*16 hr.*	*24 hr.*	*36 hr.*	*48 hr.*	*72 hr.*
A	25.0	27.2	30.6	33.6	34.5	36.2	37.0	42.2	41.2	44.5	47.5
B	19.2	20.9	33.1	36.8	37.1	41.5	....	42.0	39.4	49.0	47.9
C	26.1	21.6	28.5	33.4	*56.4*	44.7	....	42.5	31.6	44.5	46.5
D	19.8	21.3	22.1	21.3	*26.4*	42.0	42.0	36.7	33.0	48.3	48.7
E	19.8	26.0	24.3	23.4	25.4	....	....	34.0	42.0	47.0	50.1

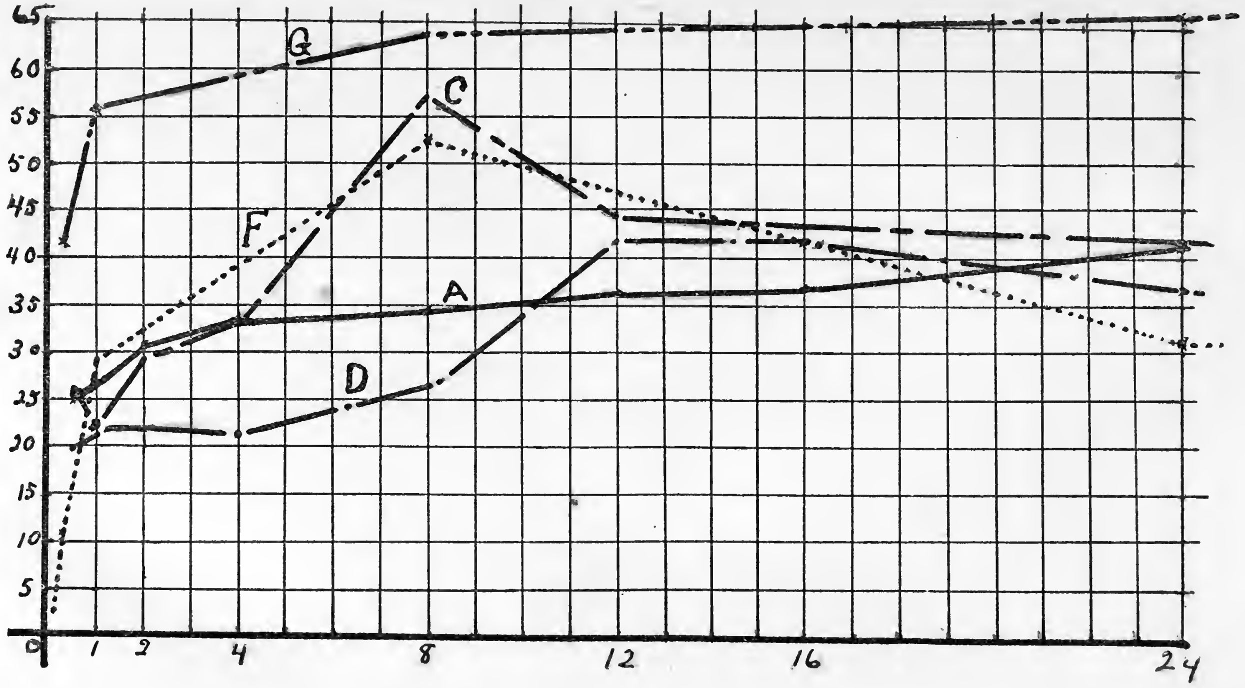
V. Conclusions

1. The rate of learning increases very rapidly during the first few sittings, then gradually approaches constancy.

2. The rate of learning varies with time of day. In agreement with Ebbinghaus and later investigators, we have found that the most rapid learning is done, by many observers, during the morning hours; of the times of day employed in this investigation, eight o'clock in the morning proved to be, on the average, most favorable, and five o'clock in the afternoon, least favorable for learning.

PLATE II. COMPARATIVE CURVES OF FORGETTING, SHOWING THE INFLUENCE OF DIURNAL VARIATION

These curves express, in graphic form, results which appear in Table III, together with the findings of Ebbinghaus (G) and Radossawljewitsch (F). The numbers in the ordinate indicate the amounts forgotten,—expressed in per cents.; the numbers in the abscissa indicate the temporal intervals,—expressed in hours. (In order to avoid excess of detail in this plate we have omitted plotting B and E; the reader may estimate the general form of these curves from the data contained in Table III, p. 22.



3. The initial and final syllables of a series are the first to be learned, and the central syllables of the series are the last to be learned. This result is in agreement with the findings of former investigators.

4. The order and position of the syllables in a series is recalled with greater accuracy than the syllables themselves. This does not agree with the results of Bean cited earlier in this paper; but his material consisted of nine consonants, while ours consisted of twelve syllables; and the difference between our findings is probably due in part to dissimilarity of material.

5. Absence of free reproduction does not mean complete oblivescence; hence the amount that has been forgotten can not be measured from a determination of the amount that can not voluntarily be recalled. However, the measurement of forgetting made by means of free unaided recall does in some cases correspond somewhat closely with the measurement according to the *Ersparnismethode.*

6. Our nonsense syllables give rise to few associations with words, except in the case of one observer for whom, in many instances, associations arise involuntarily.

7. The amount of forgetting for this observer is similar to that which Radossawljewitsch found for meaningful material.

8. No one type of imagery is strikingly superior to another as a means of learning or of remembering.

9. Our results do not justify any conclusion regarding the correlation of learning or remembering ability of men and women, because of our limited number of women observers.

10. Rapid learners may remember more than slow learners.

11. The distribution of learning-times throughout the day eliminates from the curve the error which is due to the variable influence of fatigue; failure to control this variable factor is undoubtedly the cause of the enormous deviation at the close of the eight-hour interval in the Radossawljewitsch curve.

12. The curve of forgetting for nonsense syllables in series of twelve, as determined by the lapse of time, is a uniformly progressive curve much as Ebbinghaus found; but under the conditions of our investigation, the progress of forgetting is slower than Ebbinghaus found it to be, and somewhat faster than Radossawljewitsch found.

THE ELECTRICAL SUPPLY, AND CERTAIN NEW ADDITIONS TO THE LABORATORY EQUIPMENT, IN THE STANFORD UNIVERSITY PSYCHOLOGICAL LABORATORY

By Lillien J. Martin

A. *Electrical Supply*

A different means of obtaining our electrical supply has been introduced since the publication of my earlier article.[1] The direct current which is employed for general laboratory work and for charging the batteries connected with the laboratory switch-board,[2] is generated by a Cooper-Hewitt rectifier [3] which has been substituted for the direct current motor mentioned in my earlier article. The alternating current is supplied to the rectifier from the electric switch-board.

B. *Color-Mixer*

The color-mixer which is illustrated in Figure II has been found to be much more convenient in the beginners' laboratory work in psychology than the cheaper forms which are less readily adjusted and of less constant motion. In general, it may be said that with beginners the better forms of apparatus have proved to be more satisfactory. This motor is a Westinghouse Alternating Current Motor, 1/12 H. P., 110 Volts, 60 Cycles, 3400 R. P. M.; Style No. 63782 A. It is fitted with an arbor for discs; and it cost sixteen dollars.

Figure III shows how these motors may be combined for class demonstrations in the simultaneous comparison of the different phenomena of color-mixing. The use of individual or of general switches brings the motors into action singly or in groups.

C. *Adjustable Discs*

"Paste (or pin) a white ring upon the red disc, centering it as accurately as possible. The ring is much brighter than

[1] *American Journal of Psychology,* xvii, 1906, 274-279.
[2] The rectifier and the switch-board are illustrated in Figure I.
[3] The rectifier is manufactured by the Westinghouse Electric Mfg. Co., Pittsburgh, Pa., at a cost of seventy-five dollars. It is described as 110 A. C. volts, 5 D. C. amperes, 15 D. C. volts; Style No. 104936.

its red back-ground. Paste (or pin) sectors from a black ring over a portion of the white, and set the mixer in rotation." Titchener's Experimental Psychology, I, 1, 16. Matters may be facilitated by the following modification of procedure: Instead of pasting, use three superposed discs, made as illustrated in Figure IV. The large and the small discs of each set are cut from the same sheet of colored paper; the intermediate disc contains sectors of black and of white, which may readily be adjusted to give the required brightness.

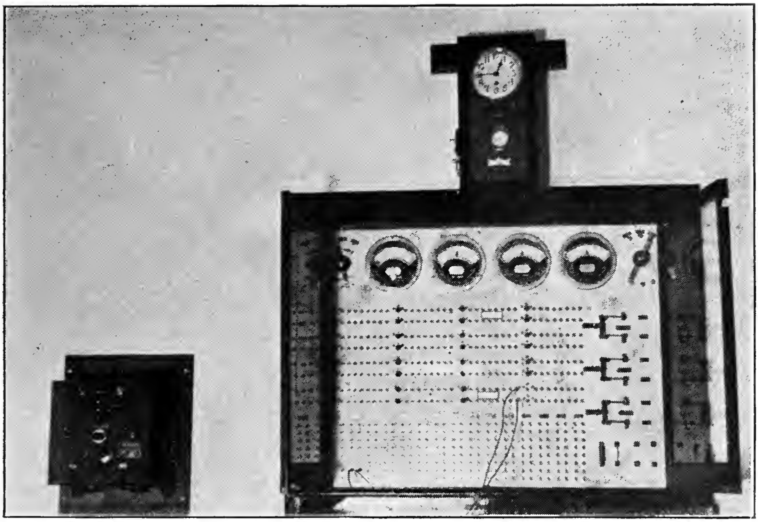


Figure I

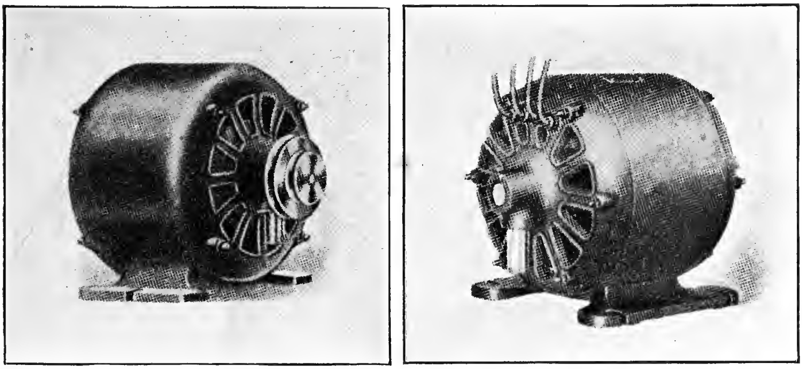


Figure II

34 B.

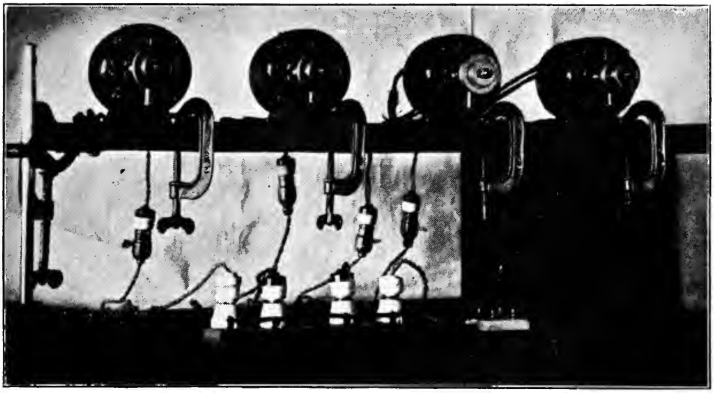


Figure III

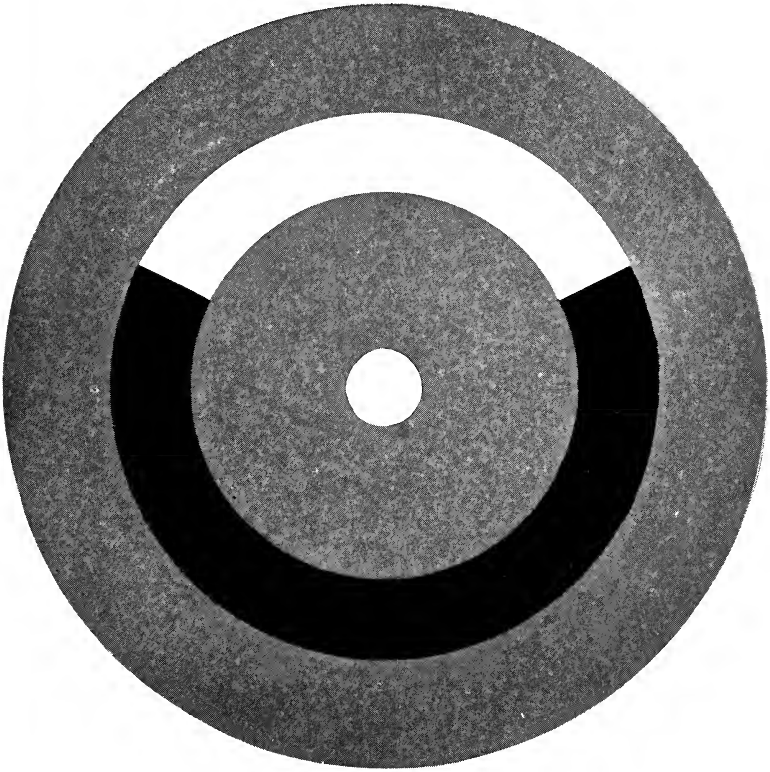


Figure IV

PRACTISE AND THE WORK-CURVE

By Frederic Lyman Wells, Ph. D., McLean Hospital, Waverley, Mass.

The experimental results to be here presented concern the effect of practise in a function upon endurance in that function; but as this is a small arc of the ever-widening circle of problems in the curve of work, some review of allied literature is desirable for an appreciation of the question's setting. In some ways it is not altogether fortunate that the investigations of the curve of work have been largely dominated by the concept of fatigue. This must be mainly due to the fact that the best-known studies of the work-curve dealt with methods in which fatigue happened to be the dominant factor. Work-curve and fatigue-curve become practically synonymous, and we incline to interpret all results of this nature in terms of a paradigma of the Mosso ergogram. We know, however, that the work-curve varies essentially according to the function that works. Half an hour's series of discrete simple reaction times should show little if any fatigue loss, but a great amount of practise gain; while a much shorter period of work with the Mosso ergograph would bring the curve to a close without the superficial trace of practise gain. Other measures, as of the controlled association type, may show at first a sort of primary practise gain, after which " fatigue " gains a certain mastery; or the work may continue indefinitely without marked fluctuations.

In a paper of some four years ago, the writer discussed the comparative value of various measures of fatigue, with special reference to the tapping test.[1] The idea upon which most studies of fatigue measurement are founded, as well as that which underlay this paper, was one of some definite test which should give a criterion of general fatigue conditions or susceptibility in the individual. This conception of fatigue measurement is one closely adhered to on the educational side, in obvious consideration of the usefulness of reducing the problem of fatigue determination in school-pupils to that of a single, simple test. The possibility of doing this depends upon the correlation of fatigue phenomena in the test with

[1] *Amer. Jour. Psych.* XIX., 1908, 345-358.

those of other functions, and of them with each other, though the facts of fatigue correlation are still but imperfectly understood. There is, in fact, no special reason to suppose that a person fatiguable in the tapping test would be so in the ergograph. There may, it is true, be generalized conditions of fatiguability affecting the nervous mechanism as a whole and all that depends on it; but we have an at least equal possibility that the function of fatiguability shows as little correlation in various psychological tests as do the gross efficiences in them. One would not seek to judge a person's reaction time by his memory for musical pitch, and we have yet to show that there is certain justification for estimating the fatiguability of one function by the fatiguability of another.

It is well known that, on largely empirical grounds, there have been sought certain indirect measures of the state of fatigue. According to Yoakum, the best promise of these would seem to lie in the direction of the sphygmograph. Offner does not regard it so favorably. Aside from some practical difficulties in the way of this particular measure, remains one from which all indirect ones seem inseparable; they will not serve as measures of individual differences in fatiguability because it is not practicable to know how the relationship of the two functions differs in different individuals (or for that matter, in the same individual at different times). To put the matter more concretely, suppose that there exists a degree of correlation between certain features of the pulse-curve and the condition of fatigue. It presumably manifests a certain sort of influence which the fatigue metabolism exercises upon the circulatory system or upon the nerve-elements that affect it. The degree to which it is affected by a specific fatigue condition will depend upon its degree of responsiveness to the changes that are thus induced. The same state of fatigue could well therefore have different effects on the pulse-curve at different times or in different individuals. Ritter expresses analogous opinions, also from the educational side. The only way to get over this difficulty would seem to be to bring forward empirical evidence that the error it introduces is not sufficiently large to vitiate the practical application of results.

There is a basic difference between two methods of psychological experiment involving continued series of reactions, whether or not they be used as determinants of a work curve. We may measure the amount done at maximum speed, or we may measure in terms of errors at a moderate speed selected by the subject. Obviously, the same experiment is not suited to evaluation by either method alone, though an endeavor to combine the two has often been made. In the first case, the endeavor is to make the experiment as free from error as possible; in the second, to ensure that it shall contain a fair percentage of errors at any rate the subject chooses. The great body of research is along the first named lines, the only extended one of the second type being that of Yoakum; but each has its advantages. The former, of which the ordinary tapping test is a type, gains in that false reactions are practically eliminated, and there is but one thing to be measured, speed; whereas in the error method the accuracy is influenced by the degree to which the 'at will' rate approximates the maximal rate, which ratio is bound to vary with the individual and with time. This is a very serious limitation where practise effects are to be compared. On the other hand, it would seem that the 'at will' rate, with scoring of false reactions, is less artificial, and corresponds more closely to the conditions of general activity. It is rather the 'at will' pressure of activity, with more or less incoherent reaction, that obtains in our daily tasks. Furthermore, as Yoakum strongly insists, such a method is less apt to be vitiated by irrelevant motor factors. It seems to the writer that the principal issue between the two methods lies neither here nor there. In the case of maximal effort we can scarcely do otherwise than interpret gradual, even though unsteady, decrease in efficiency as manifesting influences which act directly, and with some degree of continuity, upon the functions involved. That is, such factors as the actual wearing out of the mechanism; the development of fatigue toxins; or, to a less extent, reflex inhibition from fatigue sensations. In the 'at will' rate of tapping a pattern, the losses in efficiency (as given in accumulated errors) seem to be the manifestation of essentially episodic interferences. What we have here is a function whose efficiency, within ordinary limits of continuance, is not much impaired except as it is crossed at pretty definite periods by factors that produce a temporary upset in the adjustments. Such phenomena may also be seen in work at maximal rate, though while their presence in one form or another can be noted in almost any sort of continual activity, the method which Yoakum has developed from Squire is perhaps the most convenient approach to their study that has been found. The deeper causes of these interferences and the individual differences in liability to them are matters whose interest extends considerably outside their relation to the problem of fatigue. Much might be learned by the properly controlled analysis of the mental processes attendant upon such failures of adjustment. Psychologically, the method is a measure of general attentional control, rather than of the efficiency in forming a series of definite associations, as the addition test or the cancellation tests. While its data are quite as important for the problem of individual differences, it seems probable that we get at least as legitimate a form of the work-curve in measures of the latter type. Of these, the historical method for the study of the work-curve is *par excellence* the addition test. Its special advantage over other directly intellectual methods is supposed to lie in the dependence upon fewer intellectual factors, and greater consequent reliability of interpretation. The Kraepelinian investigations constitute one of the most patient and thorough-going studies of a problem of its scope which occur in experimental psychology; they have been often reviewed, and, save in some special relations to the problem, it can scarcely be necessary to discuss them here.

In the published accounts it is not always clear to the reader in just what way the Kraepelinian *Rechenheft* was used. It is possible to employ it in various ways; the digits may be added cumulatively, starting afresh with each hundred, or each digit may be added simply to the next,—the latter procedure being that followed in the present instance. The way in which the experiment may be recorded also varies. The addition may be silent, the subject merely marking with a pencil the point reached at the end of each minute or other interval, indicated by a signal. This is disadvantageous since it affords no objective account of the work done. The subject may write the answers, or only the last digit of them; this is objectionable on account of the purely motor time involved.[2] The subject may indicate by a checkmark the completion of each addition, which is electrically recorded; this gives the appearance of recording the time of each addition, but really depends upon the accuracy with which the subject can estimate the process, which is more than doubtful. Von Voss, who originated this precedure, is quoted favorably and often by Yoakum; this investigator, elsewhere quite on his guard against the motor distortion of a mental work-curve, having apparently overlooked its possibility in this particular instance.

Since we must have a motor response, let it be one indicative of the work done, and most natural for the subject. The speaking of the sums would seem to be indicated, the experimenter following on a key and noting errors. If it is desired

[2] Woodworth and Wells, Association Tests, *Mon. Supp. Psych. Rev.* 57, 1911, 9-11.

to record individual processes, it can be done by recording graphically the subject's speaking into a mouthpiece; a thistle funnel will usually answer, or a stomach tube. The Kraepelinian workers have always insisted that the errors with the experiment could be disregarded; and in normal subjects it is the rule that the errors do not essentially affect gross scores. Nevertheless it is always desirable to know to what extent errors are present, and quite obligatory where there is any question of coöperation.

It is, of course, a familiar fact that the work-curve in the addition test and elsewhere represents not merely the interaction of practise and fatigue, but is the resultant of a complex of factors, some of which are favorable, others unfavorable, and most are of an evanescent character. Not only has the existence of these factors been recognized on theoretical grounds, but considerable effort has been made to analyze and determine the precise influence of each one of them upon the final work curve. The most conspicuous study from this standpoint is Specht's comparative examination of fatigue phenomena in normal individuals and in traumatic neuroses. Here, based upon a careful review of the literature, the *Pausenversuch* is specially treated with a view to separating out a specific fatigue phenomenon from all the other factors influencing the curve. Briefly stated, the contribution which the *Pausenversuch* purports to make to the simple methods of fatigue evaluation seems to be this: Given a work curve, say in the addition experiment, consisting of ten minutes uninterrupted performance, and a similar work curve under the same experimental conditions save that between the two five-minute halves a five-minute rest intervenes. One must not estimate fatiguability merely through the course of the continuous work curve, because it is subject throughout to practise gain as well as fatigue loss. In the losses of these days without the pause is expressed the extent to which fatigue overbalances practise; and if we can find some way of calculating just what this practise gain is, then we can determine how much it has offset the real phenomena of fatigue. The amount of this practise gain is supposed to be given in the relation between the performances of the second and sixth minutes of the pause experiments. Given then a performance (with the pause) eliminating the influence of fatigue, and another (without the pause) including it, the relation between the two represents the true loss by fatigue.

To the close reasoning of Specht's paper, the following criticisms are to be adduced. The calculation of fatigue and practise from the relationship of performances at certain points in the tests rests on the assumption that these performances are essentially determined by fatigue and practise. To the extent to which these performances are influenced by such more ephemeral factors as *Antrieb, Anregung* and the like, their indications of fatigue and practise will be inaccurate. Thus an essential ratio is taken from the second and sixth minutes instead of the first and sixth, to obviate the *Anfangsantrieb* of the first minute. This is very well if one has objective means of knowing that *Anfangsantrieb* did vitiate the first minute and not the second. But if *Anfangsantrieb* unduly speeded the first minute, the performance of the second minute may be lower than otherwise, and the procedure would unduly favor those whose effort in the experiment is not sufficient to exhibit *Anfangsantrieb* or those in whom it appears more slowly. It would also be difficult to exclude, save on introspective grounds, the presence of *Anfangsantriebe* at the beginning of the second five minutes after the pause, though it is stated that they are very much less at this time. It is further possible that there is between the two halves of the experiment an *Anregungszuwachs,* "warming up" gain, that is increased under practise, as in the tapping test.[3] In the early stages of practise, the manifestations of warming-up and recovery are in the tapping test so irregular that if the same is true, in any measure, of the addition test, ratios involving the pause are very unreliable indeed. It might be said of the studies of the addition test in general that they are not sufficiently concerned with the possibility of practise effecting great changes in the relative importance of different factors in the curve. On the empirical side, the Kraepelinian analysis of the work-curve has recently undergone a searching and destructive criticism at the hands of Thorndike. According to the points brought forward in this paper, there is very little objective warrant for assigning to these factors essential import in the addition-curve at all. It would seem that the devising of these terms was an attempt to explain away certain irregularities in the work-curve in reality not subject to experimental or introspective control. The entire paper is as forceful an argument as has been presented against the application of deductive gymnastics to the study of the work-curve, and in favor of taking its phenomena so far as possible as we find them.

Some space may be reasonably devoted to these matters, since in view of the great amount of labor devoted to this

[3] *Amer. Jour. Psychol.* XX., 1908, 457-461.

analysis of the addition test, in experiments quite similar to those described below, some explanation should perhaps be given of why very little procedure of this sort is attempted here. While no one would deny a measure of existence to these different processes, they constitute too many unknown and irregular quantities in the one equation of immediate efficiency; and the writer cannot muster sufficient confidence in the quantitative analysis of these factors to feel that it is wise to attempt it. We can most nearly approach it in certain rather constant phenomena of the later stages of practise, more strikingly perhaps, in the tapping test than elsewhere. But, to quote substantially from a previous paper:

"The pragmatic significance of such measurements lies less in the determination of such abstractions as absolute fatiguability, warming-up, or impulse effects, than the objective way in which the individual responds to experimental conditions demanding the continued exercise of maximal voluntary effort. In every individual, and in every measure, the factors that determine the course of the *Arbeitscurve* have a certain way of balancing each other; and the way in which this balance varies in different individuals, and under different conditions. constitutes the essential problem of the curve of work."[4]

Attention is also called by Thorndike to the irregularity of work-curves executed by the same individual and in the same measure. It can readily be seen in the records discussed below. There are certain major features of the work-curve, including total efficiency, that regularly permit the demonstration of individual differences; in details, the factors that influence the form of the work-curve are manifestations rather of the special conditions under which the work happens to be done than of peculiarities inherent in the individual subject.

Thorndike's attitude toward the general problem is much less Procrustean than that of the Kraepelinian workers. His earlier experiments emphasize its complexity, especially the rôle played by the feelings of fatigue. Educational aspects came to the fore in a second paper, the conclusions of which pointed markedly to the feeling of fatigue as the proper point of attack, rather than the supposition of overwork. Thus we see that decreases in efficiency under continued effort, however certainly shown by well-conceived experimental method, are not necessarily the product of mental *fatigue*. The tendency of these papers was decidedly to discount this interpretation of the phenomena, and subsequent experiments have borne out this view. Some practise experiments in multiplication, re-

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[4] *Cf.* Motor Retardation as a Manic Depressive Symptom, *Amer. Jour. Insanity,* 66, 1909, 5-6.

ported in 1908, were especially instructive in showing the extent to which, in minds of considerable general training, the function was still subject to practise. A positive correlation was, however, suggested between general intellectual achievement and susceptibility to practise, which assumes further significance in connection with the similar correlation of initial ability with susceptibility to practise. Experiments reported two years later showed the contrary relationship for addition in *percentile* gains; but there are objections to regarding this consideration of the gains as the correct one. In gross improvement, these results rather indicate the greater absolute gains upon the greater initial efficiency, as other, slightly different experiments have also shown.[5] An arduous series of multiplication tests regularly produced a decrease in efficiency, the more efficient individuals being the more resistant to this decrease; this is only slightly noticeable in the first periods of the present addition experiments.[6]

Wimms has published a series of experiments which deal with the relative susceptibility to fatigue and practise, though not directly with the effect of one on the other. This investigator, more under Kraepelinian influence, employed the conventional addition experiment and a more difficult multiplication task. The addition experiment showed some positive correlation of improvement and absolute amount with the multiplication tests; but the improvability in the tests is not correlated, nor does absolute amount in multiplication correspond with improvability. The present writer ventures to believe that the greater susceptibility to practise of the multiplication test may be due in no small degree to its lesser complication with motor factors. In respect to fatiguability in the two tests, as calculated through the *Pausenversuch,* some changes with practise are incidentally noted. There is no uniform tendency; in some subjects the fatigue phenomena increase with practise, in others they decrease. In general, fatiguability in the addition experiment increased with practise, contrary to the present results, while in the multiplication test the fatiguing tendency decreased. A second group of experiments dealt with mental multiplication of the same character as is described by Thorndike. The results for two-place figures show the greater improvement with the greater ability, as noted in the work of Thorndike, and the earlier addition results; but this relationship is again broken down in the harder task. The fatigue phenomena are irregularly affected by practise,—a general

[5] *Amer. Jour. Psychol.* XXIII., 75-88.
[6] See page 46.

decrease being noted in the harder, and an increase in the easier experiment, as before. The paper deals with many other relationships in improvability, fatiguability and retentivity, though the treatment is throughout a static rather than a dynamic one.

The 'method of equal groups,' as developed by Winch, furnishes perhaps the most immediately promising line of research in the educational problems of fatigue. One such investigation by this author, of some three years since,[7] dealing with the value of evening school work, has already been described by the writer. A somewhat more extensive study is reported by Winch in 1911 concerning the degree to which school performance varies with the hour of the school day. The phase of the question considered was that of arithmetical problems. Equal groups having been arranged on the basis of preliminary tests, in which the conditions were the same for all subjects, final tests were made in which one group worked earlier in the school-day, the other later. Four series of experiments, with as many different classes, were carried out. The final tests are in both groups apt to be better than the preliminary ones ('practise'), the general relations are expressed in the following figures:

IMPROVEMENTS OVER PRELIMINARY TESTS

	Morning group	*Afternoon group*
Infants.............	12.0%	0 approximately
Girls..............	14.6%	7.9%
Boys (1)...........	18.3%	11.3%
Boys (2)...........	7.2%	4.4%

In general averages, the group which works in the morning is uniformly better than that which works in the afternoon; the differences are nowhere great, and they tend to decrease as more mature pupils are tested. Then, as suggested in Winch's paper, other topics could be compared to see which lost the most from morning to afternoon, and least favorable times be given to subjects that would suffer least. It will be appreciated, of course, that these experiments are not so designed as to throw direct light on questions of the individual psychology of fatigue; but in their appropriateness for the solution of some concrete educational problems, they seem to constitute a rather effective rejoinder to the opponents of the 'class experiment' in this field. A problem correlative to the writer's present results, and suggested by Winch's

[7] *Jour. Educ. Psychol.* I., 1910, 13-23, 83-100.

inquiry, is that of how a given state of fatigue may affect the ability to improve by practise; *i. e.,* would Winch's afternoon work have improved by practise as much as his morning work? While at least an equally important question with the present one, it is probably but just to add that it seems many times more arduous owing mainly to the apparent lack of correlation in the practise improvability of various mental functions, and the consequent difficulty of controlling the observations. This would be necessary if we are to discover how much the fatigue state had affected the individual's capacity for practise improvement. Nothing but the practise-curve tells what the practise curve will be, though an idea of the general relation of the state of fatigue to practise improvability could, of course, be obtained from comparative practise experiments with fresh and fatigued subjects, in groups sufficiently large to offset the chance errors of selection.

The experiments from which the present results are calculated have already been described. In the addition test they consist of five-minute records, scored for every minute, with ten subjects, five men and five women, through thirty experimental days. The subjects are the same for the number-checking test; but there are considered only the results of the twenty days upon which five records were taken with each subject. The tapping test involves two subjects only, also for a practise extending over thirty days.

For the purpose of the present computations, the records of the addition test were collected into six divisions, covering five days each. In each division, from days 1-5 to days 26-30, the averages for the first minute were calculated, then those for the second minute, etc. We have then for each subject six work-curves, each made up of the averages of performances on five successive days. The question is whether the work-curves of the first division show any characteristic deviation from the later ones, which have more practise behind them.

The most salient feature of the work-curve is probably the endurance which it shows, *i. e.,* the extent to which subsequent efficiency is maintained up to or beyond the point of initial efficiency. To illustrate this for the tapping test, we adopted the procedure of stating the ratio of the initial measure to the *average* of those that immediately succeeded it. This measure was termed an 'index of fatigue,' or *f,* though there now seems to be no special reason for giving it that name except that the work-curve in the tapping test happens to be normally dominated by fatigue phenomena. The measures should be more properly regarded as an 'index of endurance,' and is so considered in this paper, its previous denomination resulting from a one-sided outlook upon the processes involved. The measure then signifies the percentage which the average of the subsequent divisions of the work-curve is of the initial division. *If it is above* 100, *it indicates a general increase in efficiency; if below* 100, *it indicates a general decrease.*

The answer which our material furnishes to the question of how practise affects endurance in the work-curve is thus presented in a comparison of the *f*'s calculated from the curves at the different stages of practise. The complete table for the addition-test is shown in Table I.

The results are presented separately for each subject, and calculated separately for the men and women. There is an unmistakable tendency for the *f*'s to rise, more marked in the women than the men, and in fact there is a regular rise in the general average. The *f*'s are, with one exception, below 100; and it is seen that it is the initial efficiency which is normally the greatest in these records of the addition-test. The *f* for days 1-5 is the lowest of all, except in subject VI, where the second equals it, and in subject X, where it somewhat surpasses that for days 6-10. The general effect of practise upon endurance is therefore in this test a favorable one; that is, the subject does not lose so much by fatigue in the later stages of practise as in the earlier ones; or, to put the matter in another way, the initial division of the work-curve does not gain so much by practise as the subsequent ones do.

The following suggestion may be made in interpretation. The susceptibility to practise gain should decrease as the amount of practise increases. The series of the later days should not therefore be subject to so much practise gain within the single experiment as the earlier. As a matter of fact, they show more gain. The indication would seem to be that another favorable influence is present, which differs from practise in its ordinary conception, and whose effects are increased rather than lessened through continued practise. Such an influence we have already observed with the tapping test, in the 'warming-up' phenomena from series to series, much accentuated by practise. Here in the addition test, however, we seem to have a warming-up effect that is not dependent upon the pauses, as in the tapping test, but tends materially to counterbalance fatigue losses within each individual work-curve. We might conceive of it altogether in terms of increased immunity to fatigue, but for the fact that in single records the performance of the initial minute is occasionally

TABLE I

F's in the Addition Experiments

Subject....	Men Subjects							Women Subjects							Av. of Both Groups
	I	II	III	IV	V	Av.	M.V.	VI	VII	VIII	IX	X	Av.	M.V.	
Days 1– 5.......	87.6	84.7	93.7	84.1	90.9	88.2	3.3	89.7	89.4	89.6	86.2	89.9	89.0	1.1	88.6
" 6–10.......	93.1	85.1	94.2	90.5	102.1	93.0	4.2	89.7	94.5	94.3	86.8	88.4	90.7	2.9	91.9
" 11–15.......	93.1	86.1	96.3	88.0	96.0	91.9	3.9	92.0	90.4	93.9	90.1	94.4	92.2	1.6	92.0
" 16–20.......	92.1	88.3	92.3	86.3	96.6	91.1	3.1	97.5	93.6	90.6	93.7	92.1	93.5	1.7	92.3
" 21–25.......	93.8	93.5	95.8	85.4	96.1	92.9	3.2	97.8	99.4	90.2	94.9	92.3	94.9	2.9	93.9
" 26–30.......	94.0	92.1	99.2	91.6	95.9	94.6	2.4	95.3	93.4	91.6	96.2	96.3	94.6	1.8	94.6
Av. for each subject...........	92.3	88.3	95.2	87.6	96.3	91.9		93.7	93.4	91.7	91.3	92.2	92.5		92.2
M.V. for successive days..........	1.6	3.0	1.8	2.4	2.1	2.2		2.9	2.4	1.6	3.6	2.1	2.5		2.4

surpassed later, though uniformly enough to be evident in the average only in days 6-10 of subject V.

While the general trend of the records is thus towards better endurance under practise, the various subjects differ obviously in the extent to which they show this trend, and in the extent of their endurance *überhaupt*. Independently of practise, the staying power in the function is distinctly superior in subjects III and V to what it is in subjects II and IV. While the average endurance in the men and women is practically equal (91.9, 92.5), there is a pronouncedly greater variation among the men. The mean variation of these general averages for each subject is 3.2 for the men; for the women, .9. If we consider the mean variations in the two groups of men and women subjects separately for each five-day period, we find them consistently smaller in the women, the average being 3.3 for the men, for the women 2.0. This is a rather more marked difference than the similar one observed in the work-curve of the tapping test. But as was brought out in this previous paper,[8] it is uncertain that this relationship would be maintained in work-curves of longer duration. It may also be noted that the progressive improvement of endurance is somewhat more regular in the women than in the men.

With the number-checking test the conditions are different. The work, which consists of executing in succession five of the number-checking blanks described by Woodworth and Wells,[9] is not continuous, but interrupted on account of the times necessary to substitute a fresh blank. The length of these pauses could not have varied greatly, though it was not so precisely controlled as it would have been had the experiments been undertaken with the present calculations in view. For reasons already mentioned, only twenty days, *i. e.,* four groups of five experiments each, are available in this experiment, and they do not represent the beginning of special practise in the test. When calculated in the same way as the addition test, the results appear as in Table II.

The main differences between these figures and those of the addition test are that the *f*'s are, with one exception, well above 100, indicating increased efficiency over the initial work period, instead of decrease (in view of the pauses, we should probably expect it), and that there is little if any progressive alteration of this increase, with practise. Some individuals, as subjects VI and VII, show the same phenomena as were regularly observed in the addition test; but others, as I and IV, show rather the reverse of it. The salient fact is that the

[8] *Amer. Jour. Psych.* XX, 359.
[9] *Loc. cit.* 24-29.

TABLE II

F'S IN THE NUMBER CHECKING TEST

	Men Subjects							Women Subjects							Av. of Both Groups
Subject....	I	II	III	IV	V	Av.	M.V.	VI	VII	VIII	IX	X	Av.	M.V.	
Days 11–15[1]......	103.1	103.9	108.8	110.6	107.3	106.7	2.6	103.5	101.8	109.4	108.6	103.1	105.3	3.0	106.0
" 16–20.......	107.9	98.5	114.4	117.6	106.4	108.9	5.6	110.9	103.8	108.7	103.9	102.6	106.0	3.1	107.5
" 21–25.......	103.0	101.4	108.2	110.9	107.5	106.2	3.2	10.9	105.3	103.4	111.0	105.0	107.1	3.1	106.6
" 26–30.......	103.6	109.4	106.3	109.5	103.4	106.4	2.4	108.3	108.0	105.7	110.0	105.5	107.5	1.5	107.0
Av. for each subject...........	104.4	102.6	109.4	112.1	106.1	107.0		108.4	104.7	106.8	108.4	104.0	106.4		106.7
M.V. of this average............	1.7	3.1	2.5	2.6	1.5	2.3		2.5	1.9	2.2	2.2	1.2	2.0		2.1

[1] The records of days 1–10 include but one test on each day, and are not available for these calculations. Cf. Am. Jour. of Psych., XXIII, p. 77.

subsequent performances uniformly show marked improvement over the initial ones of the several experimental days. This improvement is more analogous to that noted as 'warming up' in the tapping test, but it is not regularly accentuated by practise. As there, the improvement is evanescent, being regularly lost from day to day; so that the best series may be some days' practise ahead of the initial one. Kraepelin's pupils might speak of *Anregung* or of marked *Uebungsfähigkeit bei geringer Uebungsfestigkeit.* The argument against its being in the nature of practise is that it should then become less marked with increasing practise, which it shows no general tendency to do. It is rather a process which repeats itself *de novo* from day to day, on the basis of the true practise gain.

But while there is slight progressive change of the endurance in this test, made in this manner, there is marked individual variation in the extent to which different subjects manifest the 'warming up' effect. As before, there is not much difference between the men and the women subjects, as groups; but the extremes are in each case found in the records of the men. The *f*'s of the subjects could be arranged in a fairly reliable order, which, it may be remarked, would differ rather widely from that in the addition test above. This would indicate that *Anregbarkeit* is not a general property of the individual, but might be great in one function and small in another, as was previously found to be the case with susceptibility to practise improvement.

For an applied psychology, at least, the interest of such data does not end with showing the presence or absence of a tendency that is pervasive. A measure of central tendency is of at least equal significance as a fixed point by which to measure the relative distances of individual variations from it and from each other. If, in respect to the effect of practise on the work-curve, some persons show one tendency and some an opposite tendency, it is clear that no universal rule for the effect of practise on the work-curve can be laid down; but it is also necessary to know whether the presence of one tendency or another, in definite degree, is a fundamental property of the individual. Different persons may vary widely about their group average; but if their differences are characteristic, they are much more important psychologically than a central tendency which may be fairly reliable for the group, but about which the same individual may vary now in one direction, now in another. Indeed, the greater the mean variation of a group average, the greater the possibility of genuine and significant individual differences about it.

That such individual differences exist in the present results has already been mentioned; *e. g.*, the inferior endurance of II and IV in the addition test, and the superior gains of III and IV in the number-checking test. Given such differences, it remains to be seen whether this is the limit of their significance. It is a general experience of experimental psychology that the interpretation of its present tests is confined rather closely to the special function of each test itself. There is little in the present results to change this view. The favorable positions in the addition test do not correspond well with those in the number-checking test. In the addition test, the Pearson coefficient of the gross efficiency for days 1-5 and the endurance of those days, is +.28, and even this relationship disappears in days 26-30, where it becomes .02. The number-checking test shows no significant relationship between gross efficiency and response to the conditions of *Anregung*. These results therefore confirm the results of previous investigations of the subject, not only that the proper study of the work-curve is the work-curve; but also such a work-curve as shall approximate in the closest possible way to the conditions under which the more vital activities of the subjects take place.

Another paper has presented in detail the effects of practise on the work-curve in the tapping test. They need be summarized only for comparative purposes here. In the main question, the results of the two subjects disagreed, subject I showing poorer endurance under practise, subject II better endurance. The practise improvement in this function is but a small fraction of that in the tests described above. These experiments thus afforded no reason to suppose a general effect of practise on the form of the single work curve, in the sense of that in the addition test. On the other hand, distinct evidence appeared in both subjects that practise improved the effect of the pauses, and that the principal way in which it improved them was by giving immunity to fatigue loss. This would again argue for a separation of the gains of *Anregung* and of *Uebung*. In all experiments we seem to see some kind of favorable influence on the work-curve,—be it one of the immunizing it to fatigue, or raising its general level,—whose effect is but exceptionally lessened by daily practise within ordinary limits, being, on the contrary, rather increased by it. It would seem then that we might best bring together certain favorable effects of practise on the form of the work-curve under the conception of an increased response to *Anregung,* recognizing, however, that this response may show itself (1) in better endurance in the single work-curve (the addition test); (2) in an increasingly favorable effect of the pause (the tapping test). In the number-checking test, this effect does not seem to be general; the phenomena of warming up are present, but are slightly and variously affected, in some persons favorably, in some otherwise, under the influence of practise.

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PARAMNESIA IN DAILY LIFE

By Theodate L. Smith

The word paramnesia as it occurs in this paper is used in its broader meaning and is applied not only to the phenomenon of apparent familiarity with something previously unknown, the *déja vu* of the French to which it is sometimes restricted, but to the whole group of errors or illusions of memory as usually distinguished from amnesias, but which I believe can be shown to involve an element of amnesia upon which the falsity depends. Some years ago, in consequence of a personal experience, my interest was aroused in these phenomena as they occur in normal individuals; and I have since then, as opportunity occurred, recorded cases of which I was able to obtain a more or less complete analysis. The material thus accumulated now amounts to about forty-five cases, which fall into three groups or types which are explicable in accordance with the psychological laws of memory.

Memory images as distinguished from those of the imagination are characterized by a conscious reference to the past, however dim and vague this may be; and if this is lost, it becomes impossible to distinguish between the two,—a fact which is sometimes of considerable importance in the explanation of plagiarisms which may, from this cause, be absolutely unconscious and thus quite innocent of any intentional deception. Helen Keller's well-known unconscious plagiarism at the age of twelve, which caused her so much unjust suffering, furnishes an excellent example of such a case in which the associations with the past having been lost, a story written by Miss Canby was reproduced as her own. The circumstances were as follows: The autumn after Helen had first learned to speak, she spent summer and fall at the summer home of her family in Alabama and Miss Sullivan described to her, in her usual vivid fashion, the beauties of the autumn foliage. Helen wrote a little story called "The Frost King" which she sent to Dr. Anagnos as a birthday present. The story was a remarkable production for any twelve-year-old child; and for a blind child, a marvel, abounding as it did in vivid descriptions of color. Dr. Anagnos was greatly pleased with it and published it in "The Mentor." A few weeks later this story was discovered to be an almost verbatim reproduction of a story written years before by Margaret T. Canby and published in a book called "Birdie and His Friends." Miss Sullivan had never seen this book and Helen, though finally convinced that she did not originate the story, could recall absolutely nothing of the way it had come to her. So far as she was concerned, the story, in spite of all her painful efforts to recall the circumstances by which it had come into her mind, still seemed to be her own creation. The explanation was finally found in the fact that four years before, Helen and Miss Sullivan had spent the summer at Brewster with a friend, Mrs. Hopkins, who possessed a copy of Miss Canby's book and who probably, though she could not definitely recall doing so, read it to Helen during Miss Sullivan's absence on a vacation. Helen had at that time been under Miss Sullivan's instruction scarcely a year and a half and had learned her first word after Miss Sullivan's arrival. The story was read to her by the only means of communication then possible, by spelling the words into her hand. It could have conveyed little or no meaning to her mind, but the spelling of strange words probably amused and interested her. It is little wonder that, when four years later the words came so readily to her pen, all previous associations with them should have been lost and they should seem her own. Many years later Miss Keller wrote, "It is certain that I cannot always distinguish my own thoughts from those I read, because what I read becomes the very substance and texture of my mind." Though it is natural that localizing associations should be more readily confused in the absence of visual and auditory sensations, this confusion is, in varying degrees, a common phenomenon of memory. An instance in which this confusion appears in reversed form is illustrated by the experience of a very bright woman who during a discussion on literary topics quoted a very apt passage from Shairp, the English critic, which she had read a day or two previously. In looking up this quotation, however, she found somewhat to her confusion that it was non-existent, being, in fact, her own commentary upon a passage which she had read in Shairp.

The attribution of quotations or ideas to wrong sources is so common as to need no illustration; and the feeling of certainty attached to these distorted memories is often exceedingly strong so that a rummage through the entire works of an author may fail to convince the subject that he has not somehow overlooked the passage sought. Misquotations, also, in which perhaps the idea of the author is altered or even completely reversed, may be accompanied by this same feeling of certainty as to the correctness of the version given.

In every complete normal memory three elements may be distinguished: (1) a past experience belonging to me; (2) belonging to me in a particular manner, i. e., as something which has originated through sense-experiences or as a mental activity of which the concomitants are reproduced with more or less fullness; (3) the experience is located in past time with more or less definiteness. In paramnesia, the illusion or distortion may be due to the impairment of any of these three elements. It may consist in the transference of another's experience to oneself or *vice versa;* in the addition of false concomitants or imaginary additions to actual events; in the dropping out of some necessary concomitant; in a confusion of mental and sensory experiences; in an apparent recognition of objects really seen for the first time; or in a false localization in time. Paramnesias have until very recently been chiefly studied in connection with hysteria or insanity where the striking and exaggerated forms occur, and discussions of the subject are to be found chiefly in the literature of psychiatry. Yet of the incipient and less exaggerated types, the daily experience of normal individuals furnishes abundant examples. Indeed so common are they that we rarely think of them as connected with the paramnesia of the psychiatrists. But let anyone undertake to describe some trifling event which occurred two or three weeks ago, and he will find the incipient *prototypes* of some of the gravest diseases of memory, though in themselves quite devoid of abnormality. He will probably have a feeling of uncertainty as to the exact date of the occurrence; or if he thinks he remembers it with certainty he is quite likely to find himself mistaken. If he gives up the attempt to locate it exactly and refers it to last week or the week before, his confidence in even that degree of accuracy may prove to be misplaced. Some details will have dropped out, others will be slightly distorted, and very probably some which belong in other connections may be added. Sometimes we have a dim consciousness of these inaccuracies and perhaps even say, 'if I remember rightly' or 'if my memory does not deceive me;' at other times, we are so sure of our accuracy that objective proof is needful to convince us of our error.

Nor is this inaccuracy confined to experiences located relatively far back in time. The same tendencies appear in incipient form in laboratory and *Aussage* experiments, where the recall follows immediately upon the experience. In Dr. Kakise's experiments, the number of repetitions necessary to reproduce a Japanese character by drawing, was, in some instances, perceptibly increased by a false memory due to the distortion of the true image through an association of similarity. In one case, this was so marked that *sixteen* successive exposures of the Japanese character were necessary before the false image was finally set aside and the figure correctly reproduced. In *Aussage* experiments, it has repeatedly been shown that in describing a picture immediately after it has been seen, objects not contained in the picture are given, the position and number of objects are altered and colors are falsely named. These falsifications are considerably increased through unconscious suggestions received from questions. For instance, in the demonstration of his method given by Prof. Stern at the Conference held at Clark University in the fall of 1909, the subject of the experiment when asked if there was anything else against the wall, in addition to what had already been described said: "Yes, there was a cupboard." And when asked its color he answered 'brown;' when questioned as to whether the table had a cover on it he answered 'yes;' and when asked to describe its color, affirmed that it was white. Neither cupboard nor table cover was represented in the picture. In the *Aussage* literature, now of considerable extent, and in that of experimental psychology, may be found the germs of every type of paramnesia. Even in experiments with very simple material, the addition or distortion of visual elements, the transference of letters or syllables belonging in one series to another, wrong localization within the series and even the feeling of 'seen before' (identifying paramnesia) attached to a letter or syllable seen for the first time are all typical errors. In Abramowski's experimental investigation of the illusions of memory, special attention was given to the study of identifying paramnesia which was artificially produced under laboratory conditions. In these experiments words in a series seen with distracted attention and immediately repeated under conditions of normal attention were invariably referred to a preceding series in which they had not occurred.

This particular form of paramnesia or double memory in which a new experience is accompanied by a feeling of having been experienced before is, in its slighter forms, very common among normal individuals. Kraepelin even went so far in one of his earlier works as to classify it as belonging almost exclusively to normal individuals; but later in the seventh edition of his *Lehrbuch der Psychiatrie* evidently came to a different conclusion, for he there says that "this sometimes occurs transiently in normal life; but in disease may last for months and is particularly characteristic of epilepsy. Hallucinations of memory also occur in paresis, in paranoid dementia and in maniacal forms of manic-depressive insanity." Ribot speaks of the *déja vu* as rare and this may perhaps be true of the more extreme cases which partake of the nature of an hallucination; for I have been able to obtain, at first hand, but two analysable cases and in only one of these was the analysis, which is here given, fairly complete.

On entering a certain room in the Albrechtsburg at Meissen, which contained a painting of the abduction of the two sons of Kurfurst Friedrich the Gentle (1455) by Kunz von Kaufen, W. was vividly conscious of having been in that room before and of having seen the painting; there was, moreover, a recall of emotions aroused by the experience, which were stronger than were warranted by the present situation. As this particular castle had not been visited before and as the painting was of comparatively recent date, being contemporaneous with a restoration of the castle within recent years, any real memory of either the castle or painting was excluded. As, however, the story of the picture was familiar and other old German castles had been seen in childhood, it seemed possible that the illusory recognition might be due to elements of similarity from these sources. The true explanation, however, was stumbled upon nearly two years later and proved to be an old illustrated edition of historical tales for children, in which the story of the abduction of the princes occurred and which contained a picture of the scene as taking place in an old castle of which the outlines bore a crude resemblance to the room in the Albrechtsburg. The vividness of the false recognition was probably due in this case to the recrudescence of the emotional reactions produced in childhood by the story, as this again occurred on seeing the picture in the old book, and was a genuine associative memory. A very similar case is given by Hawthorne, in which the explanation so closely coincides with the one above given that it is quoted in full: "Stanton Harcourt near Oxford has still in a state of good preservation certain portions of the old castle, among them two venerable towers. One of these towers in its entire capacity, from height to depth, constituted the kitchen of the ancient castle, and is still used for domestic purposes, although it has not and never had, a chimney: or rather we might say, it is in itself one vast chimney, with a hearth of thirty feet square, and a flue and aperture of the same size. There are two huge fire places within and the interior walls of the tower are blackened with the smoke that for centuries used to gush forth from them, seeking an exit through some wide air holes in the conical roof, full seventy feet above. These lofty openings were capable of being so arranged with reference to the wind, that the cooks are said to have been seldom troubled by the smoke. . . . Now, the place being without a parallel in England and therefore necessarily beyond the experience of an American, it is somewhat remarkable that while we stood gazing at this kitchen, I was haunted and perplexed by an idea that somewhere or other I had seen just this strange spectacle before. The height, the blackness the dismal void before my eyes, seemed as familiar as the decorous neatness of my grandmother's kitchen; only my unaccountable memory of the scene was lighted up, with an image of lurid fires blazing all round the dim interior circuit of the tower. I had never before had so pertinacious an attack, as I could not but suppose it, of that odd state of mind wherein we fitfully and teasingly remember some previous scene or incident, of which the one now passing appears to be but the echo and reduplication. Though the explanation of the mystery did not for some time occur to me, I may as well conclude the matter here. In a letter of Pope's, addressed to the Duke of Buckingham, there is an account of Stanton Harcourt (as I now find, although the name is not mentioned) where he resided while translating a part of the Iliad. It is one of the admirable pieces of description in the language . . . . and among other rooms, most of which have since crumbled down and disappeared, he dashes off the grim aspect of this kitchen—which, moreover, he peoples with witches, engaging Satan himself as head cook, who stirs the infernal caldrons that seethe and bubble over the fires. This letter and others relative to his abode here were very familiar to my earlier reading, and remaining still fresh at the bottom of my memory, caused the weird and ghostly sensation that came over me on beholding the real spectacle that had formerly been made so vivid to my imagination."

The phase of identifying paramnesia seems to have received more attention from psychologists than other forms of false memories and there are three chief theories, with some variants, which seek to explain the feeling of a previous experience. The oldest is that of Anjel (1877) who explains the illusion as resulting from a double perception of the same object due to a larger interval than usual between sensation and perception, which are ordinarily so closely associated that they cannot be distinguished. For some reason, the mind has not organized and localized the sensations as soon as produced and consequently when this is accomplished the result appears already known and produces the illusion. The influence of fatigue furnishes one of the strongest supports for this argument. Lalande (1893) also holds the view of a double representation of the same image, but gives a somewhat different explanation of its mechanism, believing the double image due to an unusual acceleration of mental activity and the concentration of attention on the second image. The laboratory experiments of Abramowski, previously mentioned, support this latter view. Lapie (1893) and Bourdon (1894) maintain that the illusion results from the presence of certain similar or analogous elements in the situation to some previous and forgotten experience, and with this hypothesis my own cases are in accord. According to Kindberg the illusion of memory results from the feeling of active attention and appears in states of disintegration of the mental synthesis, in states of inattention, when we are conscious of the relaxation and inattention. In this case the normal feeling of effort in assimilation is absent and this gives the feeling of something already known. It is quite possible that all three theories may be correct, as they are not necessarily contradictory and the conditions of the phenomenon are so varied, that it may well be that the different hypotheses are all applicable under diverse circumstances. That fatigue is frequently, if not always, a factor in the occurrence not only in this, but in other types of paramnesia, there is considerable evidence.

Dugas reports an interesting case of false memory in a Professor X. who received a letter from a friend apprising him of a visit in a few days. On the day that his friend was expected, he asked his mother with whom he lived a question in regard to her preparation for the guest to arrive that evening, greatly to her surprise, as it was the first time she had heard of the impending visit. X. insisted that he had told her at the table on a certain day and named those present at the time, and it required the evidence of the supposed witnesses to convince him that his memory and not his mother's was at fault. In the same month (the last of the academic year) he twice demanded of his pupils written exercises that he believed that he had assigned. His memory was very distinct as to the circumstances and as before it required irrefutable evidence to convince him of his error. Dugas thinks that these paramnesias were due to fatigue and explains them by the fact that since nervous fatigue tends to produce enfeeblement of the attention and the psychic states of sensation and memory differ less in matter than in the manner in which the mind envisages them, the distinction between them became obliterated, and with the weakening of the attention a situation mentally rehearsed was mistaken for its actual occurrence. But any distraction of attention, even when no special conditions of fatigue exist, may produce a similar result and cases of this type are of everyday occurrence. The following example is typical. A student remembered leaving his notebook under his seat in the lecture room but failed to find it there next morning. Later, he found it in his locker in the dressing room and then recalled that after having left it under the seat, it had occurred to him that it would be safer in his locker and he had placed it there, but being occupied with other things had completely forgotten the circumstance and had felt very positive that he had left the notebook in the lecture room.

Localization in time is one of the most uncertain elements in memory and unless fixed by external corroborative evidence has as almost its sole criterion the vividness with which the image presents itself to consciousness. In a general way, it is true that the clearness of an image tends to decrease in proportion as the experience recedes in time; but the very fact that we unconsciously apply this rule, leads to many illusions. Sometimes events far back in the past recur with vividness and there is then a tendency to refer them to a nearer date. There is, as it were, a foreshortening of time. In a similar way, events of childhood tend to become magnified because of their vividness. It is a familiar fact that revisiting the scenes of childhood is apt to be a disappointing experience, the hills are so much lower and the houses and trees so much smaller than we remembered them. But displacement in time frequently occurs in recent events as well as in more remote experiences as is illustrated in the following example.

A little girl of about five years who attended kindergarten regularly was presented with a muff which became one of her most prized possessions. One day, a few weeks after she had come into possession of this muff, she came to her teacher at the close of the session in great distress; her muff was missing. She remembered exactly where she had put it in the morning on a shelf and not only gave all the circumstances with great detail but her statements were corroborated by another little girl who had seen her place the muff on the shelf. Search, however, and questioning of the janitor and children failed to reveal its whereabouts. Two days later a confectioner in the neighborhood sent to inquire if any of the kindergarten children had lost a muff as one of small size had been left in his shop two mornings previously. It proved to be the lost muff. In this case neither of the children had any idea of telling an untruth and, in fact, the details in regard to the muff were perfectly accurate, only they had happened on the day previous to that on which the muff was lost and probably on other days as well, so that the memory of the habitual occurrence had proved stronger than the memory of an omission of it on a certain day.

This form of paramnesia though very common among children in whom the time-sense is characteristically weak, is not at all uncommon among adults and sometimes plays an important part in the testimony of witnesses. In the trial of Lizzie Borden (a famous murder case which occurred some years ago) the evidence really turned on whether the accused wore a particular dress, which was afterwards burned, on the morning of the murder. A group of people at a summer hotel, who sat at the same table, in discussing the validity of the evidence, tried the experiment of having each one state what dresses the other members of the party had worn at breakfast. The errors were so numerous that it was unanimously decided that any evidence on such a point given several weeks after the event would be utterly unreliable; and yet the descriptions of the costumes belonging to each person were in the main correct though in a number of cases not worn on that particular morning. This inference has since been abundantly verified in the *Aussage* experiments of Stern, who concludes that "statements subsequent to the event, in regard to the external appearance of persons, especially in regard to the color of the hair, form of the beard, clothing and its color, have in general no trustworthiness unless the attention has been especially directed to these points at the time of the original perception."

The following case, which at first sight appeared to be completely hallucinatory and to rest upon no foundation in external reality, proved later to be an amnesia in which the dropping out of one link in a chain of impressions gave an apparent falsity to the whole, and is probably typical of a whole class of cases. On the day after a reception at which about a hundred people were present, B. expressed her regret at not having been able to speak to a lady whom she had noticed to be present, and whom she had not seen for some time. She was surprised by the statement that the lady in question had not been present. This she considered a mistake; and as her memory of having seen her was perfectly clear, proceeded to describe in detail exactly how the lady was dressed, in what part of the room she was standing, with whom she was conversing, the circumstances that had prevented the meeting and the succeeding disappointment at finding that she had left before this had taken place. It was finally objectively proved to her that the lady in question could not have been present as she was not in the city. For several days the subject of the apparent hallucination was quite disturbed, as the apparent memory including her feeling of pleasure at seeing an old acquaintance remained vivid, and only after considerable hard work in going over and over the details of the afternoon was the explanation found. It proved to be the loss of an impression which was not only a fleeting one but immediately followed by a distraction which involved some emotional excitement. When half way across the room to greet the supposed acquaintance, she had been stopped and called aside to take part in a rather exciting discussion. At this moment she had perceived that she had made a mistake in the identity of the person, but this impression was so transitory as to be completely obliterated by the subsequent occurrence, thus leaving an apparently false memory, which on analysis reduced to a simple amnesia of one link in the chain of original impressions.

The transference of experiences belonging to another to oneself is curiously illustrated in the following case of a young lady in the early twenties, who, in discussing early memories, affirmed that she remembered with perfect distinctness an accident which happened at her first weighing, when her age was still counted by hours. She remembered the carpet and furniture in the room and even the colors of the impromptu weighing cradle made by knotting the four corners of a small table cover and, most distinctly of all, the sensation of falling and losing breath when one of the knots slipped. As investigation was possible, it was learned that the story was correct in every detail except that the accident had happened in the case of her elder sister and consequently two years before she was born. Her good faith was undoubted and the memory remained, as far as her own introspection was concerned, quite as distinct a part of her mental life as any actual occurrence. The memory of the room and of the pattern of the table cover used in weighing were probably genuine memories as she and her sister were both born in the same house, had remained there until she was nearly four years old and the nursery had not been changed. In all probability, she had heard the story of the accident told when she was of an age to be impressed and excited by it, and very likely the catching of breath and feeling of disturbance in circulation were actual memories only displaced in time and slightly distorted in association. A similar case of distorted association has recently been related to me by a member of the University who remembers lying on a pillow and being looked at at a very early age, when, in fact, he was not the observed but the observer, being at the time about four years of age. This reference of the experience of another to the self or vice versa is a common phenomenon of delirium, and of some types of hysteria and insanity; e. g., a patient in the delirium of fever repeatedly expressed pity for another and perfectly healthy person because he had such a terrible pain in his head. In another case, a patient personified her hands, which were swollen and painful, as two little white kittens who were suffering, and complained that the doctor would do nothing to help them. Historical instances of torture or descriptions of suffering are in delirium not only transferred as personal experiences but are afterwards remembered as such, exactly as in the case of unpleasant dreams, the knowledge that the experience was a delusion and of a purely mental character making no difference in the sense of reality accompanying the memory. The delusions of paranoiacs are often of precisely this character, the psychological difference between the memory of a vivid dream or of a fever delirium in normal individuals and the systematized delusions of a paranoiac lying in the fact that in the former case the experiences are recognized as purely mental while in the latter this recognition is wanting. In some interesting autobiographical material written down by a paranoiac and published in an early volume of the *American Journal of Psychology,* the equal ascription of reality to external and purely mental experiences is very noticeable.

I cite one more example, which is of special interest because, while like others, the paramnesia consists in an amnesia at one or two points, the memory image was unusually clear in outline and even the errors are due to suggestion from submerged associations. In a course of lectures dealing with psycho-analysis, a professor of psychology gave, among the clinical cases described in Freudian literature, the following. A young girl named Recha was, during her father's absence from home, saved from a burning house by a young man wearing a white cloak. The rescuer had been seen for a few days afterwards walking under an avenue of trees near by, but had then disappeared. On the father's return he finds his daughter the victim of a delusion that she had been saved from the flames by her guardian angel, by whose image her mind is completely possessed and with whom she is really in love in an earthly fashion. Her cure is effected by convincing her that her rescuer's disappearance is due to illness, as he is found by her father in a wretched condition, and that he is no angel but a man of depraved character and quite unworthy of her affection. Those familiar with Lessing's "Nathan der Weise," will recognize that this supposed Freudian case is the heroine of that drama and that the story is reproduced with great fidelity to the original save in the finale. The occurrence of a product of literary genius more than a century old among the clinical cases of a very modern school of therapy is in itself of psychological interest and the explanation can be traced with tolerable accuracy. Thirty-five years before the professor had taught the drama as part of a German course but had not read it since. This interval, filled with an unusually active mental life and teaching, had quite obliterated the associations, but had left the outline of the story intact except for the details of the cure. As an example of hysterical delusion cured by psychic means, the case is an excellent one and as the recrudescence was vivid, it merely followed the usual psychological law in being referred to a recent date and thus logically classified among the Freudian cases recently studied, though the professor sought it in vain among his references. The changes in the outcome are particularly interesting as they can be traced to the material of the drama itself. In the drama, Recha's cure is effected by proving to her that her rescuer is not only a real person but her brother, as she is not Nathan's own child but has been adopted by him in infancy, although she is ignorant of the fact. The suggestion that his disappearance has been caused by illness and that he may be in want and suffering is, however, made by Nathan, who reproaches Deja, the nurse, for her lack of zeal in seeking Recha's rescuer, saying, "Friendless and penniless, he may be lying without the means to purchase aid." The erroneous interpretation of his character as given by the professor also contains a partial memory, because when approached by Recha's grateful nurse and companion in the days immediately following the rescue of Recha, he simulates an indifference which he does not feel, and repulses her with rudeness and insults, because being bound by his vows as a templar he really fears to see Recha again.

As the last link in the chain of clear and submerged memories which caused the story to be transformed into a clinical case is the fact that Lessing himself puts into Nathan's mouth the psychological analysis of Recha's malady as well as the suggestion that her cure can be brought about only by psychic means. He recognizes that the strife between wounded feeling due to the rude repulse of the nurse's efforts to induce her rescuer to receive Recha's thanks and her strong feeling of gratitude and attraction toward him has produced a mental illusion which may become permanent unless overcome by convincing her of his earthly existence. And this does, in fact, lead to the happy issue of the drama. All this is so entirely in accord with the Freudian theory of a psychic trauma as the cause of hysteria that the case fits quite naturally into the modern setting of psycho-analysis. Moreover, since mental imagery, as shown by experimental studies, tends to change in the direction of the customary and habitual, the substitution of the train of associations then occupying the professor's mind for the original connections was entirely in accord with the law of habit.

From the analysis of the foregoing cases it appears that paramnesia is reducible to a partial amnesia of the associative processes, in consequence of which the memory image is distorted and appears false.

The amnesia may consist in the dropping out of one or more impressions, as a result of weakened or distracted attention during the original experience, or in the loss of time and place associations. In the latter case, there may result a confusion between objective and subjective conditions, or the memory images thus detached may form a part of new series of mental processes without recognition of their reproductive character.

Paramnesia is thus not in itself an abnormal mental process, since it results from the weakening and blurring which are characteristic phenomena of memory images, but may exhibit all gradations from the slight deviations, which occur in varying degree in all normal reproductive processes, to extreme cases where the missing associative links and resulting confusion of subjective and objective experiences may completely distort the whole mental activity.

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A COMPARISON BETWEEN EXPERIMENTAL DATA AND CLINICAL RESULTS IN MANIC-DEPRESSIVE INSANITY[1]

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The purpose of this investigation was to determine to what extent the different phases of manic-depressive insanity might be demonstrated experimentally. Five different tests were used upon eleven different female subjects. The original intention was to test each subject while in the abnormal condition and then again later when she was normal. Circumstances prevented this being carried out except with two of the subjects. But three of the subjects were also tested a second time when there had been a change in their condition. We have then sixteen different sets of data from eleven different patients.

The report will be subdivided as follows:

I. Clinical Descriptions of the Eleven Patients.
II. Results from the Five Tests.
III. Relationship between the Clinical Descriptions and the Experimental Results.

I. CLINICAL DESCRIPTION OF THE MANIC-DEPRESSIVE PATIENTS

The eleven subjects were all patients in the clinical service of the Psychiatric Institute on Ward's Island, New York City. Since some of them were tested when in different pathological conditions, it is necessary to consider them under more than one head. Pr., for instance, was tested three times; and the results of the three tests will be referred to as

[1] In making this study I am greatly indebted to Dr. F. L. Wells, now at McLean Hospital, Waverly, Mass., and to Dr. August Hoch, Director of the Psychiatric Institute of the New York State Hospitals. It was under the former's direction that the tests were planned and commenced. The latter has supplied the clinical descriptions, without which little of value could have been obtained. I wish also to express here my appreciation of the help accorded me in the Hospital Ward by Dr. C. M. Campbell, now of the Bloomingdale Hospital, White Plains, N. Y.

Pr.I, Pr.II, and Pr.III. Pr.I and Pr.II are recorded under the heading of *normal* because the patient was practically normal at the time of the first test, and she was about to be discharged at the time of the second test. She had another attack of depression shortly afterwards; and after her re-admission to the hospital she was tested for the third time. This test is recorded under the heading of depression.

We have then the following groups:

Normal: Pr.I, Pr.II, Po.III.

Depression: 1. Retarded primarily: Hi.

2. Depressed, primarily: Pr.III, Ha., Bu.

Manic: Fr.I, Fr.II, Wa., Sh., Wi., Po.I, Po.II, Re.

Depression (suggesting dementia praecox): Go.

The following are the clinical descriptions of the patients:

Mrs. L. Hi. Age 43. Manic-depressive insanity.

First attack seven months after death of husband. Rather slow, sometimes particularly with initial retardation; uneasy with feeling of impending trouble. Has difficulty in mental application. Sometimes, however, though the expression does not change from that of worry, she says she feels all right.

April 14. Rather slow at first part of interviews, somewhat anxious at times.

Miss M. Pr. Age 55. Recurrent Depression (Manic-depressive insanity).

Has had seven previous attacks of depression, all since menopause. Attacks are characterized by inability to work or plan; by feeling of depression and dullness in head; by manifestations of emotional depression at times not very marked, at others more so. Occasionally she is even somewhat agitated.

Feb. 24. Practically no symptoms.

March 10. Practically no symptoms. Discharged from hospital on March 12.

May 19. Depressed. Records of both May 12 and June 7 speak of depression and inadequacy.

Mrs. T. Ha. Age 32. Recurrent depression.

When 22 had attack similar to the present one. Present attack characterized essentially by a feeling of loss of affection and a numb and dead feeling in the body. Chafes under this and is rather inclined to talk about it. But often exhibits natural behavior without essential change in the condition. Capable of a fair amount of occupation, and not at all slow in working. Condition quite stable.

Mrs. H. Bu. Age 47. Recurrent depression.

One attack when 30. Present attack has lasted with variations, and even short intermissions, for three years. At first she was dull, agitated, slow, later the attack was characterized by a feeling that she could take no interest, had not the same affection for her family, not the same strength for working, was not so ambitious. She complained of feeling as if the nerves were dead, of the head feeling as if the brain were dead, and the body as if there were "no insides." But the outside world seemed unchanged. The condition was quite stable for a considerable period during which the tests were made.

Mrs. E. Fr. Age 38. Allied to manic-depressive insanity (maniac attack).

An emotional personality. A rather prolonged quarrel with her husband ended in this attack in which it is difficult to see the transition from the emotional upset to the pathological condition. Has shown herself, while here, easily stirred up when talking about her troubles and then sometimes rather rambling and even given to making occasional sound associations. Has made frequent statements that the nurses and patients called her names and threatened her and occasional accusations that the nurses wanted to choke her. All this is said with angry and complaining tones. Toward doctors rather coquettish and a little elated, a state of mind never shown otherwise.

Feb. 10. No hallucinations, but essentially somewhat unstable emotionally.

April 28. Less quiet than on Feb. 10. More easily stirred up, getting quite angry and breaking glass.

Miss M. C. Wa. Age 68. Manic-depressive insanity (manic).

Has had attacks for thirty years, eight in all. Admitted to hospital March 23. In her manic state she shows talkativeness and flight of ideas. She often shows more anger than elation.

April 7. Condition as above.

Mrs. M. Sh. Age 43. Manic-depressive insanity (manic).

Has had five former attacks, first, when twenty-nine, second, when thirty-one, third, when thirty-six, fourth, when thirty-nine, fifth, when forty-one. All manic, some preceded by depression. Exhibits typical manic-depressive excitement of mild type with some overactivity. Elation and flight of ideas are present. At the time of the test the condition was quite moderate though there was an evident tendency to drift a little in talking.

Miss R. Wi. Age 17. Allied to manic-depressive insanity (circular type).

When fifteen, she began to say that her sick sister wanted to kill her, became excited about it and was taken to a hospital, where she varied in mood, was mischievous and depressed. Said a colored woman was her mother, that she herself was a queen. Said she saw moving pictures. Was discharged as recovered after nine months at the hospital. Since then she has had spells in which she will not talk to anyone and others in which she will sing and shout. She was admitted here Jan. 18, 1911. A short time before admission she began to say that she was a millionaire's daughter. Dressed her hair all the time. Said she wanted to be a teacher. But on the whole was well-behaved. Then quarreled several times with her mother and ran away repeatedly. Here, she has been elated, mischievous, given to singing Bowery songs. Has romanced considerably about her sister running after her before she died, etc., and accused her mother of sexual misconduct, all of which is without foundation. Tells all this voluminously but without flight of ideas. After a few weeks here she became quieter, then languid and somewhat, though not markedly, depressed. Similar variations since then. But at no time typical flight of ideas.

April 21. A little overactive and talkative, but not flighty.

Miss M. Po. Age 43. Manic-depressive insanity (manic).

A rather unstable personality and has had repeated attacks, most of them like the present, but also some simple depressions. Talks rather slowly at times, but even then apt to be rather talkative, at other times talks more quickly. Quite often shows a peculiar effort in thinking and an appreciation of this difficulty, while there is an evident flight of ideas, a drifting—very much like one who is tired and drifts while at the same time he has a clear appreciation of his difficulty in keeping to the topic.

March 3. Talkative, drifting, flighty, manic.
April 14. Drifting and difficulty in thinking.
May 19. Well.

Miss A. Re. Age 42. Manic-depressive insanity (manic).

A case about whose history very little is known, except that she was in the habit of occupying herself with palmistry, fortune telling, etc. Under observation was generally rather quiet, but when talked to elated, bright, and with typical flight of ideas and accusing remarks, yet withal some odd references to occult matters. Later the manic traits became much less marked; but she was essentially irritable and apt to commit assault when interviewed. For the most part she sat looking out of the window. Discharged to another hospital in this condition.

April 28. Manic traits were quite marked.

Miss Go.

A girl of twenty-three who had been considerably handicapped by physical infirmity (hunchback, etc.). Her schooling was often interrupted. She was rather retiring and bashful. She got interested in music, was told that she had considerable talent, and under the stimulus of this, practised a great deal. About two and a half years before admission she fell in love with a young man but was told that he was only fooling her. This, she says, was "like a shot." She began to get depressed, had queer sensations in her stomach, cramps, etc. Then she began to develop a fear of men, turned somewhat against the family and shunned people more and more.

Under observation she showed a certain depression. This, however, was not associated with a consistent reduction of activity, under which she chafed. Her conduct was rather the outcome of her sensitiveness and feeling of inferiority which she often expressed clearly and which also came out in feelings of reference and hallucinations. She did not associate with anyone. Her hallucinations were simple, people say she is not like the rest. Quite prominent were a hypochondriacal element and a certain irritability like that of a spoiled child. The former found expression in her exaggerated complaints of physical discomforts, weakness, fear that her heart will stop beating, demands that the mother be notified as she might die to-night, etc.

This is not a simple depression as in manic-depressive insanity, but a constitutional depression in which there are even certain elements of a dementia praecox reaction, though the diagnosis of dementia praecox is by no means warranted.

II. RESULTS FROM FIVE TESTS

The five tests used in this study were: The tapping-test, discrimination of weights, the number-checking-test, a distraction test, and the free association test. They were uniformly given in the following order: Tapping, number-checking, distraction, number-checking, distraction, association, weights. A different order was tried on two male subjects, not included in this study, and it was made very apparent that the tapping test should come first and the weight test last. The tapping test afforded a very good opportunity to get acquainted with the subjects and to put them at ease. The results are also not so apt to be influenced by uneasiness. The weight test, on the other hand, tends to be troublesome. The patients have less patience with it and some rather seriously object to guess when they think two weights are equal. My second subject, not included in this study, absolutely refused to co-operate and became so wrought up that I could not put him through the remaining tests.

1. The Tapping-Test

The method here was the same as used by Wells[1] and described by Whipple[2] in his Manual of Mental and Physical Tests. The subject tapped with an ordinary "sending" telegraph key for thirty seconds, first with the right hand and then with the left hand, alternately. Five records were obtained from each hand. Approximately two and a half minutes elapsed between each series. As the tapping was recorded on a kymograph it was possible to obtain the number of taps in each five-second interval of the thirty second series. This number is the unit of measurement upon which are based the results below.

The results will be considered under three different headings. The effect of continued work will be discussed, (a) as it affects the tapping-rate in successive series of the same hand and (b) as it affects the tapping-rate within a single series. (c) The gross tapping-rate will be considered. It is plain then that the factors of practice and fatigue enter into consideration under the first two headings but not under the third heading.

a. *Relations between Successive Series.*—There are two different methods of estimating the effect of continued work in the tapping test. One is to compare the successive series of the same hand together. That is the method used here. When the opposite of fatigue is shown it is referred to as

[1] F. L. Wells, Studies in Retardation, *Amer. Jour Psychol.*, XX, 1909, 38-59. Also, Normal Performance in the Tapping Test, *Ibid.*, XIX, 1908, 437 ff; and, Motor Retardation as a Manic-Depressive Symptom, *Amer. Jour. Insanity*, LXVI, 1909, 1-52.

[2] G. M. Whipple, *Manual of Mental and Physical Tests*, pp. 105-107.

"interserial warming-up." Another method is to compare the successive series when right and left hands alternate. In this case any effect which is noted is referred to as "transference."

Wells has pointed out that the first two series (30 sec. tapping-periods) of the same hand are generally the poorest in normal cases and that there is a "well-marked tendency for the later series to be faster than the earlier." To this phenomenon he has applied the name of "interserial warming-up," "a warming-up from series to series, as distinct from a warming up process confined to a single series." This phenomenon is noted by him as being common to the right hand, but as to the left, he says, "it is by no means so evident that such a process exists."

TABLE I

SHOWING AVERAGE NUMBER OF TAPS PER 5 SECOND PERIOD FOR EACH OF five 30 SECOND SERIES. (WELLS)

Series	Right Hand						Left Hand					
	1	2	3	4	5	f. i.	1	2	3	4	5	f. i.
10 Normal.........	32.0	31.7	32.3	33.2	33.2	*102*	28.8	28.7	29.0	28.7	28.8	*100*
12 Depressed.......	28.1	27.4	27.9	28.7	29.0	*101*	27.1	26.5	26.8	26.9	27.0	*99*

TABLE II

SHOWING AVERAGE NUMBER OF TAPS PER 5 SECOND PERIOD FOR EACH 30 SECOND SERIES

Series		Right Hand						Left Hand					
		1	2	3	4	5	f. i.	1	2	3	4	5	f. i.
Normal.	Pr. I....	32.2	32.8	33.2	32.8	32.2	*102*	29.7	28.2	28.0	28.3	27.2	*94*
	Pr. II...	31.8	31.2	32.0	32.5	33.0	*101*	26.8	26.5	27.2	27.7	28.0	*102*
	Po. III..	26.0	23.2	26.5	25.5	25.7	*97*	25.2	25.5	23.0	24.2	25.2	*97*
Depressed.	Hi......	34.5	31.0	31.8	29.0	27.5	*86*	28.8	28.2	28.0	28.0	25.8	*95*
	Pr. III..	31.2	28.0	30.8	30.8	29.0	*95*	26.3	25.2	24.7	26.5	24.8	*96*
	Ha.(*a*)..	....	....	....	....	....	....	....	....	....	....	....	....
	Bu.....	25.7	28.0	28.2	26.2	26.0	*105*	23.0	22.0	27.2	23.5	22.3	*103*
Manic.	Fr. I(*a*) .	....	....	....	....	....	....	....	....	....	....	....	....
	Fr. II...	30.5	28.7	28.5	27.8	28.7	*93*	26.3	25.7	26.7	26.7	27.0	*101*
	Wa.....	26.8	27.0	27.2	27.8	27.8	*102*	23.5	23.7	25.0	25.2	25.5	*106*
	Sh......	22.5	21.7	24.5	25.7	25.5	*108*	22.5	20.8	22.7	23.3	21.5	*98*
	Wi.....	26.0	26.8	28.5	29.3	29.5	*110*	24.8	24.5	25.7	24.7	24.3	*100*
	Po. I....	26.8	31.5	31.7	32.8	32.2	*120*	23.5	27.8	27.2	28.2	31.8	*122*
	Po. II...	25.3	25.8	26.8	25.2	28.0	*105*	25.5	24.2	23.8	23.7	23.5	*93*
	Re......	27.5	24.0	29.8	30.8	31.8	*106*	23.5	24.0	27 0	27.8	31.3	*117*
	Go......	29.7	30.7	28.0	28.2	27.5	*96*	29.7	26.0	25.8	24.5	23.2	*84*

(*a*)Due to a different method of procedure, the records of Ha. and Fr. I. cannot be compared with those of the other subjects and have consequently been omitted.

A summary of his data with respect to this point is given in Table I. Here the average number of taps per five-second interval for each thirty second series is given for a group of ten normal subjects and a group of twelve depressed subjects. Between every two series with the one hand there intervened a similar series with the other hand together with two rest periods of approximately 2½ minutes. In order to aid the eye in determining whether the tapping-rate increased or decreased as we proceed from the first to the last series I have computed an index as a sort of summary. This index (f. i.) corresponds to the fatigue index (f) of Wells, which is described below, in that it was obtained by averaging the second to fifth series and dividing by the first. Indices above 100 thus indicate an increase in tapping-rate, while indices below 100 correspondingly represent a decrease.

Wells' data from ten normal subjects give average indices of 102 for the right hand and 100 for the left hand and from twelve depressed subjects indices of 101 for the right hand and 99 for the left.

The data from my experiment corresponding to Wells' results are given in Table II. The results from my three normal records give average indices of 100.0 for the right hand and 97.7 for the left. The average indices of the three depressed records give similarly 95.3 and 98.0. These average results do not agree with Wells' averages, being too low, especially in the right hand. However, four of Wells' depressed subjects do not show a gain in tapping-rate in the right hand and five do not in the left. The indices of the four are respectively, 95, 92, 96, and 96. Our average results for the normal and depressed cases are then lower than his averages but not different from some of his typical cases. The seven records from manic cases give averages of 106.3 in the right hand and 105.3 in the left hand. Four of the seven have indices of 100 or over in both hands, and the remaining three have such indices in one hand.

Wells states that one of the three criteria of motor retardation as shown by the tapping-test is that of a relative gain over the normal in the efficiency of the work that comes later in the experiments. He has emphasized this point by calling attention to the "transference" effect from one hand to the other,—the second hand showing greater efficiency, whereas in normal subjects it is just the reverse. This effect seems to be comparable to the well-known clinical observation that these subjects do better in the afternoon than the morning and better on mornings after sleepless nights than after normal nights. Franz and Hamilton[3] have shown that five minutes of mechanical vibration on the spinal cord or moderate exercise will apparently increase efficiency in some motor and sensory tests. And Hoch[4] has found that with retarded subjects the second and third exhaustion curves on the ergograph are higher than the first, whereas the normal condition is a higher second but a lower third curve. However, in one more pronounced case, he found no "warming-up" at all.

Gross[5] found in his study with the writing-balance that retarded subjects showed much *less* energy in their movements than normal subjects at the start and that continued writing only caused a decrease in the size of the characters written and an increase in the length of the pauses between characters. It is evident then that investigators do not agree as to the effect of continued work upon the efficiency of that work in retarded cases.

Hoch attempts to explain the apparent phenomenon of "warming-up," which he obtained with the ergograph, by suggesting that there exist resistances especially toward the initiation of motor impulses, and that these resistances are overcome to some extent at least during continued exercise of the function. If this is the case we should expect that the later series in the tapping test would be better than the earlier. It is evident from our data that there is this interserial warming-up in the case of Bu. but not with Hi. or Pr. III. Wells' data on this point, given in Table I, show that his twelve depressed subjects have less of this warming-up effect than his normal subjects. If the twelve are considered separately we find that only three show actual increase from series to series in both hands, eight show increase in one hand but not in the other, and one shows no increase in either hand.

It is evident then that although "transference" from one hand to the other or from one performance of one hand to another in that same hand is frequently found in the performance of motor acts, yet that not all depressed subjects show it. Hoch's most pronounced case was the exception for

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[3] S. I. Franz and G. V. Hamilton, The Effects of Exercise upon the Retardation in Conditions of Depression, *Amer. Jour. Insanity,* LXII, 1905, 239-256.

[4] A. Hoch, On Certain Studies with the Ergograph, *Jour. Nervous and Mental Diseases,* XXVIII, 1901, 620-628. Also, A Review of some Psychological and Physiological Experiments done in Connection with the Study of Mental Diseases, *Psychol. Bull.,* I, 1904, 241-257.

[5] A. Gross, Untersuchungen ueber die Schrift Gesunder und Geisteskranken, *Psych. Arbeiten,* II, 450-567.

him, while here our most serious case is the only one that showed it. Only future work can make clear just what these facts mean.

It has already been pointed out that the manic cases show an increased rate in tapping in the succeeding series as compared with the first series. Their average indices are 106.3 and 105.3 for the two hands. If the records of Po. II and Fr. II are excluded, not being typical manic cases, we have average indices of 109.2 and 108.6, respectively, with only one record below 100,—the left hand of Sh. (98). (Po. II was according to clinical evidence probably much more in a state of mental retardation than of manic excitement at the time of the test. Fr. II is classified as only allied to manic-depressive insanity.)

Similar warming-up effects with manic subjects have been obtained by other investigators. Gross, employing the writing-balance, found that the rate and manner of writing surpasses the normal records as a rule. This is almost never the case, however, at the start. Here the records are generally about equal, occasionally slower. But the act of writing increases the speed of writing in quite abnormal manner. As the work continues the writing becomes larger, the pressure greater with marked variations, the pauses between the characters shorter, the speed of the motion greater, and the entire writing more careless. Lefmann[6] reports with continued work an acceleration and increase in writing. His manic subjects, as well as most of his mixed cases, all showed this. The latter, however, showed increase in the duration of the pauses. On the whole these mixed cases seemed to exhibit retarded symptoms in initiating each new movement but manic symptoms during the movement. All these records with manic cases show " that there exists not so much a motor excitement as an increased motor excitability."

Three conclusions seem to be warranted by my data.

1st. Increased performance in the following-hand (" transference " when alternate hands are considered, " interserial warming-up " when the same hand is considered in successive tests) over the preceding hand is shown in some depressed subjects, but not in all.

2nd. Interserial warming-up is characteristic of manic subjects. (No data concerning " transference " are at hand.) Or, in other words, increasing excitement accompanies all continued motor activity.

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[6] G. Lefmann, Ueber psychomotorische Störungen in Depressionszuständen, *Psych. Arbeiten,* IV, 1904, 601-668.

	TABLE III FATIGUABILITY OF SUBJECTS IN TERMS OF INDEX		TABLE IV WEIGHT-DISCRIMINATION TEST			TABLE V NUMBER-CHECKING TEST. TIME EXPRESSED IN SECONDS			TABLE VII DISTRACTION TEST. TIME EXPRESSED IN SECONDS		
Subjects	Right Hand	Left Hand	Order of Arrangement	No. of Displacements	Coefficient of Correlation	1st Trial	2nd Trial	Average	1st Trial	2nd Trial	Average
Normal: 20 College men...						145.0	134.0	139.5			
20 College women.						123.0	116.0	119.5			
Pr. I..........	90.2	82.8	A B C D E F I H G K J	6	+95	167.8	166.2	167.0	83.8	78.8	81.3
Pr. II..........	90.0	78.7	A B D E C F G I J H K	8	95	204.8	188.2	196.5	69.9	75.3	72.6
Po. III..........	95.3	98.1	A B D E C G F J H K I	12	92	181.0	168.0	174.5	74.0	73.0	73.5
Depressed: Hi..........	102.0	97.3	B A C E D F G I H K J	8	96	......	......	......	163.3	121.9	142.6
Pr. III..........	90.3	81.3	A B D C E F J H I G K	8	91	225.0	189.0	207.0	89.0	82.0	85.5
Ha..........	......	......	A C B F E D H I K G J	14	88	274.0	......	253.5	125.6	117.0	121.3
Bu..........	93.2	84.1	B C A E D F K G I J H	14	85	957.0	507.9	a552.7	126.9	130.0	128.5
Manic: Fr. I..........	......	......	A B C E D F G J K I H	10	91	b	c 179.0	194.8	......	68.0	76.0
Fr. II..........	95.3	89.4	A C B D F E G H I J K	4	98	188.0	158.0	173.0	82.0	76.5	79.3
Wa..........	89.3	86.8	A C B D F E H G I K J	8	96	......	......	......	416.2	250.9	a333.6
Sh..........	93.2	91.7	B C D A E G F I H J K	10	93	408.4	264.8	336.6	795.8	......	..?
Wi..........	96.8	88.6	C B A F G I E D H K J	20	78	192.0	191.2	191.6	142.8	95.0	118.9
Po. I..........	120.8	89.3	A B C E D F J I H G K	10	90	250.0	188.4	219.2	124.0	88.8	106.4
Po. II..........	99.2	98.7	A B C D E F H G J I K	4	98	214.2	195.0	204.6	84.7	80.2	82.5
Re..........	99.2	90.6	B A C D E G K J F H I	16	82	188.0	183.6	185.8	86.7	82.0	84.4
Go..........	89.4	84.3	A C B D G F E J K H I	14	88	b	247.4	269.2	63.2	45.0	54.1
			a9 8 13 14 12 11 21 20 18 21 19	166							
			b19 8 19 28 16 21 45 38 32 41 49								

Table IV: *a* Total number of places displaced. *b* Total number of displacements squared.

Table V: *a* On basis of 507.9 alone. See notes. *b* Watch stopped. *c* Time actually spent in marking; not including five stops to rest the eyes.

Table VII: *a* Eyesight very defective. Little reliance can be put on these figures.

3rd. There is as much evidence for "interserial warming-up" in the left hand as in the right. (See Table II.)

b. *Relations within the Single Series.*—Table III presents the fatiguability of the subjects in the tapping test. The indices (f) used here are those advocated by Wells and are computed as follows: The average of the second to the sixth five-second periods is divided by the first five-second period. A perfectly steady tapping record would thus give a record of 100. Anything below 100 indicates fatigue and anything above 100 indicates an increase in the tapping-rate during the period. Wells found for the average of ten normal subjects the following indices: right hand 93.5, left hand 88.8. In normal individuals it practically never occurs above 100. The average indices obtained from my three normal records agree very well with these, being 91.8 for the right hand and 86.5 for the left. But one of the individual records varies considerably from these averages. This record of Po. (95.3 and 98.1) taken just before her discharge from the hospital is rather high, especially for the left hand.

For depressed subjects, Wells reports 98 and 93 as the indices respectively for the right and left hands. As these indices are considerably above the normal records it means that such subjects show less fatigue (reversal) than normal subjects. This abnormal presence of reversal is one of the essential phenomena of retardation according to Wells. But only two of his twelve subjects have indices for both hands of 100 or over, while five more have such indices for one hand. This leaves five out of the twelve who had indices in both hands below 100. Of the three depressed subjects whose records we have, two are like normal subjects and one (Hi., i. e.,—102.0 and 97.3) shows a gain in the tapping-rate in the right hand and an almost steady tapping-rate in the left. This latter record clearly shows "retardation," i. e., the inability to respond promptly to a stimulus coupled with a marked tendency to increase in proficiency as the response is repeated. It is this tendency to increase in speed coupled with the low initial performance which gives us such an index as 102. Kraepelin[7] emphasizes that the three fundamental symptoms of the depressed state are (1) associative retardation or thinking disorder (Denkhemmung), (2) motor retardation (Willenshemmung), and (3) depression (Verstimmung). Now the characteristic feature of the clinical picture of Pr's. and Bu's. attacks is the depression, while that of Hi's.

[7] E. Kraepelin, *Psychiatrie*, 7th ed., Vol. II., Chap. IX.

is the retardation. We cannot be sure from a clinical analysis that the first two subjects do not have some retardation. But the depression is so prominent that it does not allow the lesser symptom to be manifested. The tapping test, then, bears out the clinical analysis entirely, in that it shows that Pr. and Bu. are not particularly retarded but that Hi. is.

With the manic subjects we find the first four to be normal in type. The high index for Wi. in the right hand is due to the first two records which were quite high (105 and 99), the other three were normal (94, 94 and 94). The next two subjects, Po. and Re., are above the normal,—Po. with the right hand in the first test and with both hands in the second test and Re. with the right hand. The indices from Po. when she was normal, as previously pointed out, are above the normal, especially with the left hand. It is consequently difficult to say whether the abnormally high indices for her sick as well as her normal condition are due to an individual idiosyncrasy or whether there was really some retardation present in her case, even when well, which was augmented when sick. The latter explanation would be supported by the findings of Gross and Lefmann. The former found in manic cases that during remissions there is a more or less considerable psycho-motor retardation with which can be associated a difficulty of elementary thinking processes. As already mentioned, the latter reports from his study of mixed cases, to which Po. belongs much more than to that of the pure manic state, that they seem to exhibit retarded symptoms in initiating each new movement but manic symptoms during the movement. Gross finds this same condition in those mixed cases which clinically stand near to the manic. The mixed case may then exhibit both retarded and manic symptoms at the same time. Po. clearly shows this; for she exhibits little or no fatigue in these tests on all three days (barring the first trial with her left hand), the first when manic, the second when "drifting" and the third just before her discharge. In this she resembles retarded subjects. At the same time she showed unmistakable increasing excitement from series to series on her first day, and a record in this respect above the normal on the other two days—a clearly manic condition.

The results from Go. are normal, or possibly below the normal.

Retardation is indicated here in 3 of the 11 subjects;—One (Hi.) who was clearly retarded according to the clinical picture, one (Po.) who was primarily in a manic condition but showed clinically at times associative retardation, and one (Re.) (only in the record of one hand) a pure manic subject. None of the other subjects are retarded as far as can be ascertained clinically or experimentally.

c. *The Tapping Rate.*—Marsh[8] and Hollingworth[9] both find an increase in the tapping rate at night over that of the morning or afternoon. In explanation of this difference in tapping rate, the former states, "it is suggested that rapidity of tapping, as it requires a minimum of control but a maximum of neural excitement, may be expressive largely of 'nervousness.'" Marsh found a decrease of control (in steadiness test) at night to support this view. But Hollingworth finds the opposite in the same test. The matter cannot be settled at present but it does seem probable that increase of tapping rate under certain conditions may be expressive of excitement, irritability, or 'nervousness.'

My results seem to bear out some such hypothesis. Thus there is a marked decrease of tapping rate in the case of Po., a manic subject, as she improved. Her three tests were taken when she was 1st, much excited, 2nd, little excited, and 3rd, normal. The average number of taps per five seconds on the three occasions were, respectively, 31.0, 26.4, and 25.4 for the right hand and 27.7, 24.1, and 24.6 for the left. Here there is a noticeable decrease in tapping rate as her manic condition subsided. With Pr., a depressed subject, we obtain the opposite tendency. Her three tests were taken when she was 1st, "no symptoms," 2nd, normal, and 3rd, quite depressed. Her average numbers of taps on these occasions are 32.9, 32.1, and 29.8 for the right hand and 28.3, 27.3, and 25.5 for the left. Wells' results agree with these, showing that the normal subject taps faster than the depressed. His figures are 194 and 175 taps in 30 seconds for the two hands with normals as against 170 and 159 for depressed subjects. This decrease in gross tapping rate is his third criterion of motor retardation. Case III in his article in the *American Journal of Insanity* also showed a "tendency for the gross rate to be faster on good days and lower on poor ones." On the other hand, his manic subjects showed a higher initial rate than normal. Moreover, the more pronounced manic states seemed also to have a higher initial rate than the less pronounced. He states more specifically as follows:

[8] H. D. Marsh, The Diurnal Course of Efficiency, *Col. Cont. to Philos. and Psychol.*, XIV, No. 3, 1906.

[9] H. L. Hollingworth, The Influence of Caffeine on Efficiency, *Col. Cont. to Philos. and Psychol.*, XX, No. 4, 1912.

"When the psychological measure can be made sufficiently independent of special factors of co-operation it is probable that the optimum performance of manic states is quite superior to the normal as well as the depressed."

Gross found that his retarded cases were slower than the normal in the writing while the manic cases were much faster, except at the start when they were not much different from normal subjects.

Results based upon two subjects of opposite tendencies would not have much weight if standing alone. But considering that these results of mine agree with previous work, I may fairly conclude that in the manic state the rate of tapping is increased over that of the normal condition for that subject, while the reverse is true for the depressed state.

2. Discrimination of Weights

Eleven weights were used. The lightest one, marked A on the back, weighed 100 grams. The remaining 10 formed a geometrical series, each being 104% of the former. This series was prepared by Wells with great care. According to this arrangement B weighed 104 grams, C 108.16, D 112.48 + etc., until K weighed 148.02 + grams.

The weights were arranged in chance order in a row between the subject and experimenter. Starting at the right hand end the first two weights were presented to the subject and she was directed to indicate which was the heavier after having lifted each weight with her right hand. In those cases when she reported that they were equal she was directed to guess as to which was the heavier. The heavier weight was then put to the right and the lighter one was compared with the third in the row. In this way the heavy weights were directed to the right end of the row and the light weights to the left end. This process was continued until all the weights had been judged to be heavier than the weights to their left. The record was recorded by jotting down in order the letters on the bottom of each weight.

A side issue to the main problem of this report but connected with the technique of this test is worth noting in passing. As has been stated our series of 11 weights form a geometrical series in which the increment is 4%. Such a series was advocated by Galton[10] and has been used by Spearman.[11] It would conform, as Galton states it, to a

[10] F. Galton, *Inquiries into Human Faculty,* Appendix C to Everyman's Library edition.

[11] C. Spearman, General Intelligence Objectively Determined and Measured, *Amer. Jour. Psychol.,* XV, 1904, 201-293.

geometric series: "thus—WR^0, WR^1, WR^2, WR^3, etc."—where W equals 100 grams and R equals 26/25. "It follows that if a person can just distinguish between any pair of weights he can also just distinguish between any other pair of weights," whose intervals in the series differ by the same amount. In other words, this series of ours should give us equal "sensation-steps." But the results show very clearly that we do not get any such arrangement. The last two lines in Table IV present a summary of the displacements of each weight from its correct position in the arrangements of the 16 subjects. There is undoubtedly an increase in the tendency to displace the weights as one proceeds from the lightest weight to the heaviest. Weights A and B were displaced on an average 8½ places by the 16 subjects, Weights C, D, E, and F, 12½ places, and Weights G, H, I, J, and K, 20 places. If greater emphasis is placed upon the amount of displacement, as for example, by squaring the number of places displaced, we have the figures in the last row of the table. The irregularities in the results are probably due to the small number of cases. But it does seem certain that there is at least twice the tendency to confuse Weights K and J as there is to confuse B and A. Weber's Law does not then even approximately hold here. To secure a series with fairly constant "sensation-differences" means the construction of a series whose increment shall also increase.

The results from the Weight Test are given in this Table. After the name of the subject is given the actual order of arrangement of the weights by that subject. Following this order is given (1) the total number of displacements in the given order from the correct order and (2) the coefficient of correlation between the given order and the correct order. The first figure has often been used as a measure in such procedures. But it seems to the writer that a displacement of one of the weights with its neighbor, resulting in a score of 2 (as both are then displaced), is very much less serious than when a weight is two places removed, resulting in a score of 4. If the displacements were squared and then totaled the results would more nearly present the true situation. When a coefficient of correlation[12] is obtained these displacements are squared during the computation, hence there is no need to present both the squared displacements

[12] Using Pearson's Rank-Differences Formula, $1-\frac{6\Sigma(d^2)}{n(n^2-1)}$

and the correlation coefficient. The latter is given because of its better known character.

As the subjects are grouped there does not appear to be any significance between the type of attack and the results in this test. The three normal results correlate + 92 or higher. Five subjects correlate below + 90, i. e., Ha. and Bu. (depressed), Wi. and Re. (manic), and Go. It is true that the symptoms of the first two are somewhat different from Pr. or Hi., our other depressed subjects. Hi. is primarily retarded. Pr. is depressed with especial dullness of the head, while Ha. and Bu. are depressed with feelings of loss of affection and numbness and deadness of the body. The nerves are thought of as "dead" and the body as "if there were no insides to it." This additional loss of feeling in the body may account for the poorer showing in this test of these two subjects. The poor showing of Wi. and Re. may possibly be due to a "flightiness" of attention,—a common characteristic of manic subjects. But nothing of the sort was noted at the time of the experiment which differentiated them from the others. No cause for Go's. poor showing is suggested.

Wells[13] carried on a similar experiment with six weights, weighing 51, 53, 55, 57, 59, and 61 grams, respectively. He had 10 subjects, among whom 4 were normal and 4 depressed. No difference was found between these two groups when the averages were compared. But two of the four depressed gave results much below those of the others.

Such work as has been done on sensory discrimination with manic-depressive or other allied types of insanity all goes to indicate that there is no serious defect present. A few exceptions have been noted by Janet, Alter, and Hoch.[14]

Franz and Hamilton, more recently, report for one retarded case a 10 to 15 per cent. lowering of the touch threshold on those days on which the subject was subjected for five minutes to mechanical vibration along the spine. This report suggests that there is a relation between lack of feeling or discrimination and sensory capabilities.

It seems then that some insane subjects do poorly in this test. But the relationship between the form of attack and the experimental results is not at all clear.

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[13] F. L. Wells, On the Variability of Individual Judgments, *Essays Philos. and Psychol. in Honor of Wm. James,* 1908.

[14] A. Hoch, A Review of Some Recent Papers upon the Loss of the Feeling of Reality and Kindred Symptoms, *Psychol. Bull.,* II, 1905, 233-341.

3. The Number-Checking Test

This is the Cancellation test recommended by Woodworth and Wells.[15] It consists of a blank with 20 lines of numerals. Each line has each one of the 10 numerals (0 to 9) repeated five times. The subject was instructed that there were five O's in each row and that she was to go through all the rows and mark each O. She was to do this as fast as possible and yet she was not to miss any of them. The emphasis was placed equally on speed and accuracy in the instructions.

The results from the number-checking test are given in Table V. At the top of the Table are given the results from 40 college students, 20 of each sex.[16] Under these results are given the results from the three normal records, and the other subjects follow in the usual order. The first column of data gives the results from the first test, the second column gives the results from the second test made a few minutes later. The third column gives the averages of these two results.

In those cases where only one reliable result was obtained, instead of two, the result in the average column was computed on the basis of that reliable result. The average practice effect of the group, where two valid results were obtained was as 100 to 85. The missing result was computed on the basis of this ratio and then averaged with that result to give the average. Such a procedure is rather risky, but for the purposes of this experiment it is better to be able to compare all the subjects together on some common basis than to be forced to omit several from consideration.

The results are stated in terms of the time taken up in cancelling the 100 O's in the blank. Omissions were computed as follows. The time was found for cancelling a single O. Then twice this time was added to the recorded time for each omission except when a whole line was omitted. Then only five times (there being 5 O's in a line) this amount was added instead of twice the five. The reason for this procedure was that when a line was skipped it was due to accident and hence it would probably have taken only as long to cancel the O's in it as it did to cancel the other lines. But when an O was missed in a line it meant that the subject had lost count in the line or that not finding the O she had gone on rather than gone back to find it. Such an O would un-

[15] R. S. Woodworth and F. L. Wells, Association Tests, *Psychol. Monog.*, No. 57, 1912.
[16] *Ibid.*

doubtedly have taken longer to cancel than the average O cancelled. Twice the average time seems a reasonable allowance in this case. Wrong cancellations were not scored, there being only one such case, except in the case of Bu. Her case is considered separately below.

The three depressed subjects are slower in performing this test than are the two normal subjects. Four of the manic cases are no slower than the poorest normal record (of Pr. II) but they average somewhat slower than the average of the normal records, i. e., 186.3 as against 179.3. This is not much of a difference and should probably not be considered. The other two manic cases, Sh. and Po. I and II are slower than the poorest record of the two normal subjects, the former by 140.1 seconds and the latter by 22.7 and again by 8.1 seconds.

TABLE VI

SHOWING RELATIONSHIP BETWEEN RESULTS IN NUMBER-CHECKING TEST AND CHANGES IN THE ATTACK

Subject	Conditions	1st Trial	2nd Trial	Average
Pr. I	"no symptoms"	167.8	166.2	167.0
II	normal	204.8	188.2	196.5
III	depressed	225.0	189.0	207.0
Po. I	manic	250.0	188.4	219.2
II	manic	214.2	195.0	204.6
III	normal	181.0	168.0	174.5

The records of Pr. and Po. are separated out from Table V and are presented in Table VI. Here we have their records when they were normal compared with their records when sick. Pr. was normal during the first two tests and then depressed at the time of the third test. Instead of a practice effect as a result of the six tests we have the opposite effect shown on the three different days; but there is a practice effect between the two trials on the same day. One should expect an improvement in the second day's test over the first, as she was normal at both times. If it was not for this peculiarity, one would be warranted in attributing the lack of improvement on the third day to the depression. As it is, no conclusion can be reached. Po., on the other hand, was manic during the first trial, in a mixed condition during the second and normal during the third. Here there is a practice effect shown throughout the series of tests. But there is much greater improvement shown in the third day's work over the second than in the second over the first. The normal thing would be to find a greater improvement between the second and first than between the third and second. From her case it would seem that the manic attack interfered with and slowed up her performance in this test.

Subject Bu. reacted to this experiment in a rather peculiar manner. She consumed 8 minutes and 48 seconds in the first trial. Exactly 2 minutes were spent in checking the first two lines and here I helped her chiefly by encouraging her to go on. She seemed rather lost, not knowing what to do, and yet I am as sure as one can be that she really understood what was wanted. Despite the amount of time consumed in these two lines she marked but 4 O's in each line. She then commenced to go faster but to make many mistakes. Her record by groups of 4 lines is as follows:

1st	four	lines	7	omissions and	4	wrong	symbols	checked.
2nd	"	"	7	"	6	"	"	"
3rd	"	"	11	"	3	"	"	"
4th	"	"	10	"	1	"	"	"
5th	"	"	0	"	0	"	"	"

We have here a quite decided practice effect in accuracy, and although the time was not taken for the different portions of the test sheet my impression is that there was a quickening of the speed of work. The second test was performed in 2 minutes and 54 seconds less time, with no wrong symbols checked and with but 20 omissions instead of 30. There again there was a decided decrease in the number of mistakes, especially in the last 4 lines where but one was made. Both tests picture initial retardation while the first test adds also a picture of confusion and difficulty of performance.

Franz,[17] basing his conclusions upon a similar experiment, reports that both the depressed and excited subjects were slow as compared with the normal at the commencement of a period of practice.

Basing our conclusions upon all the results it seems very probable that both depressive and manic attacks interfere with performance in the cancellation test. The interference does not affect the accuracy of the work ordinarily but does increase the time of performance.

The cancellation test, while it brought out some results, was

[17] S. I. Franz, The Time of Some Mental Processes in the Retardation and Excitement of Insanity. *Amer. Jour. Psychol.*, XVII., 1906, 36-68.

far less satisfactory than it might have been. The figures on the test-blank are altogether too small and the lines are too close together. If the lines were "leaded" much of the tendency to skip lines would be eliminated. Several of the subjects had to be excused from the test because of inability to distinguish the numerals apart. Still others occupied a large part of the time in processes of seeing. But with a blank of larger print, I believe we should have here a very valuable test for such subjects. By timing the cancelling of each fifth of the blank, considerable insight as to mental retardation might be obtained.

4. The Distraction Test

Fifty postal cards were selected, composed of ten each of such types of pictures as:—scenery, public buildings, portraits, kissing pictures, etc. At the lower left hand corner of each card was pasted a small numeral. Five numerals were used, from 1 to 5. Two cards from each of the five groups were given the same numeral. The subject was seated before a table and before her were placed five slips of paper with the five numerals upon them. Numeral 1 was to her left and the others followed in serial order to the right. She was instructed to take the pack of 50 cards and to sort them acccording to the small numbers in the lower left hand corners. All the cards with a " 1 " upon them were to be placed on the pile at the extreme left, all the " 2 "s on the next pile, etc. Every precaution was taken to insure that each subject understood exactly what was wanted and also that we were timing her. The experimenter watched the sorting and whenever he noticed a card being placed on the wrong pile, he called attention to it and had the error rectified. The errors recorded in the results are errors that were not thus caught and rectified during the timing. It is fair to state, then, that the emphasis was upon accuracy rather than speed in this test, although all knew that they were being timed.

The purpose of the test was to determine whether the subject could concentrate her attention upon the work in hand even when a new picture was being presented on each card. It was believed that observation of the conduct of the subject would determine very largely the cause of noticeable slowness of performance, if any developed. That is, whether the slowness was due to motor trouble, or to distraction due to the pictures on the cards, or to other causes.

The results from the test are given in Table VII. The first and second trials and their average are presented in the three columns. The practice effect of the two trials gives a ratio of 100 to 81. On that basis the time was computed for the first trial of Fr. (the time having been lost as a result of the failure of the stop-watch) and the two results were then combined for the average. The results for Wa. are very unreliable as she could scarcely see the figures on the cards. A second trial was not attempted with Sh. as she was unwilling to co-operate properly. A proportionate amount of time was added for each mistake.

For all but two subjects, as far as could be judged by closely watching the subjects, the test resolved itself into one of motor ability plus co-ordination of eye and hand. But with Sh. and Wi. there was very pronounced distraction. The latter noticed the pictures as she sorted the cards and commented about them. During the second trial she apparently ignored all but the kissing pictures. These she still continued to observe. In reference to these she said during the first trial, "They're funny; you must give me one when you get through." And after the second trial she held up three and asked for at least one of them. A moment later she asked to be excused for a few minutes, having stuffed one into her dress. When I granted her request but insisted that she give up the card, she gave it up and settled back in her chair. Sh. placed the kissing pictures at the back of the pack instead of upon their several piles. In this way she retained them after having sorted all the others. She then continued to look at them and place them at the back of the pack. Only after considerable urging on my part was she finally led to sort them. Only one trial was made in her case because of lack of time—the trial having consumed 13 minutes and 15 seconds. Our clinical picture does not suggest any reason why Sh. should have responded to the kissing pictures any more than the other subjects. But with Wi. it is different. Her conduct, when normal as well as when sick, indicates that this sort of thing is uppermost in her mind. As will be pointed out under the association-test many of her association responses were strikingly of this nature. It is very probable then that she was distracted not because of any mental defect but rather because the particular distraction was peculiarly of interest to her. Sh., on the other hand, was not only interested in the pictures but was carried away by them, losing all interest in the experiment. Only by continued exhortation could she be led to finish the test. In this she showed an abnormal condition, entirely lacking in Wi.

As already emphasized, this test is not actually one of distraction, but rather one of motor ability and co-ordination of eye and hand. By "distraction" I have meant here the tendency to look at the pictures on the card rather than the numerals. Of course, there is "distraction" present here, if distraction is thought of as the opposite of attention. It is not surprising then that the results from this test are similar to those of the cancellation test. Both of them are tests of perception and motor response. In the cancellation test perception is more prominent than the response; in this distraction test the two factors are reversed in importance, there being five responses instead of one.

TABLE VIII

SHOWING RELATIONSHIP BETWEEN RESULTS IN DISTRACTION TEST AND CHANGES IN THE ATTACK

Subject	Conditions	1st Trial	2nd Trial	Average
Pr. I	"no symptoms"	83.8	78.8	81.3
II	normal	69.9	75.3	72.6
III	depressed	89.0	82.0	85.5
Po. I	manic	124.0	88.8	106.4
II	manic	84.7	80.2	82.5
III	normal	74.0	73.0	73.5

Here again the depressed subjects are slower than the normal subjects. The third trial of Pr., when depressed, is not so much slower than her normal records or that of Po. but the fact that she is slower is important as practice should have made her faster. Table VIII presents her three records together and shows this decrease in efficiency when depressed more clearly than in Table VII. Comparison of the depressed subjects, except Pr. who had had two previous trials, with the manic cases shows that they are all considerably slower than the manic subjects. (The record of Wa. cannot be considered as it was due to her poor eyesight.) The difference is enough to warrant a conclusion that depressed subjects are slower than either normal or excited cases. Of the four depressed subjects, Hi. is much the slowest. The clinical picture and the tapping test both have shown that she was "retarded." Her average time was from 14 to 21 seconds slower than that of Bu. and Ha. Both of them, moreover, were troubled with handling the cards. Besides this difficulty, Bu. was troubled in locating the correct pile. After she had the next card in her right hand, she would look at the piles for some time before choosing the correct one. Then she would place the card upon it in a very deliberate manner. Motor control then was faulty in all the three. In addition, Bu. had mental retardation.

Three ot the five manic cases that can be considered are as fast as our normal subjects. Wi. is slower because of the distraction already referred to. Po. is very slow in her first trial of the first day (124 sec.). Her second trial on that day (88.8 sec.) is not very much longer than the other normal or manic subjects. Table VIII gives her records together. There we see very consistent improvement in the 6 trials on the three different days. Whether there is greater improvement due to her accompanying improvement in mental condition than would take place if she had been normal on all three days cannot be determined. The relationship between normal and manic condition in this cannot be settled from the data.

Go. gives the fastest results of all the subjects including the normal ones (63.2 and 45.0). A number of tests on college men gave 60-65 secs. for the first trial and 50-45 secs. for the second. Her record must be considered as good as can ordinarily be obtained. All of the tests show that she had good motor control, although she was a cripple and of inferior constitution. However, she tired faster than ordinarily in the tapping test.

Conclusion:—Depression results in a noticeable decrease in speed in this test. The records of depressed subjects are slower than either normal or manic cases. No conclusion is warranted from the data concerning the difference between manic and normal conditions.

5. *The Association Test*

This is the familiar association test in which the subject is instructed to give the first word that occurs in response to a given stimulus-word. The list of 100 words used was that recommended by Kent and Rosanoff.[18] Their method of procedure and treatment of data were followed throughout, except that in addition the time of each response was taken. They present the results obtained in this test from 1,000 normal individuals and 247 insane patients. They record the number of times any response was given to each of the 100 stimuli-words by these 1,000 normal subjects. On the basis of these

[18] G. H. Kent and A. J. Rosanoff, A Study of Association in Insanity, *Amer. Jour. Insanity,* LXVII, Nos. 1 and 2, 1910.

frequency tables they divide all responses into three groups:—common, doubtful, and individual. The *common* consist of such responses as were given by 1 or more of their 1,000 subjects. The *doubtful* consist of "any reaction word which is not found in the tables in its identical form, but which is a grammatical variant of a word found there," as "govern" and "governed." The *individual* reactions consist of such responses as were not given by any of their 1,000 subjects. The "individual" responses may be "normal" or "pathological;"—*normal* when the response is perfectly normal in every respect, but did not happen to be given by any of the 1,000 subjects; *pathological* when the response is not thus

TABLE IX

DISTRIBUTION OF ASSOCIATION-REACTIONS OF NORMAL AND INSANE SUBJECTS

Type of Response	From Kent-Rosanoff Tables			Our data
	1000 normal subjects	247 insane subjects	32 manic depressives	16 manic depressives
Common	91.7	70.7	75.8	78.6
Doubtful	1.5	2.5	3.0	1.0
Individual	6.8	26.8	21.5	19.7
Failures to respond	0	0	0	.7

TABLE X

DISTRIBUTION OF "INDIVIDUAL" REACTIONS OF MANIC-DEPRESSIVE SUBJECTS

	Kent-Rosanoff	Our data
Normal	5.3	5.5
Sound reaction	1.5	.2
Word complement	.2	.7
Association to preceding stimulus	.5	.7
Association to preceding reaction	.4	1.8
Repetition of preceding stimulus	.0	.1
Repetition of previous stimulus	.2	.2
Repetition of preceding reaction	.8	.1
Repetition of previous reaction	.9	.4
Unclassified	8.6	10.0
Other groups	3.1	.0
Total	21.5	19.7

normal. Their totals are presented in Table IX. Here we have their total results from (1) 1,000 normal subjects, (2) 247 insane patients, and (3) 32 manic-depressive subjects. Alongside the latter are presented our results. It is evident that the two investigations agree very closely. A comparison of their results and ours when based on the medians instead of averages gives equally close agreement. Table X presents the sub-groups under "individual" reactions. Kent and Rosanoff's results are found in the first column and ours in the second. Here again there is very close agreement. Indeed, throughout all the details that we have considered, there has been a corresponding agreement. Their summary[19] applies here equally well:—"In this disorder the departures from the normal seem to be less pronounced than in the other psychoses considered." The number of "individual" reactions is in most cases not greatly above the normal average; and, so far as their character is considered, we find that many of them are classed as "normal," in accordance with the appendix to the frequency tables; among the "unclassified" reactions, which are quite frequent here, we find mostly either obviously normal ones, or some of the type to which we have already referred as "far-fetched," while others among them are "circumstantial."

Table XI presents the detailed results for each subject. Among the 1,000 normal subjects studied by Kent and Rosanoff, there are 53 which they report as having an average of 21.8 (median 21) "individual" reactions. All of the 53 had 15 or more "individual" reactions. It is evident then that 53 normal subjects in 1,000 have 15 or more "individual" reactions and that about 25 in 1,000 have more than 20 such reactions. Our three normal records average 6.3 "individual" reactions, our depressed subjects (excepting Bu.) average 9.7, and our manic subjects average 16.1. Subjects Bu. and Go. display such striking differences from the other subjects that they must be considered by themselves. As, however, Bu. is a depressed subject, it is only fair to give also the average of the depressed subjects including her. This average is 27.3. The subjects who have 15 or more "individual" reactions are Fr. (both records, 22 and 23), Wa. (20) and Re. (31, all manic subjects, Bu. (80) a depressed, and Go. (59). Sh., Wi., and Po., all manic subjects, have but from 5 to 10 "individual" reactions. We cannot postulate any difference here between normal, depressed, and manic

[19] *Ibid.*, p. 53.

TABLE XI

TYPE OF ASSOCIATION-REACTIONS OF EACH SUBJECT

	Common	Doubtful	Individual Reactions: Normal	Sound Reactions	Word Complement	Association to Preceding Stimulus	Association to Preceding Reaction	Repetition of Preceding Stimulus	Repetition of Previous Stimulus	Repetition of Preceding Reaction	Repetition of Previous Reaction	Unclassified	Total Individual	Failures
Normal:														
Pr. I	91	...	6	...	...	...	...	...	...	...	...	2	8	1
Pr. II	90	2	1	...	1	...	...	...	...	...	...	6	8	...
Po. III	97	...	3	...	...	...	...	...	...	...	...	...	3	...
Depressed:														
Hi.	91	1	7	...	...	...	...	...	...	...	...	1	8	...
Pr. III	91	...	3	...	...	...	1	...	...	...	1	4	9	...
Ha	85	...	3	...	...	...	2	...	...	...	...	7	12	3
Bu	19	...	6	...	...	6	18	...	2	...	2	46	80	1
Manic:														
Fr. I	78	...	3	...	1	4	...	...	...	...	1	13	22	...
Fr. II	72	...	6	...	7	...	...	1	...	1	1	7	23	5
Wa	77	3	11	...	...	1	...	...	...	...	...	8	20	...
Sh	89	1	5	1	...	...	...	...	...	...	...	4	10	...
Wi	90	2	5	...	1	...	...	...	...	...	1	1	8	...
Po. I	87	3	6	...	...	...	...	...	...	...	...	4	10	...
Po. II	95	...	1	...	...	...	...	...	...	...	...	4	5	...
Re	67	2	10	...	2	...	2	...	1	...	1	16	31	...
Go	38	2	13	2	...	...	6	...	...	...	...	38	59	1
Average	78.6	1.0	5.6	.2	.7	.7	1.8	.1	.2	.1	.4	10.0	19.7	.7

TABLE XII

ASSOCIATION REACTION-TIME OF EACH SUBJECT, IN SECONDS, WITH P.E. OF DISTRIBUTION

	Common		Normal		Word Complement		Association to Preceding Stimulus		Association to Preceding Reaction		Unclassified	
	Med.	P.E.	Med.	P.E.	Med.	P.E.	Med.	P.E.	Med.	P.E.	Med.	P.E.
Normal:												
Pr. I	9.8	3.1	25.0	9.5	...	...	...	...	...	...	...	...
Pr. II	8.0	2.1	...	...	...	...	...	...	...	...	12.0	12.0
Po. III	7.9	2.2	...	...	...	...	...	...	...	...	...	...
Depressed:												
Hi.	18.0	9.3	19.3	5.3	...	...	...	...	...	...	...	...
Pr. III	8.7	2.4	...	...	...	...	...	...	...	...	...	...
Ha	15.3	8.3	...	...	...	...	...	...	...	...	29.0	5.8
Bu	12.3	5.0	21.0	13.5	...	...	6.0	27.0	6.3	3.0	10.0	10.6
Manic:												
Fr. I	18.3	8.1	...	...	...	...	...	...	...	...	34.8	13.0
Fr. II	15.3	5.9	21.0	11.5	12.3	6.2	...	...	...	...	33.0	16.3
Wa	11.3	2.9	20.7	7.2	...	...	...	...	...	...	14.5	4.8
Sh	27.0	17.4	29.0	26.5	...	...	...	...	...	...	...	...
Wi	12.3	3.2	18.0	3.3	...	...	...	...	...	...	...	...
Po. I	8.7	2.3	15.0	6.1	...	...	...	...	...	...	...	...
Po. II	8.1	2.8	...	...	...	...	...	...	...	...	...	...
Re	9.9	2.2	13.5	3.9	...	...	...	...	...	...	22.0	6.5
Go	18.5	14.7	23.0	5.8	...	...	...	...	29.0	14.4	29.0	15.5

subjects except that the normal do not show as great a number of "individual" reactions as do *some* of the depressed or manic subjects.

Table XII presents the median time of the reactions of the different subjects. The probable error of the distribution is also given in order to enable one to grasp the amount of variability in the times of any group. Only the sub-groups of Table XI are given here in which there were at least 6 cases. A central tendency based on 6 cases is not very reliable except where the probable error is low. Grouping the subjects together, we have an average time for the normal subjects of 8.6 fifths of a second, 13.6 for the depressed, and 13.9 for the manic. These averages seem to indicate that both depressed and manic subjects are slower than normal subjects. But as four of these twelve abnormal records are as fast as Pr. I, it seems best merely to state that taken as a whole normal subjects are faster than either depressed or manic subjects, but that some individual depressed or manic subjects may give records as fast as normal subjects.

Let us consider the individual cases. The first two records on Pr. were when she was practically normal, the third record when she was depressed. No difference appears in the total number of reactions in any group. The time for the "common" reactions is a trifle slower in her third test than the second, but it is faster than her first test. No deduction can be made from such data, except that the depressed condition has not appreciably lengthened her reaction-time. With Po., the first two tests were made when she was manic, while the third was taken when she was normal. There is some improvement both in the increase in the number of "common" reactions given and in the decrease in the time of the reactions. But most of the improvement takes place between the first and second test when she was still manic. The improvement cannot be ascribed to her improvement in condition with any certainty. It may be only due to practice.

Fr. shows some difference in her reactions at the time of her two tests. The second record is the poorer. There is a decrease of 6 in the number of "common" reactions, an increase of 3 in the "normal" reactions and a substitution of 6 "sound" reactions for 6 "unclassified" ones. From the clinical picture we learn that she was much more easily excited at the time of the second test, getting quite angry and breaking glass. Whether or not there is any relationship between the poorer record and her increased excitement cannot be answered here.

The records of Bu. and Go. in Table XI are quite similar. Only by a study of the reaction times and a careful scrutiny of the separate reactions can one decide that they are totally different in kind. Of the 19 "common" reactions of the former 9 were reactions which only one subject of the 1,000 had given as shown in Kent and Rosanoff's frequency tables. Two of these nine would be classified as "associations to preceding reactions" and two more as "unclassified," if they were not grouped under the heading of "common." Bu. then gave very few "common" or "normal" reactions. She gave 18 "associations to preceding reaction" besides the two above, and 6 "associations to preceding stimulus." These 18 and 6 had median times respectively of 6.3 and 6.0 fifths of a second. The "common" took twice as long (12.3) and the "normal" three and a half times as long (21.0), while the "unclassified" took only ⅔ more time (10.0). It seems fair to conclude that Bu. carried on a train of thought connecting the reaction-words and responded from that train of thought to the stimulus words. Such a conception would account for the large number of "associations preceding reactions" and their very quick time. The "unclassified" words also probably belong to this class, but here the connection between the given word and what had gone before was not obvious to the experimenter. The "common" and "normal" reactions may be accidents, or what is more likely to my mind is that the train of thought was broken at these points and she gave true responses to the stimulus-words. In any case the most favored response for her was the "association to preceding reaction" while the "normal" was the least favored, with the "common" intermediate between the two.

Go., on the other hand, had 38 "common" and 59 "individual" reactions. Of these 59, 38 were "unclassified." The "common" were the fastest (18.5), the "normal" next (23.0) and the "associations to preceding reaction" and "unclassified" were the slowest (29.0). There was no train of thought here but simply an extreme example of the tendency to give "unclassified" reactions which are largely incoherent. We should conclude that Go. is one of those cases whose test-records, according to Kent and Rosanoff, "strongly resemble, in some respects, those of dementia praecox." Bu. might also be included under this heading if the type of response is alone considered, but if the reaction-time is also considered we must place her elsewhere.

Considered as groups, normal subjects give fewer "individual" reactions and respond much more quickly than do depressed or manic subjects, but some individual depressed or manic subjects give records indistinguishable from normal. As groups, manic subjects give more "individual" reactions than depressed subjects; the reaction-time is equally slow for both groups. But here again, there are conflicting individual records.

III. RELATION BETWEEN CLINICAL AND EXPERIMENTAL RESULTS

1. Depressed Subjects

a. Pr. No retardation is shown in the tapping test, but there is a decrease in her tapping-rate at the time of her third trial (when depressed) over her first two trials (when normal). She is also much slower in the distraction and cancellation tests at this third trial than at the time of the other two. The rate of tapping, of cancelling, and of sorting cards is consequently decreased by her depression. All three of her association tests were normal in character.

b. Hi. Retardation shown in tapping-test in each series, but noticeable fatigue shown between series. No record in cancellation test due to poor eyesight. The slowest of all in the distraction test. Perfectly normal in association test. The tapping and distraction tests clearly show motor trouble, thus agreeing with the clinical description of being "rather slow, sometimes particularly with initial retardation."

c. Ha. No record in tapping. Extremely poor in weight test. Very slow in cancellation and distraction tests. In the latter about the same as Bu. but faster than Hi. Association-test—normal. The feeling of numbness and deadness in the body may be the cause of the poor showing in the weight test. She complained of it at the time of the test. Her slowness in the distraction test was apparently due to lack of motor control.

d. Bu. No retardation shown in individual series but an interserial warming-up suggested it. Extremely poor in weight test, possibly due to the feeling of deadness in head and body. Very slow in distraction test, next to Hi. in this respect. Cancellation slowest of all, but showing marked improvement in each test in speed and accuracy. Only subject tested who was noticeably poor in accuracy of cancellation. Association test—noticeable train of thought, only 19 "common" reactions. The tests all suggest lack of motor and mental control. But also a slow improvement in each test.

This improvement came too slowly, evidently, to show up in one tapping record, but did so when all series were compared.

The Group.—The clinical pictures of Ha. and of Bu. are very similar. The tests would indicate that the latter is much more deeply affected. Both would seem to come principally under the heading of depression with "associative retardation" or "thinking disorder." Hi. would come principally under the heading of depression with "associative retardation" and Pr. under that of depression without formal alterations.

The tapping-test may indicate retardation by the opposite of fatigue within the series, and seems to indicate depression by lack of interserial warming-up, although this is present in Bu. The rate of tapping is also below that of normal subjects. The weight test is unsatisfactory, but extremely poor records are sometimes accompanied by loss of feeling in the body. Slowness in the cancellation and distraction tests is characteristic of depressives. The association-test shows no abnormalities except with Bu. Her record is characterized by responses from a train of thought connecting the reaction words.

2. Manic Subjects

e. Po. Rate of tapping decreased with the three tests, corresponding to her clinical condition of manic in the first, less so in the second, and normal in the third. Showed tendency to tap as fast at end of the series as at start in all three tests,—characteristic of retarded subjects (Wells). Interserial warming-up very prominent in first test, in second test in right hand, and high record in third test. The test shows, then, initial retardation but increasing excitement as the work continues. It is difficult to reconcile this tapping record with her clinical condition, unless the "difficulty in thinking" is an indication of some retardation—making her case one of the mixed states. Undoubted improvement in cancellation test accompanying improvement in attack. Improvement in distraction test also, but it is not so clear that this is due to the phases of the attack and not to mere practice. Association test—normal, all three records; a slight improvement in the tests in type of response and reaction-time but not more than what would be expected from practice.

f. Fr. No record in tapping, first time; second test normal. Time in cancellation and distraction tests equal to normal subjects. Twenty-two "individual" reactions in first of the two association-tests, 23 in second, also 5 "don't knows." The "don't knows" referred to words which she claimed she did not know although responding to them the first time. Time of the "common" reactions 18.3 fifths of a second first test and 15.3 the second; the "unclassified," 34.8 and 33.0, respectively. Only the association-test suggests she is not normal. Both of its records are abnormal, the second the poorer of the two. She was actually much more easily stirred up at the time of the second test. Whether or not there is any relationship between the two can not be answered.

g. Wa. Tapping-test, normal. Cancellation-test omitted due to poor eyesight. The latter factor accounts for the poor record in distraction-test. Twenty "individual" reactions in association-test, 11 "normal" and 8 "unclassified." Here again, only the association-test would indicate that she was abnormal.

h. Sh. Tapping-test, normal. Slowest of all, except Bu., in cancellation-test. Slowest of all in distraction-test. Here she did not co-operate but insisted on keeping the "kissing pictures" in her hands instead of sorting them. Association-test, 10 "individual" reactions, but 6 are "normal;" however, the reaction-time is extremely slow,—27.0″ for the "common" reactions. Twenty-six of her 89 "common" were species-genus. Nothing in her clinical picture suggests the cause of these results except possibly the tendency to drift in her talk. At the time of the test she was "quite moderate," typically manic of a mild type.

i. Wi. Tapping-test, normal. Weight-test, poorest of all, no explanation as to the cause. Cancellation, a trifle slower than normal, distraction, much slower than normal, due to marked interest in the "kissing" pictures. Association-test, type of reaction, normal; but time rather slow (12.3″). "Male," "female," "human," and "useful" given whenever possible, with markedly long time for the first two. Tests do not do more than show up her marked interest in sex questions.

j. Re. Tapping test, manic excitability in that the tapping rate increases with the successive series. But she shows less fatigue within the series than normal subjects (especially with her right hand)—a characteristic of retardation. Weight test, poor. Number-checking test, normal. Distraction test, a trifle slow. Association test, 31 "individual" reactions, 10 of which are "normal," 26 "unclassified" and the remainder scattered among several sub-groups. The "unclassified" were incoherent, e. g., whiskey-Nova Scotia, religion-doughnuts, eating-understanding, red-lamb, etc. Very fond of plural, e. g., deeps, ideas, seas, etc. Reaction time of "common" was 9.9, "individual" from 13.5 to 22.0. She was tested when the manic traits were quite marked. The tests indicate a manic condition except for the "reversal" within the series which is distinctly a retarded symptom.

The Group. The tapping test shows clearly an increasing rate of tapping as the work continued from series to series. But within the series there was no characteristic difference from normal subjects. It seems also quite certain that the gross rate of tapping is faster for these subjects when in the manic condition than it would be if they were normal. The weight test gave poor records with Wi. and Re.; no explanation is offered. Probably, manic cases are slower in cancellation and distraction tests than they would be if normal, some are noticeably slower. In the association test all but one had an abnormal number of "individual" reactions and long reaction times.

3. Depression (suggesting Dementia Praecox)

k. Go. Tapping test, showed a trifle more fatigue in the series than in normal subjects; no interserial warming-up. Number-checking tests, very slow. Distraction test, 20 seconds faster than any of the others, including the normal subjects, as fast as college men. This is the only noticeable case in which the number checking and distraction tests have not been reacted to in the same general way. Association test, 59 "individual" reactions, 13 of these being "normal," 38 "unclassified" and 6 "association to preceding reactions." The reaction time of the "common" reactions was slow (18.5) while to the "individual" reactions it was from 23.0 to 28.0. This subject is very troublesome to classify. Her tapping shows no interserial warming-up and noticeable fatigue within the series. In this she resembles dementia praecox subjects more than any other type. She is similar to the depressed subjects by being very slow in the number-checking test, but shows the opposite to that state in the distraction-test by being extremely fast. Her association record also suggests dementia praecox much more than manic-depressive insanity.

THE USE OF THE TERM *FUNCTION* IN ENGLISH TEXTBOOKS OF PSYCHOLOGY[1]

By Christian A. Ruckmich, Cornell University.

Problem.—A number of English textbooks which attempt to present the subject-matter, point of view, and problems of psychology to the college student, frequently use the word 'function' in connection with the description of phenomena of the mental life. It is conceivable that some systematic classification of the meaning of this term as so used could be made from a detailed study and review of these textbooks, and it is likely that a classification, made in this manner, may be of service in comprehending the angle from which mind, in each one of these textbooks, is sighted. Accordingly, with a view toward reaching a sort of logical schema of 'function,' and with an attempt to clear up, in a measure, the conceptions of mind held by these various authorities, an analytical investigation of fifteen textbooks, best representative of the class mentioned, was begun.

Textbooks reviewed.—The textbooks chosen for this investigation were the following:

(1) Angell, J. R. *Psychology,* New York, 4th ed., 1908, ix + 468.

(2) Baldwin, J. M. *Elements of Psychology,* New York, 1893, xiv + 372.

(3) Calkins, M. W. *A First Book in Psychology,* New York, 3rd ed., 1912, xix + 426.

(4) Dunlap, K. *A System of Psychology,* New York, 1912, xiv + 368.

(5) James, W. *Psychology,* New York, 1907, xiii + 478.

(6) Judd, C. H. *Psychology,* New York, 1907, xii + 389.

(7) Ladd, G. T., and Woodworth, R. S. *Elements of Physiological Psychology,* New York, 1911, xix + 704.

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[1] Revised from a paper read before the Graduate Seminary in Psychology, Cornell University, March 25, 1912.

(8) McDougall, W. *Psychology,* New York and London, 1912, 256.

(9) Myers, C. S. *A Textbook of Experimental Psychology,* 2 vol., Cambridge and New York, 2nd ed., 1911, xiv + 344, 107.

(10) Pillsbury, W. B. *The Essentials of Psychology,* New York, 1911, ix + 362.

(11) Read, M. S. *An Introductory Psychology,* New York, 1911, viii + 305.

(12) Stout, G. F. *The Groundwork of Psychology,* New York, 1903, vii + 248.

(13) Thorndike, E. L. *The Elements of Psychology,* New York, 1905, xiii + 351.

(14) Titchener, E. B. *A Textbook of Psychology,* New York, 1909-10, xvi + vii + 558.

(15) Yerkes, R. M. *Introduction to Psychology,* New York, 1911, xii + 427.

Classification of Usage.—After a detailed study of the usage of the term 'function' in the English textbooks of psychology, a systematisation based on coincidences of meaning seemed logically possible. Difficulties arose in the interpretation of usage from the point of view of the context and, sometimes, of the system. As a rule, very little margin was allowed for interpretation in the light of the system because of the danger of erroneous construction. It is hardly fair to the student or to the critic for an author to postulate general systematic points of view at the beginning of the presentation in a textbook and to commit breaches of promise from that point on. The author was, accordingly, held responsible for local renderings, in spite of the fact that he may have warned the reader not to pay heed to lapses or misuses of language.

Aside from the borrowed use of the word 'function' in the technical sense of mathematics,[2] which, for the purposes of this paper, is disregarded in our tabular arrangement, we find that, in the main, 'function' is used in two large ways, the first of which is subdivided as follows:

[2] This meaning is defined by the Century dictionary: "A mathematical quantity whose value depends upon the values of other quantities called the arguments of independent variables of the function; a mathematical quantity whose changes of value depend on those of other quantities called its variables."

Meaning		*End or Purpose*	*Relation*
(I) Service	(A) other	process or processes function or functions	organised
	(B) the total organism		(1) defensive (2) offensive
(II) Activity	activity		active

This logical classification will become clearer as we proceed with the examples that contributed to its foundation. We find, in general, that, with the exception noted above,—in the instance of the technical mathematical meaning,—every occurrence of the word 'function' in the literature means either 'service' or 'activity;' or the approximate grammatical equivalent of either one of these. That is to say, for every appearance of the term 'function,' the one or the other of these words, or the corresponding grammatical form of the one or the other of these words, can be substituted in the text without a marked distortion of the original meaning, or a misconstruction of the sentence. The determination of the 'end' or 'purpose' of the 'function,' and the type of its 'relation,' is based on explicit or, more rarely, implicit connotations interpreted from the context. The 'relation' is the aspect in which the 'function' appears.

A 'function' of the type I-A is by nature of service to other 'functions' or processes and stands in an organised relation to them; it may further, complete, or achieve one or more 'functions' or processes within the organism, but its immediate end is not that organism. Its purpose is to make possible the successful operation of other 'functions' or processes dependent on it. Examples of this type follow:

The function of the central nervous system is to control and combine the various processes which go on in different parts of the organism.[3]

The effort by which he succeeds in keeping the right *name* unwaveringly present to his mind proves to be his saving moral act. Everywhere, then, the function of the effort is the same: to keep affirming and adopting a thought which, if left to itself, would slip away.[4]

There are always resistances inhibiting the carrying out of the 'determination,' and it is the function of the specific act of willing persistently to re-inforce the determination.[5]

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[3] McDougall, W. *op. cit.*, 26.
[4] James, W. *op. cit.*, 454.
[5] Myers, C. S. *op. cit.* 332.

The formation of the elements of the process of knowledge and the inauguration of the control over movements in accordance with the mandates of experience—these are the two great functions of perceptions.[6]

Because association is of wider interest than its function in recall, we shall give it a chapter by itself. Association is the organization of experience, by virtue of which the various kinds and parts of content constitute a whole; it is the functional interconnection of the objects of experience as we find them; not a force or activity. . . . The function of the concept in perception is sometimes called apperception.[7]

In general, it may be said that attention increases the vividness of presentative states and thus renders more definite and lasting the apperceptive activities of synthesis, analysis, relation, as seen in memory, association, judgment, and reasoning . . . . It may at least be safely said that the arranging and co-ordinating power of voluntary attention greatly facilitates our earliest intuition of things. It is here that the relating or apperceiving function of active attention is most apparent.[8]

In a 'function' of the type I-B, we find an expressed reference to the interests of the total organism in and for which the 'function' exists. This kind of 'function' does something for the individual organism as distinguished from other organisms or from the environment at large. The 'function' may, under these circumstances, further the interests of the individual in protection from other organisms, or it may do this for the individual in opposition to other organisms or objects of experience. The distinction attempted is that of defense *versus* offense. In other words, the organism may strive, with the aid of various 'functions,' either to defend itself against its environment, or to express, assert, or advance its interests in its environment. The defensive attitude or relation is by far the more common, while the assertive or expressive relation occurs systematically in the textbooks of only a few authors. The former relation is styled I-B-1, and is best illustrated by the following selections:

Such differences certainly appear to be fundamental; but we shall see reason to modify this view, when we consider that both forms of attention are vital functions which are brought out and developed in the general adaptive reaction of the organism to its social and physical surroundings. If we remember that those objects which are harmful to us commonly stimulate the nerves very violently, we shall begin to see how in the general economy of the organism it may be useful to have our senses so constructed that they shall call our attention to such possible sources of danger as are represented by

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[6] Angell, J. R. *op. cit.* 171.
[7] Dunlap, K. *op. cit.* 179, 198.
[8] Baldwin, J. M. *op. cit.*, 71, 124.

these intense stimuli, even when we do not consciously desire to have our quiet thus invaded.[9]

In summary it may be said that instincts are movements, or feelings that may or may not be the result of movements, that come because of inherited connections and dispositions in the nervous system. In function they serve, on the one hand, to keep the infant alive until he may be able to learn for himself, on the other they serve to enforce general lines of conduct that are essential for the preservation of the individual, the race, and the social group.[10]

Reference has already been made to the main function or use of sensations in the conscious life of the organism, namely to furnish the material to be developed into perceptions and ideas for the guidance of the organism in making superior adjustments to the conditions of its life, conditions physical, social, and spiritual.[11]

Regarded from the biological point of view, the function of all mental process and mental structure is to preserve and promote the life of the race and that of the individual in so far as he subserves the life of the race . . . . All mental activity, then, normally issues in bodily movement; since only by promoting and guiding bodily movement can it fulfill its function.[12]

'Function' of type I-B-2 is very rare. It has the same biological significance as has I-B-1, but it carries with it more of the idea of self-aggression, the expression of an individuality, the assertion of the *ego* over against its environment. Examples of this type are:

The organism is from the first, as we have seen, an active affair, instinctly and impulsively adjusting itself to its environment and using that environment for its purposes . . . . [Under 'Function of imagination']. In answer to the question, then, why we imagine, we may say (1) because images enable us to carry on our lives in much more advantageous ways than would be the case without the aid of imagery; and (2) because we simply can not help doing so, there being within us deep-set impulsive tendencies which find in great measure their adequate and satisfactory expression in images.[13]

The chief function of reasoning is to make discoveries, to carry us beyond the limit set to observation, memory, and the simpler forms of thought. Reasoning is thus an important form of self-development, or learning, a means of acquiring new outlooks, new points of view, new bases for action.[14]

As a final type of 'function,' we have an activity whose purpose is its own acting and whose sole aspect is an active relation to the organised system: the assigned task of the 'function' is an end in itself. We should say, for instance, under this heading, that the 'function' of blood-circulation

[9] Angell, J. R. *op. cit.*, 91.
[10] Pillsbury, W. B. *op. cit.*, 256.
[11] Read, M. S. *op. cit.*, 106, 107.
[12] McDougall, W. *op. cit.*, 105.
[13] Read, M. S. *op. cit.*, 86, 216.
[14] Calkins, M. W. *op. cit.*, 163; *v.* also 274-77.

is not to act as carrier of nutriment to the tissues, but to circulate. Stout puts it tersely in the statement:

> When we say that digestion is a function of the stomach, we mean that digestion is the stomach engaged in digesting. When we say that breathing is a function of the lungs, we mean that breathing is the lungs at work.[15]

Further illustrations of this type follow:

> The organism is a whole. It possesses a certain *form,* changing from moment to moment; it exhibits manifold *functions* or activities, it also possesses *experiences.*[16]
>
> To speak, then, of consciousness and to attempt to describe consciousness as something that exists and can be analysed into constituent parts which severally exist, abstracting from or neglecting the mental activity or function, is to distort the facts very seriously and to use a method which cannot be wholly successful.[17]
>
> If the free endings of the epidermis are not organs of pain, the physiological evidence for the connection of pain with unpleasantness falls to the ground. If they are, it is still possible that their exposed position and consequent liability to injury allow them to function as sense-organs, while they are replaced in the interior of the body by more highly specialised structures; or it is possible that they have become adapted, in some unknown way, to the reception of sensory stimuli.[18]
>
> It is true that a large part of habit is ideational; the bodily functions are influenced by the processes in representative consciousness.[19]

These are some of the more frequent and apt occurrences of the use of this form of the term 'function.' It now remains to trace out the appearance of the term in every one of the several textbooks reviewed for this purpose.

ANGELL (1). In this textbook, we find frequent usage of this term, and in almost all of its meanings. Out of over one hundred appearances, there are about fifty whose meaning places them under rubric I-A of our classification, *i. e.,* 'function' used in this sense has as its end the furtherance of some other process or 'function' in the organism. It is only fair to state, however, that in a large number of these instances the word is applied to physiological rather than to psychological processes; but the term is by no means restricted to physiological usage as the following citations will indicate:

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[15] Stout, G. F. *Manual of Psychology,* New York, 1907, 49.
[16] Yerkes, R. M. *op. cit.,* 341.
[17] McDougall, W. *op. cit.,* 59.
[18] Titchener, E. B. *op. cit.,* 261.
[19] Dunlap, K. *op. cit.,* 334.

The neurones of the central (nervous) system may be grouped according to certain of their functions in three great divisions: (1) sensory neurones which bring nervous impulses in from the sense organs, (2) motor neurones which terminate in muscles and carry to them impulses from the nervous centers, and (3) central neurones which in various ways join together the members of the first two groups. As we remarked earlier in the chapter, the nervous system seems to manifest its essential value as a device whereby appropriate movements are made in response to sensory stimulations. [p. 27.]

The function of the peripheral neurones is evidently that of transmitting impulses from the sense-organs into the nervous centers, and we need discuss them no further at this point. [p. 32.]

Each of these cases illustrates the function of the sensory-motor circuit. The light rays falling upon the retina set up currents in sensory nerves, which are transmitted to cells in control of the muscles of the eyes; and these cells in turn send out impulses, which result in convergence and accommodation. [p. 100.]

From the physiological side it is evident that the primary organic function of the sensory processes must be that of instigating movements. [p. 148.]

The formation of the elements of the process of knowledge and the inauguration of the control over movements in accordance with the mandates of experience—these are the two great functions of perception. [p. 170.]

Of the subject side of consciousness it seems impossible to predicate anything save its existence. Its *function,* to be sure, must apparently remain fixed. It must always be the *knower annealing* the various elements of our experience into some sort of unity. [p. 443.]

Taken in their entirety, what do these two great bodies of fact point to, regarding the *function* of emotion, *i. e.,* (1) the temporary suspension of voluntary control in the forward movement of consciousness, and (2) the overflow of motor impulses into channels leading partly to the involuntary muscles and partly through hereditary influences to the voluntary system? . . . . *The significance of emotion as a fact of consciousness would seem, therefore, to be resident in this monitory function, represented by its compelling announcement of needed adjustments, its report of unstable equilibrium.* [p. 378.]

As we saw long since, all such expansive states of consciousness are, other things equal, intrinsically agreeable, and they afford a definite appeal to the accommodatory function of attention. [p. 425.]

There are a few instances, less than a quarter of the total number of occurrences of the term, which imply pure activity, of type II in our schema, without apparent reference to any end outside of the activity. It is extremely difficult to distinguish, at times, between the meaning just illustrated, and the meaning of bare activity; but it is possible to give enough examples of the latter meaning to justify the distinction.

It may be said that however true our account of the organic activities involved in emotional psychoses, it is, nevertheless, a false description of the facts to say that we are *conscious* in any *explicit* way of these functions of our bodily selves. [p. 372.]

Furthermore, the grasping of the object, involving as it does a definite motor coördination of an efficient kind, is *per se* agreeable, *i. e.,* it is a normal activity of functions (in this sense instinctive) adequate to the demands laid upon them. [p. 407.]

Psychologists are by no means agreed as to the precise nature of the mental activity by means by which we apprehend relations. Certain writers make the whole achievement a function of attention, and disclaim the necessity for any further explanation. [p. 248.]

The remaining occurrences of the word 'function' are classifiable under I-B-1, *i. e.,* they refer to the organism as an end and bear the interpretation of service to that organism. Clearly the interests of the organism seem to be indirectly subserved in many other cases; some of the quotations given have connotations of that kind. In fact, if a reviewer is entitled to a general impression of the textbook, he does not hesitate to say that there appears to be an undercurrent of meaning of the 'survival' type in almost all of the uses found. All that this means is that 'function' in almost every connection subserves directly or indirectly the ends of the organism as a whole, *i. e.,* helps it to survive in a hostile, or at least, in a difficult, environment. Unambiguous statements to this effect follow:

It will assist us in gaining a working idea of the nervous system to bear in mind the fact that its fundamental function consists in the conversion of incoming nerve impulses into outgoing nerve impulses causing movements tending to preserve the creature. [p. 16.]

We announced our purpose at the outset to adopt a biological point of view [*sic*] in our psychological study, and to attempt at every step to see just how the mind aids in the adjustment of the human being to the environment. If we turn from the merely general statement that the fundamental function of consciousness is to better such adaptive activities, and observe any specific instances of the process of adaptation itself, we shall always find that the actual work of accommodation is going on at the point which we call the point of attention. [p. 80.]

The function of the unpleasant in consciousness is, then, evidently to furnish an *immediate and unambiguous index of conditions which menace the welfare of the organism. . . .* The obvious function of agreeableness is consequently found in the furnishing *of immediate exponents of organic welfare.* [p. 327.]

They (instincts) represent, by common consent, those forms of reaction upon the environment which the race has found most effective in maintaining itself against the rigours of climate and geographical habitat, and against the assaults of various forms of animal life. [p. 345.]

Apparently, then, by the specific admission made in one of the above quotations, mind, for Angell, is almost entirely biological in regard to the organism. All three meanings of the word 'function,' however, are noted.

Baldwin (2). In this text we find a more frequent usage of the word 'function' than we did in Angell; but its use is confined almost entirely to rubrics I-A and II, and is distributed almost evenly between them. Only three occurrences of I-B-1 were found; a single illustration of this use will, therefore, suffice:

> The analogy, therefore, may be put something like this: the nervous system in its development has taken on the two functions called stimulation and reaction. When consciousness arises it is at least—whatever else it be—an aid through pleasure and pain to the life process, and to the further development of the system. Analogy would lead us to look, therefore, for this new factor in connection with each of the two essential nervous functions, stimulation and reaction. [p. 309.]

It follows that most of the appearances of the word do not come in contexts which would indicate a use of the term with reference to the survival of the organism. Baldwin, for the most part, has not written the textbook considered from a biological point of view. The trend of the book, taken as a whole, is toward a physiological description of mind, not as a matter of nerve-tissue conveying nerve-impulses, but as doing something mental and non-physiological but in a physiological manner. The most frequent usages come under this aspect. Sometimes the activity has an end outside of its own acting; in this case the word comes, of course, under I-A as the following:

> For it should be remembered that we must find a mechanical basis for muscular control even though we advocate a directive and selective function of will. [p. 43.]
>
> It may at least be safely said that the arranging and coördinating power of voluntary attention greatly facilitates our earliest intuition of things. It is here that the relating or apperceiving function of active attention is most apparent. [p. 124.]

In one instance 'function' as used in the sense of I-A passes in meaning to I-B-1:

> Whatever directly causes me *pleasure or pain* excites interest. Here the reference to self is so immediate that the knowing function which the attention brings with it is simply a self-preserving function. I am interested in pain to discover the cause and remove it, and in pleasure to understand and continue it. This is what pleasure and pain are for, to warn and advise; and to say they interest us is to say that they carry this function into the life of thought. [p. 246.]

Lastly there are types of 'function' which are classifiable under our rubric II:

It has already been seen that all mental activities reside, that all apperceptive processes happen, in the attention; hence the great class of emotions of activity cluster round the different phases of the attentive life. These feelings again fall into two classes, which we may call feelings of degree of adjustment and feelings of *function,* or activity proper. [p. 267.]

We are now prepared to gain a view of the entire process of imagination looked at, not as the union of these separate activities or factors, but as what appears at first sight to be, a single function of the mind. [p. 187.]

Under this head, also, as including any function, and not simply muscular activity, the pleasures arising from the gratification of the organic appetites and instincts appear to fall, they are functions of periodical exercise, and their normal working involves periodical stimulation. [p. 233.]

The general impression got from this book is the conception of mind as an active organ. If the point that mind is considered as contrasted with, and not as a part of, the physiological apparatus of the organism, is well established, then there is no danger of misinterpreting the statement that, in this book, mind is viewed from a physiological angle: it acts as a non-physiological organ in the organism, but it acts in a physiological way, *i. e.*, it works within the confines of an organised whole.

Calkins (3). In this book the occurrence of the term is comparatively rare. Out of the 25 appearances, about one-half have physiological references, and some of the remaining relate to non-psychical phenomena. But the book is important for our discussion in its attitude toward the functioning of a 'self' aggressive to its environment in the light of self-development. We have already quoted one passage in illustration of the rubric I-B-2.[20] The conception noted, however, does not emerge so much in the use of the term itself, as it does in connection with the context appearing with the term. In the following example it is difficult to decide whether the meaning of the term 'function' is classifiable under 'service' with other processes as an end, or with the organism in its aggressive attitude as an end, *viz.* I-A, or I-B-2. The context probably settles the matter in favor of the latter (I-B-2):

The functions of the imagination are by this time evident. By reproductive imagination, or memory, I hold to my past; and in creative imagination I reach out also beyond the limits of past and present. As a merely perceiving self I am bound to this desk, this loom, this plot of ground; but as a remembering self I live through, once more, the exhilarating adventures and the beautiful scenes of

[20] *V.* page 103.

my past experience, and as a creatively imagining self I am hampered neither by 'now' nor by 'then.' [p. 125.]

In a discussion of *psychology as science of mental function,* an attempt is made to vindicate the use of the term 'function' in the aspect and meaning of I-B-2. In it appears the following:

If the term 'function' be taken with the meaning 'reaction to environment' and if the environment be then described, in Professor Angell's words, as 'social' and not merely 'physical,' it must follow that a 'function' is a social relation,—in other words, a personal attitude. If, on the other hand, the term 'function' be taken in a strictly biological sense, then the account of different sorts of consciousness as different reactions to environment, ... these accounts will explain and classify mental phenomena, but will in no sense describe them psychologically. . . . The classification of a psychological experience as biologically useful is both correct and significant, but so far from fulfilling the requirements of psychological analysis, it is not psychological description at all. Such description is, indeed, impossible without the study of a self, in personal relation, emphasised or unemphasised, receptive or assertive, egoistic or altruistic, to an environment which is personal as well as biological. [p. 275.]

Illustrations of the use of the term as described under I-A follow:

Some psychologists hold that the function of redistribution belongs peculiarly to the cell bodies. [p. 288.]

It has already appeared that the function of the lenses and muscles of the eye is the formation of clear images on the retina. [p. 300.]

The meaning given under II also appears occasionally:

It is a moot question whether sense-consciousness accompanies the functioning of these lower and interior centres. The probability, however, is that in the case of the lower vertebrates, with less developed hemispheres, the excitation of lower and of interior brain is accompanied by consciousness, and that, on the contrary, excitation of the hemispheres is necessary to human consciousness. [p. 291.]

On its neural side, dissociation implies what may be described somewhat figuratively, as a blocking of ordinary 'association paths' and a consequent damming up of cortical energy. This results on the one hand in the more intense functioning of the sense centres still excited, and on the other hand in the spread of the cortical energy through less frequently used 'brain paths.' [p. 384.]

Emphasis is laid, then, on the meaning of the term 'function' as given under I-B-2, *i. e.,* it is connected with the conception of the self functioning in a self-developing manner and in a social environment of other selves. The term occurs, however, with other meanings, especially in physiological contexts.

Dunlap (4). With the exception of the use of the word 'function' in its mathematical sense, all the appearances can be classified under either I-A or II: either the 'function' serves some other process or 'function,' or it simply acts. These meanings are distributed about equally among the thirty appearances of the word. Instances where the 'function' furthers another 'function' or process are the following (I-A):

This approximate repetition of a former content is the *reproductive* function of imagination, and we speak of it simply as *reproductive imagination.* [p. 161.]

The function of the concept in perception is sometimes called *apperception.* [p. 198.]

It is scarcely probable that muscular sensation by itself can give space-content. The only betweenness of such sensation is the temporal betweenness, and its function in the production of space-content can be only secondary. But it does help to develop our space-content in a very important way, ... [p. 218.]

But there are as many occurrences of the term 'function' with the meaning of activity for its own sake, examples of which are:

Finally, psychology is not the study of the functions of the nervous system. In fact, all the essential points of psychology can be expounded, as they have been developed, without reference to the nervous system, of by reference to a conception thereof which is ridiculously inaccurate. Nevertheless, it is true that psychological principles and facts are more easily described and investigated when referred to the structure and probable activity of the brain and nerves, as understood by the person to whom described or by whom investigated, and we believe that the more closely physiological conceptions approach agreement with the actual facts of structure and function, the more facile the progress of psychology. [p. 4.]

The most conspicuous peculiarity of relation-content is that it has no definitely assignable nervous process corresponding to it. We know of no "center" in the brain for the perception of relations, and we do not know that it is a cortical function at all. We must not suppose that perceived relations depend on, or are functions of "brain-paths," or "association fibres;" brain-paths represent simply connections established between different factors of content, by the operation of which the factors function together; the physiological connection is not the same thing as the experienced connection or relation, and the physiological connection may function perfectly whether a specific relation is experienced or not. [p. 147.]

The actions of which the human body is capable may be divided usefully into two classes: physiological reflexes and conscious reflexes. The first class, which includes the actions in which consciousness plays no essential part, is but indirectly of interest to the psychologist, although of extreme importance in vital function. [p. 265.]

A 'function' may mean, then, a serviceable activity with some other 'function' or process as an end, as often as it may mean simply an activity of some sort without any definite serviceable direction. Attention must, however, be called to the more frequent use of the word with the first meaning in purely psychological connections and to the more common occurrence of the term in the second sense in purely physiological contexts.

JAMES (5). The meaning of 'function' in this book is limited to the two mentioned in connection with the review of the last book, and, out of the 25 occurrences, the majority are classifiable under the meaning of 'service' with other processes of 'functions' as an end. We find good illustrations of the meaning corresponding to our heading I-A in the following:

> Sensation, thus considered, differs from perception only in the extreme simplicity of its object or content. Its object, being a simple quality, is sensibly *homogeneous;* and its function is that of mere *acquaintance* with this homogeneous seeming fact. Perception's function, on the other hand, is that of knowing something *about* the fact. [p. 14.]
>
> The chief function of our eyes and ears is to enable us to prepare ourselves for contact with approaching bodies, or to ward such contact off. They have accordingly been characterized as organs of anticipatory touch. [p. 61.]
>
> If we then consider the *cognitive function* of different states of mind, we may feel assured that the difference between those that are mere 'acquaintance' and those that are 'knowledges-*about*' is reducible almost entirely to the absence or presence of psychic fringes or overtones. [p. 167.]

'Function' as bare activity with no purpose other than its own acting occurs much more rarely. One example may, therefore, be sufficient:

> If paths are shot-through at all, they are shot-through in consistent systems, and occasion thoughts of definite objects, not mere hodge-podges of elements. Even where the brain's functions are half thrown out of gear, as in aphasia or dropping asleep, this law of figured consciousness holds good. [p. 316.]

The most frequent use of the term 'function' occurs with the meaning, therefore, of 'service' especially when placed in a psychological setting. Occasionally, also, the meaning of 'activity for its own sake' appears, but usually only in connection with physiological processes.

JUDD (6). The full tale of meanings of the word 'function' is found in this book, and the word occurs almost as frequently as it does in Angell or Baldwin, *i. e.,* about 80 times. Out of these appearances, there are a few scattered references to the meaning as given in I-B-1, *i. e.,* of 'service' to the organism in the defensive relation toward its environment; two references to Calkins's 'social function,' our I-B-2; and the remaining references about equally divided, with a slight majority for the meaning of II, *i. e.,* 'activity for its own sake,' over the conception of 'function' as 'service' with other processes or 'functions' as an end,—our I-A. Illustrating these usages in order of notation, we have the following quotations as examples of the last mentioned (I-A):

The function of a sensation can be defined only by considering the use to which the sensation is put. . . . The function which a given sensation serves is not determined merely by the quality or intensity of the sensation; it is determined in a large measure by the relation into which the sensation enters. [p. 131.]

After the memory image is thus aroused, it is used as a percept would be used to guide action, and so becomes an important additional means of controlling behavior. The control of action is the chief function of the memory image. [p. 238.]

The important fact in an idea is thus the relation into which its contents enter. Thus idea is the function of memory factors in much the same sense that perception is the function of sensations. [p. 247.]

Turning now to the quotations which illustrate the meaning of 'function' as 'service' with the organism in its defensive relation toward its environment as an end (I-B-1), we have:

Every highly developed function of an animal is recognized in biology as having its relation to the struggle for existence. . . . With such facts before us can we escape the question, What part does consciousness play in the economy of life? From the lower forms of animal life up to the highest, we find a steady increase in the scope of intelligence. In the highest animals we find mental evolution carried so far that intelligence is very often of more significance than any other single function or even group of functions. [p. 3.]

No better illustration than this could be found of the fact that the development of all experience is in the direction of the perfection of functions. Content is here used for a time to aid in building up a habit and then the content is dropped and the function is retained. The value of memory in such cases as this is merely to enlarge the basis of reaction until the most useful type of reaction can be securely established. [p. 240.]

Two examples of 'function' used in the sense of assertive development in a social environment (I-B-2) appear:

The slightest revival of the memory image is enough to arouse the appropriate activity; that is, the function of the image is gradually selected as the important contribution to individual development, and the content factors drop out more and more. [p. 239.]

The worthy sponsors of the child unquestionably indulged, even in the early days of the ceremony, in certain exchanges of information with regard to other members of the community, and this social function which the individual served was very readily connected with the word coined to refer primarily to the religious function. [p. 265.]

The illustrations of the last type of 'function' used, *i. e.*, that of 'activity' with its own acting as an end (II), are:

Between the function of movement as exhibited in this simplest form and the same function as exhibited in the highest animals, there is a long course of development, but this development consists solely in differentiation of movements, in refinement of adjustment and organization, not in the production of a wholly new fact of life. [p. 16.]

For example, there is in this parietal region one area which is of extreme importance in the function of speech. . . . . He may be capable of articulation, which is a motor function, but he will lack the ability to interpret the impressions when he sees or hears words or to give expression to a coherent series of ideas. [pp. 54-5.]

Why should this group of animals turn to the development of all the instruments of civilization? The problem stated in this form becomes a problem of functional development, rather than a problem of physical development. The animal must have been driven at some time into a situation where his development turned upon his ability to adopt a new type of behavior and a new mode of life. [pp. 366-7.]

To sum up: while we find in Judd a small numerical majority in favor of the meaning of 'activity,' it must be noted that 'function' thus used appears, for the most part, in physiological contexts (in direct opposition to the tendencies manifested in Angell and Baldwin). The meaning of 'service,' on the other hand, applies more directly to psychological usage. In spite of the confessedly biological nature of some of the quotations given, we find that the psychological treatment of the data of human experience as given in the book, especially in connection with the usage of 'function' in the sense of 'service' with other processes as an end (I-A), leans heavily on the biological point of view, an aspect which colors the entire presentation, and stands, as it were, behind the scenes ready to prompt the actor in an emergency.

Ladd and Woodworth (7). In this work 'function' is consistently limited to the meaning of 'activity' (II). Consciousness is apparently conceived from a physiological point of view. The word occurs very frequently, beginning with the chapters on the nervous system and passing on through the chapters in which psychical phenomena are correlated with 'brain activities.' This conception holds throughout and is instanced by the following example:

Consciousness may reasonably be taken as indicating brain activity; in other words, when there is consciousness, then the brain is active. And degrees of consciousness may probably be taken as indicative of degrees of brain activity. The field of attention may therefore be taken as an index of the field of greatest brain activity. [p. 609.]

We are several times greeted with the word, 'faculties,' a term which has not yet dropped out of the psychological vocabulary of several well-known psychologists:[21]

Besides the foregoing groups, or classes, certain observations which have more or less of scientific confirmation and value, may be made regarding the physical basis of the feelings and volitions controlling the bodily members, and of the higher faculties of memory, association of ideas, etc. [p. 7.]

Several illustrations of 'function' used as 'activity' follow:

He [Flechsig] believes that the map of the cortex so obtained is also to be regarded as a map of the distribution of functions; and in particular he supposes that the regions whose fibres receive their myelin sheath early are the centres of the lower functions of sensation and movement, while the late-myelinating regions are the seat of the highest intellectual functions. [p. 224.]

In a word: *It may be possible, by training, to increase the speed and improve the quality of those general cerebral forms of functioning, to which attention and discrimination correspond from the introspective point of view.* . . . . We pass now to a study of that complex form of functioning which is called "memory," in a more special meaning of the word. [p. 572.]

There is so very little variation in the meaning of the term 'function,' although occasionally it gets biological significance from the context, that, once having noted what kind of a psychology the authors set out to write, little is left to be indicated in the review save the fact that we have here a consistently physiological presentation of consciousness.

McDougall (8). In this little book the term 'function' occurs over 40 times, but, again, as in the case of Ladd and Woodworth, except for a very few cases, with a uniformity of meaning. The meaning assigned, appears under our II, *viz.* that of 'activity' for its own end. After considering the various methods of psychological description, 'faculty' psychology, linked associations, physiological psychology, the author goes on to say:

We have already approved of the method of describing mental process which consists in speaking of it as the activity of a subject; but instead of saying that the subject exercises these activities about ideas, we agreed to say that the subject, or (as we may now say if we prefer the mode of expression) the mind thinks of objects in these various ways. Now, if we recognize a subject, we must admit that it has certain faculties; for a subject devoid of capacities would be a nonentity. And by a "faculty" we mean a capacity for an ultimate, irreducible, or unanalyzable mode of thinking of, or of being conscious of, objects; a capacity which we have to accept as a

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[21] Among them are McDougall (*op. cit.,* 201), Pillsbury (*op. cit.,* 341), and Yerkes (*op. cit.,* 293).

fact, and which we cannot hope to explain as a conjunction of more fundamental capacities. [p. 77.]

He is, in strict accordance with his program, faithful to the meaning of 'function' in the sense of 'activity;' and he is at liberty, as he does in one instance, to use the term 'faculty' as an equivalent for 'function':

He [Janet] assumes that the unity of the mind, as normally revealed in the direction of its activity towards one topic at any one moment, is conditioned by the exercise of a synthetic power or energy which is one of the fundamental functions or faculties of the mind. . . . [p. 201.]

In other places he equates 'function' with 'activity':

Now, besides abstracting from the active or functional aspect of consciousness, this method necessarily falsifies the facts by neglecting the actual changes and by breaking up the continuity of the whole stream of consciousness, both the continuity of the parts which make up the whole at any one moment of time and the continuity of the whole at successive moments. [p. 49.]

The one example of the meaning of 'function' with a biological reference, *i. e.,* our I-B-1, must be recorded:

All mental activity, then, normally issues in bodily movement; since only by promoting and guiding bodily movement can it fulfil its function. Conation is the application of mental energy to the direction and maintenance of the bodily activities by which the life of the race is furthered, and cognition governs bodily activity only through the medium of conation. [p. 105.]

It is obvious, then, that in this book we have a conception of mind similar to that of Baldwin: the mind acts much as a physiological organ; it does work; it acts out its own destiny. That this conception of acting is consistently carried out in the book is the judgment made as the result of the review.

Myers (9). 'Function' is used only eleven times in this work; all usages, with one exception, are in connection with physiological processes, and all appear in the first (or 'textbook') part. One-half of the meanings come under our rubric I-A, and the remainder under II. Of those usages that have the sense of 'service' with other processes as an end (I-A), the following is a good example:

The absence of Purkinje's phenomenon and of the photochromatic interval at the fovea, when taken in conjunction with the absence of rods at the fovea, suggests that while the cones are concerned with ordinary vision under conditions of bright adaptation, it is the function of the rods to develop colourless sensations in the dark-adapted eye. [p. 83.]

On the psychological side, we have the following example:

There are always resistances inhibiting the carrying out of the 'determination,' and it is the function of the specific act of willing persistently to re-inforce the determination. [p. 332.]

An illustration of the usage with the meaning of 'activity,' (II), is given in the appended quotation:

Motor sensations are often called 'kinæsthetic' sensations. But, strictly speaking, the labyrinthine sensations are likewise kinæsthetic in function. [p. 60.]

In general, then, 'function' is not applied to psychical phenomena in this work. In physiological contexts it may mean either 'service' or 'activity.'

PILLSBURY (10). This author uses the term 'function' about sixty times and in a variety of meanings. Of most frequent occurrence is the meaning of 'activity' (II); much less often appears the meaning of 'service' in connection with other processes as ends (I-A); more seldom 'function,' in the sense of 'service' with the organism in its defensive relation to the environment as an end (I-B-1), is found; and once the word is used in connection with the organism in an assertive and developmental attitude toward its environment (I-B-2). Taken in order of classification, we have the following examples of the usage in the sense of 'service' in behalf of other processes (I-A):

One extremely important function of the lens is the accommodation or focussing of the eye. [p. 85.]

When certain of these separate functions are grouped in one way or to the accomplishment of one end, the process is perception; when grouped in another way, imagination; in a third, memory; and in a fourth, reasoning. When other functions are introduced and practical activities are controlled, the process is will. . . . . In any event, what gives the name to the function in everyday life and in scientific usage are not the materials of which the mental state is composed or even the laws that are revealed in the operation, but the end that the function subserves. [p. 342.]

Used in connection with the survival of the organism (I-B-1), we find the following appearance of the term 'function':

The function of the nerve-cells in the colony may be regarded metaphorically as protective and connective. They compel the different parts to act together, and on the right occasion they enable the parts to respond to the external world. The nervous system enables the organism to act as a unit and not as a mass of separate organisms. [p. 20.]

As an illustration of the meaning given under I-B-2, that of 'service' to the organism in its endeavor to assert itself in a social environment, the following occurs:

The self may be approached from two distinct sides. One may ask what is in mind when one thinks 'I.' This question is on the same level as any other concerning the nature of a mental state; it is a question of structure. The other set of questions deal with the capabilities of the man; they ask what the self does in different relations, they raise problems of function. The one problem is of what the man himself appreciates as himself; the other asks what it is that makes an observer regard the man as continuously the same person, why he is trusted to act in a definite way, at all times. [p. 343.]

Finally, we come to the meaning which occurs most frequently in the book, that of 'activity' working in its own behalf (II):

The theoretical considerations may give any conclusion indifferently, and popular opinion seems to be much divided as to how far the effects of training may spread from the function actually exercised to other related functions. . . . On the one hand, the separate functions have been regarded as absolutely distinct; on the other, any training is assumed to be effective for all mental capacities. [p. 331.]

One can assert at present only that whether training in one act or in one field will be beneficial to other different acts or functions of the same sort depends upon whether the two functions have anything in common, and whether the common factor works in the same way in each of the activities in question. [p. 338.]

Read (11). In his preface this author frankly states his "belief that at the beginning of the study there is little value in the differentiation of psychology into such types as functional and structural, for both aspects of the description and explanation of mental life seem natural and harmonious to the beginner if the differentiation is ignored," and he goes on to say:

This book, accordingly, attempts to tell its "plain tale" regardless of such distinctions, yet makes use of the contributions of both types of psychological treatment. [p. iv.]

We shall not be surprised, therefore, to find no restraint placed upon the use of the term 'function.' The 40 odd occurrences of the word are, in fact, distributed over all four rubrics in our classification of meaning. Exactly one-half of the appearances of the word fall under our first classification (I-A), *i. e.*, 'function' means, in this case, 'service' in connection with other processes. Of this class, the following are illustrations:

We have indicated that the fibrous whitish courses have as their function the carrying of nervous impulses to and from the brain. The main function of the cell bodies forming the gray matter of the cord is to receive and send out again the various nervous currents. Certain reflex acts are controlled by these cell bodies. That is, some of the sensory currents are received by these cell bodies, and are immediately transferred to those motor courses which also have a functional connection with them. [p. 39.]

In the treatment of perception as well as that of sensation the main part of the story is taken up with the nature of these processes from the structural point of view. But their functions are important and should not be lost sight of. The first function of perception is its guidance of conduct by interpreting sense stimulation in terms of past experience. . . . . The second function of perception is the part it plays in the further development of knowledge. It will be made clear as we proceed that memory, imagination, conception, judgment, and reasoning are developed from perception experiences. . . . . The main function of sensations was seen to be their use in the development of perceptions. [pp. 130-1.]

Not a few references to 'function' as used in the sense of 'service' to the organism in defense of its environment, are found. Some of these are given:

The first feature of consciousness in our account, then, may be stated as follows: Consciousness, as a whole and in its various parts, has as its office or function the furthering of the life of the organism by means of the superior adjustment which it is able to bring about. [p. 19.]

Reference has already been made to the main function or use of sensations in the conscious life of the organism, namely, to furnish the material to be developed into perceptions and ideas for the guidance of the organism in making superior adjustments to the conditions of its life, conditions physical, social, and spiritual. [pp. 106-7.]

Both appearances of the term 'function' in connection with the meaning of 'service' to the organism in an assertive and developmental relation to its social environment (I-B-2) have already been quoted in illustration of the original classification.[22] There remain, therefore, examples of the use of 'function' with the meaning of 'activity' for its own sake (II):

In addition to this there must be taken into the account the likelihood of *A* discharging into *B*, rather than into any of its other functional connections, this likelihood depending upon the frequency of their working together, their recency of connection, the intensity of the nervous impulse in their former functioning, the scarcity of other functional connections, and the general brain set or tension at the time. [p. 186.]

Popular speech is apt to be misleading when it makes use of such

[22] *V.* page 103.

expressions as "the imagination" and "the power of imagination." These phrases are in keeping with a view of the mind no longer tenable. They are but abstract ways of speaking of the concrete images and of the mind's functioning in the way of imaging. [p. 224.]

Briefly, this book takes a decidedly biological point of view, for most of the usages of 'function' with the meaning of 'service' with some other process as an end, appear in biological contexts. The environment is always present in the background, and consciousness is a mechanism placed in the organism to enable the organism to adjust itself better to that environment.

STOUT (12). In this book the task of classification is simple because of the consistency in the usage of 'function.' With one exception, and that a doubtful one, all of the 21 appearances of the term come under class I-A, *viz.* that of 'service' in behalf of some other process or 'function.' To illustrate:

The function of the central nervous system is to control and combine the various processes which go on in different parts of the organism. [p. 26.]

But it [ideal construction] defeats its own end if it contradicts perceptual data; for it is ultimately founded on perception. Its materials are drawn through conceptual analysis from the concrete content of perceptual experience, and its function is to connect detached data of perception in a system through a process of conceptual synthesis. [p. 170.]

The one exception is probably our II, meaning 'activity for its own sake':

In grief there is general depression and disturbance of the vital functions, accompanied by cries, complaints, and movements which give relief by drawing off nervous energy, instead of specific motor attitudes in the way of practical adjustment to surrounding conditions. [p. 191.]

All 'functions' are therefore classifiable under the rubric of 'service' and further other processes in the organism.

THORNDIKE (13). Two meanings of 'function' appear in this work: 'service' in connection with other processes (I-A), and 'service' in connection with the maintenance of the organism in its environment (I-B-1). Examples of the first of these are:

The function of thoughts and feelings,—*i. e.*, the work they do, the service they perform, their share in the business of life,— is to influence actions. [p. 111.]

The function of a general notion or concept is to provide a constant mental sign for any one of the members of a group. [p. 116.]

Of somewhat less frequent occurrence is the second meaning of 'function,' our I-B-1:

The function of the permanence of mental changes in conscious memory and in unconscious habits of thought and action is, of course, to permit experiences to extend their influence into the future. Man and other animals as well would quickly succumb to the environment if the lessons it taught them in one hour were all lost during the next. [p. 115.]

The function of attention, is, first, to economize time and effort. The selective activity for which attention stands concentrates mental life upon the things, qualities, and conditions of moment to us and allows the rest of the universe to slip by without taking our time. It allows us to proportion the prominence any thing shall have in the mind to the importance it possesses for our welfare. [p. 118.]

In the comparatively short section of the book in which functional aspects of psychology are treated, we find, then, a biological tendency manifested, a tendency which pervades passages in which 'function' occurs in either of the two meanings noted, but, of course, more noticeably in connection with the meaning of 'service' in behalf of the organism (I-B-1).

Titchener (14). With the exception of 5 occurrences of the term 'function' with its mathematical meaning and one occurrence in the sense of our I-B-1, or 'service' with the organism in its defensive relation to the environment as an end, already quoted,[23] the remaining 17 appearances are classifiable under rubric II, as synonymous with 'activity' for its own sake. All occurrences have physiological bearing, *i. e.*, they do not refer to psychical phenomena. Examples of this predominant meaning of 'function' are:

Even, however, if we grant—and the point is more than doubtful—that contrast between feelings occurs, introspection shows that the sour quality is itself intensified; and the explanation is therefore to be sought in the sphere of sensation. The sweet-sensitive bulbs have been put out of function by adaptation to the sweet of the pudding, so that the mixed, sweet-sour stimulus affects only the sour-sensitive bulbs. [pp. 140-1.]

For the most part, the action of the heart and lungs is not accompanied by sensation. There are times, however,—after severe exertion, or during transient disturbance of function,—when the separate heart-beats are clearly sensed as a dull throbbing pressure: it is not easy to say whether the sensations are localised in the body wall or in the heart itself. [p. 189.]

The formulations of this law of dynamogenesis, as it is called, are usually sweeping, and do not always tally. In general, however, they carry two implications: that the reflex arc is the unit, the typical

[23] *V.* page 104.

unit of function, of the human nervous system; and that psychology must take account, not only of the afferent process which is correlated with sensation, but also of the efferent process which prompts the organism's response to stimulation. [p. 488.]

In the greater number of cases, then, 'function' appears to be used in this book in the sense of physiological 'activity' with, except for one case, no ulterior motive save its own activity.

YERKES (15). Out of the 15 occurrences of the term in this book, one has the sense of 'service' in behalf of the organism for defensive ends (I-B-1); the others are all classifiable under II as meaning 'activity' for its own sake. To cite examples in order of numerical classification:

It is the function of environment—the whole of which is education—so to develop each of us that the human type of will comes into existence beside instinct. Life is for every human being, first and foremost, a process of acquiring self-control. If the practices of education do not further this process with maximum efficiency, they are unsatisfactory. [p. 401.]

So much for the meaning of 'service' for the organism; a few instances of the usage with the meaning of 'activity' for its own end, our rubric II, are now in order:

It is the avowed business of physiology to study the functions of the living organism and of its parts or organs. It describes these functions in terms of energy, and, if it is consistent, never in any other terms. . . . . Consciousness, however, is not energy, although it may prove to be a manifestation or accompaniment of certain energetic phenomena in the body. Hence it cannot be described in physiological terms. [p. 21.]

Where perfect clearness of mental content exists, the attention process is at its best. It is functioning at its maximum efficiency. Where vagueness exists, the attention process is functioning partially and incompletely. [p. 294.]

Anything that lessens the secretion of the salivary glands, the pancreas, the liver, the kidneys, soon brings about conditions whose accompaniment is disagreeableness of affection. These same conditions to be sure may tend to stimulate to greater activity the parts affected, but the fact remains that so long as function fails of its normal level, we continue to "feel badly" or to "feel uncomfortable." [p. 367.]

Manifestly, then, we discover a fairly consistent usage of the term 'function' in this book, and we find that in most instances it is used with physiological reference. It most commonly means 'activity' directed toward the purpose of acting.

Summary.—While the evaluation of the tendencies peculiar to each one of the several authors is not easy, a number of comparisons, based on the review of the textbooks, can be made. Certain important facts, indeed, are almost self-evident. In the first place, two psychologists, Myers and Titchener, very rarely or never use the word 'function' with reference to psychical phenomena, but only in connection with physiological facts. In the second place, usage of the term with decided biological significance occurs, in descending order of rank, in Angell, Judd, Read, Thorndike, and Calkins. In the third place, tendencies of attributing to mind manifestly active characteristics occur, with the exception of the two mentioned above, in all of the books reviewed, but the notion of mind in all these activities as an output or product of the brain and nervous system in general, in a fashion similar to the products of other bodily structures, occurs most noticeably, in descending order, in Ladd and Woodworth, Baldwin, and McDougall.

The most frequently used meaning of 'function' is that of 'activity' (II), in both physiological and psychological contexts, but with a greater frequency of occurrence with physiological reference; less often comes 'service' with other processes as ends (I-A); then still less often appears 'service' in behalf of the organism in its defensive relation toward its environment (I-B-1); and least often we find 'service' in behalf of the organism in its offensive relation to its environment (I-B-2).

The writers who are most consistent in the use of the term, *i. e.*, who use the term with practically only one meaning are Ladd and Woodworth (II), McDougall (II), Stout (I-A), Titchener (II), and Yerkes (II).

In almost all of these writers we find two factors immanent in the meaning of the term 'function' as used. There is, as is indicated in a previous paragraph, a majority vote on the use of the term 'function' in the sense of 'activity.' Not only is this true, but there is, in addition, an underlying tendency to instil into every other meaning of the word an active principle of some sort, a 'doing,' 'performing,' 'fulfilling' principle, which makes itself felt, if nowhere else, in the context and in the expressions chosen. The term 'function,' then, in whatever meaning in our classification it may occur, spells 'activity' of some kind. The other factor is that of 'purpose' or 'end.' Mental and bodily structures are described from a teleological aspect.

This may be conveyed in two ways: 'function' is usually ascribed to a particular part of the physical or mental structure; or a setting of such a nature is definitely assigned to an activity that it becomes indispensable to other dependent activities. We may have, for example, a functioning peculiar to a given structure, such an activity as could be accomplished by no other part of the mind or body; or we may have added, explicitly or implicitly, the notion that that peculiar activity was purposefully assigned to that part of the mind or body, or, at any rate, that the total mind or body might suffer through the inactivity of any of its special parts. In the first case, there is an assignment of activity which fits into the general scheme of the whole; in the second case, the assignment of activity is definitely made toward the completion of a larger task, or for the purpose of making possible the perfection of another activity. To illustrate the distinction concretely, we may say,—to the frontal lobes of the brain the 'function' of association is ascribed: it would then be the duty of these lobes to act in the manner of *associating, i. e.,* in the established harmony of the organism that is their allotted job; now, in addition to simply acting, doing an assigned task, these lobes of the brain may associate psychical material for the purpose of furthering the function of memory within the organism: the end is, therefore, not the completion of an allotted task, but the completion of another dependent function. Teleology of one of these kinds is implied, then, in most usages of the term 'function.'[24]

The impression obtained from a detailed review of the literature, which introduces the beginner in many of our academic institutions to the science of psychology, simmers down, then, to this: In a large number of the books,—the larger number of the group selected,—whatever be the dominating standpoint of the author, mind is still considered as an active and purposeful 'organism' of the individual. It is also to be noted that very few of the writers considered were consistent in the use of the term 'function' with a set meaning. It is to be hoped, consequently, that some understanding as to exactly what is implied in the term will be reached; at least it would be of advantage to the student to know what, in a given case, the author means when he speaks of 'functions' of the mind.

[24] These underlying factors, of 'activity' and 'purpose,' came to light as the result of the empirical analysis of the textbooks. We are aware of the fact that many psychologists, the books of some of whom were included in our review, are inclined, as a rule, to account for these factors in a manner different from our own. It is our intention to treat this broader aspect of the problem in a later article.

DISCUSSION

PROFESSOR MARTIN ON THE PERKY EXPERIMENTS

By E. B. Titchener

I do not intend to offer here any general criticism of Professor Martin's recent work on imaginal complexes.[1] I am not at all sure that the formation which she terms indifferently 'image of presentation,' 'image of memory,' 'image of recollection,'[2] is identical with the memory-image of current psychological enquiry; I am not convinced by her analysis of Fechner's after-image of memory;[3] and I think that the limits of usefulness of the projection-method must be drawn more narrowly than she has drawn them. On all these points, however, Professor Martin has supported her views by experimental evidence; and I defer their discussion until further experimental evidence, for or against, is forthcoming. It is my present purpose to examine only a small part of her published work,—the experimental series IIb. and IIc., which were undertaken as a test of certain results obtained by Perky in my laboratory.[4] "The results of these experiments show clearly," says Professor Martin, "that the well-marked differences between image of memory and image of imagination, maintained by Perky on the basis of her experiments, did not exist at any rate for the observers employed in my own experiments."[5] Here we have experiment set over against experiment; Professor Martin roundly declares that the differences found by Perky did not exist for her observers; so far as these observers are concerned, the negative statement is unqualified. The question naturally arises: What, then, was the nature of Professor Martin's test?[6] Is it adequate to her conclusion? Has she repeated the Perky experiments under Perky's conditions?—In what follows I try to give an answer to this question.

(1) *The Observers.*—"Any one who has had personal experience in experimental psychology, and especially in its analytical form," writes Professor Martin, "knows how infinitely much, for the results

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[1] L. J. Martin, Die Projektionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder, *Zeits. f. Psych.*, lxi., 1912, 321 ff.

[2] *Op. cit.*, 322, 329, 332, 340, 344 f., 368, 370, etc. The terms are *Vorstellungsbild, Gedächtnisbild, Erinnerungsbild.*

[3] *Ibid.*, 346 ff., 364 ff. In her criticism of my *Thought-processes* Professor Martin appears to have overlooked Ach's explicit reference to Fechner; she has also overlooked Bentley's work on the after-image of memory (N. Ach, *Ueber die Willenstätigkeit und das Denken,* 1905, 11; I. M. Bentley, The Memory Image and its Qualitative Fidelity, this Journal, xi., 1899, 44).

[4] *Ibid.*, 398 ff.; C. W. Perky, An Experimental Study of Imagination, this Journal, xxi., 1910, 451.

[5] *Ibid.*, 412.

[6] Professor Martin states explicitly: "ich beschloss darum, die Bedingungen meiner Experimente denen der Perkyschen Versuche möglichst anzupassen:" *ibid.*, 398.

of an investigation, depends upon the availability of observers who have acquired a true psychological perspective. For such observers guarantee not only an accurate and reliable observation, but also knowledge, and therewith the elimination of fatal forms of suggestion."[7] The observers in her series IIb. and IIc. were the Misses G., H. and W.,[8] undergraduate students, two of the 'junior' and one of the 'senior' year; only one (a junior) took a 'major' in Psychology, the other two in History and English.[9] The observers in Perky's experiments IV ff. were some or all of the following: Drs. Geissler and Pyle, the Misses Clarke, Day, Rand and de Vries (Mrs. Schaub), and Messrs. Nakashima, Tsanoff and Williams.[10] All were graduate students in psychology, and the names of most are familiar to readers of the JOURNAL.

I should be very sorry to have this comparison interpreted as derogatory to Professor Martin's students; I have no doubt that they were skilful and conscientious observers. But if, in new and difficult studies, we attach importance—as I agree with Professor Martin that we must—to psychological training and experience, to 'knowledge' and the 'elimination of suggestion,' then I cannot think that in these two publications the scales are evenly balanced. On the score of 'psychological orientation' as on the score of numbers, the advantage is with Perky.

(2) *The Possibility of Suggestion.*—In the series IIb. Professor Martin's observers "received the instruction to project and to maintain upon the white wall before them, in whichever order they preferred, a visual image of memory and an image of imagination or, conversely, a visual image of imagination and an image of memory. . . . The observers were required to maintain their images of presentation, to compare them, and to base their account upon direct scrutiny and immediate comparison of the two images, and not upon memory." In series IIc. this simultaneous projection of the images was replaced by successive projection, the order being determined by the observers.[11]

Perky's "observers were left altogether in ignorance of the object of the work." "No one of them, so far as we are aware," Perky declares, "realised that we were in search of a distinction between image of memory and image of imagination. These terms were carefully avoided; the experimenter spoke only of 'image'."[12]

Professor Martin, then, asks explicitly for images of memory and images of imagination; Perky asked simply for images. The former instruction is surely more 'suggestive' than the latter. But, further, Professor Martin made free use of questioning; the questions "were in the main intended to discover whether the observable differences between image of memory and image of imagination were identical

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[7] *Ibid.*, 328.
[8] *Ibid.*, 399, 412.
[9] This information was kindly supplied me by the Registrar of the Leland Stanford, Jr., University.
[10] Perky, *op. cit.*, 438 ff.
[11] Martin, *op. cit.*, 399, 412.
[12] Perky, *op. cit.*, 437. For the insights gained and the terms employed by the observers, see 439 f. The instruction recorded 442 f. refers to "images of different types," but the terms 'memory' and 'imagination' were not used.

with those reported by Perky."[13] No less than 13 points were to be established, either by formal instruction or by question and answer: (1) the relative intensities of the two images, in numerical terms; (2) details of origin, course, disappearance; (3) color, magnitude, plasticity, contents; (4) degree and direction of attention; (5) concomitant auditory, kinaesthetic or other secondary images; consequent after-images; (6) observed movements of eyes or body; (7) relative stability of the images; (8) the tendency of the image to self-completion, or of the observer to change or to complete it; (9) the presence of associations; (10) relative pleasantness or unpleasantness; (11) relative realness and naturalness of the images; (12) mobility of the images; and (13) the influence of closure of the eyes.[14] Whether all these points were raised in every observation, and how many of them were included in the formal instruction, how many brought out by questioning, we are not told.

Perky's instructions were very general, and questioning was not employed at all. The observers were required, simply, to give a careful description of the course and character of their images. In special cases, where the conditions of the experiment made it necessary,[15] or where a series of observations was taken with a special purpose,[16] the general instruction was qualified in some particular manner: but at no time were questions put to the observer.[17] Again, then, Professor Martin's work appears to have been more 'suggestive' than our own.

(3) *The Nature of the Task.*—It is clear, from what has been said above, that the task given by Professor Martin is different from that set by Perky to her observers. Professor Martin's students were dealing, from the outset, with images of memory and images of imagination, so named and so distinguished; Perky's observers were dealing solely with 'images,' and worked out their own distinguishing terms as the work proceeded.[18] There is a well-marked qualitative difference in the *Aufgabe* of the two sets of experiments.

Again, however, Professor Martin's observers were required, in the first half of series IIb., to evoke imaginative and memory images of the same object (a vase, a beautiful woman, a book, etc.). To what extent the simultaneously paired images of the second half of this series, and the successively paired images of IIc., were also images of the same object, cannot be precisely determined from the account given.[19] So far, at any rate, as the first half of IIb. is con-

[13] Martin, *op. cit.*, 399.

[14] *Ibid.*, 399 f.

[15] As in the dark-room experiments with luminous spots, *op. cit.*, 437.

[16] As in the after-image series, *ibid.*, 446.

[17] Note 2, *ibid.*, 444, may seem to contradict this statement; but the contradiction is apparent only. The questions there referred to were put after the conclusion of the work, and the answers represent the general impression of the observers. Indeed, the Note itself implies that the questions were not asked during the experimental series, for the point at issue is left over for further investigation.

[18] *E. g.*, 'particular' and 'generic:' *ibid.*, 440.

[19] Martin, *op. cit.*, 399, 412. In the second half of IIb., G. invariably chose the same object for the twofold projection, "presumably for reasons of economy" (404); H. and W. apparently did not,—but numerical data are not given. The results of IIc. are stated in the most general terms.

cerned, we have another qualitative difference of *Aufgabe*. For the stimuli used by Perky were allowed to evoke each its own image, and no attempt was made to parallel an image of memory with a corresponding image of imagination.

Finally, there is a quantitative difference, a difference in the difficulty of the task imposed by the two experimenters. Professor Martin's undergraduate observers are told, at the very beginning of their work, to project and to maintain simultaneously, on the white wall before them, a memory image and an imaginative image of, let us say, a vase. Perky's graduate observers had a far easier task: a stimulus-word was dropped into consciousness by the experimenter, and the observer had merely to report the appearance of imagery (if any imagery appeared), and to describe his experience in his own words. Moreover, Perky's observers were given general preliminary practice and, later, such special practice as change of instruction rendered necessary;[20] Professor Martin's students seem —so far as the record goes—to have had no practice at all.[21] And lastly, the latter observers were required, as we have seen, to pay attention to no less than 13 prescribed points, whereas Perky's graduates were free to describe their images in their own way.

(4) *Voluntary and Spontaneous Production of Imagery.*—"There is undoubtedly a difference," says Professor Martin, "between voluntarily and involuntarily aroused visual images."[22] There are undoubtedly, also, differences in the meaning which we may attach to these terms 'voluntary' and 'involuntary.' Thus Professor Martin writes: "in the case of H. the memory image usually cropped up first, as a whole, involuntarily, and localised to the right; . . . an image of imagination rarely appeared involuntarily and as a whole; often a memory image cropped up in its place, and this had then to be changed till H. gained the impression that she had fulfilled instructions and had projected an image of imagination."[23] The distinction here drawn is clear enough. Yet, in the large, all of Professor Martin's images were voluntary, and all of Perky's were involuntary or spontaneous. For H., W. and G. were definitely instructed to project and to maintain images of a particular sort. Perky's observers were, it is true, set for imagery by the terms of the *Aufgabe;* they were asked to be on the look-out for images. They were, however, under no obligation or constraint to produce images; if no image came, of itself, they would so report without sense of failure; and if an image came, they were to "give themselves up" to it; "they were not in any way to control or regulate

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[20] Perky, *op. cit.*, 435 f., 437.

[21] Martin, *op. cit.*, 399. Perky remarks (448, note) that the observer *V*—an observer of what is, in our experience, quite unusual imaginal endowment—had *on one occasion* a pure fancy image and an indefinite memory image (these terms were not the observer's own!) projected side by side on the same surface. This occurrence, unique in our work, is made by Professor Martin the paradigm of her first method: her undergraduate observers are told, in set terms, to produce and project simultaneously the two sorts of image,—and are able, without further definition, and without preliminary practice, to satisfy the requirement in 4 to 30 sec.!

[22] *Ibid.*, 375, 371.

[23] *Ibid.*, 401.

their imagery, but were to give it rein, passively and as association determined."[24]

(5) *The Data of Observation and their Employment.*—Professor Martin admits, as datum of observation, any imaginal complex whatsoever, provided only that it has been labelled 'memory' or 'imagination' by the observer. Thus, an image of imagination is called for; an image of memory appears; but a voluntary change of magnitude converts it, for the observer, into an image of imagination; and it is accepted as a member of the imaginative class.[25]

Perky's procedure is different. After distinguishing the two great types of image, she proceeds: "there were, naturally, a fairly large number of intermediate forms (images with personal and place references, but unfixed in time; images with personal reference, but neither temporal nor spatial context; images with context but no personal reference). The classification of these under the one or the other of the two main rubrics would have been possible, from the records, although it would have left a margin of uncertainty, aside from that due to the possibility of an incomplete introspective account. Fortunately, we had no need to attempt it, as the clear-cut cases were sufficiently numerous for our purpose."[26] The mixed or intermediate formations are of real psychological interest; but they are not the cases to which we may appeal for the sharp descriptive differentiation of the two types of image; for that purpose we need imaginal complexes which are through and through, and from start to finish, either 'imaginative' or 'memorial.'

It follows, then, that Professor Martin's material is not comparable with that used in our work. Thus, Professor Martin calls for imaginative and memory images of a rose. The observer W. sees a red rose, which she reports as image of memory; thereupon follows the image of a green rose, which is reported as image of imagination. "The two images were alike in all details with the exception of the color."[27] To change the form or size or color of a memory image is a short and easy way to produce an image of imagination: and in view of the difficulty of their task I do not wonder that the observers, in all good faith, had recourse to it. I am surprised, however, that Professor Martin, with the Perky results before her, could suppose that observations of this kind were relevant to Perky's conclusions.

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[24] Perky, *op. cit.*, 435. The instruction on 442 f. is said to have "set up a tendency to alternation;" but, the passage continues, "this tendency was oftentimes cut across by the intrinsic suggestion of the stimulus," with the result that there were 154 memories to 103 imaginations, a ratio of 3:2 and not of 1:1. I doubt if the tendency was ever clearly conscious; I am sure that neither observer intentionally changed or modelled an image in order to force it under the one or the other rubric.

[25] Martin, *op. cit.*, 401, 403, 407.

[26] Perky, *op. cit.*, 436. I may add that we had intended to discuss, in a second publication, the nature of the mixed forms, and the temporal data (437). The ill-health of the experimenter has made it impossible for her to fulfil this intention. We are now taking up the whole problem afresh, with special reference to Professor Martin's method of projection.

[27] Martin, *op. cit.*, 403.

(6) *The Number of Observations.*—Professor Martin's series IIb. contained 50 observations of simultaneous 'doubled images' from each one of 3 observers: 150 observations in all. The number of observations in IIc. (2 observers) is not stated; let us assume that there were, again, 50 for each observer: 100 in all.[28] The full tale of observations used to test Perky's results is then 250.

Perky obtained in §2 (kinaesthetic elements) 572 visual images of pure memory and 709 visual images of pure imagination, together with an unnamed number (in fact nearly 200) of mixed forms.[29] I omit the auditory and olfactory experiments. In §3 (affective factors) Perky secured 103 pure visual imaginations and 154 pure visual memories.[30]

Again, then, the advantage is with Perky. Indeed, if I have rightly estimated the length of series IIc., Perky discarded (as mixed or intermediate forms) more observations than Professor Martin took from her three observers in the two test-series combined.

(7) *The Distinction of Memory and Imagination.*—Professor Martin left it to her undergraduate observers to make and to use their own definitions of Memory and Imagination: "the results of our experiment are bound up with the definition (*beziehen sich auf den Begriff*) that as a matter of fact was adopted by the observer."[31] Perky's observers were never required to frame any definition. Perky *found* that "a good proportion of the images thus aroused [by verbal stimuli] were of two sharply different kinds." There were images of recognised and particular things, figuring in a particular spatial context, on a particular occasion, and with definite personal reference: these images Perky herself named 'images of memory;' the observers either left them unnamed, or employed such a designative term as 'particular.' There were, on the other hand, images with no determination of context, occasion, or personal reference,—images of things recognised, to be sure, as a hunting-scene, a conflagration, or what not, but not recognised as this or that particular, individual and familiar scene or object: these images Perky named 'images of imagination;' the observers either left them unnamed, or employed such a designative term as 'generic.' It seemed evident that the classificatory terms 'of memory,' 'of imagination,' were justified by current psychological usage; though, in her Summary, Perky warns against generalisation from these fairly simple imaginal complexes to the wider psychology of memory and imagination.[32] At any rate, the difference noted was empirical, found in the introspective reports before classification had been at all attempted; and the main body of Perky's investigation is devoted to the introspective differences obtaining between those images which she, as experimenter, had named 'memorial' and those which she, again, had named 'imaginative.' I cannot speak positively for all of Perky's observers; but I can speak for most of them; and it is true of these that not till they read Perky's paper did they learn what their observations had served for,—the introspective differentiation of simple forms of 'memory image' and 'image of imagination.'

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[28] *Ibid.*, 399, 412.
[29] Perky, *op. cit.*, 442.
[30] *Ibid.*, 443.
[31] Martin, *op. cit.*, 413 note.
[32] Perky, *op. cit.*, 435 f., 452.

Professor Martin finds that "recognition is the most decisive factor" in the discrimination of memory and imagination.[33] This result accords, so far, with Perky's more highly analytical statement that "a glimpse of this [recognitive] mood could be caught, by alert introspection, at some stage or other in the course of every memory image."[34] Professor Martin, however, continues: "It is interesting that in cases where an image was cognised at first as a fancy, and later as a memory image, and conversely, the observer's change of view was not based on the observation of those peculiarities which Perky declares to be characteristic of the two forms of image."[35] I have already pointed out that we may not argue directly from images which appear spontaneously to images voluntarily aroused. With this reservation, my comment on the passage is twofold. In the first place, all such changing or dubious images would have been discarded by Perky, on the ground that they did not permit of accurate classification. In the second place, the introspective 'peculiarities' were worked out, in special experimental series, *after* the gross distinction of memory and imagination had been made by the experimenter. Perky's observers had no list of peculiarities, whereby they classified their images; the peculiarities were brought out, so to say, piecemeal, and were grouped and listed by the experimenter; and they were never brought out under the stated rubrics of memory and imagination.

Professor Martin declares that she is surprised, not only by the results of the Perky experiments, but also by their uniformity.[36] So were we. She is good enough to add that she has no wish to disparage them:[37] but I do not know that facts of observation suffer from disparagement. She has found them sufficiently interesting, as matter of individual psychology, to undertake their experimental testing.[38] I have shown above that on the ground of number of observers, training of observers, and number of observations, the test leaves something to be desired. But I have, on my side, no desire to emphasise this criticism; it is always a somewhat unthankful task, when a score of new things are waiting to be done, to turn aside to repeat the work of another investigator; and we are grateful to Professor Martin for the time and pains that she has bestowed upon our work. Unfortunately, as I believe I have shown in detail, the problem which she attacked was not Perky's problem, and the method which she employed was not Perky's method. Until, therefore, the two problems and the two methods have been combined in some synthetic study, and until a correlation has been made out by the comparison of results obtained from the same individuals, it is difficult to say what bearing Professor Martin's observations have on those of Perky, or Perky's conclusions on those of Professor Martin. As things are, the two investigations barely touch.

In conclusion, Professor Martin "disputes my right" to speak of the Perky experiments as I have done in my *Text-book*.[39] In the heat of controversy she has here, I think, done an injustice, not

[33] Martin, *op. cit.*, 413 note.
[34] Perky, *op. cit.*, 443.
[35] Martin, *loc. cit.*
[36] [37] [38] *Ibid.*, 413.
[39] *Ibid.*, 413.

solely or principally to my book, but more especially to her own wide knowledge of psychology. For she well knows at how many points a psychological system that is based upon experimental results must rely upon a single investigation. I spoke above of the thanklessness of repeating someone else's work; yet sheer repetition, repetition without variation, is again and again the demand of the psychologist who seeks to bring results together. Professor Martin knows, too, that the value of the single investigation depends in large measure upon its power to illuminate a subject, to reconcile conflicting results, to bridge a gap in our general knowledge. Had she viewed Perky's work impartially, and not through the glasses of her own method of projection, I believe that her judgment would have been different, and that her tests would have followed Perky's method rather than her own.[40] My treatment of the subject in the Text-book is discussed elsewhere.[41]

I shall welcome any repetition of Perky's experiments, whether the outcome be positive or negative, confirmation or contradiction. I am ready to extend or to amend the definitions of 'memory image' and 'image of imagination.' Meanwhile, I cannot see that Professor Martin's two experimental series are relevant to the points at issue.

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[40] In the present state of experimental psychology, it is surely at least as important to emphasise agreement as to point out difference of results. Yet there is no single passage in Professor Martin's account (*ibid.*, 398-413) in which agreement is mentioned. In fact, when allowance is made for divergence of method, there seem to be a good many observational details which resemble those reported by Perky.

[41] See Memory and Imagination: a Restatement, *Psychol. Rev.*, xix., 1912, 159 f.

BOOK REVIEWS

Contributo Psicologici del Laboratorio di Psicologia Sperimentale della R. Universita di Roma. By S. DE SANCTIS and others. Vol. I, 1910-1911. Presso il Laboratorio di Psicologia, Roma, 1912.

This is a new annual established by Professor De Sanctis, Director of the Psychological Laboratory of the University of Rome, for the purpose of publishing in collected form, the results of investigations undertaken in his laboratory. This first volume contains twenty-three papers, most of which have previously appeared in psychological journals. Not all the articles included are laboratory studies, though all deal with some topic connected with experimental psychology. Ten of the papers are by Professor De Sanctis and discuss the following subjects. "Methods of modern psychology," "External manifestations of thought" (this is a discussion of various theories and not an original laboratory investigation); "Experimental psychology and pedagogy;" several papers treating of problems connected with abnormal children, one of which is a series of mental tests, and "A new method for the study of mental work." This last consists of the use of a series of words read rapidly by the subject, and a later presentation of the words in an incomplete form, the subject being required to complete them. The difference in time between the two series is taken as a measure of the work performed.

Other contributions are as follows: "An experimental critique of the doctrine of tactile points" by Emilia Barucci; "Simple reaction time and predisposition (Einstellung) of the attention" by Isabella Grassi (translated and published in German); "An experimental introduction to the study of types of mental work" by Maria Maccagno; "Researches on the attention, memory and intelligence of children from nine to fifteen years" by Alighiero Micci; "Experimental psychological investigations on very intelligent children" by Alda Jeronutti; "A study of the dreams of children of three years" by S. Doglia and F. Banchiere, and "A contribution to the knowledge of psychic deafness." This last is a study of an individual case under observation for a period of four years.

THEODATE L. SMITH.

Reaction Time to Retinal Stimulation, with special reference to the time lost in conduction through nerve centers. By A. T. POFFENBERGER, Jr. Ph. D. Arch. of Psychol., No. 23. (Columbia Cont. to Phil. and Psychol. Vol. XXI, No. 1.) New York, 1912. iii + 73 pp.

An effort has been made in this study, by means of reaction-time, to discover the time required for nervous conduction to pass over a single synapse between two neurones. The author reviews the literature on nerve conduction and shows that the synapse has come to be considered the critical point of attack for the study of differences in reaction-time for different parts of the body. In this review of the literature, the criticisms of method are pointed out in every study, one of the chief criticisms being that the two paths chosen have not been directly comparable. The author obviates this difficulty by his choice of conduction pathways. A visual stimulus makes use of the fact that stimulation of the right side of both retinas goes to the right cuneus; while stimuli falling on the left side of either retina go to the left cuneus. Thus a direct pathway may be obtained from the cuneus on one side with the motor area on the same side and a reaction with the opposite hand; and an indirect pathway is obtained if the subject reacts with the hand on the same side as the cuneus stimulated. The only difference between the direct and the indirect pathways would be a commissural cell in the brain or cord and thus the introduction of another synapse. The apparatus consisted of a Froeberg exposure wheel connected with a Hipp chronoscope. The fixation was ingeniously obtained by introducing a modified perimeter into the experimental arrangement. On the basis of 10,000 reactions, obtained with various angles of peripheral vision, the author concludes that a very definite time is required for the passing of nervous excitation over a synapse, since the reaction-times for the indirect pathways are uniformly greater than those for direct pathways. Certain objections were raised by the author; their influence was ascertained by means of test experiments, and on this basis they were either allowed for or rejected.

One is impressed by the fact that this study shows great care and ingenuity of experimental arrangement; one can not but feel, however, that it is based on a rather uncertain foundation of anatomical and physiological hypothesis. However, if these hypotheses are valid,—and this very study may give another indication that they are,—the author has made a positive contribution to psychological knowledge.

Clark University, SAMUEL W. FERNBERGER.

Influence and Adaptability. An Experimental study of their relation, with special reference to individual differences. By ARTHUR JEROME CULLER, Ph. D. Arch. of Psychol., No. 24. (Columbia Cont. to Phil. and Psychol. Vol. XXI, No. 2.) New York, 1912. v + 80 pp.

As the title indicates, this study deals with individual differences in the general field of interference between two conflicting associations, and with the adaptability of the subject by means of which the reactions to both become automatic. Interference may be of two types: *1.* When one association is well established before another is introduced into consciousness; and *2.* when two mutually opposing associations are alternated. Several variations of experimental arrangement were employed in order that both types of interference might be investigated; and as the study was primarily one of individual differences a large number of subjects were used.

In one form of experiment the subject associated certain fingers with the pressing of certain typewriter keys; and after this habit was well established, he changed the relation of keys and fingers. In another form of experimentation, he reacted with right and left hand to stimuli of different colors, the time being recorded by means of a Forbes chronoscope. These, however, seem to be considered by the author as preliminary experiments and not of the value of his later work. The latter consisted in a variation of the Bergstrom card-sorting experiment, and was performed on thirty-four subjects, half of whom were men and half women.

From his results he concludes that interference occurs for all subjects; but it decreases with practice, and finally the reactions to both associations become so automatic that either may be called up without the appearance of the other. As regards his real problem, the author finds great individual variations in the rate of improvement, or in the actual time records. He finds no significant sex differences in the rate of improvement or in the absolute time records. The women show great variability, however, in the recurrence of the old or wrong associations.

This paper seems to be too general in its form and in the method of collecting data to be of value. For example, the author shows a positive correlation between adaptability and the traits of individuality, independence and originality, and the measure of these latter is obtained by an averaging of the opinions of twelve friends of the subjects expressed in numerical terms. The author includes several figures, but these contain so many curves that it is almost impossible to follow out any single one of them.

Clark University, SAMUEL W. FERNBERGER.

Reaction to Multiple Stimuli. By JOHN WELHOFF TODD, Ph. D. Arch. of Psychol., No. 25. (Columbia Cont. to Phil. and Psychol. Vol. XXI, No. 3.) New York, 1912. iii + 65 pp.

The author studied reaction-times to multiple stimuli presented simultaneously,—light, sound, and an electric shock being used and the time recorded with a Hipp chronoscope. These stimuli were presented singly, in pairs, or in groups of all three, and it was found that presenting the stimuli in pairs resulted in a shorter reaction-time than that of either stimulus presented singly. If all three stimuli were given simultaneously, a still further reduction resulted. It was ascertained that the reaction-time to sound was usually the most rapid, that to light the least rapid; and it was further found that light in combination with either of the other stimuli had the least facilitative effect, while sound had the greatest.

A certain group of reactions were taken to discover if the subject could pick out one stimulus from the complex and react to it; but it was found that this did not give different results from those of the same complex as a whole. An experimental arrangement was then used for reaction to successive stimuli with varying intervals between them, the reagent always reacting to the last one. The results show that this arrangement increases the time of reaction, but this lengthening of the reaction-time decreases as the interval is shortened.

This latter phenomenon is explained by the author in terms of inhibition. The reduction of reaction-time to multiple stimuli is explained in terms of summation of stimuli which can then cross the synapses between the neurones more rapidly than the single stimulus. This is considered to be a further proof of the validity of Cattell's view that the reaction event is a cerebral reflex, and the author cites similar reinforcements in the general field of reflex movement.

This is a very suggestive study, both in regard to the data which it presents in a comparatively unworked field, and as regards the problems which it raises for further research.

Clark University, SAMUEL W. FERNBERGER.

Principien der Metaphysik. Von Branislav Petronievics. Erster Band. Zweite Abteilung. Die realen Kategorien und die letzten Principien. Mit 43 Figuren im Text. Heidelberg, Carl Winter's Universitätsbuchhandlung, 1912. pp. xxxviii, 572. M. 16.

This book comprises the second division of the first volume of a *Gesammtwerk* which has evidently been conceived in its entirety and is offered to the world as a system. It is thus apparent that final judgment and adequate criticism of the author's thought must abide the completion of the undertaking; only a tentative statement of it, on the basis of the second division, is attempted in this brief notice.

The reviewer regrets that he has not had the opportunity to read the first part of Dr. Petronievics' treatise, published in 1904. The analytical outline of it, which is supplied with the present volume, indicates its general scope: an introduction to ontology, and a discussion of the formal categories, with an Appendix containing the elements of the new geometry.

In the present volume of six hundred odd pages the author undertakes first the solution of the qualitative world-problem, that is, the problem of the qualitative-quantitative structure of reality. He recognizes three historically significant positions setting out from the principle of the absolute reality of the immediately given: extreme naïve realism, moderate naïve realism, and absolute conscientialism or absolute consciousness-realism. The criticism of these three philosophical theories serves to introduce the fourth possible metaphysical attitude, which Petronievics believes he is the first one to advocate, and which he calls 'relative conscientialism.' After an analysis of the immediately given in its fundamentals, the author proceeds to more detailed elaboration of his theory, reaching his conclusions, first by an analytic-inductive, and second by a synthetic-deductive method. The theory is then applied to the solution of problems such as the instability of matter, the immortality of the soul, and the values of the dynamic and of the static *Weltstadium.* In this connection the pessimism of Schopenhauer and Von Hartmann is criticised.

The latter part of the book is devoted to the 'hypermetaphysical problem.' In distinction from metaphysics, which concerns itself with the world in so far as it is real, hypermetaphysics, we are told, deals with a still more ultimate problem, and seeks to show 'how Being is made.' This particular part of his work Petronievics clearly intends for those whose metaphysical demands know no limits,—that is, for the distinct minority of the *Journal's* readers. The topics covered are substantially the same as those of the preceding sections, but the standpoint is now throughout that of 'the ultimate essence of negation,'—a term of which Petronievics has apparently made abundant use in his first volume, and which he employs rather obscurely.

What Petronievics undertakes is a union of Spinoza's monism and Leibniz's pluralism. The 'monopluralism' which results is based, it appears, primarily on the analysis of immediate experience beginning with pp. 65 ff. of this volume. This analysis contains the metaphysical heart of the book. In connection with the dualism of subject and object, which the author considers the most fundamental fact of immediate experience, he opposes the doctrine that the *Ichsubjekt* subsists in the pure Being of the psychic content, or else exists as a special, independently given reality beside the contents of consciousness. Rather is the *Ichsubjekt* a contentless, purely formal act of knowledge, knowing itself as completely and undividedly present in the manifold contents of consciousness, and as ascertaining with absolute simultaneity these contents in all their existence and character (pp. 66-67).

Naïve realism, in both its forms, and absolute conscientialism, fail to solve the problem of the individual, for they all stand on the basis of the absolute identity of knowledge and the *Existenzmomente* of Being, and are thus compelled to admit a spatial relation between the psychic contents of the individual (p. 117). Petronievics therefore maintains the reality of extra-spatial knowledge-points, on the basis of a novel theory of space, which is apparently one of the achievements of his first volume, and according to which we are now told (pp. 121-122) that discrete space does not consist of real spatial points which touch each other directly, but that between every two neighboring spatial points there is a third, *extra-spatial* point which touches them immediately and prevents them from touching each other in an absolute sense. This is a bit of speculative legerdemain which does for the modern theories of space what the laughing Democritus, with his void, did for the *plenum* of his pluralistic predecessors.

This theory of space is now put to metaphysical service, namely to the solution of the subject-object dualism. The absolute consciousness-realism asserts the identity of Being and consciousness: *jedes Sein ist ein Bewusstsein.* Now it is precisely this identity of consciousness and Being which must be given up, if the problem of the individual ego is to be solved. And the absolute consciousness-realism, although it apotheosizes consciousness, does not do justice to the objective reality of the conscious subject. For, to make use of the author's mathematical analogy, all real points existing side by side in spatial relation to each other must of necessity be *parts* of the same world-space. A theory setting out from such a homogeneous conception of consciousness can not, therefore, account for the individual character of the knowing and willing ego. It is with the purpose of solving this latter problem that Petronievics proposes his *relative* consciousness-realism. According to it, sensation and feeling are the two fundamental classes of psychic content: the two real transcendent attributes of the ego, on the other hand, are knowledge and will. How are these two sides of immediate experience to be related? Not in homogeneous space, the author answers. The real points of knowing and willing form two objective world-spaces, completely corresponding to each other, but also entirely independent of each other; and between any two such real points of an individual there lies a subjective space of psychic content (containing a sensation-space on one hand and a feeling-space on the other), which is entirely independent of the two objective spaces of knowledge and will, as well as from the subjective spaces of other individuals (p. 139). In this way the two facets of an individual's consciousness, as well as the immediate experience of different individuals, are related to each other without the sacrifice of the distinctive individuality of any one. Consciousness is thus no longer a vague notion: it is conditioned by a real knowing-willing nature, and from the presupposition of the real nature of the knowing subject follows the possibility of absolutely unconscious constituents of Being (p. 119).

It is along this line that Petronievics proceeds, aiming to unite monistic and pluralistic principles in a system of metaphysics that should do justice to, and solve, the problem of the individual. It is an ingenious theory, carried out in exhaustive detail, with limitless persistence, and with a fearless readiness to face any logical consequences. It must be confessed, however, that the author's profundity is often purchased at the expense of clearness. The above skeleton outline gives only a mild suggestion of the elaborate intricacy which characterizes the book. One wonders, as one reads along, whether all these metaphysical and hypermetaphysical sesquipedalia are really essential to the solution of the cosmic problem,—even if the author's thought be worth the labor of mastering his language. In this respect Petronievics, original in so many ways, has remained true to the modern Germanic traditions of metaphysical exposition.

Clark University, RADOSLAV A. TSANOFF.

Das Problem der objektiven Möglichkeit. Eine Bedeutungsanalyse. Von AUGUST GALLINGER. (Schriften der Gesellschaft für psychologishe Forschung. Heft 16. IV. Sammlung). Leipzig, Johann Ambrosius Barth, 1912. pp. vii, 126. M. 4.

What do we mean when we characterize anything as possible? Wherein does possibility consist: does it refer to the possibility of judging about reality in a certain way, or does it involve the asserting an objective possibility? Is the judgment, 'this may be so,' equivalent to 'I may judge that this is so,' or 'I judge that this may be so?' And, if the relation of possibility is to be treated as existential, how does it differ from other such relations, as causality, necessity, etc.? It is with these questions that the monograph deals.

In order to solve the last mentioned problem Gallinger discusses the *Seinsverknüpfungen,* positive and negative, the notion of ground, ground of knowledge, reason and consequent. The author opposes *Ursache* to *Grund,* as referring respectively to objective and cognitive relations. While every reality has a cause, not every knowledge has a ground (p. 85). The object of possibility cannot, therefore, be an existential reality, for in the causal order 'possibility' has no meaning. Possibility is a cognitive category: to be possible means nothing else than this, to be motivated as partially factual (p. 92). From this point of view the author then discusses the various types of possibility, and the relation of possibility to negation and impossibility.

It is a clearly written book; the author's exposition is cleancut and forceful; the material is carefully organized. The study, moreover, is another instance of the increasing interest in the problems on the borderland between logic and psychology,—an indication, it is to be hoped, of an approaching *rapprochement* between psychology and theory of knowledge.

Clark University, RADOSLAV A. TSANOFF.

The Genetic Philosophy of Education. By G. E. PARTRIDGE. New York, Sturgis and Walton Co., 1912. 401 pp.

In this volume, Dr. Partridge has epitomized the educational principles contained in the numerous books and articles published by Dr. Hall during the last twenty-five years. To quote from the author, he has "tried to present for students and all those interested in education the main teachings of the genetic school as these are formulated in the writings of its most enthusiastic and strongest representative." To search through the more than three hundred books and articles in which these teachings are contained was no small task; the author has conscientiously searched out the earlier and less accessible publications as well as those of more recent date.

and has faithfully presented his findings. The point of view is philosophical, and it is in the formulation of the principles of genetic philosophy in its educational applications that the author's interest centers. This treatment has the advantage of bringing together in systematic form much that has hitherto been scattered throughout Dr. Hall's writings, though the extensive browsing required to sift out these principles from the original publications well repays the reader, for much of Dr. Hall's best work can not be systematized. Nor is it always easy to determine his exact position on many questions, since Dr. Hall himself furnishes an example of genetic evolution. His interests are many-sided, and his latest views are not always in print. While this should be taken into account in any complete estimate of Dr. Hall's work, it is a difficulty inherent in the subject and not a criticism of Dr. Partridge's method, which naturally confined his estimates to published matter. A minor point of criticism, however, mentioned because of a possible error which might arise from it, is the author's use of the term youth to indicate the years between eight and twelve. Dr. Hall does not systematically use this term to indicate the preadolescent period, but frequently as an equivalent for adolescence as in his book entitled "Youth," which is not, as might be inferred from Chap. VII of Partridge's book, a study of the years from eight to twelve, but an abridgment of his larger work on "Adolescence."

BOOK NOTES

Psychical research. By W. F. Barrett. New York, Henry Holt & Co., 1911. 255 p. (No. 4, Home University Library.)

The author has attempted to compress into a small volume an outline of psychic research, discussing in the successive chapters science and superstitions, unconscious muscular action, autoscopes, the Society for Psychic Research, personality, willing game, thought reading and transference in the normal and hypnotic state, mesmerism, suggestion, telepathy, visual hallucinations, phantasms of the living, dreams, supernormal perception, divining rod, haunts and spooks, automatic writing, spiritism.

Proceedings of the American Society for Psychical Research. New York, published for the Society, 1912. 976 p.

It gives the writer cold shudders to glance over these 976 pages, well printed, indexed, and bringing back James, Wright and others known to him, and to see his own name mentioned sporadically; and he personally cannot resist the anxious and upsetting query whether or not we are living in a sane or inverted world.

Mind and its disorders. By W. H. B. Stoddart. Philadelphia, P. Blakiston's Sons & Co., 1912. 518 p.

This second edition adds two chapters bearing on psychoanalysis and there has also been rearrangement of the portion of the book that brings out the similarity of various toxic psychoses.

Nervous and mental diseases. By Archibald Church and Frederick Peterson. Philadelphia, W. B. Saunders Co., 1911. 932 p. (7th edition, 343 illustrations.)

This seventh edition seems to be revised more than most of the editions have been and it is brought up to date. It is a valuable contribution.

Psychological medicine. By Maurice Craig. Philadelphia, P. Blakiston's Son & Co., 1912. 474 p.

The author follows the rather usual rubrics now, what is insanity, its cause, classification, symptoms, mania, melancholia, stupor, catatonia, paranoia, dementia praecox, secondary dementia, epochal insanities, intoxication, psychosis, general paralysis, exhaustion, psychasthenia, insanity in physical disease, defective development, legal relations, sleeplessness, care taking, freedom.

Elemente der Völkerpsychologie. Grundlinien einer psychologischen Entwicklungsgeschichte der Menschheit. Von Wilhelm Wundt. Leipzig, Alfred Kröner, 1912. 523 p.

It is a very great convenience to students to have the contents of the five ponderous tomes of Wundt condensed under the direction of the author himself into one. The matter is conveniently arranged under the following heads, primitive man, the totemistic age, the age of heroes and gods, the development of humanity.

Psychologie und Wirtschaftsleben. Von HUGO MÜNSTERBERG. Leipzig, J. A. Barth, 1912. 192 p.

After a section on preliminary questions, the second part of this work describes the selection of fit personalities and the third, the way to get the best possible achievements or efficiency. As exchange lecturer in the University of Berlin in the fall of 1910, the author states that he gave here for the first time in any university a comprehensive and sequent treatment of the problems of vocational training or applied psychology. This outline, written afterwards, presents the subject not only to psychologists, but to economists and business men. It will doubtless have a fuller presentation in English.

An introduction to psychology. By T. LOVEDAY and J. A. GREEN. New York, Henry Frowde, 1912. 272 p.

This book aims to be only a very elementary introduction. It is very English, but it is singular that so little note is taken of scientific studies in this field. To our mind, it is entirely inadequate, superficial and indeed, misleading. The author should be reminded that there are important things done and said in psychology that are not in the English language.

Lehrbuch der Psychologie. Von THEODOR ELSENHANS. Tübingen, J. C. B. Mohr, 1912. 434 p.

Why does a professor of philosophy and pedagogy in a technical school who is practically unknown to psychology undertake to write a textbook? The answer is undoubtedly that he deems he has a new mode of approach, as is suggested by the chapter headings: psychology as science, its idea, history, present status, method; the relations of soul and body; functions of the nervous system; processes of psychic life and concepts; feelings and will; capacities of the soul; consciousness, unconsciousness, memory, attention; development of the sense life; the regulation of the psyche, suggestion, hypnosis, insanity; the final questions of our psychology, relation of the soul to time, space, substance, psychic causality, etc.

Psychology; the study of behavior. By WILLIAM McDOUGALL. New York, Henry Holt & Co., 1912. 256 p. (No. 42, Home University Library.)

What is psychology? What is it concerned with? What questions does it answer? How does it set about its task? What are its methods? What progress has it made? Is it in the advanced or beginning stage and above all, what may we hope for it? These are the questions the author seeks to answer and begins by discussing the province of psychology, then consciousness, the structure of the mind, methods and departments, animal, child, individual, abnormal and social psychology. The book is indeed very general and, judging from the brief glance at it, would give only the most superficial kind of orientation to the beginner.

Die Projektionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder. Von LILLIEN J. MARTIN. Leipzig, J. A. Barth, 1912. 231 p.

This very valuable work, which needs very extensive review, treats first of projection methods and describes varied series of experiments, while the second part deals with localization of visual, acoustic and other conceptual images.

Ueber die allgemeinen Beziehungen zwischen Gehirn und Seelenleben. Von TH. ZIEHEN. Leipzig, J. A. Barth, 1912. 72 p.

This is based upon a lecture given in 1901 which now attains its third edition, but is printed with little or no revision.

Zeitschrift für angewandte Psychologie und psychologische Sammelforschung. Hrsg. von WILLIAM STERN and OTTO LIPMAN. Leipzig, J. A. Barth, 1912. Band 6, Heft 5 u. 6.

The two chief articles are Hölzen on Binet-Simon intelligence tests applied to idiots and Bœrthe's critical and experimental study on the same subject.

The chemic problem in nutrition. By JOHN AULDE. Philadelphia, John Aulde, M. D., 1912. 410 p.

The chapter heads are, under disorders of nutrition, a résumé of the physiological data, then come a summary of metabolism, the food problem and dietary studies, the chemic deviations in the vascular system, with the causes of heart failure, diabetes, gout, rheumatism, constitutional maladies, skin diseases, tonsilitis, diseases of the nervous system. To the latter, three chapters are devoted.

Le langage graphique de l'enfant. Par GEORGES ROUMA. Bruxelles, Misch & Thron, 1912. 304 p.

This comprehensive and more or less systematic memoir comprises a systematic story of the whole problem of children's drawing with copious citation of literature. In addition to this, the author has collected many new and typical drawings of children so that we may well point to this just at present as the latest and, as it therefore ought to be, the best comprehensive treatise on the subject. He begins by a study of the methods of approaching the subject, then discusses children's representations of a good man, then how they draw animals, proportions, movements, orientation, perspective, drawing and language, spontaneous drawings in connection with originality, voluntary attention, with certain conclusions.

The world we live in, or philosophy and life in the light of modern thought. By GEORGE STUART FULLERTON. New York, The Macmillan Co., 1912. 293 p.

"How dry" asks the author in his preface "has a man a right to be when writing upon a subject which ought to be of interest to every thoughtful man?" The book is straightforward and simple to those interested in epistemological problems, to which this author has devoted himself with unusual ability and assiduity for most of his life. Among the eighteen chapters, the following titles are suggestive: the problem of everybody's world, the world as idea and its unreality, the world as phenomena and its reality, our world and other worlds, the world of the new realism, the world without and the world within, the new realism in everybody's world, the world as mind stuff, the will, playing with the world and the world in earnest, the world of knowledge and the world of belief.

Die Entstehung des Denkvermögens. Von GEORGES BOHN. Leipzig, Theodore Thomas, 1912. 221 p.

This little volume is an exceedingly valuable and sensible compilation. The title, however, will be to many very misleading, for it really deals with experiments on the very rudimentary forms of life and represents the tropistic mechanical theory.

Youth and the race. By EDGAR JAMES SWIFT. New York, Charles Scribner's Sons, 1912. 342 p.

From a cursory perusal of this book, the writer of this note is inclined to believe that it will prove the best of the author's publications thus far. This is significant for it indicates the growing mind. It is a book which it is impossible to epitomize. He has popularized much of the general knowledge we have of adolescence and given it a very concrete and practical application. The book abounds with illustrations that are apposite. The chapters are the spirit of adventure, the ways of youth, chances to grow, the school and the community, vagaries in the school, fallacies in moral training, the spirit of the gang an educational asset, the release of mental forces. Perhaps it is impossible to expect in a book of this size a complete representation of the theme and some of the chapters seen to be more independent essays than others, but the book has a unity of its own.

Sociology in its psychological aspects. By CHARLES A. ELLWOOD. New York, D. Appleton & Co., 1912. 417 p.

This work discusses various conceptions of society and sociology, the subject matter and problems of the latter, its relations to other sciences, to philosophy, methods, biological basis, origin of society, fundamental facts, social coordination, self control, rôle of instinct, feeling, intellect, social forces, imitation, sympathy, social consciousness and will, forms of association, the theory of social order and progress and finally, the nature of society.

The family in its sociological aspects. By JAMES QUAYLE DEALEY. Boston, Houghton, Mifflin & Co., 1912. 137 p.

The author treats the family as a social institution, in early education, patriarchal family, rise of the modern family, its relations to religion, how influenced by the Reformation and the state, the American democratic family, urban conditions, marriage and divorce, the family undergoing a reorganization.

Der Alptraum. Von ERNEST JONES. Leipzig, Franz Deuticke, 1912. 149 p. (Translated by E. H. Sachs.)

The chapters treat of (1) dream and faith, (2) Alpdream or nightmare, (3) incubus and incubation, (4) the vampire, (5) Wehrwolf, (6) belief in the devil and finally the witch epidemic.

Psychoanalysis und Ethik. Von KARL FÜRTMULLER. München, Ernst Reinhardt, 1912. 34 p.

This is one of the publications of the association for free psychoanalytic investigation which has just published also A. Adler's "Ueber den nervösen Character." It has long been the writer's impression that the Freudian analysis might be extended not only to hunger as well as to love, but to ethical and even to religious life. Here we have the first serious attempt in this direction.

Die Entwickelung des Naturgefühls. Von R. HENNIG. Leipzig, J. A. Barth, 1912. 160 p. (Schriften der Gesell. f. psy. Forschung. Heft 17, iv Sammlung.)

The development of feeling for nature is characterized in successive chapters in the Middle Ages and in modern times down to Rousseau and later, with great stress upon Goethe and Saussure. Another chapter is devoted to the age of romantic feeling for nature, while the supplementary chapter on the essence of inspiration, which is about one-third of the book, comes last.

La simulation du merveilleux. Par P. SAINTYVES. Paris, Ernest Flammarion, 1912. 387 p. (Introduction par P. Janet.)

This comprehensive monograph is divided into three parts. The first treats of simulated maladies in general, their frequency, their simulation by mendicants and exploitation in piety, simulation by neurotics, hystericals, especially mythomania and pathomania. The second part is devoted to the simulation by those who are reputed supernatural and here he treats of supercharia, mythomania, spirits and apparitions, the performances of pseudomediums and occultism, false demoniacs, diabolical mythomania, impostors, fasting, ecstasy, Saturnalian revels, subconsciousness and the maladies of personality. The third part treats of simulation in the field of miraculous recoveries, impulsive frauds, miracles, with considerable attention given to one or two special cases. The work finally concludes with a retrospective diagnosis of the subject.

The influence of caffein on mental and motor efficiency. By H. L. HOLLINGWORTH. Columbia University Contributions to Philosophy and Psychology, vol. XX, No. 4. Science Press, New York (Archives of Philosophy, 22), 1912, 166 p.

This research was financed by the Coca Cola Company, but the author insists that it is scientific all the same. His general conclusion is that caffein, if taken moderately, stimulates and does not bring about any reaction. This coincides pretty nearly with Rivers' statement that "it increases the capacity for both muscular and mental work without there being any evidence from moderate doses of reaction leading to diminished capacity for work." This result is quite in contrast with the secondary reactions said to follow strychnine.

Reaction to multiple stimuli. By JOHN WELHOFF TODD. Science Press, New York, 1912. 65 p. (Archives of Philosophy, No. 25, August, 1912.)

The sections are as follows: the production of the stimulus reaction of the simultaneous stimuli; simultaneous stimuli after correction for the sound distance and also for latent periods of the sound hammer and the induction coil; ability to react to a designated group of suggested stimuli; reactions to stimuli of low or medium intensity with graded intervals, reinforcement and inhibition.

Die psychische Vererbung. Von U. Josefovici. Leipzig, Wilhelm Engelmann, 1912. 155 p.

After discussing the various standpoints in his introduction, the author distributes his space and effort rather evenly between the following three subjects: (1) biological facts and theories, (2) psychological facts and theories, (3) basal considerations and foundations. His conclusion is in favor of the continuity of psychic processes and of psychic life and that this principle is the basis of whatever truth there may be in psychic inheritance. Thus, the principle of continuity of psychic processes in connection with the principle of psychophysic parallelism can best be explained on the basis of psychic inheritance.

Münchener philosophische Abhandlungen. Theodor Lipps zu seinem sechzigsten Geburtstag gewidmet. Von Aster und Anderen. Leipzig, J. A. Barth, 1911. 316 p.

Ten of Lipps' admirers here join in contributing articles commemorative of his sixtieth birthday. They have all been pupils of his. There is a great variety of topics from Kantianism, Hegelism, the basis of moral life, perception and conception, development of space, ideas, aesthetics, to the consciousness of feeling, motive and motivation, theory of negative judgment, of existence as a determination of objects and the significance of Freud for psychology.

The classical psychologists. By Benjamin Rand. Boston, Houghton, Mifflin & Co., 1912. 734 p.

This is a companion volume to the author's "Classical Moralists" in the field of ethics and his "Modern Philosophers" in the domain of philosophy. It presents by a series of selections some of the most essential features of psychological doctrine from Anaxagoras to Wundt. It aims to be a sort of history of psychology based upon translated extracts from sources. It is interesting to the general reader and will unquestionably be of service to students. No less than 43 writers are included, William James being the only American.

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INTROSPECTION IN DEMENTIA PRECOX

By Edwin G. Boring, Cornell University

Introspection is generally acknowledged to be, if not the most important, at least one of the fundamental methods of psychology. The psychopathologist, however, in his study of the abnormal mind, has availed himself very little of the introspective method; and, in those cases in which he has done so, his use of the method has been so nearly unconscious and so correspondingly uncritical, that the results have been of very much less psychological value than those results which a refined application might have given. It is true that the psychiatrist depends largely, in a mental diagnosis, upon the verbal statement of the subject about his experiences; and the psychopathologist in a mental test, such as a comparison of lifted weights, also depends upon the verbal statement of the subject as an expression of mental facts; but such statements cannot be called 'introspection' in the proper sense of the word. They indicate facts about mind, but they do not, as a rule, directly describe mind. The material is information[1] and it is usually information uncritically accepted. As such it may prove adequate for a symptomatology, but it can hardly in itself lead to an accurate psychology.

The use of introspection in a study of association in the insane has been championed by Ley and Menzerath,[2] who write:

[1] The term, 'information,' is used throughout this paper in the meaning of *Kundgabe, i.e.,* statements about mind, but not directly describing mind.

[2] *L'Étude expérimentale de l'association des idées dans les maladies mentales,* 1911, 32.

Les premiers auteurs qui s'occupèrent de l'étude expérimentale des associations ont cru pouvoir s'en tenir en côté purement objectif; ils comparient tout simplement les réactions aux mots inducteurs et en recherchaient les liens logiques.......Ces interprétations *logiques* devaient necessairment aboutir à un échec, puisqu'elles négligeaient le côté le plus fondamental du phénomène, c'est-à-dire, le côté subjectif, *psychologique*. C'est à Alfred Binet que revient l'honneur d'avoir rehabilité le facteur subjectif en psychologie expérimentale, en y introduisant l'*introspection*..........

Nous avons personellement constaté, et nos protocoles donnent de nombreux exemples à ce sujet, que bon nombre d'associations resteraient tout à fait inexplicables, si nous n'avions, pour éclairer leur genèse, l'introspection du sujet.

These writers report experiments performed with nine classes of insanity,[3] in all of which they obtained introspections from the subjects. The introspections consist, however, of the statement of the meaning of the reaction word, of its connection with the stimulus word, or of some such simple explanation. At best the investigators have done no more than to substitute a logical interpretation by the subject for a logical interpretation by the experimenter. They have made no attempt to induce the subject to describe consciousness. He merely indicates meanings, and in doing so he is not introspecting (if we limit the use of the term introspection to a direct description of the contents of consciousness) but giving information.

Franz has recognized the availability of the introspective method in psychological work with the insane. In speaking of the difficulty of this sort of work, he writes:

The question has often been asked me whether or not the insane are more difficult to work with than normal people. I believe both to be equally easy and equally difficult subjects. Many insane do not introspect well, but few normal people do so. Some insane are more introspective than the average normal individual,—and from some no introspections can be obtained. It is not necessary to work with the patients who introspect badly or not at all; there is a possibility of a selection of subjects just as there is in normal psychological work. In general I think there is no inherent difficulty in investigating the mental conditions of the insane.[4]

The trend of psychopathology does not seem, however, to have led toward an employment of the introspective method. The writer is not aware of any paper, other than the one mentioned, in which introspections have been systematically

[3] (1) Dementia precox, (2) manic-depressive psychoses, (3) toxic excitement, (4) neurasthenia and psychasthenia, (5) hysteria, (6) nerve traumatism, (7) paranoia, (8) general paralysis, and (9) sleeping sickness.

[4] *Psychological Opportunity in Psychiatry, Jour. Philos. Psych. and Sci. Meth*.. 3, 1906. 567.

recorded. This state of affairs is probably due to a lack of trust, on the part of the investigator, in the reliability of the statements of the insane; and such scepticism is not unwarranted. In introspective studies with normal subjects, we are inclined to conjure with the term "trained observer," until the novice in the science might infer that all psychological facts are based on the experience of a few, specially educated individuals; yet even with the best observers the reliability of the method has been questioned,[5] and recent studies have sought more clearly to define the nature of introspection and more exactly to determine its limitations.[6] It is no wonder, when the validity of the report of a normal observer with psychological knowledge and perhaps years of introspective training is questioned, that there is little thought of applying the method to untrained observers of abnormal mentality. There is, however, an error in the sceptical position, an error which is both logical and empirical. The fact that reports of complex experiences by trained persons may sometimes be unreliable does not mean that the reports of simple experiences by untrained persons may not be reliable; and no rule is completely established until it is found operative in experience. Accordingly, it is proposed in this paper to examine critically the actual introspective reports made by subjects in one form of insanity,—dementia precox,—and to determine, as far as possible, the reliability of these reports.

The introspections which will be considered are those given for the course of consciousness during the learning of a pencil-maze by eight subjects with dementia precox. The writer has already published the protocols in a recent study,[7] where they are printed fully in order that the reader may determine for himself what degree of reliability he will accord them. The writer was unable at the time of that publication to discuss the introspections critically, but promised that such a discussion should be forthcoming, as soon as additional experimental

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[5] *Cf.*, Dodge, R., *The Theory and Limitations of Introspection, Am. Jour. Psych.*, 23, 1912, 214 ff.

[6] Anschütz, G., *Ueber die Methoden der Psychologie, Arch. f. d. ges. Psych.*, 20, 1911, 414 ff.

Titchener, E. B., *Prolegomena to a Study of Introspection, Am. Jour. Psych.*, 23, 1912, 427 ff.; *The Schema of Introspection, ibid.*, 485 ff.

[7] *Learning in Dementia precox, Psych. Rev. Monog. Series, Vol. 15, No. 2*, 1913. After this article was in the hands of the printers, it was found that an unavoidable delay in the publishing of the monograph would necessitate the appearance of this supplementary paper earlier than the appearance of the study upon which it is based. The latter should, however, be available shortly.

work in the way of control series had been performed. The present paper aims to fulfill that promise.

The Criteria of Reliability

We must here inquire what the criteria of the reliability of a description of consciousness are. We must establish our critical method before we can proceed to the criticism.

In the first place, we may assume that the object of a descriptive psychology is to establish definite, permanent, concise descriptions of consciousness,—the facts of psychology,—which may be based upon the verbal introspective reports of trained observers, upon information of untrained observers, upon experimental graphic records, or upon casual observation, either introspective or informational,—as given verbally by a present observer, as recorded in literature, or as indicated by such products of mind as art, language, and customs. We shall concern ourselves here with such descriptions as are based upon verbal reports of observers,—thus including trained introspection and information, between which it is not easy, at present, to draw a sharp line of demarcation.

The availability of these bases for a description of consciousness depends upon the ease and reliability with which they can be interpreted accurately as describing consciousness. Even in the case of introspection by the trained observer, which is in itself a direct description of consciousness, a certain amount of interpretation must take place; for, in order that the introspection may become a part of a systematic, descriptive psychology, the psychologist must select from the protocols, and bring the verbal expression of the observer into accord with the terminology of the science. A description, then, as a unit of a systematic psychology, is always liable to error arising from a misinterpretation of the meaning of the observer, as expressed in his report.

A more frequent source of error arises from the fact that the report of an observer, in the meaning intended by him, may still not describe the actual experience exactly as it occurred. Even observers with the best training may report factors that were not actually present in the experience, and it is one of the commonplaces of the psychological laboratory that no report ever enumerates all of the factors present in the consciousness described.

In accordance with these two possible sources of error,—the intelligibility and the accuracy of the report,—we shall divide our method of criticism. The rubrics which we shall name may not be logically exhaustive. They emphasize, however, those factors which are important as affecting reports by untrained observers; and in such form they should be approximately exhaustive in fact for the cases considered,—at least as far as can be determined at present.

I. Intelligibility of Report

The adequacy of a verbal report as an accurate description of the consciousness described depends upon the intelligibility of the report. The psychologist must interpret the language of the observer, and only to such extent as he attaches to the language of the report the meaning that was intended by the observer will the final conclusion represent the facts. We may distinguish three modes of interpretation.

1. *Empathic interpretation.*—An empathic understanding is perhaps always involved in the rapid comprehension of language. Bühler has insisted upon its importance in the interpretation of the introspections of trained observers, and it can not be denied that an experimenter can the better understand the language of an observer the better he is acquainted with him. Empathy is most important in the interpretation of the information of untrained observers. To just what extent an empathic translation of information into psychological terminology is reliable, it is not easy to say. Within certain limits, however, it seems safe to place dependence upon it. In psychology we find some grossly descriptive terms, which have been introduced into the science in order to classify certain sorts of very common experiences, and other more refined terms, which have come into use only as the result of the analyses of the laboratory. "Kinesthesis," for example, is a general term for "feelings of movement," a class of experiences separately distinguished by all individuals; the division of kinesthesis into muscular, articular, and tendinous sensations is a purely introspective difference that is not made in everyday life. There is little doubt that the experimenter may safely restate, empathically, in gross descriptive terms, what comes to him as information; for, if the observer reports, "I felt my hand moving," it is obvious that he means "arm or hand kinesthesis," or if he reports, "I see it plainly before me in my mind," it is obvious that he means "visual imagery;" but it is not possible, with safety, to press the classification to the application of those terms which are descriptive of experiences not ordinarily recognized as distinct. One can not say precisely what sensations composed the "arm kinesthesis," nor record the tint and chroma of the visual imagery. Empathic interpretation is, therefore, trustworthy

only when it substitutes for implications of consciousness such gross descriptive terms as were introduced by psychologists in order to designate the kind of consciousness implied.

2. *Interpretation by reference to a standard consciousness.*—When the typical consciousness for certain conditions is known, it may be used as a standard consciousness, in terms of which information, given under the same conditions, may be interpreted. Factors appearing in the same general manner both in the informational statements and in this standard average of introspections may with reasonable assurance be identified. It might appear, at first, as if the mere identification of certain factors of the informational statements with the corresponding factors of the standard consciousness could result in no additional knowledge beyond that already contained in the standard,—the method being thus reliable, but useless. Such a conclusion, however, is not correct; for, once the implication of the verbal expression is established by reference to the standard, the variations from the standard of the individual consciousnesses implied by the information can be roughly determined. If, for example, in the course of a consciousness in which strong visceral sensations ordinarily appear, the observer reports, "I feel sort of stretched inside," the psychologist is justified in interpreting this same phrase in other connections to signify visceral sensations.

3. *Interpretations of a standardized terminology.*—If the observer always reports in the terms of a standard psychological vocabulary, the danger of misinterpretation is reduced to a minimum, provided always that the terms are rigidly defined and that the observer is trained to understand the meaning of his terms as thoroughly as do his interpreters. Even under these conditions, however, an accurate conclusion is not always as easy as it appears. What we actually mean in psychology by the definition of a descriptive term is that we select some definite set of conditions and state that the experience or a certain part of the experience occurring under those conditions shall be characterized by the term in question. The selection may or may not be conscious; the conditions may or may not be experimental. We may say that we shall call the "sensation of cold" the characteristic experience that we have upon first touching ice, and we shall have adequately defined the term. If we say, on the other hand, that the feeling which we get from our joints shall be called "articular pressure," we have not sufficiently defined the quality, for we refer various sensations to the joints. We may, however, in this case state

the conditions of Goldscheider's experiment with the string and weight, and thus rigidly fix the term. The experience with the ice is common enough; that with the string and weight ordinarily needs to be reproduced in the laboratory. Both experiences may, at first, be consciously referred to as standards; the reference, however, soon becomes unconscious. It is exactly in terms of these simple standard experiences that the trained observer actually reports. His criteria of judgment may have been established in childhood, in more recent casual experience, in a psychological laboratory course, or later in the laboratory; but, unless the criteria are known, the interpretation of his reports is liable to error. Too often the supposedly trained observer remains untrained in the meaning of some particular term which he uses. We say that he "lacks introspective familiarity" with the particular processes concerned; yet it is not that the processes are unfamiliar, but rather that he has not been furnished with the criteria necessary for a classification of these processes in accordance with a psychological terminology.

On the one hand, then, the trained observer is liable to be misinterpreted in some point for which his criterion of classification is not the same as that of his interpreter; on the other hand, however, the observer, who is in general untrained, may easily be trained to report upon such distinctions if he is familiarized with standard experiences in terms of which he is later to report. Many a trained observer, so called, fails to distinguish between the experiences of warmth and heat; yet an observer, otherwise untrained, if given the two experiences with the names attached, readily learns to make the distinction. It is not safe, then, in interpreting a report to assume that, because most of the terms are unequivocal, therefore all must be; or that, because most of the statements are informational, therefore no real introspection can be expected.

The normal procedure in obtaining a description of a given consciousness may be taken to be that which we have just indicated: the observer is furnished with terminological criteria; he is then made clearly conscious of the experience to be described; he then describes the experience by reference (conscious or unconscious) to the criteria furnished. The order may, however, be reversed. After the observer has experienced and reported by reference to whatever criteria he is accustomed to use, he may then be furnished with other criteria and may be asked to state whether or not it was upon such bases that he made his judgments. This procedure is known as the method of *confrontation,* and consists in nothing more than an effort to determine, by questioning the observer after a report is made, just what was meant by the terms of the report.

II. Accuracy of Report

The report of an observer, in the meaning intended by him, may still not accurately represent the consciousness described. Inaccuracies in the account may be either those of omission or those of commission; that is to say, the observer may omit in his report factors that were actually present in the consciousness described; or he may include factors that were not present; or he may combine the two in incorrectly describing a complex factor. Inaccuracies of either class may arise from intentional misrepresentation, unintentional error [suggestibility], or from the making of irrelevant statements for relevant [scatterbrainedness, retardation, and perseveration].

1. *Intentional misrepresentation* may occur with unwilling subjects, with some insane, and, less frequently, with any observer. Simulation is common in some insanities, especially in hysteria. The Ganser symptom (if the incorrect statements which constitute it may be regarded as sometimes intentional) would also furnish instances under this rubric. Intentional misrepresentation is likely to occur with any subject under emotional conditions, as, for example, when the pride of the observer is involved. The novice in the laboratory may misrepresent when he feels that his report may reflect his ignorance, although such misstatements are generally unintentional. The usual critical methods may be employed as safeguards against intentional misrepresentation. Four criteria, which are applicable, follow.

a. Knowledge of the *general tendencies* of the observer.

b. Internal consistency of the report. Misstatement may betray itself as well in the protocol as on the witness stand.

c. Consistency of the description with regard to an average description of consciousness under the same conditions.[8] If the account of an unknown observer does not tally with the accounts of trained observers or of a large number of other observers who are consistent with one another, the reliability of his report may be questioned.

d. Consistency with regard to other facts of psychology. In psychology, as in any other science, reported facts that accord with the established facts of the science are more readily

[8] This criterion should be distinguished from the criterion for the interpretation of a report by reference to a standard consciousness [I, 2, above]. Here, if the reported pattern of the consciousness in question does not correspond with the pattern of the standard consciousness, the reliability of the report is questioned. In the previous case, the reported pattern corresponded to the pattern of the standard, and an unknown factor was interpreted by reference to the corresponding factor in the standard.

accepted than facts in disagreement with the general body of knowledge.

2. *Irrelevant statements* are often included in descriptive reports by some insane. There may be a general retardation of comprehension, so that a patient may not report upon one experience until he has had a second; or the patient may be unable to keep his attention fixed for a long time upon the topic of the report and thus, with a shift of attention, readily changes to an irrelevant topic. The extreme cases are nonsensical enough to be easily detected; but in other cases the irrelevant material is scarcely in itself distinguishable from the ieal description, by the side of which it may occur. Cases of material relevant to preceding consciousnesses, but not relevant to the immediate consciousness described, are common in reports from normal as well as from abnormal observers. There is a 'perseverative' tendency to report each consciousness in a series as of the same form as the last consciousness, so that the report of a change may lag behind the actual occurrence of the change. Remnants of preceding description may thus last over into subsequent descriptions after they have become irrelevant. Other cases of irrelevancy may arise where, for one reason or another, there has not been an acceptance of the *Aufgabe* to report. The degree of irrelevancy may be indicated by the four criteria already mentioned for intentional misrepresentation [*a, b, c, d*] and by another in addition [*e*].

e. Alteration of report to correspond with an alteration of experimental conditions or of questioning forms a very convenient check when irrelevant statements are suspected. The change in experimental conditions or in questioning amounts merely to the insertion of control experiments or of control questions. In extreme instances of perseveration or of retardation in the insane the meaning of the questions may be entirely altered, while the subject does not materially change the form of his reply.

3. *Unintentional error* occurs principally as the result of suggestion. In some cases of insanity, however, it may take the form of the unintentional misstatements of Ganser's symptom. Unintentional simulation is frequent enough to be regarded as a symptom of psychopathic inferiority. In both normal and abnormal cases, the error may be the direct result of some factor inherent in the mechanism of introspection, that is to say, in the way in which the descriptive report comes to be given. All the checks mentioned above [*a, b, c, d, e*] apply in this case as well as some others [*f, g*].

f. Spontaneity of report is regarded as one of the most important assurances that there is little inaccuracy due to suggestion. Reliability of report is indicated by spontaneity (1) when the information in the report is volunteered, (2) when the report is, at times, in the form of an answer that negates the implication of a question, or (3) when the verbal expression is not stereotyped.

g. Introspective report on the mechanism of description,—or at least an indication of the mechanism. If a description of the reporting consciousness is available, it will be possible to state whether the description reported attaches immediately to the processes described, or whether it attaches to representations of the processes, or whether, perhaps, it occurs automatically. When the description attaches to representative processes, inaccuracy may result from the failure of the surrogates to correspond exactly with the consciousness for which they stand. In the representation, some factors may be filled out, others may be but partially represented, others not represented at all. It may be that some of these incompletenesses and over-completenesses are intrinsic to the substitution of processes of one kind for those of another. Of the reliability of a report automatically given, even after a length of time, of the reliability of subjective certainty on the part of the observer in reporting, and of many other factors which enter into the process of description and affect the accuracy of the report, there is as yet very little known. An urgent need exists for experimental work in the investigation of the mechanism of the reporting consciousness.[9]

Experimental Series

I. Series with Cases of Dementia Precox

The reports studied are those given by eight subjects, diagnosed as cases of dementia precox, in a series of learning experiments, carried out with two pencil-mazes [M and N]. The work was performed in the psychological laboratory of the *Government Hospital for the Insane* in the summer of 1912. It is described and the reports of the patients are fully given elsewhere.[10]

II. Series with Trained Adults

It is obvious from the previous discussion that the critical examination of the introspections of abnormal subjects requires the establishment of a set of norms with which they

[9] A study of this problem is at present projected in the Cornell Laboratory.

[10] *Op. cit.*

may be compared. For this purpose, three observers, trained in introspection, were required to learn maze N, under the same conditions (as nearly as they could be duplicated) as the cases of dementia precox. The experiments were performed in the Cornell Laboratory in the fall of 1912 with the maze used for the patients. The observers were Dr. Day (*D*), graduate in psychology, Mr. Ruckmich (*R*), instructor in psychology, and Mr. Foster (*F*), research assistant in psychology. *D* and *F* were especially well trained in introspection. The observers were instructed to report "all processes relevant to the selection of the route." As a matter of fact they reported also upon other matter relevant to the total situation. It is not possible, in this article, to give the introspections even in partial detail. Summaries of the reports follow.

Observer D. 15 trials on 2 successive days. Maximal av. time, 1st day, 39.0 secs. Minimal av. time, 2nd day, 10.8 secs. Maximal single trial, exclusive of first trial, 1st day, 43 secs. Minimal single trial, 2nd day, 7 secs.

At the very beginning of learning the only prominent factors in consciousness were visual imagery and eye-kinesthesis, the visual imagery being of parts of the path, sometimes referring to the immediate position of *D's* hand and sometimes being anticipatory. She oriented herself with reference to the center or to the edge of the maze by visual imagery. Reference to the outside was also carried for her by kinesthesis in the two arms (the observers were allowed to place the left hand at the outer edge of the maze while running the course with the right hand).

As early as the third trial, kinesthesis began to become more prominent and to displace visual imagery. Arm-kinesthesis, sometimes meaning the position of the hand and sometimes meaning the direction of the course, became frequent. Later, visual imagery and eye-kinesthesis occurred only at difficult parts, often furnishing then the cue to the course. At the end of the first day, *D* reported that the center was traversed entirely in kinesthetic terms, and that beyond the center the course ran smoothly, a visual image carrying the familiarity, until interrupted by a difficulty. After that the course was taken up by "an attitudinal cue, involving both kinesthesis and visual imagery," and continued smoothly to the end, with unclear kinesthetic cues only.

On the second day, visual imagery and eye-kinesthesis soon disappeared, the directive cues remaining in terms of arm-kinesthesis. Later the kinesthesis began to become "fused together into long swings," and continued until the whole consciousness had become a single kinesthetic "swing," with fleeting attitudes occurring during its course. Organic sensations of respiration carried the *Aufgabe* to get out quickly. *D* thought that this stage was immediately preliminary to automatism.

Observer F. 38 trials on 4 days. Maximal av. time, 1st day, 204.6 secs. Minimal av. time, 4th day, 5.1 secs. Maximal single trial, 2nd day, 465 secs. Minimal single trial, 4th day, 4 secs.

At first *F* found very few "directive factors," most of the turns being made "without anticipatory imagery" and visual images having, "for the most part, only a general directive influence." Consciousness was principally composed of visual imagery and of kinesthetic and tactual sensations, referring to parts of the maze. Later the visual imagery occurred only at difficult points, notably at one point where the same mistake was made repeatedly; the kinesthetic sensations were more prominent, and the kinesthetic images acted as directive factors, that is to say, they were anticipatory to parts of the course. Visual imagery was also sometimes anticipatory.

By the end of the second day parts of the maze had become fused into "unitary, anticipatory, visual-kinesthetic complexes," with a meaning that *F* can indicate only by drawing a crooked line that traces approximately a bit of the course. On the third day, however, he reported, for the first time, that the whole course was run "within one conscious present." In it there was nothing but a fusion of kinesthetic and tactual sensations from arms, finger, and neck, with strains from the abdomen, which carried the *Aufgabe* (cf. *D*'s organic sensations of respiration). On the fourth day, the experience took definitely the attitudinal form. It was an unitary fusion of kinesthetic and tactual sensations, meaning the hand and the pencil; strain images from the two arms and the hip, meaning effort to get the hands together (a position which occurs near the finish of the course); kinesthesis from eye-movement, meaning the position of the hand (*F* thought that his eyes followed his hand); and a background of strains from the abdomen and chest, carrying the *Aufgabe* to hurry through. Visual imagery sometimes occurred with this attitude, but seems to have had very little directive significance, and was more often lacking or very scant.

At this stage *F* found the foreperiod important. In it he lived through in hasty imaginal form the whole experience of the course in more detail than he actually experienced it later. He described it as "the whole maze in a nutshell." It constituted a preparatory set for him, and, when its course was interfered with, the smooth attitude of the subsequent period was broken up.

Observer R. 38 trials on 4 days. Maximal av. time, 1st day, 35.9 secs. Minimal av. time, 4th day, 6.0 secs. Maximal single trial, exclusive of first trial, 1st day, 51 secs. Minimal single trial, 4th day, 4 secs.

The reports of *R* are more meager than those of *D* and *F* and it is more difficult to generalize from them. On the first day, there were no processes reported relevant to guidance. The procedure seemed to be largely one of trial and error. On the second day, kinesthesis was once reported as directive, and, on the third day, kinesthesis and visual imagery were each reported once as directive. A great deal of visual imagery and kinesthetic sensation occurred regularly throughout the course, but *R* could not say whether they helped him or confused him in the running. The kinesthesis often meant, not guidance, but perplexity, and was sometimes reported as meaningless. Once auditory sensations from the movement of the pencil were reported as meaning the situation and also as anticipatory to the end.

On the final day *R* continued to report visual imagery and kinesthesis, irrelevant to guidance within the maze. He stated that he found no cues at all, that consciousness seemed "to run on a lower level of attention," and that there was a "feeling." which he could not describe, that he was "perfectly independent of the movement," that he could "perform a complicated problem while running the course." From this state of affairs, he inferred that the movement had become automatic.

The reports of *D* and *F* are remarkably alike. Consciousness at the beginning of the series is very complex and broken up, visual imagery and kinesthesis being prominent. As the maze is learned, these processes assume an anticipatory function, apparently determining the course. The visual imagery gives place to kinesthesis, but recurs frequently in the most difficult portions of the route. The kinesthesis fuses together, at first for small unitary parts of the course, and later for the whole course, forming in the final stage a unitary complex, of attitudinal form, of which the core is kinesthesis, but in which visual imagery may still appear. The report of *R* is very similar as regards conscious content, although it differs in the meaning ascribed to the processes. His account of the "automatic" consciousness accords fairly well with the description of the "attitudinal" complex by *D* and *F*.

The accounts by the three observers agree with the introspections of the observers in the large "human maze," used for the study of consciousness in maze-learning by Day and the writer.[11] Here visual and verbal processes were replaced by kinesthetic, meaning the direction of turning, and the kinesthesis later lapsed into automatism.[12] The "automatism" is like that "inferred" by *R*, that is to say, it is a unitary consciousness at a low level of attention in which there is not a separate reference to the individual parts of the maze. The experience was described by one observer in the large maze as a "careening through," a phrase similar to *D*'s and *F*'s "kinesthetic swings." The support of the special introspections by those obtained with the large maze[13] makes perfectly safe the acceptance of the average account, based on the former, as a standard of comparison for the accounts of the subjects with dementia precox.

III. Series with Untrained Boys

In the study of the introspections of a class of insane patients it is natural to ask to what extent the character of the reports is due to the abnormality of the subjects and to what extent it is due to their lack of training. The question can be answered, if we establish standards of comparison with normal subjects of the same degree of training as the patients. The cases of dementia precox considered in this article were all

[11] This work is as yet unpublished. A preliminary account appears under *The Use of the Maze in Comparative Psychology, Psych. Bull.*, 9, 1912, 60.

[12] It may be thought that Day's familiarity with the typical maze-consciousness influenced her report in the learning of the pencil-maze. That this was not the case is indicated by the fact that, in her introspections on the pencil-maze, she does not report "automatism," as might have been expected, but describes a conscious attitude as a state new to her in this experience.

[13] In this work there were fourteen observers, some of whom were especially well trained in introspection. The latter included Professor Bentley and Drs. Geissler and Jacobson.

uneducated men with very poor vocabularies (with the possible exception of G). Naturally they knew nothing whatever of a psychological terminology. It was thought that their general education and command of words were about those of a twelve year old boy. Accordingly, for purposes of comparison, two boys were employed in the Cornell Laboratory to learn the maze and to give reports upon the experience of learning. One boy, *X*, was eleven years old, very alert and active, and easily interested in the work. The casual observer would characterize him as "bright." The other boy, *Y*, was thirteen years of age, slow in both movement and comprehension, and apparently less intelligent than *X*. The experimental series were conducted exactly as for the insane patients, and the questioning was of the same nature. As space does not permit a detailed account, we content ourselves with summaries of the reports.

Observer X. 98 trials on 10 successive days. Maximal av. time, 1st day, 167.9 secs. Minimal av. time, 5th day, 5.0 secs. Average time on last day, 6.4 secs. Maximal single trial, exclusive of first trial, 2nd day, 56 secs. Minimal single trial, 5th, 6th, and 10th days, 3 secs.

In the first three days, *X* frequently reported "seeing the maze in my mind," and later he said that it was as if it were "painted" or "carved" in his mind. He insisted that it was "seeing it in my mind" that enabled him to find the way, although it was suggested that other things might help. He also reported, "I have a feeling in my pencil the way it goes," but insisted that this feeling did not help him. We may presume, then, that he experienced kinesthetic sensations, meaning the maze, or the situation, or the position of his hand, and visual imagery, meaning the way to get out.

On the fourth day he reported: "I just feel the motion of my hand.....My pencil runs like a street-car on a track;" "my hand goes right through as if there were little wheels on it;" but he also said: "It just seems as if somebody were holding that same maze before my eyes, as if I were doing it without a curtain. Funny." He appeared to have set himself the *Aufgabe* to visualize; he was very eager to reduce his time and constantly wondered "how it looked." When asked later, however, if he could "do it without seeing it," he replied, "I'm quite sure I could. I guess I could get the motion of my hand as easy as I could see it in my head." His procedure on making a mistake suggested that kinesthesis was the guiding cue, for he would pause after a rapid correct movement, move back and forth hesitatingly, and then finally shoot off rapidly in the right direction, very much as the pianist, when playing from memory, stumbles through a difficult passage and then catches the swing of the movement again.

On the seventh day, he found that he could no longer increase his speed, and consequently lost his keen interest in the problem. With interest the *Aufgabe* to visualize seemed to lapse, for only very infrequently did he now report "seeing the maze." Often he insisted that he did not "see the maze," and he stated time after time, "Felt the motion of my hand. That's all." His behavior had become typically motor, and frequently, like the piano player, when checked in a difficult place, he would get past by going back and taking a "running start," which would carry him by.

On the eighth day, he once reported that he "thought of nothing" while going through, and, on the tenth day, we find this condition frequent. Only when he "got caught" did he "feel the motion of his hand." Apparently this stage corresponds to the "automatic" or "attitudinal" consciousness, reported by the trained observers.

Observer Y. 100 trials on 9 successive days. Maximal av. time, 1st day, 41.7 secs. Minimal av. time, 8th day, 7.0 secs. Av. time on last day, 7.6 secs. Maximal single trial, 2nd day, 109 secs. Minimal single trial, 8th day, 6 secs.

At first *Y* was unable to comprehend the meaning of the questions asked. His reports suggested verbal imagery, but were very indefinite. On the second day, however, he noted an orienting cue in the fact that his hand "felt different" when it was near the edge. Later he said, "When my two hands get near together, I can kind of feel it in them,"—the kinesthetic cue mentioned by *D* and *F*.

At the end of the third day, *Y* suddenly reduced his time in a trial in which he changed his whole bodily attitude, so that, instead of gazing off vacantly to one side, he fixated the curtain steadily. He reported: "Usually I just kind of look off and go through; but this time I looked hard at the curtain and tried right hard." Later he said: "There is a kind of picture of the maze,—part of it. I can't tell all the parts of it;" "I can't see the color of the board and all that. I can just kind of see the path part of it." Thus the presence of visual imagery seems assured.

Of the parts for which he reports no visual imagery, he said: "I can just tell by the way my hand goes;" "one place I can't see the maze, but I know from the way my hand goes that I'm getting near the place where I get out." The form of expression in this case, as well as in the others quoted, was unsuggested to *Y*, although resembling closely the words of *X* and of the dements.

On the last three days, *Y* continued to indicate both visual imagery and kinesthesis, although he is given during this time 42 of the 100 trials, in order to allow the habit to become entirely motor, if possible. He evidently found the work monotonous, for his answers appeared stupid and his phraseology stereotyped and parrot-like. He worked uninterestedly, gazing off to one side. He was unable to draw the route, although he reported that he "could see it in his mind." His indefinite fixation and his inability to draw suggest that visual imagery was perhaps less and kinesthesis more important than his indefinite reports would indicate.

Critique of the Reports

Let us now proceed to a critical examination of the introspective reports made by the eight cases of dementia precox. As we cannot reprint them here, we shall refer to them as explicitly as possible.

It would not be fair to seek for evidence of reliability or of unreliability in the reports of all the subjects taken in a mass. Altogether enough positive or negative instances might be found to establish their reliability or to overthrow it, while the proof might not be at all conclusive in any one of the individual cases. We must begin by measuring the report of each subject by reference to the criteria we have already established, and then see what our net result has been. We shall make the separate surveys as brief as possible, referring in each case to five factors of reliability,—a modification of our original schema, which we shall discuss later.

Subject A.

Intelligibility. A uses such phrases as "I can tell by the touch of the pencil," or "I get out by just following the pencil" for kinethesis, if the empathic interpretation be accepted. "I just came to have the right feeling for the passage; I can't explain it," coming at the end of a series, may be taken to stand for the conscious (kinesthetic) attitude reported by the normal observers.

Misrepresentation. No tendency toward dishonesty was noted. The internal consistency is good, there being only one contradiction of statement, and that one influenced by suggestion. The report—as much of it as is interpretable—is consistent with the average consciousness.

Irrelevancy. No tendency to talk "off the point" was ever observed. The report is consistent. For these reasons there was no attempt to vary conditions in order to note the effect on the report.

Suggestibility. A was ordinarily very suggestible. He showed a tendency to answer all possible questions by "Yes, sir" or "No, sir," according to their implication. He did not "resist suggestion" in the Binet test with lines. On the other hand, when the questions required considerable thought before answering, he would as frequently negate the implication of the question as agree with it. Once, in answer to suggestive questions, he apparently contradicted himself. At all other times the report is consistent with itself and with the average course of consciousness. In the latter connection, the indefinite verbal expressions coming at a time when the attitudinal consciousness might be expected, are worthy of note. A here reports: "I just came to have the right feeling for the passage;" "I just kept feeling for the doors, but I can't explain how it was;" "I don't know how to explain it. It was just the feeling for the gates and thinking how it must be,—from practice." The phrases already noted illustrate the spontaneity of expression. None of these phrases, apparently, was suggested.

Completeness. A was not at all communicative and had to be constantly urged to reply to questions fully. There is no internal evidence that his reports were not complete, but comparison with the detailed protocols of the trained observers shows them to be very meager. A does not indicate spontaneously that he is giving an account of the entire experience.

Subject B.

Intelligibility. B used the terms "general opinion" and "general imagination" very indefinitely, but later identified them as "like looking at a picture in the dark, when you can about half see it" and as "something like looking at a map." Later he denied having a "general opinion of it," and stated that he "just knew how to go," that he "had it in his mind," and that "somehow when I get in the passages it just seems as if I knew how to go." In terms of the average consciousness, these expressions must indicate kinesthesis, the last, perhaps, the semi-automatic movement.

Misrepresentation. B was inclined not to talk much, except about his troubles, and sometimes resented being plied with questions. Such answers as he gave, however, appeared to be sincere. He was not inconsistent with himself, and his sudden change of report from "general opinion" to "just knowing how to go" corresponds well with the change from visual to kinesthetic cues in the average consciousness.

Irrelevancy. While B frequently broke into his report to talk about his troubles, the line of demarcation between the two topics was always too distinct to permit the confusion of material. His report is internally consistent. A change of report, interpreted as a change from visual to kinesthetic cues, occurs in connection with the change in behavior to the typically kinesthetic form, indicating that the old form of expression did not last over after the experience it described had ceased,—that is to say, B did not make the "perseverative" error.

Suggestibility. B would stick to a point in spite of mild suggestive questions to the contrary. His report is consistent with itself and with the average consciousness. His expressions, as a rule, are not original, although they sometimes pass beyond triteness. Note, for example, the phrases quoted above for visual imagery.

Completeness. B was verbose about his troubles only. His replies to questions were very meager. Compared with the average account, he indicated only the three main phases of the course of consciousness.

Subject C.

Intelligibility. With maze M, C reports doubtfully, "I sort of see what it looks like," adding at one time, "Of course, I don't see it; I imagine it." He is, however, uncertain, and his report is not very intelligible. With maze N, however, he notes, "Seems like you just keep your pencil going like," and later, "I just sort of feel with my hand where the next door is going to be,"—phrases which one may take to mean kinesthesis. Such an interpretation is justified by his later usage: "Only when my hand stops going, then I think of it [the maze]. It doesn't pay to stop......The stopping mixes me up;" "sometimes, when I go wrong, I think more about it;" "I don't think about anything" (while going through),—a positive assertion. These three expressions evidently refer to the smooth course of the habituated kinesthetic consciousness, the heightening of consciousness when the course is interrupted, and the final, low-level, attitudinal stage, respectively.

Misrepresentation. C was very sensitive and confiding, and appeared to try genuinely hard to do everything that he was asked to do. There were no gross inconsistencies in his reports, and his accounts, as indicated above, tallied in many respects with the average account.

Irrelevancy. C always made it possible to distinguish between relevant and irrelevant remarks, although he showed a tendency to introduce the latter. When the form of questioning was changed, he always responded to the altered meaning. His reports were, on the whole, consistent.

Suggestibility. He showed a tendency to agree with the experimenter in conversation, although this tendency did not appear very prominently in the reports. Once he contradicted himself, apparently as the result of suggestive questioning, and then, observing his mistake, modified it to make it partially consistent. In the first paragraph above, we have already partially noted to what extent his account tallies with the normal. There should also be mentioned his observation of the fact that the first part of the course was oriented with reference to the center,—a fact brought out by the normal observers. The quotations already given show a fair degree of spontaneity of expression.

Completeness. C, although bashful, talked freely when his confidence was gained, and gave thus a much fuller report than the two subjects already considered. He noted, as has been seen, a fair number of points observed by the trained observers.

Subject D.

Intelligibility. D was very indefinite. It was not easy to say what he meant by "knowing how to go" and by "putting his mind on it," or whether he had any visual imagery. His "kind of indefinite feeling" of how to go may be taken to be kinesthesis,— emphatically interpreted; and his later report, from comparison with the reports of the trained observers, is certainly kinesthesis: "That maze, there's a kind of give to it,—a kind of give where the openings are. I remember the way it gives, the way it feels when you push against the openings." Once he states that he got out "by the sound of the pencil....There was a sort of indefinite rhythm about it. It played a tune,"—a description much like that of *R*.

Misrepresentation. D showed no tendency toward intentional misrepresentation, his inconsistencies being more readily explained upon another basis.

Irrelevancy. He exhibited a very baffling tendency to introduce irrelevant material into his answers without any warning. Many parts of his reports were discarded, because it was not possible to determine whether he was answering the questions or talking nonsense. The following excerpt from the notes of the experiment will illustrate the point: "Q. How did you remember how to get out? A. By the feel. Q. What do you mean by the feel? A. The way my hands feel and the way my eyes feel. Q. Do you often have a feeling in your eyes? A. Yes, they go round and round, like snakes. Q. How do you mean? A. Like snakes in a tobacco bag. I think there's gun-powder in them." And so on. At first it appears as if D were reporting a kinesthetic cue. Next one suspects eye-kinesthesis. And then it developes that he is talking without reference to the questions, although it is not possible to say just when the nonsense began and the sense ended. This tendency was very disconcerting, and makes one always uncertain of the value of his reports. It was necessary constantly to change the sense of the questions, in order to see if his answer changed accordingly. He did not, however, fall into this trap very frequently. The full record of D's work (not the published summary) contains many inconsistencies, quite evidently due to his persistence in bringing in irrelevant material.

Suggestibility. D was slightly negativistic, but the trait was not evident in his answers. There was no evidence of positive suggestibility. As much of his report as is safely interpretable is consistent, and accords with the standard account. He shows very little originality of expression except in his account of the "give" to the maze, quoted above.

Completeness. D was frequently subject to spells of mutism, and, except on one or two occasions, never talked freely. His reports, compared with the standard, must be judged very incomplete.

Subject E.

Intelligibility. E was able to explain his terms fairly explicitly. He distinguished between "memory," which is a visual image of the whole maze ("the looks of the maze with the wrong ways to go in it too"), "forming it in the mind," which is the visual imagery of the part immediately concerned ("the way it looks to go right;" without "the wrong ways to go in it"), and the "feel," which is kinesthesis. He described the "feel" and the "looks" quite thoroughly, so that there is little doubt of the empathic interpretation. This fact is particularly striking, because his consciousness did not follow the average course, but remained persistently visual. If E's account had not been especially clear, it would not have been possible to determine this fact with certainty.

Misrepresentation. E coöperated heartily in the work throughout and gave every evidence of sincerity. His report is full and internally consistent, although it departs to some extent from the average course of consciousness.

Irrelevancy. E's answers were always pertinent. He showed no tendency to talk nonsense. At one time, he reported, first, visual cues, then a conscious decision to rely upon motor guides, and finally a return to visual guidance, in successive reports, so close together, that there would seem to be little 'perseverative' tendency for the expression in one report to lag over into the next.

Suggestibility. No tendency toward suggestibility was manifest. The reports were internally consistent. The inconsistency with the average consciousness has already been mentioned. E's descriptions are, however, so explicit, for an untrained subject, that they can be taken as forming a clear exception to the normal course. Moreover, it is not to be said that E would not have formed a purely kinesthetic habit had the series been prolonged. His persistent visual imagery, at any rate, may have been due to his keen interest in the task and his effort to reduce his time,—an occurrence that happened in the case of observer *X* (*cf.* p. 158). The spontaneity of his replies is especially marked. Many of them, including those quoted in the first paragraph above, were volunteered, and not given in response to questioning. Besides the points noted, he reports, without suggestive questioning, kinesthetic cues for limiting arm movement ("I felt I was moving my hand too far"), anticipatory kinesthesis on passing through a door, and an anticipation of the movement in the foreperiod ("you have to get the movements formed in your mind before you start out;" *cf.* the foreperiod of the normal observer, *F,* "the whole maze in a nutshell," p. 156).

Completeness. E was free and very talkative. The explicitness of his reports has already been indicated.

Subject F.

Intelligibility. F spoke of "feeling the way" and later, when he was lost, of "going back for a fresh start." His procedure was typically motor, that is to say, he got rapidly over a part of the course, stumbled about, picked up his cue, and then went rapidly over the next part. Sometimes he would go back a little way and get a running start, which would take him past the difficult place. As this performance is a condition for kinesthesis. we may interpret his expression accordingly. There was no hint of visual imagery.

Misrepresentation. F ordinarily seemed to be truthful and sincere. His report is scarcely explicit enough to be internally inconsistent. It does indicate the form of consciousness that would be expected from the behavior.

Irrelevancy. F was, in general, careless, both in manner and in form of expression. He showed a slight tendency to answer questions irrelevantly, but, although he shifted easily to disconnected topics, there was generally a change in attitude, that protected his interpreter from error. When the form of the questions was changed, he was always sensible to the difference.

Suggestibility. He did not appear especially suggestible in ordinary conversation. He did not, on the other hand, exhibit very much originality of expression. He did observe the necessity of going rapidly and not "thinking much" about the task, facts which support his statement that he "goes by the feel;" and at the end he noted that "it takes just one whole draw" to get out,—the kinesthetic attitude of the normal observers *D* and *F*.

Completeness. F talked incessantly, but did not keep his attention readily upon the maze problem. His lack of interest in the introspective side of the problem partially accounts for the lack of detail in the report.

Subject G.

Intelligibility. G insisted: "I see it in my mind,"—visual imagery. His references were made explicit by describing the turns "seen," as a fire-place with a mantel, as a street, as a casaban effect on a curtain, and so forth. Later he reported being guided in part by the "sense of feel" or "touch," which he opposed to the "sense of thought," the latter possibly meaning visual imagery or perhaps merely a heightened consciousness. He said: "It brings the two senses together, the sense of feel and the sense of thought.......There are a number of places where I'm uncertain; then the thought comes in. The sense of feel is when I'm all right." In terms of the average consciousness, then, the "sense of thought" is a heightened consciousness and probably a visual one.

Misrepresentation. G was seemingly perfectly honest, and his reports are consistent with themselves and with the average course throughout.

Irrelevancy. Very occasionally G may be suspected of having introduced irrelevant material into his answers, but the proof is not positive. He always replied pertinently to questions. His reports were uniformly consistent.

Suggestibility. G was not evidently suggestible. His reports showed unusual originality of expression. The comparisons quoted in the paragraph above and many others of like nature were made spontaneously.

Completeness. G was a fluent and persistent talker. If he could have devoted all his remarks to a description of conscious processes his reports would have been full indeed. As it is, they give only the general outline of the conscious course.

Subject H. (Series cut short; less practice than other subjects.)

Intelligibility. H reports, "I follow the pencil," "I go with the pencil," "I just feel my hand go through,"—expressions which may be emphatically interpreted as kinesthesis. Nothing else in the protocols is positive. The indefinite indications of visual imagery are possibly the result of suggestive questioning.

Misrepresentation. H appeared to be honest about all matters except his delusions. His report is too meager to give opportunity for measurement on the basis of internal consistency, although there is one contradiction, which seems to be the result of suggestion. As far as it goes, it is in accord with the normal course.

Irrelevancy. There is no especial reason to suspect irrelevant material in the reports.

Suggestibility. H appeared in ordinary conversation to be very suggestible, that is to say, he was very sensitive to the meaning of everything that was said to him. Once, when asked if he "saw the maze in imagination," he replied that he did, but his later reports throw doubt on the presence of visual imagery. Still the proof is not conclusive, At no other time does he contradict himself. His expression shows no more originality than is indicated by the quotations already made.

Completeness. He was taciturn about everything except his troubles, and toward the end of the series resented being asked too many questions. At best he indicated only the general character of his consciousness.

It is now our task to bring together the loose ends of our criticism, to generalize so far as generalization is permissible. We can best do this by means of a table, in which rough scores show approximately the various degrees of reliability indicated by each criterion. Nothing but the crudest sort of quantification is intended, and even that represents nothing more than the personal estimate of the writer.

The captions of the table, it will be noticed, do not correspond exactly with those given in the discussion of the criteria of reliability above. The change is a simplification, resulting from the omission of the possible criteria that could not be determined, or were not applicable, in the present cases. The criterion of "consistency with regard to other facts of psychology" is omitted throughout, because it applies to generalizations of a larger scope than any undertaken in the present study. The "mechanism of description" as a criterion of reliability is not considered, because no accounts of the means of reporting were taken with the insane patients,[14] and because too little is known of the relation of the mechanism of reporting to the accuracy of the report. With this factor omitted, the only sort of unintentional errors that concern us we may group under the general head of "suggestibility." The degree of "completeness" should properly be indicated by all the criteria listed under "intentional misrepresentation," "irrelevancy," and "unintentional error," for the errors of omission are coördinate with those of commission; but for all practical purposes in very crude reports, such as we have to consider, completeness is indicated only by the general tendency of the subject to talk freely of his experiences and by a comparison with the standard consciousness. The latter is by far the more important.

The method of scoring adopted in the table is to indicate by "1" the very greatest, and by "5" the very least, degree of reliability. The intermediate degrees are represented by "2," "3," and "4," thus throwing all measures of reliability into a rough five-fold classification. An average (in italics) is shown for each of the main factors. It is not an arithmetical mean, but an estimate based upon all the times available. It might be regarded as a weighted mean. It is obvious that one criterion, strongly indicating unreliability, may readily outweigh others, which fail to indicate unreliability, for the criterion may be selective for different types of error, and a single one may imply a high degree of unreliability of a certain kind. When a criterion is obviously to be disregarded in favor of other measures, the score is placed in parentheses. In such a case, the score bracketed is generally based on much more meager evidence than are the other scores.

We have also scored in the table, for purposes of comparison, the reports of the untrained boys and of the trained observers. Lack of space forbids a separate discussion of these reports, but a review of the summaries given and a comparison with the protocols of the patients should convince the reader that they are approximately correct.

The bold-faced type shows the averages for the different classes of observers.

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[14] It is not impossible that some indication of the reporting procedure might be got from untrained observers. An attempt to bring out information on this point with the boy *X*, indicated that his reported description in general attached immediately to imaginal revivals of the processes in the original consciousness, or at least to representative processes from the same sense-department. When asked, for example, how he was sure that he had both seen and felt his way, he replied, without further suggestion, "Because I still see it, only it's fainter; and I feel it still, only I don't feel it very much." On the other hand, *Y*, after being given a special series with a view to determining his mode of reporting, was unable to make any satisfactory replies at all. It may be that trained observers differ in the ease with which they assume the introspective attitude toward certain experiences. Both *D* and *F*, who, however, had had training along these lines before, found no difficulty in describing the reporting consciousness; on the other hand, the description was difficult for *R*, who found that the double *Aufgabe*, to report and to report on the reporting, was distracting.

INDICATED RELIABILITY OF REPORTS

Under each factor of reliability are placed the criteria which indicate reliability for that factor. Indicated reliabilities for each criterion are scored from 1 to 5, 1 being the greatest, and 5 the least, reliability. The average score for each factor is given in italics. It is not the arithmetical mean, but an estimated classification based on the relative values of the different criteria. Numbers in parentheses signify that the criterion so marked is for other reasons discounted as an indicator of the reliability of that particular factor.

Factors of Reliability	As indicated by:	Observers															
		Dementia Precox									Untrained Boys			Trained Adults			
		A	B	C	D	E	F	G	H	Av.	*X*	*Y*	Av.	*D*	*F*	*R*	Av.
INTELLIGIBILITY																	
	Empathic interpretation	3	4	3	5	1	3	2	3		2	3		1	1	1	
	Reference to a standard consciousness	3	2	2	5	2	3	3	4		2	3		1	1	1	
	Standardized terminology	—	—	—	—	—	—	—	—		—	—		1	1	1	
	Average intelligibility:	*3*	*3*	*3*	*5*	*2*	*3*	*2*	*3*	3.0	*2*	*3*	2.5	*1*	*1*	*1*	1.0
ACCURACY																	
MISREPRESENTATION																	
	General tendencies (truthfulness) of observer	1	1	1	1	1	1	1	1		1	1		1	1	1	
	Internal consistency of report	2	1	1	?	1	?	1	?		1	1		1	1	1	
	Consistency with regard to an average description	1	1	1	?	(3)	2	1	2		1	1		1	1	1	
	Average misrepresentation:	*1*	*1*	*1*	*1*	*1*	*1*	*1*	*1*	1.0	*1*	*1*	1.0	*1*	*1*	*1*	1.0
IRRELEVANCY																	
	General tendencies ('perseverative', retardative, *etc.*)	1	2	1	5	1	2	2	1		1	2		1	1	1	
	Internal consistency of report	1	1	1	4	1	?	1	?		1	1		1	1	1	
	Consistency with regard to an average description	1	1	1	3	(3)	2	1	2		1	1		1	1	1	
	Change of report with a change of conditions	?	1	1	4	1	1	1	1		?	1		—	—	—	
	Average relevancy:	*1*	*1*	*1*	*4*	*1*	*2*	*1*	*1*	1.5	*1*	*1*	1.0	*1*	*1*	*1*	1.0
SUGGESTIBILITY																	
	General suggestibility of observer	3	1	2	1	1	1	1	2		1	2		1	1	1	
	Internal consistency of report	2	1	2	?	1	2	1	?		1	1		1	1	1	
	Consistency with regard to an average description	1	1	1	3?	(3)	2	1	2		1	1		1	1	1	
	Change of report with a change of conditions	?	—	—	?	1	—	—	—		—	—		—	—	—	
	Spontaneity of report	3	3	3	5	1	4	1	4		3	3		1	1	1	
	Average suggestibility:	*2*	*2*	*2*	*4*	*1*	*3*	*1*	*2*	2.1	*2*	*2*	2.0	*1*	*1*	*1*	1.0
COMPLETENESS																	
	General tendencies of observer	5	5	4	5	(1)	4	(1)	4		4	5		1	1	1	
	Consistency with regard to an average description	5	5	4	5	3	4	4	5		3	4		1	1	2	
	Average completeness:	*5*	*5*	*4*	*5*	*3*	*4*	*4*	*5*	4.4	*3*	*4*	3.5	*1*	*1*	*2*	1.3

Referring to the table and recalling the citations which we have made from the protocols, we find, as we should naturally expect, that the reports of the dements suffer most in incompleteness. There is a considerable difference between subject E, who coöperates heartily and gives a clear, explicit account of all that he can recall, and subject D, whose answers are very indefinite and fragmentary; but even the report of E is much less detailed than those of the trained introspectors. The other patients, besides E, give, as a rule, only the general character of the more prominent processes. It is impossible to get from them accounts of the more subtle means of orientation, of the exact course of conscious events while the maze is being run, or even of the exact character of the experiences which they do indicate or mention. One has only to read the introspections of the trained observers to realize how schematic is the account furnished, even after much patient questioning, by these untrained subjects. On the other hand, one should never lose sight of the fact that something of attested reliability is obtained, and that, for some purposes, that something is all that is required.

Next to completeness the reports are most deficient in respect to intelligibility. It is hard to interpret them with the explicitness that is often desired. We tend empathically to interpret such phrases as "I see it in my mind" or "I just felt as if my hand were being moved" as visual imagery or kinesthesis. If the phrases are varied or supplemented by others of like nature, we become more positive. If they appear in accordance with an already established course of consciousness, we are reasonably assured of their meaning. On expressions of this sort the entire interpretation in the cases considered has had to be based. The dements had, of course, no command of a standardized terminology, and there was no time to train them in the meaning of any particular terms, although there is no apparent reason why they could not have been so trained.

The chief source of error in the interpretable items reported is that which we have called suggestibility. Three subjects showed a general tendency to reply according to the implication of the question. Three indicated suggestibility by conflicting statements in the reports. All, except E and G, more or less lacked the spontaneity of expression, which may be taken to indicate freedom from suggestion. D and F were especially poor in this respect.

There was some tendency—a tendency which might be expected in cases of dementia precox—to make statements, while reporting, entirely irrelevant to the questions asked or to the whole situation. Frequently the statements have a bearing on the delusional troubles of the patient, but this is not always the case. The confusion results, doubtless, from the inability of the subject to keep his attention concentrated upon a single extraneous topic for any considerable period,—a deficiency characteristic of dementia precox. With some subjects, however, the shift of attention is so marked, by a change in bodily attitude, that there is no danger of confusing the pertinent with the irrelevant material. With others, who talk freely, the meaning of the remarks is sufficient to protect the experimenter from error. With a subject, however, who answers only in monosyllables or short phrases, as did subject D, the shifts of attention are not obvious, as the instance quoted above clearly shows (p. 162), and the danger of error is large. D is the only subject in whom this tendency to report irrelevant material throws serious doubt on the reliability of the protocols.

No evidence at all of intentional misrepresentation was found in the insane subjects tested. An investigator might not, of course, always be so fortunate. The patients were not always truthful when in the wards. The experimenter, however, made an especial effort to secure their confidence and to treat them sympathetically, and it may be that his attitude determined their behavior.

The writer was struck, when performing the experiments with the untrained boys, with a great similarity between the reports of the boys and the reports of the cases of dementia precox. The type of questions required, the form of the answers, the phraseology, the amount of material reported, all seemed to be very much alike in the two cases. We now see that according to our system of scoring, the indicated reliability of the records of the dements is only a little less than that of the normal boys. The patients were considerably less complete in their reports than the boys; but they were only slightly less intelligible. They were somewhat more given to irrelevant statement, but they were practically no more suggestible. All observers, normal and abnormal, were scored in the highest class for honesty. The patients, in fact, rank so little below the boys, that individual patients rank higher than the average for the boys. Both E and G average, on the whole, higher than the average for the boys, E not ranking below them in a single item. C is just equal in reliability to *Y*, the poorer of the two boys. If subject D had been excluded, the record of the patients would have approximately equalled that of the boys in everything but completeness. D is responsible for almost all the deficiency indicated under the rubric "Irrelevancy," a deficiency which is, however, undoubtedly an immediate result of mental disorder. The differences in intelligibility and in completeness of reports between the dements and the untrained normal subjects seem to be nothing more than one which results from a poorer command of verbal expression by the patients, coupled with an inability to observe, or at least to describe, details,—a condition that we might find in any untrained observer with a poor command of language.

Conclusions

1. Persons with dementia precox can, under experimental conditions and without prolonged special training, give reports indicating the general trend of consciousness.

2. The phraseology of the reports is simple and naïve, and its interpretation depends upon an empathic understanding of certain forms of expression or upon a knowledge of the facts of the average consciousness under the conditions of the experiment.

3. The reports are very incomplete as compared with the reports of trained normal observers, but they indicate reliably the general character of the contents of consciousness, and may in some cases include implications of special and less prominent processes.

4. The dements are more suggestible than trained observers.

5. There were no indications of intentional misrepresentation in the reports of the patients experimented upon.

6. Some of the reports are rendered unreliable by the tendency of the subject to make no distinction, apparent to the experimenter, between statements that are relevant and statements that are irrelevant to the situation or to the questions asked. This confusion may arise from the state of constantly shifting attention, characteristic of dementia precox, which arises when a subject is required to attend to a situation foreign to his usual course of ideas.

7. There is great individual variation between the dements in the factors indicating the reliability of the reports.

8. The reports are, on the whole, of about that degree of reliability that is found in reports made by untrained observers with little education and a poor command of language, and appear to differ from these reports in no characteristic way other than in the introduction of irrelevant material.

EXPERIMENTS ON COLOR SATURATION[1]

By L. R. Geissler

The following experiments are a part of a more elaborate study of color saturation which has been interrupted for the present, but may be resumed later. Nevertheless, the results thus far obtained are presented in their unfinished form, since they may be of use to others interested in the problems of color-vision.

The only experimental work on color saturation which the author has found in the psychological literature dates back to the sixties of the past century. In 1865 Aubert published[2] some measurements on the liminal sensitivity to color saturation. He determined the smallest sector of color that would appear as a just noticeably colored ring on a rotating white disc and found it to be 2 to 3 degrees, while on black and gray discs even smaller sectors were recognized. He also made some experiments on the differential limen of color sensitivity; and he found that on a black background the stimulus-increment for orange must be 0.95 per cent, for blue 1.54 per cent, and for red 1.67 per cent, in order to produce a just noticeable increase in saturation. Aubert used the Masson-Maxwell disc, and observed under ordinary daylight illumination. It is not stated whether he employed any observer besides himself; but in a later summary of his results[3] he remarks that they were confirmed by J. J. Müller,[4] who does not give any account of his experiments, and by M. Woinow.[5]

Our problem was to determine whether the number and sizes of colored stimulus-increments corresponding to just noticeable saturation differences would lend themselves to a measure of saturation. We began our attack upon this problem from the two extremes,—by gradually reducing a maximally saturated pigment-color, and by introducing more

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[1] From the Physical Laboratory of the National Electric Lamp Association, Cleveland, Ohio.

[2] H. Aubert, *Physiologie der Netzhaut,* Breslau, 1865, 138-150.

[3] H. Aubert, *Grundzüge der physiologischen Optik,* Leipzig, 1876, 531-532.

[4] J. J. Müller, Zur Theorie der Farben, *Graefe's Archiv f. Ophthalmologie,* xv., *Abth.* 2, 1869, 243.

[5] M. Woinow, Zur Frage über die Intensität der Farben-Empfindung, *Graefe's Archiv f. Ophthalmologie,* xvi., 1870, 251-264, esp. p. 256.

and more color into a colorless stimulus. Both groups of experiments were performed under an artificial daylight illumination of constant intensity and constant spectral composition. The rotating double color-disc was mounted on a Lummer-Brodhun color-mixer with peripheral attachment and viewed against a perfectly neutral grey background. The stimulus was exposed for three seconds at a time and the interval between two exposures was always made long enough for after-images to disappear. Our colored stimuli were Zimmermann papers of the following hues: red (d), yellow (h), green (l), and blue (n), mixed with Zimmermann greys of corresponding degrees of brightness. The colors were equated with the greys by the method of flicker-photometry, the voltage readings of a Weston electric speedometer being taken as an indirect indicator of the speed of the color-mixer. The grey which at lowest voltage, and hence at slowest speed, mixed with a color without flicker, as determined by several observers, was regarded as being of the same brightness as the color. The equations thus found agreed in the main with the introspectively estimated equations made by a number of observers well trained in photometric work, as the following Table shows, in which the small letters indicate the greys as taken from the Zimmermann set of thirty-five greys.

Equation of Color and Greys

Obs.	*Yellow*	*Green*	*Red*	*Blue*
	Natural Daylight			
A	a-b	e	k	r
B	b	f	m	p
C	b	d	n	q
	Artificial Daylight			
A	a-b	e-f	h	s
B	c	d	l	p
C	b	e	m	q
	Flicker-Photometry			
	b	d	m	r

Since grey *a* is the lightest in the series, almost a white, the order of our colors according to brightness is from brightest to darkest: yellow, green, red, and blue. The difference between yellow and green is the smallest; that between green and red the largest; and that between red and blue is intermediate. The use of the peripheral attachment of the Lummer-Brodhun color-mixer makes it possible to compare a colored ring on the outside, which is kept constant, with a colored disc on the inside, which can be varied during rotation.

In our first group of experiments we worked with red only. Two of the four observers were more or less trained in psychological experiments, namely, Miss Wilma Ball (B), of Cleveland, who kindly volunteered her services in the interest of psychology, and the writer (G). The other two were Messrs. G. Cadish (C) and L. Krill (K), both employed in the Laboratory and familiar with photometric work and observations.

Employing the 'method of limits' we began with maximal saturation, that is, 360 degrees of red, both outside and inside, and gradually added small amounts of grey (m) to the inside, until it looked just barely less saturated than the outside. Reversing the procedure, the inside was made definitely less saturated, and then step by step more red was added until it looked like the outside. At least six such pairs of series were taken, and in half of them the judgments 'more saturated,' 'less saturated,' 'equal,' or 'doubtful' referred to the outside as compared to the inside, while in the other half the reverse was the case. The average amount of grey thus found to be necessary in order to make the inside look just less saturated than the outside was then introduced in the outside, so that now the inside had to be changed still more, before it again seemed to be just less saturated. Then the outside was again reduced by the corresponding amount of grey; and similar series were taken to determine the next step. This procedure was continued until the outside had been reduced from 360 degrees color to about 300 degrees color *plus* about 60 degrees grey. It was intended to continue the method for red down to the color-limen, and then to repeat it with several other colors, at least at certain stages between 360 degrees and 0 degrees; but unavoidable external circumstances prevented this. It was possible to carry on the experiments for red with two observers, B and G, only in the regions of 240-222 degrees and 120-110 degrees of red *plus* the corresponding amount of grey. With C and K a few unsystematic observations were made at the same places, giving similar results.

The detailed results are presented in the following Table, in which the stimulus-increments are given for each observer. The figures in the odd columns indicate the size of the red sector in the outside ring at the stage corresponding to the preceding and succeeding steps. Observer C, for example, distinguished 360° of red in the outside from 350° red *plus*

10° grey in the inside, and the latter amount of saturation in the outside from 346.5° red *plus* 13.5° grey; or in other words, the third degree of saturation was produced by adding 3.5° of grey to the previous amount of grey. In calculating the Av. and MV. for C and K the last two approximate values were included.

B		C		G		K	
360.0°		360.0°		360.0°		360.0°	
	13.0		10.0		10.0		10.0
	9.0				6.5		8.0
	7.0	350.0°	3.5		7.5		5.0
	6.0		3.5		7.0		5.0
			5.0		5.5		4.5
325.5°	4.0		4.0				6.5
	3.5		3.0	323.5°	3.5		5.0
	4.5		4.5		3.0		
	4.0		3.5		3.5	316.0°	
	5.0		3.0		3.0		
			4.0		4.0		
302.0°			4.5		4.0		
		311.5°		302.5°			
240.0°		240.0°		240.0°		240.0°	
	3.0				3.5		
	4.5	about	4.0		5.0	about	4.5
	5.0				3.0		
	4.0				3.0		
223.5°				225.5°			
120.0°		120.0°		120.0°		120.0°	
	3.5				2.5		
	3.0	about	3.5		3.5	about	4.0
	4.0				3.5		
109.5°				110.5°			
Average and Mean Variation of last 12 steps 4.0 ± 0.5		Average and Mean Variation of last 12 steps 3.85 ± 0.5		Average and Mean Variation of last 13 steps 3.5 ± 0.42		Average and Mean Variation of last 7 steps 4.9 ± 0.51	
0.0°	1.25	0.0°	0.81	0.0°	1.56	0.0°	1.31

The Table shows at a glance that, so far as this particular pigment red is concerned, the stimulus-increments corresponding to just noticeable saturation-differences are approximately constant at such different stages as 325° red *plus* 35° grey, 230° red *plus* 130° grey, and 110° red *plus* 250° grey. It seems fair to assume that the increment-values would have remained constant at the intervening stages, and perhaps also at a stage not far removed from the absolute color-limen, which is given in the last horizontal row of the Table. If this result is verified by later experiments, it will allow us to estimate the approximate number of least perceptible differences in the saturation of our pigment-red, and to say that it is close to one hundred. We have a few systematic data for observer K on green mixed with grey *d*, which is thus a good deal lighter and, at the same time, less saturated than red. It is significant that the first four stimulus-increments required to reduce green by smallest possible changes in saturation are considerably higher than those for red, namely, 17, 14, 9, and 12, as compared with the corresponding values for red, which for K were 10, 8, 5, and 5. The difference seems to indicate that the original saturation and brightness of the color measured have some very definite influence upon the size of the just noticeable saturation-differences. If these results could be confirmed and extended to other colors, they would simplify the problem of measuring saturation far beyond our expectation; but at present we must refrain from basing any speculative conclusions upon them.

Our second group of experiments was performed under the same external conditions and by the same method. In addition to red, the colors yellow, green, and blue were used; and besides binocular vision we also made determinations for each eye separately. There were altogether nine observers: C, G, and K of the previous group, Miss M. Majerus (M), Messrs. C. F. Lorenz, Ph.D. (Lo), M. Luckiesh (Lu), A. G. Worthing, Ph.D. (W), of the Physical Laboratory, and Mrs. L. R. Geissler (SG) and Mr. F. Aeberli, M. D. (A), of Cleveland, O.[6] The observers thus represent various degrees of practice, while their age ranges between eighteen and thirty-eight. It was thought advisable to test a fairly large number of observers, in order to eliminate individual peculiarities of color-vision, and also to get a comparison of the sexes. Our results seem to justify the precaution. The monocular data

[6] Several other observers began the experiments, but for various reasons could not continue. The writer takes this opportunity to thank each and all of them for their willing co-operation.

were obtained by placing a ground glass before the unused eye.

The detailed results are presented in the following Table. Each individual value for both eyes, for the right eye alone, and for the left eye alone is the resultant of eight series of determinations, while the figures of the rows headed 'totals' are the averages of the values for both eyes, and the right and left eye. The arrows indicate the direction from smaller to larger differences of the totals, and are inserted to facilitate the grouping of the observers. If we consider first the combined results of all observers,—in other words, the averages of the totals,—we find that the smallest increment necessary to distinguish between color and no color is required for our red, varying between 0.68° and 4.06° and averaging 2.23° ± 0.85°. Then follow blue, yellow, and green and this order agrees with that according to saturation, red being the most and green the least saturated of our colors. Since our green requires a limen three times as great as that of red, it seems reasonable to assume that its saturation is only one-third as great as that of our red, and, similarly, that the saturation of our yellow is about one-half of that of our blue, that of blue about five-sixths of red, and green five-sixths of yellow. These figures agree approximately with a number of estimates of saturation made by some of our observers as well as by outsiders, even to the point of individual differences; for two of our observers, who estimated the blue to be more saturated than the red, also gave a smaller limen for blue than for red. We do not wish to lay emphasis upon these quantitative relations of color saturation, because a larger number of colors should be investigated before any general conclusions can be derived from the experiments; but we believe that our method will lead to a reliable measure of saturation.

We may now consider the results of the nine individual observers. At first glance there seems to be little agreement among them; but a closer study shows that eight give a higher limen for yellow than for blue (although one of the eight shows an unappreciable difference only), seven have a higher limen for green than for yellow, and six give a smaller limen for red than for blue. The average difference between red and blue is 1.51°, between yellow and green 2.15°, and between blue and yellow 2.90°, showing that, on the whole, red and blue were more nearly equal in saturation than yellow and green, and especially than blue and yellow. The individual deviations from the general average are smallest in the case

TABLE OF ABSOLUTE LIMINAL VALUES FOR COLOR-SATURATION

Obs.	Vision	Red	Blue	Yellow	Green
C	Both	.81	1.25	1.31	1.69
	Right	1.00	1.19	1.37	1.87
	Left	.87	1.24	1.31	1.66
	Total	.90 →	1.23 →	1.33 →	1.74
			.33	.10	.41
K	Both	1.31	5.28	6.62	7.37
	Right	2.06	6.50	6.87	7.88
	Left	1.81	5.63	7.62	7.63
	Total	1.73 →	5.80 →	7.04 →	7.63
			4.07	1.24	.59
Lu	Both	.62	3.00	5.87	10.25
	Right	.62	3.00	6.25	12.12
	Left	.81	3.50	5.87	12.56
	Total	.68 →	3.17 →	6.00 →	11.64
			2.49	2.83	5.64
Lo	Both	1.18	2.78	8.12	8.00
	Right	1.56	2.18	9.62	9.48
	Left	1.75	2.56	10.75	9.12
	Total	1.50 →	2.50 →	9.50 ←	8.87
			1.00	7.00	.63
M	Both	3.25	4.62	9.25	6.25
	Right	4.50	4.00	10.00	7.12
	Left	4.43	4.25	11.00	8.32
	Total	4.06 →	4.29 →	10.08 ←	7.25
			.23	5.79	2.83
G	Both	1.56	4.12	3.93	7.37
	Right	2.12	4.78	3.81	7.12
	Left	2.56	5.02	5.09	8.12
	Total	2.08 →	4.64 ←	4.28 →	7.54
			1.39	.36	3.26
W	Both	2.16	1.56	3.09	5.37
	Right	3.19	1.37	2.59	5.75
	Left	2.87	1.12	3.12	5.63
	Total	2.74 ←	1.35 →	2.93 →	5.58
			1.39	1.58	2.65
SG	Both	2.81	2.12	6.62	6.87
	Right	3.56	1.69	5.25	6.75
	Left	2.75	1.93	6.12	6.63
	Total	3.04 ←	1.91 →	6.00 →	6.75
			1.13	4.09	.75
A	Both	2.27	2.12	4.62	7.66
	Right	2.43	2.00	5.12	7.33
	Left	2.62	2.03	5.62	8.12
	Total	2.44 ←	2.05 →	5.12 →	7.70
			.39	3.07	2.58
Av. of all Obs.	Both	1.77 ±.75	2.98 ±1.24	5.99 ±1.88	6.76 ±1.55
	Right	2.36 ±.85	2.97 ±1.42	5.65 ±2.14	7.27 ±1.72
	Left	2.27 ±.86	3.03 ±1.39	6.28 ±2.34	7.54 ±1.93
	Total	2.23 ±.85	2.99 ±1.32	5.81 ±2.11	7.19 ±1.69
			1.51	2.90	2.15

of green, where there are only two extreme values; next follows yellow, then red, and finally blue, where the mean variation amounts to .44 of the average. So far as sex is concerned, the averaged results of the two women observers are greater in the case of three colors than the general average, and in the case of green equal to it, while the seven men average better than the total with red and yellow, equal with blue, and slightly worse with green. Age seems to have no influence upon the liminal sensitivity; and, similarly, long experience with colors and photometric work showed no effect.

It was found, again, that the binocular averages are considerably lower than the total and the monocular results. In twenty-two out of thirty-six cases the binocular averages were lower; in ten cases the right eye, and only in four cases the left eye was lowest. The right eye was slightly superior to the left with all colors but red.

Our results cannot be directly compared with those of Aubert and with the other earlier work, because of the differences in the external conditions. But we tried to determine whether natural daylight, which, of course, is several times as bright as our artificial illumination, would bring about radically different results. The two observers G and K, with whom this comparison was made, gave the following results under natural daylight: red 2.37 and 2.17, blue 1.83 and 2.96, yellow 7.84 and 6.85, and green 5.84 and 6.25; or in words, both gave a much lower limen for blue, while for G yellow and green practically changed places and for K the limen of green was lowered. Since these experiments were carried on between 11:00 A. M. and 3:30 P. M. before a brightly illuminated window facing the shining mid-day sun, the yellowish hue of the light would, by contrast, appear more saturated and therefore reduce its limen, while for G it also considerably reduced the saturation of the yellow stimulus, presumably on account of adaptation, although for K no such effect was noticeable. The striking fact is that the limen for blue in natural light was lowered.

Finally we attempted to verify some of Aubert's results by using the Masson-Maxwell disc; but instead of projecting the color on black or white, we used a grey disc of the same tint as the color, while everything else was copied from Aubert's description. K saw the innermost ring of green definitely and constantly as a faint colored ring; the next ring was seen only barely colored, and fluctuated; the third was absolutely colorless, slightly dark, and unsteady; and the other three could not be seen at all. For G, the innermost ring was clearly colored green and always visible; the next was less definitely colored but was constantly seen; the third fluctuated and was barely seen as color; the fourth was visible only during very brief intervals and showed no color, being just slightly darker grey than its background; and the other two were entirely invisible. The six small green squares which produced these rings were 5.5 mm and the intervening spaces 10 mm, thus corresponding from periphery to center to sectors of the following angles: 3°0′, 3°37′, 4°27′, 6°02′, 8°20′, and 14°50′. Accordingly the limen for G was 6°02′ and for K 8°20′; or, in other words, these values correspond as closely as can be expected with our previous data; but they are considerably greater than Aubert's figures. The difference must therefore be due to his use of black and white backgrounds instead of backgrounds of the same tint as the color investigated. The fact that both G and K could see a colorless ring slightly darker than the background seems to indicate that our equation of tint for color and grey as determined under artificial daylight was not absolutely true for the much higher intensity of natural daylight. The difference between color and background in Aubert's experiments must have been much larger, and hence may have produced colorless rings of light or dark grey which he may have interpreted as color-values; thus he may have obtained his lower limen. It must be left to further experiments to clear up the point.

We were fortunate enough to get a few observations on the color-limen from a color-blind person, but could not make a careful analysis of his defect at the time. His results afford, however, an interesting comparison with the normal averages. He gave the lowest limen for blue as 8.25° as against 2.99°; then yellow, 18° against 5.81°; then red 37° against 2.23°; and finally green approximately 140° against 7.19°,—but even then the color was named yellow rather than green. It may be mentioned in this connection that with every observer we had recourse to preliminary series for introspective descriptions of the liminal colors, the presence of after-images, and contrast-colors in the surrounding field; but the data thus far obtained are too incomplete to be included here.

ON THE INHERITANCE OF RHYTHM

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A. Introduction

It is perhaps unfortunate that the term rhythm must be used in this investigation, but usage seems to demand it. The objection to the term is that it may have three distinct meanings, physical, physiological, and psychological. The last of these concerns us here. There is certainly no danger of confusing the psychological with the physical rhythms; but confusion of the two organic 'rhythms,'—the physiological and the psychological,—is of common occurrence. For this reason it would seem well to point out the difference between the two by the following concrete example. The physiologist says that normal walking is a rhythmical act. Whenever a man is so badly paralyzed in one of his limbs that he is obliged to give the diseased member a peculiar swing in walking, the physiologist may use normal walking as a control and say that the afflicted man walks non-rhythmically. On the other hand, the psychologist ought to call the 'hobbling metre' movements of the paralyzed person a two-rhythm. The chief characteristic of *rhythm to the psychologist is the systematic accentuation and subordination of the elements of a series.* This interest at once eliminates all physiological and physical rhythms from the field of psychology.

Eugene Landry[1] has very well discussed the different conceptions of rhythm. His discussion was necessitated by his very broad definition of the term rhythm as 'la marche de l'énergie.' This would mean any activity is rhythmical where there is a succession of varying intensities. So he speaks even of the rhythm of prose recitation. He is not interested in all the intensity successions possible anywhere in the world, but primarily in the change of the energy of human action, and especially speech, from moment to moment,—in what is called psychological rhythm.

Views of rhythm, dating from early Greek mythology to the present, are based on the assumption that rhythm is instinctive. In earlier writings the hypothesis is explicitly stated, but in later editions there was apparently no use in stating a

[1] A bibliography is appended at the end of this paper.

theory so well known. The old assumption is never contradicted; and it is only by assuming the innateness of rhythm that one can justify the problems investigated and the attitude in general of the experimenters. There is only one other hypothesis that can be substituted for the old one, namely, that rhythm in general is not instinctive, or in other words, that it is the result of habit. If, with this assumption in mind, one undertakes a review of the literature on rhythm, one is bound to be shocked by the preponderance of pseudo-problems and by the great amount of attention that has been given to them. Perhaps it would not be out of place to examine a few of the most recent investigations in order that their contradictions may be shown, and also that the status of affairs concerning the problem of the origin of rhythm may be indicated.

Warner Brown undertook 'to determine which is the more essential to rhythmical grouping, the uniform time of recurrence or the uniform character of the thing that recurs.' The conclusion drawn by this investigator leaves the impression that if the phenomenon of rhythm is to be understood, it must be attacked from the temporal point of view. Lotze, Herbart, Wundt, and Titchener are supporters of this view. Brown speaks of *temporal rhythm* and of *accentual rhythm*. As a result of his work he is led to conclude that 'time aspects are fundamental and the accentual features, while necessary, are not at the root of the phenomena' because his experimental data show that 'on the whole, the temporal structure was maintained twice as well as the accentual.'

Another investigation is that of J. E. Wallace Wallin who was concerned with the extent to which the time of rhythmical groups may be varied without appreciably affecting the rhythm. Concerning this problem he concludes that: 'Absolutely periodic or regular occurrences are not essential to the appreciation of rhythm. To engender a feeling of rhythm always requires a certain amount of periodicity; but the margin of irregularity which may obtain is quite considerable.'

It seems rather remarkable that his conclusions should so explicitly contradict those of the first-mentioned investigator. It is in view of the following evidence that he drew the above conclusion. He was able to distinguish five grades or qualities of rhythm in relation to the amount of irregularity introduced between the beats of the rhythmical series. He speaks of five grades of rhythm,—excellent, good, medium, poor, and disrupted rhythms. The average amount of time-displacement or irregularity, in terms of percentage from the average, amounts respectively to 6.36 (grade excellent), 8.53, 12.0, 14.5, and 17.8 (grade disrupted). In absolute units of time these figures amount to 0.0526, 0.0734, 0.0991, 0.1182, and 0.1488 sec.

Herbert Woodrow in a paper entitled 'A Quantitative Study of Rhythm' dealt with a two-rhythm and found that he could change it from an iambic (accent on the first syllable) to a trochaic (accent on the second syllable), and *vice versa* by lengthening either the time-interval following or that preceding the accent. By a like process with the three-rhythm he was able to pass from dactylic (accent on the first syllable) to anapaestic (accent on the third syllable), and *vice versa*. Other important facts found by Woodrow are the following: (*1.*) The amount of rhythm as determined by the indifference-point may vary with the intensity of the stimulus. That particular point between two forms at which the nature of the second perceived rhythm is unpredictable, the author terms the indifference-point. This is the measure of the amount of rhythm. (*2.*) The amount of rhythm is found to increase with the rate.

Woodrow believes that rhythmical grouping is altogether a matter of temporal relations; and the effect of the accent in determining grouping is to produce temporal illusions through the over-estimation of the interval preceding the accent.

In another experiment, Woodrow discusses the 'Rôle of Pitch in Rhythm.' Here he employed the same method of measuring as before, and determined that giving one of the beats a pitch differing from that of the others does not influence the accent. Here, too, part of the paper is taken up with the consideration of the rate of recurrence of the rhythm elements. From the introspections of his observers he judged that the preferred rates of auditory stimulation range from 0.305 to 1.37 sec.

Much work has been done on the preferred tempo and there seem to be about as many different conclusions as there are investigators. For example, this tempo, according to Vierordt, is 0.62 sec; according to Stevens its lower limit is 0.53 and the upper limit is 0.87 sec; for Martius it is 0.50 sec; for Meumann it is 0.40 sec. These statistics, in the light of the theory of rhythm to which this present investigation is devoted, mean only that different people seldom make like movements with like speeds. The particular tempo which a person may choose depends in large measure upon the rapidity with which he ordinarily makes movements. This can be verified as I have done by correlating a person's reaction-time and his tempo. A high correlation is always present.

Another investigator who is generally considered to have contributed much to the understanding of rhythm is Bolton. He stated that it was his aim to reduce rhythm to a more fundamental activity of the mind. In the first part of his paper the author discusses all kinds of periodic movements from 'cosmic rhythms' to 'the incubation of fowls;' and in the meantime he shows how 'organic rhythms' have been decidedly affected by 'cosmic rhythms.' After this he talks about 'physiological rhythms,' which are no more than periodic recurrences of certain physiological functions. 'Of these, walking and speech are the most important and are true types of rhythmical activity.' All such movements he calls rhythms. These, however, are not the movements which he investigated; in his experiments he was interested chiefly in what he termed secondary rhythms. A secondary rhythm is derived from a rhythmical series of elements, according to Bolton, by accentuating one of the elements through increasing its intensity, pitch, or tone-color, etc., at regular intervals. 'Accent simply arranges the materials already rhythmical through some temporal recurrence.'

In speaking of rhythms in poetry, Bolton states that the number of accents to the verse may be four, six, or eight. The eight, however, fails to become popular because it exceeds the mental span. Because of the limitation of the mental span, the accents are limited to those two or three numerically small quantities; and it is only for the sake of variety that verses are made to contain five and three accents. Two, although it is the most primary rhythm, according to his way of expressing, seems never to have been used thus because of its extreme simplicity.

In ordinary life, groups of two things and multiples of two occur more often than three, hence rhythmic groups of two and four occur more often than of three. In a conclusion, however, the author makes the statement that 'a member of a sequence may contain *one* or *more* simple impressions,' which expression does not exclude any number.

The author raises the question of the inherent nature of a rhythmic group. The following quotation may serve to show his attitude toward this question.

'The conscious state, accompanying each wave of attention, groups together or unifies all the impressions that fall within the temporal period of the wave. As a result of a number of attentive efforts, a series of auditory impressions takes the form of a sequence of groups. This rhythmical grouping is due to the unifying activity of the mind. . . . . Each succeeding wave groups a like number of elements, so that the series is conceived in the form of groups. The rhythmical grouping is an attempt to conceive a series of sounds in a simpler form.'

It seems to be by means of this unifying activity of the mind that a series is transformed into a 'secondary rhythm.' The author's last statement, taken in connection with what he has said about the most popular rhythm, would mean that the four-groups and the six-groups are simpler forms than five-groups. If, however, all is dependent on the wave of attention, then groups of four and of six should be no more prevalent than five-groups. According to Bolton's theory of attention waves, it would seem that the five-rhythm must occur more often than the six-rhythm, since it is temporally shorter. From this dilemma his theory never escapes.

Stetson may be considered as beginning where Bolton left off. He is interested to know why the 'mind' possesses this unifying activity, or, as he would probably say, "why the human organism tends to divide a series of elements into unit groups." He is the first to see that the problem of rhythm is a problem of the activity of an organism rather than a problem of temporal consciousness. He is concerned with the causes of the organism's preference for certain groups of muscular activities,—why certain groups of particular numerical values are performed more often than others. Stetson was the first to mourn the fact that rhythm has always fallen into the hands of the investigators of attention, or the span of consciousness, or the perception of time. He says that not the temporal relations but the movements involved are the fundamental things to study if we intend to understand the phenomenon. He states further that it is a sheer assumption that regularity is characteristic of the pure rhythm, since it is easily proved that very wide irregularities can be introduced into a simple sound series without destroying the rhythm. He speaks of the human body as a device for producing rhythm. The larger muscles of the arm, for example, perform heavy movements, while the fingers execute finer movements. Repetition of this movement of the organ (the arm) naturally means a series of accented and unaccented elements of movement at *comparatively* regular intervals. "The unit group is the form in which the various muscle-sets and segments of a limb or organ can all work together, freely and easily, in a single movement cycle." The muscles of the arm can be spoken of as the major and minor muscles, the former performing the accentual movements, the latter making the finer movements.

Furthermore, combining several organs we find, for example, that the hand may make a series of movements which may be accentuated by a foot movement, or, perhaps, by a movement of the entire body. These movements may require a short time or they may require a long time. The speed differs with the particular nature of the parts of the body which move.

Stetson makes clear why two and three, including their powers and products, should be rhythms; but he does not explain any real distinction between five and three. The following quotation is his only attempt to explain why we often find unified movements (5 and 7 perhaps) in our experience. "The unity of an act seems to depend on the continuous character of its constituent movements and on the purposive habit which gave rise to it, rather than on the anatomical relation of the parts involved." But, inconsistently, he considers that five and seven are not rhythms. This may enable us to understand why he says that accentuation and subordination are '*perhaps*' the essential elements in rhythmic perception. He seems to believe that there is possibly another essential element hitherto unknown. If these two, accentuation and subordination, are the all-important factors, then why is it not possible to call five a rhythm quite as well as four? If four is a rhythm and five is not, then accentuation and subordination may not be of chief importance, because elements can be accentuated and subordinated to form groups of five just as surely as to form groups of four.

I am left to infer from Stetson's paper that he regards rhythm as instinctive. The essential element that is above referred to as hitherto unknown, seems to be regarded by Stetson as being of the nature of an instinct. He does not contradict the old view that rhythm is an instinctive something. Furthermore this is the only assumption that makes clear his attitude, viz.: "There is no reason for assuming that the nature of the unit-group of verse differs from that of other rhythms." "Most recent writers are inclined to reduce the types of feet to four; iambic, trochaic, dactylic, and anapaestic. All of the numerous kinds of feet occasionally given can be separated into these elementary forms." "The ordering of the unit-groups into larger unities is possibly a matter of historical development and might be studied in primitive art works."

With Robert MacDougall time is not such an important factor as it is with many other investigators. He concerned himself with many different problems, some of which I shall not discuss in this paper. I wish to speak of his work chiefly since he, like Stetson, superimposed his problems upon a hypothesis of instinctive numerical selection of group elements. This hypothesis is made no more explicit than Stetson makes it; but I think I am justified in simply saying that he assumes, as does Stetson, that rhythm is instinctive. Both are concerned with rhythm as a particular form of activity peculiar to certain animals and not to others. In other words, they are concerned with the fact that human beings group their actions into certain unit groups; that human beings, therefore, possess this instinct.

MacDougall places emphasis on the fact that the human organism prefers certain numerical groups of movements to others. The preferred groups are spoken of as rhythmical. The other groups do not concern the investigator after they are once determined to be non-rhythmical. He makes no search for an organic law to explain why one group should be preferable to another. To overcome the difficulty he assumes, at the start, that rhythmical action is innate, thus assuming at once the right to exclude from the investigation all group-movements which are not innate, as being non-rhythmical. His non-rhythmical groups are the prime numbers higher than three. He does not explain the selective principle which causes the human organism to choose either a four or a six in preference to a five-rhythm. This phenomenon can be explained, as we shall see, only in terms of organic activity as dependent upon the arrangement of parts of our mechanism. MacDougall convinced himself that the seven movement is not innate (and is consequently not a rhythm), and substantiated his conviction by introspections from his subjects, who said that seven furnished no feeling of rhythm. He calls the movements of two and three simple and fundamental rhythms; the movements of four and six, complex and secondary; and the movements of five, seven, or eleven, no rhythms at all. One of these introspective statements is as follows: "The sense of equivalence fell off at five and practically disappeared at seven beats, while groups of six and eight retained a fairly definite value as units in a rhythmical sequence." He obviously means by this statement that the ability to accent every second or every third element is instinctive and that four, six or eight furnish experiences of rhythm because they have the instinctive rhythms as their bases. In performing the nine-rhythm the three is still performed; but each unitary group of three becomes one element for the secondary rhythm of three or nine. The nine is a rhythm superimposed on the three having as its elements the structural units of the three.

I have not at all attempted an exhaustive review of the literature on rhythm. I have only mentioned in a brief way the contributions of some more recent investigators, as examples of those (the majority) who were chiefly interested in the time aspects of rhythmical action and of those (the minority) who subordinated this problem to others.

The contradictions noted in some of the preceding studies may not only cause one to be doubtful as to the usefulness of the primitive assumption (of instinct), but may be the cause for his becoming an aggressive skeptic. Let us now examine the following experiment which is designed to test the usefulness of our new assumption of rhythm as a habit.

B. Experimental

a. First Experiment

In this investigation I wish to concern myself with rhythm as a problem of organic activity of animal or human behavior. For some reason a characteristic of human beings is to divide repeated movements into certain numerical groups, or rhythmical units. A normal adult, if asked to beat a long succession of like strokes or to listen to such a succession of sounds manifests a tendency to group the elements into periods, that is, into successive unit groups numerically the same. The grouping factor may be an exceptionally extended pause, an exceptionally heavy stroke, a change in pitch, or any other means, although we need not assert that any or all would serve the purpose equally well. Rhythm is just this process of subjectively accentuating and subordinating elements of a series. If the grouping factor just spoken of, does not exist objectively, then it exists merely subjectively, as an illusion. I purposely speak of *units numerically the same* without implying true periodicity of time because I mean to lay particular emphasis upon the principles of accentuation and subordination, and to regard time relations of any definite nature as not essential. Time, of course, is important in rhythmical grouping since these movements must occur in time, but this is no less true of any other form of activity.

I hesitate to speak of time as being the chief factor in rhythm primarily for the following reasons. If it is the chief factor, why is it that in ordinary life the group of five if made at all, is performed with greater difficulty than the four, or the six, or the eight, but is produced with greater ease than the seven which in turn offers greater difficulty than the six, the eight, the nine, or possibly even the ten? If time were the chief factor, there would be no reason for such a preference of groups. Moreover, if it were the chief factor, the accent would be of no value. There would be no reason for making a distinction between physiological and psychological rhythms. And this failure to make such a distinction is the very mistake Bolton made.

Our movements in rhythm must correspond to the normal movements of our organs involved. The fact that our large members require more time than the smaller ones gives a basis for understanding the function of the accent, or why it is insisted upon in rhythm. There are other things which contribute a meaning to the accent, viz., the fact that in ordinary life we find it quite necessary to make lightly certain tentative or preparatory movements before the real purposive action is strongly executed. Further, there is the fact that we are bilaterally symmetrical. This affords us two means for executing like acts. One member of a pair usually becomes subordinated to the other. One makes the strong, but possibly unskilled, movements and leaves the production of the finer movements to the more skilful member. It is in this way that right or left handedness (and "footedness") exerts its influence. Our inherited structure is such that in performing purposive actions that require the use of the two hands serially we must often make weak and strong movements alternately, or one hand make two movements while the other hand makes only one.

In ordinary life two and three and their multiples are regarded as fundamental rhythms, and five, seven, eleven, etc., as no rhythms. They are regarded thus by MacDougall. He did not raise the question as to the possibility of *making* the groups of five, seven, or eleven seem rhythmical. Since they were not rhythms they were not instinctive and were therefore not considered in the investigation. The question of chief importance in my investigation naturally follows at this point. It is: Are rhythms instinctive? And if so, what kinds? In order to have a working basis for this investigation, I have formulated, provisionally, an hypothesis similar to the one which MacDougall was led to assume in view of the same facts. My assumption was that the simple movements of two and three and consequently their multiples are instinctive, and that the prime numbers higher than three are no rhythms at all.

One method of solving the problem is this:—I first tested the subject's ability to perform repeated movements in groups of two and three and then his ability to perform in groups of five, seven, eleven, or thirteen. Of course, the error in performing more complicated movements was greater. If, however, by performing certain purposive actions that will necessitate groupings of five, seven, or eleven elements, repeated movements of these numbers can finally be performed with as much ease and accuracy as of two or of three, the conclusion must follow that there is no evidence whatsoever for saying that the group-movements of two and three are instinctive and that those of five and seven are not; but only that our life activities, especially early in life, call for certain actions more often than they do for other actions.

The first apparatus used for the experiment consisted essentially of two parts, that used for developing the new rhythms, and that used in testing the subjects for rhythm. The apparatus used for developing the rhythm is a simple upright frame measuring 20″ x 20″. This frame carries four button bells on the four pieces of its structure, one above, one below, one to the right, and one to the left. Any button can be reached conveniently by a person who stands before the frame holding his hand near the center. Almost any type of purposive action can be performed by striking the buttons with a heavy, short-handled rubber mallet. Generally the purpose is to sound one or more bells in a definitely prescribed order. No counting is allowed while doing so. Take, for example, the five-rhythm. The subject performs an activity on the frame which necessitates a combination of four light movements and one extra heavy one to sound a bell. We need only to imitate a very ordinary complex of movements which occur very frequently in our every-day activities. In order to make certain that we hit a small object,—such as a nail head, for example,—it is ordinarily necessary to make certain tentative movements before the final stroke is made. This very common form of activity is employed here to develop rhythm.

For the five-rhythm only the two horizontal buttons, that is, to the right and left, were used. Let us hit first the left-hand button. The tentative activity is a simple complex of two movements, the easy stroke in the direction of the button and the back movement. Then comes the heavy and then the rebounding or second back movement. This leaves the beating hand very near the right-hand button which is to be struck next. One movement to the left is therefore a necessary adjustment. This completes our group of four small movements and one heavy one, mentioned in the order of two light movements, one heavy, and two light movements. The same, but reversed, complex follows for the right button, again for the left, again for the right, and so on.

The second part of the apparatus, or that part which is employed in testing, consists of two electric buttons to be struck with the mallets, a kymograph to record the strokes and an exposure apparatus showing thirty different stimuli, which are a mixture of letters and numericals. The exposure apparatus serves the purpose of distracting the subject so that he *can not count* his strokes. The subject must beat each time, and must call aloud the name of each stimulus when it appears. The test as to whether any movement is automatic is the subject's ability to produce it while giving attention to something else. Rhythm is automatic when it is not a voluntary counting of movements.

The purposive action on the frame to develop the five-rhythm necessitates a combination of four light movements and one extra heavy one to sound the bell as just described. This continues for ten minutes. The subject is then tested to see if the special activity has led to an improvement over a previous test. In the test he takes a rubber mallet in each hand, and with the right hand (if he is right-handed, otherwise he will use his left hand) he strikes the electric button every time a stimulus appears on the exposure apparatus. In order to make the movements rhythmical he accents every fifth element. This is done by bringing more muscles into play, by striking with both hands simultaneously. This test would be valueless, however, without the function which the exposure apparatus serves, as stated above.

The particular rhythm movements with which this division of the paper is concerned are three, five, and seven. I had subjects ranging in variety from a high-school student to a university graduate. Some were more mature than others. For this reason I expected a number of variations to appear. My intention was to obtain such a large number of subjects that I might disregard such variations, and that at the same time my conclusions might not be limited to one particular type of individual. The work was begun with twenty-one subjects. Only fifteen of these, however, completed the experiments.

The results are as follows. All were tested for various rhythms ranging from the two-rhythm to the nine-rhythm. The experimental result of all previous investigations of rhythm was again manifest beyond a doubt, viz., almost all subjects could perform the movements of two and three as well as their multiples and powers provided they were not too large numerically, but very few could produce the five and the seven rhythms. Some of the subjects, however, had the five and the seven rhythms from the very first. It is interesting and important to note here that the subjects highest up in the scale have the shortest reaction-time. It is important because it shows that the more active subjects are best. It is, of course, probable that the more active have the five and seven rhythms developed to a greater degree than the less active individuals.

In the experiment, the three rhythm, which is numerically simple and which is ordinarily considered to be instinctive, is used as a control. The average error made by twelve persons on this rhythm is 13 per cent. That made on the five rhythm after the first ten-minutes exercise is 24 per cent. (The lowest person on the five rhythm made 60 per cent, while the highest made only 1 per cent of error.) After the second exercise, the error for the five rhythm was equal to that of the three rhythm. We must not take this equality too seriously because the individual fluctuations are considerable, and the number of test persons is small. An absolute interpretation of this fact of equality would mean that five is as much a rhythm as three. At the next test, however, the error on the five is 15 per cent (or 2 per cent greater than the three), at the next it is only 5 per cent, at the next 6 per cent, and at the next and final test only 4 per cent of error.

By the heretofore prevailing hypothesis three is an instinctive rhythm. The 13 per cent of error made in producing the three rhythm is to be contrasted with each of the above figures.

Per cent of error on five-rhythm	24	13	15	5	6	4
Per cent of error on three-rhythm	13					

The figures for the seven-rhythm (average of twelve subjcts) are as follows:

Per cent of error on seven-rhythm	23.5	11	16	9	12	5
Per cent of error on three-rhythm	14					

Since some of the second group of twelve subjects were new there is a slight difference in the error made on the three rhythm. In general, the figures on the five and the seven movements lead to the same conclusions, viz., *if three is innate, the five and seven are also innate; if the three is acquired, the five and seven are also acquired.* One important thing to notice is the fact that very little practice is necessary to enable a person to perform the five or seven-rhythm even better than the three which is by hypothesis instinctive. There seems to be no reason for concluding that rhythm of any sort is instinctive, but only that generally in life certain movements are called for more often than others and hence become more automatic, more habitual. Our bodily mechanism is so constructed that in performing purposive actions, in work and play, the movements have usually a particular numerical make-up.

In contrast with MacDougall's notion that five and seven furnish no feeling of rhythm I must say that I found just the contrary to be true, or at least the contrary effect was produced. Although I do not deny that MacDougall's subjects failed to have that experience I am convinced that this feeling may be found even outside the psychological laboratory. The composer Tschaikowsky wrote musical measures of five-elements obviously because he enjoyed experiencing them. The strongest evidence is that after the experiment was over certain subjects invariably made greater errors when the exposure apparatus was going slowly than when it was going rapidly. If seven furnishes no feeling of rhythm, then the subject should be more able to produce the seven-groups when the machine goes slowly than when it goes rapidly. When it moves rapidly the subjects must react much more or altogether from their feeling of rhythm. This could not be done at first, because the subjects in question had found no opportunity to develop the feeling. The same is true for the five as for the seven. Introspections also supplemented this evidence. I believe that considerable confidence may be placed in the introspections for I was careful at all times to keep the subjects from understanding the problem. I told them at the end. This eliminated the effect of suggestibility on the part of the subjects. It was difficult if not impossible for them to tell when they were making themselves agreeable, and when disagreeable. In other words, they were not in any way influenced by what they thought I expected of them.

In order to show that the increase in accuracy upon the faster appearance of the stimuli is not due to a happening upon a *desired tempo* (the desired tempo has been of considerable interest to a number of investigators), I need only mention the fact that the fast tempo was considerably faster than any of the subjects chose to beat *in the absence of the exposure-apparatus*..

Time does not seem to play an essential rôle. As to the preferred tempo I found that there was a wide disagreement among the individuals, and that some individuals preferred different tempos from day to day.

It might seem that the chief thing to be learned from this experiment is that the major premise of many previous investigators may profitably be replaced by a new one. If we give up the old notion that rhythm is instinctive, then the new conception follows as the only and as the reasonable alternative. It appears at this stage more evident than ever that the more important problems heretofore investigated may be looked upon as pseudo-problems. If habit is at the basis of rhythm perception, the time element in all rhythm should be expected to vary. General agreement as to tempo would be truly remarkable. The time required to execute the movements, the time between the movements, the tempo, etc., should make such fluctuations as can be accounted for only when the nature of the environment that called forth the rhythm is well known.

I do not mean to say that all the previous investigations are valueless. The old hypothesis, conducive as it was to the preponderance of pseudo-problems, nevertheless furnished a background for the prosecution of some very interesting investigations.

b. Second Experiment

Let us now continue our old problem of the origin of rhythm, and at the same time try to determine *the most efficient means of developing* rhythm. The methods here used are somewhat modified. Instead of having certain persons work on one problem and certain ones on another, I had all subjects work on both problems. One subject beat the five-rhythm on the electric buttons and counted while so doing; then he was tested to determine the improvement which had resulted from this practice of counting. To develop the seven-rhythm the same subject performed the special activity of seven on the frame and then was tested. One case alone can not furnish evidence as to the part played by purposive activities in rhythm formation, so another subject, instead of practicing the five-rhythm by counting his beats and the seven rhythm by special activity on the frame, reversed the process. This arrangement was extended to all the pairs of subjects. Particular pains were taken to make the work a purposive activity which required no counting. This was done in order that the ordinary conditions of life might be approximated. In life we do not ordinarily count our movements, for example, in eating, working, playing.

We may look upon the experiment already described as preliminary. It gives a more comprehensive view to the problems with which the following portion of the investigation is concerned. In the present case, instead of having such a large number of subjects as before, I dealt with a few intensively. By taking an interested few I secured an ideal degree of regularity and promptness. Heretofore my subjects had no incentive to come to the laboratory except that I asked them. With some of them promptness was entirely out of the question. Further trouble which hampered the experiment was due to the apparatus.

Besides many minor changes, the exposure apparatus was set aside and a new one was made. In general, however, the whole testing apparatus is essentially the same as at the outset. Aside from a number of minor difficulties, there are one or two others which are due to the nature of the experiment. One of the most serious difficulties consists in the fact that any adult can and wants to strike the electric buttons faster than he can recognize and call the succeeding members of a series of promiscuously arranged letters. It requires some time to adjust one's self to call a new letter. This condition means that the apparatus must go more slowly than the hand ordinarily makes such movements. The calling makes the difficulty. The letters can be recognized soon enough; but unfortunately this fact can not help matters since recognizing alone affords no objective test as to whether the subject is conscientiously attending to the letters and not perhaps counting his strokes. One remedy for the difficulty was to use only three letters,—*S, O, R,*—which were chosen because of ease in pronunciation. These were so arranged that no two successive letters were the same, but they recurred without any regularity. The second remedy was to enlarge the slot so that two letters could be seen at the same time. This enabled the subject to call each letter just as it arrived at a certain point, designated by side arrows, and just as a hand movement is made, and to adjust his vocal organs for calling the next. I desired that the conditions be such that the subject could not devote his attention either to counting or to analyzing the period units he is asked to make, into smaller units. It may be true that when a subject acquires the seven-rhythm he may be able to describe his experience as two groups of two and then one of three, or any other convenient combination of small units such as three, three, and one. If this be the case, the test to determine whether he has acquired the seven-rhythm would be absolutely worthless provided he is allowed to make the same groups during the test. It might only show me that he has a *quasi* seven-rhythm made up of the groups two, two, and three, this being a three-rhythm with the accent produced by an extra stroke. In the first place I tried to avoid counting while the rhythm was being developed. Then for the test I arranged the difficulties of the apparatus so that there was no possibility of even the subconscious grouping just mentioned.

For the small children who acted as subjects, the arrangement was soon found to be adequate to do this; but for older people it was not difficult enough and another device was necessary. The three numerals 2, 3, and 4 were arranged at promiscuous places on the disc of the exposure apparatus so that no two of the same kind appeared twice in succession. The promiscuous arrangement was such that after the appearance of a stimulus some (irregular) time elapsed before the next one appeared. During this time the subject beat to the usual click of the metronome and continued to accompany each beat by calling out with the same loudness the stimulus that had last disappeared. By this means all forms of number imagery which might have any relation to the rhythm and practically all other imagery except the kinaesthetic images of the rhythm movement itself were eliminated.

Several factors had to be taken into consideration here. The one of greatest importance was the fact that the hand can execute the movement more quickly than the vocal chords can pronounce successive letters or numbers after time has been taken to recognize them. To overcome this difficulty the numbers were arranged in large intervals so that it was not necessary to recognize them each time the metronome clicked; and too, the range of expectation was narrowed down to one or another of three easily pronounced numbers. Another factor which we encountered consisted in a considerable tendency on the part of adults to count and thus to perform a mere quasi-rhythm. The introduction of the numbers tended more than the letters to offer a decided interference with the tendency to count. In fact, I feel quite sure that the tendency was overcome entirely. The letter system was just as satisfactory for the children as the number system was for the adults. Children do not possess the tendency to beat so fast as older people; and when they are quite familiar with the three letters involved they can pronounce them almost as well as the adult. The more coördinated a person's movements become, the faster he wants to beat.

Four small children, of five, six, six, and eight years of age respectively, were tested for the movements two to nine inclusive. As the successive groups became numerically larger, a correspondingly greater error was manifested. This increase in error was a gradual one. There was no sudden increase at five and seven as was observed with the adults. In fact, the error for each of the possible rhythms was very great, so that there seems to be little justification for saying they have any rhythm other than the two. The greater probability of the chance occurrence of the small group, two, in their life activities is possibly responsible for the small error made as contrasted with the larger groups. Two of the children did not continue the exercises long enough. The numerical results of the following tables refer, therefore, only to the other two.

The ability to perform the five-, six-, and seven-rhythms should be contrasted with that of three months previous.

		V-rhythm Errors	*VI-rhythm Errors*	*VII-rhythm Errors*
The five year old	1st	88 per cent.	85 per cent.	90.5 per cent.
	2nd	46 per cent.	64.5 per cent.	60.0 per cent.
The six year old	1st	82.5 per cent.	84 per cent.	88.5 per cent.
	2nd	69 per cent.	80 per cent.	40 per cent.

There is no marked deviation from the results of the first test; but for the second test after three months of practice one significant fact is manifest. Let us consider the two individual results thus:

	V-rhythm Errors	*VI-rhythm Errors*	*VII-rhythm Errors*
H. Five year old (error in second test)....	46 per cent.	(64.5 per cent.)	60 per cent.
D. Six year old (error in second test)....	69 per cent.	(80.0 per cent.)	40 per cent.

(1) The error made on the six-rhythm is in each case greater than either the five or the seven-rhythm. *The only possible exercise here to develop this rhythm during the investigation is the mere beating on the buttons for the test.*

(2) On the five-rhythm H. made a smaller error by 23 per cent. *H. used the purposive method to develop the five-rhythm while D. used the counting method.*

(3) On the seven-rhythm H. made a greater error than D., by 20 per cent. *Here H. used the counting method and D. used the purposive method.*

The exercises were taken twice each day, as nearly as this could be done during the three months. The periods of exercise for each subject were five minutes in each of the three methods. Pains was taken to make the conditions of work for the two subjects identical. They worked during the same times of day, for the same periods, and were tested during the same hour. A little more might be said about the three methods just spoken of. *1.* The test method consisted in the rhythmical striking of two buttons on the table, the unaccented strokes made on one button with one hand only, the accented strokes on both buttons simultaneously with both hands, with counting excluded by the requirement of recognizing and calling out the stimuli exposed at every beat, as described. *2.* The counting method consisted in rhythmical beating just as in the test method; but counting was required, and the exposure apparatus was, of course, out of function. *3.* The purposive method consisted in performing one of the specially chosen actions on the frame. For the five-rhythm, this activity has been described. For the seven-rhythm, it was the one corresponding to the following task: Strike the button to the left lightly (first movement); move to the right to suit the rebound (second movement); make a preparatory movement of adjustment to the left (third movement); strike the button to the right lightly (fourth movement); rebound as before (fifth movement); make a preparatory movement to the right (sixth movement); and now make a heavy stroke in space stopping suddenly near the center of the frame, to be followed by all the inner muscular adjustments (seventh movement, period completed) necessary to perform the same action in the vertical instead of the horizontal plane. Perform the same action again in the horizontal plane, again in the vertical, and so forth. Such a purposive action is comparable to that of a workman serving a particular machine in a factory, where, of course, he would not count, since the task can be done perfectly without thinking of any numbers. No numbers are found in the above description of the task. Similarly, what the subject in the laboratory has in mind does not include a series of numbers, but is simply the idea of the purpose, of the prescribed task.

It now remains for me to tell a part of the story which the tables do not show. Let us take a particular case. H. had acquired the five-rhythm so well that in the test he was making no errors when suddenly he stopped to laugh about and tell me how his dog 'Jack' went down stairs with all of his legs bent back. For a short time after this pause no particular movement was executed until he happened on the seven (which was by this time a rhythm) when he beat five groups of seven elements each. Ordinarily when H. happened on the seven, he usually made quite a series of seven-groups; but if he should be beating the seven-rhythm when the confusion occurred he would very likely start the five, and in that case he would beat a much longer series of five-groups than the seven-groups in the reverse transition. The same general statement can be made of D. except that she (since the seven had become easier for her) would beat the seven for a longer time than the five. Toward the latter part of the three months, I could always start the children beating the rhythms five or seven by working their arms. I could not induce them to beat three-, four-, eight-, or nine-rhythms by the same process, or any other, for that matter. As soon as I released their arms they would very soon begin beating either the five or the seven.

After a lapse of five months, H. and D. were again tested for the rhythms two to nine inclusive. Those rhythms which were learned by the purposive method were reproduced even better than at the previous test. At this test there seemed to be nothing new concerning the other possible rhythms, except that those which had been learned by the counting method, and the six-rhythm, were performed but slightly better than before any practice had been acquired. What little had been learned about them seems to have been forgotten almost entirely.

The subjects were again allowed to practice. This time only the purposive method was used since the relative inefficiency of the counting method was quite apparent; and as usual H. developed the five-rhythm and D. the seven-rhythm. At the close of seven exercises of about twenty minutes each they were able to produce the rhythms by making only an occasional error.

c. Third Experiment

Similar experiments with adults and with more complicated apparatus for adults furnished even more striking confirmations of the conclusions to be drawn from these experiments with children. More difficult rhythms were here taken into consideration. One person practiced the thirteen-rhythm on the frame, and used the counting method for developing the eleven-rhythm, while a second person reversed the process, that is, beat the eleven-rhythm on the frame and counted for the thirteen-rhythm. After seventeen periods of practice *each of these subjects made very little error in producing those rhythms which were developed by the purposive method, that of beating on the frame. On the other hand, no evidence could be found that rhythms were developed by the counting method.* It was found that instead of beating the rhythm which he had tried to develop by the counting method, the subject beat either no particular rhythm or the one learned by the purposive method. This outcome seems truly remarkable, especially since in the experiment with the small children there appeared to be evidence to justify the use of the counting method. The adults *not only succeeded in producing those rhythms* which they had learned by the purposive method, but they were *conscious of almost every mistake* made during the tests. These mistakes, however, were so few that in one case the error was 5 per cent, while in the other it was 7 per cent, the greater error being made by the person who beat the simpler rhythm, viz., the eleven-rhythm.

C. Practical Applications

In view of these results it may be interesting to discuss the " Rhythmical Gymnastics " of Jacques Dalcroze. Dalcroze is a teacher, formerly at Geneva but now at Dresden. His plan is to train his pupils to produce rhythms well, before they begin the study of music. The pupil's training is identical to what has been called *the counting method.* Dalcroze and his followers are convinced that great benefit is being wrought by this method, but to substantiate this conviction they have resorted to mere opinion. No test has been made to determine whether or not the rhythms were actually acquired by the Dalcroze method. It seems reasonable to suppose that the Dalcroze method is not altogether fruitless. The last described experiments on adults which seem wholly to discredit this means, contained only the small number of seventeen exercises. If this number were considerably increased it is possible that something would finally be gained even by the counting method. At least since this was the case with the small children there is no reason to believe that the exercises given by Dalcroze to his pupils are entirely fruitless. The writer is forced to the conviction, however, that the purposive method or method of ordinary life is the more efficient one.

The purposive method eliminates all forms of imagery except the kinaesthetic images of the movements. In the counting method, the analysis is not so complete; for by it are developed two forms of imagery, viz., kinaesthetic and number imagery. There are many activities which demand kinaesthetic imagery, but not number imagery. (My test may be cited as an example of such an activity.) For a person who possesses both kinds of imagery, the test is markedly a new activity, whose performance necessitates the elimination of the number images. It is an easy matter to inhibit these, but such an inhibition seriously disturbs the rhythm. To a certain degree the rhythm must be relearned. On the other hand, the purposive method fits the person for any activity which involves the rhythm previously learned.

Learning to perform, by means of one or many different members of the body, a purposive action of *n* movements of which one is "bigger" than the $(n\text{-}1)$ others and being then able to tap on the table in such a manner as to accentuate every *n*th tap, involves obviously a *transference* of an identical form of activity from one part of the organism to another. The extent to which and the ease with which a well-learned rhythm may be carried over from the hands to the speech organs, for example, and back again to the hands with the result that one has difficulty in recognizing it, is worthy of note. The writer was able to perform the six-rhythm without error, in the ordinary way of beating five times with one hand and then once with both. After half a dozen ten-minute exercises, the same rhythm could be beaten accurately in a new and more complicated way which will at once be described. The training which is necessary to bring about the transition is the following. Take this "sentence" of six simple words. *I I can not go go.* These six words take the place of the above six beats. While these words are spoken, the right hand beats three times while the left hand beats only twice. The right hand beats to the first *I*, the *can,* and the first *go,* while the left hand beats to the first *I,* and the *not.* In musical terminology this is a "triplet," that is, three notes played with one hand in the time of two notes played with the other hand. This activity is deceiving, for it seems as though the human organism is producing a three-rhythm with one member while with another it is simultaneously producing a two-rhythm. Careful consideration of the above method of acquiring this ability, however, reveals the fact that this is not the case, but that this is only an example of the six-rhythm with embellishments added. Instead of first one hand and then both as in other cases, each hand has here a different share of the six movements to execute. Where the hands fail to execute one of the elements of the six-rhythm, the muscles of speech execute it, and complete the six elements of the period.

The 'quintuplet' of the form five to three was learned by people in a similar manner. We employed the following sentence, which contains fifteen elements. *And now you may see*
R
L
I've crossed the big sea and got to New York. The letters
R *R* *R* *R*
L *L*
R and *L* below the sentence indicate the words accented by the right hand and those accented by the left hand. In either

case, these movements of the hand are real accents (secondary, of course, to the one main accent of the whole sentence), since each brings into play more muscles than the muscles of the speech organs which function with every syllable. Only the very superficial observer would describe this process simply as the production of the five-rhythm with the right hand and the simultaneous execution of the three-rhythm with the left hand; for this process is really the fifteen-rhythm.

The method of the previous experiment was originated by Mrs. Fannie Church Parsons of Chicago and applied to kindergarten children. There seems to be, however, no reference to her work in the literature. This method does not develop needless number images, but it does develop, in addition to the kinaesthetic imagery, visual and auditory images of the words of the sentence. These latter images may be useless, and perhaps they are a hindrance in such an activity as my test involves. Fortunately these images do not stand much in the way since they can easily be inhibited without seriously disturbing the rhythms. This system, therefore, has decided advantages over the counting-method and may be comparable in efficiency to the purposive method as previously described.

The Dalcroze method of teaching rhythms has been referred to in this paper as a *counting-method*. Some explanation is here necessary since Dalcroze says: "Ich verlange garnicht, dass man zählt. Es sind die Unbegabten welche zählen. . . . Meine Methode ist Sache der muskulären Erfahrung." ("I do not require any counting of my pupils. It is the untalented who count. . . . My method is a matter of muscular experience.") The hopes of Dalcroze are here plainly stated; but whether he fully realizes them in practice is open to question. In one sense his statement as to the untalented is a confession that counting is by no means discouraged by him or made unnecessary, for his pupils can and do count. I have spoken with a number of people who have acquired rhythms by his method; and the following stubborn fact is always to be observed. In order to reproduce the rhythmical actions learned under him, which during the course of time have almost been forgotten, they invariably count. Number images are the means for reproducing the movements. This shows that in his teaching Dalcroze has not succeeded in making the 'muscular experience' independent of number images. I have no reason for supposing that these people with whom I have spoken are untalented. I rather think the method is such that all have an inducement to count. Perhaps still another quotation is here appropriate. "Wenn man den Rhythmus seiner

Glieder empfindet, braucht man nicht mehr zu zählen." ("As soon as one perceives the rhythm of his bodily members, one does not have to count.") So it is evident that the students start out by counting; and no one but the pupils themselves can say whether or not they continue to count throughout their training. It is no wonder, then, that counting is the means for reproducing the old movements. Under these circumstances I doubt if Dalcroze's method of teaching 'rhythmical gymnastics' deserves to be regarded as a specifically new method. His success as a teacher seems to be due to personal qualities rather than to excellence of method.

Dalcroze says plainly, "Ja sicher, Rhythmus ist erblich." ("Yes, surely rhythm is hereditary.") If the rhythms are inherited, then one method is as good as another to describe what action is at any particular time desired. Why, then, not use counting since it is the simplest means? But according to experimental results, included in this paper, the avoiding of number images seems to be a matter of expediency. Dalcroze is aware of the possibility of eliminating counting after the rhythms have been, so to speak, found by the students. He conforms to natural conditions in so far as he takes pains to allow (large) movements of small frequency to be made by large body members and (small) movements of greater frequency to be executed by the smaller members. Thus it is possible for him, after persistent and consistent work, to emancipate himself from all counting. This conforming to anatomical conditions, as just stated, seems to be the whole of the 'Dalcroze method.'

D. Summary

All experimental evidence seems to point to the conclusion that rhythm is acquired by each individual, and that it is not inherited. Biological conditions,—for example, the anatomical fact that we are two-footed, two-handed, and generally two-sided, not three-cornered or star-fish like beings,— are favorable for the development of those rhythms which have usually been considered to be instinctive, while the other rhythms can be acquired only under special, somewhat artificial conditions. The best means for developing rhythm is that which approaches our ordinary life activities. In the development of a rhythm, the motor activity of the skeletal muscles plays the most important rôle. For this reason, the larger movements of a purposive activity are much more conducive to the production of rhythm than the smaller movements which accompany the almost purposeless activity of the counting method.

I feel greatly indebted to the many people who persistently acted as subjects in this experiment; and I am especially grateful to Professor Max Meyer who has been ready at all times to advise and assist me, and who even devoted much of his time to serving as subject himself.

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KINESTHESIA AND THE INTELLIGENT WILL

By George Van Ness Dearborn[1]

CONTENTS

I. Introductory

The purpose of this research and essay is to point out in terms of more or less well-known nerve-circuits a new consideration or two as to the nature of human motivation and will. In the chosen title the term intelligent is used, therefore, in a very broad sense to include several processes other than intelligence in the technical psychological usage, particularly affective tone. A more precise expression than intelligent will would be perhaps teleologic conation, however cumbersome such a term may be. Both, however, seem to the writer to express definitely enough the empirical motivation-tendency of the actual human adult.

Just as, psychologically considered, vision is undoubtedly the "queen" of the senses, so physiologically the processes inherently relating to movement, posture, weight, spatiality, etc., are assuredly the most important. In the universal integration of sensations, vision in a way may even be considered the mental homologue of the bodily kinesthesia as a little thought readily shows. Only now are educators beginning to realize the indispensable usefulness always and everywhere of kinesthesia, the "feelings of movement." Kinesthesia is about, however, to come into its own as the primary and essential sense. Without it, coördinated and adapted bodily movement and strain, concomitant to every kind of mental

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process, is inconceivable, for the (psycho) motor centers in the brain have no known clairvoyant powers and therefore their function of carefully coördinating the distant muscles, *e. g.*, of foot or hand, is entirely dependent on their continual reception of detailed information as to the relative tonal and contractural status of all the active parts to one another. Simple as this idea is, the immense practical importance for education has as yet hardly begun to seep into the effective minds of educators. Its relation in a general way to voluntary movement has recently been set forth inadequately in an article (17) by the present writer from which the following quotation may be expedient.

"The *nervous circuits* which underlie voluntary movement are really of course the functional framework, so to say, of all neuro-musculo-glandular activity. The idea is the modern successor of the reflex arc, the succession being made necessary by our increased knowledge of the nature and uses of kinesthesia. Here again our classification is arbitrary to some extent, but none the less useful, perhaps, for descriptive purposes, and withal tentative.

The simplest "circuit" that we have to consider is that which we may call intramuscular. The knee jerk, for example, is far too quick to be a spinal reflex, so that we have to suppose it a direct reaction from the thick subpatellar tendon of the muscle-mass concerned, a direct stimulation of the elastic muscle-fibers. It is obviously only for the sake of logical completeness that we start with this, for a literal circuit here is hard to define, and moreover useless for our present purpose.

The next type of circuit is similar to the vegetative circuits of the sympathetic system, such for example as are concerned in the movements of the intestines, by the plexuses of Meissner and of Auerbach. A third type is of a partly reflex and partly sympathetic nature, and represents those reactions that are primarily reflex and yet in part under the control of the sympathetic system. A good example of such a circuit would be found in an attack of painful cramp from distention of the gut. This would constitute for the purposes of our discussion an example of a circuit intermediate between one purely vegetative (under the entire control of the sympathetic system) and an out-and-out reflex. A fourth kind is the typical reflex, termed "epicritic." Other circuits go up to the bulb and may be termed medullary circuits, and comprise those that have to do with respiration and heart-action and many of the other vital functions which are apparently controlled by the medulla. The next variety of circuit we may term the nuclear circuit, such as those that go up into the big neuronal masses in the interior of the brain and whose activities in part at least are of the type of emotional reactions.

Lastly, the most complex type is that properly known as the cortical circuit, and this especially is accompanied by consciousness and, more important still, is under voluntary and initiative control. It is an important circumstance that each one of the circuits mentioned includes more or less of the apparatus of all of those below it. Thus you have a series ever enlarging upward. Anything that might happen in the lowest circuit would more or less affect the higher, and anything coming from the highest would have all the other circuits more or less under voluntary control, unless prejudicial to efficiency. The highest and longest and most complex circuit, the top one only, then, properly speaking is *voluntary* in nature.

These circuits of nervous influence, as various as their unique routes through the central nervous system, are not represented, of course, by separate neurons. Again, some of them are of actuating and some inhibitory nerve impulses, some produce action while others inhibit it, in some cases not only stopping the contraction of the muscles but actually and actively relaxing them. Sherrington especially reports an experiment upon the reflexes in dogs in which active relaxation of the mucles occurred. We need to know more of this! Inhibitions in the adult have long since reduced themselves largely to the reflex type. Moreover, the inhibitory impulses by their nature are negative and have been lost sight of, so that oftentimes we do not realize that they are present at all in the adult individual. For example, the very essence of the action of alcohol upon the finer cortical centers in the brain is an inhibition (depression?) and yet, as we all too well know, the moderately intoxicated person shows signs of anything rather than that of inhibition or negation. It needs emphasis here that in general the part played by inhibition in voluntary movement, as also in attention (13) is preeminent. Inhibition, however, is too little known still to allow of its discussion at this time in this complex connection. The work of Nikolaïdes and Dontas (confirmed by Wooley and Fröhlich) demonstrating actual inhibitory fibers in the muscle nerves, is a recent important step along this path.

There is a classification of the nervous circuits we have mentioned other than that of actuating and inhibitory, and it is one that is of more immediate use in the theory of voluntary action, namely, their division into circuits on the one hand between the skeletal muscles and the cord and into those, on the other hand, between the grey cord and the grey fabric of the hemisphere above.

As to these former nervous impulses: Few facts more helpful to our knowledge of personal control have appeared in late years than that the muscles of the body, anatomically but not physiologically individual are mainly coördinated in the posterior grey horns, these being subordinated in some certain directions (equilibrium, resistance-purchase, etc.) to supervision from the cerebellum. As a corollary of this, important very to our present intent, it is clear that the separate muscles have no direct representation at all in the cerebrum proper.

The truly personal control, then, of the cross-striated musculature comes through influences of the second variety just suggested—namely, those between the spinal horns and the grey fabric of the hemispheres, cortical and nuclear. This control, moreover, is certainly "symbolic;" that is, a properly adapted single influence probably controls a whole movement, although in the spinal grey this may involve the coördination of very many muscles in a functional group. Moreover, this control as it comes into these muscle group centers of the cord, is a *resultant* of numerous complex factors which it is the business of the hemispheres to produce by the stress and the strain, by the thrust and the pull of the individual psychophysical conditions at the moment or constitutionally, or both. It is the resultant, apparently, that controls the grey cord's group action.

One of the most essential of the factors in the neurility of voluntary control we now all recognize as kinesthesia. We do not, of course, need to enlarge upon its nature here, for Goldscheider, von Frey, James, Bastian, and many others have made its organs and its preëminently important functions in the conduct of life common knowledge manifest to all, details, however, being added continually. These kinesthetic impressions or impulses come from the moving parts into the grey matter of the hemispheres. Take the elbow or the wrist joint, the fingers, or the shoulder joint, and consider all the scores of muscles, tendons, and bones involved, as well as the skin over these moving parts, and include also the sense of touch, and we can see what an enormous complex of kinesthetic sensations and impressions must crowd continually into the central nervous system from every part of the ever-moving body.

The familiar work of Mott and Sherrington on the afferent spinal roots in dogs was quite conclusive as to the status of these movement " sensations " or influences. It will be recalled that the efferent nerves in these experiments were left complete, the outgoing motor neurons, but the dogs none the less were quite incapable of making any efficient voluntary movements, properly speaking; Munk then took up this work of Mott and Sherrington and demonstrated that the dogs after a time could make voluntary movements, but that they had to learn to do so by the vicarious use of their eyes, these organs taking up symbolically the functions usually ascribed to kinesthesia. Experiments upon certain paralytics who lack the power to make a voluntary movement show this same fact of vicarious symbolism to be true. It is sufficient to say, in short, that the kinesthetic impulses from the moving parts of the body start, or at least direct and control the voluntary movements of the individual considered as mechanical events. Unconscious kinesthetic influences appear to direct the gross movements with the help of vision, while the conscious muscle-joint impressions control the fine adjustments by an inhibitory mechanism until they, too, have become subconscious by habituation. Whether we believe with Bastian or with his opponent Ferrier as to the topographical nature of the great cortex, we cannot fail to see that the circuits between the muscles and grey cord and brain and back again are at once *the framework and the substance* of the neural process in voluntary movement, the former half of the circuit in each case being kinesthesia, be the details what they may."

II. Research

The writer is enabled to add something to the physiology of kinesthesia as the product of experimental work among his students and colleagues in the Tufts College Medical and Dental Schools and in the Sargent Normal School. This research deals directly with the functions of the nerve impulses concerned in the so-called muscular sense. The investigation was made by voluntary movements; but its results are equally well applicable to other muscular coördinations. The nerve processes that determine voluntary movements have been much discussed of late by the psychologists especially and have enlightened us not a little, while the neuro-histologists and the clinicians have been clearing up their side of the neurology of voluntary movement somewhat. In a sense, the present work forges the links connecting these two chains of information.

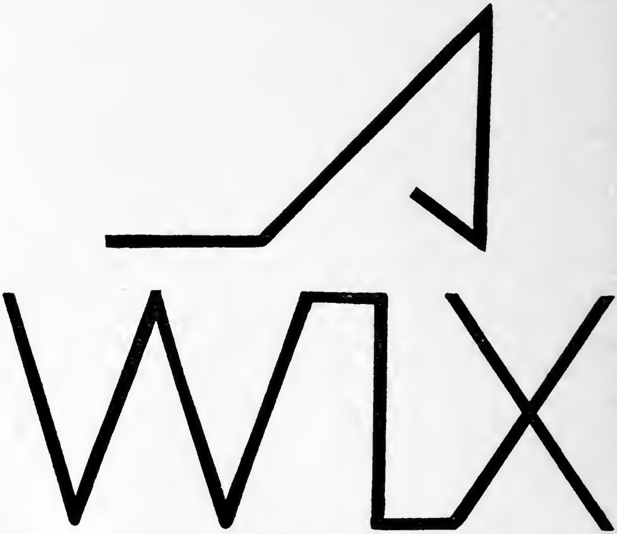
One obvious reason why progress has not been faster on the introspective side of the problem is that most of these experiments (74), *e.g.*, have studied rhythmic movements, movements made with voluntary muscles to be sure, but yet largely lacking the characters which distinguish voluntary action from that which is already more or less habitual and organized. An ideal voluntary series for such an investigation would be the movements of a novice learning to engrave without mistake a deep monogram on the side of a silver cup, or those of a youth learning to cast a small trout-fly in a steep and narrow rocky river with alders along its banks and with old beeches overhanging. Such behavior is action that is truly voluntary,—but quite as truly insusceptible of systematic scientific measurement and study. Again, we certainly can get but little information as to ordinary muscular control by the training of abandoned (75) or even degenerate muscles, such as those of the pinna. The present experiments seek to avoid both of these apparent defects by using muscles that are under excellent personal control in movements at once simple and yet, as wholes, new to the individuals making them. From such easily defined and truly voluntary movements it was hoped that useful kinesthetic information might be gleaned.

The writer has already, in several places (17, 18, 19), expressed his opinion as to the general nature of the cerebral influences determining voluntary movement (a result already reached, by the way, unbeknown to the writer, by Woodworth),—namely, in a word, that the determinant can be nothing less than the whole disposition of the brain and organism at that particular moment,—cord, sympathetic, and all. He may not, therefore, be classed with the ultra-kinesthetists who overestimate the importance of these movement-neurograms for bodily management. None the less, as everyone must admit who realizes this problem of nerve-control over thousands of otherwise unconnected muscle-units, kinesthesia must be seen to constitute the chief spinal factor of muscle-coördination, and so of voluntary control.

A. The Apparatus and the Material

This research was begun in November, 1910, and has been prosecuted at intervals since, on thirty-eight subjects, all of good intelligence but varying in age from fourteen to sixty, of both sexes, and some of them blind, the majority of them were young women between eighteen and twenty-five years old.

At first, a rather elaborate electrically recording apparatus, devised for the purpose, was employed; but this was abandoned for a method at once simpler and more efficient, and moreover closer to the actual conditions of motor training, to the knowledge of which these experiments are a contribution. The apparatus chiefly employed, then, was of the simplest,—two norms or models, shown in the figure, these being simple series of connected straight lines traced on a sheet of paper. One is a horizontal line 2 c. m. long thence 5.25 c. m. upward at 45°, then vertically downward 3 c. m., then 1 c. m. upward and backward (to the left) at an angle



of 45°. The other norm, as may be seen from the figure, is really a W, an I, and an X connected by short horizontal lines, the whole being 8 x 3 c. m. overall. In addition to these norms the apparatus comprised only a full-length lead-pencil, sheets of white paper 21.5 by 28 c. m. in size, and iron clamps to hold these sheets to the corner of a table, sometimes under a sheet of plateglass.

B. The Method of Research

The method of conducting the experiments was equally simple, as is always fitting if we are to get to the bottom of complex things. The subject, carefully blindfolded, stood before the table, was given a long pencil, and told that having been given "a certain series of straight lines" he was to "draw it on the sheet of paper before him as accurately as he could, both in size and shape." The pencil being held with sufficient firmness in the subject's right hand, the experimenter with a hand on either end of the pencil then guided the point of the latter at a moderate speed over the norm. The subject was then asked if he could repeat this voluntary movement, if he could draw what had been given him kinesthetically. If he had a sufficiently clear idea of it, a sheet of paper was placed under the pencil-point and the first record was made. Information was then requested as to the nature of the motor idea by which he had been guided (whether visual, kinesthetic, or otherwise), and the notes were written upon the sheet. Usually more than one kinesthetic perception of the norm were necessary before a sufficiently clear notion of the series was obtained; the subjects varied very greatly in this respect. After several repetitions of the voluntary movement by the subject, he was asked about the movement-feelings in his active arm and his replies were carefully set down. He was then requested to notice them as intently as he could and to concentrate his attention upon them, and meanwhile again to draw the series of lines as before. Careful inquiry was made in every case as to the success and other conditions of this process of intensification of the resident kinesthetic sensations and the degree of success, with the other notes, recorded. After a few performances alternating between the natural method and this one with strengthened movement-sensations, the blind was removed from the eyes and the norm was visually shown to the subject (no measurements being allowed), and the blind was then replaced. Note was made as to whether the visual percept seemed surprisingly large or small or neither. The former alternation of visual and kinesthetic guidance was then continued for a few performances, real intensification of the resident sensations being assured, as before, by suggestion and unobtrusive insistence. Finally, the blind was removed, and the subject was asked to draw the norm as well as he could with his eyes open, as a final memory-standard, based on all his combined experiences of the series.

Thus we have five sets of similar voluntary movements for qualitative and, if expedient, quantitative, comparison: a set made from a motor idea kinesthetically received; another on this same basis but with intensified movement-sensations; a third, made from a motor idea both visual and kinesthetic in origin, another, on this latter basis also but with the movement-sensations reinforced; lastly a set of performances made with the eyes open on the basis of the previous experiences combined. Here obviously is a wealth of material for the study of many things,—for example, the effects of intensification of the conscious phases of kinesthesia, which we use, and data for fundamental knowledge as to individual differences, which we postpone to another time.

A third but brief series of experiments was conducted on some of the subjects in the drawing, blindfolded, of 40° angles with sides about 20 c. m. long, and received (perceived) by the performer kinesthetically as in the other experiments. This work was soon abandoned for a better plan. It was, however, here brought out without chance of error that difference in direction has no primary, *natural* basis in the kinesthetic memory of visualizers, however, easy of development there. Differences in direction depend on visualization, whether it be the literal process as used by most people who see or that close imitation of it, derived probably from kinesthesia plus local signature, so conspicuous in the minds of the congenitally blind. There is assuredly some elaborate process of synthesis that is readily competent to give human beings their necessary working concept of spatiality even out of the most fragmentary experiences; that it readily arises from kinesthesia can be marvelled at by no one. The necessity for this spatiality has been discussed elsewhere, for example (37, 18, 17).

C. Notes and Results

The experiences concerned with the performances in these experiments were clear to every subject and are of such a nature as to be clearly statable by every adult of normal intelligence, for movement could not voluntarily eventuate until its conditions, in part at least, were known. The research is as much neurological as physiological and more of either than psychological, for no one can divorce in such conditions the facts of personal experience from the nerve-impulses underlying them; and herein can neurology receive more help, oftentimes, than from the histologist or ever from the clinician. The number of subjects is sufficiently large to ensure reliability; and their variety in age, sex, and race, and both blind and 'seers,' adds to this trustworthiness. The work endeavored to keep as close as possible to the ordinary conditions of average voluntary movement with the arm; the chief departure from this normality was obviously the blindfolding of the subjects, but herein lies of course the crux of the results in a study of kinesthesia. It is one of the unexplained curious facts of psychology that in unskilled movement, vision completely drowns out the kinesthetic sensations. The lines of the norms (one simpler, the other more complex and requiring better memorizing than the other) are about of the average length of voluntary movements, being much larger than the movements of the writing class, yet greatly smaller than the free arm-actions so common in ordinary behavior. This latter consideration is one of no little importance in judging the protocols of the experiments for in a free right-arm movement such as these, made with the individual standing, the continual tendency would be to exaggerate the movement beyond its actual small extent.

Of the results of this research, perhaps the most interesting for the physiology of kinesthesia is the fact that reïnforcement of the conscious movement-sensations by deliberate attention to them tends to shorten the movements and somewhat to lessen the angles between them. In other words, in visualizers, intensification of the conscious kinesthesia inhibits the related voluntary movements. Such being the case, it is difficult to avoid the conclusion that *the function of the conscious movement-sensations is the inhibition, the deliberate active restraint, of tendencies to action.* Attention to matters other than the resident kinesthesia of the active arm, as was demonstrated by trial, tends to stop the performance altogether, or to make it irregular, uncertain, and wabbly. Attention to the resident kinesthesia, on the other hand, tends simply to make the movements less, while they continue deliberately to the end without hesitation, but minimized. Considering that the movements studied are free-arm movements from the shoulder-joint, tending therefore to exaggeration far beyond their true size, this result becomes all the more striking. Almost no records, *per contra,* showed enlargement under these circumstances, also a significant fact where the voluntary conditions are so complicated and uncertain. Fifty-five per cent of the reactions showed this inhibitory effect and just about half of the subjects made reactions consistently that were of this type. May we not conclude that the more or less conscious resident kinesthesia, perhaps distinct from other kinesthetic coördinations, has special inhibitory function, as well as the passive reception of jars, vibrations, passive movements, and postures?

The experiments seem to show, moreover, that the muscles themselves are the seat of the changes that the arm undergoes when intensively attended to. The bearing of this fact upon the controversy between Reichardt (60), Pillsbury (57), etc., and Goldscheider (32) as to the relative status of the muscles and the joints for kinesthesia, is not easily cleared up at present. It would tend, if anything perhaps, to support the former, the muscle, side, for reinforcing the resident conscious sensations (nerve-impulses) lessens the extents rather than disturbs the shape based on the (extrinsic) motor ideas. This inhibition of a gross complex movement we must ascribe directly to the muscles, whether *guided* by the arthrodial afferent impulses or not.

The fact of the prompt and usually accurate *visualization* of the movement-series received into the person's central nervous system solely through the kinesthetic receptors of the arm, is an interesting product of this experimental work. Often the subject reported a clear visual image of the norm after receiving it (kinesthetically) only once, and often he proved its existence and its accuracy by drawing it (on the basis of a visual motor idea). The paths of these kinesthetic impulses and their production of a visual image constitute a problem of especial interest for the physiology of efficiency; and they would explain the conception of space in the blind. This process of visualizing varied much in different subjects. It was best and promptest in visualizers long trained in delicate motor adjustments. It was least conspicuous in the three subjects (two young women with normal vision and a youth congenitally blind) whom the psychologists would class as motiles (see below).

In any case, this prompt and usually involuntary, translation of a complex and purely kinesthetic impression into a visual image is sufficiently striking in itself, however familiar a process, and leads inevitably to a query, as yet unanswered, —why? Why cannot and does not the man of average motor skill use his conscious kinesthetic impulses in the guidance of a movement, especially a movement about which he knows nothing else? Perhaps one reason for his failure to do this lies in the inherent inhibitory action of the conscious kinesthesia—and the other phase of kinesthesia his imagination knows nothing of. But aside from this in part, the *opposition* between the resident sensations of movement (inhibitory and more or less conscious) and the 'visual' and other actuating influences, is marked; and the fact may prove suggestive. Even when reinforced by conscious effort, the resident kinesthesia and its centers (in the great cortex?) had usually no conscious memory of the norm as a whole, had nothing, in fact, usable as a motor idea; and the subject (unless a motile) thought himself wholly dependent for an efficient performance on a second-hand, mediate, visual image, product of the immediate impressions none the less which he supposed himself unable to follow as a guide. The apotheosis of 'the queenly sense of vision' is close at hand, and that notwithstanding only a few to-day suppose the eyes have more to do with a bodily movement than the placing of its termini (*a quo* and *ad quem*),the location, in short, of the ends of the action in the essential space. (Besides this, in cases of paralyses, etc., vision serves vicariously for kinesthesia in many ways; but we deal now only with normal conditions.) The conclusion is inevitable from these circumstances that there must be kinesthetic nerve-impressions in closest relation with the visual neurograms proper, yet obviously unconscious, that do furnish the data necessary for a voluntary movement, and that actually do coördinate the muscles which are active in an action. This phase of kinesthesia, actuating, determinant of the force and speed of a movement perhaps, but subconscious or even unconscious, represents, it seems, the innate impulse to activity, the instincts, the emotional syndromes, the reflexes, the correlation of all the forces behind a voluntary arction. This phase of the movement-'sensations,' however, do not represent the individual personality of a human being until complemented, held in leash and inhibited, by the conscious resident sensations. We cannot escape terming these actuations, coördinated perhaps in the posterior grey cord, kinesthetic, for their nature and use are inconceivable apart from the joints, muscles, tendons, bones, and skin,—the chief locations of the kinesthetic receptors. The opposition between these two phases of motor control, actuating and inhibitory (the former including vision and audition), is eminently natural for most people, then; but it is an opposition that is in reality an alliance and capable always (skill) of fusion for efficiency.

The various types of method by which people attend voluntarily to these two things at once in the control of an action is well brought out in the subjective reports if not from the records made by the subjects' arms, which question remains for future measurements perhaps to determine. Most often there was a conspicuous alternation between the visual image and the inhibitory kinesthetic experiences, the visually imagined guide being brought to mind just often enough to furnish current control of the movement's extent and the direction of its component parts. The accuracy of these performers was sufficiently good for ordinary behavior. People of this class constitute the great mass of men and women;

they use their eyes, sometimes, to start a voluntary movement and to stop it mostly, and they use their spinal, unconscious kinesthesia to furnish it force, speed, etc.; but they never become generally conscious of their resident kinesthesia, properly speaking, never acquire the personal valuation proper to new and free, because personally inhibited, deliberate actions, except perhaps in certain sets of movements.

The other type, as indicated by some of the reports of the subjects, all of whom were naïve, are able to *fuse* the visually imagined (but in reality mostly kinesthetic) norm with the conscious resident kinesthesia, using them together for the guidance of a new voluntary movement. The records thus made are more accurate than the others differently controlled. Moreover, the performances of these 'fusers' have an important relative stability, a constancy of form and size, and an independence of agencies which, in others, less endowed with conscious movement-sense, disturb the accuracy of the action. The numerous records show this conclusively, and this indicates much.

The people whom the psychologists class as 'motiles,' then, appear from this research to be those who, for some reason, have their conscious resident kinesthesia hypertrophied, so to say, and apparently at the expense ordinarily of the unconscious movement-neurograms and of the visual control. The subjects who were of this type made poor drawings in these experiments. Their records were distinctly less accurate than those of the 'visualizers' in both extents and regularity, with a tendency to rounded angles and curving lines. In motiles, then, the motor idea tends to adequate presentation in terms of the local conscious kinesthesia; but unless this resident plan of the movement be much more than ordinarily comprehensive, it fails in its supposed adequacy as a motor idea.

My experiments and conversations with many of the most intelligent blind people, young and old, connected with the Perkins Institution (far-famed by its miracles with Laura Bridgman and Helen Kellar) lead me to a complete corroboration of Treves' ideas (69) as somewhat opposed to those of Haller (35) on the elaboration of kinesthetic impressions into the conception of space. We can, however, go two steps further perhaps at present, and point out how similar is this blindman's space to that of seeing persons, and how organic and indispensable it is in both in the evolution, the origination, and the adult practice of voluntary movement. Our records as well as our notes show that the seeing motile is practically in the condition of the congenitally blind, but, of course, far more accurate in his action and with much greater potentiality of efficiency.

Between these partially opposed types of motor consciousness are many indefinite degrees, apparently, according as a given limb is more or less skilful in a given group of movements, in number of course uncountable. It is, then, one of the inductions of this experimental work that the motor skill of a person in general, and also in particular actions, is more or less proportional to his habit and capability of using the conscious kinesthesia for the current inhibition of actions elsewhere coördinated and actuated. As has been shown already, this actuation comes from (spinal?) kinesthesia in combination with external control, usually either visual or auditory. We physiologists are rapidly learning that all bodily processes and conditions are the algebraic resultants of balanced tendencies, whether nervous, chemical, or mechanical. The neuro-physiology of skill as in part determined by the afferent neurograms of movement, certainly is no exception to this rule. The unconscious and the conscious, the actuating and the inhibitory kinesthesia, surely share and complement each other in motor control. A person's skill, therefore, appears to be a 'function' of his habit of usefully *fusing together* his motor ideas proper and the resident movement-sensations which in him are adequately conscious. Compare Slinger and Horsley's conclusion (68) that "the muscular sense under necessity can, by education, be brought to a point at least one-fourth better than that learnt by a normal seeing individual."

But, again, compare the practically unanimous opinion and practice of instructors in all kinds of motor efficiency (music, instrumental and vocal, manual training, physical education, legerdemain, etc.) that attention to the sensations of movement disturbs the performance and is therefore to be avoided. At least one successful instructor in voice, of my acquaintance (Mr. Willard, late of Harvard University and now of the Sargent Normal School), makes this avoidance of local consciousness the very key-note of his method, substituting therefor an intensified general consciousness of effort.

Reconciliation of these two attitudes, one academically scientific and the other purely empirical, but both obviously true, would seem to lie in what has been learned in these experiments, if indeed skill does consist *in a trained fusion of the extrinsic motor ideas and the intrinsic inhibitory conscious control.*

The experiments furnish a certain limited amount of evidence that the resident kinesthetic impressions, of the right arm at least, are relatively inconspicuous in the naïve child, and become more and more manifest with the evolution of motor consciousness and skill, both in general and in particular.

This fact may have important potentialities for future research in the direction of hastening motor efficiency in a properly physiological way. Experiments in this direction are under way, but are beyond our present range.

In our present unfortunate lack of definite knowledge of the special functions of the various kinesthetic end-organs described by Golgi, Pacini, Vater, Kühne, Von Frey, Kölliker, Ruffini, Bonnet, and others, it would be almost rashness to suggest the most likely use of each. The muscle-spindles have often been termed the probable receptors of active innervation; possibly thereafter act in series the Golgi-Manzoni corpuscles at the junctions of the tendons and the muscles, then the Pacinian corpuscles in the synovia, etc., and finally, when the movement is considerable, the receptors in the skin and subcutaneous tissues. Which of these, or whether all, act both for the inhibitory function and for the actuating process that we have called, for lack of better term, the spinal kinesthesia, remains to be worked out. One might expect the arthrodial nervous impulses (32) to be concerned in the actuating processes, as guide-determinants of the initial direction.

The physiology of the reinforcement of the resident movement-impulses by attention to the arm in action is not a difficult matter nor a complex one. As in all other cases, this process leads to a hypertonia of the muscles and this in turn to an increase in the afferent impulses sent inward by the kinesthetic receptors. This increases greatly the delicacy of the (inhibitory) control, the muscles having far more tone and vigor; and defines the sensations better, making them more conscious, thus it may be allowing them to fuse with the extrinsic motor idea (visual and spinal) and meanwhile to dominate the actual movement as representative of the truly personal (cortical) will.

III. Confirmatory Evidence

Corroboration a-plenty of the results of the present research seems to be at hand from several different and wholly independent directions, especially in regard to the proposition which we have induced from the records, that kinesthesia is two-phased, one phase unconscious and actuating and the other conscious and inhibitory.

In the well-known research of Mott and Sherrington (53), the cutting of the afferent roots of one side in the ape from the fourth cervical to the fifth thoracic, led them at first to conclude that the 'will' was abolished so far as the arms were concerned by thus withdrawing the peripheral kinesthesia from the central nervous system. Munk's prompt repetition (54) of these experiments, while showing also how vicarious and withal how widespread voluntary control may be, demonstrated that when recovery of function had taken place, the movements were invariably *inexact* and *more extensive* than under normal conditions. *Per contra, sed idem sonans,* our experiments have shown that when the conscious movement-influences are exaggerated instead of abolished, the movements tend to be true to the extrinsic motor ideas but less extensive than normally. In other words, again, the resident kinesthesia is inhibitory. Other evidence for this is often at hand in ataxias, functional or organic, when of peripheral origin, the movements then showing clearly a lack of normal restraint. Moreover, it is not improper to suggest the possibility that the reason that all short movements tend to be made too large lies in the insufficient innervation 'reflexly,' so to say, of these inhibitory influences.

The neurotopographers have already found evidence for two separate kinesthetic pathways in the brain if not in the cord. v. Bechterew (5) without dissent says, "Nach seiner (Lewandowsky's) Auffassung setzt sich jede Bewegung aus zwei Komponenten zusammen, aus einem bewussten und unterbewussten Element." Bastian, too, appears to agree with this, from data purely neurological. Again, Russell and Horsley (61) express one of the results of their work on spatial orientation in this manner: "It is therefore, we submit, perfectly clear that what may properly be termed the midaxial line and region of the hand and forearm, is as definitely represented in the cerebral pallium as it distinctly is in the spinal cord." "The spinal representation of tactility [a variety of kinesthesia] finds an echo in the arrangement of that function in the sensory cerebral centres." It is not obvious why there should be this double representation unless these two sets of kinesthetic centers have in some way different functions. Those familiar with the recent brief but important symposium (6) on muscular representation in the grey cord and in the great cortex, would perhaps be inclined to believe that the 'individual' skeletal muscles are mapped out in the pallium only for some very special purpose, not discoverable by electric stimulation. That purpose now seems to be inhibition (through the resident kinesthesia).

In a closely allied realm of neurology, facts have already combined to form an essential connecting link in support of the main result of our research that the conscious kinesthesia (and by consequence, voluntary movement) are inherently inhibitory processes, just as all other kinesthesia is actuating in action. Perusal of the consensi of competent opinions published in the splendid volumes of Lewandowsky, v. Bechterew, Cajal, and others, makes it extremely probable that there are two main courses for the carrying of kinesthetic influences into the gery regions of the brain. Grant Van Biervliet's finished work (70) already referred to, and there is no longer need of seeking separate pathways or centers for tactile and movement sensations; and on this basis several discrepancies in neurotopography disappear. Touch is impossible without muscular movement; and the sensation of movement in some of its phases is but an inner touch,—indeed the blind know it by no other name. For example, the path ascribed by Cajal to tactile impressions corresponds as well to the 'somaesthetic' tract of Barker and to the kinesthetic tract of Starr as one need expect in the present relative indefiniteness of these complex matters in neurology. There is sufficient agreement already that an important avenue of ascending nerve influences extends from the kinesthetic receptors through the latero-posterior cord, the lemniscus, etc., with relative directness to the cortex of the great hemispheres. Many circumstances suggest that this route, relatively simple and direct, conveys the conscious inhibitory impulses that we have termed the resident kinesthesia, characteristically voluntary or personal.

There is, however more general agreement as to the topography of the indirect ascending pathway between the afferent grey cord and the cerebral cortex; this passes through Clarke's column, (the cerebellar bundle of Flechsig), the vermis superior, the mysterious Purkinje cells, and the thalmic ganglia (van Gehuchten). Barker suggests that ten or more neurones may be ordinated in this passage! This many-swirled flood of neural currents passing to the cerebrum represents, of course, the hereditary spatial moto-sensory outfit of the individual. For our purpose here and now it stands for that complicated actuating innervation of the voluntary musculature under the control of vision, audition, etc., in combination with the positive, impelling, impulsive kinesthesia centered in the spinal grey. It surely is intimately connected with the whole sympathetic disposition of the personality, with his feelings and emotions, with his whole intellectual memory and experience, and its intimate relationships involve the whole brain and much if not all of the whole nervous system besides. On the other hand, we have to suppose the more direct, conscious, and inhibitory, tract to convey the occasional, difficult, or wholly new personal *effort* of the individual as a personal will, related with especial closeness to the cortex of the hemispheres. Our every-day, ordinary behavior uses this tract extensively; but skill and novelty employ it intensively as the road to even greater efficiency.

Hollingworth's recent work (41) in the Psychological Laboratory of Columbia University correlates its results so far as kinesthetic matters are concerned with the opinions of Sherrington, Titchener, Angell, Woodworth, etc., but makes the ideas more explicit: "Attempts to find a single anatomical or topographical source for the sensation which serves as a criterion of extent of movement are contradictory and futile. * * * * A movement comes to be recognised as larger than others, not because it produces a more intense sensation, nor because of any geometrical correspondence of internal and external points, but because it has been *learned to be a larger movement,* one that will effect a greater change in an object with which we are dealing." Without reference to the fallacy here similar somewhat to the ancient one asking why we see objects erect when the retinal image is inverted, it is obvious that herein is a definite opinion (derived from long experimental work made in the laboratory of men as cautious as Cattell and Woodworth) which confirms our own experimental result that extent of movement is an unconscious function of the spinal grey in closest association always with brain and cord and especially with the cerebellum. A larger movement has been "learned to be a larger movement" by various experience indeed and the judgment gets no help at all from the conscious kinesthesia in this respect. Sometimes the information comes only with difficulty. Over and over in our experiments, records not a half or a third as large as the norm, whether kinesthetically or visually perceived, were reported by the agent as large enough or even too large. In other words, the local attention-strain of true voluntary movement is not translatable into size or extent. The unconscious actuating aspect of kinesthesia takes care of extent, probably, and in terms made up of energy, speed, habit, momentum, gravitation, and other things far too complex to be described at present. The activity of the conscious phase of kinesthesia exerts an entirely different kind of control over action, a control that is truly voluntary.

Further evidence, more or less direct, of the validity of this point of view may be had from the observation of the development of voluntary control during the first year of life. One fact which has regularly been overlooked in genetic physiology and psychology is the completeness of the inhibitory apparatus and activities at birth. See for example the writer's observations (14) on the little girl 'L.,' made in 1899. A few months later in the ontogenetic evolution, when deliberate control begins to be obvious, inhibition seems to develop or at least to be apparent somewhat before voluntary actuation (14, p. 195). Guyau recognizes the fundamental duality of the will-apparatus, as the following quotation attests: "La pleine volonté, c'est-à-dire le deploiement total des énergies intérieures, suppose qu' à la représentation de l'acte même qu' on va accomplir s'associe la representation affaiblie de l'acte contraire. Et ainsi, nous arrivons à cette conclusion: Il n' y a pas d'acte pleinement volontaire ou, ce que revient au même, pleinement conscient, qui ne soit accompagné du sentiment de la victoire de certaines tendances intérieures sur d'autres, conséquemment d'une lutte possible *entre* ces tendances, conséquemment enfin d'une lutte possible *contre* ces tendances." Ribot's whole psycho-physiology is permeated with this functional duality in action, as all the world knows. The evidence, experimentally produced, that this two-phasedness of the will extends throughout the action-system in a literal way and even to the kinesthesia motivating or actuating and controlling muscular coördination, adds only another factor to our knowledge of how we act.

Weir Mitchell, forty years ago, published (51) material (derived in part from the soldiers of the great War) which throws some light on the neurology of kinesthesia, especially in its relation to actual consciousness. From this, one of the most extensive investigations of the sensations of lost limbs ever made, he produces many most interesting inductions. These statements following directly bear more or less on our research-results, and, in a way, complete them. "Involuntary movements of the absent toes or fingers are frequent and in very many persons are unfailing precursors of an east wind. [*Cf.* with an inflamed joint's ability to foretell the approach of a storm.] Sometimes only one finger is thus active, or the digits flex one after another, and then slowly extend, whilst wrist movements are exceptionally rare, and the elbow and knee are never felt to change place at all.[2]

[2] It certainly is suggestive that the fingers, the most conscious and best controlled parts of the body (save the eyes and tongue) should be those most persistently and most frequently felt under these conditions. The fact suggests more of conscious inhibition in them than elsewhere in the body.

"Leaving out of the question cases which have no sense of the presence of a lost limb, we find that in a very small number there is no consciousness of power to stir any part of the absent members by force of will. All others are able to will a movement and apparently to themselves to execute it more or less effectively, although in most of the amputated such phantom motions are confined to the fingers or toes, which rarely seem to possess the normal range either of flexion or extension. Yet the certainty with which these patients describe the limitations of motions, and their confidence as to the place assumed by the parts moved, are truly remarkable; while these restricted movements are pretty surely painful, and the effort is apt to excite twitching in the stump. A small number have entire and painless freedom of motion as regards all parts of the hand. 'My hand is now open, or it is shut,' they say. 'I touch the thumb with the little finger.' 'The hand is now in the writing position,' etc. Between these cases and such as are conscious of an immobile member, every grade of difference as to motion is to be found, with equally wide varieties as to the associated pain which perhaps is most acute in such as will with vigor a motion that they seem to fail of executing.[3]

"The volition to move certain parts, continues Weir Mitchell, is accompanied by a mental condition which represents to the consciousness the amount of motion, its force and ideas of the change of place in the parts so willed to move. The physiology of the day accepts the belief that all of our accurate notions as to the amount of power put forth and as to the parts thus stirred reach the sensorium from the muscles acted on and the parts moved. It would appear, however, from the statements here made as if coevally with

[3] Here we see restrictive kinesthesia, by reason of some unknown abnormality in the spinal or other centers, so intense as to be painful—just as all sensations can pass over into some experience at least very unpleasant. If we compare the pain here produced with that made in a real anchylosed joint by a strong attempt to move it, we shall find great similarity. In the latter case the pain is obviously due to the overstimulation of the arthrodial receptors, and we may not improperly suppose that the former is due to some sort of an overaction (exerted from the 'reservoir' above?) upon the actuation-centers of the brain-stem, compelling a painful overstimulation of the inhibitory mechanism, but central rather than peripheral in origin in the real joint. In such places and cases as this it is easy to satisfy the craving of those who deny absolutely all that they personally happen not to believe, by over-hypothesizing but let us refrain; the very complexity of the conditions referred to serves as a sort of argument for our present contention of a kinesthetic duality.

the willing of a motion there came to the consciousness, perhaps from the spinal ganglia acted upon, some information as to these points. If, in reply to this, I be told that the constancy of long habit may have associated memorially with certain ganglionic activities the ideas of local movements, I should hardly feel that this was an answer, because in some of my cases the amputations took place so early in life that there was no remembrance of the lost limb, and yet twenty years after, a volition directed to the hand seemed to cause movement which appeared to be as capable of definite regulation, and was as plainly felt to occur as if it had been the other arm which was moved. Probably, then, a part of those ideas which we are presumed to obtain through the muscular sense are really coincident with, and necessitated by, the originative act of will or else are messages sent to the sensorium from the spinal ganglia which every act of motor volition excites."

As a monistic interactionist, so to say, the writer is willing to accept either of the alternatives suggested at the end of the above quotation; but to him as an out-and-out physiologist (as ignorant as a biologist of anything like metaphysics), the latter of the two seems the better. Especially does it seem the better because it undoubtedly adds an important item to the neurology of kinesthesia: namely, the needlessness of supposing that literally all kinesthetic impressions come directly and separately from the joints, muscles, tendons, bones, viscera, and skin of the moving parts. We may properly suppose, on this timely cue, that *the spinal grey is so intimately aware of all that goes on in the action-systems* (through tides of intelligence unifying all parts of the circuit) *that not only actuation but (inhibitory, conscious) control can arise and be exerted there from further up, from the great cortex.* In other words, here is direct evidence, hard to refute, that the cortical grey acts by and on *movement-complexes* represented not only in the cortex but in the grey matter of the afferent cord, the musculo-(et al.)spinal circuit being an unity in the most literal and absolute sense. This is precisely in line with the tendency in interpretations of the action of the nervous system—indefinite conscious and subconscious grouping and subordination and "symbodism" beyond present detailing (see below).

Finally, for the present discussion, Sherrington's classic work (64), especially that part of it relating to the reciprocal innervation of muscles that are functional antagonists, doubtless has direct bearing on this corresponding division of the afferent movement-information into inhibitory and actuating. Nikolaïdes and Dontas's demonstration (55) of inhibitory neurites in the frog's motor nerves (confirmed by Wooley and Fröhlich) is also suggestive of further knowledge to come. In general, however, the inhibition of a voluntary movement in current control through its conscious resident kinesthesia relates more to molar inhibition by groups of antagonistic muscles than to inhibition within the influence of a single neurone. One group of muscle-units actuates a certain movement and the movement is usually useful to the agent only when balanced and restrained by another group under conscious kinesthetic control.

Aside from the fundamental discoveries by Sherrington (64) already referred to, Lee and Everinham (48), Camis (10), and Forbes (29) have recently shown that in some mode or other separate kinesthetic paths or "nerves" control separate groups of effector neurones which in turn supply respective muscle-groups, so that we know now that there is far more division of labor among at least the afferent neurones than was suspected formerly, so far as their control of muscle is concerned in particular. Our research seems to go one step still further in this differentiation, suggesting, as it does, additional subdivision of the receptor pathways into actuator and inhibitor as already set forth. This seems to complete the logical requirements of mechanism for the understanding of the actual behavior, complex far beyond actual description at the present time.

IV. The Impulse to Activity

Just as the essence of life considered for the purpose as a 'material' process is movement, movement universal in time, in place, and in meaning, so truly is the essence of the function of the nervous system to coördinate these movements, whether molar or molecular, whether wholly within the body or the body as a whole with its particular environment. The very meaning of protoplasm physically speaking, is motion. This universal inherent necessity of movement in organism is known in psychology and in educational theory as the impulse to activity (and all too late has its universal necessity been recognised!). Sometimes a fully conscious thrust, sometimes pushing quite as hard but all unconsciously, this all-pervading vital impulse is the motive power of all achievement. It represents *actuation* of both the vegetative and the personal in human efficiency, just as the conscious personal will stands for restraint. The greatness of its importance in education and in explanatory psychology we need not pause to more than mention.

Through many thousands of varied receptors the ever-active tissues start and send a flood of impulses inward to the central nervous system, (the sympathetic partaking in this primal function); and this flood stops only with the abolition of protoplasmic, especially muscular, tonus, at death. This obviously is the 'spinal' kinesthesia described to some extent above, fused intimately so far as service goes with the visual, auditory, and other receptor impressions, especially the first. This flow from numberless motile springs keeps full the reservoir of kinetic or potential energy on which the whole effector side of the organism's activity continually draws for its life so far as coördination and guidance are concerned. These obviously underlie and determine the entire motivity, both vegetative and personal, of man.

Complex beyond conception is this basal kinetic impulse, psychologically as well as neurally; and we shall not, nor need we, make any attempt to describe either its phenomena, or its mechanism in anatomical terms. It stands for the motivation of behavior and its apparatus therefore must include every active factor of the psychophysical organism that makes for mere activity, for unguided, unvoluntary movement. Conspicuous in this ultimate or nearly ultimate integration is, of course, the whole conscious and subconscious process of emotion, and the two basally opposed feeling-tones of euphoria and dysphoria so essential in efficiency as well as in human happiness. In closest relationship, too, with this, the vegetative back-bone, so to call it, of the organism, are all the sensation-'centers' (spinal possibly and surely thalmic) and all the psychomotor 'centers' concerned in the motor adaptations of each of them. In short, the whole nervous system more or less is at once affected by and affects the bodily mechanism of the impulse to activity, behavior's motivation-tracts.

Behavior, indeed, physiologically viewed, is controlled motivity and the control is the circuitous guidance of the reflex and voluntary kinesthesia. Thirty-five years ago (a long time in the history of the feelings of movement), Stanley Hall, in a neglected article in *Mind* (34), expressed the basility of its nature thus: "The feeling of motion we have shown is the simplest, earliest, most universal, known, psychic rudiment of animal life. It is distinguished from every other sensation in being identical with its objective cause or aspect which is also motion." This uniqueness of kinesthesia has been too little appreciated in psycho-physiology, and its significance hardly understood in its metaphysical relationships as well as in its physiological bearings. The impulsive phases of the motion we should now think of as subconscious, the sensation as conscious, representing intensive control, coming rather from the voluntary personal inhibitory guidance, as already set forth.

Let us now look for a little at a rare condition known in its typical form to nerve-specialists as *abulia,* for by so doing we shall accomplish two things at once, more or less completely: we shall illustrate the absence or negation of the else universal impulse to activity, and perhaps help to explain this interesting condition.

The term abulia is Greek for will-lessness, absence of volition proper; the research and literature of the subject is mostly French. Rare or perhaps unknown in childhood (in itself an interesting theoretic circumstance), abulia is found oftener in women than in men, and consists simply (so far as external phenomena are concerned, it is simple enough) in a functional inactivity, a will-lessness carried to any extent, it thus being sometimes a matter of life and death. There is no loss of muscular power and no disturbance either of the sympathetic vegetative musculature or of the reflex habitual behavior, the abulia relating solely to the deliberate motivity. Understanding in simple abulia is unimpaired, so that the patient may have the most vivid concepts of the necessity of action. Actual cases are often complicated, of course, with many phases of hysteric, neurasthenic, "psychasthenic," and psychiatrical conditions, but pure abulia is inherently a lack of deliberate conation.

One of the natural results of the obvious exhausting 'overworking' of the Rolandic area as the recondite 'center of voluntary movement,' is an awakening of interest in this condition. Logically speaking, it is the only known defect in voluntary action proper, for it apparently is due neither to a true paralysis of nerve-knots nor to disturbance in muscles or their nerves. Abulia is of especial interest on this account.

The abulic's ideas that she should or must act are unexcelled examples of mere ideas, associative products that are of service to no one and to no thing—the grist of intellectual mill-wheels that can only be thrown away when it is ground. The very notion of ideas unexpressed in any way, unmixed with feeling-tone, unmaterialized by will, has in it something ironical, something artificial and unreal as if the by-products of the language-processes characteristically human to be sure, but none the less with inherent liability to useless action—like some neglected grist-mill by the brookside whose stopping-mechanism has been deranged, and which, until worn out, makes noise and dust and jolt and jar beyond all reason. In some such mode appear to the writer to be any association of ideas never used because never materialized in real bodily action and thus related to the rest of Nature. Such, in a sense, are the psychologically interesting ideas of the abulic that she should act, must act,—and yet does not. Mourre, for example, considers abulia to be inherently the inability to transform an idea into its act as the product or consequence of victory among antagonistic ideas. The abulic, he says, knows that she wishes to do a thing and at the same time knows that she does not do it, because the inhibitory association of ideas prevents. In true abulia, he thinks, the patient deems her will free; but she fails to act because she chooses not to do so, fearing she might fail. There may be also, he supposes, a diminution of the vital sensations and desires, making the motogenic balance of pleasantness unusually small. The physiology of abulia, Mourre concludes, is some organic neural disturbance, while the psychology is the difficulty of voluntary effort.

Sometimes abulia follows an asthenic infectious disease, *e. g.*, typhoid, as in a case described (59) by Raymond and Janet.

There is no other condition quite like abulia. Melancholia, katatonia, certain phases of paranoia, and even some cases of dementia and of amentia, exhibit somewhat similar external bodily conditions (that is of deliberate inaction), but in all of these the conscious motivity is wholly different in ways both too familiar and too complex to allow of present rehearsal. Here in abulia alone have we a defect in the very physiological *motives* of the personal activity.

One is reminded inevitably of the numerous persons who, it is customary to say more or less sarcastically, were 'born tired,' who are in short, to the uninformed lay-mind, lazy. The world still has in mind the dire hook-worm's timely vindication of his thousands of victims in the South, and now perhaps it is another (but relatively small?) group of inactives and inefficients, the mild abulics, to whom we owe the courtesy of excuse. The king of optimists himself would not wish to deny that there are plenty of individuals of many sorts whom habit and inclination have made truly lazy; but certainly there are many others whose tendency to inaction must be classed as a mild degree of abulia, those especially with a psychasthenic *Belastung* whether constitutional or acute. In thinking over the persons of this type in one's acquaintance, one finds, in surprisingly large per cent, evidence of deranged metabolism of the depressive trend, a low metabolic plane of efficiency with the balance leaning toward mal-nutrition. Physiologically these individuals oftentimes are cases of functional abulia of a mild type, but more complex and so harder to unravel than even the well-pronounced cases with the voluntary mechanism obviously deranged.

The one indisputable argument for defense in these conditions bases itself in the fact that to a thoroughly normal person, activity is pleasant. To the sound mind in a truly sound body a normal amount of suitable work is a delight the most reliable if not the most intense, of all human delights, whether it come from the deep action of the diaphragm or from the normal routine of the day's vocation, whether 'sport' or 'work.' Three conditions may be noted in passing, related to this matter that are among the hardest of the great world's cruel facts and which have gone far to prejudice mankind against the idea of work: *1.* multitudes are engaged in ill-adapted and hence uncongenial work; *2.* multitudes, including these former more or less, are working longer if not harder every day than a later age of economics will allow; *3.* multitudes, more even than the others, are not in that normal neuro-musculo-visceral vigor to which even proper work is a gratification. Had the world as yet ameliorated out of these harsh primeval conditions of a livelihood, one of the well-based prejudices of our race, that work is hateful and to be avoided, would already be of only historic anthropologic interest like the preglacial distribution of the hairy Ainu. All this to introduce the negative premise that psychomotor activity under perfectly normal conditions of mutual adaptation between the individual and his work renders a distinct balance of pleasure to the maker—yet work, unlike virtue (Twain), is not its only reward. Perhaps it is unnecessary to point out that exertion renders its inherent recompense largely through the exercise of the sense of kinesthesia—the music of the feelings of movement, tribute of the rhythm of katabolic expending, the great joy of motion, index of our life.

We may properly go a step farther without fear of serious contradiction, and say that when this euphoric balance is lacking, bodily activity in general tends to be reduced to a minimum; on the other hand, when the truly metabolic bodily activity is lacking (tonus being the true index of this activity) the euphoric balance is zero or actually dysphoric. Those of my readers who have suffered, for example, from a long exhausting illness will readily appreciate at first hand this state of atony in soul and body—a will-less condition of carelessness, passivity, a *laissez faire,* not of necessity, perhaps seldom, painful, but wholly negative so far as deliberate action is concerned, and often so to a very surprising extent. Matters which a few days or weeks before seemed of the utmost personal and family importance are now, in this functional abulia (so to call it) completely indifferent, whether they be done or not—the fortunate condition which very frequently indeed, normally in the aged, deprives death from disease of all unpleasantness to the chief actor in the common tragedy. The main factor in physiology of this condition of illness is certainly an akinesthesia arising in the decadent atony of the body and soul. When one's foot "goes to sleep" from pressure on the nerve there is a condition oftentimes almost purely akinesthetic and one interesting for the ready observation of voluntary movement lacking in the local or resident kinesthetic influences. Compare it with the exalted consciousness (agony) of the opposite state as exemplified in hydrophobia and in strychnine poisoning and its tenor is still more clear. In nightmares, too, sometimes there seems to be a more or less general tonic spasm of the muscles so that the sufferer is unable to move against the opposing terror, to jump out of the way, to cry out, or what not. Perhaps in these cases the painfulness of the experience is due to over-stimulation of the conscious inhibitory kinesthesia, for some reason for the moment not under voluntary control. By force of will made up in the waking state and impressed on the subconscious, this cramp in the nightmare may readily come under habitual voluntary control and its pain thus be avoided. (Personal communication from John Frederique Herbin.)

The writer has already in various places (especially 15) set forth, in a manner more or less orderly, his particular ideas as to the neuro-metabolic basis of the fundamental emotional tone of euphoria or of malaise. The primal sympathetic centers are concerned and without much present doubt (38) the optic thalmus, both being, of course, always in the closest possible and most intimate relation with the remainder of the cerebral and spinal grey. All these grey nuclei of the vegetative life (representing as coördinators or adjustors the moods temperament, dispositions, organic habits of every phase, degree, and permanency) are probably under the unceasing influence of those afferent neural influences which we have called the actuating kinesthesia. The reservoir of nerve-center energy, thus largely supplied with tone and information, furnishes at once the nervous motivity and the conscious incentive, which, because at least mildly pleasant, normally urges to activity. When lacking this pleasant tone or in such a condition possibly of 'block' that knowledge of this pleasantness cannot come into the brain (save *e. g.*, by way of suggestion or hypnosis?), there is the functional condition of abulia, or so it seems likely.

To our present understanding, then, the abulic state, whether thus 'functional' or organic, whether primary or secondary, is a defect or a default in the impulse to activity. It is represented inherently, we may neurologically conjecture, by some disturbance in the actuation of the voluntary muscles probably as much central as peripheral. In abulia, obviously, the muscular tonus is much lowered and this greatly lessens the kinesthetic influences. On the other hand, the spinal afferent grey also has lost much of its tone, partly because of local mal-nutrition perhaps (a decrease of the chromatin?) (25), but partly too probably because its normal stimuli or supplies of exciting energy from the muscles are partly cut off in part too doubtless by some unknown derangement of the central adjustor-mechanism. This intimate inter-dependence of the musculatures, especially the voluntary, and the will need not be detailed here. The kind and degree of will that would be possessed by one 'completely' paralyzed is too esoteric for scientific discussion; meanwhile no case of truly complete paralysis (that is, an instance in which the central innervations, afferent and efferent, of the muscles, the essential thing, are abolished) has been described, for a person in this condition would be practically dead.

Another condition involving deficiency in the common impulse to activity and probably in the general kinesthesia fundamentally, is *feeblemindedness,* now so prominently before the educational and economic world. I am not familiar with any definite information or even suggestions as to the etiology of this important condition, the ordinary theory being simply that it is related to some arrest or mal-development of the cerebral neurones. Granting that this is likely, it is interesting to look further at the implications of this defect.

We have already sufficiently pointed out the flood of nervous influences that is continually pouring into the nerve-axis, chiefly from the action-system, the skin and the viscera; and we have indicated that these influences become integrated, in the higher levels of the cerebral grey, into conscious (largely inhibitory) motor ideas or into emotions or emotional tones which act as resonators and so produce the effect, the '*animus,*' behind and beneath all voluntary behavior. Only a brief consideration is needed to convince one how important these streams of kinesthetic and coenesthetic impulses must be in the mental and bodily deficiencies of feeblemindedness.

Whether central or peripheral (more likely it is both), the afferent feeders of the psycho-motor reservoir are probably determining factors in this condition.

It has scarcely been sufficiently emphasized as yet by competent writers on feeblemindedness how frankly motor is the basis of this defect, phylogenically or ontogenically,—the latter, the personal, factor playing perhaps the leading rôle. This factor is getting more and more of its due importance. For example, in the very latest discussion of the brain of a very young human foetus, the authors (7) say: "There is very definite evidence that, in the complex of phylogenetic and ontogenetic factors which subserve the process of evolution, the latter from a very early period play a predominant part."

Norsworthy (56) finds no difference in kind between the feeble-minded and the normal and concludes that "in general, at least so far as intelligence is concerned, idiots do not form a separate species, but simply occupy a position at the extreme of some large distribution, probably approximately that expressed by the normal probability curve." But, of course, idiots are not especially efficient members of society, and an underlying defect very general in nature must be sought in some phase of the moto-sensory organism. Shuttleworth and Potts (67) certainly suggest a forward step in their remarks on the sense of 'touch' (kinesthesia) in the feeble-in-mind: "The tactile function is not only the most general, but in some respects thè most important of our senses, and in the normal baby its evolution takes precedence of the rest. Impressions through the eye and ear are criticized through the sense of touch, and this natural development, so serviceable in the spontaneous education of all healthy young animals, [cherche le chaton!] must be imitated in our endeavours to bring up towards the normal standard the sensorial training of imperfect children. In some cases we shall find coarse, insensitive hands which must be drilled into sensibility by grasping hard and soft objects and discriminating the resistance of and the surface impressions of such varying substances as polished marble, sandpaper, velvet, silk, etc. * * * * Such lessons will, of course, form incidents of the object-lessons which play so important a part in early education."

Unless the present writer is misjudging, there is here suggested the defect which above all others is fundamental in the etiology of feeblemindedness. The statistics recently published by Goddard of the Vineland Training School (31), from measurements made in nineteen institutions by as many observers, will be of great importance in the general theory of the relations of body and mind. These show a correlation between birth-weight and feeblemindedness (the feeble-in-mind being heavier), and between growth and feeblemindedness, the growth of the defectives being much more erratic, earlier to stop, and less in the total than is the case with normal children and adolescents. Growth, it must be recalled, is a reaction to stimuli; and the stimuli in defectives are probably deficient in some way. Here is what one might term a structural ataxia wholly homologous to the functional ataxia which is the one always obvious element in the behavior of these inefficients (aside from the moral "degenerates").

G. E. Johnson (43) indicates that in the feeble-minded the spontaneous movements, which are, of course, the most conspicuous phenomena of this class of defectives, are "of the fundamental kind and not accessory," meaning thereby that they are confined to the gross muscle-movements, primary or elementary, in all our animal life rather than to the delicate adjusting movements of personal control. Swaying of the trunk, motion of the jaw, swinging of the arm, rolling of the head, or the simplest finger-movements are among the most prominent of these fundamental movements. Moreover, says Johnson, "in the willed movements the difference between the control of the fundamental and of the accessory muscles was much more marked in the feeble-minded than in normal children. This was the more noticeable the greater the degree of idiocy. Some who could execute gross movements with regularity and control, were wholly deficient in the execution of finer movements. Even those who walked strongly were utterly devoid of the grace which results from a well-developed sense of muscular coördination and control. Nothing is more striking than the clumsy awkwardness of idiots. Sometimes where the control of the fundamental had been nearly perfected, there seemed a positive gap, as if the accessory had not developed." The proportion of finger-movements to arm-swings in the feeble-minded to those in the normal boys of like age (13.6 yrs.) was as 81 to 93.6. Here certainly is evidence that a deficiency in the conscious kinesthesia if not also in the unconscious actuating kinesthesia, is probably a factor in feeblemindedness whether the lack be central or peripheral or both.

Down (26) says: "Want of muscular coördination is the great fault of the feeble-minded."

Goddard in a personal communication to the writer says: "We have no data at all on the question of blind-folded subjects except incidentally; some of our visiting psychologists and others have tried to do [the form-board test] blindfolded and as I remember it, their time was often as much as three minutes. I should expect that the majority of the feeble-minded would never get the blocks in place." The form-board as most of my readers doubtless know already was an invention of Seguin and as adapted by Goddard consists of a board "of wood an inch and a half thick, sixteen inches wide and twenty-two inches long, having in its upper surface ten more or less variously shaped depressions into which are loosely fitted wooden blocks of corresponding shapes [but thicker],—a circle, a square, a rectangle, triangle, star, diamond, hexagon, half-circle, and oval. The test consists in placing the blocks in their proper depressions, and the method, accuracy, and speed are all factors in the ensuing judgment of the child's mentality" (A. Holmes: 42).

Without multiplying such evidence of the aimless and ill-directed gross motivity of the feeble-minded resolved into akinesthesia, we may reasonable express the tentative opinion[4] that perhaps even the essence of feeblemindedness is some lack in the kinesthetic receptive fields or centers. These influences or stimuli continually bombard the central adjustors and so give rise to the activity which, guided and helped by the cerebral grey (biological will and intelligence and motive), results in curiosity, interest, psychophysiological evolution and education. Education is the reaction of personality to its environment; and reaction is inconceivable without kinesthesia. Certain is it that I at least can get no clear concept or understanding of the vegetative impule to activity in a complex organism, which lacks that kinesthetic deluge of energy into the reflex motor centers by which directly the voluntary muscles (whether also under vegetative dominion, or not) are actuated. In short, the 'attitude,' actuating and inhibitory, unconscious and conscious, is the creation, to a greatly predominant extent, of kinesthesia.

V. Conscious Control

Corresponding as homologue to the actuating sub-conscious or unconscious vegetative impulse to activity is personal conscious control of this activity and of the tendency thereto. How large a part the resident movement-sensations play in voluntary control of body and mind is little realized by the great world of practical educators (manual-training instructors, gymnastic teachers, school-teachers, etc.), not less than

[4] This indeed should not be hard to prove or disprove histologically by one fitted and inclined to the important research.

by the world's other army of industrial workers and employers. Some of the thoughtful psychologists,—as may be seen from the year's and last year's researches,—are awake to the pressing need of application here, application whose reward will surprise many by its size. Here lies the road which he who reads may run. That the road, however, is none too well known, even to extensive travellers along biologic highways, is illustrated in Professor Lehmann's extensive new "Grundzüge der Psychophysiologie," wherein, out of 742 pages, kinesthesia occupies less than three! But with this we are at present not further concerned. Kinesthesia is coming into its own theory, as it is sure to come into its own practice. See, for a recent example, Ioteyko and Kipiani's work (42 A and 42 B) on the ease with which children are taught kinesthetically to write and to draw; compare Montessori's general method (51 A).

H. C. Warren, for example (72), promptly recognised it as the reason for the extraordinary success (whether advisable or not!) of Doctor Maria Montessori in teaching the little children of five to write: "On account of the careful preliminary training in the motor equivalents, they form letters more accurately than the ordinary child who is merely attempting to imitate a visual copy. The words which I actually saw written by children of five and six were far more regularly formed than those of most ten or twelve-year-olds taught in the ordinary way. Moreover, the act of writing really interests the child, and he is constantly practicing it and improving his chirography." In inculcation and habit of 'motor equivalents,' underlain always by intensive kinesthetic attention, lies the undoubted short-cut to motor skill, as the writer has been preaching in a desultory way for some years. At present our concern is briefly with kinesthesia as related to the personal will proper.

It would be fatuous to rehearse the important considerations, however interesting when new, by which we have learned to regard the highly civilized and cultured adult as a product of personal (or of inherited) inhibitions or restraints of instinctive and reflex tendencies common to the more complex brutes and to savage man. Yet, few theoretical things are of more importance than this if we would truly understand the human personality. Man is man indeed only because racially and personally he has grown into the habit of inhibiting himself from brutishness or at least from savagery. Sociologically and biologically the essence of manhood is restraint,—inhibition of his impulses to untoward activity. Vegetative behavior is inherently impulsive, reflexly conative, but a *man* is characteristically *an inhibitory agent with much,* complex and various, *to inhibit.* That is, in brief, the human impulse to activity is a most complicated algebraic correlation of forces or tendencies, full of new conditions to be met continually, and demanding, therefore, ever new combinations of the sensation-influences which orient him in his material and spiritual environment, and which constitute him, and thereby constitute him, man.

In general, then, conscious control, being a matter for the greater part of the cross-striated musculature, is apparently dependent on the conscious inhibitory kinesthesia, since these nerve-impulses, as we have seen, are especially related to the careful, that is, personal, adaptation to needs in skill, in grace, in every kind of efficiency dependent on adjustment as distinct from actuation. Less inadequate discussion of this phase of the feelings of movement is reserved for another paper when further experimental evidence of its importance can be offered.

VI. Notes on the Neurology of Meaning

From a largely physiological viewpoint we have, in the foregoing, described much that properly may be said to be the basis of the will. The will, however, is more than body in action, more even than the neurocircuits which control its action. Will has mental aspects as well as somatic, relations of human personality outside of matter, *meaning* as well as motion. In the results of the preceding research-work, particularly in the duality of kinesthesia's phases, there is obviously the suggestion of a contribution to the mental aspect of volition and, indirectly at least, to the euphoria-dysphoric basis of feeling.

Part of our mind's meanings are firmly related to the vegetative mechanism, hereditary and mostly subconscious, which underlies the more properly personal elements of our behavior. On the other hand, many meanings, equally efficient or more so as stimuli to action, are characteristic of the personal freer side of our common nature and may have only indirect relation to that part of ourselves that defends us, feeds us, and leads us to the getting of children. Perhaps it will seem not improper if we quote from a previous article on apraxia (18) the following hasty outline of a typically vegetative meaning, namely the meaning of space. This is an all-important meaning for humanity, as Immanuel Kant long ago expounded and many, notably Lotze, since. So far as behavior is concerned, space is, of course, one of the substantial frameworks of our experience.

As the writer has elsewhere (*Moto-Sensory Development,* 1910) in a general way suggested, observation of the development of voluntary movement in the infant during the first three years or so (a much-neglected medical field) indicates that power of deliberate action, save in the simplest innate movements of reaching and clasping with the fingers, develops *pari passu* with the conceptualization of space. This, we may not doubt, is much more than a coincidence. The infant is at first an all but reflex thing, moving under the stimulus of the inherent impulse to activity, and he becomes a free agent, personality, only as he becomes aware of the empirical three dimensions in which his body, like his wholly objective world, is constructed, and in terms of which alone initiative movement acquires meaning in any logical sense. In these terms alone to the finite mind is orientation or order: can any one, then, suppose that this universal ground of action is unrepresented in the integration of his will? * * * *

So far as spatiality is concerned (surely in its relation to deliberate movement a case in point) the "reservoir" in the young infant's brain fills from many afferent sources indeed, and they, like other complex neural conditions, are beyond present exact detailing. Because he could not draw them wholly on the map, no one doubts that New England is covered with a network of railway tracks. Among the factors of this working knowledge of spatiality, however, in the evolving infant mind and therefore mechanized in the adult, are certainly these: *A,* the resident kinesthetic influences from the limbs moving so continually under the impulse to activity, and especially those coming from the joints, muscles, tendons, skin and bones of the arms and hands, pioneers in the evocation of the deliberate will. *B,* the kinesthesia from the extrinsic eye-muscles and from such other important and widespread muscles (of the neck, back, abdomen and even legs) as assist in directing the line of sight always on the (stereoscopically placed) foveæ centrales. *C,* the kinesthetic and special impressions integrating into equilibrium and coming from the semicircular canals, the eyes, and perhaps from the protopathic receptors of the abdominal viscera. *D,* in some way as yet hard to understand, the local signs of the surface and perhaps somewhat of the interior of the body, especially of the mouth cavity. *E,* the motor mechanisms of each of the senses so far as involving adaptive or adjustive muscular movements that afford awareness of direction or of space directly, for example the tongue, the retina being a conspicuous other example on the olden-time natavistic theory of spatiality. All these and probably other afferent influences gradually teach the baby that there is a *depth* for his deliberate exploration. A little later the kinesthesia from the locomotor mechanism, that of creeping, tumbling, walking, etc., enlarge and vigorously confirm the spatial concept thus intrinsically developed. Into the details of these various factors of original space-conception we need not even attempt to go, nor is it necessary to do so in order to be assured that their afferent impulses come from every portion of the body and directly or "symbolically" involve every portion of the brain. One need only consider how widespread is the visual "tract" through the hemisphere 'fore and 'aft; how expanded and how involved are the auditory centers if we include the essential vestibular functions; how spread-out and interknit are the taste "centers," those of smell, of touch (shown by Van Biervliet recently to be a phase of kinesthesia) and of orientation, equilibrium and local signs. Such in the sketchiest of outline only is something of the brain-neurology, it may be, of the afferent intelligence that fuses into the fast-growing infant mind as a basal working knowledge of the space in which alone he can move and so have his being.

In proportion as the individual intimately knows this space as represented to him thus by the nervous system is he capable and skillful and efficient, master of himself and of his plastic environment. In short, in the neural (and muscular) mechanism that develops originally a personality's notion of space we appear to have the foundation more or less of the neuro-muscular mechanism of voluntary movement in its most obvious details and framework both at once, and what develops in the infant becomes the (often subconscious) determinant of the behavior of the adult.

Let this be taken only as an example of a whole group of meanings in which body and mind exhibit, both subjectively and objectively, their inherent unification in some transcendental mode that we cannot understand.

Obviously kinesthesis is the chief immediate means to our percept and our concept of spatiality.

Another whole important range of meanings, namely those of the emotional sort, have their ground more or less frankly in the kinesthetic and coenesthetic influences (the latter being largely kinesthetic). Exposition of these meanings would furnish material for interesting monographs in plenty; and few topics in the relation of body and mind would render more profit of practical scientific and educational use. Mind has a motor basis, and nowhere else is the direct connection of kinesthesia (actuating and inhibitory) and significance more clearly to be seen than in the affects. Graphic and plastic artistry amply show this motor ground, as all who are familiar with physiological esthetics clearly recognise. It is music, however, the freest of them all, in which the kinesthetic factor is most apparent. Of course, the over-evolved music of the ultra-'classic' kind, more intellectualism than feeling, exhibits this basis less clearly than does real music, the purely affective musical music of the opera, the ball-room, and the battle-field, intended for enjoyment or for use rather than for a bastard instruction in artificial technique. In the experience of this real kind of music, conscious kinesthesia is altogether dominant, and provides the psychological framework of this stirring mode of experience. This domination of the rhythm-aspect of music (and rhythm, I take it, is wholly and inherently kinesthetic and coenesthetic) has generally been recognised, but in my opinion always inadequately, as, for example, even by the latest writer on the subject, Weld (73). Lack of space makes inexpedient the elaboration of this important relation here. In general, it is to be observed that the vibrations that form important parts of many kinds of experience, and which are perceived, of course, wholly kinestically, have so far failed to receive the attention they deserve at the hands of psycho-physiologists. Helen Kellar (44) has some extremely pertinent and interesting remarks anent this theme; and what to her is clear consciousness we may be sure is at least subconsciously but none the less *effective* in every one of us.

How unifiedly intimate the receptor impulses are in such examples as we have suggested on the one hand to bodily movement and on the other, logically quite different, to our consciousness, is apparent almost at once. Without kinesthesia, space is a meaning clearly unintelligible; and music based on rhythm is well-nigh inconceivable. Wundt's principle of 'creative resultants' has long been the model for explanations of meanings similar to these. They who refuse to admit the efficiency of this method[5] must be surviving believers in that narrow notion of mind that limits it to immediate awareness, ignoring the vast complex deeps of 'the subconscious.' Numberless meanings have been synthesized out of the primary sense-experiences (attitudes) in every normal child's brain and mind and the neurility representing them in the adult has long since sunken out of consciousness, save as intensive (more or less inhibitory) effort in skill or other new and personal

[5] For instance, Wm. McDougall (50) in his brief clear argument, "The Psycho-physics of 'Meaning.'"

deliberate effort floats them to and on the conscious surface of the stream of mental action.

Just as every intended voluntary movement depends for its integration and realization on the implicit spatial frame-work of kinesthetic, visual, auditory, etc., neurility, kinetic or potential in the central nervous system, so every bit of understanding that helps constitute our minds or serve as a basis of volition, has its basis, mediately or immediately, in the two-phased innervation of the vegetative and the personal will. The far-reaching practical value of this understanding, of these meanings, may be exemplified in the use of objects (eupraxia), material or otherwise. Each object, in so far as deliberately made by sane man, has a meaning, namely, of course, its final purpose or use. This use, in general, is not understood as a real understanding save as the individual actually uses the object. Of course, this information may be and often is obtained at second-hand, as one learns from books instead of by first-hand observation; but we have to suppose the brain-paths concerned to be the same substantially in both cases—namely, the grey fabric of the hemispheres in its entirety or in part.

The *early elaboration of human behavior into very numerous psychomotor complexes* on a basis of economy and habit devolves especially, we may reasonably presume, on two important portions of the brain (connected intimately, however with all the rest)—the frontal lobes (via the central) and the cerebellum. If we know anything at all of the frontal cortex it is that one, at least, of its duties is this elaboration of motor ideas, how to use objects, for example, or how to rotate the arm or roll a shot between the fingers. These centers have in early years elaborated many such psychomotor complexes, have interrelated them, and have put them in bewildering intimacy with all the other centers in the brain and below it. Broca's convolution furnishes what is probably by far the most complex example of an eupraxic center, and its function is just the elaboration, interrelation, and extra-relation of a certain set of objects, namely, words. These, spoken, written, printed, graven, depicted, sculptured or materialized by the sign-manual constitute the physical basis, so to say, of the one uniquely human process of language-formation and language-expression. Need one try to imagine the details of Max Mueller's speech-center in terms of neuraxones, dendrites, nerve-cells, or of ions and osmosis and reversing colloids? Yet this must be done some time and by some one or else all our fashionable localization be given up.

In a way quite homologous to this, every common object other than words must have representation in the frontal cortex, we may assume. If, then, a man does not know how to use a sextant or an ediograph or a planimeter, or a pencil or a pair of shears, it is so only because his frontal cortex has never acquired or, from disease, has lost that particular psychomotor complex. No more can be taken out than has been put in. As has been often well pointed out, the newborn child is almost wholly apraxic, although even then something in his mechanism of efficiency represents the use of his mother's breast just as it represents and provides the means of finding it in space. The cleverest adult obviously is he who has in his frontal cortex the greatest number and the greatest perfection of detail and relationship of these motor ideas (neurograms) of movements of utility.

Each one of these is properly a meaning and there is no reason whatever to suppose that each one could not be fully accounted for in neurogramic terms if we knew the exact itinerary of every forceful impulse concerned in setting them into the mental process. Proof of the falsity of this supposition is part of the burden imposed upon those who doubt it, for it is the *natural* supposition, rational in every phase of psycho-physiology. Those who doubt it simply fail to imagine the complexity of the human brain as a working machine, or the possible combinations and interactions of its three or four thousand million neurones.

President Sanford in a recent suggestive essay (62) makes explicit certain phases (the mathematical in particular) of meaning in relation to kinesthesia; and even while his arguments strive to show the limitations of sensation in this respect, they lend us distinct aid and comfort when they are properly oriented. "The concepts of physics," he says, "tend, in a word, to state all physical phenomena in terms ultimately reducible to dermal and kinesthetic experiences, and by that very fact physics is prevented from explaining anything that lies outside the field of dermal and kinesthetic experiences because of the impossibility of translating one sort of sensation into another sort. * * * * [Yet,] we can in some cases indeed transfer meanings and use symbols derived from one sense as carriers for meanings derived from another sense, as for example, when we treat geometrical relations by algebraical symbols in analytical geometry. But this is merely a transfer of the language signs, *i. e.,* of the signs of symbols, and not of the original symbols themselves. In every case the meaning must come from somewhere (*i. e.,* ultimately from some sensory experience) and the meaning then gives limits to the symbols as if they had originated along with it." But the present writer would respectfully inquire of President Sanford (and of Wm. McDougall) if one of the most basal and universal processes of psycho-neural life be not just this particular process of 'transfer' unlimited and fusion. Normally,—that is naturally—no one sense ever is active alone, as indeed many recent researches,—*inter alia,* those of Sewall (63), Head and Holmes (38), Head and Sherren (37), Crile (12),—have testified designedly or by implication. Functional integration is all but universal. Here is suggested one of the five or six respects in which descriptive analysis has gone so far as plainly to mislead, implying a simplicity which in fact in no wise exists. The "transfer of language signs, 'only' signs of symbols" though they be, is certainly all that one need postulate for 'explaining' the elaboration of much in the way of meanings and value out of 'simple' kinesthetic and tactile experiences. Half of what the reader knows and more has come into his soul by just this process of transfer and fusion and symbolism in his brain.

But hear Doctor Sanford again about the internal kinesthesia of the body called by another name of "general and organic sensations:" "And yet even touch itself [kinesthesia] furnishes but the outer and less important part of our empirical selves. A deeper self is the self of feeling and emotion, the self that loves and hates, that strives and aspires, that enjoys and suffers, and for this another group of senses is chiefly responsible—that inner group of general or organic sensations. It is of the reverberations of excitations within the field of these inner senses that the moving part of our emotions is constituted. The loss of feeling robs us of all that part of ourselves that makes life or anything else valuable [a word of wisdom the school ma'ms need to learn]. And that part of ourselves we owe chiefly to our general and organic sensations. To sum up briefly, I may say that we get from the chief senses, singly or in coöperation, four characteristically different abstractions: From touch [kinesthesia] we get the world of space and material reality, and force acting upon us; also, from motor touch, energy, active efficiency, and freedom; from vision we get space and the world of things, though in a somewhat different way from that in which touch gives them to us; from hearing we get our symbolic machinery of thought; from the general and organic senses, our most intimate intuitions of ourselves and the basis of our emotions." Clearly there's ample room here for all one, even a partisan, need claim at present for kinesthesia in its relation with the intelligent will:—space, material reality, force, energy, active efficiency, freedom, and "all that part of ourselves that makes life or anything else valuable." And patient research will demonstrate to the most dogmatic 'animist' (for animists all have bodies too) sooner or later the pathways, the currents, the tidal-chart, so to speak, of the neurility by which all these experiences are represented in our mortal days.

About the logical limit of present seeming aloofness from obvious nervous impulses along named tracts and nerves, is exemplified in numberless abstract concepts of relation tinged with emotional tone. All other mental processes are relatively, always relatively, simple. But even here the kinesthetic influences come more directly into the exploit than the average psychologist as yet realizes. Merely to point out the road sometimes is an acceptable service, even if we stay behind.

Wm. McDougall cogently states and trenchantly discusses a case (the telegram-argument) which may, perhaps, usefully be taken as an instance of average complexity and of more than average interest,—as one would expect from the "hands" of the Oxford psychologist for whom psychology is "the positive science of the behavior of living things." Let us quote the situation (50): "A man receives from a friend a telegram saying—'Your son is dead.' The physical agent to which the man reacts is a series of black marks on a piece of paper. The action outwardly considered as a series of bodily processes, consists, perhaps, of a sudden, total and final, cessation of all those activities that constitute the outward signs of life; or in the complete change of the whole course of the man's behavior throughout the rest of his life. And all this altered course of life, beginning, perhaps, with a series of activities that is completely novel and unprecedented in the course of his life, bears no direct relation whatever to the nature of the physical stimulus. The independence of the reaction on the nature of the physical impression is well brought out by the reflexion that the omission of a single letter, namely, the first of the series (converting the statement into 'Our son is dead') would have determined none of this long train of bodily effects, but merely the writing of a letter of condolence or the utterance of a conventional expression of regret; whereas if the telegram had been written in any one of a dozen foreign languages known to the recipient, or if the same meaning had been conveyed to him by means of a series of auditory impressions or by any one of many different possible means of communication, the resulting behavior would have been the same in all cases in spite of the great differences between the series of sense-impressions." The constant meaning behind all these extraneous conditions McDougall supposes to constitute "the essential link in each case between the series of physical impressions and the series of physical effects." He denies absolutely any evidence that such spiritual values of meaning as that conveyed by certain little curved lines of ink on the telegram-blank can be expressed at all in neural terms.

Yet, on page 342 of the same interesting argument for 'animism' we read a paragraph that seems to imply answer to previous doubts. "We have, then, very strong grounds for maintaining that all mental retention and reproduction are conditioned in two very different ways; one of these ways, the way of motor habit and automatism and mechanical association, is adequately accounted for by the conception of the formation of neural associations by the repeated passage of the current of nervous energy between neuron and neuron, each passage leaving the track more open for subsequent passages. (Synaptic resistances lowered?) This is the only plausible and in fact seems to be the only possible conception of the way in which mental retention can be conditioned by cerebral structure or function; but the strict limitations of this mode of retention, especially the need of many repetitions of the impressions even in very simple instances of mechanical association, show that we cannot regard it as the sole or principle condition of the higher form of retention or true memory. This we see depends upon meaning; and meaning as we have seen, is just that all-important factor in mental process to which we can assign no immediate physical correlate among the brain processes,"

It is the expression "the strict limitations of this mode of retention" to which almost anyone who was really searching for concomitant somatic conditions (instead of afraid he would find them?) would naturally object. Logically it wholly begs the question and neurologically it limits knowledge and scientific explanation and presumption somewhere near the *beginning* of the nexus between psychosis and neurosis. The present writer might reasonably be classed as an 'animist' by one who affected this particular class-name, yet he certainly hopes to see started, at least, description of the homologue of every mental process in bodily terms and that without the doctrinal exclusion of any amount of interaction between the two empirical series. This seems the easy road, identified for once with "the straight and narrow way," by which the idealist may expect scientific (psychological) satisfaction.

Memory and habit-formation and meaning symbolized to the mind by material symbols, are facts as obvious and certain as the Cartesian 'cogito.' The symbolic method of representation in the hemispherical grey is put beyond hypothesis by the cold facts of apraxia, etc., already suggested; and at once underlying and surmounting these are the neurograms, less known but certain, representing impulse, feeling, desires, character, motives of the most basal nature, broad and deep and long as humanhood itself. Combined, these three factors seem to the writer to suggest the method, at least, of the concomitance of meaning. If the kinesthetic and retinal impressions from that grievous telegram do not in the artificially strict terms of the physiological laboratory, appear as mechanic stimuli of a life-time of asthenia and misery, at any rate of change, it is assuredly because once more, as forever yet, the *complexity* of the problem is underestimated, almost overlooked,—even by the learned Oxford reader in philosophy! One seeks too much from a little given, demanding bricks to build cathedrals to the skies without the utterly indispensable straw! The defect in our psychophysiology, in other words, is quantitative and not qualitative.

It is time the traditional notion of 'traces' 'impressed in' the brain by experience immediate or otherwise were given up out of psychophysiology for good and all. Nothing at all is gained by retaining this neo-phrenological idol of the school, while on the contrary it is a cogent example of how much the antiquated make-shift concept prevents us from advancing. There *are* no 'traces' on the brain-tissue so far as anyone has been able to learn. Memory, habit, understanding, feeling, meaning, and the rest are not dependent on any kind of 'grooves' made in the living grey fabric of the great hemispheres. Eighty-five per cent of water is this intricate layer of cells and dendrites that we call the cortex; and the ceaseless activity and change, its hurrying life, exceeds that of every other portion of the organism.

But if not as material traces or vestigia how, then, can we think the mode of retention and fusion and interaction by which alone perception, imagination, conception, reason, meaning, and allied processes are thinkable facts at all? Assuredly we shall accept the alternative (it seems to the writer the exclusive alternative in view of the facts known to him) namely, of course, the conception of active kinetic strains and stresses in the "three or four thousand millions" of neurones making up the brain. Floods of neural influence of many kinds along the separate conducting pathways, infinite variations and relations of tonus in the nerve-cells of these neurones!

And pray why not? And let Wm. McDougall and all others who could be human disembodied, tell us why there is not here sanction in plenty for every phase of intelligence, vegetative or personal, brute or human, the crudest matters of gross fact or the most subtle shades of feeling and of meaning, momentary and individual in its influence or affecting myriads of other men in *secula seculorum.* Here is *living energy,* energy in the one thing the most complicated of all things known to the human mind, so complex that it furnishes a thoroughly rational basis for even the filmy gossamers (in Huxley's famous phrase) of the soul's experience. Why refuse to a mechanism of this description,—nay, intricate beyond all present description or imagining,—any meaning however sad or overwhelming, however far and wide from the material symbols, 'stimuli,' on the paper page? What are these symbols but cues, but keys, into this marvellous maze? What matter how simple the stimuli—does not the wondrous labyrinth of living energy with its billions of pathways remain the same, and in combination, in *living,* far in excess of every finite dream—rich in possibilities beyond the dreams of even animistic avarice?

I, for one, know no complexity in mind for which an organism so integrated is not the homologue and peer.

This maze of living energy is kinetically the direct *continuum,* for the most part, of the kinesthetic flood of influences. We get here, perhaps, a-field from the definite kinesthetic pathways technically so described by the histological neurologists; but we certainly do not at all get beyond the possibility of representing psychophysic motives in terms of energy or the somatic influences in terms of motives—whichever way one prefers it stated. Implicated in every moment of the behavior of that miserable father, whether statable as actuative or inhibitory, were kinetic (or potential?) nerve-strains representing the mediate or immediate variations in some action-system (if we include therein the epithelia). Attitudes are but crystallized kinetic kinesthesiograms (if such a term be not uncoinable?). And certainly his behavior, as a series of conscious and subconscious motives actualized, was the immediate outcome continuously of attitudes whose tenor was fixed "infinite aeons ere our race began" in the instinct of complex parental love and care.

Strangely enough, some of McDougall's own theory, adopted by many and suggested (probably quite unknown to McDougall) by Claparède (11) eight years ago, helps materially in the acceptance of the full value of kinesthesia in concomitance. His theory of fatigue (49) seems to contain a point of view at least (it is probably much more—a physiological reality in some mode or other) which, made a bit intensive, would more or less belie the notions as related above of the same McDougall self-deemed an animist. This viewpoint overlooks his reservoir-idea of the central nervous system already used above, a fertile concept on which to base a theory of motivation in the terms of nervous circuits already suggested. The name reservoir-idea is all but self-explanatory; it assumes (more or less after Sherrington, Bastian, *et al.*) that the afferent or receptor side of the nervous system plus the central adjustor influences, constitute properly a reservoir of neurility on which the efferent, effector pathways and mechanisms draw one at a time (the efferent common paths of Sherrington (64)). The whole trend of kinesthesiology conducts the same way and each helps the other. Carrying this idea only a step or two further in an entirely justifiable, yes, necessary direction, we may suppose the neuronal knots, grey matter, everywhere, in the sympathetic ganglia and the cord as well as in the brain, to be surcharged with nervous tension, potential or kinetic energy, 'awaiting' only a chance to functionally discharge. Its function would cause it to discharge quite as much, perhaps, in feeling and in temperamental matters, quite as commonly or at least as normally, into cerebral mazes standing for early-acquired complexes of affective or cognitive *meanings* as into attentive consciousness seen as voluntary movement new in every case or in the habitual reflex aspects of attention-behavior (13).

Normally the actual activities of the real man or woman are arranged in well-defined functional groups. This is clearly necessary owing to the almost universal principle of habituation. These habit-groups of contractile and secretory activities are not so much muscular and epithelial groups or complexes as they are neural, for it is, of course, just the one sole business of the nervous system to bring about this very kind of adapted integration of unitary movements into activities such as make up the actual functions, just as these combined and unified in turn are an individual life. We have not far to seek nowadays in the anatomic arrangement of nervous system to find the structural basis of this habit-grouping of muscular and glandular actions. One order of them we find based in the almost innumerable association-complexes of the brain and spinal cord; these are the so-called "higher" functions and are concerned immediately with the freer or voluntary activities. The other order of action-complexes are basally of the vegetative kind, are more or less reflexly (that is, mechanically or chemically) determined, and have their basis in the numerous ganglia and other functional groupings of the "autonomic nerves" (a necessary new name for the familiar sympathetic plus certain other similar nerves concerned with the alimentary canal). One sees in the sympathetic groups of ganglia, some of them arranged along the sides of the spinal cord; others (for example the solar plexus) huge orderly knots of protean neurones in the abdominal cavity; while still other are scattered in large numbers, but functionally related, in the various viscera of the thorax and abdomen. As is well known, it is almost the entire duty of these ganglia and of the fibers relating them, to direct the activities of the involuntary ("smooth") muscle and of the epithelium, whatever be the precise functions of the particular muscles or glands. This functional set of nerves is intimately related, in ways too complex to be here described, with the cerebro-spinal axis of nerve-paths. Moreover, it represents not only the movements but probably the normally ill-defined sensations of the tissues it supplies.

Now, because these unit-complexes of the vegetative functions of life (nutritional, circulatory, reproductive, etc.) represent the real biologic basis of the nature of the individual, we have to think of them as forming to a considerable extent the *fundamental somatic character of the man or woman as determining his or her biologic, phylogenic interests.* Thus, for example, the man with an abnormally capacious and habitually overburdened alimentary canal, that of the glutton, will have these visceral characters in some way represented in the habit-complexes of his sympathetic ganglia; in the sympathetic of the nymphomaniac the same conditions, properly adapted, must be present; and so on in all the organic systems. The unit-complexes especially of the sympathetic represent then, in some way, without a question, the basal vegetative peculiarities of the individual. It is these obviously that determine more than all else the biologic and phylenic interests and tendencies and meanings of every person as a social unit, as a link in the chain of the generations. August Hoch in a discussion of dementia precox before the American Neurological Society, succinctly stated the same group-action of the nervous system and added another principle germane to the neurility of meaning, thus (39):

"Our memories are grouped, as it were, in more or less extensive and more or less circumscribed complexes or centers of attraction, in the formation and cohesion of which special interests take an important part. We can conceive of the mind, therefore, as made up essentially of trends of interest. In the course of individual development certain main tendencies of the personality develop which then take the lead, while other tendencies become repressed. These repressed trends exert nevertheless a marked influence on the conscious thought and activity, as Freud has shown; but in normal life they do so mainly through the fact that the energy they supply is led into profitable channels. Every trend naturally pushes toward a realization in the direction of its feelings. If this is in harmony with the main tendencies of the personality this is useful and represents the dynamic force behind our thinking and our pursuits, adapted to the environment and the given situation. If, however, trends which are not in harmony with the main tendencies of the personality and which are, therefore, under the influences of repression, no longer find an outlet in profitable channels, but assume a more or less independent dominating rôle, it is not to be supposed that the laws which govern normal mental activity should be suspended; on the contrary, we shall expect to find the same principle of the trend pushing toward its realization, while at the same time the other tendencies of the personality assert themselves in repressing influences as well as in adjustment reactions, but owing to the disturbance of balance between the usurping trend and the main tendencies of the personality, the thinking and acting is then no longer adapted to the actual situation, but appears as something strikingly out of contact with it, and is of a simpler, more crude type."

In a somewhat like way Morton Prince (58) recently has said, "Meaning is derived from and determined by past experiences. That is to say, ideas have associative relations to objects, thoughts, actions, conduct, stimuli, constellated ideas, etc., *i. e.*, past experiences represented by conserved complexes. As a result of such previous experiences various associations are built up and these complexes form the setting or context which gives ideas meaning." "In the building of complexes [*e. g.*, meanings] as we have seen, an affect becomes linked to an idea through an emotional experience. * * * * Now we must distinguish between the process which determines the meaning of the idea and the process which determines the presence of the affect in consciousness. * * * * That which determines the meaning is, as we have seen, the setting which provides the secondary images and the associated ideas, and, therefore, the point of view. That which determines the affect is an association or linking of the whole affective mechanism (including the physiological reactions, *i. e.*, vasomotor, respiratory, secretory phenomena, etc.) to an idea. It does not give the meaning, but provides the impulsive force which tends to carry the idea to fruition. * * * * It is not a logical necessity that the original experience which occasioned the affect should always be postulated as a continuing unconscious prcoess to account for the affect in association with the idea. It is quite possible, if not extremely probable, that in the simpler types, at least, of the emotional complexes, the association between the idea and affect becomes so firmly established that the conscious idea alone without the coöperation of an unconscious process is sufficient to awake the emotion."

This application of Pavlov's discoveries is beyond cavil still more likely to work backwards, for it's a poor rule indeed that will not work both ways. Even an unconscious (actuating) kinesthetic influence coming into the "set" of the brain is readily competent to awaken a conscious idea, colored by whatever affect phylogeny and personal habit have rendered it in that particular individual. This complex serves, then, indefinitely as a motive of behavior.

Our present research into the phases of the movement-'sensations' adds a little, it may be, to this promising way of considering the nervous system's kinetic thrust, for it suggests that the duality of phase (actuating and inhibitory) found to obtain in kinesthesia proper, may be extended into the brain's grey fabric, and thereby underlie the determination of behavior —conation in its widest inclusion. Not by physiological chance, we may be sure, was the awful meaning of that well-supposed telegram asthenic and depressing to the total behavior, as a psycho-physical series, of the father, for the written symbols on the paper formed a well-established kinetic nexus with everything in his grey cortex and below bearing him thus downward. On the other hand, had the symbols meant, "You are a grandfather: ten-pound boy named for you; all well," these too would have fitted into a nexus, of the opposite tenor and effect, sthenic and stimulating, but an integration complete in itself as represented in the phylogenic and ontogenic fabric of his kinetic nature. It is not easy for the writer to understand why McDougall should venture (deductively?) to set a perfectly arbitrary limit to the concomitance of his neurons and refuse them a share in meaning when the continuity in all respects and everywhere is so complete. Why should one require a wholly gratuitous insert ("products in consciousness of a purely psychic activity") at one particular place in a series seen to be complete without it as soon as we know our anatomy and especially our neurophysiology better than now? I have no doubt of the "products in consciousness of a purely psychic activity," but I believe them irrelevant in a series competent without them, however unified the two series be. The very essence of brain-action is synthesis and symbolism endlessly subtle and intricate, as McDougall's explicity of the reservoir-idea indeed implies. Animism *needs* no denial of the brain's subtlety and intricacy—it rests on too firm a basis and is itself too broad to require a shoring up by any destructive criticism of a series in itself utterly beyond, in its intricacy, man's present comprehension. The law of parsimony, if nothing better, would suggest the superfluity of this particular instance of interaction.

The two neural factors underlying human behavior, then, seem to be tentatively considerable as two-phased, on one hand a set of action-influences or attitudes (more or less directly related to the total compound innervation of the action-system in use at the time, and including kinesthesia proper, touch, vision, and hearing), and on the other hand, a set of kinetic fusions, involving probably in some measure of influence the whole central nervous system. These latter more especially perhaps constitute the meaning of the experience, relatively passive or active, its intent, its personal motive or purpose, all meaning being sooner or later reducible to motivation, kinetic or potential. The former, the action-set proper, is the more or less subconscious association between the more mechanical neurones of the nervous system, sympathetic, spinal, cerebellar, and cerebral. The latter (the personal, purposive, restrictive control-influences, including the resultant of the feeling-tone and of the conation proper) constitutes, in my opinion, the more conscious meaning of the organic motion represented, however remotely. Meaning as feeling plus conation plus expression-innervations, seems to underlie both the kinesthetic phases disclosed in the research, but is conscious meaning only in the inhibitory motor ideas of the implicated behavior. The meaning of that sad telegram to the father, for example, was represented in his kinetic neurograms (sympathetic, spinal, cerebral) as certain definite modes of behavior. Meaning seems irrational save on this pragmatic motor basis, involving immediately or remotely, by the way of the neurones organic "expression" or its negation.

The majority of psychologists perhaps would suppose the correlates of meanings to be vague 'drainage'-flows between groups of central neurones acting on principals still much in debate. But whatever be the cerebral harmony of subtle action, the motive force for this meaningful "association" or fusion-process, with its "high synaptic resistances," etc., etc., is not at all apparent save on lines somewhat like those sketched above. With these tentative principles and facts in mind we shall think of meaning-fusions as a more or less direct functional continuation of the kinesthetic (*et al.*) influences flooding even the central nervous system from the action-system—the entire musculature voluntary and vegetative, and the nervous centers. The immediate meaning-innervations have, of course, an immediate independence of the current flood of kinesthetic influences, but just how much or for how long we do not as yet know. Nor do we need to know this exactly so long as we realize that the energy just behind the meaning (if we may so speak, metaphorically) comes from the muscular chemism by way of kinetic energy in muscle and nerve-cell and floods the brain with a living "head" of motivation force. The body's action as a whole, universal as tonus and more patently, keeps the great coördination-tracts tingling with uncountable thousands of kinetic impulses primarily kinesthetic.

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MAGICAL FACTORS IN THE FIRST DEVELOPMENT OF HUMAN LABOR[1]

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The beginnings of human labor imply more of psychological problems than theoretical economy and ethnology usually admit.

We notice the common characteristic of primitive tribes, that they work or labor far less than we. I am using the word "labor" in our presently accepted sense of its meaning as a continuous, purposive and organized activity, comparatively independent of accidental stimuli, and sharply distinguished from play. Play finds immediate satisfaction in itself; while labor is always swayed by, and directed towards, more or less remote ends.

The comparatively small amount of labor engaged in by primitive peoples, obviously, is of great consequence for all early civilization. Putting aside moral considerations of leisure, egoism, and so on, this fact is usually interpreted as due to a lack of intelligence and knowledge. If primitive man, it is supposed, could know the useful consequences of a stronger and better organized form of activity, he would learn it and begin to work.

However, there are many instances, proving that primitive man, while possessing knowledge and understanding of certain kinds of economic labor, yet fails to apply himself regularly and continuously. Psychologically, he is unable to do so. On the other hand, the same men are not so absolutely unrestrained, not so shiftless and dominated only by momentary impulses, as are our tramps or criminals. Under certain conditions, we see them performing extensive and very accurately regulated kinds of work. All primitive tribes try, by complicated procedures, directly to influence the weather, the movements of the stars, birth, sickness and death, puberty, the reproduction of plants and animals in nature, and other

[1] This article is a summary of two addresses given at the recent Cleveland meeting of the American Psychological Association and before the Philosophical Club of Columbia University.

Detailed materials and more elaborate discussion will appear in a series of monographs, entitled: "Arbeiten zur Entwicklungspsychologie."

natural processes—a thing we do not attempt at all, or which we treat in a much more indirect manner.

Even where the co-operation of human activity *can* be causally very effective, as in preparing for war or the chase, in making weapons, adornments or other implements, and in the beginnings of agriculture, the care of animals or barter: the *form* itself of such activity is seldom free from very irrational factors. The more the end is subjectively important, and the less clearly its real causal relations are understood—the more primitive man's active behavior is surrounded by, and impregnated with, *magical* elements. Oftentimes a semi-rational procedure is prepared for, or followed by, a purely magical ceremony. A large part of a primitive tribe's waking life is filled with singing, music and, especially, dancing. All these—quasi-play and quasi-artistic—activities were originally more than mere amusement. First they are immediate, involuntary expressions of emotional tension. Very soon they pass into a form of conventional ritualism. Dancing and related rhythmical activities, in particular, are believed directly to effect remote ends that are subjectively represented in a more or less confused form. Dancing and music are the magical instruments *par excellence*.

An African boy was asked by a missionary why he did not work, at least as much as did his aged parents. He answered, "I dance day and night." Obviously, he implied that this activity is much more important for the whole life of his tribe, than the more rational work of women and old men could possibly be.

In many primitive languages we find one word used as a term for dancing, singing, magical ceremonies and working. In Latin "cantare" originally meant the same as "incantare." In Greek, the word for labor, "ergon," is derived from "orgia," in the sense of a rhapsodic psychophysical behavior. In semi-barbarous religions most of the gods as well as the priests are dancers, singers and musicians.

Karl Bücher collected a large amount of material, proving the rhythmical character of primitive labor. However, his genetic analysis is incomplete and his psychological interpretation is too intellectualistic. He neglects the dominating emotional factors in such phenomena, evidenced by the magico-religious apperception, in the primitive mind, of rhythm and music as well as of all those forms of activity believed to be vitally important.

When we seek in human development for the beginnings of a complicated, continuous, organized and serious activity,—

in a word,—for the beginnings of labor in the psychological sense, we find them in magical ceremonies or in those forms of regulated occupation which are centred around magical behavior and institutions.

They are the first regulated forms of social activity—regulated not only in time and space but also qualitatively.

There are facts which tend to prove that, originally, man (like animals) even for eating and sleeping observed no fixed times. Magico-religious ceremonies usually occur about sunset. The more important ones, like initiation or burial ceremonies, since they are participated in by large numbers, even from distant tribes, must be decided upon weeks or months in advance. These ceremonies break the earlier custom of isolated eating (which is also—but in a more negative and primitive sense—magically conditioned). Of course, this first eating and drinking in common must be largely prepared for in advance and, to a certain extent, presupposes self-restraint and forethought. The place for such ceremonies also must be fixed and prepared. It seems that, generally speaking, the first psychologically accentuated places were the holy and dangerous ones, where a man had died or lay buried, where ancestors had performed some magically important activity, where a woman first felt the stir of foetal life, and where at set times ceremonies are to be performed.

Qualitatively, these ceremonies are better ordered and differentiated than are any other forms of primitive activity. Any change in the succession or form of such acts is believed to cause immediate illness or death; a single mistake, occurring, for instance, in one of the numerous, complicated dances of Indians of the Northwest is punished by death. The initiation-ceremonies (especially those for the boys) with their long-extended preparation, are characterized by painful and other impressive forms of treatment.

Genetically speaking, such magical and religious regulations of the life first teach self-restraint, growing independence of momentary stimuli, self-sacrifice,—all of which are essential conditions of man's *capacity* to work.

Even those members of the tribe who take no active part in a ceremony are deeply influenced by it and by many of its relations. The women and young children are usually excluded from all of the more important ritualistic performances, objects and places, because they are considered too weak to undergo the varied and terrible dangers involved. This fact increases, for the whole tribe, the emotional value of the magico-religious performances of the men. At the same time, it constitutes a basis of the first forms of division of labor and all social organization. The women are habituated to the renouncing of a large number of pleasures and important experiences, and they are accustomed to continuously care for their young children; they also perform many semi-mystic activities pertaining to their sexual and maternal life. All of these things co-operate in giving to women a peculiar class of duties and volitional habits, such as the care of fire and water, the regular supply and preparation of food (even for the future), the first forms of pottery, textile industry, agriculture and market-barter. While such economic activities were originally the exclusive task of women, the more they become complicated, of importance for the whole tribe and intricately involved, the more they are shared in by the men. For instance, all relations to foreigners, with their political and juridical consequences, are regulated by the men, because of the semi-mystic dangers emanating from every stranger.

The political and related affairs are usually the exclusive business of the adult, unmarried initiates, who are able to fight and hunt. This group, at an early stage of cultural development becomes consolidated and differentiated from the whole tribe. From its peculiar location in the camp, evolves the "Men's-House;" originally it excluded all uninitiated and all married people. From this germ there have gradually grown up the temple and memorial hall, the place for regular dancing and music, the forum, court and King's palace, the arsenal, fortress, guest- and clubhouse. The activities taking place in the Men's-house, originally have the general characteristic of being dangerous, of vital importance for the whole tribe and therefore shot through with magical elements. In this same house we first find regulated and socially organized industry.

Every magical performance itself is believed to be dangerous in so far as it is effective and powerful. Therefore, the number of persons participating tends to be increased, in order that individual danger may be diminished. Magico-religious reactions condition the earliest careful social division and correlation of functions. On the same basis arises the peculiar role of the *old* men and women, who are better acquainted with the ceremonial rules, apperceived as concurring with the will of ancestors. The whole group of women gradually come to perform determined kinds of ritualistic activity, even in the absence of the men, but nevertheless in functional correlations with their qualitatively different activities.

Finally, the first regular occupation or profession, which is engaged in by *single* individuals, is that of the magician, medicine-man or prophet. The political or military leader is usually the same man, or one closely related to him; at any rate, he is held to possess extraordinary magical, and consequently hereditary, powers. In an analogous manner, the differentiation of the more special, industrial professions, such as that of the blacksmith, is supported and conventionally fixed by the belief in a supernatural gift, with which such men are endowed.

These magical, and therefore dangerous, powers radiate through the apparel, the implements and every act or thing tangibly connected with these persons. And the more powerful a person, the more his substantialized magical forces survive his death, regulating the lives of the following generations.

Thus, from magico-religious qualities and "taboo" arise all primitive social privilege and every institution of group or personal property.

It is obvious that all of these relations are of utmost importance for the strengthening and deepening, for the individual as well as for the social differentiation and integration, of voluntary activity.

The confused but intensive feelings (with their habitually regulated reactions) towards the supernatural dangers and powers, effectively co-operate in liberating the will from exclusive rule by momentary impulses and instinctive mechanisms. Once escaped from this thraldom, the individual and social will continue to twine itself about an increasing number of more highly developed irrational supports. Step by step, intermediate links between momentary stimuli and human reaction become more numerous, harmonious, and structural, while the originally disconnected magical stirrings, organize and differentiate themselves into religious, aesthetic and moral feelings, into social conventions, political institutions and economic practices.

It would be a mistake to infer from these considerations that the development of civilization originally was purely magical or religious, that religion had a development distinctly previous to that of economy, social organization, artistic production, etc. On the contrary, it is a common characteristic of primitive civilization that all of these directions of human behavior are undifferentiated. However, the higher the vital importance of an institution or form of activity is held to be, the more it is freighted with and integrated by irrational elements of a magico-religious character. These elements though originally unanalyzed from the beginning form centers of emotional stress and crystallization. From thence take their origin, numerous and ramified channels of volitional directiveness. To such facts is closely related even the evolution of the judgment and the intelligence, which is continuously supported by the evolution of emotional and volitional reactions in their totality. Every peculiar ability or knowledge is originally apperceived as being a magical power, substantialized in certain persons and related objects. The first conception of causality is that of magical necessity; and, later on, the idea of universal natural law is prepared for by the conception of the all-embracing power of the Divine.

We are unable to understand the psychological continuity of human development towards our individual and social forms of life without genetically taking into account the magical and religious reactions of primitive mind upon every impressive experience, and to every situation of vital importance.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF STANFORD UNIVERSITY

SIZE AND DISTANCE OF PROJECTION OF AN AFTER-IMAGE ON THE FIELD OF THE CLOSED EYES

By Frank Angell and W. T. Root, Jr.

A. Occasion for the Experiment

During the Fall semester of 1910, a beginner's class in experimental psychology in Stanford University had occasion to estimate the size of an after-image of a disc 1 cm. in diameter on the field of the closed lids. The results of this off-hand estimation were varied; some thought the image exceedingly small, but a fraction of an inch in diameter, others thought the image much larger than the actual disc. This led to an attempt to see if there is any law or regularity governing the apparent size of the after-image or whether it is merely a matter of individual determination. Specifically the question called for a determination of the apparent distance of the after-image from the eye and its size on the closed field of vision, i. e., the field with the closed lids.

The only references to the subject which the writers were able to unearth were an allusion by Fechner in the *Psychophysik,* harking back to an investigation on complementary colors published in 1838, and a casual remark by Hering in an early number of his *Beiträge.* Fechner's statement runs as follows:—"Schaut man eine Lichtflamme oder irgend ein farbiges Object, was ein Nachbild zu liefern im Stande ist. aus einer grössern Entfernung als der deutlichsten Sehweite an, so wird das Nachbild im geschlossenen Auge dann kleiner zu seyn scheinen, als das Object; grösser dagegen, wenn man dieses aus kleinerer Entfernung als der deutlichsten Sehweite, anschaute. Das geschlossene Auge beurtheilt also die Grössenveränderungen, welche das Nachbild je nach der Entfernung des Objects erfährt, richtiger als das offene Auge die Veränderungen in der scheinbaren Grösse des Objects selbst. Denn die Objecte scheinen uns, vermöge einer uns zur Gewohnheit gewordenen Association des Urtheils mit der Empfindungen, bei verschiedenen, nur nicht gar zu grossen. Abständen betrachtet, immer dieselbe Grösse zu behalten; obwohl natürlich ihr Bild im Auge hiebei einen verschiedenen einnimmt. Im geschlossenen Auge vermissen wir die Umstände, welche unser Urtheil hiebei leiten, und es bleibt bloss die Empfindung des Raumes übrig, den das Bild auf der Netzhaut einnimmt; wobei ein unwillkührlicher Vergleich seiner Grösse mit der Grösse stattfindet, welche das Bild bei Betrachtung des Objects aus der deutlichsten Sehweite erlangt."[1] For Fechner therefore the size of the after-image in the field of the closed eye seemed to agree with that of the inducing object when the latter was placed at the point of clearest vision, though this

[1] *Poggendorff's Annalen,* XLIV, 1838, 524.

opinion would seem to have been an *obiter dictum* springing from general impressions. At any rate there is nothing to show that Fechner had tried to work up the matter quantitatively.

Hering's observation occurs in the midst of a criticism of Wundt's projection theory of vision, and is to the effect that the size of the after-image in the field of the closed eyes depends on the distance at which one images it.[2] But this again appears to be a statement based on casual observation or perhaps on the inference that the conditions affecting the size of the after-image in the open field of vision would also govern it when the field was closed. With so skilled an observer as Hering, it is well to 'go slow' in opposing opinions in matters optical; but at any rate none of the observers in the present investigation found any noticeable change in the size of the image though imagining it placed at different distances.

B. Description of Experiment

Apparatus. After considerable experimenting as to form, size, color and distance of the object producing the after-image, the following arrangement was found most satisfactory. The inducing object was a 22 cm. square of red paper (shade No. 1, Bradley Educational Colored Papers), placed with its center at the level of the eyes on a sheet of gray (Bradley green gray No. 1) 52 cm. x 62 cm. The whole was tacked to a vertical wooden screen, the base of the screen being on a line parallel to and 71.12 cm. from the side of the table at which the observer was seated. To a second wooden screen like the first, two sheets of green gray paper (Bradley No. 1) 52 cm. x 62 cm. were tacked at the top only, allowing the upper sheet to be easily put out of the way. The under sheet was ruled in 5 mm. squares, every fifth line being heavy, making heavy lined 25 mm. squares. A meter stick fastened to the edge of the table between the observer and the first screen served as a scale along which the second screen was moved with its plane parallel to the plane of the first screen and at right angles to the line of vision of the observer. A headrest completed the apparatus. The room was lighted by two south windows behind the observer, and lying to his left and right. The light was kept as nearly uniform as possible.

The results noted here are from three observers, *A., C., L.; A.* and *C.* are members of the Psychology Department; *L.* is a student with considerable experience in laboratory method and in introspection. A fourth reagent took part in the investigation, but on account of visual troubles which made the development of after-images uncertain, the findings of this reagent are not entered in the tables.

Comparison and Projection of After-images

The observer was seated directly in front of the square of red paper, as close to the table as possible. The second screen was placed along the edge of the table in front of the observer, thus making a vertical wall in his face. By aid of the headrest the head was pushed forward until the nose almost touched the screen. The distance from the eye to the screen was measured; then the first or stimulus screen (with the square of red paper) was adjusted until the distance from the eye to the red square was exactly 71 cm. The second screen was then removed. This procedure was carefully followed from day

[2] *Beiträge zur Physiologie,* Heft 1,638.

to day, thus keeping the distance from the eye of the observer to the screens constant and the position of the head unchanged.

Securing the Distance of Projection

The following instructions were given to the observer:—"On the signal, 'Ready,' fix the gaze and attention on the center of the red square. (Indicated by a small dot of white paint. A fixation of 10 seconds was allowed.) On the signal, 'Close eyes,' do so and note the size of the after-image on the fields of the closed lids. Then project the image on the screen of gray paper which has been placed before you. Try to project the image on the surface of the screen (not through it), at the place indicated by the dot. (A dot was placed directly on a level with the eyes on the second or projection screen at the time of adjusting the headrest. The dot assisted in holding the after-image quiet and also prevented increasing the distance between eye and screen by sideward and downward projection.) Note the size of the projected image and compare size with the image on the closed lids, using the latter as a norm. Give introspections. The judgment-terms you are to use are as follows: much less, less, doubtful, like, larger, much larger. State whether the image is clear or dim."

The procedure was then as follows: The observer developed an after-image, closed his eyes and noted the size in the darkened field. Meanwhile the experimenter had placed the second screen of gray paper in front of the observer,—in the preliminary experiments the screen was placed about 65 cm. from the eye of the observer to begin with,—who then projected his image on the gray screen and compared its size with that of the image projected on the closed field of vision. The experiment was then repeated, the projection-screen being moved nearer each time until the image on the screen was pronounced smaller than the image with the lids closed. In this series a region of like and doubtful judgments was usually passed through. In a subsequent series the time and space order were reversed. When binocular vision became difficult, as the screen approached the face, one eye was used.

Practice in developing and comparing after-images was given for several days until the observer was thoroughly familiar with the whole procedure. During this time the experimenter was able to outline roughly the distance from the eye at which the images under the two conditions of projection were pronounced 'like.' Having roughly blocked in the field, the projection screen was moved back and forth 6 or 7 cm. at a time. A set of experiments consisted in securing a series of judgments from much greater to much less, passing through an intermediate series of 'like.' Ten such series of experiments were performed with each observer. With increasing practice, there came a very considerable increase in ease in comparing the two forms of after-image, so that many judgments of larger and smaller were delivered with a feeling of complete security. One of the reagents was able to make a double comparison, i. e., after comparing the image with eyes closed and the image on the gray screen he was able to close the eyes again and make the reverse comparison. Still, for obvious reasons the comparison of images is not as easy a process as comparing outside objects, so that in giving figures of the results it seems better to indicate the region within which the images may be regarded as approximately equal rather than to figure out set distances.

C. Results

The regions of fluctuating, like and doubtful judgments for the several reagents are as follows:

Table I

	Cm.	*Larger*	*Smaller*	*Like and Doubtful*
	17,8	4	3	3
	17,1	4	3	3
C.	16,5	2	4	4
	15,9	2	4	4
	15,3	2	6	2
	8,2	8	1	1
	7,6	5	3	2
A.	7,0	4	3	3
	6,4	1	3	6
	5,7	0	4	6
	5,1	0	10	0
	9,5	7	0	3
	8,9	3	0	7
	8,2	2	0	8
	7,6	3	0	7
L.	7,0	3	0	7
	6,4	3	0	7
	5,7	0	1	9
	5,1	0	3	7
	4,5	0	6	4

For *C.* therefore, there was a stretch of about 2 cm. lying between the distances of 15, 9 and 17.8 cm. from the cornea where he was unable to distinguish clearly or securely between the sizes of the two after-images; the corresponding values for *A.* are 2 cm. between the distances 5.7 and 7.6 cm. and for *L.* 2.5 cm. between 6.4 and 8.9 cm. But these figures differ widely from the distances of clearest vision for the several reagents.

Using Scheiner's method for determining the near-point of distinct vision, the following values were obtained as averages of 10 determinations:—

	Right Eye	*M. V.*	*Left Eye*	*M. V.*
C.	19,5	1,3	24,1	2,1
A.	74,9	3,1	73,3	2,1
L.	33,2	3,6	31,9	5,7

These figures indicate that the nearest point of clear vision does not determine the distance at which we seem to project the after-image in the field of the closed eyes or, what amounts to the same thing, in a darkened field; and the question arises: How far away is the image projected? Assuming that the apparent size of the image is governed, as in the case of the opened eyes, by the distance to which we project it, we can measure the size of the image projected on a screen and then compare the results with the figures which give the distances at which the two forms of image appeared approximately equal.

The next step of the experiment accordingly was to measure the size of the image projected on the gray paper ruled into 5 mm. squares previously mentioned, the inducing square of red paper being moved in or out with steps corresponding to those used in the first part of the work. This measurement, which was usually accomplished by comparing the upper or side lines of the image with the lines on the paper, was at first anything but an easy task; to hold the image bright and clear on the paper and at the same time measure the spaces covered by it was something requiring considerable practice.

In determining the dimensions of the projected after-image the ruled screen was moved in or out for *A.*, over a series of 20 steps: for *C.* and *L.* the steps were 18 and 15 respectively. From these series we shall select here only those which, in the first experiment, correspond to the region where the difference in size of the after-image in the light and dark fields was not noticeable, i.e., the region where they seemed about equal. These regions and the length of the side of the corresponding after-image are given in mm. in the following tables:

	C.	*A.*	*L.*
Region of equality..................	159—178	57—76	39—64
Side of after-image (av. of 10).......	55— 63	25—30	20—25
M. V. of readings....................	2,5 2,3	2,4 2,6	2,2 1,6
Geometric projection of after-image at above distances	49— 55	18—24	12—20

We find then very considerable differences in the size of the after-images in the darkened field; but we are not prepared to say on what this difference depends. Noticeable is the difference between the apparent size of the image and its size calculated on the basis of the dimensions of the schematic eye. And in every case the observed image is larger than the geometrical projection of the retinal image. Broadly speaking, this difference increases absolutely and so of course relatively with decreasing distance of projection. Certain observations, made during the course of the investigation, may later be of value in working out the factors determining the size of the image in the darkened field. In the first place, the readings and measurements were practically alike for both eyes and for one eye. In equating the size of the two forms of after-image, series for each eye supplemented those for both eyes as given above, and the results always fell within the limits of the figures for both eyes.

Next, convergence seemed to play no important rôle in determining the size of the image or at least not a prominent rôle. Hering states that neither convergence nor accommodation plays any part at all.

One of the writers (*A.*) thought he noted the effect of convergence, but until he acquires somewhat of the *Virtuosität* in the control of the eye-muscles which Hering seems to enjoy, he prefers not to press the matter.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXI. The Effect of Verbal Suggestion on Judgments of the Affective Value of Colors

By Inez Powelson and M. F. Washburn

The method followed in this study may be briefly described as follows. Pieces 2.9 cm. square were cut from all of the ninety Bradley colored papers. The observer was shown these colored squares one at a time on a white background, and was asked to judge the pleasantness or unpleasantness of the color, using the numbers from 1 to 7 in the usual way. The order of the colors was determined by chance at the outset, but was adhered to thereafter during the research, so that the conditions of affective contrast should be uniform. The colors from the thirty-sixth to the fifty-fourth in the series, that is, the middle eighteen of the series, were presented with an accompanying verbal suggestion as to their affective value. The entire experiment was performed twice with the same subject, at sittings separated by an interval of several days. In the first sitting, for half of the observers, the verbal suggestions accompanying the middle eighteen colors of the series were suggestions of unpleasantness, and in the second sitting the suggestions were of pleasantness. For the other half of the group of observers this procedure was reversed, the pleasant suggestions being given at the first sitting and the unpleasant ones at the second sitting. The suggestions took the form of favorable or unfavorable adjectives pronounced by the experimenter as the color was shown. For example, when a given color was shown in the series with unpleasant suggestion it would be accompanied by the adjective 'faded;' when the same color was shown in the series with pleasant suggestion, its accompanying adjective would be 'delicate:' another color would be termed 'warm' in the series with pleasant suggestion, and 'crude' in that with unpleasant suggestion. When an observer had completed both parts of the experiment, the following values were calculated: the average affective values of the colors in the first series exclusive of the middle eighteen, that is, the colors with no accompanying suggestion in the first series; the average affective value of the colors with no accompanying suggestion in the second series; the average affective value of the colors under the influence of the unpleasant suggestions, and the average affective value of the colors under the influence of the pleasant suggestions. The usefulness of finding the average affective value of the colors unaccompanied by suggestion, in both series, lay of course in the fact that these numbers served to indicate whether the difference in the values of the colors with opposite suggestion in the two series was the effect of the suggestions, or the expression of a general tendency to lower or raise the values in the second trial. For instance, suppose that the average value assigned to the colors under unpleasant suggestion was .5 lower than their average value under pleasant suggestion, the series with pleasant suggestions having been given first. If now the average value of the colors unaccompanied by suggestion was in the second series higher than in the first series, then it would seem probable that the lowering under unpleasant suggestion was really due to the suggestions, and not to some influence affecting suggested and unsuggested judgments alike. It is possible to obtain a numerical representation of the strength with which the suggestion acted in a positive or negative direction (obviously in some cases the effect of this direct verbal suggestion would be negative, influencing the observer toward the opposite affective accompaniment). Such numerical values may be calculated by finding the difference between the average affective values of the 'unsuggested' colors in the two series, the difference between the average values of the colors under the opposite suggestions, and subtracting these differences from each other, paying regard to signs. Thus for example: for one observer the series with unfavorable suggestions is the one given first. The average value of the 'unsuggested' colors in the first series is for this observer .38 lower than the corresponding value in the second series. The average value of the 'suggested' colors in the first series, that is, the colors under unfavorable suggestion, is 1.5 lower than the value of the colors under favorable suggestion in the second series. We may estimate the effect of the suggestion by subtracting .38, the amount of difference due to causes other than suggestion, from 1.5: it would thus be numerically represented by 1.12. In the case of another observer, to whom also the series with unfavorable suggestion was the first one presented, the value of the 'unsuggested' colors was .15 lower in the first series, and the value of the colors under unpleasant suggestion was .44 higher than that of those under pleasant suggestion. We must conclude the influence of suggestion to have been negative in this instance, with a force represented by the sum of .15 and .44; the unfavorable suggestions, so far from lowering the values of the colors they accompanied, overcame a slight tendency to lower values in the series where they occurred, and produced a total raising of .59. Of course it must be admitted that these numerical values are anything but exact measures of the suggestibility of the observers under the given conditions. On the one hand, the average tendency to raise or lower the values of the 'unsuggested' colors in one series as compared with another might not have manifested itself in the judgments made on the eighteen colors, in the middle of the series, had these been unaccompanied by suggestion. And on the other hand, other influences beside suggestion may have helped to produce the difference between the values of the colors under unfavorable and those under favorable suggestion. In comparing the results from individuals, therefore, small differences cannot be taken as having any significance.

We had thirty-five observers, all young women students. Twenty-five of these gave results indicating a positive effect of suggestion in altering the judgments of affective value. In eight cases the amount of alteration was more than 1, an amount which could hardly be due to any influence but that of suggestion. In six other cases the alteration was .50 or more. In eleven cases it was less than .50, and in six of these was negligible in amount. Ten observers gave results indicating a negative influence of suggestion, tending to alter the judgments in the direction opposite to the suggestion. In only one of these ten cases, however, did the amount of alteration exceed 1; in two other cases it was .50 or more, and in five cases it was negligible in amount. The averages of the numerical values described above were .64 for positive suggestion and only .38 for negative suggestion. We may conclude, therefore, that direct verbal suggestion regarding the pleasantness or unpleasantness of a color has a fairly decided positive effect on the judgments of observers of the type and under the conditions found in our investigation.

A Note on the Comparative Pleasantness of Colors and Articulate Sounds. The average affective value assigned by all the observers in the above study to all the colors was 4.18. The average affective value of the nonsense syllables as estimated by the observers in the preceding study was 3.6. Colors, therefore, taking the agreeable and disagreeable together, are distinctly pleasanter than articulate sounds.

DISCUSSION

PROFESSOR DODGE'S RECENT DISCUSSION OF MENTAL WORK[1]

By F. M. URBAN

The wider field of dynamic psychology comprises the study of all the conditions of mental phenomena; but it is legitimate to define an investigation of narrower scope as the science of psychodynamics, which is the doctrine of energy transformations conditioning mental phenomena. This definition is slightly different from the one given by Lehmann, who defines psychodynamics as the exact doctrine of the quantitative influence of simultaneous or successive mental states on one another. Mental work can not be defined in mechanical terms, nor by opposing it to play. Neither is it possible to give this term a consistent introspective connotation since introspective differences of mental processes are no indication of conditioning energy transfers. The common measure of mental work is number and time as is seen from the adding test, where the number of operations performed in a minute is used as a measure. This measure is useful for practical purposes, but it is essentially non-dynamic; and it is even of restricted practical use because it must be assumed either that the output is maximum or that the effort is sustained and uniform. Feelings of strain and effort, the introspective indicators of mental work, are useful in some cases, but inconsistent and mutually contradictory in others. They are not the invariable conditions of mental work, but only its occasional by-products.

There remains the possibility of defining mental work in terms of organic metabolism as determined by calorimetric observations or by chemical analysis of the products of organic combustion. Theoretically, all nervous activity involves metabolism; and the question is whether we are able to correlate mental activity with metabolism and, consequently, to express it in thermodynamic units. The respiration calorimeter is not suited for psychological experimentation; but we may use the pulse-rate as a measure of metabolism because calorimetric experiments have shown that it is, under certain restrictions as to blood-pressure, proportional to metabolism, not only during predominantly physical work but also during long sustained mental activity. This correspondence is much closer for long than for short periods. This limits the scope of such investigations which, moreover, remain preliminary so long as they are not supplemented by calorimetric determinations. If we could experimentally eliminate all extraneous activities we might use the pulse-rate for arranging mental as well as physical activities along a general metabolic scale; and it is conceivable that these co-efficients of metabolism, expressed in thermodynamic units, may become a means of analysing the obscure conditions of mental life which introspection can not reach.

In the traditional experiments on the influence of mental phenomena on circulation, little attention is given to the initial state of the subject which is necessarily influenced by the instrumental arrangement and by the unfamiliar surroundings. The following specifications are

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[1] RAYMOND DODGE, Mental Work: A Study in Psycho-dynamics. *Psychol. Rev.* XX, 1913, 1-42.

formulated for the recording instrument to be employed: 1. Legibility of every pulse-wave to at least 0.005 seconds; 2. evidence, in the curves, of changes in blood-pressure; 3. simple and inconspicuous attachment to the subject; 4. transmission to an adjoining room so that the recording devices may not become an object of the subject's attention; 5. greatest possible freedom of movement for the subject; 6. frictionless recorder; 7. comparable records in two different sittings of the same individual as well as of different individuals. The experimental problem thus defined was solved by a telephone-galvanometer-sphygmograph consisting of a telephone receiver, the circuit of which was completed by the string of a galvanometer. The movements of the steel armature, which rested on an insulating surface directly against the skin over the artery, were sufficient to induce measurable currents in the coils of the magnets. The receiver was placed over the temporal artery, which affords the best conditions for a permanent attachment; it was kept in place by an elastic band. The movements of the needle were photographed by means of Dodge's falling-plate device. The instrument gives almost complete freedom of movement to the subject; and only very violent bodily activity disturbs the record. The most serious limitation of the instrument is that the height of the pulse-waves in different records can not be directly compared because it depends on the initial distance of the armature from the magnets and on the velocity of its motion.

Physical activity is correlated with an accelerated pulse, from which it follows that physical and mental work may be equated. Measurements of their respective metabolism show that the mental processes of multiplication, reading, or learning a series of twelve familiar words involve less work than 6, 4, or 2 genuflections; they are closely approximated by the raising of a one-pound weight once every four seconds. The relatively small energy transformations of mental work are not surprising since electrical and heat products of nervous tissue in action are relatively small in comparison to analogous products of muscular action. We call attention to Dodge's treatment of the pulse-waves, which is novel. He gives what practically amounts to tables of distribution of the wave-length for the different experiments and attempts to draw conclusions from these data. He then tries to explain his results in the light of information as to the way in which the subjects behaved before and during the experiment.

The paper sets a new standard for sphymographic experiments. It shows that the subject may be given complete freedom of motion, which is absolutely essential for experiments which last some time. Keeping the subject motionless in the same position produces a strain and interferes with the normal development of the processes under observation, not to mention that in a somewhat prolonged sitting the subject may fall asleep, thereby depriving the experiment of all psychological interest. It seems that this requirement disposes of the plethysmograph and definitely settles the question in favor of the sphymograph as the instrument to be used in future investigations along this line.

It may be questioned whether Dodge's instrument furnishes the final solution of the problem. Some of his curves differ so widely from those traced by means of the Marey sphymograph that one can not let them pass unchallenged. The experimental critique by Mach and Marey has shown that the sphymographic curves are essentially correct; and any new instrument which traces them differently must remain under suspicion until evidence is forthcoming that the tracings are correct. Past failures make Dodge's success appear very remarkable; and the most surprising thing, as a matter of fact, is that his curves are as good as they are. I am indebted to the head of one of the largest firms of instrument makers in Germany for the information that within the last two years he has refused to accept ten offers to construct electric recording devices of the telephone type because past experience led him to believe that such an instrument would not work. The mechanical reason is that vibrations of small amplitude, though they may be of even very high frequency, are transmitted correctly by the telephone; but the telephonic transmission of motions of considerable amplitude is inaccurate. It may be that Dodge just struck the happy medium in certain cases, but failed to do so in others.

Dodge's attempt to make a statistical study of the pulse-waves is equally significant; and it is my opinion that the solution of the problem can be reached only in this way. One of the most commonplace observations in plethysomographic and sphymographic experiments is that certain variations of the curve occur in a great many cases, but fail to occur in other cases, although the objective and the subjective conditions seem to be exactly alike. It is bootless to say that the conditions must have been different, because if they had been alike they would have produced the same result. Indeed, if we were in possession of all of the information necessary to distinguish between these groups of conditions, the difficulty would not exist at all, and all further argument would be futile. The problem is to find out the differences between these groups of conditions which seem to be exactly alike in so far as our knowledge goes and which, nevertheless, produce different results. It seems a rather obvious plan to apply those notions which have proved useful in the study of similar problems in psychophysics, and to eliminate these accidental variations by a statistical treatment of the results.

Such an investigation will, in all probability, require a much closer analysis of the pulse-curves than has hitherto been attempted; and it will be necessary to study not only the length and the height of the curves but also their shape. Wave-form depends upon a number of conditions which we can unravel to some extent at least. The elasticity of the arterial wall, the action of the heart, and the blood flow through the arterial system all have a definite influence on the form of the sphymographic curve and they may be determined from it. It will be necessary to study not only the single elevations but also the variations in form which appear in the course of an experiment. This requires a considerable number of measurements which could not be obtained without great difficulty from curves of the size which can be traced today; and it seems that such an investigation could not be undertaken until our technique has been so improved as to enable us to trace accurate curves of considerable size.

It is not possible to discuss fully Dodge's specifications for the ideal recording instrument, because this would open up the whole question of the method of expression. Specifications 3, 4, 5, and 7 will very likely meet with undisputed approval; number 2 might be omitted, since several physiologists believe that the pulse-curves do not show any evidence of blood-pressure. Number 1 seems to demand a degree of accuracy which really is not needed. Dodge gives his tables of distribution in intervals of 0.020 sec., which make it appear to be a waste of energy to trace curves which could be measured with an accuracy of 0.001 sec. Number 6 is only another way of insisting on photographic registration, since this is the only recording device which is really frictionless. This advantage really amounts to very little so long as only the length of the waves is measured because an ordinary recording device would give this quantity with sufficient accuracy. It may be of some consequence if the form of the curve is taken into consideration; but even in this case one may rely on the observations of Mach and Marey that friction does not materially affect the tracing so long as it does not exceed certain limits in proportion to the impulse. Photographic registration, on the other hand, has the very serious drawback of being expensive and a little cumbersome, as is proved by the fact that Dodge took records during a very small part of the experiments only. I am inclined to substitute for number 6 the specification that the tracing must be correct with a sufficient degree of exactitude,—this to be tested by Mach's method; and for 1, that the curves be so large that they can be measured conveniently. I also favor the requirement that, for purposes of investigation, subjects highly trained in introspection should be used exclusively, for only in this case will one be able to correlate definite mental events with definite changes in the pulse-curves.

It may be doubted whether it is an improvement of the status of sphymographic experiments to link them up with the study of energy transformations. Any information as to energy transformations which take place as concomitants of mental processes certainly would be an extremely valuable addition to our knowledge; but no such information is at present available nor is it likely to be forthcoming soon, for calorimetric experimentation in psychology will be at least as difficult as plethysmographic or sphymographic analysis. One may venture to believe that the difficulties will be even greater because to the difficulties of isolating the mental processes will be added the difficulties of apportioning the proper amounts of energy to the different physiological processes going on at the same time. That this is not an imaginary but a very real difficulty is proved by the recent discussions between Lehmann, Exner and Hellpach on the notion of metabolism (as measured by the amount of carbonic acid secreted) during mental work. Dodge's hope that thermodynamic analysis may one day help us toward a better understanding of processes not accessible to introspection refers to an ideal state of knowledge, and has nothing to do with the present state of affairs. Similar hopes were expressed in regard to plethysmographic experiments; but the prophets have been singularly reticent of late.

Let us suppose, however, that all the experimental difficulties have been overcome successfully, and that we know the energy transformations corresponding to every mental process. Does that really give us a measure of mental work? The principle of the conservation of energy compels us to refer these energy transformations to the concomitant physiological processes, in which energy can be neither gained nor lost. The entire amount of energy, as determined by calorimetric measurement, is consumed by them; and no energy remains to be referred to the corresponding psychical processes. We have a complete understanding of the energy transformations which are involved in the physiological processes; but we are as far from a dynamic psychology as ever, for we can not equate mental work with physical energy, although we can correlate them. From this it follows that psychodynamics as defined by Dodge has the same limitations as the doctrine of some thirty years ago that psychology must express mental events in terms of brain-physiology.

The following interesting glimpse of individual psychology may be mentioned. On page 2 Dodge states that "actual science knows no other extension of knowledge except correlations." This is, of course, the well-known Mach-Pearson view which denies causal relations, and insists that the study of relations is the real field of science. In his paper on the "Theory and Limitations of Introspection"[1] Dodge finds fault with introspection because it "has never been able to fill out the causal relations of any fact of consciousness." The contradiction between these two statements is obvious; and it is all the more surprising because only a few months elapsed between the appearance of the two articles. It seems that the author vacillated between these two views as often happens when our ways of thinking are not yet fully adapted to a new idea. This is especially true in a case where the new view makes such large demands on the adaptibility of the subject as Pearson and Mach do when they ask us to give up the notion of causal connection and substitute for it the idea of functional dependence. A sentence on page 7 of the article under discussion might lead one to believe that transfers of energy which condition consciousness are the causal relations which Dodge had in mind when he denied that introspection could fill out the causal relations of the facts of consciousness.

Several passages of the paper are sure to arouse the antagonism of the reader who expects to find a calm argumentation. Expressions like "Our entire experimental knowledge of mental fatigue is on the yardstick basis" or "dynamic psychology has long been cast into the rôle of the ill-favored sister" smack of the sensational style of William James, which is not entirely pleasant to everybody. A patriotic plea for psychodynamics as a truly American science makes a curious impression at the beginning of a scientific article. The translation of the German *Bahnung* by facilitation does not seem correct; the term reinforcement appears to be in more general use. It is true that the questions connected with the problem of mental work have not aroused the interest they deserve; but it is misleading to mention Lehman as the only neglected worker in the field. It would have been not more than just to mention the name of Charles Henry, who has worked in this field for almost twenty years and who lately published a very remarkable book on *Sensation et énergie*. Dodge certainly fails to do justice to the views of the extreme introspectionists; and it would not be surprising if his views call forth a pointed answer from that quarter where every item of information which is not of purely introspective nature is ruled out of psychology. It seems to me that the only standpoint which equally takes both aspects of the problem into account is the one lately developed by psychophysics; its field is here defined as the study of those mental phenomena which are directly accessible to introspection, and of the processes and objects connected with them. This definition at once specifies the immediate object of psychology and provides a legitimate place for the information about correlates of mental phenomena which we can obtain from physiology and comparative psychology.

[1] This JOURNAL, XXIII., 1912. 226.

Die Praxis der Konstanzmethode. By F. M. URBAN. Leipzig, Wilhelm Engelmann, 1912. pp. 26.
Hilftabellen für die Konstanzmethode. By F. M. URBAN. *Arch. f. d. ges. Psychol.,* XXIV., 1912. 236-243.

The second paper contains certain tables and an explanation of their use for the solving of the calculations required for the evaluation of data taken by the method of constant stimuli. The first paper also contains this same material, along with other considerations, and we shall leave the discussion of them until later.

The first paper, as the name indicates, is a short description of the method of constant stimuli, or more properly, of the calculations connected with that method. This is really a brief summary of all of the former papers of the author dealing with this subject. Of all the psychophysical measurement methods, we have perhaps, the best understanding of the processes, as well as the theory, of the method of constant stimuli. There have been a great many attempts in the last few years to elucidate the hypothesis upon which this method is based and also to lessen the work of calculation as much as possible. With the method in its present form, the author hopes that it can be used for clinical purposes with as great ease and greater accuracy than any of the other psychophysical measurement methods that are used to-day.

In the method of constant stimuli, a judgment is considered as a chance event dependent upon the relation of the intensity of the standard and comparison stimuli and upon the psychophysical constitution of the subject. The experimental procedure of this method aims to ascertain with what relative frequency the different judgments occur upon our various comparison stimuli. These relative frequencies are viewed as probabilities with which the judgment will occur; and this leads directly to the notion of the psychometric functions, which give the probabilities of our judgments for all intensities of the comparison stimulus. The calculations are effected either by interpolation or by assuming an appropriate analytical expression. Such an assumption has the character of a hypothesis which must have two general requisites: it must fit the nature of the experimental procedure, and it must fit the facts of experience. The so-called Φ (γ) Hypothesis fits the facts and is the basis of the method of constant stimuli. The calculation of the thresholds and their co-efficients of precision is rather cumbersome, because it requires the solution of a system of equations by the method of least squares.

Urban has found that, by adopting a scheme proposed by Wirth (*Psychophysik,* 1912, p. 213) this work can be reduced considerably by employing tables which require only the space of two pages. These are the tables spoken of as appearing in both papers, along with the directions for their use. These tables are sufficiently extended to enable one to calculate the constants h and c for a comparison series of fifteen stimuli. In using them, one merely looks up the values of the products for the observed relative frequencies of the different comparison stimuli; these products are then added, giving directly the sums necessary for the setting up of the normal equations, which are then solved directly. The only check on the correctness of our calculations that is necessary is the assurance that we have copied our numbers accurately. The author shows that these tables may be applied only in cases where the number of judgments on our comparison stimuli are 25, 50, 100 or multiples of these, as the probabilities are calculated only to two decimal places. He also goes into a consideration of the accuracy of the final determination of the constants h and c, in view of the number of decimal places retained in our products.

The value of these tables can not be overestimated, as the experimental procedure of the method of constant stimuli has been so standardized that, it seems to the reviewer, with a little care and foresight, no case should occur where the tables cannot be applied to the results. An entire calculation for a series of seven comparison stimuli with the help of these tables will require only about 15 to 20 minutes; and with practice even this time may be shortened. Indeed, the reviewer has seen an entire calculation for such a series completed in less than 10 minutes. Such a calculation can not be effected in less than an hour with the help of an adding machine or any of the large multiplication tables; and even more time than this is required when using logarithms for obtaining these products. Furthermore, the chance of committing errors of calculation is very much reduced by the use of these tables. Thus, with their publication, it seems to the writer that the method of constant stimuli has become the most practical of any of the psychophysical measurement methods. With this method alone, the subject has no knowledge of the objective relation of the stimuli, which knowledge may cause an error of expectation. The theory underlying this method is probably better known than that for any of the others. The one argument against the use of the method of constant stimuli has been that the calculations required were lengthy and cumbersome; now, with the publication of these tables, this final difficulty has been removed.

Clark University, SAMUEL W. FERNBERGER.

The Consciousness of the Universal. By FRANCIS AVELING. London, Macmillan and Co., 1912. pp. x, 255.

Aveling has made a descriptive experimental study of the presence in consciousness of the universal idea. Such questions as the growth of meaning, the nature of abstraction, the existence of a thought element in meaning, and the relation of imagery to thought are examined; explanatory hypotheses and suggestions are added; and the book includes an historical sketch of seventy-two pages.

The experimentation was divided into two parts. In the first, Aveling attempted to arrange conditions under which meanings should be formed and become associated with nonsense words. The method consisted essentially in the systematic presentation to trained observers of ten sets of small pictures, with ten corresponding nonsense words,—the pictures representing familiar objects, such as carpenters' tools. musical intruments, etc., and each set being sufficiently homogeneous to justify its being grouped under one name. The words were then presented alone, and introspections obtained on the appearance in consciousness of the meaning which the word had acquired from its association with the pictures. The results were as follows: *1.* Four stages were distinguished in the growth of meaning, ranging from an initial stage in which the words had not yet acquired meaning, but served merely as antecedents to the revival of a visual image of one of the pictures, usually of the one shown during the sitting at which the introspection was taken, to a final stage in which the stimulus word could hardly be discriminated from its meaning, with which latter no imagery was discovered. These four stages were often not clearly marked, any one being sometimes exaggerated, sometimes short-circuited, by an observer. *2*. Abstract, imageless mental contents are present in thinking. Visual imagery, the presence or absence of which was the most striking feature of the protocols, varied from time to time, and after a few experiments underwent changes which Aveling characterizes as 'blurring,' 'becoming vague'; yet at the same time the observers reported certain knowledge of meaning. Meaning is not a product of images alone, nor in combination; to enter into meaning, the image must be present *as* something, *i. e.*, as subsumed under a concept. The strong and growing association obtains between the word and the concept under which the pictures and their images are subsumed. Such concepts are imageless, and they are present in all meanings; they are mental elements, irreducible to any form of imagery. Their presence is indicated by the appearance in the protocols of such expressions as 'knew what it meant,' 'had idea of,' etc., absence of imagery being sometimes asserted by the observer. These imageless concepts 'must have been abstracted from the pictures with which the word was learned;' and from the fact of the existence in the percept of both sensorial and conceptual elements, Aveling argues that there are two kinds of abstraction, sensorial,—or the envisagement of a sensorial element apart from accompanying sensorial elements,—and conceptual,—or the abstraction of the 'ideational concept or imageless thought content' from the sensorial elements for which it may come to stand in consciousness. *3*. Regarding the relation of imagery to thought, Aveling advances the following hypothesis, adducing in its support excerpts from the introspections of his observers, with interpretations and arguments: The main associations, and only necessary ones, in thought processes obtain between pure concepts and the conceptual elements of images. Where images are revived, in so far as they may be regarded as purely sensorial, they are revived by reason of a conceptual element in virtue of which alone they can become present to consciousness as images. The purely sensorial elements may in many cases be considered as by-products of the conceptual elements with which they occur in consciousness. The function of imagery is to impart stability to the concept. Moreover, thought deals with things; and the presence of things in consciousness is characterized by the sensorial content. The genesis of the concept cannot be explained without sensorial implications; and the close association between the two in perception gives rise to the tendency observed for the concept to reproduce imagery.

In the second part of the experiment, Aveling investigated the functioning in judgments of the ten words with their acquired meanings. The method consisted in the presentation of the word, orally or visually, with a modifier and verb, the observer being asked to supply a predicate. By using different modifiers,—'all,' 'no,' 'the first,' etc.,—it was aimed to induce respectively three sorts of meanings,—affirmative and negative universal, and particular. The observers were asked to give an introspective description of the meaning of the subject and predicate, and especially to note whether the meaning of the subject had a general or particular reference in consciousness. Aveling found that universal meanings tend to be present as concepts, in pure form or accompanied by imagery, while in particular meanings imagery is prominent. He expresses the belief that imagery which accompanies particular reference is more clear and vivid than that which accompanies universal meanings. Exceptions appeared, in that imagery was prominent with universal meanings in twenty per cent. of cases, and concepts were reported with individual meanings in four per cent. of cases. The first is accounted for on the ground that imagery appeared when thought was baffled, or occurred as representing pictures that would not fit the concept employed; also that concepts were overlooked by the observer in many cases where their presence is clearly to be inferred (131-132; 177, footnote). In discussing the four per cent. of cases where concepts were reported in individual meanings, Aveling maintains that the meanings were not truly particular, and that vague images were overlooked (188; 190; 254). The meaning of the subject may be accompanied by awareness of no reference to one or more pictures; by conscious reference to everything that could be included in the word; and by conscious reference to all or some of the pictures associated with the word. Such reference Aveling calls 'conceptual overknowledge;' and he regards it as a separate conceptual element, forming a fusion with the meaning content, for the reason that it is described by the observers in the same terms as those used in describing concepts,—'awareness of,' 'idea of,' etc. The meaning of the predicate, which may appear as a concept, occurs sometimes before, sometimes with, the word that expresses it. The former is more likely to be the case with universal, and the latter with particular judgments. Aveling is uncertain as to the explanation of this, but suggests that imagery, prominent in individual subjects, may 'conflue' into the predicate, and strengthen the tendency of a word-image to appear with it.

Aveling's method is in many respects an improvement upon methods of previous investigators in this field. As he himself points out, it induces the transfer of a meaning already formed to a new symbol, and not the formation of a meaning itself. In the opinion of the reviewer, Aveling's introspective findings do not justify his conclusion that the imageless concept is invariably present in the thought processes; nor, indeed, do they warrant the statement that the imageless concept exists. In many instances the author has allowed himself an unwarranted liberty in interpreting his protocols in favor of the conceptual element. For example, on pages 186 and 187, the statement appears that "We have . . . . . to record the presence of imagery noted by our observers as giving the meaning of the subject in all three forms of judgment." A few lines on, and on pages 110, 131f, and 202, we read that failure of the observers to mention concepts does not establish their absence, and that "concepts were *de facto* present and operative, but they were not reported by our observers." In dealing with the individual meanings, where imagery should have been present, but was not recorded, the author states (254): "We are quite confident" that these cases are to be explained "by the presence of kinaesthetic sensations, which we have frequently observed in ordinary life as giving 'individual' meaning to words." Again, on page 148f, in discussing his theory of concepts, Aveling remarks, among other things, in answer to the possible objection that failure to report images is no certain indication of their absence, that "the absence of mention of images in a very considerable number of the protocols . . . . . is to be taken rather as a strong indication of their absence than otherwise; since in other cases images were duly noted and reported by the same observers." If vague sensorial contents are assumed in a few cases, why not in more? And on what grounds can an investigator allege the presence of a content, not noted in a very considerable per cent. of his introspections? Aveling nowhere describes the concept; and his treatment of it as a phenomenological content demands either its description, or proof of its unmistakable recognition by the observers as co-elemental with such factors as sensation and affection.

In many cases there seems to be evidence that the supposed concept is a product of the observer's inability at the time to analyze or even describe his mental content; the introspective descriptions are often vague and indefinite, and even self-contradictory. This appears especially in the following introspection, and those following it: "I had a distinct memory idea* of a hammer, with no image and no word, 'Goral'"—the nonsense word—"was in consciousness at the time, but did not express the hammer. They co-existed co-ordinately. The idea of hammer was localized above the stimulus word" (98-99). What can an introspector mean when he states that he has an *imageless idea* of a hammer, and adds that this imageless idea is *definitely localized in space?* It would appear that Aveling and his introspectors employ the terms 'imageless' and 'idea' in a wholly novel sense. The introspections and arguments advanced (especially 111-112) do not seem sufficient to establish the validity of the statement that images and their combinations are, of themselves, meaningless.

Again, in discussing 'conceptual overknowledge,' Aveling does not do justice to the fact that he had explicitly asked his observers to watch for particular or general reference (203). The treatment of imagery, from the point of view of its relative clearness in particular and universal meanings, is confessedly inadequate (189-190). The author apparently considers only two alternative modes of conscious representation of meaning,—more or less concrete visual or auditory imagery on the one hand, and the imageless concept, on the other (103 f); and of these two alternatives he accepts the latter. The possible significance of kinaesthetic attitudes, determining tendencies, activity consciousnesses, affective tonings, etc., is largely neglected. We must, then, conclude that additional data are needed, before the existence of the abstract, imageless concept, as postulated by Aveling, can be established.

Clark University, S. C. Fisher.

Conditioned Reflexes Excited by Visual Stimuli in the Dog Following Extirpation of the Occipital Lobes. Thesis for the degree of Doctor of Medicine. By N. K. Toropoff. From the physiological department of the Imperial Institute for Experimental Medicine. St. Petersburg, 1908.

Dr. Toropoff points out the lack of precise experimental data concerning the functions of the occipital lobes in higher vertebrates and the resulting divergence of views held by various physiologists. This state of things he is inclined to attribute largely to a fatal shortcoming common to all previously employed methods of investigation, namely, the circumstance that in all of them the sole criterion of the influence of a stimulus of any kind upon the nervous system of an animal is more or less complicated motor reaction. Thus in studies

*The terms 'idea,' 'ideopresentation,' are consistently used by Aveling in the sense of imageless contents of conceptual nature, *cf.* 97, 109, *et al.*

of the functions of the visual centers the effects of extirpation were judged from impairment of the ability of avoiding obstacles and from failure of the movements which are normally excited by the exhibition of attractive or fear producing objects: such results are difficult to observe and to interpret.

These considerations led the author to undertake a study of the effects of extirpation of the occipital lobes in dogs with the aid of the method that had been developed about six years previously in the laboratory of Professor Pavloff, it having been demonstrated by the many researches of Professor Pavloff's school that any act of perception may be brought into connection with salivary secretion.

The simple or direct reflex of salivary secretion which is normally excited by the presence of food or of dilute acid solutions in the mouth is utilized for the development in the experimental animal of a series of "conditioned" reflexes by a process of association training, as follows: any convenient mode of stimulation is selected, depending on the special needs and applied several times a day, being closely followed each time by the introduction into the mouth of a very dilute solution of hydrochloric acid; eventually the artificial stimulus alone suffices to excite the secretion of saliva.* Three, four, or five such conditioned reflexes can be readily developed in the average dog.

The conditioned reflexes are, of course, a cortical function, and their loss or persistence following the extirpation of certain areas furnishes a ready means of localization of cortical sensory centers.

Successful experiments were carried out on four animals. The special object in each case was to determine the ability of the animal to discern *form* (dark cross made of cardboard, appearing in front of a white screen), *movement* (four black feathers mounted on a rotating axle, a black shadow moving to and fro on a white screen brightly illuminated by means of a magic lantern), and sudden changes in *intensity of light* (turning on electric lights in a room previously darkened, or turning them out in a room previously brightly lighted).

Following the establishment of a salivary fistula, each animal was subjected to a course of training until the conditioned salivary reflexes in response to the proper visual stimuli were developed. To make possible in each instance a demonstration of the specificity of the defect of function resulting from the extirpation, a conditioned reflex in response to an auditory or a tactile stimulus was also developed. The operation of extirpation was then performed and on the animal's recovery the presence or absence of the conditioned reflexes was ascertained.

In *Animal A.* ("Volchok") a portion of each occipital lobe was removed bounded below by a horizontal line extending backward from the upper end of the Sylvian fissure, and in front by a vertical line erected at the middle of the lower boundary. In this, as in the other animals, the first effect of the operation was the disappearance of all conditioned reflexes; but after several days salivation in response to sound, light, and a moving shadow reappeared; only the ability to discern (indentify?) objects was permanently lost.

In *Animal B.* ("Castorka") a larger portion of each hemisphere was removed, the lower boundary being the same as in *Animal A.*, and the front boundary being a vertical line erected at the anterior

* For a description of the method in English and for a full bibliography (to 1909) see the article by Yerkes and Morgulis in the Psychological Bulletin, Vol. VI., No. 8.

end of the lower boundary. The results were similar to those observed in *Animal A.*

In *Animal C.* ("Gryzhka") a still larger mass of brain tissue was removed: the front boundary was the same as in *Animal B.,* but the lower boundary instead of being horizontal extended obliquely downward and backward at an angle of about 135° in relation to the vertical line. Again the conditioned light reflex returned as did the conditioned sound and tactile reflexes; the discernment both of form and of movement was lost. Further, an added phenomenon was observed, "the chaotic state:" a whole series of non-specific auditory and tactile stimuli—electric bell, pricking of any part of the skin surface, application of heat or cold—produced reflex salivation.

Finally, in *Animal D.* ("Sultan") the largest mass of brain tissue was removed: by an oblique cut starting from a point at about the middle of the great longitudinal fissure and extending downward and backward through the upper end of the Sylvian fissure fully one-third of each cerebral hemisphere was excised; in other words, not only the occipital lobes were removed but also portions of the parietal and temporo-sphenoidal lobes. All the conditioned reflexes, including those for auditory and tactile stimulation, were lost, with this exception: in the training prior to the operation the dog was grasped by the jaw whenever the acid solution had to be introduced into the mouth and thus a special conditioned reflex had been formed unintentionally, and this reflex was found after the operation to have been preserved, so that while no secretion of saliva resulted from stimulation with scratching, sound, or light, trickling at the fistula was observed regularly every time the animal's jaw was grasped.

Not the least interesting result in the entire series of experiments was a demonstration of the apparently unimpaired educability of *Animal D.* following the operation: Dr. Toropoff easily succeeded in developing a new and highly specific conditioned reflex to occur in response to stimulation with the odor of camphor.

A. J. Rosanoff.

Stuttering and Lisping. By E. W. Scripture. New York, The Macmillan Co., 1912. pp. xiv., 251. Price $1.50.

This is a practical book, which has been prepared to meet the needs of physicians and teachers. Part i. discusses Stuttering (description and cause; symptoms, forms, nature; diagnosis; therapy; methods of treatment); Part ii., Lisping (general discussion; negligent, organic and neurotic lisping; cluttering); and Part iii. outlines 18 sets of Exercises. There are over a hundred illustrations, including a large number of graphic records. Dr. Scripture's experience in the speech-department of the Vanderbilt Clinic, as well as his theoretical work on phonetics, have well fitted him for the task he has here undertaken.

The Psychology of Insanity. By B. Hart. Cambridge Manuals of Science and Literature. Cambridge, University Press; New York, G. P. Putnam's Sons, 1912. pp. ix., 176. Price 40c net.

After an outline of the history of insanity, through the demonological, political or social, physiological and psychological periods, and a brief characterisation of the psychological point of view, the author arranges his material under three heads: symptoms, classification, explanation. The keynote of classification is Dissociation, and of explanation, Conflict. The greater part of the book is taken up with a discussion, in terms of Freud's and Jung's hypotheses, of this explanation by Conflict: successive chapters deal with complexes, repression, manifestation of repressed complexes, projection, irrationality, phantasy or day-dreaming. In a final chapter on the Significance of Conflict the writer allows great importance to the sex-instinct, but also lays emphasis on the 'herd-instinct' of Trotter.

The details are held thoroughly in hand, and the style of the work is easy and pleasant; only the continual recurrence of the didactic 'now' becomes somewhat annoying.

Ueber den Traum: experimentell-psychologische Untersuchungen. Von J. MOURLY VOLD. Herausgegeben von O. Klemm. Zweiter Band. Leipzig, J. A. Barth, 1912. pp. vi., 449-879. Price Mk. 11.

The first volume was reviewed in the JOURNAL, xxii., 1911, 455 f. Enough was said at that time to indicate the importance of the work. The present, concluding installment of the material covers (1) normal, non-experimental dreams, with stimulation of the lower extremities; dream phenomena in pathological conditions of the lower extremities; (2) experiments on stimulation of the upper extremities; glove-experiments, etc.; experiments with left-handed persons; and (3) experiments on back and foot; the part played by touch and temperature sensations in dreaming; dreams composed of a number of elements (nightmare; the dream of dreaming; dream speech); the dependence of dreams on various conditions (brief muscular excitation on the evening preceding the dream; visual images; habitual dreams; inheritance of dreams). It is evident that the hoped-for theoretical discussion of the dream-consciousness is not forthcoming; we are again left with a number of somewhat discontinuous observations, valuable in themselves, but needing systematic treatment. There is still no index.

Aristoteles über die Seele. Neu übersetzt von A. BUSSE. Philosophische Bibliothek Bd. 4. Leipzig, F. Meiner, 1911. pp. xx., 121. Price Mk. 2.20.

Since the appearance of Kirchmann's work in 1871, we have had a number of translations of the *De Anima*: in German, those of Bender, Rolfes, Essen and others; in French, that of Rodier; in Italian, that of Razzoli; in English, those of Wallace, Hammond, Hicks. Dr. Busse, who bases his translation on the text of Biehl, though he admits a number of conjectural emendations, sets his work in direct opposition to that of Kirchmann: "bevor wir die Frage der sachlichen Richtigkeit stellen, haben wir erst die Frage nach dem richtigen Sinn zu lösen." He has utilised the results of his predecessors, but relies especially upon the Greek commentators, "die in ihrem ganzen Denken dem Verfasser soviel näher standen und deshalb seinen Gedankengängen lèichter folgen konnten." A brief introduction (pp. vii.-xviii.) outlines the history of psychology from the Pythagoreans to Aristotle, and says what is necessary of the reliability of the Mss.; the text then occupies pp. 1-94; and the following notes (pp. 95-115) are explanatory of the text, or deal with salient points of textual criticism. The translation is clear and straightforward; and the cheapness of the book should give it a wide popularity.

A Brief History of Modern Philosophy. By H. HOEFFDING. Translated by C. F. Sanders. New York, The Macmillan Co., 1912. pp. x., 324. Price $1.50 net.

Professor Höffding has here compressed into 300 pages of fairly large print the history of philosophy from the beginning of the sixteenth to the end of the nineteenth century. When the German edition appeared in 1905 the reviewer read it with interest,—read it, however, rather as a summary of the writer's views and attitude than as a text-book for the use of students. It is, indeed, not easy to see what place the work has as a text; it is too full of facts, too much of a *catalogue raisonné* for the beginner, and it is too sketchy for the advanced student; possibly it will come to its own as the basis of a course of more discursive lecturing. The translation is adequate at the beginning, but grows somewhat careless as the book proceeds.

The nine 'books' deal respectively with the Philosophy of the Renaissance; with the Great Systems (Descartes, Hobbes, Spinoza, Leibniz); with English Empirical Philosophy; with the Philosophy of the Enlightenment in France and Germany; with Kant and the Critical Philosophy; with the Philosophy of Romanticism; with Positivism; with New Theories of the Problem of Being upon a Realistic Basis (modern materialism; Lotze, Hartmann, Fechner, Wundt; Bradley, Fouillée); and with New Theories of the Problems of Knowledge and of Value.

Précis d'autosuggestion volontaire; éducation pratique de la volonté. Par G. BONNET. Paris, J. Rousset, 1911. pp. iv., 302. Deuxième édition, revue et augmentée. Price fr. 3.50.

"One fact dominates the whole situation. It is the undeniable preponderance of autosuggestion in all the acts which have as intent and result the amelioration of our physical, intellectual and moral condition. Autosuggestion is everywhere; it intervenes on every occasion." Such is the text of the present work. As to the base of autosuggestion, it is to be sought in nerve-force, which is simply a mode of the universal force of electricity; "the will is a cerebral faculty by which we are able, *freely,* to dispose of a part of our nerve-force in the interest of some determinate, physical or intellectual task." The author outlines a history of hypnotism, and gives numerous instances of autosuggestion from his own experience. Then, turning to practical matters, he writes on the Education of the Will, on Self-reliance, on the Concentration of Thought, and on Personal Power. Many of the exercises recommended would have a wholesome effect; others seem to the reviewer to be distinctly questionable. The whole work is a mixture of science, common sense and a sort of mysticism, of which one can only say that it will probably do more good than harm to the uninstructed reader. We note that Dr. Bonnet accepts the experiments of Elmer Gates on the variation of color in breath-deposits with variation of emotion; Elmer Gates is "professor of psychophysics at the national *Smithson* Institute at Washington."

Mitbewegungen beim Singen, Sprechen und Hören. Von F. KRUEGER. Leipzig, Breitkopf und Härtel, 1910. pp. 22.

In this essay, which is reprinted from the *Zeits. d. Internationalen Musikgesellschaft* (xi., Heft 6 u. 7), Dr. Krüger outlines his first, provisional attitude to the Rutz theory (this JOURNAL, xxii., 1911, 450). He opens with an account of the lecture delivered by Dr. O. Rutz at Leipzig, and of the subsequent discussion. He then marks off the problems of voice-training, medicine, historical criticism and aesthetics from the purely theoretical problem which involves physics, physiology and experimental psychology. Next he turns to the facts of observation; and he decides that both on the side of audition and on that of movement and carriage Rutz has made out a very good case; a song rendered in the 'right' type 'sounds better,' there is something alike in all renditions of the same type, the type is auditorily recognisable, the designative terms used by Rutz are well chosen, movements and changes of posture can be seen in others and felt in oneself. What, now, of the scientific setting of these facts? As regards audition, we are in presence of changes of 'tint' in the wider Helmholtzian sense, or (in the author's terminology) of a 'complex-quality.' As regards movement and posture, we have to remember the facts of sensory co-excitation, and of motor reflexes and concomitant movements; the larynx is not a musical instrument inserted into an indifferent body, but has extraordinarily varied functional and anatomical connections. The adjustments of the trunk to which Rutz has called attention are involuntary concomitant movements, not separately perceived; they may be classed psychologically as a group of expressive movements; by their effect for sensation they enter into that particular 'complex-quality' which is known as feeling.

The Social Direction of Human Evolution: an Outline of the Science of Eugenics. By W. E. KELLICOTT. New York, D. Appleton & Co., 1911. pp. xii., 249.

This little volume, based on three lectures delivered in Oberlin College in 1910, is, as the subtitle indicates, an introduction to the study of eugenics. Ch. I. discusses the sources and aims of the new science, with quotation from Galton, Pearson and others. Ch. II. reviews the biological foundations of eugenics, with elementary discussion of fluctuation and variation, Mendel's Law and the statistical phenomenon of regression. The author rightly insists that, while "millions of dollars and an incalculable amount of time are spent annually" upon endeavors to raise individuals from a lower group up to or toward the average, the benefit to society would be immeasurably greater "if the same amount of energy and money were spent in moving individuals from the middle classes on up toward the higher." That there is a positive relation between order of birth and intelligence (p. 126) seems to be settled by the recent work of Cattell (*Psych. Bulletin,* Feb. 15, 1913, p. 54: "the first-born child has the best chance to become a scientific man"). Ch. III. treats of human heredity and the eugenic programme. Many human traits are known to Mendelise, but "little can be said regarding Mendelian heredity of mental traits because the psychologist has not yet told us how to analyse even the common and simpler psychic characters into their fundamental units." A number of sample family-histories are here charted; Goddard's Kallikak family furnishes a welcome addition. As to the programme of eugenics, it consists (1) in the "extensive collection of exact data," (2) in research into differential fertility, human variability, effects of nurture, and so on, (3) in immediate practice—positive, as sterilisation, and negative, as opposition to celibacy and warfare, and (4) in "the spread of the facts, far and wide, through all classes of society."

The Sexual Life of the Child. By A. Moll. Translated by E Paul, with an Introduction by E. L. Thorndike. New York, The Macmillan Co., 1912. pp. xv., 339. Price $1.75 net.

The author, as one would expect from his previous works, has given us a monograph that is both comprehensive in scope and sane in judgment. We note a few conclusions, taken at random: "I regard as one of the gravest scandals of our present penal system the ease with which a girl who makes a pretty curtsy in the court, and who appears to be shamefaced when giving her evidence, is believed by the judge or magistrate;" "I have been forced more and more to the conclusion that . . . . . the importance of the factor of sexual experiences in the causation of disease has been greatly overestimated by Freud;" "the sexual enlightenment of the child is advisable; . . . . . but for effecting enlightment . . . . . the school is unsuitable; this matter can best be undertaken by some private person, and above all by the mother; . . . . . choice of the time . . . . . must be guided . . . . . more especially by the indications of psychosexual development;" "it has not been proved that masturbation during childhood . . . . . is generally dangerous; the possibility of danger . . . . . is, however, increased by long-continued and frequently repeated masturbation, also by the artificial postponement of the voluptuous acme, and by congenital predisposition to nervous disorders."

Professor Thorndike gives the book a strong recommendation, though he wisely adds a word of warning to those "who are unused to descriptions of symptoms of diseases, abnormalities, and defects." The meat is, indeed too strong for the average 'educated' parent; and this for the simple reason that the knowledge which Dr. Moll purveys is not counterbalanced and put in perspective by like knowledge of the other great systems and functions of the body. A very useful, and on the whole a very reassuring little volume could be made up by selection of the author's conclusions and recommendations, with omission of details.

Historical Studies in Philosophy. By E. Boutroux. Translated by F. Rothwell. London, Macmillan & Co., 1912. pp. xi., 336. Price $2.50 net.

The World We Live In, or Philosophy and Life in the Light of Modern Thought. By G. S. Fullerton. New York, The Macmillan Co., 1912. pp. xi., 293. Price $1.50 net.

A First Book in Metaphysics. By W. T. Marvin. New York, The Macmillan Co., 1912. pp. xiv., 271. Price $1.50 net.

Conduct and Its Disorders Biologically Considered. By C. A. Mercier. London, Macmillan & Co., 1911. pp. xxiii., 377. Price $3.25 net.

We are glad to call attention to these books, though they lie somewhat far afield from the proper interest of the Journal. Professor Boutroux, whose reputation in France is perhaps second only to that of M. Bergson, and who is well known in this country by his study of William James, here discourses of five great figures in the history of thought: Socrates, the founder of moral science, Aristotle, Jacob Boehme, Descartes and Kant. Professor Fullerton essays, in simple and straightforward style, "the working out of a sober realism, which will not refuse to accept suggestions from the idealist where such seem helpful, but which will take pains not to be misled into doing injustice to the unmistakably real world given in experience." The book is meant for the plain man, and all technical notes are relegated to an appendix. Professor Marvin gives us a "student's first book in philosophy," which aims to "form a system of closely connected topics," to "represent consistently one contemporary philosophical tendency," and to accord with "the preceptorial method of instruction." After an Introduction, in which different views regarding the nature of philosophy are set forth and a definition of philosophy and metaphysics is offered, the book takes up in order the Nature of Science, and the Problems of General and of Special Metaphysics. Finally, the study of conduct, according to Dr. Mercier, resolves itself into the study of action and the study of ends or purposes. His first Book therefore examines the modes of human action under a number of headings,—spontaneous or elicited, abundant or scanty, instinctive or reasoned, original or imitative, etc.; his second Book, which is much longer, considers the ends that conduct strives to attain and the means by which these ends are compassed, dealing (always in the light of survival-value) with self-conservative and social conduct under all their manifold forms,—custom and fashion, patriotism and philanthropy, marital, parental and filial conduct, recreative and aesthetic conduct, investigation, religious conduct, etc. Dr. Mercier writes with a sincerity and vigor which compel respect, even if they do not always command assent.

Development of Religion and Thought in Ancient Egypt. By J. H. Breasted. New York, C. Scribner's Sons, 1912. pp. xix., 379. Price $1.50 net.

This modest little book, which contains a course of lectures delivered upon the Morse Foundation at Union Theological Seminary, is a work of real importance to students of comparative religion and social psychology. Professor Breasted is known both by his field-work in Egypt and the Soudan, and by his History of Egypt; and he here gives us in broad outline his view of the growth, consolidation and decay of the Egyptian religion.

The most important body of sacred literature in Egypt is, Professor Breasted insists at the outset, not the Book of the Dead, but the older 'Pyramid Texts,'—which are, in fact, "to the study of Egyptian language and civilisation what the Vedas have been in the study of early East Indian and Aryan culture." The content of these texts is sixfold: funerary and mortuary ritual, magical charms, very ancient ritual of worship, ancient religious hymns, fragments of old myths, and prayers and petitions on behalf of the dead king. It appears from them that a court-religion, the worship of the sun-god Ra, ran parallel with the worship of the Nile-god Osiris, the deity of the common people. "The fact that both Re and Osiris appear as supreme kings of the hereafter cannot be reconciled, and such mutually irreconcilable beliefs caused the Egyptian no more discomfort than was felt by any early civilisation in the maintenance of a group of religious teachings side by side with others involving varying and totally inconsistent suppositions. Even Christianity itself has not escaped this experience." Later, in the feudal age (B. C. 2160-1788), the moral sense emerges, and social justice becomes the official doctrine of the state; these ethical ideas are, in the writer's opinion, not of Osirian but of Solar origin. Still later, Amenhotep IV. (B. C. 1383-1365) attempts, and fails, to introduce a reform of religion on a monotheistic basis. And yet later we have the triumph of sacerdotalism; religion degenerates into usages, observances, scribal conservation of the old writings, and reaches its final decadence in the Osirianism of the Roman Empire.

There are, perhaps, two main points which may be urged in criticism of the book: first, that the Pyramid Texts are really terms in a series of documents which passes through and beyond the Book of the Dead; and, secondly, that the source and origin of the moral ideals which appear in the worship of Ra and Osiris have not been sufficiently cleared up. Professor Breasted may reply, with truth, that our knowledge of the earliest history of Egypt is still very imperfect. There is, at all events, no question as to the skill with which he has grouped his material in these lectures, and the value of the book to the student who is not expert in Egyptology.

The Lushei Kuki Clans. By Lt.-Colonel J. Shakespear. London, Macmillan & Co., 1912. pp. xxii., 250. Price $3.25 net.

The Tribes of Northern and Central Kordofan. By H. A. MacMichael. Cambridge, University Press, 1912. pp. xv., 259. Price 10/6 net.

The first of these monographs, published under the orders of the Government of Eastern Bengal and Assam, describes the tribes inhabiting the hilly district which stretches, roughly, from Chittagong on the southwest to Manipur on the northeast. The population scattered over this area of some 25,000 square miles may be classed as agricultural; the tribes were originally semi-nomadic, moving their villages to clear new patches of jungle as the old clearings became infertile; but they are now settling down to permanent residence and are taking to plough cultivation. They use an interesting series of measures of length, expressed by reference to the human body; there are some sixteen or seventeen measures ranging from *chang-khat* or the distance from tip to first joint of the forefinger to *hlam* or the distance a man can stretch with both arms extended. A curious measure of weight is *chuai,* as much as can be supported if hung from the tip of the forefinger palm downwards. A mouth-organ of gourd and reeds is a ruder form of the Japanese *sho;* similar instruments, under various names, are found in Borneo; and a one-stringed bamboo fiddle is constructed like the Malagasy *valiha* or the *satong* of Sarawak, but is bowed with a bamboo strip and not plucked as a harp. A high degree of religious tolerance is shown in the sketch-map on p. 63; here a Lushai has drawn the route from his own village to the village of the dead; but the Christian's village is shown to one side, with its own road leading under the protection of Ĭsua (Jesus) to a special Christian heaven.

The book deals in the regular way with domestic life, laws and customs, religion, folk-lore, and language of the Lushei and the non-Lushei clans, with an appendix on the families and branches of the Lushei. It is regrettable that the author uses Lushai for the inhabitants of the Lushai hills at large, and Lushei for the single clan which, under the rule of various Thangur chiefs, came into prominence in the eighteenth century: misprints are always possible, while in spoken reference the two words are indistinguishable. The volume is illustrated by water colors and photographs; the index is fairly full, but not always reliable; a map shows the localities inhabited by the several clans, and their probable place of origin.

Mr. MacMichael, whose book is comprised in the Cambridge Archaeological and Ethnological Series, deals with the tangle of tribes that dwell in northern and central Kordofan,—aiming to describe the antecedents of these tribes so far as any information on the subject can be gleaned from extraneous sources or from current native tradition. He rightly declines to accept Budge's identification of the truculent Bakkara with the Menti of the Egyptian inscriptions, and of the black tribes of Sennar with the Automoloi of Herodotus; he thinks, on the other hand, that the Kuraan, a black race of Tibbu stock, may be identified with the ancient Garamantes. His work is largely a matter of balancing divergent traditions and accounts, of striking probabilities and of exploding myths; but, though he declares himself to be a mere tyro in ethnology, his book contains some useful ethnological material. It is illustrated by photographs; there is no map.

Biological Aspects of Human Problems. By C. A. Herter. New York, The Macmillan Co., 1911. pp. xvi., 344. Price $1.50 net.

In this thoughtful and original essay, the late Dr. Herter, professor of pharmacology and therapeutics in Columbia University, has sought to approach certain problems of human life from the biological standpoint, and to interpret certain biological laws in their bearing upon human life. Bk. i. treats of the human body as a mechanism. The mechanistic theory of the living individual is accepted without reservation, though the writer declines to extend it to the 'social organism,' or indeed to press to any length the analogy between the individual and the state. The two functional powers which lie at the heart of human life are reproduction and growth, and consciousness and will. As regards the former, Dr. Herter inclines to a mnemic theory; as regards the latter, he holds that "the rational view as to the nature of consciousness is that sensory impulses, carried into an extremely elaborate cerebral mechanism, liberate there, through chemical changes in the ganglion cells, a kind of energy which manifests itself by giving to the individual the property of awareness of self;" "consciousness is a function of complex associated nervous structures in exactly the same sense that the motion of a limb is a function of complex associated neuromuscular structures." Free-will is an illusion; but scientific fatalism does not lead to hopeless resignation.

The author now passes to two instincts which "in their phylogeny or racial ancientness appear to be the most fundamental of all instinctive qualities in living protoplasm," the self-preservative instinct (Bk. ii.) and the instinct of sex (Bk. iii.). The four chapters of Bk. iii. discuss the instinct of survival, the defences of the body, self-preservation and the mental life, and death and immortality. The common interest in a future life points to a grounding in the instinct of self-preservation. "In the entire range of biological phenomena there is nothing to suggest that a continuation of life for any species is probable or necessary or desirable. . . . . I should like to observe the effects of teaching intelligent children . . . . . that a belief in personal immortality appears unreasonable and unnecessary in the light of science, and . . . . . is not improbably a . . . . . form of egotism based on the insistent obtrusiveness of the instinct of self-preservation." The three chapters of Bk. iv. are entitled Sex and the Individual, Sex and Social Relations, and the Male and Female Mind. They contain a great deal of common sense, and some heresy.

Bk. v., which was left incomplete by the author, deals with the fundamental instincts in their relation to human development; the three chapters are headed The Arts and Religion, Education and the Future of the Race, and The Fruits of Education. In these chapters there is, as is natural from their state of preparation, a falling-off from the standard of the earlier Books; given what precedes, their teaching is almost commonplace. A brief Conclusion sums up the leading ideas of the essay. There is no index.

On the whole, the volume is notable as expressing the mature views of an exceptionally intelligent and experienced man of science. That the exposition is strongest on the side of biology, weakest on that of psychology, is only what might have been expected.

Geschichte der Psychologie. Von O Klemm. "Wissenschaft und Hypothese," Bd. viii. Leipzig und Berlin, B. G. Teubner, 1911. pp. x., 388. Price Mk. 8.

Outlines of the History of Psychology. By M. Dessoir. Translated by D. Fisher. New York, The Macmillan Co., 1912. pp. xxix., 278. Price $1.60 net.

A History of Psychology, Ancient and Patristic. By G. S. Brett. London, G. Allen & Co., 1912. pp. xx., 388.

The Classical Psychologists: Selections Illustrating Psychology from Anaxagoras to Wundt. Compiled by B. Rand. Boston, Houghton Mifflin Co., 1912. pp. xxi., 734. Price $3.50.

These four books, which evidence a wholesome revival of interest in the history of psychology, will be heartily welcomed by psychologists. The best of them, in the opinion of the present reviewer, is that which heads the list. Dr. Klemm, who is *privatdozent* for philosophy in the University of Leipzig and an assistant in Wundt's laboratory, has our current psychology always in mind, and his history is essentially an attempt to trace the genesis of current doctrine. Hence the plan of the book,—which begins with a section on the general tendencies of psychology, metaphysical, empirical, explanatory; continues with a discussion of the development of fundamental concepts (definition of psychology; subject-matter of psychology, consciousness; classification; the mental element; methods of psychology; mental measurement); and ends with an historical outline of the most important psychological theories (sensation, space perception, feeling, will). The emphasis on recent achievement gives the reader a sense of reality which does not often attach to historical writing; and if the perspective is radically different from that of most works on the history of thought, this is not to say that the author is mistaken in his judgment. Altogether, an excellent little book.

Professor Dessoir finds that mind has been of interest from three points of view: those of psychosophy, of psychology proper, and of psychognosis. Practical and artistic interest (psychognosis) he deals with summarily in his Introduction. The theological and metaphysical interest (psychosophy) and the biological interests which culminate in modern psychology are set forth, in strictly chronological fashion, in the body of the work; the four chapters are entitled The Ancient Conception of the Life of the Soul (from the earliest times to the patristic period), The Doctrine of the Soul in the Middle Ages and the Renaissance, Psychology of the 17th and 18th Centuries, and Psychology of Recent Times. Professor Dessoir has a keen feeling for historical continuity; it is all the more unfortunate that his work ends with the third quarter of the last century. As they stand, however, the two histories of Klemm and Dessoir are mutually supplementary.

Professor Brett's History ends with Augustine; Siebeck takes us to Thomas Aquinas. Those who have studied their Siebeck will, perhaps, add little to their knowledge by the reading of Professor Brett's book. At the same time, Siebeck is difficult and dry; and Professor Brett is—if dry—less difficult. "The business of the historian," the author remarks, "is to record rather than interpret. He should confine himself to giving such interpretations of these phenomena as were actually given by writers contemporary with the events, and so presenting the views of both the believers and the sceptics. . . . . A history of psychology must not anticipate; it must be a record of beliefs about the soul and of the growth of the human mind in and through the development of those beliefs." That is one idea of writing a history of psychology; Klemm's is another. And it seems to the reviewer that Professor Brett, good and useful as his work is, has really chosen to fall between two stools: that of an impartial, non-anticipatory, monographic record, which, if it is to be thorough, demands far more space than he has taken; and that of a consecutive, developmental history, which demands—besides dates, biographical data, reference to cultural background—a constant prevision of the future. Let him, however, disarm criticism here as he has done in his preface. "The data included may appear to some badly selected; others will desire things that have been purposely omitted; in view of this it is permissible to indicate what method has been consciously pursued. The main emphasis is laid on what may be called psychological data in the strict sense; around these data are grouped such theories as diverge from the phenomena of consciousness to derivative doctrines of the soul's antecedents, environment, and future possibilities. The relevant parts of medical and religious theories are regarded as supplementing psychology in two different directions; the treatment of them is subordinated to psychology as the main theme." The book appears as a volume in Muirhead's *Library of Philosophy.*

A volume of selections is fair game for the critic; it is always possible to discover errors in translation, and to indicate passages that were more deserving of translation than those actually given. Dr. Rand's judgment is no more impeccable than that of another. On the whole, however, he has accomplished his self-imposed task with success, and the student who browses in his pages will make acquaintance with many an author who would otherwise remain unknown,—may (who can say?) be led by these sips and tastes to read the original writers in their entirety. It seems hardly necessary to give space in such a book to James Mill, Bain, Spencer, Lotze (Ladd's translation of the *Outlines*), Mach (Open Court translation), James, and the current translations of Wundt: are not these things in every library, at the call of the student? Yet perhaps, for the sake of historical perspective, it was worth while to include them; there is room, at any rate, for difference of opinion.

The Kallikak Family: a study in the heredity of feeble-mindedness. By H. H. GODDARD. New York, The Macmillan Co., 1912. pp. xv., 121. Price $1.50 net.

Dr. Goddard has been fortunate enough, as the archaeologists say, to make a 'find;' and he has also had the training which enables him to utilise his discovery to the utmost. Here are the facts. A family of good English blood of the middle class, settling on land purchased from the proprietors of the state in colonial times, maintains throughout four generations its reputation for probity and respectability. A scion of this fourth generation does two things: by casual intercourse with a feeble-minded girl he starts a line of mental defectives; and thereafter he marries a woman of his own quality, returns in this way to the traditions of his family, and starts a second line, of a respectability equal to that of his ancestors. Dr. Goddard has been able to follow out these two lines of descent through six generations, and thus has his finger on "a natural experiment of remarkable value to the sociologist and the student of heredity;" he is able, indeed, to make his comparisons upon far surer grounds than those on which Dr. Winship based his study of the Jukes and Edwards families. The 'good' line (Kallikak = good-bad) comprises 496 persons; here we have individuals prominent in various walks of life, while nearly all are owners of land or proprietors. The 'bad' line, coming down from the son of the original Kallikak of the fourth generation and the nameless feeble-minded girl, comprises 480 descendents; 143 of these were feeble-minded, 36 illegitimate, 33 were sexually immoral, mostly prostitutes, 24 were confirmed alcoholics, 3 were epileptics, 82 died in infancy, 3 were criminals, 8 kept houses of ill-fame, while only 46 have been found normal. Moreover, these people have married into other families, generally of about the same type; the collaterals have been traced and charted; and Dr. Goddard now has on record no less than 146 individuals.

What is the moral? "Such facts as those revealed by the Kallikak family drive us almost irresistibly to the conclusion that before we can settle our problems of criminality and pauperism and all the rest of the social problems that are taxing our time and money, the first and fundamental step should be to decide upon the mental capacity of the persons who make up these groups." Segregation and colonisation, the author thinks, "is not by any means as hopeless a plan as it may seem to these who look only at the immediate increase in the tax rate." As for sterilisation, as distinct from asexualisation, "we may, and indeed I believe must, use it as a help, as something that will contribute toward the solution, until we can get segregation thoroughly established." But, after all, "the first necessity is a careful study of the whole subject, to the end that we may know more both about the laws of inheritance and the ultimate effect of the [surgical] operation."

The book is written for the lay reader, and the strict scientific evidence for the positions taken and the conclusions drawn will be presented in a larger and more technical volume. It need not be said that this work will be eagerly expected. Meantime, the present account of the Kallikaks will do good service. The style is clear and simple; and the occasional lapses into 'journalese' will probably not offend the public to whom the monograph is primarily addressed. The well-arrayed genealogical tables, the many photographs, and the story of 'Deborah,'—a twenty-two-year-old representative of the 'bad' line, now and for the past fourteen years in the Training School at Vineland,—all add to the interest of the work for the general reader.

Experimental Psychology and Pedagogy. By R. Shulze. Translated by Rudolph Pintner. New York, The Macmillan Co., 1912. pp. xxiv., 364. Price $3.75 net.

This book, which is said to have had a large sale in Germany, is neither a complete text-book, nor a manual of experimental psychology, nor a manual of tests. The material is drawn from a number of sources. There are two chapters dealing with the problems of measurement and the mathematical treatment of results; eight chapters describing a number of the classical experiments in experimental psychology and experimental pedagogy which the author thinks are available as tests; and a chapter each on speech, physical work, mental work, and physical correlations. The author lays no claim to completeness in any direction; he has chosen rather to present his material in a popular style; and neither the technique of the experiments nor the results are to be relied upon as standard.

However much one may sympathize with the author's plea for experimental investigation as a basis for pedagogical work, the use of many of his experiments as tests is questionable. The stimulus threshold for sound can hardly afford an index of musical talent; the method of expression is not available as a test for feelings, because psychologists are not agreed as to the correlation between organic changes and feeling; and, again, it scarcely seems worth while, from the pedagogical point of view, to determine the reaction type of the child, since there is, as yet, no correlation between the type of reactor and mental ability. Moreover the technique of many of the experiments and much of the apparatus are too difficult, both for the child and for any experimenter who is not equipped with a thorough laboratory training. On the other hand, the author's use of photography in the study of mimicry and gesture, as an objective expression of emotion, is well worth the attention of English readers. Aside from these criticisms on the pedagogical side, there are two or three errors of fact. It is an unavoidable inference that the illustrations, which the author publishes, of Traube-Hering waves and Mayer waves are sphygmograms: on the contrary they are, apparently, volumetric tracings. The statement that the Traube-Hering wave is coincident with the respiration wave is incorrect. Again, the author confuses the muscular reaction with the mechanized reaction. There is no reason for supposing that the sensory reaction might not become fully as 'mechanical' as the muscular reaction.

The book is admirably printed; there are more than three hundred illustrations,—many of them cuts of apparatus taken from trade catalogues; and there are numerous photographs of actual experiments as performed by the author. These advantages, together with the easy and non-technical exposition, make the book serviceable for the general reader who desires some knowledge of experimental investigation in psychology and pedagogy. Indeed, for this purpose, we have none better, though the reader should be warned against accepting Schulze as authoritative in so far as details in methods or results are concerned. Except for an occasional hint of German style in the English words, the work of the translator is well done. It is unfortunate that he has not cited works in English. References to Sanford, Titchener, Judd and Myers for the technique of experiments, and to Thorndike, Pearson, Whipple and Brown for mathematical treatment of results and psychical correlations, would have added to the value of the book.

H. P. Weld.

Free Will and Human Responsibility: a Philosophical Argument. By H. H. HORNE. New York, The Macmillan Co., 1912. pp. xvi., 197. Price $1.50 net.

Historically, the author tells us, mankind has passed from bondage to freedom; from custom and habit to progress and change; from determinism to indeterminism. He then reviews and rebuts nine arguments for determinism, and rehearses twelve arguments in favor of free will. So far as the intellect is concerned, free will has the better of it. But feeling and instinct must have their say; and so we turn to pragmatism, where "the wish is father to the thought," and are shown that the choice between determinism and indeterminism 'makes a difference'—to the advantage of libertarianism.

Professor Horne is arguing to a foregone conclusion, and his writing, though clear enough, is superficial. It would, indeed, not be difficult to invert his reasoning; to show that mankind has passed historically, from superstition to law, from anthropomorphism to causation; to review and rebut twelve arguments for interminism, and to rehearse nine arguments for determinism, with the result that, so far as the intellect goes, determinism has the upper hand. The appeal to pragmatism is of the nature of an appendix; it is not integral to the main thought of the book; but here, again, Schiller and Thorndike would suffice for a reversal—at the author's level—of the conclusion at which he arrives.

Die Sprache des Kindes. Von A. WRESCHNER. Zürich, O., Füssli, 1912. pp. 43. Price 80 pf.

Vergleichende Psychologie der Geschlechter. Von A. WRESCHNER. Zürich, O., Füssli, 1912. pp. 40. Price 80 pf.

The first of these booklets gives a useful summary of our knowledge regarding the appearance and development of language in the child; the exposition is based upon the works of Neumann and the Sterns, supplemented by the personal observations of the author. The second booklet reviews the experimental psychology of sex; it would be far more useful than it is had the author given references. He concludes that, in general, women excel in sensitivity, memory and feeling; men in motility, spontaneous mental activity (discrimination, power of judgment), and energy of will. He favors coeducation.

The System of the Vedânta. By P. DEUSSEN. Authorised translation by C Johnston. Chicago, Open Court Publ. Co. 1912. pp. xiii, 513.

The name of the distinguished Indologist Paul Deussen—editor of Schopenhauer, and author of the monumental *Allgemeine Geschichte der Philosophie,* four of whose six divisions are now available—is sufficient guarantee of the classical nature of the book before us. Dr. Carus has done yet another service to the contemporary world of philosophy and psychology by publishing Mr. Johnston's translation of *Das System des Vedânta;* and although that work appeared in 1883, and is long since familiar to the student of oriental philosophy, psychology and religion, we may safely predict that it will now be read by many to whom the German original has remained a sealed book. The Vedânta system has five principal parts: theology, cosmology, psychology, the doctrine of transmigration, and the teaching of liberation. Of these, the third and fourth are of greatest interest to the psychologist; and Deussen's exposition is as clear as the nature of the subject will allow: witness, *e. g.,* the summary on the interaction of body and soul, p. 341.

The Evolution of Educational Theory. By J. ADAMS. London, Macmillan & Co., Ltd.; New York, The Macmillan Co., 1912. pp. ix, 410. Price $2.75 net.

This book shows the same clearness of exposition and lightness of touch that characterised the author's *Herbartian Psychology.* It shows also a true historical perspective, a generous width of reading, and for the most part a sound critical judgment. Nevertheless, we lay it down with a feeling of disappointment. Perhaps—for the work is the first number of Sir Henry Jones' series entitled *The Schools of Philosophy: a History of the Evolution of Philosophical Thought*—perhaps we had expected something different: something more positive, more systematic, more trenchant. What we get is a balance of argument and opinion which leaves us a little bewildered by the writer's evident optimism. However, let us look at the book itself.

Ch. I is largely taken up with questions of terminology—education and instruction, teacher and pupil, educator and educand—and with a provisional definition of education as a bipolar process, of a deliberate sort, consisting in the application of personality and the communication of knowledge to a reactive personality, with a view to the modification of development. Ch. II discusses the data of education: individuality, heredity, environment, time. Ch. III, on the historical aspect of educational theory, is characteristically discursive, though it culminates in the distinction of three educational epochs, the Socratic, that of the Renaissance, and the modern. We then pass to a series of historical chapters: ch. IV opens it, a trifle paradoxically, by a discussion of the probabilities of prehistoric times. Thereupon emerges the problem which the author regards as of greatest importance for professional teachers, the problem of formal discipline as opposed to specific education. Ch. V treats of the latter in all its stages from the matter-of-course standpoint of early society down to present-day vocational training; and ch VI gives a presentation of the theory of formal discipline, of the educational organon, which historically falls between those two extremes. After this we are on familiar ground; the great theories of humanism, naturalism, idealism and mechanism are set forth in as many chapters; and the well-known names receive their due meed of criticism and appreciation. Ch. XII summarises the educational outlook, so far at least as Professor Adams' temperament allows him to summarise at all; we are to expect much from 'personal cards;' we are to give a bias towards future life-work, and perhaps in the later stages of education to cross the borderline of the definitely vocational, at the same time that we do not neglect preparation for the leisure of life; we are to improve the status and the calibre of our teachers by "the development of influences already at work;" we shall attain an educational democracy in which "all will have an education suitable to the state to which their inclinations and capacities have called them." It is all hopeful, and it all seems a little vague. But the chapters are pleasant and profitable reading, and will be useful as chapters, even if they do not cohere into a determinate 'platform.'

Principien der Metaphysik. Von BRANISLAV PETRONIEVICS. Heidelberg, Carl Winter, 1912. 570 p.

After the introduction, the author first discusses the general purpose of being and the principle of negation. Then follow the general analysis of immediate experience and the establishment of the general determination of categories of being, the world as full of and as without relations, *sein und werden*. The second section develops negation. The first deals with the formal principles, time, space, number, motion. To this chapter there are various and elaborate appendices of a geometrical nature. The second division deals with the real categories and discusses extreme naive realism and the standpoint of metaphysical actuality. Then comes extra naive realism from the standpoint of metaphysical actuality, then absolute conscious realism or conscientialism from the metaphysical point of view; analysis of immediate experience and its basal facts, consciousness and will as the two extra conscious attributes of the ego, quantitative structure of the content of consciousness explained on the basis of the quantitative structure of the nature of will; qualitative differences, changes of the content of consciousness based on will. Then follow chapters on conscious matter, conscious soul and immortality, the dynamic world stadium and its value, negation in its relations to time, space, intensity, number, change, quality and quantity. Subsequent chapters deal with the relations of negation to the mind, thought and knowledge, assuming the indentity of thought and being, with a final chapter on the limits of knowledge in the field of original principles.

BOOK NOTES

Essais de synthèse scientifique. Par EUGENIO RIGNANO. Paris, Félix Alcan, 1912. 294 p. (Bibliothèque de philosophie contemporaine.)

The topics treated are the synthetic value of transformationism, the biologic and the energetic method, the doctrine of mnemonics, the affective tendencies, what is consciousness, religious materialism, socialism.

Reaction-time to retinal stimulation. By A. T. POFFENBERGER. New York, Science Press, 1912. 73 p. (Columbia Contributions to Philosophy and Psychology. Vol. XXI, No. 1.)

After a history of the inquiry into the speed of nerve conduction, the writer discusses its speed through nerve centers, the analysis of conduction paths, experiment on reaction time to stimulation of different retinal areas through time reactions and finally, synapsis time.

Vorschläge zur psychologischen Untersuchung primitiver Menschen. Von RICHARD THURNWALD and others. Leipzig, J. A. Barth, 1912. 124 p. (Beiheft 5, zur Zeit. f. ang. Psy.)

This publication consists of a number of articles by different people, color and space, sense, memory, apprehension, suggestibility, comprehension, counting, expressive movement, gesture and language, science and art, investigations of the mode of thought, sociology, world Anschauung. This ought to be a valuable vade mecum for the field ethnologist.

The question of association tests. By FREDERIC LYMAN WELLS. Reprinted from Psychological Review, July 1912, Vol. XIX, pp. 253-270.

This valuable monograph treats of questions of procedure, cancellation, number checking tests, addition and naming tests, formation of new associations, logical relations, the understanding of instructions, free association experiments, while the appendix has a list of 1,000 stimulus words for the latter.

Interference and adaptability. By ARTHUR JEROME CULLER. New York, Science Press, 1912. 56 p. (Archives of Philosophy, No. 24. Columbia Contributions to Philosophy and Psychology, Vol. 21, No. 2.)

The first chapter describes typewriting and discrimination reaction experiments; the next, card sorting. Then follow chapters headed individual differences, discussion and summary.

The imaginal reaction to poetry. By JUNE E. DOWNEY. Laramie, Wyoming, University of Wyoming, 1912. 56 p. (Bulletin No. 2, U. of Wyoming Department of Psychology.)

The author discusses first the characteristics of the different forms of imagery, then general characteristics of the imaginal reaction, inner speech, etc., while in the three other parts of her treatise, she treats of the dependence of reaction upon material as seen in suggestion and style; the affective reaction, poetic fragments, with summary conclusions. She believes it possible to utilize method of style in determination of an author's type reactions and that vividness of imagery may be shown to contribute to the affective reactions to poetry.

Das Problem der objektiven Möglichkeit. Von AUGUST GALLINGER. Leipzig, J. A. Barth, 1912. 124 p. (Schriften der Gesell. f. psy. Forschung, Heft 16, iv Sammlung.)

The successive chapters here are concatenation of proof, positive and negative, the idea of cause, the ground of knowledge, cause and effect, the idea of objective possibility, empirical, regulative, concrete, abstract and hypothetical.

Ethics. By G. E. MOORE. New York, Henry Holt & Co., no date, 256 p. (No. 52, Home University Library of Modern Knowledge.)

The author treats of utilitarianism, the objectivity of moral judgments, the results of the test of right and wrong, free will, intrinsic value and concludes with notes on a few books.

Critique of impure reason. By FREDERIC LYMAN WELLS. Reprinted from Journal of Abnormal Psychology, June-July, 1912. 7 p.

Probleme der ethno-psychologischen Forschung. Von RICHARD THURNWALD. Leipzig, J. A. Barth, 1912. 33 p. (Sonderabdruck aus Beihefte z. Zeits. f. ang. Psy., Heft 5.)

A theory explaining the neural basis of subjective consciousness and the pure ego. By J. J. SEELMAN. Milwaukee, George Seelman & Sons Co., 1912. 31 p.

Lectures on moral philosophy.. By JOHN WITHERSPOON. Princeton University Press, N. J., 1912. 144 p. (Early American Philosophers.)

Association tests. By R. S. WOODWORTH and F. L. WELLS. Psychological Review Company, Princeton, N. J., 1912. 85 p. (Psychological Review Monograph, Vol. 13, No. 5, December, 1911.)

A note on the prognostic value of hallucinations in the manic-depressive psychosis. By EDMUND M. PEASE. Reprinted from American Journal of Insanity, Vol. 69, No. 1. July, 1912. pp. 119-123.

Spiritual surgery. By OLIVER HUCKEL. New York, Thomas Y. Crowell, 1912. 109 p.

Constructive eugenics. By WILLET M. HAYS. Washington, 1912. 13 p. (Reprinted from American Breeders Magazine, Nos. 1 and 2, Vol. 3.)

Die Bedeutung der Motilitätsprüfungen für objectiv-neuropsychische Studien. Von W. BECHTEREW. 1910. 40 p. (Sonderabdruck aus Folia Neurobiologica, Bd. 10.)

Mortality statistics, 1908. Washington, 1910. 705 p.

Mortality statistics, 1909. Washington, 1912. 810 p.

Studies in linguistic psychology. By ROBERT JAMES KELLOGG. Linguistic Psychology Series of the James Milliken University Bulletin, June, 1912. Vol. 1, No. 2. Published by the University at Decatur, Ill. pp. 65-128.

Das Problem der Willensfreiheit. Von G. F. LIPPS. Leipzig, B. G. Teubner, 1912. 104 p. (Aus Natur und Geisteswelt.)

The measurement of induction shocks. A manual for the quantitative use of faradic stimuli. By ERNEST G. MARTIN. New York, John Wiley & Sons, 1912. 117 p.

Folelsesbetoningens. Intellektuelle Egenvaerdi. (Er der nogen grund til en dualisme mellem erkjendeke og folelse?) By T. PARR. Kristiania, Olaf Norlis Forlag, 1912. 95 p.

English witchcraft and James the First. By GEORGE LYMAN KITTREDGE. New York, The Macmillan Co., 1912. 65 p. (From studies in the history of religions presented to Crawford Howell Toy by pupils, colleagues and others.)

Bulletin 74. Government Hospital for the Insane, Washington, D. C. Edited by William A. White, M. D. Washington, Government Printing Office, 1912. 94 p.

A dictionary of the Biloxi and Ofo languages. By JAMES OWEN DORSEY and JOHN R. SWANTON. Washington, Government Printing Office, 1912. 340 p. (Smithsonian Institution Bureau of American Ethnology, Bull. 47.)

Boletin de la Sociedad Española de Biologia. Año 11, Núm. 15, Agosto, 1912. Madrid, 1912.

On the determination of alkylamines obtained from urine after kjeldahl digestion. By C. C. ERDMANN. From the Journal of Biological Chemistry, Vol. 9, No. 2, April, 1911. pp. 85-92. (From the Chemical Laboratory of the McLean Hospital, Waverly, Mass.)

Following darkness. By FORREST REID. London, Edwin Arnold, 1912. 320 p.

Psychanalysis, its theories and practical application. By A. A. BRILL. Philadelphia, W. B. Saunders Co., 1912. pp. 337.

This book certainly supplies in its way a long-felt need, for we have no attempt to state concisely the principles of the Freud psychology in English. The chief chapters are psychoneuroses, dreams, obsessions, doubts, phobias, psychanalysis, mechanism of paranoia; the psychopathology of everyday life, hysterical fancies and dreamy states, the Oedipus complex, the only or favorite child in adult life, anal eroticism and character, Freud's theory of wit. In a work of this size the author cannot, of course, go much into detail, and the work is on the whole essentially elementary and introductory. Some may think that the author has been too prone to use cases from his own experience as illustrative material, and sometimes to the exclusion of the better material which Freud supplies, but in answer to this it can be said that a physician of experience can speak more vividly of his own, that it is natural for one who has done much translation to show a little independence, which the author's merit certainly justifies, and finally that the Brill cases are good in themselves and well selected. The intelligent reader cannot but regret, however, that more space was not given to the extremely important and suggestive departures of Jung, who seems to be breaking away from the master and evolving a set of views of his own, with which, however, Brill seems to have no great sympathy. The author is a practising physician, and readers of this journal will wish that he had given a little more attention to the psychological aspects of the subject, which just now seem to be looming up far beyond the ken of Freudians. This, however, must not detract from the great merit and serviceability of this most timely and welcome book.

Correlations of mental abilities. By BENJAMIN R. SIMPSON. Teachers College, Columbia University, Contributions to Education, No. 53. New York City, Teachers College, 1912. pp. 122.

After describing the general methods of investigation, the author describes the administration of the tests in detail, their order, instructions given in each variety and individual differences in the power to interpret; then comes the scoring of results, reliability, significance of tests, and analysis of general intelligence as shown by the difference between the good and the poor groups. Especially interesting are the tests of memory, association, apperception, motor control, test in selective thinking. Best is the comparison of these results with those obtained by other investigators.

The dynamic foundation of knowledge. By ALEXANDER PHILIP. London, Kegan Paul, Trench, Trübner & Co. Ltd., 1913. pp. 318.

Among the 38 chapters which constitute this book, which aims to present a new dynamical theory of matter, which owes something to the late J. B. Stallo, the author discusses the sensible world, the affirmative judgment, necessary postulates, activity, origins of metaphysics, realism, potential and actual, middle ages, terms of nature, sensationalism and intellectualism, defects of idealism and realism, nature of knowledge, cause, reason, materiality, necessity, energy, space, matter, axioms, science and reality, nature and art, ethics, science of language, meanings, unity of knowledge, opinion, general and singular, finally the applications of his dynamic theory to economics, education, and metrical standards.

Die Realisierung; ein Beitrag zur Grundlegung der Realwissenschaften. Von OSWALD KÜLPE. Erster Band. Leipzig, S. Hirzel, 1912. pp. 257.

This book constitutes an important pronunciamento of its author although it is only the first volume. In it he first discusses the permissibility of general realization, under which he treats the proof of consensualism, the logical difficulties of transcendence, the actual presentation of objects in consciousness; and in a second chapter he considers the proving of objective idealism. When the author has printed his work completely we hope for a fuller review.

Ästhetik der Gegenwart. Von E. MEUMANN. 2d ed. Leipzig, Quelle & Meyer, 1912. pp. 180.

The author gives an historical basis and then describes the foundations of empirical aesthetics by Fechner, whose influence he traces through another chapter. He then traces the chief direction in which the problems of present-day aesthetics have developed, defines the domain of research, the psychology of aesthetic recreation and enjoyment, the aesthetic aspect of art and of nature. A good compend with atrocious print.

Über den Willensakt und das Temperament; eine experimentelle Untersuchung. Von NARZISS ACH. Leipzig, Quelle & Meyer, 1910. pp. 324.

After an introduction in which the author treats of the object of his experiment and its limitations, describing systematic experimental self-observation in its relations to investigations of the will, the second chapter treats of his combined process and its application, the technical apparatus, the experiment, its results and the various persons taking part in the experiment. Then come the phenomenological results for the different series and results. The third chapter is devoted to the inferences from the data thus acquired concerning the act of will itself, and then the deed it does, results and applications, errors, variations, the abbreviated, weak and trained will. Chapter four is devoted to feeling and temperament.

Umriss einer neuen analytischen Psychologie und ihr Verhältnis zur empirischen Psychologie. Von WALTHER SCHMIED-KOWARZIK. Leipzig, Johann Ambrosius Barth, 1912. pp. 318.

In the first part the author discusses the essence of analysis, the empirical and analytic type of knowledge and psychology. The second part is entitled psychological systematics and treats of consciousness as a whole (the ego and the now), historical systems of the content of consciousness, various kinds of content and their divisions, and then takes up the three kinds of content which the author calls sensation, feeling and *Strebung.*

Zur Analyse der Vorstellungen und ihrer Gesetze; eine experimentelle Untersuchung. Von K. KOFFKA. Leipzig, Quelle & Meyer, 1912. pp. 392.

After characterizing the difference of the idea of description and function and the problem and methods, he gives a series of experimental tests in qualitative and quantitative analysis, also completion tests with optical stimuli, with restricted reproduction, and then passes to *Vorstellungen,* visual, acoustic, etc., and finally discusses determination, association, latent *Einstellung,* associative factors, the understanding of words, etc., on the basis of his experiments.

Experimental studies of mental defectives; a critique of the Binet-Simon tests and a contribution to the psychology of epilepsy. By J. E. WALLACE WALLIN. Baltimore, Warwick & York, 1912. pp. 155.

The more we discover of the psychology of the epileptic, the more successful will be our educational methods. The results of this book, says Whipple in his introduction, have added to our knowledge of the Binet-Simon tests. This work shows that these are far from being simple. Moreover they have many imperfections and limitations, so that this monograph makes a valuable contribution to the critique of the tests. They were made in the New Jersey State Village for Epileptics at Skillman. The author treats at length of the variations in mental and physical tests in relation to age and gives a practical guide for the administration of the scale in measuring intelligence. He has limited himself to a purely experimental and empirical exposition, assuming that the facts tell their own story and leaving the reader to work out their amplifications as well as to draw his own conclusions from the facts supplied.

Poètes et névrosés. Par Arvède Barine. Paris, Hachette et Cie, 1908. pp. 362. 2nd edition.

This interesting book consists of a critico-psychological analysis of the works of four writers, Hoffmann, Quincey, Edgar Poe, G. de Nerval, all of whom are believed to be neurotic. The author seeks by careful study first of their life, then of their works, to define the direction and extent of their alienation and also to consider whether and if so, what, it contributed to their literary success.

L'Année Psychologique. Paris, Masson et Cie, 1912. 18th volume. pp. 525.

This number is marked by a memoir and picture of the late A. Binet. It also contains a number of articles from his pen. The volume is larger than usual and possibly more interesting.

Autokinetic sensations. By Henry Foster Adams. Psychological Review Publication, July 1912. pp. 45.

His experimental results touch the following subjects: white light one cm. square, comparison of square and perpendicular lights, horizontal and oblique lights, McAllister's figures, the effect of size of light, monocular and binocular vision, voluntary control, suggestion, effect of light background, explanation, historical review, bibliography.

Selected papers on hysteria and other psychoneuroses. By Sigmund Freud. Authorized translation by A. A. Brill. Second, enlarged edition. New York, The Jour. of Nervous and Mental Disease Pub. Co., 1912., pp. 215.

The chief papers here translated are the psychic mechanism of hysterical phenomena, the cases of Lucy R. and Elizabeth v. R., the psychotherapy of hysteria, the defense neuro-psychoses, the rôle of sexuality, "wild" psychoanalysis, hysterical fancies and bisexuality.

Einführung in die Psychologie. Von Adolph Dyroff. 2d ed. Leipzig, Quelle & Meyer, 1912. pp. 144.

This booklet begins with the problems and aids, then treats psychic life in general, sense life, the life of concept, thought and speech, feeling and instinct, will and freedom, attention and apperception. There are no cuts, the print is fine and abominable, the literature at the end of each chapter copious and well-selected.

Questions of the day in philosophy and psychology. By HERBERT LESLIE STEWART. New York, Longmans, Green, & Co., n. d. pp. 284.

The author treats reforms in psychology, the sub-conscious, interpretation of genius, growth of public opinion, pragmatism, recidivism, pessimism, value of judgment, the cult of Nietzsche.

Über das Studium der Individualität. Von A. LASURSKI. Pädagogische Monographien, hrsg. v. E. Meumann, XIV. Band. Lpz., Nemnich, 1912. pp. 191.

This covers much the same ground as Stern's Differential Psychology. It begins by considering the idea of inclination or *Neigung* and then treats the neuropsychic organization and what are the essential bases of characterology, and how its various analyses are made, and concludes by defining the relations between individual and general psychology.

Problems in eugenics. Papers communicated to the First International Eugenics Congress, held at the University of London, July 24th to 30th, 1912. London, Published by the Eugenics Education Society (6, York Buildings, Adelphi, W. C.), 1912. pp. 490.

The papers here printed are classified as follows: 8 under biology and eugenics; 5, practical eugenics; 2 under education and eugenics. The largest divisions are sociology and eugenics, with 9 papers, and medicine and eugenics with 8.

L'Année Pédagogique, publiee par L. CELLÉRIER et L. DUGAS. First year, 1911. Paris, Félix Alcan, 1912. pp. 487.

The articles are on School and Life, by E. Boutroux; Ideal and Education, by Cellérier; Sympathy in Education, by L. Dugas; Psychological Study of the Methods of Teaching, by L. Cellérier; Primary Teaching, by X; and a bibliography for 1911.

Les maladies de l'énergie: les asthénies générales, épuisements—insuffisances—inhibitions—clinique—therapeutique. Par ALBERT DESCHAMPS. Paris, Alcan, 1909. pp. 496.

This work treats pathological defects of vigor; the first part is clinical and treats of the sources of energy, definition, etiology, symptomatology, thermic symptoms as distinct from chemical and chemico-physical, symptoms of sleep, varieties of aesthesia, pathogeny. The second part deals with therapeutics, general ideas, rest, isolation, aerotherapy, alimentation, blood pressure, remineralization, medication, etc.

Geschichte der Psychiatric. Von TH. KIRCHHOFF. *Allgemeine Therapie der Psychosen.* Von A. GROSS. Leipzig, Franz Deuticke, 1912. pp. 208. (Handbuch der Psychiatrie, hrsg. von G. Aschaffenburg. Allgemeiner Teil. 4. Abteilung.)

The historical part includes only 48 pages. In the second part the more interesting topics treated are prophylaxis and psychotherapy. In one subdivision of the latter subject its relations to religion and philosophy are treated, particularly determinism and responsibility. The author is a great believer in the therapy of occupation.

The influence of caffein on mental and motor efficiency. By H. L. Hollingworth. New York, The Science Press, 1912. pp. 166. Archives of Psychology, ed. by R. S. Woodworth. No. 22, April, 1912. Columbia Contributions to Phil. and Psychol., Vol. XX, No. 4.

Perhaps the best result of this interesting series of experiments is the complete absence of any trace of secondary depression or of sensory reaction consequent upon the stimulation, this result being of course quite in contrast with the secondary reactions believed to follow the use of such a drug as strychnine.

The history and status of psychology in the United States. By Christian A. Ruckmich. Reprinted from the American Journal of Psychology, October, 1912, Vol. XXIII, pp. 517-531.

Plant response as a means of physiological investigation. By Jagadis Chunder Bose. New York, Longmans, Green & Co., 1906. pp. 781.

Die Deizbewegungen der Pflanzen. Von E. G. Pringsheim. Berlin, Julius Springer, 1912. pp. 326.

Race Improvement. By La Reine Helen Baker. New York, Dodd, Mead & Co., 1912. pp. 137.

Die Projektionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder. Von Lillien J. Martin. Leipzig, J. A. Barth, 1912. pp. 231.

Berkeley. Versuch einer neuen Theorie der Gesichtswahrnehmung und Die Theorie der Gesichtswahrnehmung verteidigt und erläutert. Tr. by Raymund Schmidt. Ed. by Paul Barth. Leipzig, Verlag von Felix Meiner, 1912. pp. 152.

Über die Tonmalerei. Von Paul Mies. Sonderabdruck aus "Zeitschrift für Ästhetik und allgemeine Kunstwissenschaft," hrsg. von Max Dessoir, VII. Band, 4. Heft. Stuttgart, Ferdinand Enke. pp. 578-816.

Über die Tonmalerei. Von Paul Mies. Inaugural-Dissertation zur Erlangung der Doktorwürde, Bonn, 1912. Stuttgart, Druck der Union Deutsche Verlagsgesellschaft, 1912. pp. 55.

Über eine Unruheerscheinung: die Halluzination des Anrufes mit dem eigenen Namen (ohne und mit Beachtungswahn). Von Max Löwy. Separatabdruck aus den "Jahrbüchern für Psychiatrie und Neurologie," XXXIII. Band. Leipzig, Franz Deuticke, 1911. pp. 131.

Grundlagen einer organischen Weltanschauung. Von M. Krewer. Berlin, Leonhard Simion Nf., 1912. pp. 73.

New phrenology. By Shepherd Ivory Franz. Reprinted from Science, N. S. Vol. XXXV., No. 896, pp. 321-328, March 1, 1912.

Zur Kasuistik der Brunnenkrise (des Brunnendusels, des Brunnen und Baderausches). Von MAX LÖWY. Sonderabdruck aus der Zeitschrift für Balneologie, Klimatologie und Kurort-Hygiene, IV. Jahrgang, n. 12. Berlin SW 48, Allgemeine Medizinische Verlagsanstalt. 1911-1912. pp. 7.

The function of the vibrissae in the behavior of the white rat. By STELLA BURNHAM VINCENT. Behavior Monographs, v. I, no. 5, 1912. Cambridge, Mass., Henry Holt & Co. pp. 81.

Zur Kasuistik seltener "dyshumoraler" (innersekretorischer) Störungen. Von MAX LÖWY. Sonderabdruck aus der "Prager Mediz. Wochenschrift," XXXVI., Nr. 34-37, 1911. pp. 37.

Experimental oral euthenics: an attempt objectively to measure the relation between community mouth hygiene and the intellectual efficiency and educational progress of elementary school children. By J. E. WALLACE WALLIN. Reprinted from the Dental Cosmos, issues for April and May, 1912. pp. 32.

Eight months of psycho-clinical research at the New Jersey State Village for Epileptics, with some results from the Binet-Simon testing. By J. E. WALLACE WALLIN. Sonderabdruck aus der Zeitschrift, "Epilepsia," Vol. III. Leipzig, Johann Ambrosius Barth, pp. 366-380.

The present status of the Binet-Simon graded tests of intelligence. By J. E. WALLACE WALLIN. Reprint from the Alienist and Neurologist, Vol. 33, No.2, May, 1912, St. Louis. pp. 14.

Southern Medical Journal. (Journal of the Southern Medical Association.) December, 1912. Vol. V., No. 11. Southern Medical Journal, Suite 905 Van Antwerp Bldg., Mobile, Ala. pp. 732-806.

Psychologie und Erziehung: Ansprachen an Lehrer. Von WILLIAM JAMES. Aus dem Englischen von Dr. Friedrich Kiesow. 3d ed. Leipzig, Wilhelm Engelmann, 1912. pp. 134.

Die Situation auf dem psychologischen Arbeitsfeld. Von REINHOLD GEIJER. Berlin, Leonhard Simion Nf., 1912. pp. 90.

Archiv für systematische Philosophie, hrsg. von LUDWIG STEIN. Neue Folge der Philosophischen Monatshefte, XVIII. Band. Heft. 4. Nov. 15, 1912. Berlin, Leonhard Simion Nf., 1912. pp. 367-488.

Myxödem und Kretinismus. Von J. WAGNER V. JAUREGG. Lpz., Deuticke, 1912. pp. 91. (Handbuch der Psychiatrie, hrsg. von G. Aschaffenburg. Spezieller Teil, 2. Abteilung, 1. Hälfte.)

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THE RÔLE OF KINAESTHESIS IN THE PERCEPTION OF RHYTHM[1]

By Christian A. Ruckmich

CONTENTS

I. Introduction

The experimental investigation of the perception of rhythm has grown so extensive and, at the same time, so indefinite in scope that the writing of an introduction which shall be adequate to the general problem is now altogether out of the question.[2] The subject of rhythm has been carried over into many fields both inside and outside of the science of psychology: within, it has been related to attention, work, fatigue, temporal estimation, affection, and melody; without, it is fre-

[1] From the Psychological Laboratory of Cornell University.

[2] We expect to publish a complete bibliography on the subject of *rhythm* in the next number of this *Journal.*

quently mentioned in connection with music, literature, biology, geology, gymnastics, physiology, and pedagogy. If we follow out its progress within the range of psychological investigation, we find, again, an intricate plexus of results, theories, and issues. Emphasis has been laid on various component factors of consciousness involved in the perception of rhythm: changes of affective processes; effect of pitch, of duration, of intensity of stimulus on rhythmic perception; types of possible rhythmic perception; part played by different modalities of imagery; bases for rhythmic grouping and accentuation. An adequate summary of the work done even in this limited field would fill a fairly large monograph.

It is now, furthermore, no longer possible to undertake a set of experimental investigations, as was done twenty years ago,[3] to cover the entire range of the perception of rhythm. Attacks directed at the problem from various points of view, and with various methods of procedure, are the run of the day. No one, as far as the literature tells, has since that time attempted to make a complete study of rhythm.

A. Historical.—There seems to be one factor, however, which, when singled out and brought into perspective with other factors, has been assigned a prominent place in the discussion of rhythm. It would not be permissible to say that this factor has itself been the subject of discussion, for, curiously enough, it has often been taken for granted,—regarded, as it were, from an established point of view. Kinaesthesis of one sort or another, or motor expression consciously represented in the form of imagery or perceptual complex, is regarded by most investigators in this field as essential to rhythmic grouping and accentuation; it is considered, by a few, as non-basic but contributory and perhaps, indeed, as jointly essential; it is not mentioned at all by a few others. The point is, however, that the last class of investigators do not enter the lists polemically against those who emphasize the presence of the kinaesthetic component in the perception of rhythm. They simply advance a theory which turns out to be different.

Meumann comes to the conclusion that rhythms are perceived as the result of a mental activity conditioned either by the temporal quality of the perception or by the unitariness of meaning logically superimposed upon the impressions perceived. He says:

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[3] *Cf.* Bolton, T. L. Rhythm, this *Journal,* 6, 1894, 145-238.

In every case of rhythmical perception we group the isolated sensations of sound into a more or less complex system of ideas that are temporally arranged.[4]

For some observers the combining of the impressions is at the same time a temporal concatenation in the sense that the members of a group appear to follow one another more quickly, while a pause is experienced between every two groups.[5]

The other form of grouping is typified by the following quotations:

This process of grouping was recalled and described to me by my observers as an '*inner combination*'; the single beats were taken out of their incipient isolation and appeared as parts of a whole; the groups, on the other hand, were clearly separated one from another.[6]

The peculiarity of the whole experience of rhythm, it seems to me, is its *pure central initiation.* Either the reproduction of familiar meters, or better, perhaps, the *unequal distribution of the energy of attention* must be considered in this connection.[7]

But the course of 'thoughts' becomes itself the condition of rhythmisation. The character of the entire rhythm is preëminently determined by this delimitation of meaning. The groups which appear collectively to the ear (and, in this sense, the rhythmical units) are constituted as a kind of rhythmical treatment, which is peculiarly characteristic of Goethe's lyric verse.[8]

Woodrow, in closing the summary of his experimental work, also emphasises the temporal aspect of rhythmical grouping:

This close correspondence between the rhythmical grouping and the temporal grouping, or rather this correspondence in the points where both disappear, indicates that rhythmical grouping is a temporal grouping; that is, that rhythmical grouping is determined by the duration of the subjective intervals, not by the objectively measurable intervals, but by the subject's consciousness of these intervals, that is, by the intervals considered as mental magnitudes.[9]

Continuing to consider these classes of authorities in an inverse order, we find a group who conclude that kinaesthesis is a factor which is jointly essential to the perception of rhythm. Wundt, for example, gives a genetic explanation for rhythm in terms of the movements of locomotion, and then makes the essential characteristic of rhythmical perceptions dependent upon the changes in the course of affection, with a motor tendency in the background of consciousness:

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[4] Meumann, E. Untersuchungen zur Psychologie u. Aesthetik d. Rhythmus, *Philos. Stud.,* 10, 1894, 283.

[5] *Ibid.,* 304.

[6] *Ibid.,* 303.

[7] *Ibid.,* 304.

[8] *Ibid.,* 396.

[9] Woodrow, H. A quantitative study of rhythm, New York, 1909, 66.

So we have every reason to consider the movements of locomotion as the natural origin of rhythmical perceptions.[10]

Consciousness is rhythmically disposed, because the whole organism is rhythmically disposed. The movements of the heart, of breathing, of walking, take place rhythmically. . . . Above all, the movements of walking form a very clear and recognisable background to our consciousness.[11]

Pleasure and displeasure constitute, as we saw above, subordinate forms of rhythmic affections which, because they parallel in a relative equivalence the peculiar contents of pleasure and displeasure, we can designate as *formal* components of the aesthetic effect of feelings. In contrast to this, the characteristic *contents* of rhythmical effect results from the *specific form of rhythmical movement* which is, in turn, capable of very numerous gradations and in which varying mixtures of different components of affection and changes between qualitatively differing and, indeed, opposed affections, are possible. Because, at the same time, this contents always partakes of a certain affective character, we can contrast with it that formal or abstract affective quality of pleasure and displeasure under the concept of the *affective* components of rhythm.[12]

In essentials Stumpf does not disagree with Wundt in this notion of the factors which are important in rhythm:

Judgments of time and intensity are further connected in the comprehension of rhythm; this we shall best discuss, however, in connection with the theory of affection.[13]

It looks, indeed, as if our sense of rhythm and time was essentially developed in connection with the movements of locomotion.[14]

Ebhardt assigns to a number of factors the responsibility of giving rise to rhythmical phenomena, but lays most stress on the importance of affective tone:

The following assumptions were made as hypothetical explanations of these phenomena: peculiarities of motor action, change of direction of attention, and the combination of members into groups or the separation of groups in the process of comprehension and in the process of execution corresponding to it.[15]

This affective tone, it appears to me, forms an essential component of rhythm; it must be present if there is not to be a failure to comprehend the rhythm: the player would miss something if he could not successfully arouse and maintain an affective contents.[16]

But by far the greater number of investigators and systematic writers on the subject of rhythm emphasise the primary

[10] Wundt, W. Grundzüge d. physiologischen Psychologie, 6th ed., 1911, vol. III, 32.

[11] Wundt, W. Introduction to psychology (trans. Pintner), New York, 1912, 5.

[12] Grundzüge, vol. III, 147.

[13] Stumpf, C. Tonpsychologie, Leipzig, 1883, vol. I, 135.

[14] *Ibid.*, 340.

[15] Ebhardt, K. Zwei Beiträge zur Psychologie d. Rhythmus u. d. Tempo, *Zeits. f. Psychol.*, 18, 1898, 127.

[16] *Ibid.*, 140.

importance of kinaesthesis and of motor response in rhythmical perceptions. Whether this motor response is consciously represented, a few of these writers do not specify, but the majority use the phrase as an equivalent for kinaesthesis. Taken up in historical order, we find Bolton, the pioneer American investigator, making the statement:

> We come now to the subject of muscular movements and their relation to rhythm. Most subjects felt themselves impelled by an irresistible force to make muscular movements of some sort accompanying the rhythms. If they attempted to restrain these movements in one muscle, they were likely to appear somewhere else. . . . The question we have to decide is, are these muscular movements and associations the result or the conditions of the rhythmical grouping? With Ribot we accept without hesitation the latter. . . . Each impression as it enters into consciousness tends to find expression in a muscular movement, but the intensive changes in the series of impressions produce corresponding changes in the intensity of the sensations, which must find expression in different degrees of muscular activity. In order to express these different degrees of sensation, the muscular movements must rise above the merely nascent state in which they ordinarily occur, and manifest themselves in visible muscular movements.[17]

Ettlinger, while working and theorising more about the aesthetical aspect of rhythm, says:

> The descending grouping of two elements [trochee] represents, therefore, the transition from a period of rest to a period of advance, the initiation of a movement. If such a group stands at the beginning of a series, then it indicates with especial clearness the origin of a movement. Outside of this, inasmuch as there is no return to a period of rest to be found in it, it points on this account to a perpetuation of the movement. When, therefore, it appears at the end of a movement, it does not bring that movement definitely to completion, but allows it to be dissipated in empty space.[18]

MacDougall accounts for the temporal element in rhythmical perception on the basis of organic activity and of kinaesthesis:

> The fundamental conditions of the rhythm experience are therefore to be looked for in the laws of periodicity of functioning in the bodily organism. It is because these processes take place under conditions of regularly recurrent change that the time element becomes important in rhythmical sequences.[19]

Elsewhere, in much the same manner, he describes the effect of mechanical conditions imposed on the expression of rhythm by processes of muscular contraction and release.[20]

[17] *Op. cit.*, 234.
[18] Ettlinger, M. Zur Grundlegung einer Aesthetik d. Rhythmus, *Zeits. f. Psychol.*, 22, 1900, 186-7.
[19] MacDougall, R. The relation of auditory rhythm to nervous discharge, *Psychol. Rev.*, 9, 1902, 465.
[20] The structure of simple rhythm forms, *Monog. Suppl., Psychol. Rev.*, 4, 1903, 327.

Speaking of kinaesthesis, Miner, who has made a special study of motor rhythms, concludes:

I believe it is time to recognise that it is this element in the conscious experience which adequately affords the explanation of the main fact of rhythm,—the group feeling. To be sure we do not stop here. There are many other attributes of the rhythmic impression to be accounted for. . . . The advantage of the kinaesthetic thesis which is here offered is that it gives a satisfactory mode of approach for these other problems.[21]

Stetson, under the heading, *Suggestions for a motor theory of rhythm,* opens a discussion of the motor theory as applied to rhythmical perceptions with the words:

If the basis of rhythm is to be found in muscular sensations, rather than in the supposed activity of some special 'mental' function, the nature of the movement cycle involved is of greatest interest.[22]

And he concludes:

Assuming such a movement cycle, in which the tension between the two opposing sets never comes to zero until the close of the series, it is not difficult to arrange many of the facts of rhythmic perception under the motor theory.[22]

One of the six conclusions, drawn by Koffka as the result of experimental investigations, reads:

Motor imagery has, however, the greatest significance for rhythmical experiences. It appears almost universally, and is, for the most part, very difficult to suppress.[23]

Agreeing, in the main, with Stetson, Bingham assumes a motor theory for both rhythm and melody:

The experimental study of rhythm has, however, disclosed a motor phenomenon essentially like the large, basic motor activity underlying a melodic unity.[24]

Krueger can also be classed in this group:

The affectively toned auditory experiences, especially the rhythmical, never appear without the characteristic sensory components of motor origin.[25]

There are several systematic writers who have given the kin-

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[21] Miner, J. B. Motor, visual, and applied rhythms, *Monog. Suppl., Psychol. Rev.,* 5, 1903, 17.

[22] Stetson, R. H. Rhythm and rhyme, *Monog. Suppl., Psychol. Rev.,* 4, 1903, 453ff.

[23] Koffka, K. Experimental-Untersuchungen zur Lehre vom Rhythmus, *Zeits. f. Psychol.,* 52, 1909, 104.

[24] Bingham, W. V. D. Studies in melody, *Monog. Suppl., Psychol. Rev.,* 12, 1910, 83.

[25] Krueger, F. Mitbewegungen beim Singen, Sprechen, u. Hören, Leipzig, 1910, 22.

aesthetic theory place in textbooks of psychology. Among these Titchener may be mentioned:

The author was formerly disposed to attribute a separate rhythmical perception to hearing, but recent observation has convinced him of the existence of kinaesthetic sensations due to the contraction of the *tensor tympani* of the middle ear. . . . In the author's opinion, this [visual] rhythm is always kinaesthetic, based upon eye-movement, upon slight movements which tick off the successive impressions, or upon some other form of intermittent kinaesthesis.[26]

Wallin, one of the most recent investigators in this field, says:

Practically all the subjects made use of kinaesthetic factors—movements of the body, or tongue, or head, or finger.[27]

And Swindle, who gives us the latest study, remarks:

In the development of a rhythm, the motor activity of the skeletal muscles plays the most important rôle.[28]

There is evidence enough, then, that most of the investigators in the field of rhythm conclude that kinaesthesis of one sort or another plays the most prominent part in rhythmical perception and in its development. This is the factual side of the case: with possibly only a few exceptions, all the statements made are the result of observations under controlled conditions.

B. Theoretical.—A large literature has recently made its appearance in connection with discussions concerning the fundamental principles of psychology. With it has come the slogan of 'the science of human behavior.' This plea for the study of behavior brings with it, on the theoretical side, a heavy stress on the motor responses of the organism. Statements like the following, selected at random from the works of expositors of this type of psychology, illustrate the tendency toward a renewed emphasis on kinaesthesis:

The science is being developed on the one hand by zoölogists and on the other hand by comparative psychologists. These scientists are studying those visible movements of the animal organism which constitute the external physiological processes.[29]

We may express the relation which actually obtains between them by saying that physiology investigates the processes of the parts or organs of which any organism is composed, while psychology investi-

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[26] Titchener, E. B. A text-book of psychology, New York, 1910, 345.

[27] Wallin, J. E. W. Experimental studies of rhythm and time, *Psychol. Rev.*, 19, 1912, 295.

[28] Swindle, P. F. On the inheritance of rhythm, this *Journal*, 24, 1913, 202.

[29] Parmelee, M. The science of human behavior, New York, 1913, 1.

gates the activities of the organism as a whole, that is those in which it operates as a whole or unit.[30]

If psychology is to be defined as the science of human behavior, the term 'behavior' must be used in the widest sense. It must include everything from the simplest movements of walking or of fingering the pen to the activities involved in swaying an audience by speech or in carrying to completion some great engineering work.[31]

A complete explanation of any phase of consciousness can be neither in terms of sensation nor in terms of movement exclusively, but must include both.[32]

The scheme of habit which James long ago described—where each return or afferent current releases the next appropriate motor discharge—is as true for 'thought-processes' as for overt muscular acts.[33]

The psychology which I should attempt to build up would take as a starting point, first, the observable fact that organisms, man and animal alike, do adjust themselves to their environment by means of hereditary and habit equipments. These adjustments may be very adequate or they may be so inadequate that the organism barely maintains its existence; secondly that certain stimuli lead the organisms to make the responses.[34]

In theory at least, if not in substance, all of these references hark back to the well-known and oft-quoted passage from Ribot:

It is always necessary to recall that fundamental principle: every mental state is accompanied by manifestations which are physically determined. Thought is not—although many, influenced by tradition, would admit it to be—an event that has passed into a suprasensible, ethereal and incomprehensible world. We repeat with Setchenoff, 'no thought without expression,' *i.e.*, thought is a word or an act in a nascent state, that is to say, the beginning of a muscular activity.[35]

But already has this movement to emphasise the motor response as an interpretable datum of psychology found antagonists. Thorndike assails the 'ideo-motor' theory in part with these words:

The theory of ideo-motor action has been for a generation one of the stock 'laws' of orthodox psychology. It is taught as almost axiomatic in standard treatises—is made the explanatory principle for phenomena of suggestion, hypnotism, obsessions and the like—and is used as the basis for recommended practices in education, psychiatry, religion—even in salesmanship and advertising. . . .

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[30] McDougall, W. Psychology, the study of behavior, New York and London, 1912, 35.

[31] Pillsbury, W. B. The essentials of psychology, New York, 1911, 2-3.

[32] Pillsbury, W. B. The place of movement in consciousness, *Psychol. Rev.*, 18, 1911, 99.

[33] Watson, J. B. Psychology and behavior, *Psychol. Rev.*, 20, 1913, 174 (note).

[34] *Ibid.*, 167.

[35] Ribot, Psychologie de l'attention, Paris, 1889, 20.

The connection whereby likeness or representative quality, in and of itself, created a bond between a thought and an act, would be 'mysterious' if it existed. But it does not exist.[36]

If the motor response is to be taken *ipso facto* as a criterion to which we are to refer the contents of consciousness, with the additional strong subsumption that it is the only criterion, then it appears that the elaborative functions of the cortex are summarily ruled out of court.[37] That under varying conditions we consciously ascribe divers meanings and references to our kinaesthetic complexes, and that kinaesthesis, aside from other contents of consciousness, is only a part of the material which the cortex elaborates, is apparently overlooked. As to the direct interpretation of 'movement-curves' as graphic or even symbolic representations of conscious processes, it seems only necessary to indicate that there is a manifest confusion of the categories of at least two sciences, psychology and physics. Even casual introspection of our everyday experiences shows that an extensive or an intensive motor response may count for very little in consciousness at one time, while an imperceptible movement or a kinaesthetic image may mean much at another. Psychological meaning is not a matter of extent or intensity of movement, but of extent of elaboration.[38] The spatial and temporal attributes of the physical universe cannot be taken over bodily into the psychological.

A psychophysical interpretation of the rhythmical consciousness can also be criticised from approximately the same angle. The assumption of an equivalence between the psychical and the physical is an error into which the investigator is apt to fall if he insists upon too rigid an interpretation of psychophysical data, or if he handles those data as if they exhausted the contents of consciousness under examination. Quantitative psychology must be made coördinate with qualita-

[36] Thorndike, E. L. Ideo-motor action, *Psychol. Rev.*, 20, 1913, 91-106.

[37] *V.* Titchener, E. B. Psychology of feeling and attention, New York, 1908, 309; also Bentley, M., this *Journal*, 17, 1906, 293f.

[38] Ebbinghaus, H. Grundzüge d. Psychologie, Leipzig, 1908, II § 70.
Höffding, H. Outlines of psychology, Leipzig, 1893, 114.
James W. Principles of psychology, New York, 1890, II, 3.
Ladd, G. T. Psychology, descriptive and explanatory, New York, 1894, 661.
Thorndike, E. L. Elements of psychology, New York, 1905, 65.
Titchener, E. B. A text-book of psychology, New York, 1910, 367.

tive psychology; neither is sufficient without the other.[39] So, while we cordially welcome psychophysical experimentation in this special field when its results are duly qualified in the manner indicated, we do not think that Woodrow's characterisation of the method of procedure and of the problem is at all comprehensive:

> The typical procedure in the investigation of these rhythmical factors has been to present a series of sounds or other stimuli, some of which are longer, more intense, or of different quality than the others, and to ask the observer to describe his impression of the series, especially as regards the way in which the stimuli appear to be grouped. Such a procedure is characteristically a psychophysical one, just as much as when two weights are presented to an observer for him to lift, and then to describe his impression of them with special reference to their relative heaviness. In both cases the investigation is one of the relationship between certain conditions existing in the environment and the nature of the subject's consciousness under the existing conditions.[40]

All this is but part of the problem. Not only must we investigate the "relationship between certain conditions . . . . . and the nature of the subject's consciousness," but, in order to establish the relationship between conditions that are known, *i. e.,* the physical conditions of the experiment, and the "nature of the subject's consciousness" which is unknown, we must investigate the nature of that consciousness. The history of the reaction experiment tells the same story: after decades of psychophysics, it was left for Ach to publish in 1905 his book *Ueber die Willenstätigkeit und das Denken,* in which for the first time a systematic psychology of volition and action, based upon introspective data from the reaction experiment, is presented.[41]

Although several investigators have contributed introspective material in the field of rhythm, there is still a dearth of analytical observations systematically controlled and obtained from trained and practised subjects. So the statement which Meumann made in 1894 still holds, in a measure, to-day:

> What we still lack, more than anything else, is a comprehensive description of the psychical phenomena of the inner life, which we call rhythmical, and a reference of these phenomena to the action of general psychical factors, as well as the investigation of the conditions which make rhythm possible.[42]

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[39] *Cf.* Titchener, E. B. Experimental psychology, New York, 1905, vol. II, pt. I, xxxviii.

[40] Woodrow, H. The rôle of pitch in rhythm, *Psychol. Rev.,* 18, 1911, 54.

[41] *V.* Titchener, E. B. This *Journal,* 21, 1910, 416.

[42] *Op. cit.,* 273.

II. Experimental Procedure.

It was partly in view of the fact that introspective material of an analytical sort is comparatively scarce, partly because we believed that in previous investigations not enough emphasis had been laid on that aspect of kinaesthesis which gave it *meaning* or *reference,* and partly on account of theoretical considerations in regard to the importance of contributing factual data which might help to strengthen or weaken current views centering about the motor responses of organisms, that this investigation was undertaken. The nature of the rhythmical consciousness had not been sufficiently analysed from the introspective side to warrant generalisation about its contents.

A. Problem.—The task before us was to produce by physical means a series of impressions which would, under ordinary conditions, group themselves together in a rhythmical relation; to secure as faithful a description of consciousness, under these conditions, as practice and training on the side of the observers would permit; and to use as effective means of control and check on conscious contents as methodological approach and systematic observation on the side of the experimenter would allow. Further analysed, our problem involved a variation of conditions in order to present rhythms of different kinds under objective control, and a variation of instructions for the purpose of bringing different types of consciousnesses under subjective control. Some of these instructions focused attention on the kinaesthetic complexes present, but these instructions were given only after there was definite evidence of actual kinaesthesis.

B. Observers.—The observers in this experiment were either members of the Department of Psychology or graduate students of one or more years' standing. They were: Dr. Geissler (Ge), instructor, Mr. Foster (F), research assistant, Mr. Boring (B), assistant in the department; Miss Burr (Bu), Miss Day (D), Mr. Edwards (E), and Miss Goudge (G), all graduate students in psychology. In a few observations Dr. Bentley (Be), assistant professor, and the writer (R), instructor in the department, also took part. None of the observers reported in all of the experiments; D and G served, however, in about three-fourths of the total number. Arranged in descending order, according to the amount of introspective training and practice, the observers may be classed as follows: Be; Ge; F; D; B, E, G, and R; Bu.

In order to ascertain the degree of musical training which the observers had, as well as to determine the approximate nature of their experiences in every-day life when they were listening to musical renditions, those observers who took part in the more important experiments were requested to answer the following confidential questionary:

1. Have you had any courses in any branch of music in high school or in college? If so, outline them.
2. Have you had any training in singing outside of school and college courses? If so, to what extent?
3. Do you play any musical instrument? If so, with what degree of proficiency? For how many years have you played?
4. Can you tell the approximate absolute pitch of any given note?
5. Can you analyse any given chord into its constituent parts? Can you name the given chord?
6. Do you consider yourself possessed of a good 'musical ear'?
7. To what extent, concisely stated, do you appreciate music?
8. Psychologically considered, how does music generally affect you —what phenomena does it arouse?
9. Do you carry melodies auditorily? If not, how do you carry them?
10. To what extent do melodies play a part in your every-day consciousness?

(After you have answered the above, uncover and answer question No. 11 below this slip.)

11. UNDER THIS INSTRUCTION RECALL A CONCERT OR RECITAL WHICH YOU HAVE RECENTLY ATTENDED: What have you now in consciousness?

Although most of the observers stated that they would be willing to have the answers to these questions published, the writer considered it fairer to treat the information obtained as confidential. While, therefore, the records of the questionary are available in the protocol, we must rest content with generalisations based upon the answers received without making their application personal. Almost all of the observers had had practice in playing some musical instrument with some degree of proficiency; over one-half of the number had received instruction in piano-playing, organ-playing, violin-playing, or singing, covering periods from about one year to five years or more in duration. A few had learned the rules of writing harmony and melody; one observer possessed the sense of absolute pitch. Almost all could analyse a given chord under attention; but very few could name its constituent parts. All appreciated good music in a fairly high degree; the writer believes that there is sufficient evidence for the statement that in all cases the aesthetic judgment was well developed in the field of music, and that there was real musical enjoyment. Most of the observers hum, whistle, or sing melodies to themselves, ordinarily, although they may at times imagine melodies in terms of auditory-kinaesthetic complexes. For some of them, organic and visceral sensations play a large part in the appreciation and recall of musical experiences. In these cases the rhythm may be conveyed "in indefinite kinaesthesis." Another found "muscular sensations in body, neck, and head which come from an involuntary attempt to keep time and to use bodily movement to help me do so." Under the instruction to recall a concert or recital, and to report upon the consciousness thus aroused, almost all of the observers got associative visual, kinaesthetic, auditory, and auditory-kinaesthetic imagery. "Then auditory-verbal images, 'Evening Star,' and auditory imagery of certain parts of the selection, *especially* of the loud clarinet tones, strain sensations in throat, and kinaesthetic imagery in arms (playing piano) with vague auditory images of piano notes accompanying clarinet. Visual images of clarinet player and his chair, localised in part of the room." "Auditory images and kinaesthetic images in throat (of the notes) and kinaesthetic sensations in right foot and ankle (the latter meaning 'keeping time,' and also 'an aid to recall')." "Also vague auditory imagery of high-pitched notes for the 'swings.' Then verbal idea: 'and symphony—Beethoven.' Vague visual imagery of Boston Symphony Hall." "The present memory of this song is a visual image of Bispham on the stage, open mouth very clear, with two very clear successive throat-feels, meaning a high and a low note respectively. An auditory component accompanies the upper note but the lower one is entirely kinaesthetic." "My memory consists of vague visual images of the players and strong kinaesthetic images of bodily position."

The value of these answers as introspective material for our experiment is, of course, very doubtful. The material was, for one thing, not obtained under experimental conditions of control; for another, it has all the shortcomings of a questionary method of attack. It did, however, orientate the writer in regard to his observers, and gave him an estimation of their value in a series of experiments of this nature.

At one time in the series, when the investigation concerned itself with rhythms whose constituent members differed physically in pitch, it became advisable to test the observers who took part in that particular problem in regard to their ability to discriminate pitch. The *DL* was accordingly determined for D, E, and G, by the *method of limits*. Two Spindler and Hoyer forks pitched at a^1 (435 vd.), with attached automatic hammers, were used,[43] and 40 series, in addition to the usual number of preliminary series, were taken. The results gave: *DL* for D = .4 vd., for E = 1.82 vd., and for G = 1.86 vd. These results average below the mode obtained by Mount and Smith in investigations with 781 undergraduate men and women.[44] Their results taken from crude tests with steps of one or more vd. show the greatest percentage of frequency at 2 vd. (21 + %) for a' of 435 vd. Our steps were uniformly equal to .65 vd. In comparison with what has been found in psychophysical experiments of this sort, our results are a trifle high, indicating relatively poor pitch discrimination (with the possible exception of D). Luft, by the same method used on himself, found a *j.n.d.* of .251 vd. at c'' and one of .232 vd. at c', the octave which includes our own determinations.[45] Titchener says: "The value of the *DL* (absolute) as obtained under the described conditions has never exceeded 2 vs. for either set of forks, and has fallen as low as .75 v."[46] While, therefore, in terms of Luft's results our observers were on the whole poorer in pitch discrimina-

[43] These are listed in catalog no. 21 as no. 130b, but riders and automatic hammers are not listed as attached.

[44] *V.* Seashore, C. E. The measurement of pitch discrimination, *Psychol. Rev., Monog. Suppl.,* 13, 1910, 43.

[45] Luft, E. *Philos. Stud.,* 4, 1888, 511.

[46] Titchener, E. B. Experimental psychology, New York, 1905, vol. II, pt. II, 126.

tion, nevertheless, in terms of Titchener's statement, one of our observers, D, was below the .75 v. mentioned, *i.e.,* better, and the other two were not unfavorably comparable with the worst.

C. Apparatus.—It is almost inevitable, at the present stage of investigation, that the arrangement of apparatus for the adequate control of experiments in acoustics, and especially in the field of auditory rhythm, should be complicated. Parts of the apparatus must be constantly modified to suit new conditions, other parts must be invented for the same purpose. In the early preliminary experiments an ordinary Maëlzel metronome was used. It was, however, carefully selected with a view to eliminate any qualitative differences in the beats. The settings used gave: 42, 48, 66, 92, 152, 176, and 200 beats to the minute.

The metronome was placed on a heavy piece of harness-felt and covered over on all sides, except on the side toward the observer, with an inner lining of cotton batting and an outer layer of harness-felt, held in place by a caging built up of wire supports. This cage measured in all about 70 cm. in diameter and 40 cm. high. It prevented the possibility of echoes from the walls of the room, while it at the same time permitted the sound of the metronome to be heard clearly by the observer, who sat with his back toward the opening of the cage, 2 m. away. This arrangement, of course, also eliminated to a great extent the clang elements present in the ordinary metallic click. Rubber pads on the chairs in the room and felt pads on the feet of the table used for the metronome made occasional movements of these pieces of furniture practically noiseless. A contrivance for starting and stopping the metronome when the experimenter was at a distance away from the instrument was used. This consisted essentially of a wire bar padded with felt and fastened on one end directly to the side of the cage at its front opening and, on the other end, by means of a rubber band to the opposite side of the cage. At the latter end a piece of string was attached which the experimenter held in his hand. The pendulum of the metronome was then placed at one of its extreme positions against this horizontal bar. When the string was pulled, the pendulum was released; when the string was released, against the pull of the elastic rubber band, the bar sprang back in place and caught the pendulum. With a little practice the experimenter was able to catch the pendulum at one of its extreme positions, so that the next pull of the string would find the pendulum ready to begin an excursion. A contrivance of this sort was found necessary because of the advisability of having the experimenter near the observer, while the latter was observing the rhythm produced, in order to detect, if possible, any movement of exposed parts of the body, or any change in breathing. The crudity of such a method of experimental control of the observer is realised and acknowledged, but the series was of so preliminary a nature that it was not considered necessary to take detailed and accurate means of registration until the problem had been clearly outlined.

In the second part of the preliminary series, where an objective rhythm was produced in terms of differences of intensity of sounds, a Titchener rhythm-box was used in connection with the metronome.[47] But, like the latter instrument, this rhythm-box had also to be modified to meet our requirements.

It became necessary to begin a rhythmical series on an unaccented beat, *i.e.*, with an unintensified member of the group; it was also imperative that a rhythm be started and stopped at will. For this reason a releasing and arresting device was attached to the box. The principle of this was practically the same as that used in connection with the metronome in the first part of the preliminary series. Its operation was entirely noiseless. A full account and an illustration of the attachment has already been published.[48]

The observer sat in the same position in this second series of the preliminary set as in the first, and in all other respects the physical conditions were the same.

In the main series of experiments a complete change of apparatus and of the general experimental conditions was necessary. It was our intention to work as far as possible with pure tones combined to produce a two-membered group. These tones were to be variable within certain limits in pitch, intensity, and duration. Two adjacent rooms in the center of the upper floor of the laboratory, and therefore comparatively free from disturbing noises, were used for the rest of the experimental series. One of these rooms, an inside dark-room, in which the observer sat, was separated from the other, in which the experimenter and the greater part of the apparatus were placed, by a heavy stone wall 50 cm. thick. Since the variability of the tones produced, in regard to pitch, intensity, and duration, was the factor that separated the entire main series of experiments into smaller divisions, it will be well to designate these divisions in some arbitrary manner. If we call the two large divisions of the preliminary series, the one concerned with subject rhythmisation, the other with objective rhythmisation, respectively *A1* and *A2*, then we may call the divisions in the main series, *viz.*, one concerned with objective differences of duration, another with differences of intensity, and the third with differences of pitch, respectively *B1, B2,* and *B3*.

In the experiments *B1*, two tones were produced, whose relative duration was variable. In all cases the intervals between the tones were constant: one short interval, and one long interval of approximately twice its length, followed one another in alternation, *e. g.*, tone (variable duration)—short interval—

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[47] Listed in Stoelting's catalog, Dec., 1909, as no. 7414.

[48] Bentley, M., Boring, E. G., and Ruckmich, C. A. New apparatus for acoustical experiments, this *Journal*, 23, 1912, 513.

tone (variable duration)—long interval—cycle repeated, *etc.* The intervals were kept uniformly constant in duration throughout all of the series *B1, B2,* and *B3*. It must also be understood that whenever mention is made of the variability of one of the factors of the tones, *i. e.,* pitch, intensity, or duration, the other factors, not spoken of as variable, are constant. A tone, for instance, which is variable in duration, is not variable in pitch or intensity.

For meeting the conditions of the *B1*-set of experiments, a fork of Koenig manufacture and belonging to the Helmholtz set[49] for synthetically reproducing vowel sounds, was enclosed in a sound-proof box. The fork of 256 vd. was used with its cylindrical resonator because it produced the clearest and best-carrying tone of the set. Connected electrically in series with it, but, of course, outside of the sound-proof box, the exciting-fork, from the same set, was utilised for the purpose of keeping the c'-fork in vibration. Opposite the opening of the resonator of the c'-fork, a telephone transmitter of especial construction was placed.

The transmitters and receiver used in these experiments were made with a view to the most faithful, but not the most intense, reproduction of sound and had been chosen at the recommendation of a member of the Department of Physics. The type of transmitter was no. 227W and that of the receiver was no. 128W, both made by the Western Electric Co., of New York City. That the sound produced by this telephonic circuit was normally tonal in character is shown by the following quotations selected from the reports of the observers when the apparatus was working under standard conditions: (D) "Noticed tonal difference and began saying 're, ti,' 're, ti,' *etc.*" At another place D refers to the sounds as being 'musical' and 'bell-like.' (E) "Auditory sensations from first stimulus, perceived as tone; but I am unable to perceive the rhythm." "Tones seemed to hold the center of consciousness this time." "Tones were immediately reproduced auditorily—it seemed as though I could hear my own voice singing them." G remarks: "Now feeling of doubt became clearer especially in the period between the groups of tones—at other times general trunk kinaesthesis would be clearer—always clearer when tone was pleasant." "At these times, auditory sensations of tone, a kinaesthetic swing of the trunk with each auditory sensation." B: "Auditory perception of first tone, accompanied by kinaesthetic strains in chest." "Then auditory perception of second tone interrupted it. I do not know what happened to kinaesthesis then (as I think I was surprised at quickness with which second tone appeared)." F: "Attention went from the tones to visual image of paper on which I was to write." "Now attention goes back to louder, lower, tone (*i.e.,* after judging) and it immediately became first tone of a foot, I had lost one tone." In addition to these statements, it may be said that the observers generally had no

[49] For a description of this apparatus *v.* Helmholtz, H. The sensations of tone, (trans. Ellis), 2nd ed., London, 1885, 119-23.

trouble in assigning a pitch to the sounds produced. That the tones produced were wholly free from clang-quality can not be asserted. When the entire apparatus was working at its best, however, a tone with only a few clang elements resulted.

The telephonic circuit which embraced the transmitter passed out of the sound-proof box through a suitable induction-coil and storage battery. From the secondary of the induction-coil, it was led to an interrupting apparatus of special design.

Various types of interrupting devices were tried out only to be discarded. The Meumann time-sense disc, geared to the Ludwig-Baltzar kymograph, was tried in connection with small interrupting magnets for making and breaking circuits relayed from the time-sense disc, but difficulties arose in regard to the improper response of the magnets owing to hysteresis. Later an Edison phonograph with an electric motor whose speed was kept constant by means of a ball-governor was substituted.[50] The governor automatically shunted the current through a resistance-coil when the speed tended to increase. On the cylinder of the phonograph a wax-record was mounted. The record was covered in the following manner: over a little less than one-half of its circumference and extending the whole length, a strip of smooth white paper was pasted; the remainder of the surface was covered with two strips of triangular shaped tin-foil, separated by an oblique section of white paper whose width was equal to one-half of the wider strip of white paper. The triangles were equal right-angle triangles and their hypotenuses were adjacent to the oblique strip of white paper; they were inverted and apposed so that the point formed by the union of the longer arm and hypotenuse of one was in circumferential line with the shorter arm of the other, and *vice versa*. In place of the ordinary style and diaphragm, a contact-arm ending in two platinum-wire fingers was substituted. These wires were connected through switches to the secondary circuit of the telephonic system, so that whenever the two fingers passed over a section of the tin-foil the circuit was completed. As the record was rotated, then, these fingers would pass, for instance, at the extreme left of the record, over the circumferential width of a strip of tin-foil nearest the tip of the first triangle, making a very short contact, then over the width of the oblique section of white paper, making no contact and, therefore, the interval between the two members of the group, then over the width of the second strip of tin-foil, this time nearest the shorter arm, and, therefore, comparatively a long contact, and finally over the widest strip of paper, giving the long interval between the groups: in all producing an iambic rhythm. By shifting the arm along the length of the record, gradual changes, from iambic at the left, through spondee in the center, to trochaic at the right, could be effected. The amount of these changes was read off from a millimeter scale on the record. It is understood, of course, that the intervals, both within and without the group, remained constant. The times of these component factors of the rhythm-group were: first member, variable from 0″ to .6″, the interval between members constant at .5″, the second member, variable from .6″ to 0″, the long interval

[50] A sketch of this type of phonograph can be found in the Standard Dictionary, edition of 1908, under 'phonograph,' pg. 1329.

between groups, constant at .9″; in all, the time for a complete cycle was 2.0″.

This method of interrupting the circuit had to give way to another, because the contacts between the platinum points and the tin-foil were not of the best. Almost invariably a scraping noise could be heard in the receiver. To eliminate this, a rotating cam-device for interrupting the circuit was resorted to. The phonograph-motor was still used, but its cylinder-drum was used as a driving pulley with a belt-gripping attachment. The belt passed over the pulley of the rotating cam-device and allowed triggers to drop alternately into small mercury cups. A complete description of the mechanism and use of this apparatus has already been published.[51] It has since been slightly modified with respect to the operation of the triggers. They are now made to drop more quickly and more deeply into the mercury cups, which are placed side by side, in that their power-arms are comparatively longer than before; since they are now made of heavy piano-wire, and the contact points are therefore sharper and thinner, better contact is made. With this device the time values as shown on kymographic records, a 50 vd. fork writing the time-line, were as follows: first member, variable from 0″ to .44″, the interval between members approximately constant at .45″, the second member, variable from .5″ to 0″, and the interval between groups approximately constant at 1.0″; in all, the time for a complete cycle was approximately 1.8″. Kymographic records were also taken in the same way for the purpose of controlling the speed of the electric motor at various points on the scale of the sliding rheostat. The speed of the motor was regulated to keep the time of the cycle of the rhythm between 1.8″ and 2.0″ throughout all of the series.

The secondary circuit of the telephonic connection passed from this interrupting device, through suitable switches and through a tube in the wall, to the dark-room where it ended in a receiver of the type mentioned. A head-band was attached to the receiver. The observer wore this receiver continuously throughout a single experiment over the better ear, if there was any choice. It fitted very comfortably and its presence was usually hardly noticed.

The system of signals was arranged so that a light-flash from a 4 c.p. incandescent electric light prepared the observer for the rhythm.

It was agreed that the light should remain on during the period immediately preceding the experiment. When the experimenter was ready to begin, this light was turned off. When the observer was ready, he pressed a button which rang a muffled bell in the experimenter's room. After three seconds from this signal, the switches which started the rhythm were thrown. A double-arm, double-throw switch connected at a single movement both primary and secondary circuits. This was done to insure against leakage from one circuit to the other. It was necessary to keep the wires which carried the alternating current for the signal-light well out of the way of the telephonic circuit, because it was found that the receiver circuit would detect with the greatest delicacy any electric variations within

[51] Bentley, M., Boring, E. G., and Ruckmich, C. A. *op. cit.*, 511-13.

a meter if the systems ran parallel for more than about 30 cm. The observer's eyes were protected from the light by a screen.

The experimenter's room was connected with the observer's room by a speaking tube through which special instructions were given and introspections reported. A stop-cock disconnected this tube while the rhythm was being produced, in fact, through the whole period of observation.

For the *B2*-set of experiments, where the members of the rhythm-group were changed in regard to intensity, only slight alterations in the apparatus were necessary. A second fork, manufactured by Max Kohl, of the same vibration-rate and of the same tonal quality as the first was enclosed in another sound-proof box. Opposite to its resonance-box a transmitter, of the same type as the first, was placed. The telephonic system mentioned above was duplicated so far as its primary circuit was concerned. In one of the primary circuits, however, a 111 ohm resistance-box was inserted.[52] By pulling various plugs, resistances from .1 ohm upwards could be introduced. In this series of experiments, the primary circuits were led to the interrupting device.

New cams had to be made to suit the new conditions. Instead of two shifting cams operating on the same trigger, two cams were made with the same contour, *i.e.,* they were in every respect, save in position on the spindle, identical. In mounting them on the spindle, they were separated about one centimeter and arranged so that their depressions gave a spondee rhythm of the same kind as the other cams had produced in terms of duration when they were operating together, *i.e.,* each cam produced a single rhythmical member whose duration was equal to the duration of one of the members of the rhythms produced in series *B1* when the cams were set at the spondee adjustment; but since they were tripping levers which dipped into separate cups of mercury corresponding to the two circuits of the primary telephonic circuit, both cams together produced a rhythmical group which was spondee in terms of duration, as in series *B1,* but was variable in terms of intensity, depending upon the adjustment of the strength of the current relatively in these two circuits. When the resistance or, what amounts to the same thing, the current was equal in both circuits, the result was a spondee rhythm,[53] when the intensity of the current corresponding to the first connection made was greater than that of the other, a trochaic rhythm would be produced; and when the intensity of the current corresponding to the last contact made was greater than that of the first, an iambic rhythm resulted. In these experiments, the secondary circuit passed directly from the induction coils of the telephonic system to the receiver in the adjoining room without, of course, entering the interrupting mechanism.

[52] This type is listed in the Physical and Chemical Catalog No. 23, March, 1912, issued by the C. H. Stoelting Co., on page 140 as No. 2939.

[53] The point of subjective equality was determined by a long series of judgments given by all of the observers concerned.

It is evident, then, that in this series, a change of intensity in the two members could be effected by throwing resistances into the primary circuits, without thereby changing the constancy of the durational component.

In the *B3*-set of experiments, no change was made in the interrupting device. Since the series occasioned variations in pitch between the two members of the group, while the durational and intensive factors remained constant, the forks hitherto used had to be replaced by two forks of identical construction, mounted with riders, and giving a range of about 76 vd., from 228 vd. to 304 vd., a major third (a♯-d′♯).[54] They were electrically self-exciting, but the noise of this excitation was not audible through the telephonic receiver, mainly, perhaps, because the transmitter was mounted opposite the resonator of the fork and therefore not in the immediate vicinity of the slight noise produced by the excitation. The forks were about as free from clang elements as the others had been in the other series. The resistance-box, which had done duty in the variation of intensity in the previous series, was cut out of the circuit, and the cam-device for interrupting the primary circuit of the telephonic system was left unchanged.

The forks were equated for intensity as nearly as possible by making the physical conditions identical. This was practicable in this set of experiments because all the elements were the same. In the previous series, different forks were used, the transmitter had therefore to be in a relatively different position for each fork, and the wiring had to be different. An extended set of judgments on the relative pitch, intensity, and duration of the two forks was, nevertheless, undertaken for the sake of practice on the part of the *O*'s and with a view to correct any possible wrong adjustments. We also had difficulty here, as well as in previous sets, with complications produced by one attribute on the adequate judgment of another. Judgments of intensity would very often be confused with judgments of pitch and *vice versa:* but more of this in another section.[55]

Pneumographic records were also taken in this set of experiments. The Verdin pneumograph was adjusted to *O* for chest expansion, rubber tubing was then led through the wall to a Verdin tambour which, together with a Jaquet clock, recorded on the drum of a Ludwig-Baltzar kymograph during the period of the experiment. Later a Kronecker interrupter was used at the 1/25 sec. setting in place of the Jaquet clock

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[54] These forks were of a new type, made to Titchener's specifications by the C. H. Stoelting Co., of Chicago.

[55] Stumpf enters into a discussion of this difficulty in his *Tonpsychologie,* Leipzig, 1883, I, 347.

at 1/5 sec. because it was considered desirable to get a more accurate reading.[56]

At first the kymograph was placed in the room with *O,* but we soon found that there was considerable distraction caused by the noise of the kymograph-motor. For this and other reasons it was taken out of the room.

In a short series of experiments, light-flashes were used to produce a rhythm. Let us call this series, *L.* Connections were made from a source of direct electric current at about 100 volt pressure through one of the levers of the interrupting device, and from the same source through a resistance, bringing the pressure down to about 50 volts, then through the other lever of the interrupting device. From this device the wires passed on through suitable switches to the adjoining room where they ended in a Mazda incandescent light of 40 watt consumption. This light was placed in a small box, 17 x 17 x 20 cm., whose inner surface was lined with white Bristol board in order to reflect as much light as possible. On one side of the box was a circular opening 1 cm. in diameter, which was covered with translucent architect's tracing paper. This box was placed midway between *O* and a white screen a trifle over a meter from *O* so that the light from the opening in the box was reflected from the screen, covering an area about 60 cm. in diameter.[57]

The opening of the box was, of course, toward the screen. The screen was bounded by black cloth, which limited the field of illumination to some extent. A 4 c. p. incandescent light was allowed to diffuse its light from a point two meters behind *O* over the screen in order to produce conditions unfavorable to the formation of after-images. *O* was allowed to regard the screen in the period before the experiment began and in the period of preparation. The rhythm produced was, as were all the rest, two-membered, in which either the first or the second flash was the more intense—and also, owing to the conditions, the more extensive—of the two, or with another arrangement of the wiring, both were of equal intensity (and extensity), *i.e.,* the interrupting device was so arranged that it would deliver a current through the Mazda light of 100 v., then 50 v., *etc.,* or 50 v., then 100 v., *etc.,* or 100 v., then 100 v., *etc.,* or 50 v., then 50 v., *etc.* The Mazda filament lends itself very well to this sort of flash, because it reaches its maximum illumination or glow very quickly (in about .05 sec.). The receiver was naturally disconnected from the telephonic circuit in this particular series.[58]

[56] This interrupter is of the lamella type and is made by G. Hasler, Bern.

[57] The conditions were approximately those of the experiments in light-rhythm by Koffka, *op. cit.,* 6.

[58] After reading this account of apparatus used in the experiment, the importance of physical instruments in our investigation will doubtless be realised. That at least one recent writer challenges our

D. Method.—In these experiments over 700 observations were taken during a period of two years, from February, 1911, to February, 1913. Each one of these observations was a detailed introspective account of consciousness during the period of the presentation of the rhythm. Three hundred and sixty-five of these were in the preliminary series, which covered about one half-year's experimentation; the remainder, 313, much more detailed and analytical in character, extended over the remaining year and a half.

In the preliminary experiments, series *A1* and *A2*, subjectively accented and objectively intensified rhythms were investigated. In the series *A1*, which dealt with subjective rhythms, the first instruction used was:

(*a*) To assume mentally and physically as relaxed a condition as possible: to give a general introspective account of the rhythmical consciousness, with special reference to the process of grouping of the metronome-beats and any change that may occur in that process.

Under this instruction B dictated 17 introspections, Bu 21, D 20, and Ge 4. With every *O* except D the metronome was allowed to run for 45 sec. and the introspections were taken after that period. After the first trial, D having remarked that the changes in her rhythmical consciousness were so numerous that she could not well remember them, her introspections were taken after a 15 sec. play of the metronome. All of the *O*'s preferred and were allowed to keep their eyes closed during these experiments. Noticing that most of the introspections centered about factors relating to accent, we considered it advisable to focus introspection more closely upon this point. To this end the instruction was changed to the form:

(*b*) To notice in terms of what psychical factors accent is determined.

Under this instruction B dictated 1 introspection, Bu 15 introspections, D 12, and Ge 8.

In the second set of experiments, *A2*, objectively intensified sounds were presented to *O* in rhythmical groups by means of the metronome and rhythm-box.

Again we followed in the main, as we had done in the A^1-set, the suggestions in Titchener's *Manual*,[59] but evidently for a different

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right to grant this importance, we may gather from a passage in Verrier's *Old Testament and Semitic Studies*, Chicago, 1908, 177: "It is almost superfluous to point out in this connection, that facts which require instruments for their discernment have no place in the study of rhythm."

[59] Titchener, E. B. Experimental psychology, Vol. I, pt. I, 176-7; pt. II, 339-47.

purpose than that outlined in the text. Our aim was not so much to discover what form the group assumed or what value, in terms of intensity, duration, or quality, was assigned to the various members of the group, but to obtain a fairly accurate description of the constitution and pattern of the rhythmical consciousness under the influence of the perception, with different degrees of difficulty, of the objective rhythms described below. Bolton had come to the conclusion that the normal group is the two-group; more difficult is the three-group, or the twice-three-group; and still more difficult is the five-group.[60] The rhythms that we used were: ('— —), (— '—), ('— — —), (— — '—), (— '— —), ('— — — —), ('— '— — —), (— — — '—), ('— — — — — —), ('— — '— — —), ('— '— — '— —). Each group occurred twice at three different rates each time (152, 176, 200 per min.). The consecutive order was haphazard, though definitely outlined before the experiment was begun. This means that an easy or difficult rhythm was presented to *O* without his foreknowledge as to the degree of its difficulty. The rhythm usually lasted 15 sec. At the very end of the series, this time was changed for reasons given later.

This *A2*-set of investigations was considered the more important of the two preliminary sets because, in the first place, the *O*'s had gained practice in the introspection of the rhythmical consciousness in the course of the first set; and again, because the experimenter took greater pains to concentrate the instruction on such crucial points of attack as had already been suggested in the course of the *A1*-set and in the early part of the *A2*-set; and finally, because more introspections were taken and a longer period of time was spent on this set.

In these as well as in previous experiments, the usual 'ready' signal was verbally given. The introspections were then taken down word for word and are preserved *in toto* in the protocol. The first instruction was:

(*a*) To describe the rhythmical consciousness, giving especial attention to (1) the ease with which the particular rhythm is perceived, (2) any change in the rhythm that may occur, (3) the placing of major and minor accents, and (4) the duration of the beats.

Under this instruction Bu dictated 35 introspections, D 24, and Ge 20. Toward the end of the series, it was suggested, especially in the introspections of D and Ge, that there was some change in the nature of consciousness during the period of the playing of the rhythm. This induced us to turn the instruction into the form:

(*b*) To observe any changes that may occur in the course of the rhythmical consciousness.

Under this instruction Bu dictated 15 introspections, D 16, and Ge 20.

Believing that we could get much better results by resorting to the method of 'fractionation,' well-known in the reaction

[60] Bolton, T. L. *op. cit.*, 212.

experiment, we divided the whole period into two parts, called respectively the 'fore-period' and the 'after-period.'[61] The former began with the objective playing of the rhythm and ended with the clear perception of the rhythm on the part of *O*, at which time the objective rhythm was stopped at a signal from *O*. The latter began with a clear perception of the rhythm and ended with the close of the usual 15 sec. period.

It must be recognised that (1) the dividing point in this fractionation, if arbitrary, was well suited to our investigation because we were hoping to find the critical phenomena which centered about this point, and (2) that it was necessarily indefinite, because the recognition of the rhythm may be gradual and not climacteric, but that with practice this fact presented no unusual difficulty. While a sharp, distinctive fractionation of the period was ideally aimed at, a slight prolongation of the fore-period or the after-period did no serious harm.

Bu gave 9 consecutive introspections under the instruction covering the fore-period, D 15, and Ge 13. The instruction read:

(*c*) To observe the rhythmical consciousness until the moment when the rhythm is clearly perceived (at which time a signal is to be given to the experimenter to stop the objective rhythm), giving no attention to the report of the kind of rhythm perceived, but making sure that the kind is definitely determined.

Then Bu, D, and Ge each gave ten introspections under the instruction pertaining to the after-period. The objective rhythms previously reported on were repeated for these introspections. The instruction was:

(*d*) To observe the rhythmical consciousness after the moment when the rhythm is clearly perceived, giving no attention to the report of the kind of rhythm perceived, but making sure that the kind is definitely determined.

Convinced that this method had given enough practice in the recognition of the dividing-point of the entire period, we repeated, in the course of a few weeks following, the rhythms used before, but with instructions *A 2 c* and *A 2 d* arranged in haphazard order.

A direct question was then put to each *O* in the form:

(*e*) What difference, if any, do you think, is there between the

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[61] For a discussion of the advantages and disadvantages of this method *v.* N. Ach, Ueber d. Willenstätigkeit u. d. Denken, Göttingen, 1905, 19 f., and G. E. Müller, Zur Analyse d. Gedächtnistätigkeit u. d. Vorstellungsverlaufes, Leipzig, 1911, 75 ff.

rhythmical consciousness under instruction *A2c* and the rhythmical consciousness under instruction *A2d*?[62]

Finally the rhythmical consciousness was allowed to degenerate, as it were, under instruction *A 2 d,* by the lengthening of the period from 15 sec. gradually through stages of 20, 25, 30, 35, 40 and 45 seconds duration to the extreme length of 1 min. The *O*'s were not told the purpose of these gradual increases in duration, and were generally and quite surprisingly unaware that the period had been lengthened. D gave 11 introspections, and Ge 7. These experiments ended the preliminary series.

With the commencement of the main series of experiments, there was a complete change of apparatus, and, owing to the beginning of a new academic year, there was also a change of *O*'s. D was the only *O* who continued to serve after the preliminary series. In this part of the main series, which we shall call *B1,* a two-membered rhythm was produced, the difference in whose members was one of duration—other physical factors were constant. Our aim in thus separating these factors was to isolate our conditions as far as possible and to see whether, when we had made these conditions as simple as we could, the facts discovered would be the same or different for each analysis. A 15-sec. period was used, which gave time for about 8 complete rhythmical cycles. Ten different changes in the relative duration of the two members were within the limits of the efficiency of the apparatus, and these were made in haphazard order. The first instruction was:

(*a*) To give an introspective analysis of the perceptual consciousness.

D gave 15 introspections, E 8, and G 12. Noticing that, in spite of the fact that there were no differences in pitch and intensity between the members as they were physically produced, the *O*'s were frequently reporting such differences, we saw no way of avoiding the giving of full and explicit instructions as to the physical possibilities involved. The next instruction, therefore, was:

(*b*) The sounds which you will hear will be objectively of the same intensity and pitch but will vary in duration as follows.

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[62] This is a sort of modified 'method of confrontation' as it was used by T. Okabe, in his *An experimental study of belief,* this *Journal,* 21, 1910, 590. For a criticism of the method *v.* K. Koffka, *Zur Analyse d. Vorstellungen u. ihrer Gesetze,* 1912, 21.

Then in every instance the type of rhythm was announced. This instruction was supplementary to *B1 a.* D dictated 7 introspections, E 8, and G 2. But the *O*'s persisted in making the confusion of judgments of intensity, pitch, and duration. A series of judgments was then given with sole regard to the estimation of pitch, intensity, and duration of the second member in terms of the first. Using the settings which corresponded to judgments of equal intensity and equal pitch in each individual case, and with the knowledge on the part of the *O* that this was being done, we repeated instruction *B1 a.* This resulted in 5 introspections from D, 5 from E, and 10 from G.

We found, after we had taken this number of introspections, that two of the *O*'s had come to think that the experimental conditions were not what the experimenter said they were, and that he, perhaps, was trying the effect of suggestion on the perception of rhythm. To meet these conditions the following instruction was given:

(*c*) Throughout the series of rhythmical experiments, suggestion is not resorted to; the knowledge given is in terms of true objective conditions.

Under this instruction D gave 8 introspections, and E 2. After this, we allowed the rhythm presentation to continue for 45 sec. and asked for as detailed an account as was possible under these circumstances. D gave 20 introspective reports, E 6, and G 24.

In order to get at the differences in conscious pattern during the course of the period, we again resorted to the method of 'fractionation,' dividing the total period into three parts as the following instructions will show:

(*d*) Give a minute analysis of consciousness during the period extending from the 'ready' signal to the first auditory impression; and report upon the pitch, intensity, and duration of the members of the group presented.

(*e*) Give a minute analysis of consciousness during the period extending from the first auditory impression to the time when the two auditory impressions are perceived as belonging to a group; and report upon the pitch, intensity, and duration of the members of the group presented.

(*f*) Give a minute analysis of consciousness during the period extending from the time when the two auditory impressions are perceived as belonging to a group to the end of the experiment; and report upon the pitch, intensity, and duration of the members of the group presented.

D gave 10, 7, and 9 introspections under the above instructions, respectively; E gave 4, 4, and 1; and G gave 5, 6, and 8. A few introspections among these were taken under the last instruction while the period was lengthened gradually to 1 min. Finally, the above instructions were given in haphazard order, to make sure that the *O* was not getting practised to the point of giving a 'habitual report,' *i. e.,* in stereotyped form. D gave 24 of these introspections, and E 9.

In the *B2*-set of experiments, changes were made in the relative intensity of the two members of the rhythm-group, while the other factors remained physically constant. The most frequent settings of the resistance-box which introduced these changes of intensity were 0 ohms, 2.5 ohms, 3 ohms, 4 ohms, 10 ohms, and 20 ohms. At first all the *O*'s were called upon to give a series of judgments on the relative duration, pitch, and intensity of the two members. Then a general instruction was given:

(*a*) Give a detailed introspective account of the perceptual consciousness and report upon the relative intensity of the two members.

D gave 2 introspections, E 4, and G 14. D was then asked to 'fractionate' her reports as in *B1 d, e,* and *f*. She gave 7 introspections under this instruction. G gave 3 on the first period only. D was also asked:

(*g*) To introspect consciousness during perception with as passive an attitude as possible.

She gave 8 introspections as the result of the instruction. A series of additional judgments was then taken on account of a slight improvement in the apparatus. Finally D and E were asked:

(*h*) To give an introspective account of consciousness; and to report upon the relative pitch, duration, and intensity of the two members.

Under this instruction, the period was made as long as 1 min. D and E gave 3 introspections apiece.

In the third set of experiments (*B3*), changes in pitch were effected, while the other components remained constant. The changes were again made in haphazard order and ranged from a♯-d′♯. At first a long series of judgments was taken in order to secure practice in the discrimination of the relative pitch of the two sounds. Then the method of 'fractionation' was at once resorted to, and since the instructions were similar to those of *B2 d, e,* and *f,* we shall designate them in the same way, *B3 d, e,* and *f*. B gave 7, F 5, and G 8 introspections in all. Since this series of experiments began at the commencement of a new academic year, a change of *O*'s was made necessary. This accounts for the presence of B and F as *O*'s in this part of the series. Finally instructions *B3 d, e,* and *f* were given in haphazard order. B gave 8 introspections, F 13, and G 10.

In order to get at the significance of kinaesthetic factors in the perception of rhythm in a slightly different manner, we aimed to find out what would happen to the perception of rhythm when an instruction was given to suppress these factors. Accordingly the following was given:

(*x*) Report upon the course of consciousness while inhibiting kinaesthetic processes which are relevant to the rhythmical grouping of the auditory perceptions.

This was done with a pitch-rhythm as a stimulus. B gave 2 reports, F 5, and G 4.

To discover whether what we had found in regard to auditory rhythms would also hold true of visual rhythms, we tried a short series with light-flashes differing in degree of intensity, and forming a two-membered group on the same pattern as the auditory rhythms. After a series of preliminary judgments with respect to the relative intensity of the two members, the instruction was given:

(*a*) You will see a series of flashes on the background opposite you, which background you will fixate upon. Report your total consciousness, and judge the relative intensity of the two flashes.

Under this first instruction of this series, which we will call *L,* B wrote 2 introspections, F 5, and G 6. The instruction was then 'fractionated' as before. Under these instructions, which were at first given in order and then in haphazard arrangement, and which we will label *L d, e,* and *f,* B wrote 5 introspections, F 7, and G 6. It will be remembered that in the description of apparatus we mentioned that pneumographic records were taken in both series, *B 3* and *L.* In one or two places, supplementary questions were asked where the introspection was not perfectly clear. These questions will be given in connection with the introspective results.

At the end of the entire investigation, another 'confrontation' question was put:

(*z*) Can you describe, in a general way, the course of kinaesthetic processes during the period of the experiment?

To this question B, D, F, and G gave detailed answers.

III. Results

A. Preliminary Series.—Under the first general instruction, *A1 a,* we obtained the following typical introspections:

(B) A two-group with decided pauses between the two members. There is a difference between the two members which does not seem to be qualitative, intensive, or durational. I wondered what this was. I discovered very vague kinaesthetic or organic sensations in chest and abdomen. These are different for first member and second member, but it is not clear how. First and second seemed to sound alike but *meant* something different due to these organic sensations. Toward the end, idea of four-group suggested itself. This was followed by hearing of four-group of two pairs. I do not think that the pause between the four-groups was longer than the pause between the pairs of one four-group. At the end of the four-group, however, there were kinaesthetic sensations in trunk meaning, 'this is the end of it.' These sensations were more intense than those accompanying the hearing of the preceding beats. [*Rate: 48 per min.*]

(B) First beat, then long pause, then second beat. Anticipated second beat with the idea of the two-group. Two-group followed and continued when second beat came. Long pause between groups. Very little accent, if any, on first beat. Kinaesthetic sensations in head and body, repeated apparently the same in each group. Vague feelings of swaying (leaning forward) to left in time to the two members of the group. When the idea occurred, it came as a visual image of the leaning. Qualitative differences of the beats discovered toward middle of the experiment. I had idea (not verbal), 'will this change the grouping?' Four-group suggested: next pair became attached to the preceding pair, making the four-group. Four-group continued until the end. The pairs of the four-group seemed exactly like the individual two-groups before,—only that the thing that made the four-group was that one pair persisted as image during the sounding of the second pair. The image was very clear,—probably a memory-after-image. This was in terms of auditory images. There were also less clear visual images consisting of two brown dots on a gray background—meaning the two beats. As soon as the group is completed, the whole complex of the four-group vanishes at once. There seems to be no image persisting from the second pair of the four-group. [*Rate: 42 per min.*]

(Bu) Four-group with accent on third beat. Class in calisthenics suggested in terms of visual imagery. Time was considered good for this but still a little slow. Watched the exercises go on with kinaesthetic reproduction in arms. Auditory imagery of music. Strong visualisation of girl at piano. I think my foot moved three or four times. [*Rate: 42 per min.*]

(Bu) Four-group with accent on three. It kept singing itself (auditory imagery) like a hymn. Later said: 'well done' like a chant. Foot movements corresponding to the four beats. First and third beats were pitched. Finger moved also. I tried to get rid of 'well done' suggestion. [*Rate: 42 per min.*]

(D) Two-rhythm with accent on first beat. Kinaesthesis in throat without articulation and occasionally with counting, 'one, two.' Head moved according to rhythm—down for accent, brought back for non-accent. Strain for accented 'one,' relaxation for unaccented 'two.' Breathing in on 'one' and out on 'two.' There were qualitative changes in the experiment. Visualised soft brown spot on one accented beat. For most of the time, visualised a solid figure of steel-gray-bluish color with angles. More of the accented ones were seen and they were larger. For the first two or three beats there was a tendency to localise the sound behind with a corresponding kinaesthetic strain in the neck. First two or three beats were not rhythmically grouped. Comforting and pleasant rhythm. Feeling of relaxation. [*Rate: 92 per min.*]

(D) 'Tick, tock' said at the beginning for a few beats. No strong sense of rhythm. It was a rhythm of 'sound,' 'silence.' There were strain-sensations during the sound, and relaxation during the silence. Visualised 'sound' as a space of one inch, and 'silence' of four inches from left to right. The edges of these were clean-cut. Sound was seen as a black line, silence as a long gap. Rather interesting. For a while realised that breathing corresponded: inspiration during sound, expiration during silence. Sounds were heard very slowly, with tremendous pause between them. [*Rate: 42 per min.*]

(Ge) Second beat started a one-two rhythm with accent on 'one.' The rhythm seemed to be more intense than before, accompanied by verbal counting. Gave up counting and watched for the peculiar beat (mentioned above), but counting still interfered with the perception of this peculiar beat. It was lost again, but later found to be recurring every sixth beat. It seems to be higher in pitch, of less volume, less intensive (?). Idea occurred where this could come from. Bell-arrangements [of some metronomes—not the actual case with the one used in the experiment] were suggested; then gave it up. [*Rate: 152 per min.*]

(Ge) After a few strokes of no rhythm, a 'one-two' rhythm practically without verbal counting and consisting of sounds only. The accent was on 'two.' Then a few groups of 'one-two' rhythm with accent on 'one.' This was reënforced by verbal counting in terms of kinaesthetic-articulatory sensations. Then confusion followed with no rhythm. Then a 'one-two' rhythm with accent on 'two.' Doubtful whether this lasted until the end. Started to be unpleasant, less unpleasant later, and indifferent toward the end. Required vague organic adjustment, preceded by unpleasant organic disturbance which gradually wore off. [*Rate: 92 per min.*]

As regards some of these introspections, allowance has to be made for the elementary stage of the experiment at the time. In general, however, certain facts already begin to show. Kinaesthesis is prominent; but it may be kinaesthesis of movements of limbs or head, of gross bodily movements, of respiration, of vague organic disturbances in chest and abdomen, or of articulation. Again, in many instances, kinaesthesis may be accompanied or entirely replaced by a series of visual images of movement. Finally, there may occur visual images of a purely symbolic character, in themselves sufficient for carrying the rhythm. Occasionally, and, so far, very rarely indeed, a purely auditory rhythm, without visual or kinaesthetic accompaniments, is reported. In almost every case of reported head, limb, or respiratory movement made by *O*, *E* was able to verify the report by actual observation of the movement. As far as *E* could not, there was also complete physical relaxation.

Under instruction *A1 b*, which was directed to the introspection of 'accent,' we have the following reports:

(B) Kinaesthetic sensation (or image) of slight nod of head on accent. . . . Deep kinaesthetic or organic sensation in head (like a quick pressure) accompanied by a visual image of something in head pressing downward and frontward toward the mouth. (This may be the explanation of the nod I mentioned above.) Perhaps the kinaesthetic and organic sensations are intensified or else changed at the accent; may be only a change in breathing.

[*Rate: 92 per min.*]

(Bu) Accent is marked by left foot being put down. . . . Kinaesthetic sensation from left foot, down with accent, up with non-accent. Organic sensations in head and trunk of moving in time with the rhythm. These sensations are more intense on the accent and less intense on the non-accent. . . . Accent felt more on the left side of the body. Down movement with the accent.

[*Rate: 92 per min.*]

(D) With inspiration came the accent. Accent means strain; non-accent, relaxation. *Spannungsgefühl* less pleasant than *Lösungsgefühl,* but both are by no means unpleasant. . . . Feeling of strain that was continuous through the series but strongest on the accented beat. . . . Accented beats seemed louder, . . . sixth beat louder and visually sharper. There was a long pause after the accented beat. [Note: a description of a visual pattern of figures symbolising the beats follows.] . . . Duration of the beats about the same. Intensity very much the same, though intensity is more marked on the accent. . . . Every accented beat is marked by kinaesthetic sensations of strain in chest (breathing inhibited). There is relaxation for the non-accent. Auditory sensations of accent more intense. Localised accented beats higher up (in kinaesthetic terms of eye-movement). Kinaesthetic sensations in throat more intense and sometimes higher in pitch for accent.

[*Rate: 152 per min.*]

(Ge) Greater clearness for accented sound. Greater intensity and extensity of articulatory-kinaesthetic sensations accompanying counting. I am uncertain whether greater clearness involves greater subjective intensity of sound. . . . I am surer now about difference in clearness: there is a greater clearness and a longer ring, with a higher pitch, like G—GABC. [*Rate: 92 per min.*]

(Ge) A 'one-two' rhythm with accent on 'two.' Second sound differed from first in having greater clearness and in having a peculiar qualitative difference at the end. It seems to last longr. A certain determination was set up to attend to accented sound—I can't get away from it. Felt tongue moving quite markedly extensively and intensively on accented 'two,' while 'one' is articulated more easily (by opening of the mouth). The tongue in saying 'two' seems to interrupt the even breath coming out of the back of the mouth. The verbal 'two' was accented partly by the effort of change involved in the articulation of 'two.' These two different factors are not simultaneous, but sometimes accent is carried on by the auditory differences, and sometimes by the motor differences.

[*Rate: 92 per min.*]

We may summarise these results by pointing out that (1) there is usually strain for the accent; (2) the kinaesthetic sensations are, for the most part, intensified and, without exception, changed in form or complexity. Especially is this true of the sensations of respiration. (3) Where there is visual imagery, this also changes. Introspective facts reported by one or more *O*'s, but not by all, are: greater clearness for accent, higher pitch, changes in duration, in length of pause after accent, greater intensity of sound. Three points are certainly clear: (1) the kinaesthetic complex changes for accent and non-accent, (2) kinaesthesis on the accent is more intensive and is felt as strain or tension, while kinaesthesis on the non-accent is less intensive and is felt as relaxation, and (3) kinaesthesis, prominent as it is, may be temporarily or entirely replaced by visual or auditory complexes.

Passing on from subjective to objective rhythmisation, we obtained the following introspections from the general instruction *A2 a:*

(Bu) Rhythm: 1, 2″ [*as given; rate, 152*].[63] Kinaesthesis of moving foot up, with non-accent; down, with accent. Kinaesthetic sensations of head-movement up, with non-accent; down, with accent. Pleasant affection. 2 much longer than 1, and seemed higher in pitch. Heard tune in the rhythm. Came readily and underwent no change.

(Bu) Rhythm: 1″, 2, 3′ [*given: 1″, 2, 3; rate, 200*]. Moved hand up, and then down with accent, in terms of kinaesthetic sensations. Pleasant affection. No change in accent after the first three or four measures—at first it was on 2. First measure undecided.

(D) Rhythm: 1″, 2, 3, 4, 5 [*given: 1, 2, 3″; rate 200*]. 1 was as long, approximately, as the sum of the durations of 2, 3, 4, and 5, but I am not sure about this. At first strain for the accented and relaxation for all of the unaccented. Kinaesthesis of nodding of head with the accent. Then, in addition, strain-sensation for the four unaccented (2, 3, 4, 5), relaxation for 1. 1 became shorter then (staccato). Counted only once (one measure) to make sure how many there were in it. No tendency to continue afterwards. Not easily gotten; not pleasant but interesting.

(D) Rhythm: 1″, 2, 3, 4, 5 [*as given; rate, 152*]. 1 more intense, clearer, and lasted longer. Later, 4 became accented, lasted longer, and became more intense than 2, 3, and 5. Kinaesthesis of counting to get rhythm which was hard to get and not securely held. More difficult to attend to it than to the rhythm in the preceding experiment [which was a 1″, 2, 3 rhythm]. Not very pleasant.

(Ge) Rhythm: 1″, 2, 3 [*given: 1, 2″, 3; rate, 176*]. First few strokes unrhythmical; caught in second measure. Once thought it was 1″, 2″, 3. Verbal idea, 'perhaps a slip in raising the cover.' Qualitative difference between accented and unaccented. Not merely a difference in intensity. 'Brighter' [mentioned in a previous introspection as a characteristic of the accented beat] now interpreted as

[63] Note: ″ means major accent; ′ means minor accent.

meaning 'higher pitch.' Unaccented more drawn out. Accented stroke sharp and definite—unaccented lost themselves temporally. Pleasant, 'of-course' rhythm.

(Ge) Rhythm: 1, 2" [*as given; rate, 176*]. Found at once and kept throughout, mostly as an auditory rhythm, except at the beginning where there was slight verbal counting. Attention very strongly on accent which seemed longer, higher pitched, and more intense than the lower, muffled, and briefer unaccented. Slightly pleasant, with 'familiarity tag.'

In quoting these introspections, in order to allow for practice and accommodation under the instruction, we have followed the plan of selecting one introspection of each *O* from about the middle and one from near the close of the series. The fact which the introspections, taken together with the complete set from which they are abstracted, brings out is that objective accent is, as a rule, of longer duration, more intense, higher pitched, and somewhat clearer than the unaccented beat. A numerical count of the instances of actual mention of these points in the reports is plainly deceptive because, while a characteristic may be noticed, yet very often, owing to the recency of a previous mention or to the unimportant nature of the appearance, no specific account will be given. To support our statement of the facts, however, and with the above reservation, the following table is appended:

No. of times accented beat reported

	Longer	More intensive	Higher	Clearer
Bu................	16	1	3	0
D.................	15	9	1	2
Ge................	4	5	7	2

In addition to these reported differences between accented and unaccented members, there were introspections on the kinaesthetic, visual, and auditory complexes mentioned in the first series, under the influence of subjective rhythmisation. They did not, however, appear so frequently, nor did they play so prominent a part. This may be due to the fact that the instruction was not focused on this problem.

Our next instruction called for an observation of the changes in the course of consciousness (*A2b*), and gave us results that were especially valuable. During the remainder of the series of experiments, it was this problem, *i. e.,* what are the significant *changes* in consciousness, casually hinted at in the beginning, that occupied a good deal of our attention. The problem is specifically suggested, for example, in such introspections as the following:

(Bu) I counted for the first few measures only. [*1, 2, 3, 4"; rate, 152*].

(D) Feel that head must be nodded when rhythm is difficult. [*1, 2, 3″; rate, 200*].

The nodding of the head helped to decide what the rhythm was at the beginning. [*1″, 2; rate, 176*].

Kinaesthesis of nodding of head, done to fix rhythm. [*1, 2″, 3; rate, 200*].

(Ge) Second and third measures counted; articulation stopped after this. [*1″, 2, 3, 4, 5; rate, 176*].

Mostly auditory rhythm—there was no necessity for verbal accompaniment except in two doubtful places. [*1, 2″; rate, 176*].

In accordance with this instruction (*A2 b*), we obtained the following results:

(Bu) Rhythm: 1″, 2, 3′, 4 [*given: 1″, 2, 3, 4; rate, 200*]. Easily gotten. I did not count after the rhythm was decided. This was done in the first few measures. I thought of jog-trot in time to the rhythm (in terms of kinaesthesis of my own body). Visual imagery of the cadets in double-quick time. Auditory imagery of the band music. All of this occurred toward the end. Pleasant affection.

(Bu) Rhythm: 1″, 2, 3 [*as given; rate, 152*]. When I got it in the second measure, I stopped counting. Auditory imagery of one of the melodies of Easter church songs. Visual imagery, in about the middle of the series, of a minuet-dance. At the very end, kinaesthesis of entering into the minuet. Pleasant affection.

As a rule, Bu has kinaesthesis of nodding of head and of articulatory movements throughout the period, but in two or three reports, she notices, as in the above, that this ceased when the rhythm was certain. In such cases, an irrelevant sort of auditory or visual complex seems to take the place of kinaesthesis. There may be, however, a vague kinaesthesis to accompany this new complex. D is more definite on this point, and, in the large majority of introspections, reports a shading off of the kinaesthetic factor, especially in the capacity in which it occurred at the beginning of the period:

(D) Rhythm: 1, 2″ [*as given; rate, 152*]. . . . . Kinaesthesis marked until near the end when the rhythm began to take care of itself. I did not attend to it with so much effort.

(D) Rhythm: 1″, 2 [*given: 1, 2″; rate, 176*]. This lasted for about two measures. 1 was longer and more intensive. Then a 1′, 2″, 3, 4 rhythm accompanied by movements of the head—left with first two beats, right with last two. Head really moved for the accent of the first beat. Then there was a plain 1″, 2 rhythm with no kinaesthesis. Rhythm was soothing. I felt drowsy.

(D) Rhythm: 1″, 2, 3 (′?), 4 [*given: 1, 2, 3, 4″; rate, 176*]. Rhythm was troublesome at first. Kinaesthesis of jerking of the head to get it, also of counting. Then kinaesthesis dropped out, but I am not positive about this. There was relaxation for the unaccented (last three beats), but this changed to slight feeling of strain, due, perhaps, to slight adjustments of breathing. Affection indifferent. Time seemed to be quickened at the end.

Often a general bodily strain or tension is reported by D as being very prominent at the beginning of the period and as wearing away at the end. The intensity and the pitch of the accented beat, when they are carried kinaesthetically, diminish also. Ge frequently notices that the rhythm becomes merely auditory, with an attitude toward it that is called 'passive:'

(Ge) Rhythm: 1, 2, 3, 4″ [*as given; rate, 152*]. Counted once. Not a particularly easy rhythm. At the beginning it was accompanied by kinaesthetic sensations of movements particularly in the back of the mouth and partly in the eye-wink. Then there was a passive attitude of merely listening with the expectation of the accent and with the filling in of the beats. More of a time-rhythm. Time during the expectation period (during the unaccented beats) is felt as strain. Affection somewhat pleasant, straining. The rhythm was caught indifferently.

Occasionally there is also an illusion of an increase in the speed of the rhythm toward the end of the period. This illusion is also reported by D. With Ge it is evident that when a rhythm is simple and not difficult, the auditory nature of the rhythmical perception, and the passivity of attention as mentioned, are characteristic of the entire period:

(Ge) Rhythm: 1″, 2″, 3, 4 [*as given; rate, 152*]. Very easy and very comfortable rhythm. Passively taken in in auditory terms, except occasional tendency to move parts of the body (especially the right side) in a 1, 2 rhythm—one way with the two accented beats, and the other way with the two unaccented beats. This is an indication of movement, a translation into space perception of that which was not in itself spatial. It seemed to be more of the nature of kinaesthetic ideation of muscular contraction with the two accented, followed by kinaesthetic ideation of relaxation with the two unaccented beats.

As the result of the 'method of fractionation' after long practice and with instructions *A2 c* and *A2 d* arranged in haphazard order, we obtained the following introspections of the first period:

(Bu) Conscious attitude of hesitation during first measure; with the second measure came the verbal idea, 'I must get this.' Then articulatory sensation of counting for two measures. I decided that the second of these was right. Caught it certainly about the fourth measure. Feeling of hesitation and doubt disappeared. Pleasant affection instead. I ceased counting; attitude of satisfaction.
[*Rhythm: 1″, 2, 3″, 4, 5; rate, 200*].

(D) Auditory sensations of successive sounds of varying intensity. 'Muddled feeling' in terms of organic sensations and 'mixed-up' kinaesthesis. Feeling of strain, chiefly in head. Then out of the muddle of kinaesthesis, I distinguished definite kinaesthesis in throat of definite sequence and intensity. This gave the rhythm. Then 'muddled feeling' went. Affection more pleasant, kinaesthesis less prominent. [*Rhythm: as given above in* Bu.]

(Ge) Great confusion. Succession of sounds for a long while. Difference in intensity of sounds noticed. Unsuccessful attempts to count, until one accent appeared and was called 'one.' Every time that accent came it was called 'one.' Strong organic background with unpleasant affection, and involuntary inhibition of breathing (this constitutes part of 'confusion'). Breathing resumed after successful counting started. Somehow, but I don't know how, first few strokes seemed to form a visual series (but not of visual images) into which strokes were translated. It was as if kinematic pictures passed too fast to be caught, except their motion. *Bewusstseinslage* of movement present. This may have been due to eye-movement; but eyes were closed. It was more like the trembling of the eye-lids. Localised visual movement in eyes.
[*Rhythm: 1", 2", 3, 4", 5; rate, 176*].

Introspections descriptive of the second period:

(Bu) Kinaesthetic sensation of moving forward with a sort of swaying movement. Left foot put down with accent. Articulatory sensations of counting. Verbal idea meaning, 'I must forget the rhythm and cease counting.' Visual imagery of people marching in parade. Pleasant affection. Auditory imagery of music with tune to suit the rhythm, accentuating with swaying motion.
[*Rhythm: 1", 2; rate, 152*].

(D) Mildly pleasant feeling of familiarity dying away after a while. Total relaxation, almost drowsy. Auditory sensations forced themselves on me. Attention passive. Rhythm gradually became unpleasant. Painful sensations in ear-drums becoming more intense toward end. Organic sensations and 'mussed-up' breathing. Rhythm decidedly objective. ['Objective' has been described by D as meaning lack of kinaesthesis that carries the rhythm, and also as meaning that the 'rhythm was definitely localised somewhere outside of me']. Attitude of resentment toward it. [*Rhythm: 1", 2; rate, 200*].

(Ge) Rhythm became clear as soon as verbalised with the accent. It was verbalised, in terms of articulatory sensations of tongue-movements, until the end, but became less intense, easier, and less extensive. Affection changed from unpleasant in fore-period to slightly pleasant in this period. Very concentrated attention on auditory rhythm. This was accompanied or marked by pressure of eye-lids against eye-ball, more particularly at beginning, but as a background to the whole. This was less marked at the end.
[*Rhythm: 1", 2", 3, 4", 5; rate, 200*].

In answer to the question (*A2 e*) concerning the difference between the two periods, the following statements were given:

(Bu) I am sure there is a difference. My whole attention is taken up with the desire to catch the rhythm (in the first period) and I have only those sensations which help me make that decision, *i.e.*, various kinaesthetic and articulatory sensations; whereas in the other case (in the second period) there is an attitude of satisfaction and verbal ideas meaning 'I can think of whatever I want to.'

(D) I should say that kinaesthesis was decidedly more prominent in the first period. In the first period the affective course is the reverse of that in the second period, *e.g.*, first period, unpleasantness changes to pleasantness; second period, pleasantness changes more gradually to indifference or even unpleasantness with organic sensations to correspond. There was more effort at attention during the first period. Attention more passive in second period. Auditory sensations are more prominent in the second period. The first period is more interesting and the experience more subjective with the feeling of bodily participation through means of organic and kinaesthetic sensations. There is a feeling of familiarity which comes just at the time of recognition and lasts a little afterwards.

(Ge) Rhythm-consciousness under the second instruction becomes more automatic, which term means a dropping-out of counting, a decrease in clearness, and a wearing-off of the affective tone. The rhythm-consciousness of the first instruction is somewhat influenced by the consciousness of the previous period—a lapping over of the consciousness of the previous period into the rhythmical consciousness (perception of sound). The difference between the two consciousnesses is a gradual change. Under the second instruction, I do not know when to begin to introspect, while under the first instruction I do not know where to stop. The main difference is one of degree of complexity and also, to some extent, a gradual change in the attributes of the conscious processes. [*Question:* Which consciousness is the more complex? *Answer:*] The first one is more complex. The attributes of the mental processes are at a higher level, *e.g.,* at first they are clearer, more intensive, more extensive, and more pleasant, and they gradually change from more to less. The first consciousness is richer in contents and the contents are of higher degree. The second consciousness is poorer in contents and the contents are of lesser degree. The second consciousness is more automatic.

The results of these preliminary experiments corroborate the above description taken in a *post mortem* manner. They may be summarised by the characterisation of a typical rhythmical consciousness as present under these experimental conditions. We must, however, exercise the greatest care in not giving too much prominence to this 'typical rhythmical consciousness.' It is not factual, but descriptive of a class of rhythmical consciousnesses; as such a class-description it must suffer all the qualifications that obtain under the type of generalisation by the method of approximation. We may say, then, that usually at the beginning there is confusion of some sort, accompanied by a complex of mixed kinaesthetic, and, occasionally, visual and auditory sensations, and by unpleasant affection. The contents of consciousness is full, but it is not orderly. There is little or no pattern. Soon,—and the rapidity with which this change takes place seems to depend, among other things, upon the degree of difficulty of the rhythm and upon the mental set and the physical condition of *O*,—some phase of kinaesthesis becomes prominent and is definitely localised and recognised in the introspection. More rarely, some series of visual imagery comes into the foreground. If these processes come into the focus of attention individually, they are usually followed by a combination of several processes, all more or less equally prominent, and all well-ordered and recognised as forming a pattern. Attention is, on the whole, of a high level and discloses a consciousness rich in content. A feeling of strain characterises this high level of attention, and the rhythm becomes definitely fixed in perception. A feeling of satisfaction now ensues, and the affection is reported as pleasant. Should the rhythm be more than moderately difficult, and should it, therefore, not become definitely fixed, or should the mental set of *O* be such that he can not make the rhythm 'fit in,' then pleasant affection may never be reported, and strain-sensations may continue in a vague degree until the end. This was actually the case with some of the odd and infrequently heard rhythms used in the experiments. Then, ordinarily, sensations of strain gradually die away, attention drops in level, kinaesthesis grows less intensive and extensive, and finally vanishes completely or becomes irrelevant to the rhythm. The rhythm is heard merely in terms of auditory perceptions. All kinds of associative ideas may now come into consciousness, but, in the experiments with the 'degeneration' of the rhythmical consciousness, they, too, disappeared. Affection changes gradually from pleasantness to indifference and sometimes to unpleasantness. The rhythm is occasionally reported as 'boresome.' At the extreme duration of one minute in the 'degeneration'-experiments, we found a consciousness almost barren of content, characterised as 'drowsy' and 'sleepy.' The introspections were, as a rule, very brief. As a matter of fact the *O*'s were about ready to fall asleep and often said so. Indeed, consciousness strongly resembled that in the preliminary stages of hypnosis.

This description is based upon facts collected from the preliminary reports of the *O*'s. The evidence is clear and definite, and as far as the typical description goes, it is free from conflicting statements in the introspections of the several *O*'s. But, owing to the preliminary character of the series, the inadequately controlled physical conditions, and the lack of training on the part of the *O*'s, the results so far obtained demanded more substantial corroboration. For this reason we isolated the physical attributes of tonal stimuli as duration, intensity, and pitch; we trained our *O*'s by means of a long series of judgments and introspections; and we shaped our instructions to cover the special points of attack.

B. Main Series.—The following are extracts from the reports of *O*'s under the first instruction in the series (*B 1 a*):

(D) Trochaic rhythm as before. It began at once but did not seem so markedly a rhythm as before. Breathing (inspiration for long, expiration for short) corresponded. This was my natural rate, therefore it did not help the rhythm materially. Kinaesthesis of counting 1 . . . 8 or more at the beginning to help the rhythm—I believe to give it more body. Rather monotonous affair. At about the 8th group, noticed a tonal difference and began saying 're, ti,' *etc.* Verbal idea, with kinaesthesis giving it meaning, 'All this is rather aloof from me,' *i.e.,* from the auditory part of my experience. Kinaesthesis was subjective; auditory sensations localised outside, to the right. Some kinaesthesis of eye-movement (?) which corresponded to the duration of the members. [*Rhythm:* .5", .05"].

Measures not sharply marked off from one another. In my schema there was no break. Schema: kinaesthetic sensations from eyes which would go as visual schema, but it was not seen as such. Some kinaesthesis in throat of counting 1 . . . 8 (and on ?) with overlapping 're, ti.' Then slight head movement, up for high, down for low, accompanied this. [*Rhythm:* .5", .05"].

Distance between members of a rhythm not as long as that between measures. Recognised this as objectively true because of lack of exact correspondence with breathing. Tendency to hurry up one phase of breathing and retard the other. This was slightly bothersome. [*Rhythm:* .4", .2"].

Slight tendency of visualisation of two members as flat thick lines with short distance between them and an implication of a long distance on either side. Lines were seen only two at a time. Kinaesthesis of this more marked than visualisation. Fixation in front. Feeling of balance for the two halves of the measures conveyed by equal intensity and duration of the members and the similarity of the kinaesthesis for the two, although the first was visualised to the left and the other to the right; kinaesthesis was directed correspondingly. Not very marked kinaesthesis located in upper part of trunk, a little throat-kinaesthesis, vague and indefinite. [*Rhythm:* .05", .5"].

Attentive attitude and fixation as before. Qualitative difference in pitch—first higher. Throat-kinaesthesis and auditory imagery corresponding to the quality of the tone. Another long interval and a repetition of the same. Then experience began to be rhythmical—although, perhaps, made so by subjective experience: breathing with an effort to bring the two together. There was also vague kinaesthesis, organic sensations, and some eye-movement (which was important). Toward end there was a trochaic rhythm with very long intervals—ready to break apart at the slightest provocation. Experience slightly unpleasant—so much of a bother—I was set to get the rhythm at all costs. [*Rhythm:* .45", .1"].

(E) Auditory sensations—not very clearly perceived at first. Following first auditory sensation there was a conscious complex which I do not analyse [analysed later: there was doubt in regard to rhythm which was carried by kinaesthetic sensations located (?) in neck, body, and limbs with slight contraction], but which carried the meaning: 'what is the rhythm,' or 'I want to know what it is.' At about this time kinaesthetic sensations located in neck, back, and head, from nodding with the rhythm. Also at this time, or immediately following, kinaesthetic sensations in throat, tongue, and lips as if from incipient singing of tones. After-images from tones, very vague, if at all; memory-image of each couple held over from end of one stimulus to the next. [*Rhythm:* .5", .05"].

At about 3rd group, perception of rhythm, accompanied by pleasant affection and ideas meaning: 'I have caught the rhythm,' 'it goes easier now,' and with following groups as they came there was a perception of the rhythm and recognition of each following group as being like the 3rd group just recognised. [*Rhythm*: .5", .05"].

Attention good throughout the series and tones were in the focus of consciousness. All other contents were much more marginal. There was one fluctuation—this occurring in the interval between the 2nd and the 3rd group, and in this interval there appeared for a moment, very near to the focus of consciousness, kinaesthetic sensations (including general bodily sensations, mentioned above) and perception of comfortableness (not analysed).

[*Rhythm*: .25", .25"].

Both were equally long; this came with the second sound and the perception of the rhythm, and was carried in terms of kinaesthetic sensations which were rather unpleasant. There seemed to be some slight strain-sensations among these. At the same time came ideas (kinaesthetically carried?) which conveyed the meaning, not definitely outlined or worded: 'these tones are fainter and lower in pitch and I can't reproduce them myself as easily as I could the others.' Slight interval. Perception of second group. Perception that these were slightly more intense—due to kinaesthetic sensations from ear. Perception that these sensations formed a group like the first. Unpleasant affection. Unanalysed complex—surprise, vexation, and hesitation. During this time slight strain-sensations in chest, neck, and throat aroused by changed breathing and attempting to reproduce tones internally. Kinaesthetic sensations were quite prominent throughout the period. Toward the end, idea, carried verbal-kinaesthetically, 'I don't like this so well.' [*Rhythm*: .25", .25"].

(G) Knowledge that this sound is the same in intensity, pitch, clearness, and duration as that of the first member. Then same kinaesthesis of muscular tension. Organic sensations pleasantly toned. Pressure sensations of position. Organics. Pressure sensations and sensations of muscular tension were always blended throughout but there were different degrees of blending. Interval following was longer—carried organically. All of this repeated to the end. After the second member of the first group, visual imagery came into consciousness, represented by: [here figure was drawn showing series of black dots, growing more heavy and thick as series progressed, and arranged in a line ending with the heaviest dots to the right]. Line tilted at an angle greater than 90° to the plane of vision. These dots were light and the background black.

[*Rhythm*: .25", .25"].

Auditory sensations of first member in focus. Sensations of general muscular tension. Strain-sensations in left ear and head. Organic sensations very slightly pleasantly toned. Pressure-sensations, *etc.*, of my position. Auditory sensations (or imagery?) faded away and sensations of muscular tension, organics, and pressure-sensations present during interval. Consciousness was at a lower level. Auditory sensations of second member, and knowledge that this was longer, clearer, and more intense (pitch: higher ?), carried in organic sensations. This was in focus. Sensations of general muscular tension. Strain-sensations in ear and head. Organic sensations pleasantly toned (slightly different from those corresponding to first member ?). Pressure sensations of my position. Interval, during which strain, pressure, and organic sensations were present (longer than the interval between members, carried organically ?). All of these processes repeated exactly in the same way to the end. [*Rhythm: .35", .15"*].

We already begin to note the prominence of kinaesthetic factors in the rhythmical perception, but in the reports of both D and E most of these factors have to do with the initial perception, the 'getting,' of the rhythm, and give place to purely auditory patterns when the 'fitting-in' of the rhythm, or what D describes as giving it 'body,' has been accomplished. G, who is apparently more organic in type, has the kinaesthetic factor present in varying degrees throughout the period; but she, too, distinguishes between the general muscular components and the strain sensations which shift about during the period and undergo various 'degrees of blending.' It is difficult to tell what reference all these changes in the texture of consciousness bear for her, because she seldom reports 'meaning;' yet, from a consideration of her descriptive statements, it is evident that kinaesthesis is most often connected with a judging attitude, *i. e.*, with the estimation of durational, intensive, and qualitative peculiarities of the tones and their intervals. We find, however, with all the prominence given to kinaesthesis, that other complexes come into consciousness, especially near the end of the period when the rhythm has been 'caught.' All of the *O*'s report visual imagery; some report pure auditory complexes without kinaesthetic accompaniments.

Since it was the aim of the series to present a purely durational rhythm, and since from the introspections given it was clear that this object had not been attained on the conscious side in that all of the *O*'s reported pitch and intensity, a series of instructions, *B1 b,* and *c,* were given in order to cancel this tendency on the part of the *O*'s. We find, then, that when the instructions, which called for a fractionation of consciousness into three periods, were given to the *O*'s, there was a much greater uniformity of report on the durational factor alone.

If we consider first the reports given for the period lasting from the 'ready' signal to the perception of the first sound, we find again that kinaesthesis centers around the instruction:

(D) Kinaesthesis in head particularly around eyes which meant a vague realisation of *Aufgabe*. Bodily posture more rigid and strained than times without this *Aufgabe*. Eyes turned toward right ear—attitude meaning expected sound coming from that direction. Breathing slightly strained—result of a new *Aufgabe*. Affective tone perhaps slightly unpleasant—meaning bother with new *Aufgabe*. [*Rhythm: .08", .34"*].

Rather strained bodily posture. Strain-sensations in trunk and top of head. Eyes turned to right. Kinaesthesis around them meaning: vaguest flicker of realisation of *Aufgabe*. Vague visual imagery of apparatus at my ear. Affective tone rather unpleasant. Trunk-kinaesthetic strains increased. Then sounds came in terms of auditory perceptions. [*Rhythm: .3", .1"*].

(E) Cutaneous sensations from pressing button. Then dark visual background which was large and into which there came slowly visual imagery of speaking-tube. Then visual imagery of table changing into visual imagery of other room and *E* seated. Next, conscious complex, mostly kinaesthesis, meaning: *E* is giving me a fairly long interval before the tones. Instructions appeared dimly in kinaesthesis. Perception of what I had to do—understanding of it—carried in kinaesthesis. [*Rhythm: .3", .1"*].

Tactual after-image from pressing button. Visual after-image from light—these faded. Dark visual background. General bodily sensations in chest and different parts of body from pressure of clothing, chair, *etc*. Visual imagery of *E*'s room, *E*, and apparatus. Some expectation carried probably entirely in kinaesthesis. Slight strain-sensations which came from attending to (listening for) sounds. [*Rhythm: .22", .1"*].

(G) Sensations of general muscular tension. Respiratory sensations—holding breath slightly. Strain-sensations in ears and head. Sometimes vague visual images of 'pitch,' and 'intensity' (as if written), and something else which stood for 'duration' (?). All these formed an expectant attitude of what I was going to do, *i.e.*, the *Aufgabe*. At very lowest level of clearness—just barely conscious—a confused mass of organic sensations—all 'muddled up.' All processes above mentioned were present during the entire period—excepting visual imagery which came only a few times. They were, however, varying in degrees of clearness, *e.g.*, strain-sensations in ears were very clear—sometimes sensations of general muscular tension. [*Rhythm: .3", .1"*].

In comparison with this sort of description, we find that a report of the second period, from the perception of the first sound to the perception of the rhythm (instruction *B 1 e*), shows a different pattern of consciousness:

(D) Was set for a two-membered rhythm (I infer this). Sounds localised to right and in front—to right of right ear. Bodily posture quite rigid and alert. At first sound there occurred a certain complex of kinaesthesis (especially around eyes) which meant anticipation of rhythm as trochaic. This anticipation was in terms of estimation of particular amount and intensity of strain which accompanied auditory sensations. Then came second sound which confirmed my decision—confirmation in terms of relative amount and intensity of kinaesthesis. Then attitude of assurance—in terms of deeper, freer breathing, tendency to expand chest and to hold head straighter. Rather pleasant. [*Rhythm: .26", .14"*].

A lot of eye-movement with corresponding sensations in neighborhood of eyes which meant a comparison of sounds for duration in terms of eye-strain. A comparison, too, of throat-kinaesthesis which accompanied sounds. Experience quite unpleasant accompanying sensations of nausea and tightness in the diaphragm region. Verbal idea, after a while: 'of course, that was a trochaic rhythm.' Then verbal idea: 'you have only compared the duration.' Then more eye-movement which meant a comparison of intensities of kinaesthesis. [*Rhythm*: *.22″*, *.22″*].

(E) Perception of first group. Slight strain-sensations. Feeling of doubt carried probably by strain-sensations. Perception of second group. Recognition of them as a kind that had been given before. Then judgment followed in words 'short, long.' Next group, words repeated in internal speech. Judgment 'equal' was made for intensity and for pitch—came by kinaesthetic sensations especially in neck and head (nod of head) which were equal.

[*Rhythm*: *.22″*, *.22″*].

Perception of first member, second following, and then perception of group. First was perceived. Then some unanalysed background processes which I do not now analyse. Perception of second with perception of each being equal in pitch and in intensity. Judgments of these not given in the same way—not same processes in one judgment as in another: one, spoken in internal speech, another, kinaesthesis carrying the meaning. Experience of this experiment (*Aufgabe*) does not run easily yet. Strain-sensations in face and from body, especially from changed breathing involved in listening to tones and in trying to make judgments. [*Rhythm*: *.3″*, *.1″*].

(G) Second longer in duration. When I passed my judgment I felt that if the second had been equal in duration, judgment of pitch would have been made more certain—carried in organic sensations. Also felt rather unpleasant when I felt uncertain about pitch and intensity—accompanied by strain sensations. Carriers of rhythm: kinaesthesis of body (in trunk) and respiratory sensations tending to keep time with rhythm. . . . Carriers of rhythm were always in upper level, but varied somewhat in degree of clearness with the exception of two or three times when I was trying to get a final judgment on intensity and pitch. These times I made a supreme effort. Then strain-sensations and sensations of general muscular tension characterise, and the carriers of the rhythm were out of consciousness. [*Rhythm*: *.04″*, *.38″*].

In general, in this period, kinaesthesis clusters about the clear perception of the rhythm, the 'carriers of the rhythm,' and also about the judging complex. It has changed in meaning from that which it bore in the previous period, *viz.*, the comprehension of the *Aufgabe* and the expectation of the rhythm; it is often the same sort of kinaesthesis, connected with movements of the eyes, breathing, *etc.*, but it is recognised as referring to something else. Throat-kinaesthesis seems to play a prominent part in the judgment and realization of the rhythm, as well as general strain-sensations indefinitely localised or general throughout the body. In some cases visual imagery also plays a rôle.

In the last period, however, if the judgment has been properly passed, most of these kinaesthetic complexes relax, become less clear, and less intense. If, as in G's case, kinaesthesis persists, it may become irrelevant to the rhythm.

The entire pattern of consciousness becomes different while the rhythm is still present in perception:

(D) Throat-kinaesthesis at first. This soon died away. Auditory experience left to itself. Almost went to sleep. Interesting only at first. Pleasant throughout. [*Rhythm: .12", .3"*].

Bodily posture relaxed—no accompanying kinaesthesis. Affective tone indifferent or mildly pleasant; began to feel drowsy. After a while sounds became provoking—intruding upon a relaxed consciousness—unpleasant organic sensations. Strong inclination to shake head —to get them off (incipient kinaesthesis). Unpleasantness increased but intermittently so. At times the sounds did not bother so much as at other times. [*Rhythm: .08", .34"*].

Bodily posture relaxed. Throat-kinaesthesis much reduced, then died away gradually. Respiration ceased to correspond. Eye-kinaesthesis continued for a while much less intense. Began to have achy sensations in ears. Affective tone changed from indifferent to positively unpleasant. Increasing irritated feeling. Sounds seemed to force themselves upon me—I couldn't escape them. [*Rhythm: .08", .34"*].

(E) Clearest contents of consciousness were the auditory sensations, and there were also general bodily tactual sensations. Very soon a great number of visual images appeared—images of people around the laboratory and the rooms of the laboratory, especially this room [dark-room]. Clearness of auditory sensations changed from time to time—once or twice they became marginal. Visual imagery changed quite rapidly, and there appeared from time to time repetitions of judgments—they were now all made in terms of internal speech. There soon came a comfortable feeling, rather of an enjoyment of the situation. Strain-sensations prominent in attention became slightly less, especially sensations in face, over the eyes, and in chest,—some also in arm and hands. There seemed to be no visualising of tones as I have noticed before—all auditory. There were also from neck and head (from nodding of head). This meant that I was satisfied with the judgments I had made; that judgments were correct and that I need not attend quite so closely for the purpose of making a judgment. . . . [*Rhythm: .25", .14"*].

(G) Verbal idea: 'Now I have found out what the rhythm is; I will just give myself up to the thing.' [*Rhythm: .3", .1"*].

Throughout the experiment, auditory sensations from tones in receiver with weak sensations from breathing (I tried to keep time) *plus* kinaesthesis in trunk—locality abdominal and above abdomen—also pleasant. Accompanying these, during the first part of the period, fairly weak sensations of general muscular tension. These gradually got fainter and disappeared—their place being taken by general relaxation during which general bodily kinaesthesis became clearer and more intense, and those of keeping time also became clearer and more intense—I mean the sensations of breathing. [*Rhythm: .25", .14"*].

At first part of third period, sensations of general muscular tension, located principally in head and trunk—these gradually got fainter and less clear until they disappeared and a feeling of relaxation took their place until the end of the experiment. Feeling of relaxation became more intense—rather soothing. Then, auditory sensations of tone, and these were accompanied by general trunk kinaesthesis which was pleasant. Kinaesthesis located principally in region of abdomen and above abdomen. This kinaesthesis, as the period progressed, became less clear and intense, and shifted. [*Rhythm: .12", .3"*].

G has, however, much more kinaesthesis accompanying the rhythm throughout the entire period than have the other *O*'s. While there is a marked shift in pattern of consciousness, with accompanying shifts in the relative intensity, clearness, and modality of the processes, and while there is a decided change in the meaning which kinaesthesis bears, *i. e.*, a tendency to mean 'relaxation,' a great deal of kinaesthesis from breathing survives and accompanies the rhythm. *E*, however, noticed that G was never as sure of her judgments as were the other *O*'s. If, therefore, a doubtful attitude was taken in this period, G never assumed a passivity of the sort experienced by all of the other *O*'s throughout the entire experiment. She says, for example:

Intensity, pitch, and duration equal. This settled fairly quickly, but continued to judge intensity until near the end—to make sure. [*Rhythm: .22", .22"*].

When the last period was lengthened to one minute, the facts above mentioned in connection with the observations were verified. That the results obtained in this 'fractionated' series are not rendered invalid by this method of 'fractionation,' can be assumed from the comparison of these results with those that were obtained when the period was not subdivided.[64] All of the results given are in agreement, with the possible exception of G's observations, for which a special hypothesis has been advanced.

To continue with the results obtained in series *B2*, in which a rhythm was produced in terms of physical differences of intensity, we find further confirmation of the above facts. Since these confirmations were obtained under new conditions, they may be quoted. Under the first general instruction *B2 a*, we get introspections such as the following:

(D) Acted under *Aufgabe* to judge duration first. Comparison of duration in terms of throat-kinaesthesis accompanying the two sounds, which was in turn measured in terms of eye-kinaesthesis, *i.e.*, throat-kinaesthesis accompanying each sound was accompanied by a certain amount of eye-movement which corresponded in length to the duration of the sound. There may have been slight visualisation of throat-kinaesthesis as two gray bands. Visual part very slight, chiefly in terms of eye-kinaesthesis. That for the second sound was longer

[64] The *O*'s were asked to give an estimate of the 'completeness' and 'accuracy' of their introspections on the basis of 100%. None of the estimates given were below 80% for either one of these; most of them ranged from 90%-100%.

than that for the first, therefore judgment that the second was longer. Judgment repeated itself three or four times automatically. Next judgment one of pitch. Two sounds were localised (visualised ?) in correspondence with their pitch. First sound localised above second—judged higher. Throat-kinaesthesis involved somewhat. Repetition of first two judgments 'longer and lower.' For third judgment, kinaesthesis of throat was measured for intensity. In correspondence with intensity of throat-kinaesthesis—visualisation of two sounds as rounded figures, first to the left of second. First seemed to stand out more (in third dimension toward me) but there wasn't very much difference between them. Finally made verbal decision. [*Rhythm*: *1>2*].

(E) Perception of first tone and imagery present became dim, tone becoming clearer. Slight organic shock as attention turned from imagery to sensational complex. Tones perceived merely as tones for a moment or two—no judgments being made as to rhythm. Then suddenly words 'long, short' spoken in internal speech accompanying the realisation that these tones seemed to be very much like memories of other 'long, short' series. This was not a judgment. Judgment on intensity was made kinaesthetically—a stronger contraction, tension for first, less for second. About this time judgment as to duration was made [equal] merely in terms of slight nod of head which meant 'equal.' Some confusion like inhibition of judgments—strain-sensations were present. Finally pitch judged by a nod of the head—this carried judgment 'same.' Complex of bodily sensations throughout the experiment, partly from change in breathing. [*Rhythm*: *1<2*].

(G) Sometimes I just gave myself up to the rhythm; at these times, auditory sensations of tones, a kinaesthetic swing of the trunk with each auditory sensation, pleasantness, and a general muscular feeling meaning relaxation would be in a cross-section of consciousness. This 'giving myself to the rhythm' occurred at the first only: during the latter half I judged intensity, pitch, and duration. Then there were marked strain-sensations in ears, and a general feeling of strain,—rigidity. [*Rhythm*: *1>2*].

In the series of fractionations which followed, *B2 d, e,* and *f,* we obtained such introspections as these for the first period:

(D) Assumed usual position, eyes turned toward direction of right ear. Expectation in terms of kinaesthesis of head and eyes that sound would come from that region. Otherwise performance quite automatic—no *Aufgabe*. My actions were all automatic. [*Rhythm*: *1>2*].

(G) Strain-sensations in region of ears, forehead, and eyes; kinaesthetic and pressure sensations from breathing, meaning *Aufgabe* in general and expectancy of the rhythm (part of the *Aufgabe*) in particular. [*Rhythm*: *1>2*].

For the second period, we have:

(D) First auditory sensation compared in terms of kinaesthesis of head, eye, and throat. There were unpleasant organic sensations. Feeling of perplexity—this lasted quite a while—at same time verbal idea: 'I wonder if the first member is really longer or shorter than the second member.' This in terms of kinaesthetic attitude. Before this a perception of grouping in rhythm. Second member higher.

Idea, chiefly attitudinal, 'whether it seems longer because it is more intense.' Comparison of amount of strain accompanying each sound for duration. Sometimes second longer than the first; sometimes first longer. Final decision, probably equal in duration; second is equal or longer. Intensity, second weaker. Unpleasant coloring in terms of organic sensations throughout the period.
[*Rhythm:* $1>2$].

Characteristic of the last period is this description:

(E) Repetitions of judgments in terms of kinaesthetic processes, in nods very largely, not clearly in speech. Attention to tones not so good as before; general bodily sensations especially from breathing. Pleasant affective tone becoming clearer—seemed to occupy consciousness more fully with the tones. Occurrence of question in terms of verbal-kinaesthesis, 'Was I right?' Then a re-judging of the rhythm. Then assurance of the correctness of the judgment of the rhythm. Again attention much poorer on tones. Much pleasanter affective tone occurred with general bodily sensations. Sensations from change in breathing. Reappearance of *Aufgabe* inattentively in consciousness; words in internal speech in regard to judgment arose from time to time. At one time, visualisation of tones as spots to left—in front of me—one being more conspicuous than the other. Nothing else that is of any importance—nothing that I can recall. Consciousness not very rich, not nearly as rich as consciousness in previous period. [*Rhythm:* $1<2$].

These data further establish our conclusion. Kinaesthesis in the first part of the period carries the meaning of expectancy and the *Aufgabe;* consciousness is, however, not rich in processes, nor complex in pattern. When the rhythm is first heard, kinaesthetic strains, muscular contractions in head, throat, neck, and the other parts of the body, appear as references for the interpretation of the rhythm and as aids to its clear perception; consciousness is rich in processes of several modalities,—visual, auditory, kinaesthetic complexes come in. Gradually, a large part of these kinaesthetic processes disappear; a few may remain; some may become associated with the meaning of relaxation. Irrelevant imagery may appear, verbal ideas may arise; but, in general, consciousness again becomes relatively poor in contents, and less clear in attention. All of these facts were more emphatically brought out when the entire period was lengthened to one minute (*B2 h*). We were interested in the question how much kinaesthesis would be present in the rhythmical consciousness when the condition of passivity was insisted upon by *Aufgabe*. For this reason we gave D instruction *B2 g*. Under this instruction she says:

(D) Fixation and passive attitude as before. Deep, full breathing with some attention on that as well as on right ear—lazy feeling after a few seconds—then sounds came, localised in right ear. First one longer and louder than second—fairly smooth and rather musical.

Immediate acceptance of trochaic rhythm. Vague bit of kinaesthesis chiefly in head, in eye-region, somewhat in terms of respiration, *i.e.*, breathing became more easy after acceptance. There was involuntary throat-kinaesthesis accompanying each sound, really a repetition of each sound as it came. Part of time remained passive but after a while attention wandered from sounds in my ear to kinaesthesis in my throat. A comparison of throat-kinaesthesis accompanying sounds in terms of their intensity and also intensity of eye-kinaesthesis accompanying throat-kinaesthesis. Then verbal idea: 'you are not following *Aufgabe,* you should relax.' This was followed by readjustment of bodily posture, decrease of throat-kinaesthesis. Then later, verbal idea, chiefly attitudinal: 'the rhythm seems much more alive when there is throat-kinaesthesis. It seems rather monotonous without it.' Affection indifferent. [*Rhythm: 1>2*].

With a change in two of our three *O*'s, and with our apparatus altered to give differences in pitch in the objective sounds produced, we pass on to the next series of introspections. As the result of fractionating the entire period, we obtained characteristic introspections of the first period, of which the following are abstracts:

(B) Strong, clear, tactual-kinaesthetic sensations from hand and forearm (pushing button), overlapping temporally auditory sensations and ear-strain *plus* eye-movement sensations (mean sound of buzzer with slight effort to hear it). Followed by a considerable period characterised by kinaesthetic sensations in head, like a very faint dizziness *plus* slight eye-strains *plus* visual sensations of very dark, uniform, black field (means blankness, nothingness, a sort of unconscious expectancy). Followed by kinaesthesis in chest—strong, means: 'oh, I must listen for the rhythm.' Interval. Followed by auditory sensations *plus* visual imagery of *E plus* eye-strains (means *E* walking heavily on floor). Followed by sensations—conscious attitude, perhaps,—means: 'no, that isn't *E;* it's someone pounding.' Very fleeting. Followed by interval characterised by expectancy, impatience. I do not remember content. Quite unpleasant; somewhere in the interval I wondered whether anything had gone wrong and the rhythm wasn't going to come; also thought how long the period was; also wondered if I could ever report it all. These things came up with prominent kinaesthesis and a little visual imagery, generally accompanied by eye-strain. There may have been slight verbal tags. I cannot, however, remember the content in detail. [*Rhythm: a#, d'*].

(F) Pressed button, turned eyes toward left ear. Strain-sensations begin and continue throughout the period. These strains are localised especially in abdomen, though somewhat less intense strains in chest, arms, neck, and face. The abdominal strains seem to color the entire experience slightly unpleasantly, especially after I have waited a little and the sound doesn't come. Very vague auditory imagery of a sound and auditory verbal imagery: 'pitch,' 'rhythm,' during the period. [*Rhythm: d', a#*].

(G) Pressure and muscular sensations in right hand from pressing button. Then mass of kinaesthetic sensations from trunk, upper part principally, and at times taggy very unclear auditory-verbal kinaesthesis, all standing for the instruction. Then strain-sensations in left ear-region and some other unanalysed organic sensations—forming expectant attitude of what was to come.

[*Rhythm:* c', $a\#$].

For the second period, we have the following descriptions:

(B) Auditory perception of first member. Followed by strong kinaesthesis in arms, trunk, and head (means: adjustment to situation and effort to attend to sounds). I think above was accompanied by unclear kinaesthetic sensations in chest (meaning: expectancy of second member, sort of anticipation of the two-rhythm). Auditory perception of the second member, including strong kinaesthesis in body. The second perception came not suddenly but as if anticipated. I think the kinaesthetic factor was not new, but the old anticipatory kinaesthesis became clearer and probably intensified. The second member came almost as familiar, although I cannot say that the familiarity was carried by anything more than the kinaesthesis mentioned above, and the whole consciousness was more indifferently than pleasantly toned. After second member there was period of strong kinaesthesis, very different from that mentioned—meaning was uncertainty; I should say now that it was uncertainty as to whether I had yet perceived the rhythm or whether I should still go on.

[*Rhythm:* $a\#$, c'].

(F) First two sounds which came to me formed a rhythmical foot this time. In period of waiting for sound to come, I had talked to myself, something like this: 'there is really a rhythm coming, maybe it will be like the last.' Turned eyes toward ear, strain-sensations, auditory images of rhythm to come ('high, low') as in last experiment. Now when sound did come it was high like the last, and I think I held my breath, or at least the strains in the upper part of my chest and face kept steady, as if I were expecting the second part of the rhythmical foot (steady strains in abdomen, chest, eyes, and face are signs of expectation of sound in the first period. 'Steady' does not mean steady in intensity and clearness, but in duration). Now when the first sound came I think the abdominal strains decreased or disappeared this time while the other strains of the fore-period did not till after I had at least gotten a first foot.

[*Rhythm:* d', $a\#$].

(G) Auditory sensation, then another auditory sensation. Accompanying the second, unanalysed organic sensations—principally in trunk, meaning slight surprise and a 'stutter.' Then auditory sensation of first member, then auditory sensation of second member, accompanied by unanalysed organic sensations meaning: 'not so much difference in pitch.' Then unanalysed sensations (organic) in trunk meaning: 'judge as soon as possible.' Again auditory sensations of first member and auditory sensations of second member, accompanied by unanalysed organic sensations meaning: 'second slightly higher in pitch.'

[*Rhythm:* $a\#$, d'].

The reports for the last period follow:

(B) Period was very long and I cannot more than indicate its general form—mostly in *Kundgabe*. . . . Later a very long period in which rhythm was in unclear consciousness. This was a period of waiting for the sound to cease. The prominent thing in it was eye-pressures and strains. Many clear processes of note-taking—*e.g.,* visual images of the eye-pressure, meaning: 'that's what's in consciousness now.' Organic processes from all over the body would become clear at times and then fade away, leaving the eye-pressures again (these were not the usual eye-movement strains). I can describe the whole period as one of restlessness and impatience for the period to end, with growing unpleasant affective tone. . . . Later period in which I remembered that I probably ought to report pitch differences. Now high tone came into clear consciousness, clearer than low. I seemed to take it analytically in an attempt to describe it. It seemed particularly pervading and granular, rather compressed and thrusting—these are all really meanings of other processes in consciousness, principally visual imagery and ear-kinaesthesis. [*Rhythm: d', c'*].

(F) Attention after this wandered from sounds in receiver to other sounds. . . . I talked to myself: 'why doesn't he shut it off?' 'period is long,' *etc.*

[Note: F, left to himself in this period without any definite task, devises little problems and experiments with subjective shifting of the accent regardless of its objective fixation. In a number of introspections he describes this process in detail. In the prominence of kinaesthetic strains that are called forth with this effort, his consciousness thus described is very similar to one which E once reported when, under the instruction with full knowledge, he was told that a certain form of rhythm was forthcoming, though, because of a slip in *E*'s technique, its direct opposite was produced! Strain and other kinaesthetic sensations forced a subjective rhythm into consciousness by suggestion from the instruction, in direct opposition to a marked objective form.]

[*Rhythm: d', a#*].

(G) With each auditory sensation at first a slight kinaesthetic swing of trunk from left to right with a feeling of general relaxation in terms of general muscular sensations *plus* pleasantness. Kinaesthesis disappeared, relaxation grew more and more distinct, auditory sensations less clear. Then a mass of organic sensations (unanalysed), meaning: 'suppose I must judge duration and intensity.' Then a mass of muscular sensations *plus* unpleasantness, meaning: 'effort to judge.' Judgments carried by unanalysed organic sensations. After judgments, visualised 'duration.' Then relaxation gets more and more distinct again; pleasantness present; auditory sensations less distinct also—they had gotten more distinct during judging. [*Rhythm: a#, d'*].

Several *O*'s had frequently referred to sensations of respiration in their observation; D had repeatedly noted that her respiration kept time with the rhythm; E, F, and G are conscious of breathing sensations when they are judging the rhythm. It is, in general, inconceivable that the rate of breathing could have corresponded with the rhythm as it was reported, *i. e.,* in its simple form, because a reference to the account of the apparatus will show that the rhythmical cycle was uniformly executed in 2 sec., while the average rate of breathing of the *O*'s tested was about 3.75 sec. for the complete cycle: no *O* ever breathed more slowly than one cycle in 5.7 sec. or faster than one cycle in 2.7 sec. That respiratory sensations carried meanings is factual, and, from the theoretical side, plausible. The curves for all the *O*'s tested show a retardation of rate in the period before the rhythm is heard, but no marked variation from the normal rate after that; *i.e.*, there is no uniform change in the rate at any definite place in the period outside of the one described, and the variations during the entire period are not noticeably different from the variations in the corresponding normal curve. Instances to illustrate this fact, taken at random from typical curves, are: a normal curve on a given day gives an average rate for B of 4.04″ ± .34,″ during a rhythm following this it is 4.08″ ± .14″; for F, normal 4.04″ ± .77,″ rhythm-curve 4.01″ ± .096″; for G, normal 3.47″ ± .1,″ rhythm-curve 3.35″ ± .17.″ It is evident, then, that whatever significance respiration may have on the conscious side, physically there is no ground for belief, on the basis of some fifty kymographic records of the rates of breathing during rhythmical perceptions, that there is actual physical correspondence between this rate and the rate of the rhythm perceived.

We tried the effect of attempting to rule out, by means of an instruction (*B3 x*), the conscious presence of kinaesthesis during rhythmical perception.[65] These are our results:

(B) Uncertain whether there was relevant kinaesthesis or not. I felt before starting that I did not know what to do to inhibit kinaesthesis,—that if the instruction did not work automatically, I should be helpless. During fore-period verbal imagery (auditory-kinaesthetic) kept coming up: 'no kinaesthesis,' 'don't,' *etc.* Intense chest-strains and strains (vague) in arms and legs, all meaning resolve to inhibit. Auditory perception of first member was followed immediately by increase of chest-strains and general bodily tenseness, meaning: 'now,' 'don't get kinaesthesis.' . . . Perception of second member was at first purely auditory. Then it became a complex with other processes meaning: 'this is different'—from first member. Most prominent was visual imagery of a black and white streaked, almost formless thing, floating a little below and to the left of center of visual field meaning second member. The blackness of the image meant 'this is blacker than No. 1' thus carrying the distinction. . . . I should say the order of clearness was: clearest, first auditory sensation, then visual imagery; less clear, but quite clear, kinaesthesis relevant to effort to inhibit kinaesthesis relevant to grouping; unclear, kinaesthesis relevant to grouping, if present, and auditory sensations after visual sensations had appeared. . . . In general the effect of the *Aufgabe* was to make the experience very kinaesthetic in that whatever processes there may have been relevant to the grouping came upon a very complex background of fairly clear kinaesthesis. The experience was unusually kinaesthetic,—perhaps, less relevantly kinaesthetic. [*Rhythm: d′, c′*].

[65] This instruction is similar to *B 2 g, q. v.*

(F) Lay back passively at first. But again, as soon as rhythm was 'going nicely,' as soon as I was 'in the swing of it' thoroughly and 'felt at home,' I noticed again the tendency of the throat to follow or sing the rhythm. Tried to stop this and did so, though I think it also involved holding breath. Now when throat was tense and I again put full attention on sounds, I found that my head tended to nod forward with accented beat. I simply couldn't prevent it. In résumé, I tried to attend passively, had actively to stop throat kinaesthesis. Then holding throat steady, I would no longer attend passively to tone but had to attend actively. When I began to attend actively, my head began to nod (actually, or in image), and I couldn't prevent it. [*Rhythm: d', c'*].

(G) With first two auditory sensations, strain sensations in head and unpleasantness, meaning: 'scrappy.' Then with the auditory sensations, vague taggy kinaesthesis in throat and slight strain-sensations in temples and head were the constituents of a disjointed talk to myself relevant to the tones. Then bobbed up for a moment a mass of unanalysed organic sensations in trunk meaning: 'I have to judge the rhythm clearly, two successive sounds as a group: no kinaesthesis.' Then just auditory sensations, tones, and a slight feeling of relaxation in trunk and pleasantness and beginning of vague kinaesthesis in front of chest, a sort of pressure inward. Then organic sensations meaning: 'that won't do.' Then very intense strain in top of head, forehead, temples, and also with each tone a pressure in ear, located near ear-drum, if not there. This becomes somewhat clearer. Organic sensations meant: 'that I could be positively certain that they were in groups of two members.' [*Rhythm: d', c'*].

We note that under the instruction consciousness becomes unusually full of strain sensations and other kinaesthetic complexes because of the effort to inhibit. We also note that in several cases, for moments in the periods of both B and F, a purely non-kinaesthetic rhythm was experienced. The rhythm, it is true, is under these conditions not permanent, nor is it actively perceived as a rhythm; nevertheless, a certain perception of rhythm is possible without kinaesthesis.

We next ventured into an investigation of light-rhythms, again by the method of fractionation (series *L d, e,* and *f*). All *O*'s perceived a rhythm. The three periods were in contents much what we have described in regard to other series, with the exception that kinaesthesis of eye-movements was very marked in all of the introspections—a result probably due to the extensiveness of the flashes on the screen. Although some of the kinaesthesis did carry the meaning of relaxation in the third period, especially for G, this was of far less frequent occurrence in this series than in any of the others. Otherwise the reports are not sufficiently different from reports of corresponding periods in other series to warrant insertion.

In answer to the 'confrontation-question' put to B, D, F, and G, the following statements were made:

(B) I think, although I am very doubtful, that the fore-period sometimes contained expectation carried in kinaesthesis. I am also inclined to believe that the rhythm was sometimes anticipated and I think that this anticipation was in terms of kinaesthesis. Probably there was visual imagery, too; again I do not remember. I am pretty sure that sometimes in the mid-period the rhythm was perceived before the second member occurred, the second member being carried anticipatorily in some manner,—sometimes, I believe, by organic chest-strains which immediately relaxed or changed as soon as the second member appeared. The change meant, I believe, 'the rhythm is perceived.' Kinaesthesis in the after-period carried impatience for the rhythm to conclude.

(D) As I now remember the experience there was (1) considerable kinaesthesis during the fore-period—in the region of the diaphragm (from rigidity of posture, and slightly inhibited breathing), and around the eyes (from the fixation of the region of the right ear). If the fore-period lasted, there was gradual relaxation with corresponding lowering of kinaesthesis. (2) The first sound of the rhythmical series was accompanied by heightened kinaesthesis, I believe (greater strain of bodily posture). There was a great deal of eye-movement kinaesthesis accompanying the auditory impressions with increased violence of 'diaphragmatic sensations' (constrained breathing) until 'clear perception of the group as a rhythm.' Kinaesthesis of nodding of the head usually accompanied auditory impressions. (3) This clear perception was accompanied or immediately followed by a reduction in amount, intensity, and kind of kinaesthesis. The sensations in diaphragm-region became those that mean easier, fuller breathing; they became less relevant to the situation, and on much lower level of clearness. There was gradual relaxation of muscles around eyes, with corresponding reduction in kinaesthesis. If there had been sensations from nodding of head or trunk in time to the beats, this was gradually much reduced. If the period continued for a long time, the kinaesthesis was ultimately much reduced. If now the period continued *ad nauseam*, there was a new set of kinaesthesis (though visceral sensations predominated). I should not swear that relevant kinaesthesis ever wholly lapsed.

(F) In the fore-period, always or nearly always, I think my introspections will show strain sensations of considerable intensity, of considerable extent (distributed through especially the chest, shoulders, face—especially in eye-muscles—and about the ears). It seems to me that in general these strain-sensations keep up (without any marked decrease in intensity—without alternation of strain and relaxation) until sound is heard. Before the time when the period was divided, certainly I should say that in the fore-period, these strain-sensations were not as intense. When the *Aufgabe* is to introspect from the beginning to the judgment (period 2) my general impression is that the strains in the fore-period were less intense than when the instruction was on period 1. Certainly the sounds are the processes which form the focus of attention; with the coming of the sounds there is a relaxation of the strain. When the sounds are heard, not in a rhythm, there are sometimes, I think, feelings of confusion which are partly kinaesthetic—not so much strain, I think, as muscular and articular—not so definitely localised, not so intense, and not so clear as are the strain-sensations of the fore-period. The strains in the eye-muscles still persist after the coming of the sounds with little or no decrease in intensity, but certainly with decreased clearness. Let me add: that it seems as though the eye-strain and the ear-strain in the fore-period were fused with the other strain sensations—forming part of the general 'expectancy.' They persist at a lower level of clearness. What there is different about consciousness when the rhythm is perceived, is very difficult to say: there is, of course, a lack of 'confusion.' The sounds themselves are clearer than when they are heard not in rhythm. Sometimes there is kinaesthesis in the throat which begins when the perception of the rhythm comes, sometimes there is actual or imagined nodding of the head, but I am not sure but that this is true especially when the rhythm is first perceived and that both of these may be lacking and the rhythm be perceived. I am inclined to think that the kinaesthesis in the throat especially is a process which comes in partially or entirely because I have to judge pitch—and also I am not sure but that the nods of the head come in, partly because I formerly had to judge intensity and partly because nods of the head seem in a way to keep the two members of the group more or less apart so that the judgment of pitch is easier. My *Aufgabe,* expressly given by *E* or given to myself with regard to my behavior (conduct after the judgment, period 3), has been extremely varied—therefore not enough alike to generalise.

(G) During the judgment of pitch, intensity, and duration, only weak kinaesthesis was present with each auditory sensation, and sometimes, maybe, not at all (this last point very uncertain). During this time (time of judgment) muscular sensations meaning strain were present; sometimes weak, sometimes intense. After judgment, muscular sensations, meaning relaxation, gradually get more intense and clearer, and then remain at fairly constant intensity and clearness; and the kinaesthesis with each auditory sensation becomes more intense and clearer—gradually, and finally does not vary much. Usually the kinaesthesis is located on one side of the body with the first auditory impression, and on the other with the second. Kinaesthesis nearly always in trunk—though its exact localisation and extent at times (not very often) vary somewhat with or during various experiments. I remember, with some uncertainty, when it was localised in head for part of the experiment (when I did not have to introspect that period). When I had to judge and 'give myself up to the rhythm,' kinaesthesis (although varying in intensity and clearness) was, on the whole, more intense and clearer than when I was to judge in the first place.

When allowance is made for individual variations in these reports, our analysis of the typical consciousness does not lack confirmation. It is plain that kinaesthesis shifts both in formation and in meaning through the period.

IV. Conclusion and Summary

We have obtained a sufficient number of introspective analyses of the rhythmical consciousness from adequately trained observers, and under the isolating conditions of an experimental procedure, to make possible what we consider valid conclusions. They are as follows:

1. There is a decided change in the kinaesthetic processes present in a rhythmical consciousness from the time that the first auditory impressions which form a rhythm are heard to the end of the period of the experiment. These processes vary (a) in texture, *i. e.*, there is a substitution of qualitatively different processes; (b) in clearness; (c) in intensity; and (d) in meaning or reference.

2. There may be a perception of rhythm without accompanying kinaesthesis, in terms of (a) visual imagery, or (b) auditory imagery, or sensation.

3. There is usually a marked change in the affective tone throughout a typical period of rhythmical perception, from slight unpleasantness before the rhythm is grasped, through pleasantness when it is thoroughly perceived, to unpleasantness when it continues without change.

4. There are individual variations in the amount of kinaesthesis, in the degree of its prominence, and in the type of meaning of kinaesthesis that corresponds to the rhythmical perception.

5. Generally, kinesthesis is most prominently connected with the initial clear perception of the type and form of the rhythm.

6. Instruction is almost invariably carried in consciousness by kinaesthesis.

We might sum up our main conclusions in a single formula: Under the conditions of these experiments, it proved that, whatever was the material presented for rhythmisation (equal and equally spaced sounds for subjective rhythm; sounds of different intensities; tones objectively varying only in duration, in intensity, in pitch; flashes of light differing in intensity), kinaesthesis was essential for the establishment of a rhythmical perception. That perception once established, however, rhythm might be consciously carried, in the absence of any sort of kinaesthesis, by auditory or visual processes.

LUTHER'S EARLY DEVELOPMENT IN THE LIGHT OF PSYCHO-ANALYSIS

By Preserved Smith, Amherst, Mass.

"No villain need be; passions spin the plot;
Men are betrayed by what is false within."
—*George Meredith.*

Nowadays in all lines men are turning less than formerly to dramatic incident, and more to psychological struggle; less to the outward phenomenon and more to the inward cause. Novels and plays alike depend less than they once did on catastrophe and more on internal development. Historians, too, are laying less stress on plainly visible signs than on less obvious spiritual causes.

In the Reformation, for example, the Diets of Worms and Augsburg are not only exciting but they are significant moments, for they focussed the attention of Europe. Yet they would never have been possible but for long and silent preparation, an important part of which took place in the solitary cell of a Wittenberg cloister. Far be it from us to attribute the greater part of any strong historical movement to the influence of a single individuality. On the other hand let us not, in our desire to simplify, forget the importance of personality. Few great revolutions have been more dominated or more fully represented by a single man than was the Reformation; the age thought his thoughts and spoke his words, and, in short, received a durable impression from his genius. Realizing the tremendous importance of many other factors in the movement, let us for the moment devote our study to this one, asking what manner of man was Luther, and what were the sources of his spiritual power. Such questions are, of course, capable of only an imperfect answer. Psychology has hitherto hurled itself on the problem of genius as on an impassible barrier. It is quite certain that the whole of a man's life is accounted for by natural causes, the elements of his heredity and environment, but at present so large a portion of these forces are and must remain unknown, that we are forced to act on the maxim somewhere enunciated by William James: "The originality of a man does not date from something anterior; on the contrary certain other things date from it." But while fully realizing that all cannot be explained, we believe that very much can be ascertained by careful study of the materials at hand and by the comparative method.

In undertaking a candid, and, as far as in me lies, a scientific examination of a great man's psychological origins and growth, I must protest, against the possible offence that some of my conclusions may give, that nothing is less my purpose than "to drag the radiant in the dust." It is surely evident that great men are subject to exactly the same natural laws as their fellows. The common assumption that spiritual value is undone if a lowly origin be asserted is surely false, as James[1] has so well pointed out; far more than we realize or like to admit, our highest impulses of love, religion, and morality are rooted in physical, even in pathological conditions. If the branches of the tree reach toward heaven, its roots strike deep into the dark bowels of the earth.

Approaching our subject then, exactly as we would any other man, and taking up first his heredity, it is difficult to deny that he inherited a taste for drink and possibly some of the defects that go with that diathesis. His father, like so many of his contemporaries, drank to excess, that is, to the point of intoxication.[2] Martin himself apparently avoided this extreme, but that he drank a great deal more than was good for him cannot honestly be denied.[3] It is possible to see a consequence of this in the weakness, both of mind and body, of his second son, also named Martin.[4] The direct results of alcoholism are as yet problematic; but it is generally agreed, and this is all that is claimed in the present study, that it probably conduces to mild forms of neurosis and unhealthy excitability. In Luther's case this showed itself in his most

[1] W. James. *Varieties of Religious Experience,* 1908, pp. 10ff.

[2] E. Kroker. *Luthers Tischreden in der Mathesischen Sammlung,* 1903, No. 193.

[3] On this, my *Life and Letters of Martin Luther,* 1911, pp. 318ff, and H. Grisar, *Luther,* ii, 1911, 244-65.

[4] In a play called "Maternity" (English translation in *Three Plays by Brieux,* 1911), the French physician Brieux has given a terrible picture of a case parallel to that which we are studying; an old peasant who occasionally became drunk has a son who stops short of this but drinks steadily and much, and as a consequence has feeble offspring. But this conclusion may be denied.

A German physician named Drenkhahn believes that as alcoholism decreases other nervous diseases increase. An answer to him by E. Bleuler, Alkohol und Neurosen. *Jahrbuch für psychoanalytische und psychopathologische Forschungen,* 1912, iii, 848ff.

conspicuous fault, a hot temper and almost unbridled violence of language.

His childhood was very unhappy. Almost all his reminiscences of it are either of corporal chastisement or of spiritual terrors. His father was a sturdy character, but hard; he once whipped his boy so severely that the latter hated him for it and fled from him for a season.[5] His mother, too, beat him at one time for stealing a nut, until the blood flowed;[6] but in her case he seems to have borne no lasting resentment, for, on recounting this incident many years later he immediately adds that such discipline was meant heartily well. It was with tender affection that he remembered his mother's pathetic little song:

> If no one's kind to you and me
> The fault, I think, of both must be.[7]

The sexual life of the child begins sooner than is realized by most adults, cut off from their infancy by a curtain of amnesia. This would not be worth while noting for itself alone; but the momentous results from these first impressions for the whole spiritual life of the individual, justify or rather necessitate a close examination of this otherwise unpleasant side of the child's experience. Now Luther is a thoroughly typical example of the neurotic, quasi-hysterical sequence of an infantile sex-complex; so much so, indeed, that Sigismund Freud[8] and his school could hardly have found a better example to illustrate the sounder part of their theory than him.

According to these psychologists, the first sexual feeling of the child is usually evoked, quite unconsciously of course, by one of the parents. The boy, in total ignorance of the nature of his own feelings, finds himself his father's rival for his mother's love.[9] This gives rise to jealousy and hatred of the

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[5] *Luthers Tischreden,* ed. Förstemann und Bindseil, 1844ff, iv. p. 76.

[6] Kroker, *op. cit.,* no. 753.

[7] *Luthers Werke,* Weimar, XXXVIII, 338.

[8] Freud's writings are full of this: *e.g.,* Eine Kindheitserinnerung des Leonardo da Vinci. *Schriften zur angewandten Seelenkunde,* VII, 1910. *Cf.* O. Rank, *Der Mythus von der Geburt des Helden,* 1909, with quotation from Freud, p. 7. E. Jones, *Der Alptraum in seiner Beziehung zu gewissen Formen des mittelalterlichen Aberglaubens,* 1912.

[9] "The first sexual excitation of the boy comes from the mother; his first hatred is for the father." Quotation from Freud, in O. Rank, *op. cit.,* p. 7.

father,—just as we have seen to have been the case with Luther,—and these feelings are soon transferred to other adult males. As the consciousness of sex emerges, whatever is connected with that is noted by the child and thus stamped upon his memory. As soon as Luther went to school he had the same unhappy experiences with his teachers that he had had with his father. He not only records with justifiable resentment that one morning he was whipped fifteen times without fault,[10] but also he remembered the obscene jokes made by his pedagogues so plainly that nearly fifty years later he was able to repeat one of them to his students.[11]

The fascination of sex for the child is not purely erotic, but is also due to the mystery with which the subject is surrounded. He broods long over its problems, as for example how babies are born. While still in the uncertain stage Martin was intrigued and impressed by hearing that a pretty young woman of the village had brought forth a dormouse.[12]

When first the secret of marriage is revealed to the boy, he naturally applies his knowledge to himself, particularly in that relation in which he is most interested. It is undeniable that Luther's thoughts turned in this direction. The story which he repeated oftenest in later life was one of incest between mother and son.[13] He says he heard the scandal while a student at Erfurt; but his mind must have been in some measure prepared for it, or it would not have made so deep an impression on him.

At this sensitive stage the boy's mind was filled with spiritual terrors. The devil intervened in all daily life; he and his angels made storms;[14] the child could not throw a stone in a lake without provoking the devils living in the water to raise a tempest.[15] There was a witch who plagued his mother, for she could make children cry themselves to death.[16] What anguish there must have been in this thought for the child! And this old woman, too, or at least one like her, was connected in his early thoughts with the mysteries of marriage.

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[10] G. Lösche, *Analecta Lutherana et Melanthoniana,* no. 545.
[11] Kroker, *op. cit.,* no. 753.
[12] *Commentary on Genesis, Werke,* Weimar, XLIII, 692.
[13] *Colloquia,* ed. Bindseil, I, 68; II, 367. *Tischreden,* ed. Förstemann und Bindseil, IV, 78. Lösche, *op. cit.,* no. 308 with three parallels in unpublished manuscripts of table talk. *Corpus Reformatorum,* XX, 589. Kroker, *op. cit.,* 706, 90. It also occurs elsewhere in his voluminous and poorly indexed works.
[14] Kroker, *op. cit.,* no. 170.
[15] *Lauterbachs Tagebuch,* ed. J. K. Seidemann, p. 65.
[16] *Tischreden,* ed. Förstemann und Bindseil, III, 96f.

A story he "heard as a boy," and related many years after,[17] was about such an one who showed herself in malice the superior of the devil; for when the evil spirit had tried in vain to separate a man and wife, she accomplished the result by hiding a knife under their pillow, thereby giving the husband the idea that his spouse intended to murder him, with the effect that he murdered her. This story, so banal in itself, stamped itself upon Martin's memory because it was the expression of a suppressed dread; the fear that the witch might do something even worse than make him cry himself to death, might separate his father and mother, turning the hated parent against the dear one. No one familiar with the researches of Freud and his scholars into the mind of the child will find this explanation of the story far-fetched or other than necessary. It is truly remarkable that of the very few recollections of his early childhood related by Luther in his table talk, so large a proportion should support the theory here advanced of the deep influence of this infantile complex on his subsequent nervous and spiritual development.

This influence showed itself in two ways; first in his obsession by the devil, secondly by the peculiar part played in his growth by the idea of concupiscence. The former is usually dismissed with a few words about Luther being a man of his age, emerging from the superstitious peasant class. But this only explains the smallest part of his belief, namely the form which it took. This, undoubtedly, he borrowed from the Bible and the current German demonology. But the nervous diathesis which made the devil so real to him, in short the obsession, the "Zwang," was peculiar to himself. Careful study into the lives of his contemporaries, Erasmus, More, Melanchthon, Calvin, show that although they inherited much the same superstitions as he did, believed in the devil and occasionally attributed great misfortunes, public or private, to his direct interposition, their personal consciousness was untroubled by what with Luther was the most real and terrible experience in his life. The fact of the obsession might be verified by hundreds of citations[18] from the Wittenberg professor's works and table talk. A glance at the relevant words in the indices of these volumes will turn up any quantity of descriptions of how the devil appeared at night to dispute with him, and of the vividness and horror of his apparition. The most graphic description of these visions is, perhaps, that in

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[17] Preger, *Luthers Tischreden nach Schlaginhaufens Aufzeichnungen.* No. 196.

[18] A collection of them in Grisar, III, 231ff.

the book *On Private Masses and Ordination*,[19] in which a long argument with the Prince of evil is fully set forth. When the spirit appeared Luther tells how his heart beat, so that he almost died on the spot. But such visits were nothing extraordinary. He assures us he disputed with the devil every night.[20] Dreams came to him because Satan was there to prevent his resting, for "the devil can torture me so that sweat pours from me in sleep. . . . . My hardest battles have been in bed."[21] About the year 1513, while studying in the Wittenberg cloister at night, he heard the devil roar thrice from hell, and had not the courage to wait to hear more.[22]

Turning now for an explanation of this clearly abnormal condition to the specialist in nervous diseases, we read[23] that such obsessions are due to suppressed subconscious forces; devils, psychologically considered, are functional symbols of the repressed but not eliminated elementary sexual life. The origin of the belief in the nightmare, the incubus and the personal devil, is, in short, due to a condition of the nerves, frequently brought on by an abnormal infantile sexual life. Belief in such devils is but a projection of early experienced dread and forcibly repressed wishes, the wish namely to imitate certain functions of the father and the wish to spite him.[24] In a sense the image of the devil is but a projection of the image of the father. A commonly observed proof that these superstitious obsessions are really connected with the infant's sexual life, and one particularly prominent in Luther's case, is the prevalence of disgusting methods of putting the fiend to flight. These are neither more nor less than the child's ways of spiting its parents, and at the same time gratifying a primitive sex impulse.[25] If there is anything in this hypothesis, which seems to be established by broad observation, Luther's case offers strong independent support of it. He states over and over that he found argument of no avail

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[19] "Von der Winkelmesse und Pfaffenweihe," 1533, *Werke*, Weimar, XXXVIII, 197f.

[20] *Tischreden*, Weimar, I, 469. *Cf.* no. 802.

[21] *Ibid.*, 508. References might be multiplied, *e.g.*, *Tischreden*, ed. Förstemann und Bindseil, III, pp. 4-96, *passim*.

[22] *Tischreden*, Förstemann-Bindseil, III, 93. Freud relates a closely parallel case from his own experience, interpreting the hallucination as the child's remembrance of hearing its father snore. *Kleine Schriften*, II, 71.

[23] E. Jones, *Der Alptraum*, p. 70.

[24] *Ibid.*, p. 71.

[25] *Ibid.*, pp. 91, 95.

with Satan, but only some words[26] of untranslatable coarseness, or some act[27] or gesture which is simply unimaginable to persons with no first-hand knowledge of the Reformer's conversations. It is true that he inherited a certain body of these ideas along with his contemporaries Bugenhagen and Cellini;[28] nevertheless it is undeniable that they received from him an emphasis rarely if ever found elsewhere, and only explicable on the hypothesis of real neurosis.

Luther himself was, of course, unable to analyse his own feelings, but he showed surprising insight by the remark that it was the harsh discipline of the home that drove him into the monastery.[29] It was this cruel experience which gave him the dread of the powers of the other world, and which also emphasized the idea of concupiscence as the very instigation of the devil, to be fought against and crushed out at any cost.

A powerful impulse in the same direction was given by the meeting in 1497, when Martin was attending school at Magdeburg, with a Prince of Anhalt who had renounced his worldly position and become a Franciscan, in which quality "he so fasted, watched, and mortified his flesh that he looked like a death's head, mere skin and bones."[30] This was Brother Lewis,[31] born a Prince of Anhalt-Zerbst in 1456, and baptized William. He had taken the vows in 1473, studied theology, and engaged in deeds of charity as well as in works of self-mortification. In 1497,[32] when Luther saw him, he was at Magdeburg engaged in the pious office of mediating between the citizens of that city and their archbishop Ernest. He died in 1504 at Marburg. The extant fragments of his writings show that he emphasized the fear of God and the day of judgment,[33] though he did not forget to mention the love of Jesus. Luther speaks only of having seen the man, but it is

[26] "So kompt der teufl baldt und disputirt mit mir so lang bis ich sage: leck mich in dem a—." Preger: *Luthers Tischreden nach Schlaginhaufens Aufzeichnungen,* no. 31.

[27] If argument does not help, then, says Luther, "weyse man den Teuffel flugs mit eim furtz ab." *Tischreden,* Weimar, I, 469. *Cf.* p. 64. *Cf.* "Bugenhagen's way" of putting the devil to flight, Lösche, no. 337. *Cf. Tischreden,* Weimar, nos. 83, 812. Grisar, III, 255.

[28] On Bugenhagen, last note. For Benvenuto Cellini, his *Memoirs,* book I, chap. 64.

[29] Kroker, *op. cit.,* no. 753.

[30] *Werke,* Weimar, XXXVIII, 105. *Tischreden,* ed. Förstemann & Bindseil, III, 303.

[31] L. Lemmens: *Aus ungedruckten Franziskanerbriefe des XVI Jahrh.* 1911. pp. 8-22.

[32] *Ibid.,* p. 17.

[33] *Ibid.,* pp. 15f.

probable that he knew him quite well, as he did later the priests and monks of Eisenach. His subsequent relations with the family of Anhalt were warm, and can be traced as early as 1515, when he had the friendship of Margaret of Anhalt,[34] wife of Prince Ernest (who died 1516). The influence of such a man as Brother Lewis in turning the susceptible boy from a wordly calling to "religion," as the monastic life was then significantly called, must have been marked. At Eisenach, where Luther attended school for three years after leaving Magdeburg, the priestly influences were very strong. He knew well the priest of St. Mary's Church, John Brown, and the Franciscans of the local chapter.[35]

At the university of Erfurt the influences pushing him towards the monastery must have been still stronger. The town was large and flourishing;[36] the students led a turbulent[37] and fast life, so that Martin himself branded the institution as no better "dann ein hurhauss und bierhauss."[38] This, however, only put into stronger relief the devotion of the numerous bands of monks.[39]

The studies at the university were naturally medieval in their presentation of life. Even those students who professed devotion to the humanities, cultivated the neo-classics of the Renaissance rather than the genuine models of antiquity. For example, when Martin heard his later opponent Jerome Emser lecture on comedy in the summer of 1504, the text was not Plautus or Terence, but Reuchlin's *Sergius,* the hero of which was a rascally monk, whom, in the opinion of Emser, Luther

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[34] This little known fact is proved by Luther's letter to Margaret, November 4, 1519, published in *Mitteilungen des Vereins für anhaltische Geschichte und Altertumskunde,* 1904, x. 137f. In this he speaks of not having seen her for a long time, and that he knew her in or before 1515 may be inferred from the letter of his friend Link to Margaret of January 22, 1515. W. Reindell: *W. Linck aus Colditz,* p. 253.

[35] *Luthers Briefwechsel,* ed. E. L. Enders, I, 1, 3.

[36] A chronicler of 1572 called Erfurt the largest city in Germany. *Mitteilungen des Vereins für anhaltische Gesch.* x. 61. According to W. Köhler the town then had 32,000 inhabitants. *Im Morgenrot der Reformation.* Ed. Pflugk-Hartung, 1912, p. 347.

[37] *Cf. Eobani Hessi De Pugna Studentium Erphordiensium* . . . 1506, my *Luther,* p. 444. Sermon of 1539, Weimar, XLVII, 666: "At Erfurt I saw many killed."

[38] *Colloquia,* ed. Bindseil, III, 101. On the other hand I believe there is no authority for the story attributed to Luther about the assiduous attendance of the students on the lectures of Gambrinus and Tannhäuser, given, after Hausrath, by Dr. McGiffert: "Martin Luther," *Century Magazine,* LXXXI, 177.

[39] On this my *Luther,* 1911, p. 8, references, p. 442.

must have taken for his pattern.[40] The Reformer tells us that the first poet he read was Baptista Mantuan, then Ovid's *Heroides* and Virgil, after which the study of scholastic theology prevented the perusal of any more verse.[41] If the frank sensuality of the one Roman author, and the pathetic grandeur of the other, would take him out of the cramped world he knew, their modern imitator, whom he first read, would impress the reader deeply with his own extremely cloistral spirit. The *Eclogues*, first published at Mantua in 1498, enjoyed immense and immediate popularity, and were frequently reprinted, among other places at Erfurt in 1501,[42] just as Luther was matriculating. The first six eclogues treat of love, with what Balzac calls "la friande concupiscence des ecclésiastiques;" that is, it is described as the most alluring but wickedest thing in the world, a passion inspired by no god but Satan.[43] The description of women is a choice specimen of monastic invective against the dangerous sex. Luther, whose mind was evidently much preoccupied with this side of life, read and repeated the lines over so often that forty years later he was able to quote them word for word:

> Femineum servile genus, crudele, superbum. . . . .
> mobilis, inconstans, vaga, garrula, vana, bilinguis,
> imperiosa, minax, indignabunda, cruenta,[44]

and so on for many lines. The seventh eclogue is on entering the monastery, to which "Pollux" is moved by a vision; the eighth is on the "religion of the shepherds;" the ninth on the morals of the Roman Curia, and the tenth relates a controversy between observant and non-observant monks. It can be no mere coïncidence that in several points these writings foreshadowed experiences soon to come into Martin's own life. The seventh eclogue particularly worked upon his impressionable, morbid fancy, preparing him for the vision he was soon to have. All the influences to which he was subjected worked on him in the same sense; they made him look with terror upon the world as the primrose path of dalliance to hell, with profound distrust upon his own powers of resisting its temptations, and upon the monastery not so much with dread as the painful means of avoiding damnation, as with a sort of love as the harbour and refuge from a sea of troubles

[40] G. Kawerau: *Hieronymus Emser*, 1898, p. 10.
[41] *Tischreden*, Weimar, I, no. 256.
[42] Last reprinted by W. P. Mustard, Johns Hopkins University Press. 1911. For the editions, p. 35.
[43] *Eclogue* II, 112.
[44] *Ecologue* IV, 110ff. Luther's quotation, Kroker, *op. cit.*, No. 729.

where at last a man might find peace from the terrible war of the flesh against the spirit and security from the frightful dream of the wrath to come. If, many years afterwards, he said that impatience and despair made monks,[45] he was thinking of the despair that came to him immediately after his profession; in 1515 he could assert that despair made not a monk but a devil, and that no one was ever a good brother who had not taken the vows from love.[46]

Yet it is possible that he would never have entered the cloister had it not been for a special experience so marked and sudden that as early as 1519 one of his friends compared it to the conversion of the Apostle Paul.[47] His mind was prepared by the terror caused by an epidemic of the plague in the spring of 1505. Such visitations, particularly in times before science had come to protect and strengthen men, frequently cause demoralization. In this case many of the students fled to their homes and some entered the monastery.[48] Martin, too, returned home, to find two of his brothers dead or dying of the terrible disease.[49] The state of his mind can easily be imagined as he turned back to the University. On the journey, on July 2, at Stotterheim, near Erfurt, he was overtaken by a thunderstorm, a meteorological phenomenon which throughout life he regarded as directly due to supernatural agency. In a particularly vivid flash of lightning, followed by a crash of thunder, his overwrought imagination saw a heavenly vision, a divine warning to leave a temporal for a spiritual vocation, and in a paroxysm of terror he cried out: "Help, St. Anna, and I will be a monk."[50] Once before, when he had wounded himself, he had called on the Virgin for aid in like manner,[51] but had stopped short of taking the vow. Now that he had uttered it, he felt it to be binding, and though he regretted it, honorably discharged his promise by entering the Augustinian cloister two weeks later.

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[45] *Tischreden,* Weimar, I, No. 1034. *Colloquia,* ed. Bindseil, I, 124.
[46] J. Ficker: *Luthers Vorlesung über den Römerbrief,* II, 318.
[47] Crotus Rubeanus to Luther, Enders, II, 208.
[48] Eoban Hess's poem on the plague, reprinted in my *Luther,* p. 444.
[49] According to a sermon of 1544; Scheel: *Dokumente zu Luthers Entwicklung,* 1911, p. 19.
[50] This is told most fully in a saying of July 16, 1539, in *Lutheri Colloquia,* ed. Bindseil, III, 187. The same story, differing in details, is related by Justus Jonas in 1538. Scheel: *Dokumente,* p. 30.
[51] *Tischreden,* Weimar, no. 119, where Luther gives the date Tuesday after Easter. The editor conjectures this was in 1503.

Leaving aside much that is otherwise interesting in the history of the next dozen years, and confining our study exclusively to the Reformer's inward development, we find the key to understanding it in the psychological truth that a man's reasonings, opinions, philosophy, are fully as much the effect as the cause of his outward experiences and actions. The reasons he gives for his belief are in fact but the explanations of the experience which led him to that belief. William James asserts[52] that emotions follow and do not precede the bodily state; that we fear because we run away, reverence because we kneel, and love because we kiss. Argument is proverbially useless in changing a man's opinions on the deepest things in life; his attitude towards them is so rooted in his temperament and general culture that only by changing them can we alter his belief. So with Luther: the dogmas of the bondage of the will and of justification by faith only, the foundation-stones of early Protestantism, were attained not by logical deduction from Biblical or any other premises, but merely as an interpretation of his own subjective life.[53]

The most general and long continued of these experiences was concupiscence, the importance of which in Brother Martin's development has already been recognized,[54] though the psychological reason for this, namely that events in his infancy had given this side of life an abnormal, in his own interpretation supernatural, importance has not been seen. It is true that many men, perhaps all men who have any spiritual life, have at times felt the severity of the war of the members against the spirit, the great paradox of wanting to do that which one hates. The annals of monasticism are full of such men, who rolled in snow and lashed themselves with thorns to keep the body under. But it is doubtful whether anyone ever felt the conflict more keenly than did Luther, or gave it the importance in his system that he gave it. Through it all it is to his great credit that, there is

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[52] *Psychology* (1890), II, 449ff.

[53] Father H. Denifle notices that Luther's opinions were won from his experiences. *Luther und Lutherthum,* I^2, p. 447. Prof. F. A. Christie notes the same of some of Luther's ideas. *Harvard Theological Review,* 1912, April, p. 243.

[54] *E.g.* by Denifle, *op. cit.*, by W. Braun: *Die Bedeutung der Konkupiszenz in Luthers Leben und Lehre,* Berlin, 1908, and now by W. Köhler: *Im Morgenrot der Reformation,* ed. Pflugk-Harttung, 1912, p. 371.

good reason to believe, he never sinned with women.[55] To those who realize what the struggle cost him, his repeated assurances that his public battles with princes and false brethren were easier than his inward struggles with the flesh and the devil, seem perfectly natural.

His writings and sayings in later life are full of strong expressions about the horrors of monastic celibacy. For one saying that as a monk he did not feel much lust,[56] there are scores which state the direct opposite. "It is easier," he once exclaimed, "to bear chains and prison than desire; he to whom the gift of chastity has not been given will not get it by fasting and vigils. It happened to me, though I was not much harassed, that the more I buffeted myself the more I burned."[57] The monks, he tells us elsewhere, were tempted by pollutions almost every night, so that they dared not celebrate mass at daybreak.[58] In one place he calls celibacy a terrible torture,[59] and in another, "a sort of secret homicide."[60] So exclusive was his preoccupation with this temptation that he says before his break with the Church he thought there was no sin but lust.[61] Similar expressions might be multiplied indefinitely, from his book on *Monastic Vows,*[62] his *Answers to Duke George,*[63] his *Disputations,*[64] and his letters.[65] Turning from late reminiscences to the early writings of the Reformer, his notes on Lombard's Sentences (1510-11) we get the same impression. The longest note[66] is on concupiscence, which is described as "the disobedience of the flesh and sin;" another entry[67] expresses the opinion that the lust of the flesh compels

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[55] This is proved by the absence of self reproach, and of positive evidence, and by his own testimony, as *Tischreden,* Weimar, I, no. 121: "I never looked at women even when they confessed. . . . At Erfurt I heard none, at Wittenberg three only."

[56] *Tischreden,* Weimar, I, no. 121.

[57] *Colloquia,* ed. Bindseil, II, 352.

[58] *Ibid.* II, 355.

[59] *Ibid.* II, 364.

[60] *Tischreden nach Schlaninhaufens Aufzeichnungen,* no. 348.

[61] *Tischreden,* Weimar, I, No. 126.

[62] Reprinted Weimar, vol. VIII. P. 583 he calls monasteries "lupinaria Satanae." P. 585 he says: "No celibate is without lust."

[63] Both reprinted Weimar, vol. XXXVIII.

[64] *E. g.* P. Drews: *Disputationen Luthers,* 1895, p. 579.

[65] *E.g.* his letter to Reissenbusch, March 27, 1525. Erlangen ed. LIII, 286, *cf.* Enders, v. 145

[66] *Werke,* Weimar, IX, 73ff.

[67] *Ibid,* p. 75.

the spirit to be impure. A third note[68] makes a fine distinction between the guilt of original sin, which may be abolished, and concupiscence, which is evidently conceived as original sin itself, a doctrine, indeed, to which the Wittenberger held all his life.[69] The hardest part of the battle came in the first years for which the fewest contemporary sources survive. Moreover in his public lectures and sermons he would naturally say less about this side of his private life than he felt. Nevertheless the courses he gave on the Bible for 1513-1516 occasionally refer to the matter. In a number of places he glances at it; in one note, apparently about 1514 or 1515, he says that he has learned, both from his own experience and from what others tell him, that even after a man has banished impure thoughts from his waking hours the enemy attacks him in sleep.[70] The lectures on Romans, too, emphasize the bitterness of the war between flesh and spirit.[71] The effect of all this on Luther's theology will be presently shown in detail. Here in general it must be remarked that he discovered that the more he strove against lust the less he accomplished; concupiscence was, he confessed, invincible.[72] With all his frantic efforts he could do nothing against it, nothing to merit salvation or God's favor. Therefore he was doomed necessarily to reprobation unless God of his free grace had mercy upon him. His will was impotent; his works could not justify him, but only God could help him; consequently salvation came from faith in him alone. This in broad lines is the essence of his development in the cloister, but, of course, it was not so simple. Many influences came in to modify his evolution, which must therefore be studied in greater detail.

The first three years in the monastery were a period of great depression. This was partly due to the losing battle with the flesh, for he regarded desire in itself as evil, and when he felt it at once concluded that all was up with his salvation.[73] Partly, too, it was purely physical depression, a pathological condition of the nerves due to their overwrought condition at the time of entry, and to the severe and gloomy discipline

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[68] *Ibid,* p. 75.

[69] *Tischreden,* ed. Förstemann und Bindseil, II, 10. *Werke,* Weimar, IV. 626. *Römerbrief, Scholien,* 107, 162. Grisar, III, 3.

[70] *Werke,* Weimar, III, 423.

[71] J. Ficker: *Luthers Vorlesung über den Römerbrief,* 1908, *Glosse,* pp. 58, 68, and often elsewhere.

[72] *Römerbrief, Scholien,* p. 110.

[73] His own words, Scheel: *Dokumente,* no. 52.

of the rule. Such states are not uncommon even in otherwise normal natures; some physical cause, in this case fasting and the self-inflicted pains of asceticism, produces a state of anhedonia.[74] in which melancholia often takes the form of believing oneself damned. Looking back on his experience, Martin could speak of the cloister as hell, in which the monks were lost souls.[75] In 1518 he described his own sensations as "so infernal that no tongue can tell nor pen write them, nor anyone who has not experienced them believe them; so that, had the agony endured a half or even a tenth of an hour, the man who felt them would have utterly perished and his bones have been reduced to ashes. Then God appears horribly angry, and so does all creation. There is no refuge, no consolation, either within or without, but all things speak accusation. Then the man weeps: 'I was cast out from before thy face,' nor does he even dare to say: 'Lord, correct me not in thine anger.' In this moment, wonderful to say, the soul cannot believe that it will ever be redeemed, save that it feels the punishment is not yet complete."[76] Remembering Dante and other medieval visions of future punishment we must allow that this frightful fear of hell, so little sympathetic to our sceptical age, was in part a natural consequence of the religion which concentrated men's thoughts upon the world to come. In part, however, and perhaps chiefly, the obsession of dread was a direct result of the morbid condition of the young monk's nerves. Another form of the same phobia was the conception of God as a cruel enemy, the root of which idea must be found in the thinker's neurosis, just as was his obsession by the devil. Luther himself vaguely felt the similarity of his feeling towards both God and the devil at this time when he said: "When I looked for Christ it seemed to me as if I saw the devil."[77] His dread of his Creator, in fact, could not be so much the effect as the cause of his conception of him, for the simple reason that long after his dread had departed, his conception of God, as a cruel and capricious tyrant, "who seemed to delight in the tortures of the wretched and to be more deserving of hatred than of love,"[78] remained precisely

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[74] James: *Varieties of Religious Experience,* p. 145.
[75] *Werke,* Weimar, XXXVIII, 148.
[76] *Werke,* Weimar, I, 557.
[77] *Werke,* Weimar, XLV, 86.
[78] See more fully the quotations from *The Bondage of the Will* (1525) in my *Luther,* p. 208.

the same as before. Luther himself naturally attributed all his dread to the teaching of the Romanists: "They made of Christ nothing but a stern, angry judge, before whom we must tremble, as he would thrust us into hell. So they painted him sitting on a rainbow, with his mother Mary and John the Baptist on either side as intercessors against his frightful wrath."[79] Protestant scholars[80] have been at much pains to justify sayings like this by researches in the tracts of the opening sixteenth century, but on the whole they have failed. If there were representations of God's wrath, there was also emphasis on his love.[81] The only certain fact to which Luther testifies is his own subjective feeling, and of that he has no doubt: "I did not love, I hated the just God punishing sinners, if not with silent blasphemy, at any rate with a great wail of indignation, saying that it was not enough that God should beat down poor sinners, eternally lost, with original sin and his law, but that even his gospel added woe to woe."[82]

Angstneurose, if we may borrow from the Germans a word not exactly translatable by either "phobia" or "melancholy" or "obsession," shows itself in other ways, chiefly by a pedantic scrupulosity.[83] Such was exactly Luther's case. "I was a pious monk," he says, "and held to my rule so strictly that I dare assert that if ever a monk got to heaven by monkery I should have done so."[84] At the time of his novitiate the instructor of the young monks was John Jenser von Paltz, an old martinet, and a particularly firm believer in the efficacy of works.[85] He put his charges through a course of discipline which in Martin's case was the very worst thing possible. His life was now bounded by a vicious circle; the more he was depressed the harder did he ply the works of asceticism, and consequently, the worse became his mental and

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[79] *Werke,* Weimar, XLVI, 8.

[80] *E.g.* G. Kawerau: *Luther in katholischer Beleuchtung,* 1911, pp. 52ff.

[81] Grisar, I, 153ff.

[82] Scheel: *Dokumente,* p. 17.

[83] S. Freud: *Kleine Schriften zur Neurosenlehre.* Erste Folge, 2d ed. 1911, p. 63.

[84] *Werke,* Weimar, XXXVIII, 143. *Cf.* also his remarks about hurting his health and wounding his conscience while a monk by "good works." *Ibid.,* XLIII, 615.

[85] On him, T. Kolde: *Die deutsche Augustiner-Congregation und J. von Staupitz,* 1879, pp. 168, 174ff, and the article in the *Realencyclopädie.*

physical condition. He referred the new access of despair to some fault of his own, and confessed all sorts of imaginary sins. "My confessor once said to me," he relates,[86] "when I kept continually confessing foolish faults, 'You are a fool; God is not angry with you but you with him.'" Again:[87] "I often confessed to Staupitz, not about women, but about real difficulties, and he would say, 'I do not understand you.' So said they all." It is possible, indeed, that these sins were not purely imaginary, but only exaggerated. The psychologists are convinced that when a person persistently accuses himself of things he has evidently not done, it is because he has really done things he is ashamed to confess. Numerous instances have been discovered of young people who have confessed to murder, arson and all kinds of crimes of which they were innocent, because their conscience was tormented by their inability to break themselves of the habit of self-abuse, which, they had often been told, destroyed all moral stamina.[88] The habit is unfortunately so common that there would be nothing surprising in finding it in Luther; and only slightly exceptional in his case is the seriousness with which he took it. Indeed he seems to hint at this practice in the saying last quoted; he confessed, "not about women, but about *die rechten knotten,*" real difficulties, or scandals, or hindrances.[89] In his lectures on Romans (1515) he speaks of the "voluntaria et solitaria pollutio" with sufficient detail to excite suspicion.[90]

Luther's malady occasionally resulted in characteristic *crises des nerfs*. On one occasion, according to Dungersheim and Cochlaeus, the brother found him rolling on the floor like one possessed, crying: "It is not I."[91] He also speaks in later life of the intense discomfort it often caused him to look upon the cross, a fact correctly interpreted by Grisar as a nervous symptom.[92]

His earliest extant letter (April 22, 1507)[93] shows that he was greatly wrought up over the prospect of saying his first mass. When he actually came to celebrate that event, the thought that he was going to address God in person horrified

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[86] *Tischreden,* Weimar, I, no. 122.
[87] *Tischreden,* Weimar, I, p. 240. *Cf.* Bindseil: *Colloquia,* II, 290.
[88] S. Freud: *Kleine Schriften zur Neurosenlehre,* I, 87ff.
[89] *Knote* defined *Rätselfrage, Anstoss, hindernde Grund,* in A. Götze: *Frühneuhochdeutsches Glossar,* 1912, s. v.
[90] Ficker: *Römerbrief, Scholien,* p. 26.
[91] Grisar, III, 598.
[92] *Ibid.,* 708.
[93] Enders, I, 1.

him and almost made him flee "like another Judas."[94] Indeed he stopped, and would not have continued but for the sharp admonitions of his prior.[95] A similar experience occurred some years later, on June 7, 1515, when he was walking in a procession carrying the host through the streets of Eisleben.[96] This last experience (if the dating is correct) is the more remarkable as Luther's other extant writings show that by this time he had won considerable self-possession, whereas all the indications point to the conclusion that the earlier years were spent in anguish. The horizon began to clear about the time of his first call to Wittenberg (October or November 1508).[97] The change was due in a measure to the affection and help given by the Vicar General, John von Staupitz, without whom the younger man, in his own opinion, would surely have gone to hell.[98] But it was not the theology of his older friend that helped him, for the similarity of their views later exhibited was due to what Staupitz learned from Luther.[99] Martin was helped most of all by the more cheerful surroundings and by the active work of study and teaching, in which his abounding energies were no longer turned inward, but found an adequate outlet. Not only were his powers thus given scope, but his attention was turned away from excessive concentration on the war with the flesh to higher cultural ends. The psychologists have a special name for this diversion of these impulses from sexual to professional or intellectual interests; they call it "sublimation."[100] The fact itself, that it is the worst thing in the world for a hypochondriac to spend all his time attending to his own temptations and that the best thing for him is to get into a field of absorbing work, this fact is so evident that it needs no learned terminology to recommend it. The point to notice here is that a considerable measure of relief came to Luther as a result of purely external causes; not

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[94] Kroker, *op. cit.*, no. 750.

[95] Schlaginhaufen, *op. cit.*, no. 324.

[96] *Tischreden*, Weimar, no. 137.

[97] His letter to Braun, March 17, 1509, shows that he is calmer, thinking of God as "our God, who will sweetly govern us forever." Enders, I, 4ff.

[98] Enders, XIV, 189.

[99] Kolde: *Die deutsche Augustiner-Congregation*, 296. Luther himself says that Staupitz helped him by turning his attention from "Satanic illusions" to the work of public teaching, Weimar, XLIII, 667.

[100] S. Freud: *Kleine Schriften zur Neurosenlehre*, 2te Folge, 1909, p. 186. This "Sublimierung" is sometimes the direct result of suppression of sex (p. 181), but abstinence does not always tend to develope liberators and men of action (p. 190).

as the consequence of a change of his ideas; least of all does it indicate that he had at this time acquired the doctrine of justification by faith only, for that was still in the future.[101]

[101] As I have recently maintained that Luther's "conversion,"—*i.e.*, the special experience which brought him the message of justification by faith,—came at this time (my *Luther*, p. 15), I must justify my change of opinion. The principal reason convincing me that Luther had *thus early* acquired his fundamental doctrine, was the story which Paul Luther remembered hearing his father tell in 1544 and which he wrote down thirty-eight years later. (Köstlin-Kawerau, I, 749; my *Luther*, p. 19). According to this while at Rome (December, 1510) Luther began climbing the Scala Santa, but suddenly remembering the text "The just shall live by faith," desisted. In any case the reminiscence of a boy of eleven, not written down by him until he was forty-nine, must be unreliable, but now that the story has been discovered in one of Luther's own sermons in a very different form, Paul's version of it must be abandoned. In 1545 (*Zeitschrift für Kirchengeschichte*, XXXII, 607) the Reformer says that while at Rome he ascended the holy stairs with the purpose of getting the soul of an ancestor (his grandfather?) out of purgatory, but that when he arrived at the top he thought, "who knows whether this prayer avails?" This is certainly no proof that he then had the doctrine of justification by faith alone. Rereading of the sources has convinced me that he acquired it in 1515 or 1516. I shall devote another article to the study of Luther's development between 1508 and 1516.

THE FLUCTUATION OF LIMINAL VISUAL STIMULI OF POINT AREA

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I. Introduction

In a series of articles published 1906-08,[1] the writer reported the results of an experimental study of the phenomena usually attributed to fluctuation of attention. These phenomena, it was claimed, belong to three sense fields: visual sensation, auditory sensation, and cutaneous sensation. The problem was raised, it will be remembered, in 1888 by Nikolai Lange,[2] who gathered together the instances of intermittence of minimal sensations and found for them a common explanation in the conception of an instable or fluctuating attention. The recurrent changes in the limen of sensation producing the intermittence are, he contended, due to involuntary changes in the degree of attention given to the stimulus. Previous to the series of articles mentioned above, two other explanations had also been given: (a) Involuntary changes in the adjustment of the sense organ in case of vision and audition, primarily accommodation in case of vision (Münsterberg,[3]

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[1] C. E. Ferree: An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, *Amer. Jour. Psychol.*, XVII., 1906, 81-120; The Intermittence of Minimal Visual Sensations, *Amer. Jour. Psychol.*, XIX., 1908, 59-129; The Streaming Phenomenon, *Amer. Jour. Psychol.*, XIX., 1908, 484-503.

[2] N. Lange: Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception, *Philosophische Studien*, IV, 1888, 389-422.

[3] Hugo Münsterberg: Schwankungen der Aufmerksamkeit, *Beiträge zur experimentellen Psychologie*, Freiburg, 1889, 69-124.

Heinrich,[4] and Heinrich and Chwistek);[5] and (b) an overflow of excitation from the circulatory and respiratory centers in the brain (Münsterberg,[6] Lehmann,[7] Slaughter,[8] Taylor,[9] etc.).[10]

In the series of articles mentioned above it was shown on the negative side that in case of vision at least, intermittence cannot be ascribed to any of the previously mentioned causes; and on the positive side that it is a phenomenon of the adaptation and recovery of the sense organ. Intermittence was denied in case of minimal cutaneous sensation,[11] and the

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[4] W. Heinrich: Die Aufmerksamkeit und die Funktion der Sinnesorgane, *Zeitsch, f. Psychol.*, XI., 1896, 59-76; and Ueber die Intensitätsänderungen schwacher Geräusche, *ibid*, XLI., abt. 2, 1907, 57-59; Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke, *Bulletin International de l'Academie des Sciences de Cracovie*, Nov., 1898, 363-382.

[5] W. Heinrich und L. Chwistek: Ueber das periodische Verschwinden kleiner Punkte, *Zeitsch. f. Psychol.*, XLI., Abt. 2, 1907, 59-74.

[6] *Loc. cit.*

[7] Alfred Lehmann. Ueber die Beziehung zwischen Athmung und Aufmerksamkeit, *Philosophische Studien*, IX., 1894, 66-95.

[8] J. W. Slaughter: The Fluctuations of the Attention in Some of their Psychological Relations, *Amer. Jour. Psychol.*, XII., 1901, 313-334.

[9] R. W. Taylor: The Effect of Certain Stimuli upon the Attention Wave, *Amer. Jour. Psychol.*, XII., 1901, 335-345.

[10] Münsterberg ascribed to this overflow, in case of respiration, an effect on the muscular control of the eye. During inspiration there was more accurate control of fixation and accommodation and during expiration a less accurate control of these adjustments. Lehmann leaves us in some doubt as to just how he believes the effect is produced. He says (op. cit., p. 84): "Wir sahen dass die Reactionen am haufigsten sind in der Nähe des Inspirationsmaximums. Hier ist eben der Blutdruck am grössten, und von diesem Zustand muss angenommen werden, dass er für die psychologische Arbeit des Gehirns günstig sei. Wir wissen ja, dass das Blut, während der Arbeit irgend eines Organes, demselben reichlicher zufliesst. Deshalb ist es höchst wahrscheinlich, dass auch die Arbeit eines Organes erleichtert werde wenn durch irgend eine Ursache eine Vergrösserung des Blutzuflusses herbeigeführt wird." Slaughter and Taylor are inclined to believe that the overflow affects the sensory cells directly. In their experiments a plethysmographic record of the peripheral blood pressure was taken while the fluctuations of the visual stimulus were being observed. They conclude that their results show a coincidence between the maxima of the plethysmographic curve and the phase of visibility of the fluctuation record. Two kinds of maxima are found in the plethysmographic tracing, one due to inspiration and the other forming the crest of a long vaso-motor wave of unknown cause, commonly called the Traube-Hering wave.

[11] In 1907 the experiments in cutaneous sensation were repeated by Geissler (L. R. Geissler: Fluctuation of Attention to Cutaneous Stimuli, *Amer. Jour. Psychol.*, XVIII., 1907, 318-321). A mistake

phenomenon was left open for further consideration in case of auditory sensation. A part of the work done at this time still remains unpublished. Some of it covers points still in dispute. For that reason two articles will be added to the former series. The first is in answer to an article by Heinrich and Chwistek entitled: "Ueber das periodische Verschwinden kleiner Punkte,"[12] and is intended to clear up, if possible, at least so far as the writer's work is concerned, the last point in dispute between the adaptation and accommodation theories. Heinrich and Chwistek maintain that the fluctuation of minimal visual stimuli of point area is caused by periodic changes in the curvature of the crystalline lens and offer their results for stimuli of point area as evidence that the fluctuations of stimuli of all areas are caused by changes in accommodation. In one of the former studies[13] the present writer had worked with stimuli ranging from 2 mm. x 2 mm.—42 cm. x 38 cm. in area. He found that stimuli of these areas fluctuate just as readily for aphakial as for normal subjects, and that changes in accommodation, therefore, can not be considered an essential factor in the production of the phenomenon. It had never occurred to him to work with stimuli of point area. In the present study, however, stimuli

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was made by him in interpreting the writer's method of stimulating the tongue electro-cutaneously that has not yet been corrected. He says, "In repeating Ferree's experiment with electro-cutaneous stimulation of the tongue, we found some difficulty in eliminating the touch, pressure, and taste sensations set up by the electrodes. The best results were obtained by applying a 1% solution of cocaine to the fore part of the tongue, upon which two strips of tin foil (Christmas tree foil), hammered as thin as possible, were laid. The strips were connected with the interruptor of a Du Bois-Reymond induction coil." Christmas tree foil was not used in the original experiments. This material was rejected at once by the present writer as unsuitable. It is much too stiff and gives rise to pressure sensations. Narrow strips of very thin and pliable wrapping foil were used instead. When these were placed on the fore part of the tongue moistened with spittle, the observer was utterly unable to tell whether or not they were in contact with the tongue when the coil was not working. Neither did they under the action of the current give rise to taste sensations. Of the two procedures the writer would prefer the one used in the original experiments. It seems to him obviously better to make the electrodes of wrapping foil than to use the stiffer material and cocainize the tongue into insensibility to contact, more especially since Geissler's observers report that the cocaine itself sets up distracting sensations in the tongue.

[12] W. Heinrich and L. Chwistek: *Zeitschr. f. Psychol.*, XLI., 1907, 39-74.

[13] See An Experimental Examination of the Phenomena Usually Attributed to Fluctuation, 98-108.

of point area have been used. From the results of this study it will be shown that the fluctuations of these stimuli present no especial case; for (a) they occur just as readily for aphakial subjects as for subjects with normal eyes; and (b) identified by the tests used by the writer in his earlier experiments they correspond just as closely to adaptation phenomena as do the fluctuations of stimuli of larger area. In the second paper, work on the fluctuation of auditory stimuli will be reported. In this work the writer has succeeded in getting conditions under which no fluctuations occur, whether the stimulus be tone or noise. His results also enable him to explain without recourse to central factors or the tensor mechanism of the middle ear the fluctuations which do occur under experimental conditions different from those he has used. The completion of these two pieces of work rounds up, so far as the writer knows, all of the outstanding points in his case against fluctuation of attention in its original meaning.

II. The Accommodation Theory

That involuntary changes in accommodation are a factor in causing the fluctuation of minimal visual stimuli was proposed first by Münsterberg in 1889.[14] Münsterberg held that the fluctuation of these stimuli is due to two causes: unsteadiness of fixation and involuntary changes in accommodation. Although different views may be held with regard to the essential physiological and psychological factors in attention, all must agree, he says, that when a visual stimulus is attentively observed the eye is fixated and accommodated so as best to receive the impression on the retina. But this adjustment cannot be uniformly maintained for any length of time. Involuntary changes both in fixation and accommodation occur. These changes weaken and confuse the light impression received on the retina, hence an object just noticeably different from its background will alternately disappear into this background and become distinct from it.

The effect of lapses in accommodation is too obvious, he thinks, to need special explanation. The rays of light are no longer sharply focused on the retina and the image of the object blurs and becomes indistinguishable from its background. For unsteadiness of fixation, however, the case is not quite so clear. The explanation is as follows. Fick, Kirschmann, and others have shown that the sensitivity of the retina to colorless light attains its maximum at a certain

[14] Hugo Münsterberg: Schwankungen der Aufmerksamkeit, *Beiträge zur experimentellen Psychologie, Freiburg,* 1889, pp. 69-124.

distance from the fovea. Thus when the eye loses its fixation, the image of the object fixated travels towards a more sensitive part of the retina. This will cause the image of the rings on the Masson disc, for instance, which in the traditional fluctuation experiment are made just noticeably darker than their background, to lighten and become equal in brightness to the background. This gives the phase of invisibility. When fixation is regained, however, the ring again becomes noticeable. This gives the phase of visibility. These two factors, then, the lightening of the image of the ring due to involuntary changes of fixation and the blurring of its outlines due to involuntary changes in accommodation, should, according to Münsterberg, be considered as the cause of the alternate appearance and disappearance of the rings on the Masson disc which were attributed by Lange to fluctuation of attention. Since the writer has already shown in his first article[15] that involuntary changes in accommodation cannot be considered as an essential factor in these fluctuations,[16] space will be taken here only to point out that changes in fixation should also not be considered essential factors in the sense in which Münsterberg considers them factors. In the first place they could, in any event, have an effect only in case the stimulus was darker than the background. If the stimulus were lighter than the background, the brightening of the image would make it stand out more distinctly from the background than before, instead of causing it to disappear into the background as it is observed to do in fluctuating. Moreover, the explanation can have little or no application to the fluctuation of colored stimuli. Since both of the latter classes of stimuli fluctuate just as readily as the former, the principle can be regarded as having little value for purposes of explanation. And in the second place, this factor could not in all probability even cause the fluctuation of stimuli darker than the background, for the increased sensitivity of the extra-foveal retina would not only cause the ring to brighten, but also the background immediately surrounding it. The effect of the factor would

[15] See An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, pp. 84 and 94-96.

[16] In the original article stimuli ranging in area from 2 mm. x 2 mm.-38 cm. x 42 cm. were used. For the writer's observers fluctuations never occurred when a stimulus 38 cm. $\times$ 42 cm. or larger was observed at a distance of 1 meter. In the experimental portion of the present paper it will be shown that changes in accommodation are not an essential factor in the fluctuation of stimuli of smaller area than 2 mm. x 2 mm., namely stimuli of point area. Thus with the present paper the demonstration will have been finished for the whole range of areas for which fluctuation occurs.

thus be merely to raise both the gray of the stimulus and of the background in the brightness scale, not to make them equal, unless indeed one were affected more than the other by an amount that would be noticeable in sensation, which can hardly be possible since the difference between them is, to begin with, only just noticeable.[17]

Münsterberg supported his explanation by the following experimental evidence. The norm of the period of fluctuation was established for each of his subjects and the following variations of conditions were made. (1) A "prismatische Lorgnette" which moved the field of vision slightly to one side was placed before the eyes. When this was held steadily in position, the period of fluctuation was affected very little, but when it was removed and interposed every 2 seconds, causing the eye to move quickly to the side to follow the shift in the object fixated, the period was very noticeably lengthened. (2) Involuntary blinking was caused every 2 seconds by means of a sharp sound. Fluctuation was prevented. When the eyes were closed voluntarily every 2 seconds, the same results were obtained. This, Münsterberg thinks, was because of the relief of muscular strain produced by the blinking. That is, as the lids are closed, the eyes move downwards and inwards; as they are opened, upwards and outwards (Bell's phenomenon). This frequent relief from the strain of fixating and accommodating so freshens the muscles, he thinks, and improves their action that disappearance never ensues. (3) The whole apparatus bearing the Masson disc was slowly moved back and forth, up and down, and sidewise. Each movement was executed in 2 seconds. Thus, in order to fixate the moving stimulus, the eye was kept continuously moving. The accommodation was also kept continuously changing. In a companion series of experiments the head was moved slowly from side to side. In this case also, in order continuously to fixate the stimulus the eye was compelled to move.[18] Fluctuation did not occur in either series of experiments. Again Münsterburg thinks fluctuation was prevented because the muscles were kept in such a fresh condition that accurate fixation and accommodation could be maintained throughout the observations. A moment's reflection will show (1) that these assumptions cannot be wholly true. The attempt of the eye to follow the moving stimulus, whether the movement was apparent, produced by the interruption of the "prismatische Lorgnette" or actual, produced by the moving of the apparatus bearing the stimulus, must have resulted in the image falling now to this side, now to that side of the fovea. If so, the fixation maintained was far from accurate.[19]. Likewise when fixation was lost in blinking, it was doubt-

[17] If, for example, one were very much lighter or darker than the other, the greater sensitivity of the extra-foveal retina might affect one more than the other enough to cause a noticeable change in the difference between them, but this can scarcely be assumed to be the case when one is only just noticeably different from the other.

[18] The movement was equal in amount to the movement of the head and in the opposite direction.

[19] This frequent shifting of the image from the position previously occupied by it on the retina would give abundant chance for the adapting retina to recover, and thus in terms of the adaptation theory to explain the absence of fluctuation.

less regained through a series of small oscillatory movements as commonly happens before the eye comes to rest in taking a new position. And (2) even were the assumptions true, the argument is not at all differential for Münsterberg's theory. The same effect on fluctuation would be expected in terms of the adaptation theory. An abundant reason was given for the stimulus never disappearing in the effect of the eye-movement on restoring the adapting retina. Eye-movement, it will be remembered, exerts its effect on adaptation in two ways. There is (a) an indirect effect. As the result of the movement the image falls on a fresh area of the retina and the area previously stimulated is given a chance to recover. (b) There is a direct effect which is much greater than the indirect effect, namely, the influence of eye-movement upon the amount and direction of the lymph streams that are continually moving hither and thither in the retina. A detailed discussion of this effect was given in the writer's earlier work.[20] Eye-movement is thus an essential factor in both theories. The relation to fluctuation ascribed to it in the two theories, however, is very different. In Münsterberg's theory, eye-movement helps to cause the disappearance of the stimulus, while in the adaptation theory it is the most important cause of the reappearance of the stimulus. With regard to the relative merits of these two views, the writer will say that a few minutes' observation of a liminal stimulus should be enough to convince anyone that a voluntary eye-movement, for example, instead of causing the stimulus to disappear, will on the contrary serve to keep it distinct; and, if it has disappeared, will cause it to reappear. For a detailed demonstration that involuntary eye-movement acts in the same way and that it is the chief factor in rendering adaptation intermittent, see the writer's earlier articles "An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention," and "The Intermittence of Minimal Visual Sensations."

Münsterberg also conducted a series of experiments in which a comparison was made of the rate of respiration and fluctuation. The results showed that when the respiration was in short quick gasps, the rate of fluctuation was increased; and when it was slow, the rate of fluctuation was decreased. In explaining this result he attributes to breathing an influence on the muscular control of the eye. With the inspiration there is an increase of the muscular control; and with the expiration, a decrease.

The accommodation factor was next taken up by Pace.[21] Pace compared the fluctuations obtained by his subjects before and after the paralysis of their ciliary muscles by a solution

[20] See The Intermittence of Minimal Visual Sensations, *Amer. Jour. Psychol.*, XIX., 1908, 112-129; and The Streaming Phenomenon, *Amer. Jour. Psychol.*, XIX., 1908, 484-503.

In the blinking experiment ,in addition to the effect of the accompanying eye-movement, the blinking would have itself produced an effect on fluctuation. That is, the closing of the lid shut off the light coming from the stimulus and gave the adapting retina a chance to recover.

[21] Edward Pace: Zur Frage der Schwankungen der Aufmerksamkeit nach Versuchen mit der Masson's Scheibe, *Philosophische Studien*, VIII., 1893, 388-403.

of sulphate of atropine, and found no significant difference in his results. He concluded, therefore, that changes in accommodation could not be considered as essential to the phenomenon.

The theory, however, would not down. It was revived by Heinrich, and Heinrich and Chwistek in a series of articles extending from 1896-1907.[22] In the article entitled: "Die Aufmerksamkeit und die Funktion der Sinnesorgane," Heinrich establishes the following principles which he considers of importance in explaining fluctuation. (1) When the attention is directed away from optical impressions (a) the lens takes the curvature characteristic of far seeing, and (b) the lines of sight tend towards the parallel position. The demonstration of these principles, however, cannot be considered as having any very direct bearing on the explanation of the phenomenon of fluctuation, for it may very well be conceived that the voluntary direction of attention away from visual impressions would cause changes in the accommodation and fixation of the eye of a magnitude that would be significant, while the involuntary lapses of attention occurring during the prolonged observation of a stimulus would not cause these changes at all. At least for the purpose of explanation of the phenomenon of fluctuation, the demonstration of the former cannot be considered the equivalent of the demonstration of the latter. And (2) when the attention is directed away from all optical impressions, involuntary changes take place in the curvature of the lens. This conclusion is based upon the recurrent changes that take place in the breadth of the pupil and upon the behaviour of images reflected from the anterior surface of the lens. In drawing this conclusion from the first point of evidence, Heinrich obviously assumes a closer connection between the changes in the breadth of the pupil and changes in accommodation than can safely be done. Any one who has studied the reactions of the pupil under a very wide range of conditions can not help but know that this 1:1 correlation does not exist. Moreover, the connection has not been found in a large enough percentage of cases to make it safe, even plausible, to assume that it exists in any situation in which it has not yet been demonstrated. In his second point of evidence, Heinrich does not describe the behaviour

[22] W. Heinrich: Die Aufmerksamkeit und die Funktion der Sinnesorgane, *Zeitschr. f. Psychol.*, XI., 1896, 410-431; Ueber das periodische Verschwinden kleiner Punkte, *ibid*, XLI., 1907, 59-74, und Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke, *Bulletin International de l'Academie des Sciences de Cracovie*, Nov., 1898, 363-382.

of the images observed. Apparently, however, the description is supplied in a later paper published in coöperation with Chwistek entitled "Ueber das periodische Verschwinden kleiner Punkte." At least a method of demonstrating changes in the curvature of the lens based on the behavior of the image reflected from its anterior surface is described here. But the validity of this demonstration is strongly open to question. In the experimental section of the present paper it will be shown that it is much more plausible to ascribe this behaviour to involuntary eye-movement than to involuntary changes in accommodation,—that in fact the same kind of behaviour has been described by de Schweinitz[23] and others, in case of the images reflected from the cornea, as one of the common phenomena of ophthalmometry due to eye-movement.

In the article entitled "Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke,"[24] Heinrich discusses the effect of variation of intensity, or differences in intensity between the stimulus and its background on the fluctuation of visual stimuli. Marbe[25] had found that an increase of intensity increases the phase of visibility, and conversely a decrease of intensity decreases the phase of visibility.[26]

This, Heinrich thinks, is just what should be expected were the disappearances caused by recurring lapses in the adjustment of the lens. As will be shown in the next section of the present paper, however, these results offer no differential evidence in favor of the accommodation theory. They are just what would be expected in terms of any theory that has yet been advanced to explain the fluctuation of minimal visual stimuli. Heinrich also notes that for one of Marbe's observers the

[23] G. E. de Schweinitz: *Diseases of the Eye,* Philadelphia and London, 1902, p. 739.

[24] *Op. cit.,* 366-369.

[25] Karl Marbe: Die Schwankungen der Gesichtsempfindungen, *Philosophische Studien,* VIII., 1893, 615-637.

[26] Marbe apparently was the first to make any separation of the phase of visibility from the phase of invisibility in drawing his conclusions, and even he did not take any account of the phase of invisibility in making his comparisons. The total times of visibility of his stimuli under the different conditions alone were compared. This tendency to break up the total period of fluctuation into its phases for purposes of comparison was a step in the right direction, but it was little heeded by his successors.

Marbe concludes that neither the fluctuation of the "Schrödersche Treppenfigur" nor the visual sensation is periodic. The phase of visibility of the visual stimulus increases with the increase in the difference in intensity between the stimulus and its background. The length of the period of fluctuation is a function of this increase.

phase of visibility was decreased when the image of the stimulus fell on the paraxial portions of the retina. This also, he says, is just what should be expected were the disappearance caused by changes in the curvature of the lens. So is it also what should be expected were adaptation the cause of the fluctuation, for one of the most conspicuous differences between the phenomena in the central and peripheral retina is the greater rapidity of adaptation in the peripheral retina.[27] Again, then, the evidence cannot be considered as differential. Heinrich also takes into consideration in this paper the results of Pace with the atropinized eye. He claims that changes in the curvature of the lens may still be observed when atropine has been used to paralyze the muscles of accommodation. While the present writer by no means contends that the muscles of accommodation are completely paralyzed by the use of atropine, still he would maintain that Heinrich's claim is strongly open to question if it is based on the kind of observation described by him and Chwistek in the article "Ueber das periodische Verschwinden kleiner Punkte." In any event the point is no longer of importance to the explanation of fluctuation, for it has been shown since that time by the present writer[28] that eyes from which the lenses have been removed and which under careful test shown no residual accommodation, get the fluctuation apparently just as readily as the normal eye.

In the article "Ueber das periodische Verschwinden kleiner Punkte," Heinrich and Chwistek, using stimuli of point area, attempt as their *experimentum crucis* to demonstrate the

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[27] In fact in the writer's own experiments, designed to show the correspondence between adaptation and fluctuation in the peripheral retina, the farther the stimulus was moved towards the periphery of the retina the shorter became the phases of visibility and the longer the phases of invisibility. These experiments were made differential for the adaptation theory (a) by the method of variation of areas, and (b) by showing a rough correspondence between the phase of visibility in the fluctuation experiments with the adaptation time for different visual stimuli from center to periphery of retina. No. 27 gray and the red, green, blue, and yellow of the Hering series of papers were used as stimuli. Only a partial list of the results obtained was published, however, because the writer did not at that time consider the phenomenon in indirect vision worthy of more space. The main arguments were established for direct vision and no more data were included for indirect vision that were needed to show in a general way that the phenomena here present no exception. (See "An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention," 116-119.)

[28] See An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, 84 and 94-96.

coincidence of fluctuation and changes in accommodation. Heinrich had mentioned the desirability of making this demonstration in the preceding paper, but had discarded the idea as infeasible because of the conditions under which the experiment would have to be conducted. In choosing to work with point stimuli in this study, Heinrich and Chwistek have admittedly selected the conditions most favorable to the accommodation theory, for by Heinrich's own statement in the explanation of his theory in an earlier paper[29] changes in accommodation would be more apt to cause the disappearance of point stimuli than of stimuli of larger area. Notwithstanding this admission, however, the results they get with point stimuli are advanced in the later paper as evidence that the fluctuation of stimuli of all areas is due to involuntary changes in accommodation. Their work with stimuli of point area will be taken up in detail in the next section of this paper.

III. The Fluctuation of Stimuli of Point Area (the Work of Heinrich and Chwistek)[30]

Heinrich and Chwistek maintain that the fluctuation of visual stimuli of point area is caused by periodic changes in the curvature of the crystalline lens. They also offer their results for stimuli of point area as evidence that the fluctuations of stimuli of all areas are caused by changes in accommodation. Four arguments are advanced by them in support of this conclusion. (1) Periodic changes in the curvature of the lens are directly observable. Moreover, these changes are found roughly to coincide with the fluctuations of the point stimulus when both observations are conducted at the same time. They describe two methods of demonstrating this change of curvature. One may be considered popular, the other technical. The popular demonstration may be conducted as follows. Prick two holes in a cardboard nearer together than the breadth of the pupil of the eye. Hold the card close to the eye and look through the holes at a bright light. The holes will be seen as two dispersion circles with a bright overlapping area. When the curvature of the lens changes, the overlapping area alternately contracts and expands. That is, as the lens becomes more convex, the dispersion circles become smaller and the overlapping area becomes narrower; and conversely, as the lens becomes less convex the circles become

[29] See Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke, 366-67.

[30] W. Heinrich and F. Chwistek: *Zeitschr. f. Psychol.*, XLI., Abt. 2, 1907, 59-73.

larger and the overlapping area becomes broader. This change in the overlapping area, they say, can readily be observed. Their technical demonstration was accomplished by means of an ophthalmometer. Their method was as follows. Two spots of light were thrown on the eye of the observer by means of two mirrors reflecting the light from a lamp properly placed with reference to these mirrors and to the eye of the observer. These images were observed by means of an ophthalmometer. Their description of method is extremely meager. They say: "Lässt man die beobachtete Person den Punkt fixieren, dessen periodisches Verschwinden untersucht wird, und dreht die Glasplatten des Ophthalmometers, bis man in dem Instrument die beiden von der vorderen Linsenfläche reflektierten Bildchen als drei Punkte sieht, so offenbart sich jede Krümmungsänderung der Linse dadurch, dass der mittlere Punkt bei grösseren Änderungen sich spaltet, bei kleiner breiter wird. Man beobachtet dann ohne weiteres, dass die Linseneinstellung nicht stabil ist, sondern dass sie kleinen periodischen Aenderungen unterliegt. Diese Aenderung konnte mit unserem Instrument durch die Drehung der Platten um höchstens 0.5° kompensiert werden. Es war uns unmöglich die Aenderungsrichtung aus den Bewegungen des Punktes zu erkennen."[31] While these changes in the image reflected from the observer's eye were being recorded by a second person, the observer himself recorded the fluctuations of a point stimulus. Simultaneous records were thus obtained which could be compared in order to determine whether the phases of fluctuations coincided with the phases of changes in the image reflected from the eye. The point stimulus consisted of a small black point on a white ground or a small white point on a black ground, 0.1-0.3 mm. in diameter, observed at a distance of 70-150 cm. Two observers, Herr Sk. and Herr Zacz, were used. The eyes of both were normal, or emmetropic. Chwistek recorded the changes in the images reflected from the eye. Their results are stated as follows. For observer Sk., 776 phases were recorded. "Einseitige Notierung vom Herrn Sk., d. h. notiertes Verschwinden des Punktes ohne entsprechend notierte Akkommodationsschwankung ergab sich in 38 Fällen. Einseitige Notierung vom Herrn Chwistek, d. h. notierte Akkommodationsänderung ohne entsprechende Aufzeichnung des Verschwindens des Punktes fand man in 40 Fällen." For observer Zacz, 296 phases were recorded. "Einseitige Notierung vom Herrn Zacz in 31 Fällen. Einseitige Notierung vom Herrn Chwistek in 32 Fällen."

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[31] *Op cit.*, 60-61.

(2) Within the range of areas used by them an increase in the area of the stimulus was found to give longer phases of visibility and shorter phases of invisibility. And, conversely, a decrease in the area of the stimulus was found to give shorter phases of visibility and longer phases of invisibility. Points were observed ranging for one observer (emmetropic) from .2 mm.-.5 mm. in diameter, at distances ranging from 100 cm.-126.5 cm.; for another observer (2.5 myopic), .2 mm.-.5 mm. in diameter at distances ranging from 35 cm.-39 cm.; and for a third observer (4 D. myopic), .2-1.5 mm. at distances ranging from 15 cm.-28.5 cm.

(3) The phase of visibility was also found to vary with the intensity of the stimulus or with the brightness difference between the point and its surrounding field. The greater was this brightness difference, the longer the phase of visibility was found to be as compared with the phase of invisibility, and the less was this brightness difference, the shorter was the phase of invisibility.

(4) When the stimulus was placed just beyond the far point for an observer with myopic eye, it was found to become periodically more and less distinct. Also two points placed at this distance were found alternately to blur into one and to separate into two. Two observers were used in these experiments.

Before passing to his own experimental evidence that involuntary changes of accommodation are not an essential factor in the fluctuation of stimuli of point area, the writer has the following comments to make on the work of Heinrich and Chwistek. (1) In this work they have created for themselves a special problem, that is, they employed stimuli of point area and strongly supraliminal intensity. The fluctuation of such stimuli has never been ascribed to the fluctuation of attention. Historically considered, then, they are not working with the phenomenon to which they primarily make their conclusions apply; and, moreover, they have not in any way shown in a satisfactory manner the propriety of applying their conclusions to the phenomenon explained by Lange as due to the instability of attention. (2) Their popular demonstration of involuntary changes in the adjustment of the lens is strongly open to question. Employing 124 subjects, the writer has not been able to make it work in a single case in which care was taken to rule out extraneous factors which would themselves cause the phenomenon. For example, extreme care must be taken to hold the card steady. Any variation in the distance of the holes from the pupil of the eye will cause a variation in the breadth of the overlapping area. Especially must care be taken that the card does not touch the lid of the eye, for movements of the ball of the eye and more particularly of the lid change the distance of the card from the eye. These movements are often unnoticed unless the observer is especially looking for them, and are frequently of sufficient range to cause a change in the size of the dispersion circles. Without a doubt the phenomenon, when it has occurred, has been, so far as the writer's experience is concerned, an artifact due to the conditions under which the observations were made. (3) Their technical demonstration by means of the ophthalmometer is, in the writer's opinion, just as strongly open to question. The writer criticizes this demonstration, however, with reluctance because of the meagerness with which they have described their method of working and observations. The following points, however, may be noted. (a) Working as they did, two images should have been observed, one reflected from the cornea, the other from the anterior surface of the lens.[32] Both images should have been very much alike, with the exception that the one reflected from the cornea should have been larger and more distinct. Nothing is said in the article, however, that would give evidence to the reader that more than one image was observed, or that the image described was actually reflected from the lens. But even if it were granted that the image observed was reflected from the lens, it would signify little, for the phenomenon described by them could have been caused just as well by involuntary eye-movements as by changes in the curvature of the lens. That is when the eye is accommodated, the anterior surface of the lens is hyperbolic in shape and varies in curvature considerably from point to point. A movement of the eye would, therefore, cause the rays of light forming the image to be reflected successively from points at which the surface had a different curvature. Each difference in curvature would give a difference in the size of the image reflected. Eye-movement would, therefore, produce the same effect in the size of the image as changes in the convexity of the lens. That is, movements of greater range would correspond in effect to the changes in convexity of greater magnitude, and, conversely, movements of lesser range to the changes in convexity of lesser magnitude. In fact, the phenomenon they describe is one of common

[32] An image reflected from the posterior surface of the lens might also have been observed. But since this image is inverted and is besides very indistinct, it may be considered as having no bearing on the discussion.

observation in case of the corneal image, and in this case no attempt has been made to ascribe it to recurrent changes in the curvature. For example, de Schweinitz, in his treatise on the diseases of the eye, says:[33] "Nothing is more common than to see the images of the mires [the mires correspond to the lights used by Heinrich and Chwistek] separate and overlap so that the apparent curvature of the cornea seems to change while under observation. The changes are due to slight movements of the eye which bring different portions of the cornea into view." We know that there are many involuntary eye-movements per minute even with the best control of fixation that can be obtained.[34] It seems more plausible, therefore, to attribute the phenomenon observed by Heinrich and Chwistek to the involuntary eye-movements which we know occur in abundance, than to use it as a proof of a new phenomenon, namely, the involuntary changes in the curvature of the lens, even if it be granted that the image from the lens was observed. At least, it may be said that Heinrich and Chwistek were not warranted in concluding as they did, without having secured any differential evidence to bear out their conclusion or without even having considered eye-movement as a causal factor. (d) Since the corneal image is known also to double and overlap, a rare opportunity was given to Heinrich and Chwistek, in making these observations, to compare the behaviour of the corneal image with that of the image reflected from the lens, if that really were the image they observed, and to determine by the presence or absence of coincidence in the two sets of changes, whether the doubling and overlapping of the images reflected from the lens has the same or a different cause from the doubling and overlapping of the images reflected from the cornea. Had both images really been observed or had the characteristic doubling and overlapping of the corneal image even been known to Heinrich and Chwistek, one can hardly conceive that their conclusions would have been drawn without recourse to this means of determining whether or not both sets of changes should be ascribed to a common cause. In short, judging from their report as it stands; from the fact that the ophthalmometer as it is ordinarily constructed and used is intended only for the observation of the corneal images, and that such a phenomenon as they describe would

[33] G. E. de Schweinitz: *Diseases of the Eye,* Philadelphia and London, 1902, 739.

[34] See C. E. Ferree: An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, *Amer. Jour. Psychol.*, XVII., 1906, 113–115; also The Intermittence of Minimal Visual Sensations, *ibid.*, XIX., 1908, 83–112.

have been extremely difficult to observe in case of an image reflected from the lens; and from the fact that descriptions of similar behaviour on the part of the corneal image are given by other observers, the writer cannot help but think, without any wish to be hypercritical, that considerable grounds are given for suspecting that Heinrich and Chwistek have observed the doubling and overlapping of the corneal image which is commonly attributed by de Schweinitz and others to involuntary eye-movement.[35] Moreover, the crux of their argument is that they have actually observed a coincidence between the fluctuation of the visual stimulus and the changes in the adjustment of the lens. This, they contend, gives a certainty to their argument not yet attained in previous work on the problem. But even if the question whether or not it was a lens image that was observed be disregarded, it will be seen from the above discussion that is strongly probable that the coincidence they actually observed was between eye-movement and the fluctuation of the visual stimulus and not between changes in the curvature of the lens and the fluctuation of the visual stimulus.

(4) Their explanation of the effect of variation of area on the fluctuation of a visual stimulus could apply only to stimuli of very small area. Moreover, even in the case of very small areas the effect they got is just what might be expected as the result of increase of area either in terms of Loria's explanation of the fluctuation of stimuli of point area[36] or in terms of the writer's explanation: adaptation interfered with by eye-movement. They make two cases of their explanation of how changes of accommodation cause the fluctuation of stimuli of point area: (a) when the stimulus is a black point in a white ground and (b) when it is a white point in a black ground. In the former case the rays of light coming from the margin of the black point are not sharply imaged on the retina when the lens changes focus, hence they spread over the dark space on the retina corresponding to the black point. It is obvious that this spreading of the marginal light could blot out the dark space only in case the black stimulus were of very small area. Hence the explanation could not apply at all to stimuli of the size ordinarily used in the work on fluctuation. In the latter case the rays of light coming from the white point are not sharply imaged when the accommodation

[35] The writer leaves himself willingly open to correction on this point, however.

[36] See Heinrich and Chwistek: *op. cit.*, p. 60; also Stanislaw Loria: *Untersuchung über das periphere Sehen, Zeitschr. f. Psychol.*, XL., 1905, 160-186.

changes, and are spread over the surrounding dark space. Since strongly supraliminal stimuli were used, it is extremely doubtful whether even very small stimuli could be carried below the limen of sensation from this cause. Moreover, because strongly supraliminal stimuli were used and no attempt was made to control the intensity of the stimulus, an increase in the area of the stimulus would function for sensation as an increase of intensity.[37] Therefore, from this cause alone, according to the theories advanced either by Loria or by the writer, an increase of area would produce an increase in the phase of visibility. Even in case of stimuli of point area, then, the effect of increase of area described by Heinrich and Chwistek offers no differential argument in favor of the explanation advanced by them. Furthermore, the theory of fluctuation of attention was meant to apply only to stimuli of liminal or approximately liminal intensity. When such stimuli are used, an increase of area produces just the opposite effect. For example, working in 1906 with liminal stimuli ranging in area from .5 x .5 cm. to 15 x 15 cm., the writer found that an increase of area caused a decrease in the phase of visibility and a corresponding increase in the phase in invisibility. And in the experimental section of this paper it will be shown that the same effect is produced in case of liminal stimuli of very small area. In both of these cases care was taken to keep the stimuli liminal in order that an increase in the area of the stimulus would not produce an increase in the intensity of the sensation. (4) The fourth argument advanced by Heinrich and Chwistek has no differential value whatever. It was first used by Heinrich in 1898, as applied to stimuli of larger area.[38] A more intensive stimulus, he thinks, is not so liable to be blotted out by involuntary changes in accommodation. Therefore, he concludes, the more intensive is the stimulus the longer should be the phases of visibility and the shorter the phases of invisibility. It is obvious, however, that this result is just what should be expected from adaptation as a causal factor. It should be expected even were it held that fluctuation is due to instability of attention. In fact an increase in the phase of visibility and a decrease in the phase of invisibility would be the natural consequence of an increase in the

[37] We seem to have here a violation of one of the most fundamental principles in experimental procedure, namely, when it is wanted to determine the effect of a given factor, the effect of all other factors should, if possible, be eliminated from the results of the experiment.

[38] W. Heinrich: Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke, *Bulletin International de l'Academie des Sciences de Cracovie,* Nov., 1898, 363-382.

intensity of the stimulus in terms of any theory that has yet been advanced to explain fluctuation.

(5) The writer is in some doubt as to what is meant by the fifth argument. "Befindet sich der Punkt, dessen Verschwinden man beobachtet, innerhalb des Akkommodationsbereiches der Linse, so beobachtet man nur das periodische Verschwinden desselben. Die Verhältnisse sind komplizierter, wenn man den Punkt ausserhalb des Fernpunktes aufstellt was beim myopischen Auge leicht ausführbar ist. In diesem Falle zeigt sich, dass der beobachtete Punkt, der jetzt nicht scharf gesehen wird, periodisch verschwindet, aber auch periodisch schärfer gesehen wird."[39] In the first place he cannot understand why the above result should be expected, were changes in accommodation present, for when the far point is actually reached the ciliary system should be completely relaxed. It is difficult then to see how the lens can be allowed to become any flatter, unless indeed it be held that the theory of accommodation commonly accepted for the human eye is incorrect. And in the second place, working under the conditions described by Heinrich and Chwistek, the writer has been unable to get anything that might be called three distinct and separate stages of clearness of his stimulus. Moreover, any stimulus of supraliminal intensity, fluctuating from any cause whatsoever and especially from causes purely retinal, would be apt to have, although not sharply defined, maximum, minimum, and intermediate degrees of distinctness. This the writer's observers were able to get at whatever distance the stimulus was put from the eye, but they were utterly unable to detect the three distinct and separate stages that are reported by Heinrich and Chwistek. Nor were they ever able to see the stimulus as clearly beyond the far point as they were at the far point or nearer than the far point. In short, there was never at this point what could be considered a norm of clearness which was succeeded either periodically or even at irregular intervals by a degree of clearness in excess of this norm. Continuing, Heinrich and Chwistek say: "Das lässt sich am besten durch folgendes Experiment illustrieren: Stellt man nicht weit ausserhalb des Fernpunktes des myopischen Auges als Objekt zwei Punkte, die so nahe liegen dass sie als ein Fleck gesehen werden, so beobachtet man, dass die Punkte periodisch auf kurze Zeiten getrennt erscheinen." The writer has not succeeded in getting this phenomenon when working beyond the far point with the myopic eye. It is, however, of common occurrence for any eye when the points are placed

[39] *Op. cit.*, 66.

at or slightly nearer than the limit of clear vision for these points and are regarded for any length of time. The points alternately blur into one and separate into two. In all probability both retinal and accommodation factors are involved in this result, but no definite estimate can be made of how much importance should be assigned to either until comparative records be made for subjects without lenses and for normal subjects. In the writer's opinion, however, the above experiment comes the nearest of any yet described by Heinrich and Chwistek to giving tangible evidence that involuntary changes in accommodation occur. But even to demonstrate clearly that these changes occur, would not prove that they are essential or even important factors in the fluctuation of minimal visual stimuli even of point area.[40] That they are not essential factors will be shown by the writer in the next section of this paper.

IV. Experimental

In this section of our paper we propose to show (1) that involuntary changes in accommodation are not essential or even important factors in the fluctuation of minimal visual stimuli of point area, and (2) that, identified by tests used by the writer in his earlier experiments, these fluctuations correspond just as closely to adaptation phenomena as they do for stimuli of larger area.

Probably the most convincing proof that one can offer that involuntary changes of accommodation are not essential to the fluctuation of stimuli of point area is the results obtained from aphakial subjects. Observations were made by the writer upon four aphakial subjects. They were all above sixty years of age, and three were above seventy. All of them had had the lenses removed from their eyes from 15-20 years before. Both the advanced age of the subjects and the long period that had elapsed since their lenses were removed favored the absence of any residual accommodation. To make sure of this point, however, they were each tested as follows. The subject's head was clamped in a head-rest and a card bearing letters of very fine print (3½ point type) was slid along a meter rod supported at the level of his eyes in the

[40] Lest it be thought that this experiment shows some coincidence between changes in accommodation and fluctuation, it may be pointed out that the cycle of changes experienced by the two points does not even include disappearance. The points merely blur into one and separate into two. That is, the only phenomenon cited by Heinrich and Chwistek that really gives any tangible evidence of involuntary changes in accommodation does not even occur in a series in which fluctuations are found.

median plane. The card was placed at his point of clearest vision as determined by the focus of his glasses and was moved both nearer and farther until just noticeable dimming took place. Every precaution was taken to secure accuracy. For one of the subjects the card could not be moved more than 2 mm. from the point of clearest vision without becoming less distinct. Very little more movement was required for any of the subjects. It may be safely said that all were practically without accommodation. Two of these observers were the same as were used by the writer in the earlier investigations made with stimuli of larger area. Opportunity was thus had to determine whether or not changes in accommodation play a more important rôle in the fluctuation of stimuli of point area than of stimuli of large area. So far as could be told from the records in both cases, they do not play a more important rôle. In cases of stimuli both of large and of very small area, the fluctuations occur for the aphakial subject with apparently no greater variation from the normal subject than is found from individual to individual with normal eyes.

Of the methods used in the former work to demonstrate that fluctuation is a phenomenon of the adaptation of the sense organ, only three were available for stimuli of point area. In the first of these the stimuli were made of different colors. Speaking of this method in the first paper[41] of the former series, the writer says, "Colors and grays were found to have an order of fluctuation times corresponding to their adaptation times. Four colors, red, green, blue, and yellow, gave very different fluctuation periods as compared with each other and with No. 27 Hering gray. The visibility times obtained were in the following order: red, green, blue, and yellow, the yellow being nearly four times as long as the red.

"The complete adaptation times for sheets of the same colors were found to have the same order of length and a rough correspondence as to ratio of length. Further, a striking fact came out with regard to the phases of invisibility. Since red, for example, has a shorter phase of visibility than green, one might naturally expect that its phase of invisibility would also be shorter than the phase of invisibility of green. The reverse, however, is true. Red has a longer invisibility than green, and this peculiarity is especially marked if one considers the proportionality between the phases, *i. e.*, the ratio invisibility: visibility. The same thing is true of the complementaries blue

[41] An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, p. 86.

and yellow. Clearly, we cannot look for a central explanation of this peculiarity; but it seems just what we might expect of adaptation from the standpoint of the compensation theory. The recovery process for the red is the green process. The green process is longer and seemingly more tenacious than the red, as is shown by the adaptation experiments proper, and is further borne out by the longer duration of the green after-image. A similar relation obtains in the blue-yellow process." In the earlier work, the stimuli were gotten as follows. Squares of the color of the size that was wanted were pasted on a gray of the brightness of the color. The stimulus was rendered liminal by letting the light pass from the colored paper through a sheet of milk glass, matt on one side, placed at such a distance from the color as to render its intensity liminal. The intensity was easily regulated by slight changes in the distance of this glass from the colored paper. The light reflected from the colored papers could not be used, however, for stimuli of point area, because the milk glass mentioned above had to be used to reduce the intensity of the stimulus and it was impossible to get this glass thin enough to give noticeable color with stimuli of point area. Light was transmitted through color-filters instead. The stimulus was gotten as follows. A hole was pricked through a gray cardboard with a fine needle and covered with one or more layers of colored gelatin. In front of the card, in contact with it, was placed the sheet of milk glass, matt on one side. The hole was illuminated by a row of lights placed behind the cardboard, normal to its surface, at a distance sufficient to render the stimulus liminal. By this arrangement a just noticeable point of color was presented to the observer seated in front.

A stimulus given by reflected light has always yielded more differential results in former experiments with the method of colors than a stimulus by transmitted light. This is probably due to the fact that we were able to get from the former type of stimulus more color in proportion to the white light present, thus better bringing out the color differences in the liminal stimuli. The poorer method had to be used, however, because as stated above milk glass with one surface matt could not be obtained thin enough so that a point of colored paper pasted upon a background of equal brightness could be seen through it.[42]

[42] In case of the colored papers the liminal stimulus and surrounding field were of the same brightness, because the paper giving the stimulus was pasted on a gray of the brightness of the color. The only effect of the milk glass in front was to change the general scale of brightness of color and surrounding field. No brightness inequality was produced.

The registration of results was secured by means of a Ludwig-Baltzar kymograph, a telegraph key and an electro-magnetic recorder, a Jaquet chronograph (set to seconds), and a lamp rheostat to cut down the current from the lighting circuit. All of this apparatus was screened from the observer by means of a sliding curtain. The work was done in a long room with the windows all at one end. Thus cross lights, unequal illumination of the background, etc., could be avoided. The illumination of the room was kept fairly constant by means of thin curtains covering the windows.[43] The observer sat with his back to a high window and his head in a head-rest fastened to the edge of a long table, along which the frame bearing the stimulation apparatus was moved as required. The time used throughout was 1 sec. The following results were obtained. As in the earlier work with stimuli of a larger area, red showed a shorter phase of visibility and a longer phase of invisibility than green; and blue, a shorter phase of visibility and a longer phase of invisibility than yellow. In spite of the poorer method we were required to use, the results obtained were almost as strongly marked as they were when the same method was used with stimuli of a larger area. These results have been verified at the time this work was done and since by a large number of observers practiced and unpracticed. The results of three observers chosen as typical will be reported here. Tables I-III have been compiled from these results.

TABLE I

Obs. C.—Fluctuation with stimuli of the four principal colors of point area showing that the phases of visibility and invisibility have the characteristic adaptation and recovery peculiarities of these colors just as they have with stimuli of larger area.

Stimulus	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red............	4.36	.79	1.68	.65	2.585	.385	6.04
Green...........	5.30	.93	1.13	.42	4.690	.213	6.43
Blue............	7.75	1.12	1.41	.71	5.496	.181	9.16
Yellow..........	13.10	1.76	1.32	.59	9.925	.100	14.42

In the case of the stimulus by transmitted light, this result was not so effectively secured because of the greater difficulty of equating the point of light and the surrounding field.

[43] To keep the illumination constant presupposes a means of measurement. At the time the writer had at his command no means of measuring the illumination of a room by daylight. For a method of

TABLE II

Obs. G.

Stimulus	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red	5.1	1.12	1.26	.54	4.047	.247	6.36
Green	7.43	1.54	1.14	.37	6.517	.153	8.57
Blue	10.9	1.59	1.46	.52	7.466	.133	12.36
Yellow	Did not fluctuate at all during period of observation.						

TABLE III

Obs. Ca.

Stimulus	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red	1.82	.59	1.62	.32	1.123	.890	3.44
Green	2.82	.53	1.27	.36	2.243	.450	4.09
Blue	4.15	.55	1.70	.35	2.441	.409	5.85
Yellow	5.22	.86	1.52	.51	3.434	.291	6.74

In the second test strips of colored paper of the breadth of a point and 5 cm. in length were used.[44] They were pasted on a gray background of the brightness of the color in each case and were observed as liminal color on the matt surface of the milk glass placed in front. They were arranged first with their longer dimension in the vertical plane, then in the horizontal plane. The former arrangement favored a maximal disturbance of adaptation for observers having the greater range and frequency of eye-movement in the horizontal plane, and gave with these observers in the fluctuation experiments a corresponding increase in the phase of visibility and decrease in the phase of invisibility. Conversely, the latter arrangement favored a minimal disturbance of adaptation for these observers and gave a corresponding decrease in the phase of visibility and increase in the phase of invisibility.

doing this, see C. E. Ferree and Gertrude Rand: An Optics-Room and a Method of Standardizing Its Illumination, *Psychol., Rev.*, XIX., 1912, 364-373.

[44] In this test we were able to use colored paper because strips, although only of the breadth of a point, could be seen when 5 cm. long through the sheet of milk glass we used.

For each observer careful records were made of the frequency and range of movement and the total time the eyes were moving according to the methods described in the former papers.[45]

Speaking of this test in the first paper of the former series, the writer says, pp. 84-90, "A more direct experimental confirmation than was afforded by the method of variation of areas of this view that eye-movement interferes with the course of adaptation and is also the conditioning factor for the wide range of variability found in the phases of visibility and invisibility in the fluctuation experiments, is given by the following results. An examination of average frequency of eye-movement in the horizontal and vertical planes during fixation showed that three of our observers had a marked excess in both frequency and range in the horizontal, while the fourth had an excess of frequency in the vertical, but of range in the horizontal plane. This appeared to mean that for three observers, there was greater change of stimulation, and consequently greater relief for the adapted elements, in the horizontal than in the vertical direction; while the reverse was true, though probably to a lesser degree, for the fourth. To test this interpretation, stimuli longer than broad were used, *e. g.*, slips of paper 5 mm. x 40 mm. When these were placed with the longer dimension vertical, the shorter dimension would fall in the direction of greater unsteadiness of fixation for the three observers who had the excess of eye-movement in the horizontal plane. Consequently, a maximal interference with adaptation for these stimuli would be obtained, and one might expect an increase in the phase of visibility and a decrease in the phase of invisibility. On the other hand, if the longer dimension were placed in the horizontal and the shorter in the vertical plane, the minimal interference possible for these stimuli would be secured, and a decrease in the phase of visibility and an increase in the phase of invisibility should ensue. For the fourth observer with the stimulus arranged as described above, the reverse should be true, but probably not in so marked a degree, since his range was greater in the horizontal, which fact to a certain extent would counteract the effect of frequency. . . . . That these methods of arrangements of stimulus caused a marked change in the phases of visibility and invisibility for each

[45] See An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, 113-115; and The Intermittence of Minimal Visual Sensation, 84-87.

observer will be seen by inspecting the Tables. Indeed the correspondence between the quantities: $\dfrac{\textit{Visibility} \div \textit{invisibility}}{\textit{visibility}^1 \div \textit{invisibility}^1}$, and $\dfrac{\textit{frequency}}{\textit{frequency}^1}$, is much closer than was anticipated."[46]

The results for the strips of point breadth are given in Tables IV-VI. For all the observers whose results are given in these tables, both the range and frequency of eye-movement were greater in the horizontal than in the vertical plane.

The third test was based upon the fact that the time required for a colored stimulus to adapt depends to some extent upon the surrounding field.

The question of what is meant by adaptation is logically raised here; among the followers of the Hering theory, it has come to mean, apparently, simultaneous induction, and Aall, reviewing the writer's first article,[47] assumes that that is what is meant by adaptation in that

TABLE IV

Obs. H.—Fluctuation with horizontal and vertical arrangement of the stimulus. Showing how arrangements that favor maximal and minimal interference with adaptation affect the phases of visibility and invisibility. Stimulus 3 mm. x 50 mm.

Stimulus	Arrangement	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red....	Vertical....	2.95	.64	1.68	.31	1.756	.569	4.63
"	Horizontal..	1.08	.23	2.29	.40	.471	2.120	3.37
Green...	Vertical....	4.04	.72	1.45	.26	2.786	.358	5.49
"	Horizontal..	1.69	.38	1.76	.37	.960	1.041	3.45
Blue....	Vertical....	5.40	.86	2.03	.51	2.660	.376	7.43
"	Horizontal..	2.51	.49	3.10	.46	.806	1.235	5.61
Yellow..	Vertical....	6.99	.97	1.45	.29	4.82	.207	8.44
"	Horizontal..	3.55	.72	2.10	.56	1.928	1.690	5.65

[46] For a more complete understanding why arranging the shorter dimension of the stimulus in the direction of the greatest eye-movement causes relatively long phases of visibility and short phases of invisibility; and conversely arranging the longer dimension of the stimulus in the direction of greatest eye-movement causes relatively short phases of visibility and relatively long phases of invisibility, see The Intermittence of Minimal Visual Sensations, 112-129; and The Streaming Phenomenon, 484-494.

[47] *Zeitschr. f. Psychol.*, XLIII., *Abt.* 2, 1906, 456-457.

TABLE V

OBS. R.

Stimulus	Arrangement	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red....	Vertical....	3.60	.75	1.46	.24	2.548	.405	5.06
"	Horizontal..	1.14	.28	3.40	.75	.335	2.982	4.54
Green...	Vertical....	4.30	.62	1.39	.19	3.093	.323	5.69
"	Horizontal..	2.42	.47	2.36	.37	1.025	.975	4.78
Blue....	Vertical....	4.93	1.08	1.68	.34	2.933	.340	6.61
"	Horizontal..	2.95	.54	4.53	.89	.651	1.535	7.48
Yellow..	Vertical....	6.61	1.23	1.46	.31	4.527	.220	8.07
"	Horizontal..	3.26	.79	2.75	.49	1.189	.843	6.01

TABLE VI

OBS. G.

Stimulus	Arrangement	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Hering gray No. 27	Vertical....	8.48	1.32	2.03	.39	4.177	.239	10.51
"	Horizontal..	3.14	.71	3.21	.56	.978	1.022	6.35
Red....	Vertical....	5.09	1.12	1.84	.29	2.766	.361	6.93
"	Horizontal..	1.65	.38	3.61	.78	.457	2.187	5.26
Green...	Vertical....	7.42	1.29	1.94	.41	3.824	.261	9.36
"	Horizontal..	2.52	.48	2.98	.48	.845	1.182	5.50
Blue....	Vertical....	8.99	1.47	2.84	.39	3.165	.315	11.83
"	Horizontal..	3.88	.84	5.99	.97	.647	1.542	9.87
Yellow..	Vertical....	11.49	1.97	1.90	.32	6.047	.165	13.39
"	Horizontal..	4.84	.97	3.86	.89	1.254	.797	8.70

article. The writer, however, by no means believes that the tendency of a color to lose its saturation on prolonged exposure to the eye or of all grays to become mid-gray is due entirely or even to any considerable extent to simultaneous induction. He grants an influence to the surrounding field when there is a surrounding field, and is at present making a quantitative study of that influence, but it is obvious that the influence of the surrounding field can have no part in the phenomenon called general adaptation, for in that case the whole retina is stimulated by the same kind of light. It can apply to local adaptation alone and even in local adaptation it cannot be considered a factor of primary importance. In 1838–1840 the loss of saturation experienced by a color on prolonged exposure to the eye was explained by Fechner[48] as due to the exhaustion or fatigue of the retinal elements. This explanation was adopted by Helmholtz, and became a feature of the Young-Helmholtz theory. Hering,[49] however, following a suggestion made by Godart, 1776,[50] and elaborated by Plateau, 1833-1835,[51] chose rather to consider the retina compensating in function. A compensating retina, it is obvious, should not exhaust. Hering bore himself out in this general position by claiming that the eye is ordinarily exposed to stimulation by white light from 15-18 hours during the course of a day, and yet at the end of that time it has not noticeably lost in its sensitivity to white light.

Hering himself apparently has not based his claim on experimental evidence. At least he neither offers results of his own nor quotes from the work of others. His conclusion seems to be drawn wholly from general observation. He says (Ueber Ermüdung und Erholung des Sehorgans, *Arch. f. Ophthal.,* XXXVII., 1891, (3), p. 2): "Anderseits ist es eine bekannte Thatsache, dass wir des Abends nicht merklich schlechter sehen als des Morgens und dass dies auch dann noch der Fall ist, wenn dem Tage eine in hellen Raumen durchwachte Nacht und ein neuer schlafloser Morgen folgt. Also einerseits fortwährende Ermüdung und zwar eine so schnell—vor sich gehende, dass schon nach einer wenige Secunde währenden—Fixierung eines weissen Objects auf dunklem Grunde sich die Folgen der "Ermüdung" durch ein deutliches negatives Nachbild verrathen, und anderseits trotz solcher fortwährenden raschen Ermüdung keine merkliche Beeinträchtigung des Lebens selbst bei tagelanger Belichtung der Netzhaut." He contends (p. 1) that according to the theory of fatigue, advocated by Helmholtz and Fick, this should not be. During an exposure of several hours to white light, the eye never has a chance completely to recover, hence should become from beginning to end of the period progressively more fatigued.

This conclusion is not at all in agreement with experimental results obtained by C. F. Müller (Versuche über den Verlauf der Netzhautermüdung. Diss. inaug., Zürich, 1866), for example, who from the results of his tests of the loss of sensitivity of the eye to white light from morning to night, concludes: "Am Abende erscheint der Retina irgend ein Object nur in 0.49 derjenigen Helligkeit, in welcher es ihr am Morgen erschienen wäre." Moreover, he found that the shape of the curve of fatigue undergoes a very decided change during the course of the day. Aubert also disagrees with Hering. He says

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[48] G. T. Fechner: *Pogg, Ann.,* XLIV., 1838, 221, 513; XLV., 1838, 227; L., 1840, 193, 427. The theory was conceived earlier by Scherffer (*Abhandlung von den zufälligen Farben,* Wein, 1765; also *Journal de Physique de Rozier,* XXVI., 175, 273), who explained the negative after-image by the conception that the retina is diminished in sensitivity by fatigue produced by previous stimulation.

[49] Ewald Hering: *Zur Theorie vom Lichtsinne,* 1874; *von Graefe's Archiv,* XXXVII., 1891, (3), 1, and 1892, XXXVIII., (2), 252.

[50] de Godart: *Journal de Physique de Rozier,* VIII., 1776, (1), 269.

[51] Plateau: *Ann. de Chimie et de Physique,* LIII., 1833, 386; LVII., 1835, 337; *Pogg. Ann.,* XXXII., 1834, 543. More fully in *Essai d'une théorie générale, etc. Mem. de l'Acad. de Belgique,* VIII., 1834.

(Moleschott's Untersuchungen, VIII., 1862, 251; see also Beiträge zur Physiologie der Netzhaut. *Abhandlungen der Schlesischen Gesellschaft,* Breslau, 1861, 39): "Es erscheint mir also aus obiger Bemerkung hervorzugehen, dass *im Laufe des Tages durch die Einwirkung des Lichtes die Empfindlichkeit unserer Retina fortwährend abnimmt,* so dass wir am Abende weniger empfindlich gegen Licht sind, als des Morgens." Moreover, without supporting evidence either from general observation or from experiments on color, in fact in complete disregard of this evidence, Hering, as he has done in many other cases in his work on the optics of color, has generalized with regard to the retina's response both to white light and to colored light from the results of observations with white light alone. For example, it is scarcely necessary to point out that the eye cannot be exposed from 15-18 hours to colored light without loss of sensitivity to color. Without dwelling further, however, on the evidence for and against a compensation theory, it will be sufficient for our purpose here to point out that if one were to hold to a compensation theory in the Hering sense, it would be necessary for him to seek some other explanation than exhaustion for the loss of sensitivity of the eye, apparent or real, to color or brightness. Hering apparently conceives that this happens only in case two surfaces of different quality are juxtaposed, and then all that takes place is that each is induced over the other and the qualitative difference between the two tends to disappear. There is, then, no real loss of sensitivity of the eye to either. Both become alike because by induction they are mixed to equality. The following objections may be offered to the explanation. (1) As stated before, it cannot apply to general adaptation. Yet it is well known that the eye loses its sensitivity to color when the whole retina is stimulated by that color, in fact more rapidly than when only a part is stimulated, except perhaps in case of certain combinations of color and surrounding field. (2) It can apply to local adaptation only in case the two fields juxtaposed both belong to the brightness series. For example, when the eye is exposed for some length of time to a white surface contiguous to a black or a light gray to a dark gray, the lighter surface is observed to darken and the darker to lighten. This might be explained by the mixture of the two qualities by induction. The evidence afforded by the observation, however, is not at all differential, for the phenomenon may be explained just as well by exhaustion. A different situation entirely is presented, however, when the two contiguous surfaces are colored. In this case there is very little in the phenomenon that could by the most favourable interpretation be construed as a mixture to an intermediate color quality. For example, when red and blue are stared at in juxtaposition, we should expect, in terms of Hering's explanation, both surfaces to become purple with no more loss of saturation than would be attendant upon distributing each color uniformly upon both surfaces. This, however, is not at all what takes place. The prominent effect is loss of saturation. The two surfaces tend to become alike for the most part only because both tend towards gray. The blue, it is true, does acquire a tinge of violet, but it does this as the result of adaptation even when red is not juxtaposed. It probably does become slightly more reddish by being alongside the red, but the evidence of induction is not great. The red, likewise, may be modified a little by being alongside the blue, but the effect is even less noticeable than it is for the blue. Similar results are gotten with green and yellow. In case the colors juxtaposed are complementary colors, the results of induction should be towards a cancellation to gray. But again the tendency towards gray which is actually observed affords no differential evidence for this theory of induction, because the shift towards gray can be explained just as easily in terms of the exhaustion theory. And that induction can have little to do with the phenomenon may be shown by the facts (1) that the tendency would have been towards gray had the whole retina been stimulated by one of the colors alone, and (2) that so far as can be told, the process is hastened little, if any, by the juxtaposition of the two colors. In the Lichtsinne, 1878, pp. 36-37 Hering describes the experiment upon which he bases his explanation of adaptation in terms of simultaneous induction. His device for stimulating the eye consists of a white and black surface juxtaposed. No attempt is made to extend the experiment to color. Moreover, in drawing his conclusions, no heed whatever is given to what would happen were the whole retina stimulated by light of one quality. This is a truly remarkable instance of a broad generalization made from a slender basis of fact.

The writer, then, does not wish it to be understood that he explains the fluctuation of minimal visual stimuli in terms of simultaneous induction. He has called this fluctuation a phenomenon of the adaptation and recovery of the sense organ, meaning by adaptation here, as in the original article, the progressive loss of sensitivity of the eye to colored and to colorless light caused by prolonged exposure. Just what the factors are in adaptation, will be made the subject of a further paper. They vary under different circumstances. In case of local adaptation, simultaneous induction is one of the factors, and in certain especial cases it may exert considerable influence, as is recognized in the test described above; but to make it the sole cause of the adaptation of the eye to its stimulus seems to the writer, in the face of the experimental evidence, to be little short of absurd.

In the earlier experiments it was found that by keeping the surrounding field constant and varying the stimulus, or conversely, by keeping the stimulus constant and varying the surrounding field, a difference in the period of fluctuation was obtained, showing itself chiefly in the phase of visibility. The same thing held in the recognized adaptation experiments. The variations in the phases of visibility and invisibility that were produced in the one, were produced in the other; the only departure from precise correspondence being that the differences were more marked in case of the recognized adaptation experiments, as would be expected from the longer duration of the process. The old series of Hering papers was used both in these experiments and in the experiments with stimuli of point area because combinations more favorable to rapid adaptation could be found in this series. Some of the combinations most favorable were the vermilion of the series on the blue-green, and the vermilion upon Hering gray No. 27; and some of the most unfavorable combinations were dark red on yellow, and dark blue on yellow. The combinations favoring rapid adaptation gave in the fluctuation experiments a short phase of visibility and a long phase of invisibility, and conversely, the combinations unfavorable to rapid adaptation gave long phases of visibility and short phases of invisibility. Although the writer had carefully determined in an earlier experiment with large areas which were the favorable and which the unfavorable combinations, still in order to make the correspondence between fluctuation and adaptation still more complete, in cases of stimuli of very small area both adaptation and fluctuation experiments were conducted in the present study. As was the case in the earlier experiments, the advantages of a stationary stimulus and surrounding field had to be sacrificed in these experiments, because the use of the milk glass to reduce the saturation of the stimulus, as was done when a stationary system was used, would also have reduced the saturation of the color in the surrounding field. This would not have been desirable for the purpose of the experiment. Accordingly, the Masson disc with the broken radius of point breadth was substituted for the stationary system. In case of the adaptation experiments, a point of color of full intensity was pasted upon the various backgrounds and observed at the proper distance. In conducting this adaptation series with stimuli of point area, we were not only getting the results needed for comparison in our fluctuation series, but by using stimuli of full intensity, we were applying our test under precisely the same conditions used by Heinrich in his fluctuation experiments. In both cases the effect of the favorable and unfavorable combinations was plainly marked in the results. For the results of these experiments see Tables VII-X.

TABLE VII

Obs. R.—Showing that combinations that influence adaptation time correspondingly influence fluctuation for stimuli of point area just as they do for stimuli of larger area. Fluctuation series, stimulus-ring 0.3 mm. broad and of liminal intensity.

Stimulus	Background	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red....	Blue.......	4.258	.81	1.930	.83	2.220	.450	6.215
Red....	Orange.....	6.041	.98	1.327	.59	4.552	.219	7.368
Red....	Yellow-green	6.10	.91	.933	.32	6.538	.152	7.033
Yellow..	Red........	9.166	1.10	1.125	.26	8.147	.122	10.291

TABLE VIII

Obs. B.

Stimulus	Background	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red....	Blue.......	3.30	.82	2.871	.88	1.149	.870	6.171
Red....	Orange.....	5.083	.94	2.416	.75	2.103	.475	7.499
Red....	Yellow-green	4.125	.59	1.540	.64	32.678	.373	5.665
Yellow..	Red........	6.230	.94	1.050	.32	5.933	.168	7.28

TABLE IX

Obs. R.—Showing that combinations that influence adaptation time correspondingly influence fluctuation for stimuli of point area just as they do for stimuli of larger area. Adaptation series, stimuli of point area and of full intensity.

Stimulus	Background	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red...	Blue.......	13.0	1.09	2.577	.76	5.044	.198	15.577
Red...	Orange.....	11.323	.90	1.112	.58	10.182	.098	12.435
Red...	Yellow-green	11.032	.87	.781	.32	14.122	.070	11.813
Yellow.	Red........	200.	No	fluctu	ation.			

TABLE X

Obs. B.

Stimulus	Background	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red...	Blue.......	5.761	.59	5.833	.654	.987	1.012	11.594
Red...	Orange.....	6.636	.98	4.723	.76	1.405	.711	11.359
Red...	Yellow-green	10.751	1.05	4.854	.97	2.215	.451	15.605
Yellow.	Red........	200.	No	fluctu	ation.			

V. Conclusion

In conclusion the following points may be reviewed. (1) The work offered by Heinrich and Chwistek in support of the accommodation theory for the fluctuation of stimuli of point area was done with stimuli of full intensity. In using stimuli of this intensity Heinrich and Chwistek have created for themselves a special problem. The doctrine of fluctuation of attention has never been applied to the fluctuation of stimuli strongly supraliminal in intensity. (2) Their strongest and most direct argument for the accommodation theory is their claim of having directly demonstrated a coincidence between involuntary changes of accommodation and fluctuation. The validity of this claim, however, rests primarily upon whether or not they have given a valid demonstration of the involuntary changes in accommodation. Their demonstration of involuntary changes in accommodation is strongly open to question. Employing 124 observers, the writer has been unable in a single case to make their popular demonstration work when care was taken to rule out extraneous factors which would themselves cause the phenomenon. And their technical demonstration with the ophthalmometer is in terms of a phenomenon which is described by de Schweinitz and others as one of the common phenomena of ophthalmometry due to eye-movement. The coincidence, then, which they claim to have observed between the fluctuation of stimuli of point area and changes in the curvature of the lens, is in all probability a coincidence between eye-movement and fluctuation. (3) Moreover, none of the evidence they have offered as indirectly proving the accommodation theory can be considered in any sense differential. All of it can be explained just as easily either in terms of the writer's adaptation theory or in terms of Loria's theory for the fluctuation of stimuli of point area. Some of it can even be explained in terms of any theory that has yet been advanced to account for the fluctuation of minimal visual stimuli. (4) The fluctuation of stimuli of point area presents no especial case. For (a) involuntary changes in accommodation are not an essential factor in these fluctuations. They take place for aphakial subjects apparently just as readily as for subjects with normal eyes. And (b) identified by the tests used by the writer in his earlier work these fluctuations correspond just as closely to adaptation phenomena as do the fluctuations of stimuli of larger area. (5) The fluctuation of minimal visual stimuli whether of large or small area is a phenomenon of the adaptation and recovery of the sense organ. And by adaptation is meant the progressive loss of sensitivity to colored and colorless light caused by prolonged exposure of the eye to these lights. It is not simultaneous induction. Simultaneous induction can be considered only as a minor factor in the adaptation of the eye to its stimulus.

THE CHARACTERISTIC FORM ASSUMED BY DREAMS

Eliott Park Frost, Yale University

It is as rare for a dream of any length to follow a single path or direction as for a train of waking thought to do so. A pulse of attention, fatigue, or a new moment of interest severally enters to shift conscious processes into new channels, and a similar shifting takes place in dreams. In this latter case, however, a study of the explicit phenomena of dreams has shown interesting peculiarities that call for further description. These phenomena appeared, somewhat as by-products, in the investigation of the implicit factors in dreams, conducted through a period of several months.

Attending to dreams aggravates them, fortunately for the experiment if not for the experimenter. The *Aufgabe* appears to exert an influence during sleep-hours, so that by attending for a long period to dreams one comes to be a light sleeper and a prolific dreamer. In time, literally *trains* of dreams come readily to mind and can be for the most part transcribed to paper in considerable detail. It is perhaps only under these conditions, when dream-material is abundant, that the true characteristic or type-form of the dream series appears, and may for this reason have escaped general notice.

Unless the conditions be unusual, as when some fixed idea monopolizes the dream,—a fatigue phenomenon,—or when the duration of the dream be actually brief, a dream is found to be constituted of several *dream-phases,* or *motifs,* distinct one from the other, in visual and other imagery, in characters and in situations, but linked together by some associative nexus, usually of an imaginal, but occasionally of an affective sort. Something from one dream persists into and ‘sets off’ the succeeding dream-phase. By means of this relational element, antecedent dreams are often recalled at a time considerably subsequent to the recall of the end-dream or dream at the waking moment. Or, again, chains of dreams are pictured at once *in situ* upon awaking. In any case there appear to be characteristic schemata to which the content conforms perforce, and which therefore must be considered in evaluating even these implicit (Freudian, and other) factors in dreams.

What these schemata are will appear if we take an actual dream-manifold and analyze it. Such an one is the following, taken from notes made at the moment of awaking (abbreviated):—

". . . . . standing on a side-hill with a friend. Note in distance an ENORMOUS AIR-SHIP approaching at FIERCE SPEED. It SHOOTS by overhead, circles, turns over and over gracefully . . . . . the movements GROW SLOWER and it comes to earth. I . . . . . make conversation with the pilot and passenger, the latter discussing the fine points of the air-craft with me. . . . . By this time the airship has become greatly REDUCED IN SIZE to the proportions of an ordinary MONOPLANE . . . . . the passenger sits in a LITTLE HAMMOCK in the rear near the propeller. . . . ."

There is now a shift of scene, with new surroundings, new characters:—

". . . . .Am beside a BROAD lake near a LARGE hotel watching the arrival of guests. In the foreground stand some PAINTERS' LADDERS on a pair of wheels, and I call out: 'Tell the carpenters to come for their ladders'. . . . . have a sense of saying something very strategic as though concealing the fact that these were not really ladders but something else (?). . . . . Now . . . . . go down to the water to greet arriving guests. . . . . The scenery SHRINKS . . . . . the guests become REDUCED IN NUMBER to three intimate friends whom I now undertake to introduce one to another . . . . . some names are forgotten and embarrassment ensues. . . . ."

Here, again, occurs a complete shift of scene.

"Am now in a GREAT FOREST, ENORMOUS TREES, CROWDS of gaily-dressed people, horses, etc. . . . . Seem to be directing the presentation of a pageant . . . . . movement of figures to and fro as I gesture and command. . . . . Gradually the figures grow SMALLER, the field as visually imaged is RESTRICTED . . . . . I now appear to be watching dolls perform on a cardboard stage in some ROOM or other."

Here is a dream manifold in three distinct parts. As a matter of fact the separate parts were recalled upon waking in reverse order, the last scene in the forest being the first recalled, then the lake-scene, and lastly, while the other two dreams were being put to paper, the first dream of the air-ship was brought back to mind. In this series of dreams at least four phenomena are significantly illustrated:

(1) A given dream-episode is launched abruptly, "full-blown." At its initiation all the elements of imagery are maximal; things then seem brightest, largest, widest, highest, loudest, quickest, etc. So in the dream appears the 'ENORMOUS AIR-SHIP,' 'FIERCE SPEED,' 'BROAD LAKE,' 'GREAT FOREST,' 'CROWDS OF PEOPLE,' etc., at the *beginning* of the respective episodes.

(2) Upon the appearance of such a congeries, a process of condensation begins at once. Things pale, grow small, are suppressed or disappear altogether, sometimes retaining a kind of form with loss of substance, as it were. Meaning and content are dimmed or changed. So, above, the 'MOVEMENTS GROW SLOWER,' 'AIR-SHIP REDUCED IN SIZE,' 'LITTLE HAMMOCK,' 'SCENERY SHRINKS,' 'GUESTS REDUCED IN NUMBER,' 'FIGURES GROW SMALLER,' 'PEOPLE BECOME DOLLS,' and a 'ROOM' takes the place of outdoors.

(3) As before suggested some one factor in a given episode persists, becoming the nucleus of the succeeding episode. The latter is abruptly initiated and incorporates the nucleus in a new complex. Thus the monoplane of (1) becomes the carpenters' ladders of (2), and the presenting of friends in (2) becomes the directing of a pageant in (3). In no two successive episodes of a dream-series was the second or later episode wholly independent of the episode preceding. However great the apparent gap between the two, some point of community could always be found.

(4) Finally in certain dreams imaginal elements, while still vivid, may play a rôle relatively subordinate to an affective element. In case a strong affective element, *e. g.,* FEAR, is present, a temporal summation of the dream-emotion may occur, persisting through successive episodes to culminate in arousing the sleeper. In such cases the affective element often itself becomes the link of connection between episodes, when perhaps no other traceable element can be found. This is relatively rare however.

* * * * * * * * *

Now is it possible to go beyond the mere description of these phenomena? Can they be explained? One or two suggestions may be adventured:

(1) There appear to be rhythms or phases in the continuous dream analogous to attention waves.

(2) These rhythms differ from the body-rhythms of breathing, heart-beat, etc., in the abruptness with which they are initiated. In this respect they are analogous to a new moment of attention, and suggest an 'explosion' of energy rather than a pendular wave.

(3) A residuum of energy from one phase acts as a stimulus in the release of energy for a succeeding phase.

(4) Finally, each phase may contribute an increment of energy to vaso-motor centres, where it does not find outlet at once but is gradually summated, until there is a sudden awaking, marked by a cry, start, forced breathing, perspiration, or similar physiological accompaniment; these motor phenomena serving as channels of drainage for the sympathetic system.

SUPPRESSION AND SUBSTITUTION AS A FACTOR IN SEX DIFFERENCES[1]

By M. E. Haggerty and E. J. Kempf

Among the factors which influence the rate and efficiency of mental association is the tendency toward suppression and substitution. That the strength of this tendency constitutes a distinct sex difference, being stronger in women than in men, is indicated by the results of tests which are here reported.

The subjects were 12 women and 16 men, students in a psychological laboratory. Each had had an elementary course in psychology and had worked a term and a half in the laboratory. They were, therefore, especially good subjects.

The tests used were selected from the Woodworth and Wells association tests. They fall into two groups. The first group included the two cancellation tests, the two naming tests, the substitution test and the two directions tests. All the tests were made under the supervision of the director of the laboratory. In each series of the first group, the first test was given to a student by the director. This student was then instructed how to perform the test and he gave it under supervision to the other 27 subjects. The logical relation tests, which constituted the second group which were given by Dr. Kempf included the opposites test, the verb object test, the action-agent test, the attribute-substance test, the subordinate concept test, the agent-action test, and a reverse opposites test. The latter was made up by selecting the true opposites of the Woodworth and Wells list of forty-opposites. This test was given by Miss Mitchell under Dr. Kempf's direction. In the logical relation tests each stimulus word was pronounced by the experimenter and the subject responded orally. The oral reaction word and reaction time were individually recorded.

In the logical relation series the time was taken with a stop watch. The same was true of the other series excepting where the time was long. In those cases an ordinary watch was used. The same method was used throughout each series so that within a given series each test had the same degree of accuracy.

[1] From the Psychological Laboratory of Indiana University.

The tests were first undertaken for practice work. Care was taken from the first, however, to see that all the conditions were thoroughly standardized so that the results would be reliable. When the first seven series were completed it seemed worth while to gather the results into a table separating the men and women into different groups. When this was done there appeared a distinct sex difference in favor of the women. This difference occurred not only as concerns the whole set of tests but it appeared in each separate series. The per cent of difference ranged from 8% in the Form naming test to 15% in the Cancellation of 2 test.

In discussing these results with the subjects it was suggested that the women might have made more errors than the men and thus increased their time. It was possible at the time to make an examination of the results of the cancellation tests and of the easy directions test only. In both of these there were more errors for the men. It does not seem probable therefore that the superiority of the women was due to careless work.

A second suggestion was that the greater length of time occupied by the men was due to wide individual variation. This would be individual rather than sex difference. In figuring the average for the whole group of tests it was found that the M.V. for the men was slightly greater than for the women, 13.5″ for the latter as against 16.5″ for the former. The number of wide variations, however, were about equally distributed between plus and minus variations so that we do not seem to find the explanation here.

The apparent sex differences might be attributable to the women being a more highly selected group than the men. In order to test this hypothesis the grades of all were obtained from the University office. All the grades in all the subjects for all the time the students were in college were considered. The average for the women was 86.96 and for the men 84.72. This difference in favor of the women thus correlates very closely with the difference shown in this series of tests. We may, therefore, conclude that the women did better in this series of tests because they were a better selected group.

Whatever may be the explanation of this apparent sex difference the fact that such a difference occurs serves to make conspicuous *the opposite results obtained in the logical relation tests. Here the rate of efficiency is in favor of the men and against the women.* This difference is made apparent by the two charts of curves.

In each of the group I tests the women use less time; in each of the logical relation tests they use more time. The reversal of superiority is so complete and so striking as to deserve consideration.

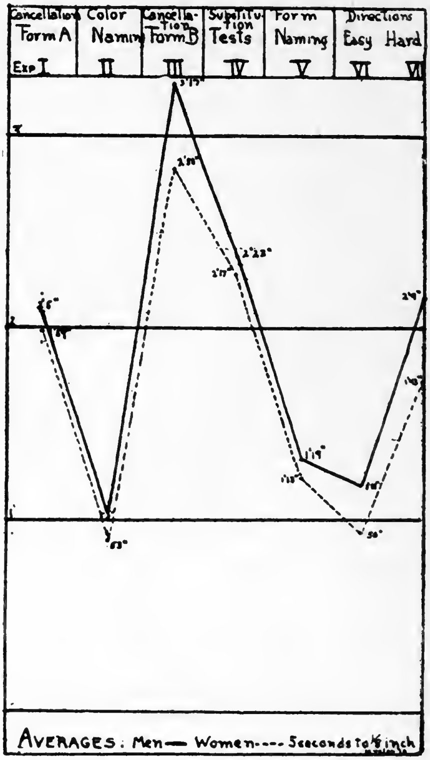

Figure I. Solid line, average of 16 men; broken line, average of 12 women. Distance on ordinates indicate time in seconds occupied in completing entire task. *Women excel men in each test.*

The factors which influence the rate of association are so numerous that variations in reaction rate are to be expected. These influences are found in these experiments but it is difficult to see why any one of them or all of them together would so operate as to produce the exact reversal of sex differences that appear. Thus a possible cause of the reversal might be the method of experimental procedure. This, however, was standardized for each test at the beginning and remained the same for that series throughout. Men and women were treated alike. The position at the table, the

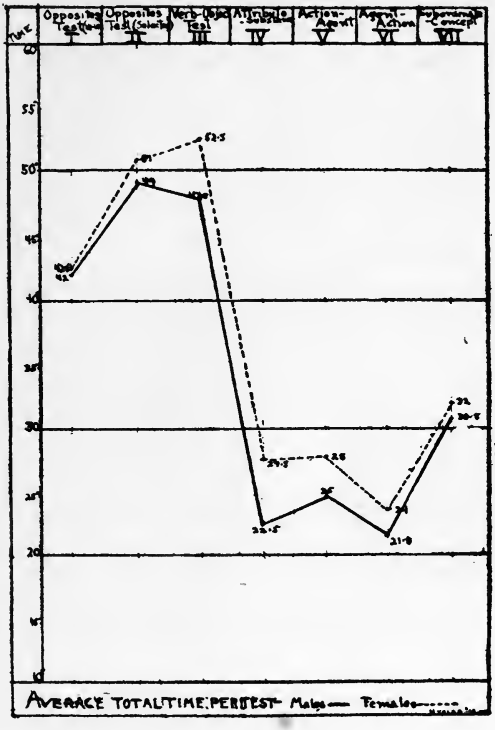

Figure II. Solid line, average of 16 men; broken line, average of 12 women. Distance on ordinates indicates time in seconds occupied in completing entire task. *Men excel women in each test.*

instructions at the beginning, the manner of presenting the stimulus, the recording of time remained the same for all subjects in a given test.

A second cause might have been the unfamiliarity of material. The subjects were mostly college juniors, seniors, and graduates. The simple material used in the tests must have had practically the same familiarity to all. This may have been slightly less true of the logical relations tests. Yet even here the associations called for are all so commonplace that the occasional unfamiliarity reported by one or more subjects could not have greatly altered the total results.

A third possible cause might have been the attitude of the subjects toward the experiment. It might have been that the women were more interested in the Group I tests and the men more alert in the logical relations test. There does not, however, seem any valid ground for assuming this in these tests. All the subjects were seriously minded in the experiments and understood from the beginning that it was important to do the work rapidly. While there may have been individual variations from test to test it seems quite improbable that either sex should have a change of attitude sufficient to account for the changed results.

A fourth possibility is that the personality of the experimenter might have affected the subjects differently and thus altered the rate of efficiency. It is to be noted, however, that in the Group I tests there were seven experimenters all producing the same results, and in the logical relations test there were two experimenters, one man and one woman, both securing like results and results diametrically opposed to the results of the first seven. In fact the reverse opposites test was devised and given to a woman to make the tests with the aim to see if the experimenter in the logical relations tests was the cause of the reversal. The results from this test, however, were in harmony with the results of Dr. Kempf. It, therefore, seems safer to look elsewhere for the cause of the difference.

If one turns from these general conditions, none of which seems adequate as an explanation of the differing results obtained from the two sets of tests, to an analysis of the results of the logical relations tests he finds as one outstanding fact the presence of confusion as evidenced by lengthened reaction times. This lengthening of reaction was frequently several times the normal average reaction. Thus one subject whose normal time was one second consumed 5.8″ in responding to the word *win*. In such a case one reaction was practically the equivalent of six and it became necessary in computing the normal average to devise a method for eliminating such unusual reactions. For such elimination the following rough method was adopted. First the average of the series was found. To this average was added twice the average varia-

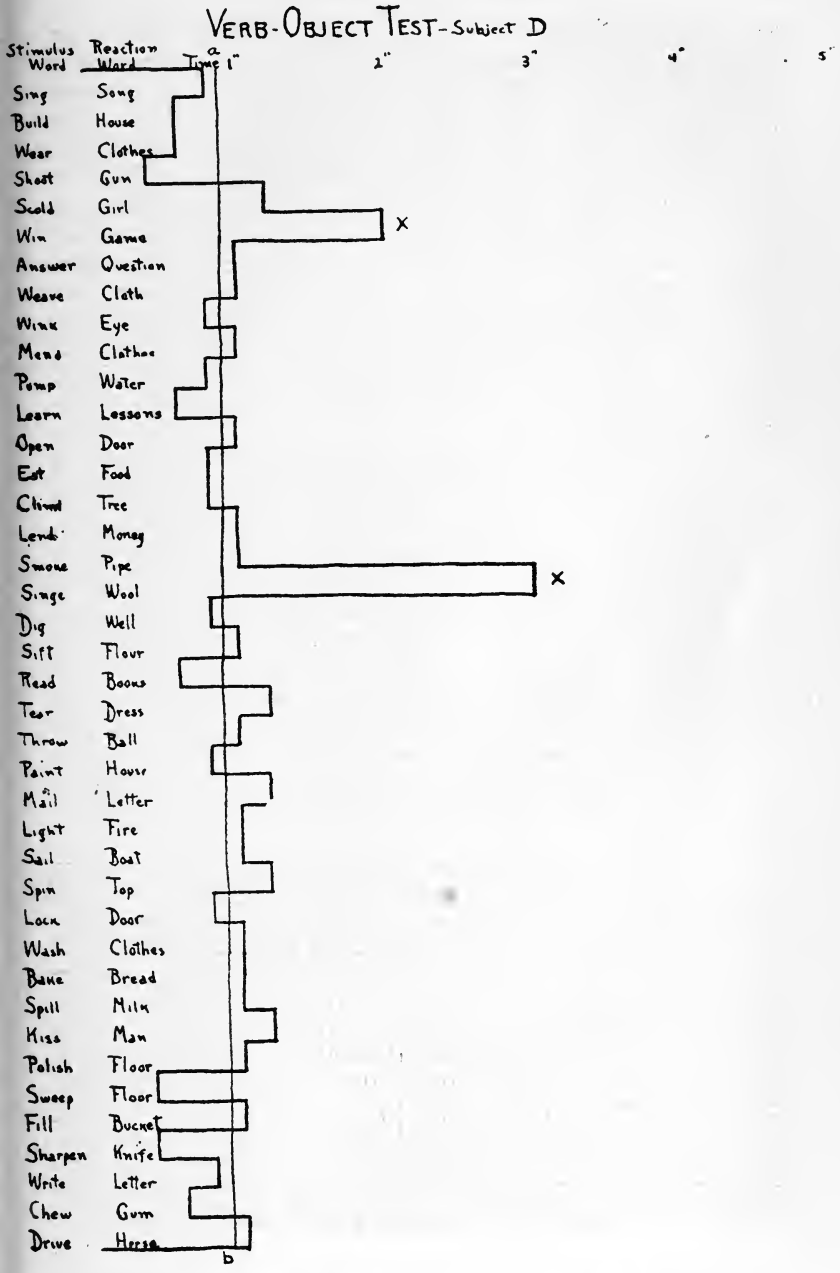

Figure III. A page showing method of recording reactions and the process of eliminating abnormal reactions. The line ab marks the average. The two reactions marked X were eliminated in making the second average which is the basis of the curve in figure II.

tion as determined by estimation.[2] All reactions exceeding this time were eliminated as abnormal. For the others a new average was found which was counted as the normal average for the individual in question. This average which is the basis of the curve on page 417 does not, therefore, involve those reactions which were highly costly in time.

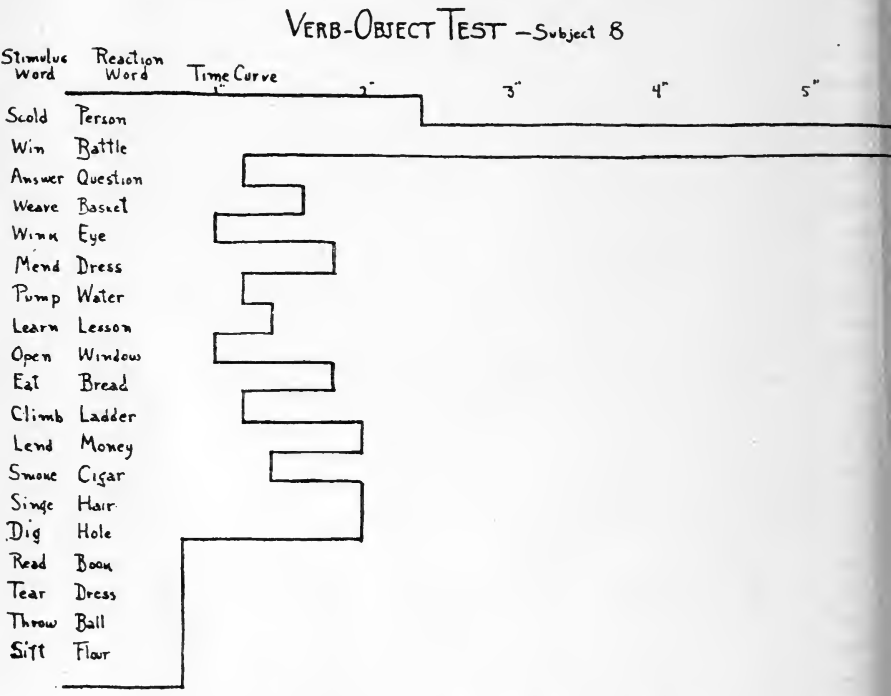

Figure IV. Verb-object test showing conflict, repression, substitution, overlapping or confusion, confession and return to normal reactions. The word *win* brought up a visual image of a valentine with a verse about "winning heart." The subject did not wish to say "heart" because it "sounded silly." "Heart" was therefore suppressed and "battle" was substituted. The effect of this confusion continued to some extent until the subject voluntarily "confessed" the foregoing substitution. The last four reactions illustrate an effectual readjustment.

[2] The records were taken on graphing paper. The length of reaction time was indicated by horizontal lines as shown in figure III, page 419. In eliminating the excessively long reactions a line representing the average time of all reactions was drawn from top to bottom of the chart. The experimenter then estimated the average

The elimination of the abnormal reaction, however, does not in certain cases eliminate the *influence of that particular reaction.* In the case of the word *win,* noted above, the lengthened reaction time is found to be due to a case of conflict followed by suppression and substitution. The subject, who was a young woman, explained the experience as follows: "The word win recalled a visual image of a valentine with a verse about winning heart. Heart sounded silly so I repressed it and substituted 'battle.'" This explanation, however, was not given until sometime after the reaction. Twelve other reactions intervened and the effect of suppression is shown in the disturbed reaction times which follow the conflict. After the twelfth reaction the subject volunteered the explanation given above. The explanation acted as a catharsis to relieve the conflict and a normal adjustment followed as appears from the reaction times of the associations which follow the explanation.

It is evident then that the elimination of the one exceedingly long reaction time does not eliminate the effect of conflict arising in the case of the particular reaction eliminated. A considerable number of the succeeding reaction-times were lengthened with the result that the average time for the whole series is greater than it would have been if the conflict had not arisen. A fair inference from this and similar cases where conflict occurred is that the presence of conflict tends to lengthen not only the total time of a series of reactions but also the average time of the individual reactions exclusive of the particular conflicting reaction in question.

It does not yet appear, however, why this phenomenon of lengthened reaction time due to conflict should make a distinct sex difference. If we turn to the number of conflicts which the two sexes show a reason is apparent. The number of conflicts for the women is greater than for the men in every test but two. For the seven tests the number of conflicts stand in the following ratio:

	Men	Women
Reverse opposites test	3.75	5.
Opposites test	6.25	8.33
Verb-object test	6.6	10.
Attribute substance test	1.2	6.6
Action-agent test	1.2	5.
Agent-action test	3.1	1.6
Subordinate-concept test	6.6	2.5

variation from this line and all reactions that were more than twice this variation were eliminated as abnormal. This method while crude is speedy and served to get rid of the widest variations. It is at the same time more accurate than direct judgment.

It will thus be seen that if the presence of conflict causes a lengthening of reaction time this would account for the women having a longer reaction time than the men in all but the last two tests.

Inasmuch, however, as these last two tests have fewer conflicts for the women than for the men and yet the times for the women are longer a further explanation is needed. This further explanation is found in the presence of less costly conflicts. It must not be supposed that when the long reaction times have been eliminated we have thereby gotten gotten rid of all the cases of conflict. It is perfectly clear from the records of individuals that there were numerous conflicts which did not come under our scheme of elimination. To discover these the following method was used. If a range of fluctuation covering one second is allowed between the lowest and the highest reaction times, it is found that the women tend to transgress this limit oftener than do the men. Thus in the verb-object test the twelve women transgressed the limits of reaction either below or above, almost always above, 64 times while the sixteen men exceeded the one second range only fifty times. This gives a ratio of 5.7 for the women to 3.1 for the men. The ratio of conflicts computed in this way for the other tests is as follows:

	Women	Men
Opposites test	3.5	1.8
Reverse opposites test	2.8	1.3
Subordinate concept test	3.1	1.9
Action-agent test	2.1	1.5
Agent-action test	1.8	.8
Attribute-substance test	1.7	1.2

The ratio for whole group of tests is 21 for the women to 11.6 for the men. Thus it is seen in every one of the logical relations tests the ratio of fluctuations is considerably greater for the women than for the men.

It thus appears that the reason the women fall behind in the logical relations tests is that they were more subject to confusions which produce wide fluctuations in reaction time, the fluctuation being usually in the direction of lengthening the time.

The data at hand do not, however, enable us to judge surely as to the cause of the confusions. It is not probable that it was of the interference type due to overlapping stimulation. This sort of confusion occurred in the tests of Group I especially in the naming tests and there is no reason to

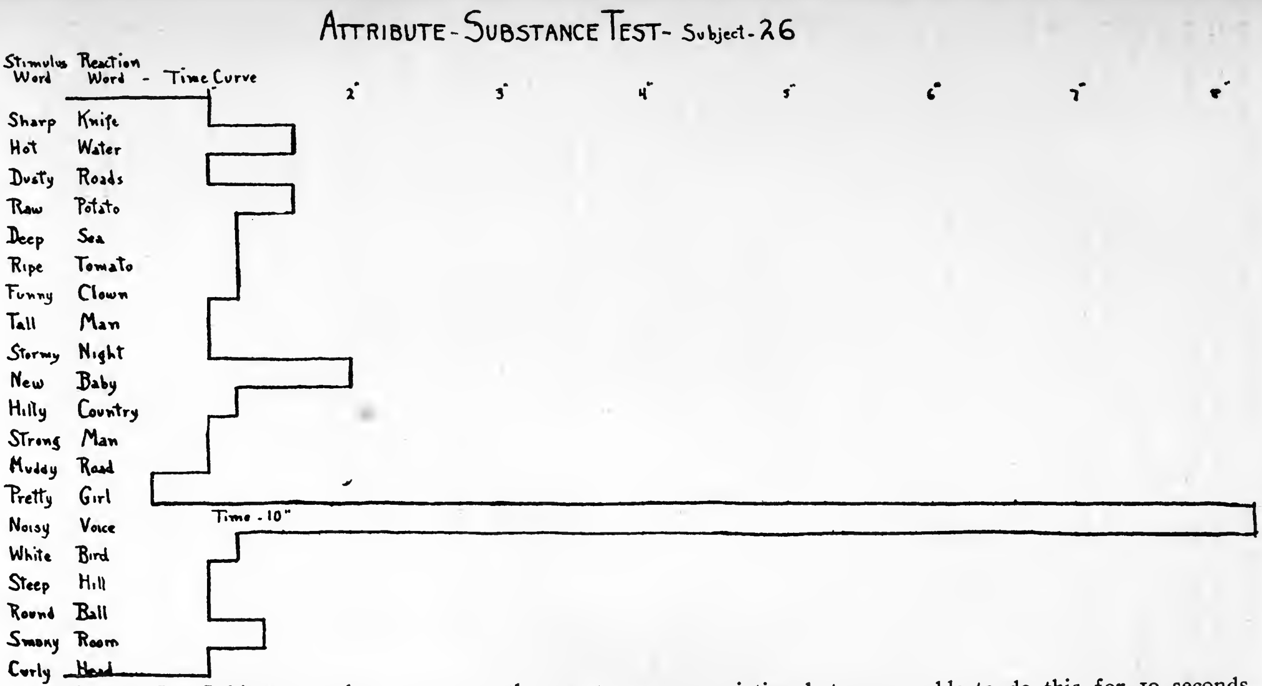

Figure V. Subject sought to suppress the spontaneous association but was unable to do this for 10 seconds when he laughed at his "ridiculous plight." He explained that the word "noisy" brought up a whole constellation of ideas, images, etc., connected with an incident of the previous day. His effort at suppression being unsuccessful, he laughed and the sound of his voice suggested the word "voice" which he spoke.

believe that there is any ground in this sort of confusion for sex differences.

In some of the cases of confusion where we have introspective data the lengthening of reaction is due to the suppression of spontaneous associations and the substitution of secondary associations. The instances in which we have these introspective data are not numerous but they are definite and occur among both sexes. Thus a young man responded "girl" to the word "pretty" in .6 seconds. To the next word "noisy" he failed to respond for ten seconds and then began to laugh. The sound of his own voice suggested the word "voice" which he spoke. He explained his hesitation as follows: on a previous day he had been giving an association test and used the word "kiss." The subject, who was a young woman, responded "noisy." This had amused him and when the experimenter in this test said "noisy" it brought back the whole constellation of ideas, images, etc., connected with the previous experience. He was unable to suppress the matter entirely and could make no substitution until he laughed at his own "ridiculous plight." This broke the cramped set of mind and he found a word.

If we could infer from such cases that all the confusions were due to conflict, suppression, and substitution we should have to explain why such conflicts were more numerous among the women than among the men. Here are a group of twelve persons whose associative mechanisms in one set of tests work with greater efficiency than those of another group of sixteen persons but who in a second set of tests find their association processes blocked, confused, diverted and inefficient. Analysis indicates that the blocking is due to inhibitions accompanied by emotional excitement. The inhibitions in turn seem due to the tendency of the individual to protect himself from embarrassment. The mind exercises a censorship over its overt expressions suppressing those felt to be inappropriate to the situation and selecting others. This process consumes time with the result that those persons in whom the tendency is strongest occupy the longer time and thus appear by the measure of the tests less efficient than those in whom the tendency is weak.[3]

[3] It should be pointed out that the tendency to be "on guard" would not only influence the length of time in the actual case of conflict but just because the tendency was there it would tend to lengthen the reaction time even though no conflict arose. A mind on guard against self-surprise and self-embarrassment is perforce a slower mind than one from which this tendency is absent or at a minimum.

To reason in the opposite direction, the women, inasmuch as they show the shortest average association time in those tests where no conflicts are possible and the longest average reaction time in those tests where conflict is likely to be frequent must possess the tendency toward suppression to a greater degree than do the men.

Our data, while allowing this as a probable inference, hardly warrant us in asserting it as a proved fact. There are some cases of lengthened reaction where there is no apparent effort to suppress. Thus one person responded "potato" to the word "raw" and remarked that he was fond of potatoes. To the word "new" he said "baby" and noted a recent event in his friend's family. To each of these associations there was a slight emotional tinge which might very well account for the delay of response. Such delays, however, are not likely to be so expensive in time as the cases of suppression and substitution.

Again it might be that differences in imagery might cause differences in reaction time, concrete imagery being more expensive than verbal. This might very well account for individual differences and if it could be shown that the women had more concrete imagery than the men this might have a bearing on the sex differences.

We happen to have the imagery studies made in the course of the class-work. These are not thoroughly accurate, but so far as they go they do not seem to show any sex differences in imagery such as would account for the differences shown in the tests.

However, the conclusions that the differences shown in these tests are due to the greater tendency of the women to be "on guard" against embarrassment can be regarded as tentative only. The fact if it can be shown to be a fact of general application is so important that further investigation along this line should be made. It must be said that the experimental study of adult sex differences has not as yet yielded any very significant results. There seems to be a general conviction that there is a feminine type of mind different from the masculine but in just what this difference consists we have as yet no measured experiments to prove. In view of this lack of scientific data on the matter it seems worth while to set forth the facts and suggestions contained in this paper.

IMPROVEMENT IN A PRACTICE EXPERIMENT UNDER SCHOOL CONDITIONS

By M. E. Donovan and Edward L. Thorndike, Teachers College, Columbia University

The importance of knowledge concerning the amount and rate of improvement due to practice under school conditions and concerning the effect of equal amounts of training upon the individual differences found amongst a given group in a given trait is so great that we venture to report a very slight contribution to it.

The experiment consisted in measuring the effect of approximately sixty minutes' practice (in approximately 30 periods of 2 minutes each, given twice daily for the five school days of the week) at adding columns, each of ten digits (0 and 1 not being used). The subjects were twenty-nine boys in a fourth-grade class in New York City.

The score used as a measure of efficiency was the number of examples done *correctly*. That is, no credit was given for an example containing any error. We are unable to report how much interest in the work and in improvement there was.

The group as a whole improved, as the result of the hour's practice, from an average score of 2¾ examples correctly done per minute in the first two periods, to a score of 4½ examples done correctly per minute in the last period. The results thus emphasize the very great gain probably to be expected from applying the method of the practice experiment to certain functions whose improvement is a part of the school curriculum. The individual amounts of improvement are shown in Table I.

If we compare the improvement of the eight boys who showed the least ability at the start (4, 4, 5, 6, 7, 7, 8 and 8 examples done correctly in four minutes) with the seven who showed the most ability at the start (21, 19, 16, 16, 15, 14 and 14 examples done correctly in four minutes,) we find that the latter made equal or greater gross gains (8 on the average to 7.6 for the less able group). What happens when individuals of different abilities are given equal practice in addition is shown still more clearly by Table I, which gives the average scores for: First, the four boys of initial ability 4, 5 or 6; Second, the four boys of initial ability 7 or 8; third, the four boys of initial ability 9; fourth, for the seven boys of initial ability 10, 11 or 12; fifth, for the three boys of initial ability 13; sixth, for the five boys of initial ability 14, 15 or 16; seventh, for the two boys of initial ability 19 and 21.

These results, showing so little power of equal additions to training to reduce individual differences, make it improbable that a very large fraction of the differences found among school children can be justly attributed to differences in amount of training. Since the argument on this point has been stated by Thorndike (*Amer. Jour. Psychol.*, xix, 1908, 383 f.), and by Wells (*Amer. Jour. Psychol.*, xxiii, 1912, 75-88), we will say no more about it. The results of the present study are in entire accord with the view presented by these authors.

TABLE I

SUCCESSIVE SCORES IN ADDITION: FOURTH-GRADE PUPILS

Individual	Number of periods practiced	First two	Second two	Third two	Fourth two	Fifth two	Sixth two	Seventh two	Eighth two	Ninth two	Tenth two	Eleventh two	Twelfth two	Thirteenth two	Fourteenth two	Fifteenth two	Sixteenth two	Gross gain,—First two to Last two
x	25	4	6	5	8	10	9	9	4	3	4	9	6	4				0
y	31	4	7	7	9	5	8	8	9	8	13	11	9	9	12	10	11	7
r	32	5	4	5	5	4	4	5	5	6	8	5	6	7	9	10	11	6
f	31	6	6	10	12	7	11	9	14	17	14	13	12	9	16	13	14	8
Average for x, y, r and f........		**4.8**	**5.8**	**6.8**	**8.5**	**6.5**	**8**	**7.8**	**8**	**8.5**	**9.8**	**9.5**	**8.3**	**7.3**	**?**	**?**	**?**	**5.3**
s	32	7	13	15	16	16	19	13	16	21	17	19	21	15	20	19	22	15
B	31	7	7	6	8	7	9	10	10	5	10	9	7	9	11	10	11	4
m	32	8	10	13	13	13	12	12	13	10	12	14	14	14	14	16	15	7
v	32	8	10	10	15	11	16	11	14	16	20	20	16	23	16	18	22	14
Average for s, B, m and v......		**7.5**	**10**	**11**	**13**	**11.8**	**14**	**11.5**	**13.3**	**13**	**14.8**	**15.5**	**14.5**	**15.3**	**15.3**	**15.8**	**17.5**	**10**
e	31	9	6	8	12	9	10	14	9	8	11	13	14	11	16	17	16	7
k	32	9	8	4	9	8	7	10	8	6	7	9	6	7	11	9	9	0
n	29	9	10	12	12	12	10	9	12	11	13	15	15	14	13	16		7
A	32	9	14	13	14	15	12	16	15	19	18	17	17	21	21	21	16	7
Average for e, k, n and A.......		**9**	**9.5**	**9.3**	**11.8**	**11**	**9.8**	**12.3**	**11**	**11**	**12.3**	**13.5**	**13**	**13.3**	**15.3**	**15.8**	**14** (app.)	**5.3**

TABLE I—*Continued*

Individuals	Number of periods practiced	First two	Second two	Third two	Fourth two	Fifth two	Sixth two	Seventh two	Eighth two	Ninth two	Tenth two	Eleventh two	Twelfth two	Thirteenth two	Fourteenth two	Fifteenth two	Sixteenth two	Gross gain,—First two to Last two
b	27	10	11	16	15	10	13	16	14	18	19	19	17	17	20			10
w	30	10	12	12	13	11	17	18	11	11	15	14	12	14	12	13		3
i	32	11	13	12	13	12	15	15	12	10	15	13	14	16	18	18	19	8
C	31	11	13	16	17	11	14	12	14	17	12	12	19	19	14	9	13	2
c	31	12	14	11	13	10	11	15	9	14	16	15	11	11	14	12	18	6
j	32	12	14	15	14	17	21	15	13	18	18	16	21	18	21	22	25	13
q	32	12	10	14	13	13	12	13	15	15	11	12	14	11	10	11	16	4
Average for b, w, i, C, c, j and q		**11.1**	**12.4**	**13.7**	**14**	**12**	**14.7**	**14.9**	**12.6**	**14.7**	**15.1**	**14.4**	**15.4**	**14.9**	**15.6**	**14.7** (app.)	**17.4** (app.)	**6.6**
t	32	13	10	10	13	14	16	11	15	10	9	14	13	14	15	15	13	0
u	32	13	10	16	17	14	16	19	18	20	19	20	20	21	27	23	25	12
z	32	13	11	18	13	17	13	19	21	15	19	16	20	20	17	25	26	13
Average for t, u and z		**13**	**10.3**	**14.7**	**14.3**	**15**	**15**	**16.3**	**18**	**15**	**15.7**	**16.7**	**17.7**	**18.3**	**19.7**	**21**	**21.3**	**8.3**
a	32	14	12	16	16	14	15	17	13	16	16	15	19	19	22	20	16	2
d	29	14	13	10	12	12	12	16	12	7	12	9	14	14	10	16		2
p	29	15	9	15	18	16	13	12	11	17	13	14	12	15	15	16		1
h	32	16	15	18	25	23	23	25	26	28	28	32	29	25	31	34	35	19
o	30	16	16	19	24	24	25	24	21	22	21	15	23	25	25	26		10
Average for a, d, p, h and o		**15**	**13**	**15.6**	**19**	**17.8**	**17.6**	**18.8**	**16.6**	**18**	**18**	**17**	**19.4**	**19.6**	**20.6**	**22.4**	**?**	**6.8**
g	28	19	21	20	23	21	24	22	26	27	29	31	32	32	26			7
l	32	21	24	29	27	27	27	25	31	26	25	28	29	28	28	30	36	15
Average of g and l		**20**	**22½**	**24½**	**25**	**24**	**25½**	**23½**	**28½**	**26½**	**27**	**29½**	**30½**	**30**	**27**	**?**	**?**	**11**

DISCUSSION

THE METHOD OF EXAMINATION

By E. B. Titchener

During the past few years we have published, from the Cornell Laboratory, a number of studies in which we sought to test the 'method of examination' or 'method of questions.' We owe the suggestion of this work, for the most part, to the publications of the Würzburg school. It is clear that the method, if it is reliable, greatly extends the scope of experimental psychology; but it is also clear that, if the results obtained by Külpe and his co-workers are accepted, the system of psychology must be rebuilt from the ground up. We were ourselves unable, when we repeated their experiments, to confirm some of the most important theses of the Würzburg investigators; we thought that we found in their method a definite and familiar source of error; and we therefore saw no reason for an immediate rewriting of our psychology. It seemed best to suspend judgment until we had made trial of the method in our own behalf.

No laboratory, of course, can devote itself *in perpetuum* to a single method and a single range of topics. We have, I hope, given the method of examination a fair test; and we may now, I believe, summarise—in a tentative and provisional way—the conclusions that we have reached. That is the object of the present paper. And since a scientific standpoint is most sharply defined by contrast, I shall take account, in what follows, of the—uniformly unfavorable—criticisms which Dr. Koffka, of Giessen, has published in the *Zeitschrift für Psychologie* upon the studies to which I have referred. In matters of detail, some of Dr. Koffka's objections seem to me to be well taken, others appear positively to miss the point; I shall, however, try to subordinate minor differences to the main questions at issue. I have, on my side, this principal charge to bring against our critic: that he does not recognise the serial nature of our work; he does not see that we have been feeling our way to clearness, step by step and problem by problem; he does not realise that the progress from Okabe's article on *Belief* to my own discussion of *Description and Statement of Meaning* is as real—*si parva licet componere magnis*—as the progress from Marbe to Bühler. Otherwise he would surely have noted that the shortcoming of an earlier paper is redeemed, at least so far as intention and effort are concerned, by the plan of some later study.

Another introductory remark must be made. It is natural that a student, who has spent two or three years upon this method of examination—reading, criticising, observing, planning, interpreting—should, when he comes to publish, lay stress upon the positive outcome of his work. He has been no less patient, no less ingenious, no less single-minded than his colleague, whose study of sensation or perception has been based upon methods of a stricter type; and he is eager to show that his labor has not been wasted; he wishes to make his contribution to the store of psychological fact. It is, nevertheless, not quite fair to take him at his word, and to judge his results as they are offered. The reader with a wider perspective will understand that the general method is itself on trial, and that, while the statements of the author's *Summary* are a proper subject of criticism, yet the critic fails of insight if he forgets their genesis, their attainment by a practically unaided observation. We shall never discover, by the naked eye, the details that lie plain in the field of the microscope; but where the microscope cannot be employed, a gross description may have real scientific value. We shall go astray if we bring the eye into comparison or rivalry with the microscope; but the critic is also at fault when he confuses the two orders of observation, when he judges them both by the same standard; for results, after all, are a function of method.

Okabe on Belief.[1]—In his study of belief, Okabe had six observers, four trained and two relatively untrained.[2] His method was to lay before these observers, in visual or auditory form, sentences or mathematical expressions which were calculated to arouse belief or disbelief; or, later in the work, to present sentences or mathematical expressions in pairs, the one member of which should arouse belief, the other disbelief.[3] The observers were to attend and to understand; if then belief or disbelief appeared, they were to close their eyes and to dictate a report of their experience. No time-limits were set, and no time-records were taken.[4]

The main results of the study were three. The first was that the experience of belief may be either explicit or implicit; that is to say, belief may appear as a complex course of specific content-processes, or may be bound up with, incorporated in, a particular consciousness, with no conscious representation beyond the mode of occurrence of this consciousness itself. The distinction thus drawn is no new psychological discovery; the reports of Okabe's observers confirm and extend observations of Ach, Messer and Störring;[5] but the independent confirmation, and the extension to the untrodden field of belief, are surely worth while. The second result was that belief and disbelief are consciousnesses of the same kind. This, again, is not a new discovery. "The true opposites of belief, psy-

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[1] This JOURNAL, xxi., 1910, 563-596.

[2] Koffka remarks that the reports of the untrained observers, which "differed in important points from those of the others, were dismissed as *Kundgabe*" (*Zeits.*, lxiii., 1912, 398). The facts in the case will be found in Okabe's paper, 575 f.

[3] Okabe speaks here of "a method [not *the* method] of paired comparisons" (580). I do not know why he should not; but Koffka objects to the name (*Zeits.*, 397). Okabe gives (*loc. cit.*) the reasons for his change of method.

[4] Koffka thinks that the duration of the single test, and the number of tests made at a sitting, should both have been recorded (*Zeits.*, 397). The criticism is just. We supposed, at the time, that we might obtain more complete descriptions if the observer knew that he was *not* to be timed (*cf.* Wundt's criticism of 1907, *Kleine Schriften*, ii., 1911, 277, 281, 293); but later experience showed that observers very soon grow accustomed to the stop-watch. The sittings were always of an hour's length, and our papers show the number of tests made in every hour; we did not find that this number was significant; but the fact should have been stated.

[5] *Op. cit.*, 592 f.

chologically considered," says James, "are doubt and inquiry, not disbelief;" and Bain had said the same thing before him.[6] Okabe thus confirms, by the evidence of his observers, an opinion already expressed as a personal conviction by psychologists of note. The third result—which is, in fact, a set of results—consisted in a rough analysis and differentiation of the experience of belief as it varies with type, affective disposition, and so forth. Here is new material; and its value is the value that attaches to a first, direct, observational report of a fairly complex situation.

Koffka, however, has two sweeping objections. In the first place, Okabe's trained observers show theoretical bias, and his own conclusions rest upon an arbitrary selection of reports.[7] I can hardly be expected to meet this criticism otherwise than by referring the reader to the article itself. In the second place, we have not attempted a real analysis: we do not say whether all our observers attached the same meaning to 'belief' and 'disbelief;' we do not say what visual quality (*Sehqualität*) is involved when an observer 'sees' agreement or disagreement; and we do not undertake a special study of the *Aufgabe*.[8]

As regards the *Aufgabe*—I take up Koffka's points in reverse order—I grant that we leaned somewhat heavily on the work of our predecessors in Germany; we sought, later, to correct this mistake. As regards the 'seeing' of agreement, I can only suppose that our critic has, in some extraordinary fashion, misread Okabe's statements. In his first analytical summary of the reports obtained from one of his observers, Okabe speaks of "a 'perception' of agreement or disagreement, of harmony or disharmony, which was difficult to analyse, but seemed in every case to be preponderantly visual." So far is he from reference to a 'visual quality' of agreement that he hesitates to use even so loose a term as 'perception.' Later on, when the reports grow fuller, he allows himself to speak outright of the perception of agreement; and finally, when he summarises the whole body of results for purposes of confrontation, he says: "the core of belief-disbelief is to be sought in the arrangement and behavior of visual images."[9] Is it necessary to point out that arrangement and behavior are spatial and temporal affairs, independent of sensible quality? or that there is a progressive analysis as this observer gains in practice?

There remains the objection: "nirgends ist die Frage aufgeworfen, ob alle Vpn. dasselbe unter belief und disbelief verstanden." The critic is mistaken: there is a passage in which that question is explicitly raised.[10] I do not rest, however, in this formal reply. We find, in the reports, cases of a passive acceptance which follows upon understanding; we find typical consciousnesses which we and the observers call belief and disbelief; and we find consciousnesses of certainty, of positive conviction. We find that there are various shades or *nuances* of belief, which the observers seek to express by adjectives. All these things are set down, in such detail as the method permits. And our object was, of course, to learn what sorts of consciousness are covered by the term belief, as this term

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[6] *Ibid.*, 564 f.
[7] *Zeits.*, 397 f.
[8] *Ibid.*, 398 f.
[9] *Op. cit.*, 570, 577, 583, 589, 592.
[10] *Ibid.*, 576.

is ordinarily employed,—not to erect a norm of belief, to which the observers should be brought to conform.

Clarke on Conscious Attitudes.[11]—Clarke's aim is to bring the conscious attitudes—"certain large and comprehensive experiences, not evidently imaginal in character"—to the test of introspective observation, and thus to discover whether or not they are analysable. Her early tests yield a number of incidental analyses, of which she writes: "Many of these incidental analyses are, evidently, imperfect. Even at the best, the observers might report in large and sweeping phrases, such as 'bodily attitude' or 'organic sensation.' There is, however, no doubt that the reports were intended at the time to represent the attitudes themselves, and not merely incidental or concomitant occurrences." And she supports this position by the direct acknowledgments of her observers.

"It is obvious," says Koffka, "that analysis meant, for the author and her observers, nothing else than the exhibition of the sensory contents present at any given moment. . . . These sensory contents may be irrelevant to the thought, or may be the necessary condition of the arousal of a thought, or may finally be the thought itself. Why this third possibility should alone be recognised, neither the author nor her observers explain."[12] The reply is that this possibility was not alone recognised. It was precisely because there were other possibilities that the observers were instructed, at the outset, to give complete introspections, and were confronted, later on, "with an outline of their reports upon various attitudes, and were asked to say whether, so far as they could remember, the analyses were, as analyses, correct. The regular answer was that they were correct, and in several cases the observer added, of his own accord, that he could reproduce the attitude, at the moment, and that it corresponded with the analysis given." The sensory contents were, then, certainly not irrelevant; nor were they, so far as the observers could tell, the condition of some further and specific conscious contents; they were the given conscious factors of the attitudes themselves.

But, the critic goes on, "it is sheer enigma that an observer can say, by direct observation, that 'approval' is 'pleasantness with some general kinaesthesis.' Pleasantness and general kinaesthesis are first of all just that,—pleasantness and general kinaesthesis."[13] To be sure they are, when you treat them in separate chapters of a text-book. But the objection, taken literally, denies the possibility of psychological analysis in any field. What we have in the present case is simply this: that under the instructions given, and under the conditions of the experiment, the 'attitude of approval' factorises into pleasantness and some general kinaesthesis. Nobody asserts that the analysis is adequate; Clarke herself, as we have seen, regards her incidental analyses as incomplete; the point is that the attitude, in the particular circumstances, does not wholly resist analysis, but factorises in the manner stated. Clarke then proceeds to more detailed analyses, which the reviewer dismisses as worthless.

But, once more, Clarke speaks of imagery as "carrying thought. Yet in that case thought would be, after all, a specific conscious

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[11] This Journal, xxii., 1911, 214-249.
[12] *Zeits.*, lxiii., 1912, 219.
[13] *Ibid.*, 219.

contents; and this, again, is directly denied."[14] Can the criticism be seriously intended? It would mean that every psychological term must find representation as a *besonderer Bewusstseinsinhalt*. It would mean that when, for instance, we have analysed an 'emotion,' we shall have to add to our analytical results a specific conscious contents 'emotion.' And more than that: it would mean that every general term in logic or ethics or aesthetics must have its special conscious representative: whatever psychological analysis may discover in the judging, approving, appreciating consciousness, there must always be a specific contents of judgment, of approbation, of appreciation! For it is clear from the context that the thought which Clarke's imagery 'carries' is understood by her to be logical thought.

I can only suppose that the reading of Clarke's paper made upon the reviewer a generally unfavorable impression, and that, when he came to write his review, he was more concerned to express dissatisfaction than to work out psychological principles. At all events, I cannot see that a system of psychology would be possible, if these principles are to stand. I hear the notes *c-e-g* struck on the piano. I may recognise the chord, as the common chord in the key of *c*-major. I may also analyse it into tones and noise; I find, perhaps, a form of combination, an elementary aesthetic feeling, an organic reverberation. Is it "sheer enigma" that a practised observer can resolve 'the common chord in the key of *c*-major' into these components? and will Koffka deny the possibility of psychological analysis at large? Or again: I say that my discrimination, in certain terms of a psychophysical series, was based upon absolute impression. Would discrimination, in such a case, "be, after all, a specific conscious contents," although "this is directly denied?" Koffka must, apparently, reply in the affirmative. I submit, once more, that the psychological principles which underly his criticism are at least unfamiliar, and that a mere appeal to them as axiomatic does not justify his adverse judgment of Clarke's work.

The remainder of Clarke's article, which is dismissed as beneath consideration, is taken up with a demonstration of graduated steps from clear imagery to 'imageless thought,' and with detailed analyses, explicit and genetic, of particular conscious attitudes. Clarke, I may add, discusses the *Aufgabe;* registers the times of reaction; and often mentions the place of a report in a sitting or series.[15]

Titchener on the Psychological Self.[16]—I should not have been surprised if our critic had ignored my note on the *Consciousness of Self*. As it is, however, I must defend myself against a hailstorm of critical condemnation. I had found, in a current text-book, the statement that 'I am always, inattentively or attentively, conscious of myself, whatever the other objects of my consciousness.' This statement was at variance with my own experience, and I wished to put it to a preliminary test. Koffka objects, first, that the concept of self, "which springs from a whole number of sources, but assuredly not

[14] *Loc. cit.*

[15] Okabe published in October 1910, Clarke in April 1911. Koffka's reviews were apparently sent to the editor at the same time: that of Clarke came out in November, that of Okabe in December 1912. May I not justly express surprise at Koffka's failure to point out that some of the glaring defects of Okabe's method had been corrected by Clarke?

[16] This JOURNAL, xxii., 1911, 540-552.

from those of a scientific psychology, is employed simply as a psychological concept."[17] That is true. It was the psychological self that I was concerned with, the self that is discussed in the psychologies as part of the subject-matter of psychology; all other selves were indifferent to me. If I have erred, a great many others are also at fault. Should not the blame be shared? and should we not be told why a psychologist may not investigate the conscious representation of a concept—whatever its origin? Koffka, to be sure, adds that "wir die wichtigsten Aufschlüsse wohl von der Pathologie zu erwarten haben." Let that be granted: and I, at any rate, have nowhere denied it: still, is it, in the present connection, anything but a paralogism? For the generalisation that I was combating is certainly not derived from pathological sources.

"It is simply assumed," the critic proceeds, "that everybody understands by the word self-consciousness not only a perfectly definite experience, but also an experience that is somehow the same (*irgendwie gleichartig*)." The objection, if it can be urged with any show of reason at all, affects the statement that I was examining rather than my own procedure. For my second question called for a description of the self-consciousness, which should be made "as definite as possible. Is the consciousness of self explicit (*e.g.,* visual image, organic sensations)," I asked, "or implicit (intrinsic to the nature of consciousness, inherent in the course of consciousness)? Can you bring out the character of the self-consciousness by comparing or contrasting it with other phases of a total consciousness?" Neither in the formulation of this question nor in my treatment of the reports did I make the assumption attributed to me.[18]

"As we have to do in the present instance with an experience of extremely complicated structure," the reviewer continues, "this [uniformity] is not self-evident, but on the contrary is highly improbable." I can, again, only wonder at my critic's psychological principles. Genetically, the concept of the self may be of an extreme complexity; but is there any reason *a priori* why, in the consciousness of the educated adult to-day, it should show an "extremely complicated structure?"

There remain two objections of a more practical kind. "There are," observes our critic, "far simpler experiences in face of which the author's method meets with flat failure." I myself refer to cases in which a somewhat similar method proved unreliable, and mention the additional safeguards upon which I rely.[19] Koffka, however, is speaking of sources of error other than those that I had discovered. He must, therefore, have in mind cases in which my method was exactly—and fruitlessly—anticipated. I do not know of them; and he, unfortunately, does not give references. He objects, finally, that the method "introduces into the laboratory the old psychology of reflection."[20] I should say that the method furnishes a rough way of discovering whether the old psychology of reflection is already

[17] *Zeits.,* lxiii., 1912, 212.

[18] *Op. cit.,* 545 f., 550 f.

[19] *Ibid.,* 540.

[20] It is characteristic of the criticism evoked by this method of questions that the charge which Koffka here brings against me, in particular, is precisely the charge brought by Wundt against the Würzburg school in general: see *Physiol. Psychol.,* iii., 1911, 551 ff., esp. 553 f.

entrenched in the laboratory; I do not see how a request for direct observation, with a judgment of presence or absence, could of itself introduce that psychology. I return to this point later.[21]

Jacobson on Meaning and Understanding.[22]—The novel feature of Jacobson's work on the perception of letters and the understanding of words and sentences is the attempted separation of 'description' from 'statement of meaning.' The observers were required "to put direct description of conscious processes outside of parentheses, and statements concerning meanings, objects, stimuli and physiological occurrences inside."[23] Jacobson, now, is taken to task, first of all, because he further instructed his observers to report their experiences in strict temporal order. "It has not occurred to the author that he could do better by setting various temporal limits, objectively, to the process under examination."[24] This is a little harsh, when even Okabe had had recourse to fractionation![25] But, indeed, the critic has not read with sufficient care. Fractionation may give you the objective temporal order of occurence; but it tells you nothing of the observer's temporal attitude during an extended report. And Jacobson was interested in the method; he wished to find out whether the time-relations noted in the extended reports of previous investigators were reliable.[26] A second point against him—that the nature of his 'statements of meaning' is not clear[27]—must be acknowledged as fair criticism. It should be said, however, if we are to continue fairly, that Jacobson was aware of this defect, and himself calls the reader's attention to it; and also that he expressly raises the question of the difference between associates which carry meaning and associates which do not.[28] Koffka charges, thirdly, that the method of reinstatement or repetition betrays ignorance of "the rules for the use of introspection."[29] As if these *Vorschriften* could be written down with mathematical accuracy, and as if a rule did not vary with variation of the conditions of observation! Jacobson refers to Wundt's article of 1907, and was familiar with the discussion of 1888.[30] He wished to learn whether, if the original conditions of the observation are restored,[31] the method of reinstatement is still

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[21] P. 440 below.

[22] This Journal, xxii., 1911, 553-577.

[23] *Ibid.*, 555.

[24] *Zeits.*, lxiii., 1912, 380.

[25] *Op. cit.*, 567.

[26] *Op. cit.*, 553 f. Jacobson found also that the temporal instruction "was of material aid in the correlation of 'process' and 'meaning.'"

[27] *Zeits.*, 381.

[28] *Op. cit.*, 556 f., 566 ff. A more sympathetic critic, who had observed the serial nature of our work (p. 429 above), would perhaps have anticipated a further study, dealing with this outstanding question.

[29] *Loc cit.*

[30] *Op. cit.*, 556; W. Wundt, Selbstbeobachtung und innere Wahrnehmung, *Phil. Stud.*, iv., 1888, esp. 298 f.

[31] *Ibid.*, 555. Koffka's further criticisms—as that Jacobson should have consulted the reports of tachistoscopic experiments—seem still to show that he misses the writer's interest in the general method. I am inclined to agree that Jacobson's modifications of the method of questions, ingenious as they are, do not materially advance it; but I think that they were worth trying; and at any rate there could be no question of a recourse to the tachistoscope. Again, however,

worthless. He found that his observers did not hesitate to report failures; that sometimes specific differences were noted between the two experiences; and he decides on the whole that "the results are encouraging, though we offer them only as a first contribution to the settlement of the question." These careful statements do not, surely, show "ignorance of the rules for the use of introspection!"

Titchener on Description and Statement of Meaning.[32]—In this paper I recur to some of the points which Jacobson had left obscure, and in particular to the question of the nature of his bracketed 'meanings.' I seek to characterise the attitudes implied in, or demanded by, the two modes of report which he had called for; and I find that the one is the attitude of descriptive psychology, the other that of logic or of a logical common sense. The distinction, then, is now no more difficult for us than for Bühler,[33]—though, as I need not say, it is not Bühler's distinction. And with it we seem to have gained the right to draw up that provisional and tentative summary of which I spoke at the beginning of this article,—the summary of our conclusions in regard to the method of examination. There are, it is true, many questions which still remain without answer;[34] and our own use of the method has, as the preceding paragraphs show, been roundly condemned. But the condemnation rests, in good part, upon psychological principles which are anything but secure; and I hope, in despite of it, that the following remarks may not be without value.

We are not to suppose that the method of questions has reached the limit of its usefulness. We may expect, on the contrary, that future work will extend both the range of its application and the variety of its forms. There seems, however, to be no likelihood that we shall gain from these extensions any power or any knowledge that we have not, in principle, already obtained. The method, as method, has been sufficiently tried, and may now be appraised.

I. I begin with a negative. It does not appear that the method of questions will ever avail, of itself, to settle disputed questions of a systematic kind: that it will enable us to decide, *e.g.,* for or against the distinction of 'act' and 'contents,' or to compose the issue between imageless and imaginal thought, or to prove or disprove the existence of a 'form of combination.' For, in the first place, the conduct of the method, the empirical procedure, is always open to a criticism which derives its canons from the more rigorously experimental ways of working that have established themselves in the fields of sensation and perception. Criticism of this sort will hardly influence the author of any given study; it will seem to him to be irrelevant, off the point; when he adopted the method, he will say, he took it with full acceptance of its limitations; he turned his back deliberately upon the refinements of experimentation; to pick minute flaws, to pepper the work with objections of detail, is only to do what he could do for himself; it shows a misunderstanding of his aim and intention. But then this same author, when it is his turn to criticise, finds himself in a quandary. Unless he is

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it is characteristic that Koffka's recommendation is identical with that of Wundt to the Würzburg group: *Kleine Studien,* ii., 1911, 291.

[32] This JOURNAL, xxiii., 1912, 165-182.

[33] *Zeits.,* lxiii., 381.

[34] See this JOURNAL, xxiii., 1912, 507; xxiv., 1913, 154.

to confine himself to a discussion of principles, he too must come down to detail; and there his opponent will meet him with his own retort. And so the critical volleys are fired, back and forth, and neither side will acknowledge a hit. The spectator, no doubt, will form a positive opinion; yet it may be questioned whether this opinion is induced directly and solely from the results of criticism. We have, in a word, a gross and loose method which is attacked or defended with all the art and skill that an experimental methodology can provide; and we cannot hope, under these conditions, to settle the kind of question that was raised at the beginning of this paragraph. But, in the second place, the results of the method—since, as I have said, results are a function of method—must themselves be gross and loose, capable of various interpretation. So an introspective report that is published in support of a certain thesis may be seized upon by a psychologist of different persuasion, and interpreted in a radically different sense; or the charge of 'arbitrary selection' may be laid against the presentation of results, as the charge of 'bias' or 'prepossession' may be laid against the conduct of the method. For these defects I see no promise of remedy in the method itself; and in so far my experience has brought me into agreement with Wundt.

II. I believe, nevertheless, that the method is of value. I can better show wherein this value consists, however, after I have characterised the method which I think will replace the method of questions as an accredited procedure in experimental psychology. Wundt, as is well known, would base a psychology of the 'higher' processes upon a study of mental 'products,'—so that the psychology of thought would emerge, *e.g.*, from a psychological study of language:[35] a direct experimental modification of these processes is for him impossible, and their direct observation is of necessity inexact. But while one will hardly find an experimentalist who denies the importance of *Völkerpsychologie,* one will hardly find one, either, who accepts Wundt's division of the science. And the method which we must look to, if we are to transcend this division, is—it seems to me—a method of the type of Ach's 'systematic experimental introspection;' a method, that is, which secures description under experimental conditions so strictly controllable that we may hope, by manifold repetition, to attain accuracy of report.[36] We cannot, in other words, dispense, in the study of the higher processes, with the experimental aids which have helped us to an understanding of the lower.

Ach's method, however,—in the particular form that he gave it,—has been severely criticised by G. E. Müller.[37] It is dangerous, Müller

[35] See, *e.g., Physiol. Psychol.,* iii., 1911, 554.

[36] N. Ach, *Ueber die Willenstätigkeit und das Denken,* 1905; *Ueber den Willensakt und das Temperament,* 1910. I say purposely "a method *of the type of* Ach's." The attempt to work out, in detail, the grounds of this qualification and the nature of the method contemplated would lead me too far afield, and would perhaps in any case be premature. It is plain, however that very many experimental procedures, over and above those employed by Ach, may be incorporated into the method. I suggest also that, in the sphere of the higher processes, direct may be supplemented by indirect modes of approach.

[37] *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes,* i., 1911, 137-143. I have discussed this criticism, from another point of view, in this JOURNAL, xxiii., 1912, 503 f.

remarks, for several reasons, to have free recourse to question and answer, aside from the fact that questioning unduly prolongs the report and thus puts too great a strain upon the memory of the observer. It is out of the question that the introspective report of a fairly complicated consciousness should be even approximately complete. The perseverative tendencies, on which Ach relies, are selective and at times misleading; and we have no proof that they are strengthened by the intent to observe. Finally, it is dangerous to suggest to the observer that the contents of the after-period are, as perseverative, identical with those of the experimental consciousness; we know of cases to the contrary; and the observer should therefore be instructed to report only such experiences as he remembers, with assurance, to have occurred during the experiment.

It is, now, a little disquieting to find that Müller's criticisms bear upon the 'systematic introspection' and not upon the 'experimental' of Ach's method; we seem to be back again in the fruitless discussions of the method of questions. But let us see! There is no physical compulsion to ask questions; the experimenter may rely upon the repetition, under identical or under modified conditions, of the task set to the observer. Or if, from lack of time or from the appearance of carelessness or of stereotyped reaction in the observer, questions seem to be indicated, then the reports that are obtained (and later reports that are possibly influenced) by questioning may be treated by themselves, and may be compared with the spontaneous reports of the same and of other observers.[38] Again, the requirement of a complete report may be so phrased that the observer understands it, simply, as the demand for a non-selective report; he is to give an account of everything that he can remember, and not to pick out the items that he himself thinks important. The idea that the contents of the after-period are necessarily the same as those of the experimental consciousness may be corrected, as Müller points out, by suitable instruction. On these three points, then, it seems possible to offer a straightforward reply to the critic, and to safeguard the method. The essential thing is, after all, that the method permits and prescribes a truly experimental control of conditions. The advantage is twofold. First, the repeating of the observation, and the varying of its conditions, allow us to test report by report; the method becomes, in a very real way, self-critical:[39] and, secondly, the standardising of conditions fulfills one of the chief requirements of a good method, that the observations may be exactly repeated by other investigators.

But I have said nothing of the perseverative tendencies, the foundation upon which Ach builds. Here we have, in fact, a disputed issue of systematic psychology; and here Ach's method, at any rate

[38] *Cf.* my remarks, *op. cit.*, 505 f., and the references given 503 f. It is noteworthy that E. Westphal, who employed Ach's method (Ueber Haupt- und Nebenaufgaben bei Reaktionsversuchen, *Arch. f. d. ges. Psych.*, xxi., 1911, 432), was careful to phrase his instructions in such a way that "the observers were held down to a report solely of what had, with certainty, been actually experienced" (434).

[39] Westphal's use of the 'synthetic method' (*op. cit.*, 359 ff., 397 f.) is a case in point. After reading certain things *out of* the reports (analytical method), Westphal proceeds to read these same things *into* the instructions of a new experimental series (synthetic method). The reports thus become, in so far, their own test.

in its present form, is plainly inadequate. It may be said, however, that the acceptance of perseveration, as a principle of explanation, is not essential to the use of the method, and that the study of perseveration, as a general term for certain observed phenomena, can be carried on in experiments of less complexity. While, then, it is regrettable that Ach, in the statement of his method, has laid stress upon perseveration,—since by so doing he has exposed himself to criticism which the method does not enable him to meet,—still the method itself does not stand or fall with his personal interpretation, and the rightness or wrongness of this personal view can be determined 'out of court' by other methods. Müller's criticisms do not affect the core of the method, which is, once more, the securing of description under rigorously experimental conditions.

III. I think, then, that the method which is to supplant the method of questions will be of the type of Ach's 'systematic experimental introspection.' And now I return to the method of questions itself.

In his reply to the objections urged by Wundt against the method of examination, Bühler remarks that his first paper was meant to furnish "a preliminary orientation in regard to certain fundamental questions of the psychology of thought."[40] This statement, literally taken, points out the chief use of the method: it enables us to make a first survey, to get our general bearings, in new fields of work. For many years, *e.g.*, experimental psychology looked a little nervously upon such things as doubt, wish, belief, desire, the formations that we now know as 'conscious attitudes;' and the method of examination has brought us face to face with these experiences, and has shown us where the problems lie, even if it has failed to furnish us with a satisfactory analysis. Ach is therefore right in ascribing an heuristic value to the method.[41] It will always be of service where new ground has to be broken, and where the formations are so complex that an immediate recourse to experiment, in the strict sense, is forbidden.

That is, undoubtedly, the chief value of the method; there are, however, other ways in which it may be useful. Thus, we have seen that it is eminently provocative; it is potent to call forth criticism and counter-criticism; if it does not settle questions, at least it cannot avoid raising them. Hence, as the method itself shows where problems lie, so does the criticism which it evokes help toward the formulation of the problems, and show what pitfalls are to be avoided, what precautions taken, when the problems are approached in a properly experimental way. Criticism, I said above, has in general been sharpened to a point too fine for the method and results against which it was directed; within the boundaries of the method, therefore, it has been ineffective and unconvincing; but this same keenness and delicacy will prove of value when the investigator is passing from the method of examination to the other and more refined method of systematic experimental introspection. It is true,

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[40] K. Bühler, Antwort auf die von W. Wundt erhobenen Einwände gegen die Methode der Selbstbeobachtung an experimentell erzeugten Erlebnissen, *Arch. f. d. ges. Psych.*, xii., 1908, 103.

[41] N. Ach, *Willensakt und Temperament*, 1910, 16 f. It should be noted that Wundt admits the usefulness of the 'experiment without instruments' in cases where the problems are very simple or where but little work has already been done upon them: *Kleine Schriften*, ii., 1911, 274.

no doubt, that as the coarser method is reduced more and more to a means of preliminary exploration, its provocativeness will steadily decrease. But even if it produce less controversial writing in the magazines, it will continue to work as ferment in the group of co-workers in the laboratory.

I am thus led to a final point. All those who have worked with me by the method of questions have assured me that they have learned from it a great deal of psychology; some have even proposed that it be introduced into the undergraduate laboratory courses. To be set over against a complex experience, and to be asked for a complete description, is—these observers say—variously informing: now, perhaps, one realises the inadequacy of current terms and formulae; now, one learns how much of one's supposed psychology is a matter of borrowed categories or of logical reflection; now, again, one makes novel discoveries as to one's mental constitution; always one is directly impressed by the complexity and elusiveness of consciousness. This advantage must not, I think, be exaggerated. A stricter method might yield the same results; it is not necessary that, because conditions are more rigorously controlled, the experimenter do all the psychologising. And there is, besides, the danger, in this method of questions, that the lines of system harden, and that interpretation be too positive. So far as it goes, however, the judgment of my co-workers must be recorded in favor of the method.

A method which possesses an exploratory value, a critical value, and an educational value,—even if the two latter values are of a lower order than the first,—is not likely to disappear; the prospect, as I remarked at the beginning of this discussion, is that it will extend its range and multiply its forms. But a just appraisal will hardly give it rank with the approved methods of the science. The psychology of the 'higher' processes lies in the hands of *Völkerpsychologie* and of the method of 'systematic experimental introspection.'

PROFESSOR YUZERO MOTORA

Through the kindness of Dr. Hikozo Kakise, a former Fellow and a Ph. D. of Clark University, now in Japan, the American Journal of Psychology has received an account of the funeral ceremonies of the late Professor Yuzero Motora who died December 13, 1912, besides four pictures of Professor Motora at different stages of his life from early childhood up to a period just preceding his death, and a photograph of the funeral ceremonies.

Dr. Kakise has translated and epitomized an address by Professor Sho Watase on Professor Motora's life at Johns Hopkins University, which was one of twenty memorial addresses that are to be published eventually in connection with a number of comments upon his life and character by those who knew him best.

Professor Watase said: "It was in the autumn of 1886 that I became acquainted with Dr. Motora, in Baltimore at Johns Hopkins University, where I was with him for two years, so I will tell you of my personal experiences with him in the university at that time.

"When I first arrived in the city of Baltimore I found Dr. Motora, Dr. Nitobe and Dr. Nagase already studying there. I stayed with them for the first few nights, during which Dr. Motora performed a strange experiment upon me. You may imagine my surprise when, before going to bed, he put on each of us a kind of wire helmet. When I, full of curiosity, asked what it meant, he said: 'During our dreams there occur certain electrical changes in our brains, so if we transmit the electric current from one brain to another by means of the helmet, all of us may have the same dream at the same time.' Though the experiment was repeated several nights we got no noticeable result. It was our opinion that the fault might not be with the doctor's theory, but rather with ourselves, for the helmets often came off while we were asleep as we were restless. His experiment thus ended in laughter, and I have never heard of it since.

"The Psychological Laboratory, where Dr. Motora worked, was then attached to the Biological Laboratory, where Physiology was taught by Professor Martin, Zoölogy by Professor Brooks, and Psychology by Professor Hall, who was Dr. Motora's teacher and who later became President of Clark University. Many young men who were then studying under him have now become famous scholars. Among them are Donaldson, then making a study of the brain; Hodge, of the nerve-cell; Sanford, of experimental psychology; Burnham, of education; Hyslop, of Greek philosophy; and Jastrow, who had finished the university course but was still working in the Psychological Laboratory.

"Dr. Motora had also been appointed Fellow of the University, which was the first in America to adopt this system and which gave annually the sum of ten thousand dollars to twenty fellows, or five hundred dollars to each. The Japanese who had received this honor since the foundation of the university numbered four,—Dr. Kuhara, Dr. Mitsukuri, Dr. Sato, and Dr. Motora. President Gilman was of the opinion that the fellowships should be given only to promising students, and there should be no discrimination of nationalities in the domains of science. It was chiefly due to this enlightened policy of his that these Japanese were able to share in this benefit.

"Dr. Motora studied Sociology and Economics besides Psychology. He took the degree of Doctor of Philosophy in 1888. By this time he seemed to have gone pretty deeply into the study of Philosophy, and I believe the topic of his dissertation was philosophical rather than psychological, for I remember once hearing a friend advise him against choosing Philosophy as his major and Psychology as his minor subject. The philosophical turn which so strongly marked his subsequent career as a psychologist and teacher undoubtedly took its origin from this period. Dr. Motora's life at Johns Hopkins was doubtless the most successful and the most important of his school-days in laying the foundation for his future studies and career."

Dr. Hall remembers Dr. Motora chiefly as a laboratory student with whom he experimented for a year on the study of "Dermal Sensitiveness to Gradual Pressure Changes," a paper on which was printed in the first number of the AMERICAN JOURNAL OF PSYCHOLOGY, 1888. It was a study that has always been referred to with respect by those who had occasion to treat the subject, and showed remarkable deftness of manipulation as well as ingenuity of method on the part of Dr. Motora. He also participated with Dr. Hall in the study of sleight of hand performances and was able to do a number of very interesting things, and all in a way which showed that he had a very deep interest even then in mystic phenomena. He was, as Dr. Hall remembers him, a man of the most serious and earnest character, quiet and modest, apparently with no other interest whatever than his psychological and philosophical studies, his zest for the latter probably being more pronounced than for the former. On a recent visit to this country, when he was a guest at Dr. Hall's house for a few days, he showed that his interest had gone over very largely into the field of a philosophy that seemed to focus upon religious subjects; and nearly all his conversations were upon a type of religion which should embody and unite the chief truths in the faiths of the Eastern and Western worlds.

Dr. Burnham has especially pleasant memories of Professor Motora as a fellow-student at Johns Hopkins University. He recalls him as a student of the best type, at once sane, temperate, industrious, enthusiastic; as a thinker, distinctly philosophical, bold, original, independent, and vigorous; as a friend, pleasing, reliable, satisfying, and with a large fund of good-fellowship; in character possessing all the sturdy virtues, dependable, trustworthy, dignified, and helpful. He was thoroughgoing and original as a thinker and scholar; and even at that time he possessed apparently distinctly those virtues which make a teacher in the older and larger significance of the word, one whose influence indirectly by character and scholarship may be even greater than his direct influence as an instructor. There is something distinctly wholesome, satisfying, and supporting, even in the memory of Motora's character and good-fellowship.

President Edmund C. Sanford writes: "It is now nearly thirty years since I first met Professor Motora as a fellow-student of psychology and philosophy at Johns Hopkins University. He was a year ahead of me in his university rating and already engaged in research while I was going through the preliminary preparation, and therefore looked up to and envied. Later I served as subject in some of the experiments which he made on sensations of gradual changes in pressure, and still later he kindly assisted me in a similar way in a study of the legibility of letters.

"In the laboratory, in the lecture-room and in the spirited discussions about Dr. Hall's seminary table I came to know Dr. Motora better than I had then known any of his countrymen, and to know him was at once to respect and to feel drawn toward him. Perhaps it was his quiet, reserved and yet friendly manner, admirably fitting the philosopher that he was, perhaps it was his points of view, not always fully grasped by me and having the possibilities of the orient about them, perhaps it was comradeship in the enthusiasm of the place. Whatever it was, it attracted me to him, and fostered a friendship for him which was gladly renewed in 1903 when he contributed a characteristic article to the *Festschrift* for Dr. Hall, and gave me keen pleasure at the sight of him on his recent visit to this country and Europe.

"The individual incidents of our comradeship at the University, so far as I recall them, were for the most part trivial, but the core of one conversation I have never forgotten. It must have been soon after a seminary discussion on the psychology of meditation, mystic contemplation and similar matters—all, to my occidental mind, wholly antithetical to active efficiency. In continuing the discussion afterward, Dr. Motora remarked that many of the most distinguished men of action in his own country were accustomed to practice just such withdrawals from the world of their daily business. His remark forced me to realize, as I never had done before, that there is no necessary antagonism between such states of deep reflection—akin to the essence of prayer—and vigorous activity; indeed that deep contemplation may itself furnish at once the inspiration and the supreme guide of executive efficiency.

"I suspect that Professor Motora himself, though a philosopher and scientist rather than a man of affairs, may have been speaking in part from his own experience, and that the practical regimen of this combination of contemplation and action may be one of the lessons which the West may learn with profit from the East."

A CORRECTION

I wish to take the blame for the mistake which Ferree (on pp. 379-380 of this number) attributes to Geissler. So far as I recall, the facts are as follows. When Ferree consulted me with regard to a method of stimulation of the tongue, I suggested Christmas-tree foil. As I heard nothing more of the matter, I supposed that this material had been used. Ferree was ill at the time that his article was published, and I had the responsibility of proof-reading. The sentence "Strips of very light tin foil were used as electrodes" (this *Journal*, xvii, 1906, 119) was changed by me by the insertion of "(Christmas-tree foil)" after "foil;" I thought that the naming of the material might be useful to others. The mistake caused Geissler a good deal of trouble,—for which, as well as for the misrepresentation of Ferree, I am very sorry. E. B. TITCHENER.

FIFTH REPORT OF THE POLISH PSYCHOLOGICAL SOCIETY

Mr. E. Abramowski was appointed as director of the Psychological, Dr. E. Flatau as director of the Neurological Laboratory.

The Society arranged 15 meetings, at which the following papers were read:

Dr. Joteyko-Rudnicka, Report of the Work of the Psychological Laboratory at the University of Brussels; Dr. Ada Silberstein, Ugliness and Esthetics; Dr. Fl. Znaniecki, Efficiency in the Light of Pragmatism; St. Lesniewski, The Problem of Existence in the Light of Grammatical Norms; Dr. Josephine Kodis, Metaphysics in Science; Dr. Vincent Lutoslawski, Integral Psychology; Dr. W. Radecki, Psychoelectrical Phenomena; Dr. W. Tataskievicz, Apprehension and Judgment; Dr. Cygielstreich, The Influence of Emotions on the Development of Brain Diseases; Dr. Tl. Znaniecki, Practical Reality; T. Kievski, Philosophy in the Normal School (Gymnasium or Polish School); Dr. T. Halpern, How to Understand "The View of the Universe"; St. Lesniewski, Contradictory Views on the Theory of Language (Speech); Dr. B. Bornstein, The Euclidic Character of Space; Dr. J. Ochorowicz, Experimental Tautology.

Researches of the Psychological Laboratory:

E. Abramowski, Analytical Telepathy as a Phenomenon of Cryptomnesia; The Influence of Fixed Attention on the Breathing and Pulse; Influence of Will in Emotional States, Measured by the Galvanometer; The Connection of Emotion in Reference to Memory; A. Cygielstreich, Subconscious Creativeness in Children; Subconscious Creativeness in Adults; Mary Lipska, Connection between Discriminative Perception and Vocational Fitness; W. Makowski, Emotional Coördinations Measured by Galvanometrical Reactions; T. Lapinski, Subconscious Creativeness in Mental Diseases; J. Warszavska, Subconscious Creativeness in Children.

Researches of the Neurological Laboratory during the year 1911: Dr. M. Bornstein, Changes in the Spinal Cord Induced by Depression; Studies on Landrié's Disease; Dr. Flatau, The Parallel Between Functions and Real Changes in the Nervous Fibres; Dr. Flatau and Dr. Handelsmann, Analytical Experiments as to the Treatment of the Inflammation of Nerve Centres; Dr. Gepner, The Influence of Natri Nucleinici and Salversan on the Nervous System of Animals; Dr. Handelsmann, The Treatment of Neuralgia; Experimental Works on the Brain, etc.; Dr. Koelichen, Chromatophorma Medullae Spinalis; Studies on the Anatomy of Lymphatic Lines, etc.; Dr. Rotstadt, Studies on Myasthenia; Dr. Synechowicz, Studies on the Pathological Anatomy of Alzheimer's Disease; Studies on Changes in Parkinson's Disease; Studies on Meningitis, Cerebro-spinalis epidemia, etc. Women students were working in this Laboratory chiefly as *laborants.*

CONVENTION OF EXPERIMENTAL PSYCHOLOGISTS

The tenth annual conference of Experimental Psychologists was held April 10th to 12th, at Wesleyan University, as the guests of Professor Raymond Dodge. The first two sessions were occupied by the reports of the investigations in progress at the various laboratories represented, about twelve in number. At another session, the directors of several of the larger laboratories gave a résumé of their own work during the last ten years, since the starting of these conferences, and discussed the advances made in the general field of psychology during that time. Some mention was also made of the problems which it seems desirable to attack in the near future.

At one of the sessions there was a lively discussion as to the "Behaviorist" viewpoint recently advanced by Professor J. B. Watson. Those present were surprisingly in accord against such an envisagement of the problems of psychology. Unfortunately Professor Watson was not present to defend his position.

One session was devoted to tests—their theory, value and limitations—in which papers were presented by Dr. F. L. Wells and Dr. H. H. Goddard, although the latter was unable to be present in person. In the discussion, the general opinion seemed to be that the word "test" was a misnomer; as those now applied do not give indications of the degree of mentality of the subjects with any amount of finality. The question of the attitude of the experimentalist towards those who may attempt to apply psychology to business, industry, vocational guidance and similar practical problems, was brought up and discussed in some detail. The general opinion seemed to be that such applications had better come directly from the universities rather than from independent workers on the outside.

Professor Hugo Münsterberg delivered a semi-public lecture on his recent investigation of the mind reading of Beulah Miller, which aroused a great deal of interest.

The social features of the meeting included a luncheon, tendered by the University, and a dinner at the home of Professor Dodge; as well as a smoker at the University. On this latter occasion Professor Dodge demonstrated some of his apparatus, which was most ingenious indeed.

It was decided that the next meeting be held at Columbia University in the spring of 1914.

Clark University. S. W. Fernberger.

BOOK REVIEWS

The Place of Illusory Experience in a Realistic World. By Edwin B. Holt. In *The New Realism.* New York, The Macmillan Co., 1912. pp. 491. Price $2.50 net.

The main contention of the author of this essay is that all mental phenomena are the same in kind as the phenomena of the physical world. He argues (1) that this thesis holds for sensation, perception and image; and (2) that contradictions, which constitute the content of an illusory experience, are not subjective, but objective, in nature. He devotes the greater amount of his space to the development of the first of these two arguments, which is also the more interesting to the psychologist.

Errors of space and of time receive scant attention. It is enough to show that for each of them an analogous phenomenon may be found in the physical world. If one presses an eyeball out of place, and keeps both eyes open, one will see double; so does a stereoscopic camera. "Or, again, an astigmatic eye distorts its object; so does a roughly cut glass lens." We see some heavenly body many years behind time: what advantage over us has the photographic plate? Many of the errors in secondary qualities are parallel to those of space and time; but the complementary after-image and the pure hallucination offer greater difficulty. The former is met by a new theory of vision based on Meisling's view that the cones resonate to waves of light. It has been found that, when the capacity of the receiving mast of a wireless telegraph system is tuned to a given length of Hertzian wave, it is *ipso facto* tuned to a second wave-length as well, which may be regarded as its complementary wave. Light waves and Hertzian waves are closely related physically, so that, if the view of Meisling is correct, the complementary colors of vision are nothing but a true presentation in consciousness of the fact of complementary attunement of light-resonators. The case of pure hallucinations raises two questions: (1) How can these purely hallucinatory secondary qualities have any sort of being other than a subjective and mental being? and (2) How can they assert *themselves to be* or how can the *realist* pretend to *assert them to be* the real object? The answer to the first question is long and involved. The doctrine of specific nerve-energies is first attacked, on the ground that physiologists have been unable to discover any trace of specific nerve-energies in the differences between nerve-impulses, nerve-fibrils, cortical cells and synapses. Furthermore, a satisfactory theory of specific nerve-energies must furnish an explanation, not only of qualitative differences between sense modes, but also of qualitative differences within sense modes. No theory, not even Helmholtz' theory of audition, meets this demand with success. Specific nerve-energies must, therefore, be given up. The author admits that there are qualitative differences; but he thinks that they can be analyzed into quantitative differences, and for this reduction he carefully prepares the way. Physiologists have found that the nervous impulse presents periodic fluctuations of a frequency much higher than had previously been suspected. The nervous response follows the rate of stimulation, and the periodicity of the impulse may rise as high as one thousand in the second. This discovery is accepted as the physiological basis of the author's explanatory psychology. Recent experiments in audition, particularly those of Lord Rayleigh, point to a vibratory theory of audition; and a combination of the Rutherford and Meyer theories proves adequate to the facts. In the field of vision, the resonance theory falls readily under the new point of view. The light waves may act photo-chemically on the visual purple of the rods, and physically on the cones. Still, of course, the question remains: How do the periodic vibrations become qualities? And the answer is that the qualities are a kind of form-qualities, "in which the temporal subdivisions are so small that the time-sense cannot discriminate them, whereas the frequency-magnitude, or the *density,* still remains perceivable." But it is not the factor of density of the nervous impulse that is the secondary quality; it is rather the density of the series of some relatively primitive sensation which is the secondary quality. As form-qualities these densities have two characteristics which differentiate them from other form-qualities: (1) their principle of organization is time, and (2) they are of a lower order than the *Gestaltqualitäten* with which we are familiar. The task of psychology, now, is to analyse all form-qualities, as far as possible, by introspection. Of the original five senses, taste and feeling have already proved to be fusions; the clang can be analysed into partials, orange into red and yellow, and even green is phenomenally yellow and blue. When, however, introspection can go no further by its usual method, it may try a new method. A quality that lies in a series between two others may be eliminated, on the ground that it possesses the same ingredients as these two. Eventually, then, we shall get down to a single element, such as one kind of atom, variously organized, such as in three sizes of molecule. This is the primitive entity whose density constitutes a secondary quality. It is not mental or subjective in substance; on the contrary, it is the same in substance as the physical element.

This reduction completes the author's first argument. Realism is able to assert the reality of an hallucination, because the nervous system is able to generate within itself nerve-currents of frequencies whose density-factor is the same as that of ordinary peripheral stimulation. It remains to say a word concerning his theory of consciousness. "Consciousness is the group of (neutral) entities to which a nervous system, both at one moment and in the course of its life history, responds with a specific response." Either the object, or the color (secondary quality) on the object, is *specifically responded to* if the nervous system can pick up and transmit the vibrations which are sent out by it. Consciousness, moreover, is not in the skull; it is out there in space, precisely where it appears to be.

The author next takes up the problem of error. No thing, be it brickbat or image, can of itself assert anything about reality or unreality. Such a bare content is in logic known as a 'term.' A term, or system of terms, simply is, and is neither true nor false. Contradictions, however, may exist among sets of assertions or propositions about a term. A proposition is supplied by experience, and, if it be about a mental term, may coexist with this in the mind. Terms in relation which are physically impossible, as for example the round square, are also mentally impossible, *i. e.,* unthinkable. But the mind may entertain contradictory propositions about the round square; and physical laws, which are propositions, are habitually in a state of contradiction, as for example when two laws of motion oppose each other.

"A thought, then, which negates another thought is neither more nor less significant than a physical law which negates another physical law."

We have tried to give an abstract of the author's views as we understand them. We have not indicated in any degree the ingenious way in which the argument is presented, nor have we hinted at the apt illustration, the pleasing metaphor, the friendly sarcasm which abounds throughout. Our principal objection is that he writes as an advocate, and not in the dispassionate way in which scientific theories should be discussed. The acceptance of the argument as it stands means the acceptance of new theories of vision and audition, of a new kind of form-quality—to say nothing of the old, of a thorough-going atomism, and of a strange theory of consciousness. The author would agree that the only test of a theory is an appeal to the facts; we wish that he had at least indicated how his theory of vision would explain simultaneous contrast. Again, it is not quite fair, where there is some disagreement as to what are the facts, to choose one set of observations and entirely to ignore the others. For example, there are psychologists, who are aware of the stimulus-error and who work with spectral colors, and not with pigments, who still insist that orange is a simple quality. There is also good reason for believing that the rods are organs of night vision only. In which case the cones, according to the novel theory, must reasonate to white light. What would a resonator, tuned to a pair of wave-lengths, do with so complex a wave as that of white light? The author cannot say that each resonator picks out its own wave-length, for the retina would then be an analyser, and that would imply specific nerve-energies. Moreover, we should, in such case, be able to analyse mentally white light as we do a clang. Similar objections might be raised against the theory of hearing: we will only remark that Wundt, twenty years ago, considered such a theory, but declined to accept it because it cannot satisfactorily explain clang-analysis. As regards the view of secondary qualities as densities, one can only speculate. The sole evidence which the author brings to the support of his theory, the roughness and smoothness of the tapping experiment or of the flicker experiment, is hardly germane; for roughness and smoothness are not simple qualities, as is the quality of a color or a tonal sensation. If, however, we were to hazard a guess, we should agree with Montague that the author has given us an interesting explanation of intensity, but not an explanation of quality at all. As to form-qualities there is still the possibility, as Titchener has pointed out, that the problem of meaning is involved; so that, just as no content can of itself assert anything, so none can mean anything,—not even triangularity.

Cornell University. H. P. Weld.

Das Problem der Willensfreiheit. Von G. F. Lipps. Leipzig, B. G. Teubner, 1912. pp. iv, 104.

This booklet, published in the series *"Aus Natur und Geisteswelt,"* is based on *Volkschulvorträge* delivered by the author at the University of Leipzig. Lipps accepts Kant's statement of the problem of freedom: that man is at once free and determined; but he seeks a solution more adequate than that of Kant. Instead of splitting hairs over the meaning of Freedom and Determinism, the author proposes to examine the essence of our own being and conduct, in an endeavor to solve thereby the puzzle of the free-determined character of our willing and acting. To this end, he presents in brief outline the part played by reason in various Greek theories; Augustine's doctrine of the good and the bad will, and the modern rationalistic accounts of the relations of mind and body. Kant, he thinks, is enabled to hold causality and freedom side by side, only by lifting morality out of the world of phenomena, and thus making man a member of two universes. The relation between these two human natures remains a mystery for German idealism; that mystery Lipps seeks to grasp in terms of actuality. As thinking beings, he holds, we are convinced of the thoroughly necessary character of human conduct. Man must act in the way in which he does act. The preconditions of each act, however, can never be completely indicated; and this inevitable residuum of uncertainty leads us naïvely to postulate a will essentially free. Thus the problem of freedom and determinism, Lipps concludes, involves the discrimination between the naïve and the critical attitude towards human conduct.

Clark University. RADOSLAV A. TSANOFF.

Ethics. By G. E. MOORE. New York, Henry Holt and Company; London, Williams and Norgate, 1912, pp. 256.

The aim of the author is "to state and distinguish clearly from one another . . . the most important of the different views which may be held upon a few of the most fundamental ethical questions" (p. 11). This he attempts to do by devoting a third of his book to an analytic statement of utilitarianism, and the remaining two-thirds to an examination of possible criticisms of that theory. Moore defends utilitarianism against the line of attack which consists in saying that right and wrong are merely subjective predicates. To say that an action is right or wrong, intrinsically good or bad, is not to say that one has towards it any mental attitude whatever. Nor is there sufficient reason for accepting as the test of right and wrong the intrinsic nature of the action, the motive prompting it, or its probable consequences. The discussion of free will "concludes with a doubt" (p. 222). The egoistic objection to utilitarianism is likewise rejected; but after disposing of all other criticisms of that theory, Moore advances his own, which he considers fatal; utilitarianism claims that rightness and wrongness depend on the intrinsic value of the consequences of our actions, and yet it does not rightly decide what constitutes intrinsic value.

The book is intended for the lay reader; the continual iteration of apparently simple ideas seems to indicate that the author was perhaps too well aware of his task as popularizer. The central place which is given to utilitarianism doubtless provides a convenient opportunity for presenting the author's own point of view in minute and finely spun reflections on hedonism; but it leaves the reader with a wrong historical perspective of ethical theory. The method of exposition, also, is unfortunate: the popular reader is offered hairsplitting distinctions and abstract explanations in terms of the conventional *A* and *B*. Unduly replete with technicalities which are out of place in an elementary treatise, and lacking almost any concrete illustrations from daily life, this book is at once too subtle and too dry for its purpose.

Clark University. RADOSLAV A. TSANOFF.

Elemente der Völkerpsychologie, Grundlinien einer psychologischen Entwicklungsgeschichte der Menschheit. Von WILHELM WUNDT. Zweite unveränderte Auflage. Leipzig, A. Kröner, 1913. pp. xii, 523. M. 14.

The monumental volumes of Wundt's *Völkerpsychologie* find not only a summary but also a crowning supplement in the *Elemente der Völker-*

psychologie. Here the entire mental history of man is outlined in a continuous narrative; the various activities, such as custom, myth and religion, and art, are not separated but dealt with in conjunction. We pass from 'Primitive Man' to 'the Age of Totemism,' then to 'the Age of Heroes and Gods,' and finally into 'the Development toward Humanity.' It is safe to say that no other man could have told the story as Wundt has; his vast learning, powerful psychologic insight, vivid sense of history, and, not least, his stylistic ability to present states of flow and change have produced a work of tremendous and awing effect.

It is not necessary to recall here the justification of a social psychology as an inevitable consequence of the rejection, by empirical science, of the metaphysical postulate of an individual soul as the substratum or receiver of experience.[1] Familiar, also, are the general results of the social psychological method: the exclusion of reflective rationalizing explanations, in which logical processes are falsely projected by the explainer into the communal developments. In the *Elemente* one can see the larger results of this methodic precaution. Nowhere is a reflection about the consequences of a development assumed as the cause of that development. Hence the frequent reversal of the naïve view of things: the demon is not a causal explanation of natural happenings, but a creation of emotions, especially fear (p. 355), for the primitive knows only magic causality (pp. 90 ff.). Again, the god grows out of the demon, religion out of the beliefs in demons and spirits, and law out of custom. All this is too familiar to require comment.

In one respect, however, the *Elemente* differs, even externally, from the *Völkerpsychologie.* In the latter the subject of language receives two volumes, placed at the beginning of the work,—naturally enough, for language is 'the universal substratum of mental culture' ('die allgemeine Trägerin der geistigen Kultur,' *Elemente,* p. 487). And it appears, at first, when one studies these volumes, that Wundt's social psychology has done for our knowledge of linguistic development exactly what it has done for the other spheres of social activity. Especially the processes of linguistic change had been interpreted only too much as if they were acts of logical reflection; by putting an end to such interpretation and showing the concrete psychological character of changes in language, Wundt has done an inestimable service to the science of linguistics. In the *Elemente,* however, we find but a few pages in the division on 'Primitive Man' (I, §§ 5 and 6) devoted to language. A sketch of gesture-language and one of a supposedly rather 'primitive' language, the Ewe of Togoland, is all we receive. Of the development from lower to higher forms, or even of any criteria of distinction between these. we learn nothing. This is due to the fact that minute analysis of the processes of change is excluded from the *Elemente.* Descriptions—and Wundt is a master of what may be called kinetic description—of typical stages of the social institutions suffice for the purposes of this book. Could they not have been given for language also? In the *Völkerpsychologie* Wundt has contributed much toward the detailed psychological interpretation of the processes of linguistic change, but toward a history of the development of language ('die generelle Entwicklung') he has given little. The origin of language is splendidly treated and there are valuable ideas and discussions which have bearing on the general further evolution of language; but an outline of this evolution or even a sufficient indication of the direction of

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[1] *Völkerpsychologie*[3], I, 1, p. 9; *Grundriss der Psychologie*[11], § 2, 1.

development there is not. This, I believe, accounts for the scant treatment of language in the *Elemente,* where only such an outline, with portrayal of typical stages would have been in harmony. Toward this we find in the *Elemente* only a sketch of the origin of vocal language in the light of gesture (Wundt's greatest single linguistic contribution lies here), and then the description of a 'primitive' language.

To regard Ewe as such is, however, a mistake. Language, like the other communal activities, changes most rapidly where there is most contact of communities,—where there are wars, migrations, and, above all, transferences of language to new peoples; and Africa, everything indicates, has long been the scene of all these happenings (*Elemente,* pp. 136 ff.). Further, Ewe is spoken by several millions of people and even serves as a literary language.[2] It is a member, moreover, of the widespread West-Sudanese family of languages, which possibly may be related to the other great family of the Bantu languages. Plainly, then, this language has behind it a long history of spread, migration, and change. This becomes a certainty when we learn more of its forms. It is a nearly monosyllabic language: the languages of whose history we know anything show a constant shortening of the word toward monosyllabism. This is the direction in which the languages of Europe have developed, especially, of course, English. When, further, we learn that in Ewe the word-order is fixed, we must entirely refuse it the title of a 'primitive' language, for linguistic history everywhere shows us that the syntactic utilization of word-order is a gradual accomplishment. Wundt thinks it a primitive characteristic that in Ewe the modifying word follows its subject ('man big,' not 'big man'), as in gesture-language. It is obvious that fixed word-order allows of only two possibilities, the one realized in Ewe, as in modern French (where we can see it growing out of the free word-order of Latin), the other, for instance, in modern English and in Chinese. Of the Indo-Chinese family of languages,—divergent modern forms of a single older speech,—Chinese and Burmese let the modifier precede, Tibetan and Siamese let it follow. In other words, the correspondence of the Ewe word-order with the order of gesture is, for the question in hand, accidental. When Wundt further cites lack of inflection as a primitive characteristic, he runs directly counter to the evidence of all known linguistic history. Wundt further cites the prevalence of what is called sound-symbolism, but this, again, is a feature which we see growing in some highly developed languages, notably German and English,—as Wundt admits (*Elemente*, p. 67). The symbolic words,—such as, for instance, in English, *clash, crack, crunch, sputter, splutter,*—originate by the same processes as other words and can by no means be cited as traces of the birth-hour of language. No objection can be made to the statement that the method of expression in Ewe is highly concrete. It is an accepted doctrine, however,—and one supported by Wundt's own chapter on Semantic Change in the *Völkerpsychologie,*—that abstract expressions develop at need out of concrete, provided the individualization of concepts (cf. below) has once taken place. A language like Ewe has no highly abstract expressions because the people who speak it have no occasion to speak of abstract matters. Should the occasion arise, the words would soon find themselves.[3]

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[2] Finck, *Die Sprachstämme des Erdkreises,* p. 119; cf. also Cust, *The Modern Languages of Africa,* pp. 203 ff.

[3] Cf. especially F. Boas, *Handbook of American Indian Languages,* I, 64 ff.

The source of Wundt's error lies in the fact that, to repeat, his social psychology does not contain in regard to language a view of the general development comparable to that of the other fields of social activity. It would be needless here to give a detailed statement of Wundt's views on points related to this question. Suffice it to say that the rationalizing interpretation, which here also reverses the true course of development, is not entirely overcome in the *Völkerpsychologie.* In its extreme form this interpretation sees 'primitive' language as a system of monosyllabic words, each with a separate conceptual content; in the course of development these gradually merge into an 'agglutinative' state, in which a number of them lose their full conceptual value and become modifying affixes; finally the syllables of such groups lose their formal and semantic identity still more, until polysyllabic inflected words, like those of Latin and Greek, result. As the historically observable course of events is always diametrically opposed to this, the auxiliary supposition becomes necessary that development continues only until a language 'enters into history' (by being recorded in writing), at which point there begins a period of 'decay.' This theory, developed chiefly by August Schleicher, may be seen in its application to the various languages of the world in A. Hovelacque's book, *La linguistique* (fourth edition, Paris, 1888). The selection of Ewe as a typical 'primitive' language,— even though Wundt is far beyond the grossness of such theorizing as I have described,— is nevertheless a reflex of such views. Actually,— that is, wherever the facts are accessible,—language is always seen to develop from longer words to shorter, from words involving more experience-content to words of simpler conceptual value. Or, more correctly, the sentence of imperfectly analyzable associative structure, whose parts merely resemble parts of other sentences, gives place to the sentence fully analyzable into separately apperceptible units (words) which are felt to occur with unchanged identity in other sentences. It is this contrast which really embodies the linguistic phase of Wundt's statement (*Elemente,* p. 73): 'So ist das Denken des Primitiven fast rein assoziativ. Noch ist die vollkommenere Form der Verknüpfung der Begriffe, die apperzeptive, die den Gedanken in ein Ganzes zusammenfasst, nur spurweise in der Verbindung der einzelnen Erinnerungsbilder vorhanden.' We must, accordingly, mark as most primitive those languages in which the sentence scarcely or not at all breaks up into words, but is analyzable only as an associative complex, in the sense that parts of it resemble parts of other sentences. Thus a language in which 'I-cut-bear's-leg-at-the-joint-with-a-flint-now' is a single highly inflected word [4] is a relatively primitive language. By the same token Latin *ambulo, ambulas, ambulat, ambulabam* are as sentences more primitive than, say, the English equivalents *I am walking, you are walking, he (she) is walking, I was walking,* because the English sentences consist of several independent symbols each with conceptual value (words), while the Latin expression views each occurrence as a whole, with only associative indication of resemblance to other occurrences.

The importance of a proper understanding of these things for the mental history of man is not only guaranteed by the function of speech as the substratum of communal mental life, but follows immediately from the nature of our concepts of quality, action, and relation. These, —as no one has, to my knowledge, better described than Wundt (*Völkerpsychologie*[3], I, 2, p. 513 ff.),—depend for their existence upon

[4] R. A. Marrett, *Antropology,* p. 139.

a separately apperceived object of symbolic value which serves as dominant element in the complex forming the concept. This symbol-object is, of course, the word: without it no concept of action, quality, or relation can exist. Hence, without independent words for such ideas, no scientific thought is possible. The central thread of the mental history of man is a development whose most immediate external manifestation is the attainment of linguistic symbols for concepts other than those of objects. L. Lévy-Bruhl, in his *Fonctions mentales dans les sociétés inférieures* (Paris, 1910), has observed the connection between primitive habits of thought and non-isolating habits of speech. So much is certain: no people, so far as is known, has arrived at what may properly be called logical or scientific thought without speaking a language at least as far along toward conceptual expression as Sanskrit, Ancient Greek, or Latin. The 'magic causality' of the savage becomes fully intelligible only when we learn that his thought lacks the linguistic forms which make possible our logic. I shall quote a few passages from the *Elemente* which, now more or less parenthetic, could, by a juster and fuller treatment of the evolution of language, have become integral, and, I venture to think, central motifs of the discussion. Pp. 91 ff., especially p. 93: 'Kausalität in unserem Sinne existiert für den primitiven Menschen nicht. Will man auf seiner Bewusstseinsstufe überhaupt von dieser reden, so kann man nur sagen: ihn beherrscht die Zauberkausalität. Diese aber bindet sich nicht an Regeln der Verknüpfung der Vorstellungen, sondern an Motive des Affekts.' P. 463 f.: 'das Heldenzeitalter . . ., dessen Grundstimmung die Gebundenheit an die objektive Welt ist, in die zwar das Subjekt seine eigenen Gemütsbewegungen hinüberströmen lässt, die es aber niemals von den Objekten zu lösen vermag . . .'

University of Illinois. LEONARD BLOOMFIELD.

The Measurement of Induction Shocks. A Manual for the Quantitative Use of Faradic Stimuli. ERNEST G. MARTIN, Ph. D. New York, John Wiley and Sons, 1912, vii. pp. 117.

This is a collection and systemization of a series of papers published during the last five years. In physiology to-day there is a great deal of work with the artificial stimulation of tissue, and induction shocks are usually used for this purpose. For quantitative work it is necessary to have an exact measurement of the intensity of the shock in order to control one's own experiment or to repeat those of some other investigator. This book is an exposition for the calibrating of induction apparatus so that the value of the shocks may be expressed in stimulation units and so that the calibration can be determined in any ordinarily equipped physiological laboratory. Martin does not present a new method but rather an extension and systemization of other methods of recognized worth.

The factors which may affect the strength of the faradic current are: I. Variations in the primary coil, due to (1) the amount of current yielded by the source; (2) the key whereby the current is made or broken. II. Variations in the secondary coil, due to (1) the position of the secondary with relation to the primary coil; (2) the electrical resistance of the tissue which is being stimulated; (3) the contacts between the stimulating electrodes and the tissue to which they are applied. These factors can all be determined mathematically and a clear and lucid explanation is given of the determinations of these variables. Besides different inductoria present structural differences which may cause variation, due to (1) the dimensions and the number of turns of wire; (2) the presence or absence of the iron core; (3) the difference in physiological shock between the make and break. These are also determinable mathematically and the methods for obtaining these determinations are given. There is also another variable factor considered which, however, is not capable of mathematical determination: the effect on the stimulus of the manner of making and breaking the primary current. Although the effect of this factor may not be calibrated, still rules are given by means of which it may be kept constant.

The author gives a straightforward description of the procedure for making these various determinations with only enough theoretical material so that these procedures may be clearly understood. A short, concise description of the various apparatus and devices used is also included along with very helpful diagrams. The reader need not fear being plunged into a complicated theoretical mathematical discussion as the book succeeds in what it purports to be,—"a manual rather than an exposition of principles."

Clark University. SAMUEL W. FERNBERGER.

The Belief in Immortality and the Worship of the Dead. Vol. I. The Belief Among the Aborigines of Australia, the Torres Straits Islands, New Guinea, and Melanesia. By J. G. FRAZER. The Gifford Lectures, St. Andrews, 1911-1912. London, Macmillan & Co., Ltd.; New York, The Macmillan Co., 1913. pp. xxi, 495. Price, $3,25, net.

Dr. Frazer, who has always been interested in the attitude of primitive peoples to their dead, has here brought together such information as is available upon the subject of his title. The book does not, perhaps, offer much that is new to the student of social anthropology who has followed the course of the science since 1890, or even since 1900; it is rather surprising—and in view of the rapid disappearance of "aborigines" everywhere, it is reassuring—to note how many of Dr. Frazer's references are of quite recent date. The evidence is marshalled, however, with great literary skill; though the task which the author has set himself is purely descriptive, comparison is not altogether lacking; and once in a while we are treated to an excursus such as readers of The Golden Bough have come to expect. Some of the chapters are a trifle gruesome, since primitive man, like the lower and middle classes of more civilized societies, rejoices to manipulate and decorate a corpse; but we have grown used, of late, to plain speech in matters anthropological; and plainness of speech is necessary if we are ever to understand.

Dr. Frazer believes that "the worship of the human dead has been one of the commonest and most influential forms of natural religion, perhaps, indeed, the commonest and most influential of all" (p. 23). The statement contrasts squarely with Eduard Meyer's dictum: "in Wirklichkeit spielt der Totendienst in der eigentlichen Religion bei den meisten Völkern eine sehr geringe Rolle." It is true that Dr. Frazer takes the word "worship" in a wider sense than Professor Meyer; and it is true that in Australia and the Torres Straits he finds "germs" and "elements" of worship rather than worship itself,—in British New Guinea "a real worship of the dead, *or something approaching to it*" (italics mine), in Dutch New Guinea "something which might almost be called a systematic worship of the dead,"—and that only when he reaches New Caledonia does he allow himself the positive statement: "on the whole we may conclude that among the natives there exists a real worship of the dead" (p. 338). Yet the contradiction, if lessened, is still not removed; and it is well to remind ourselves that "the worship of the dead" is a slippery and controversial phrase. Controversial, too, is the question of the relation of magic to religion; Dr. Frazer, as is but natural, sees his own view confirmed in cases where another interpretation appears possible. In such matters, difference of opinion, in the present state of our knowledge, is inevitable, and a clear-cut hypothesis has at any rate the value of a fixed point of rally and attack. I could wish, on the other hand, that Dr. Frazer might some day break loose from associationism, which as psychology is outworn and as theory of knowledge is, I imagine, in no better case. We shall not solve the puzzles of the primitive mind until we approach it by way of a sound general psychology.

The impression which the book leaves is that of the tremendous consequences—moral, social, political, economic—which the belief in human immortality has brought in its train: once again, in the history of science, a remote and curious study proves to be of great "practical" importance. In detail, the volume is full of interesting things. Ghosts in Central Melanesia are "naturally in a dazed state at first on quitting their familiar bodies" (p. 358); and ghosts in the civilized world, if we may trust the mediums, suffer a like disability. Among the Melanesians, again, faith (quite logically) kills as well as cures; the natives of the Banks Islands have invented a portable ghost-shooter,—which sometimes hits the wrong man; and the Fijian, learning that he is the object of "malicious animal magnetism," lies down and dies (pp. 387, 414). Interesting from another point of view are the questions to which, as yet, no answer can be returned: "the whole question of the meaning of burnt sacrifice is still to a great extent obscure" (p. 349); and there are many special (p. 462) and more or less general (p. 428) practices of which the same thing must be said.

The printing of the book is excellent. Aside from a few minor slips in the foot-notes, I have marked only one misprint: *consumeri* for *consumere* on p. 346.

E. B. T.

The New Philosophy of Henri Bergson. By EDOUARD LE ROY. Authorized translation by Vincent Benson. New York, Henry Holt & Co., 1913. pp. v-235.

In the first division of this book, Le Roy discusses the method and the teaching of Bergson, giving a survey of the problems of immediacy, duration, perception, liberty, evolution, consciousness, life, matter, knowledge; while in the second division, he treats each of these problems in a more detailed and critical manner. Le Roy conceives the critic's task to be one of interpretation of the spirit of the work, rather than to be one of mere enumeration of contents. He believes that misunderstandings to be feared, should be pointed out and anticipated. That Bergson believes Le Roy to have accomplished this we learn from a statement, quoted in the introduction, which Bergson made to Le Roy—"Underneath and beyond the method you have caught the *intention* and the *spirit*. . . . Your study could not be more conscientious and true to the original." That Bergson has not over-praised becomes clear upon a reading of this masterful little presentation of Bergson's philosophy, but what is perhaps not made so clear is the proof of Le Roy's statement that the readers of Bergson "will find the curtain drawn between themselves and reality suddenly fall and reality stand forth fully revealed." Rather, we should say, that if the curtain is to fall it will more probably do so after a reading of Le Roy than after reading Bergson. The author regards Bergson's philosophy as effecting a revolution equal in importance to that effected by "Kant or even Socrates." For him this philosophy is not a poetical delusion but the result of exhaustive research. Whether we give or refuse complete or partial adherence to it, we have at least all received from it a regenerating shock. He finds it to be in accord with the tendency of the age to question the justification of a deification of science, to distrust the adequacy of intellectualism and to employ, instead, a method of complete experience. To-day everything is regarded from the point of view of life, and there is a tendency more and more to recognize the primacy of spiritual activity.

The following are the main points of Bergson's philosophy, as interpreted by Le Roy. Common-sense (the result of intelligence "living, working, acting, fashioning, and informing itself") before the rational and perceptive function has emerged, is prepossessed in a practical direction, and accordingly has subjected the facts of primal intuition in the direction of utility. Now, even perception, in the usual sense, means the resolution of a problem, the verification of a theory. But we must so mobilize our perspective faculties that we become capable of following all the paths of *virtual* perception, of which the common anxiety for the practical has made us choose one only. Philosophy must free intelligence from these utilitarian habits by endeavoring at the outset to become clearly conscious of them. In order to come into immediate contact with reality, philosophy must renounce the usual forms of analytic and synthetic thought and achieve a direct intuitional effort. "This does not mean to quit experience, quite the contrary, but to extend it and diversify it by science; while, at the same time, by criticism, we correct in it the disturbing effects of action and finally quicken all the results thus obtained by an effort of sympathy, which will make us familiar with the object until we feel its profound throbbing and its inner wealth." This intuition is not a mysterious, mystical thing; it is a method of going from things to concepts,—of incessantly creating new thoughts and incessantly recasting the old. It is thus in opposition to the cinematographic method of analysis by concepts, which, though it presents ever so large an accumulation of conceptual actions, will never reconstruct the movement itself. There is one case in which this "sympathetic revelation" is almost easy to us,—that is in the case of ourselves. This inner world is one of pure quality. In its true nature it is an uninterrupted flow; it is ceaseless change. It is becoming, progress, growth; it is creative process which never ceases to labor incessantly; it is *duration*. Man is free when his acts proceed from his entire personality. In the study of external nature, also, if we do away with the ready-made concepts of mechanism and finality, we find that vital evolution is a dynamic continuity, a continuity of qualitative progress; it is essentially duration, an irreversible rhythm, 'a work of inner maturation.' By the memory inherent in it, the whole of the past is forever present in it . . . that is, it is experience. It perpetually invents, defying alike anticipation and repetition. The vital impulse consists in a "demand for creation." "Consciousness as the original and fundamental reality, always present in a myriad degrees of tension and sleep and under infinitely various rhythms, is present everywhere. Its effort sends out a current of ascending realization which again determines the counter-current of matter. Thus we should not speak of mind and matter, but rather of spiritualization and materialization, the latter resulting automatically from a simple interruption of the former." Thus, in a word, the philosophy of Bergson is a philosophy of *duration*.

Especially valuable are Le Roy's answers to many of the sweeping criticisms that have been made of Bergson's philosophy. Philosophical intuition is not "aesthetic intuition": it is not a return to Romanticism, but a new logical method. "It does not seek to attain knowledge by renouncing intelligence, placing it under tutelage, or abandoning it to the blind suggestions of feeling and will, but seeks to reinforce intelligence by the initial resources (now represented in instinct), which under the dominance of practical demands it has sacrificed." The philosophy of Bergson does not abolish the problems of morality and of the intellect.

Although a reader can hardly agree with many statements which the author's enthusiasm and complete acceptance of Bergson's philosophy leads him to make, and although he is not bound to accept the statement that this is a new philosophy (for he may rather agree with those who find much reason to term Bergson the "modern Heraclitus"), yet the book as a whole is the most valuable appreciative statement of the spirit and trend of the philosophy, that the reviewer has found in a reading of much of the Bergsonian literature.

Clark University. IVY G. CAMPBELL.

BOOK NOTES

Genetics; an introduction to the study of heredity. By HERBERT EUGENE WALTER. New York, Macmillan, 1913. pp. 272.

This is by far the best summary that has appeared in English or any other language upon the subject from a biological point of view. With the aid of its 72 charts and diagrams, it presents a broad and comprehensive picture of recent studies in heredity, its carriers, variation and mutation, the inheritance of acquired characteristics, the pure line, segregation and dominance, reversion to old types and the making of new ones, blending inheritance, determination of sex, and finally the last two chapters apply these principles to man and treat human conservation. Of course many will wish the author had expanded these latter chapters more. Nevertheless it is necessary to know the basis of the work done on plants and animals and this it is the chief purpose of the author to give.

Über das Studium der Individualität, von A. LASURSKI. Pädagogische Monographien, hrsg. von E. Meumann. Band 14. Leipzig, Otto Nemnich, 1912. pp. 191.

This book, translated from the Russian into the German, which is dedicated to the memory of Dostojewsky, ends with a program for investigating personality in its relations to the environment. The author's key-word is *Neigung* or inclination, which he uses evidently in both a psychic and physical sense. The physiological basis of the soul is given abundant recognition, but this is, after all, a basis for psychological and genetic treatment. The analysis of character is made much of. This work is a résumé of many long articles which the author has published in Russian in the last five years. It is more general and philosophic than W. Stern's "Differential Psychology," but is animated by much the same ideas.

Freud's theories of the neuroses. By EDUARD HITSCHMANN. (Nervous and Mental Disease Monograph No. 17.) New York, Journal of Nervous and Mental Disease, 1913. pp. 154.

This is a very timely translation of an attempt to make a digest of Freud's views upon this subject, and is a compend which will immediately associate itself with the recent conspectus of Brill. He first treats the general theory of neuroses, then follow chapters on the true neuroses, the sex instinct, the unconscious, the dream, hysteria, obsessional neuroses, psychoanalytic method of investigation and treatment, general prophylaxis, application of psychoanalysis; then he gives a chronological review of Freudian writings and concludes with reference to publications in English.

The science of human behavior. By MAURICE PARMELEE. New York, Macmillan Co., 1913. pp. 443.

This book and the author's attitude are interesting and timely. Although not proclaiming himself an adherent of the extreme view of

Watson, he takes a position not unlike his. He begins with the physicochemical basis of behavior, then its anatomical and physiological basis, behavior of the lower animals, tropism, evolution of behavior, of the nervous system, reflex action, localization, instinct, nature of intelligence, consciousness, sensation, attention, feeling, pleasure-pain and emotion as conscious elements, personality, intelligence, consciousness and the nature of mind, beginning of social evolution, insect societies, especially ants, vertebrate societies, factors of social evolution.

Die philosophischen Auffassungen des Mitleids; eine historische-kritische Studie. Von K. Orelli. Bonn, A. Marcus und E. Webers Verlag, 1912. pp. 219.

The writer first gives us an historical account of the conceptions of pity in ancient philosophy, the patristic, scholastic age, and in the sixteenth to the eighteenth centuries, and then treats of the English, French, Dutch, Kant and his followers, discriminating between speculative, systematic, sociological, and evolutionary viewpoints. In the second systematic part he attempts a psychological explanation. It is first as feeling and idea or a complex of ideas, its pleasure and pain attributes and its relations to self, its object, the sufferings of the pitied one, the results of it, its value as a motive and a quietive. Finally as to its ethical evaluation, he treats this from the standpoint of emotionalism, intellectualism, social altruism, individualism, egoism, and distinguishes between organists and mechanists, pessimists and optimists, passivists and activists, dogmatists, sceptics and hedonists and utilitarians, and finally discusses its share in ethics and the metaphysical treatment of it.

A contribution to a bibliography of Henri Bergson. New York, Columbia University Press, 1913. pp. 56.

This bibliography includes 90 articles and books by Professor Bergson, including translations, 417 articles and books about him in 11 different languages. The one translated into most languages is his "Introduction to Metaphysics." This bibliography was compiled by the staff of the library of Columbia under the direction of Professor Dewey, who has a short introduction.

General paresis. By Emil Kraepelin. (Nervous and Mental Disease Monograph Series No. 14.) New York, Nervous and Mental Disease Publishing Co., 1913. pp. 200.

This is an authorized English translation by Dr. J. W. Moore and is one of a series of Nervous and Mental Disease Monographs. We have in English now only the authorized "Introduction to clinical psychiatry," but in the field treated in this volume there have been very important additions to our knowledge of late, although we are far from the solution of the paresis problem. The topics are general symptomatology, clinical forms, postmortem finding, etiology, nature, diagnosis, treatment.

L'Umorismo; note di estetica psicologica. Da Giuseppe Fanciulli. Florence, "La Cultura Filosofica," 1913. pp. 128.

The writer first treats of the humoristical temperament, with something of its material and its psychological characteristics, then of the artist, and finally of contemplation, including the intellectual and the motor factors.

The origin and nature of life. By BENJAMIN MOORE. New York, Henry Holt & Co., n. d. pp. 256.

The writer considers and compares physical and psychical evolution, genesis of electrons, atoms, the cosmic evolution of sense, chemical compounds of the earth, building materials for living matter, evolution of colloids, origin of life and how it came to earth, living organisms at work, waking and sleeping, fatigue and respiration. A very brief bibliography of one page is appended.

Wahrheit und Wirklichkeit. Untersuchungen zum realistichen Wahrheits-problem. Von ALOYS MÜLLER. Bonn, A. Marcus und E. Webers Verlag, 1913. pp. 64.

The author contrasts the idea and the criterion of truth, its conformity and worth theory, and after giving his general results, discusses at some length the possibility of various systems of truth and the reality character of logical laws.

The metaphysics of historical knowledge. By DEWITT H. PARKER, University of California Publications in Philosophy, Vol. 2, No. 5. pp. 103-186. Berkeley, University of California Press, 1913.

After considering the general character of historical knowledge and the nature and possibility of representative knowledge of the past, the author discusses at some length the nature of time, temporal experience, the scope and properties of time, the metaphysical status of the past, the nature of historical truth, historical verification, historical truth and insistence.

Psychologische Untersuchungen, herausgegeben von THEODOR LIPPS. II. Band, 2. u. 3. Heft; Theodor Lipps: *Zur Einfühlung.* Leipzig, Wilhelm Engelmann, 1913. pp. 491.

This is the long promised and long expected work which will be read with the greatest eagerness by those interested in the subjects the author treats. These are first of all *Einfühlung,* first for newly-made objects, second for the determination of objects; then the activity of apprehension, relations, *Einfühlung* and *Urteil,* the ego and its objects, activity, *Einfühlung* and the impression, the empirical deception of optical measurements, *Einfühlung* into free sense appearance, and finally *Einfühlung* in general. The author has indulged to the full his prejudice against any kind of index or introduction, so that the reader has no guide or compass, and, in fact, no orientation save to read the whole book itself.

Grundzüge der Ethik, mit besonderer Berücksichtigung der pädagogischen Probleme. Von ELSE WENTSCHER. Leipzig, B. G. Teubner, 1913. pp. 116.

This little introduction treats first of the derivation of ethical norms, the origin of ethical ideas and the analysis of knowledge, the eudaemonicological process of the foundation of ethics, moral good according to Socrates and Plato, Kant's imperative, psychological analysis of the will activities, problem of freedom, the actualization of ethical ideas in life, and the ethical foundations of pedagogy.

The elements of psychology. By DAVID R. MAJOR. Columbus, Ohio, R. G. Adams & Co., 1913. pp. 411.

This is designed as a first book for the beginner. Instead of being a champion of structural psychology or advocating a psychology in terms of behavior, the writer has chosen a third eclectic course with no special regard to the agreement of topics and matter. He has included a brief description of the structure and function of the nervous system, and leans chiefly on the very diverse authorities of James and Titchener. Such a book as this would have been a godsend to teachers years ago, but to-day, with such a large number of elementary textbooks built upon a plan varying in but really unessential details from this, it is a little difficult to justify its existence; at least the observant reader cannot avoid thinking that the author might more profitably have spent the same time and energy in attempting to do some more special work.

A first course in philosophy. By JOHN E. RUSSELL. New York, Henry Holt & Co., 1913. pp. 302.

This book has been awaited a long time, and will greatly interest all those concerned for the subjects of which it treats. The author has been an active and able teacher of philosophy for nearly a quarter of a century, and he has tried to set forth the results of his thinking in a lucid way in order to encourage students to philosophize for themselves, and has also maintained a commendable degree of impartiality between different schools. The bibliography, too, has been carefully chosen and is admirably fitted for its purpose, and is not too copious. Part first discusses reality, its meaning, nature, the one and the many, the soul and its relation to the body and cosmology; the second, epistemology, takes up the doctrine of knowledge and its meaning, Kant, Royce, pragmatic theory of knowledge, meaning of truth, reality and the object, and finally, objections to the pragmatic theory; the third part is devoted to conduct, the problems of morality and of religion.

Aus dem Seelenleben des Kindes; eine psychoanalytische Studie. Von H. VON HUG-HELLMUTH. Leipzig, Franz Deuticke, 1913. pp. 170 (Schriften zur Angewandten Seelenkunde, hrsg. v. Sigm. Freud. 15. Heft.).

This is an attempt to describe the psychic life of the young child in terms of the Freudian psychology, laying much stress upon the various traits that Freud conceives as polymorphic perversity. The writer has done her task well, but from a very narrow standpoint. Most of her knowledge seems to be derived from Scupin's *"Bubi's erste Kindheit,"* which is a record by days and weeks of the development of a single infant. She has glanced here and there at Compayré and B. Goltz, but few others. While students of childhood will be grateful for her little epitome, it is very evident that there is a vast body of facts entirely outside her purview which bear upon the topic, a great many facts which should be treated from the psychoanalytic standpoint. It is to be hoped that we may have a new edition of the book which will attempt something of this work.

Philosophie des Möglichen. Von Johannes Maria Verweyen. Leipzig, S. Hirzel, 1913. pp. 240.

The writer first considers the value of the possible in education to reality, then the fundamental questions that underlie it, its relations to the freedom of will, the difference between what is thinkable and what is conceivable, the *Verdinglichung* of possibility, the possible and the historic method, the possible in theology and finally in life.

A study in incidental memory. By Garry C. Myers.. (Archives of Psychology No. 26, February, 1913.) New York, Science Press, 1913. pp. 108.

The studies here deal with incidental memory for proportions and areas of well-known objects, incidental memory for words, letter square tests, the watch dial experiment, test of events with dates, rapid estimation of the number of letters in words, incidental memory for extent of motion.

Einführung in die Lehre vom Bau und Verrichtungen des Nervensystems. Von Ludwig Edinger. 2nd enl. and rev. ed. Leipzig, F. C. W. Vogel, 1912. pp. 234.

This second edition has been brought up to date and gives due recognition to the discoveries that have been made since the first edition of 1909. All in all, it is perhaps the best, the most compact, of all the compends upon this subject, with its 176 cuts.

The game of mind; a study in psychological disillusionment. By Percy A. Campbell. New York, Baker & Taylor Co., 1913. pp. 80.

The writer treats seeing, thinking, knowing, feeling, remembering, consciousness, as games in a way rather more unique than clever.

Jahrbücher der Philosophie. V. 1. Edited by Max Frischeisen-Köhler. Berlin, Ernst Siegfried Mittler und Sohn, 1913. pp. 384.

This work is to be a critical survey of present-day philosophy. The present volume contains eleven articles by different writers, the chief of which are the theory of knowledge and the border questions of logic, natural philosophy, the principle of relativity, the problem of time, the philosophy of organic life, the basal questions of psychology, a survey of experimental psychology in 1911 (by Messer), the philosophy of history, aesthetics.

Confession d'un incroyant; document psychologique recueilli et publié avec une introduction, par le Dr. Eugène Bernard Leroy. Paris, Émile Nourry, 1913. pp. 93.

This is an interesting and extremely frank statement covering the four periods of infancy, first communion, adolescence and maturity.

Economics as the basis of living ethics; a study in scientific social philosophy. By John G. Murdoch. Troy, N. Y., Allen Book and Printing Co., 1913. pp. 379.

The chief topics in this work are the economic interpretation of history, ethics and theories of property, ethics and productivity theories of interest, Austrian-Yale theory of interest, interest as exploitation, economics in Kant's ethics, ethics and economic determinism.

Der Traum, psychologisch und kulturgeschichtlich betrachtet. Von RICHARD TRAUGOTT. Würzburg, Curt Kabitzsch, 1913. pp. 70.

The effect of conjugation in paramecium. By H. S. JENNINGS. And *Biparental inheritance and the question of sexuality in paramecium.* By H. S. JENNINGS and K. S. LASHLEY. Reprinted from the Journal of Experimental Zoology, Vol. 14, No. 3, April, 1913. pp. 279-446.

Memory, a contribution to experimental psychology. By HERMANN EBBINGHAUS. Translated by Henry A. Ruger and Clara E. Bussenius. New York, Teachers' College, Columbia University, 1913. pp. 123.

Bewegungslehre, Heft 14/18. VOLKMANN. Charlottenburg 4, Friedrich Huth's Verlag, n. d. pp. 95.

What is new thought? the living way. By CHARLES BRODIE PATTERSON. New York, Thomas Y. Crowell & Co., 1913. pp. 248.

Le basi psicologiche della costituzione. della società. Da GUALTIERO SARFATTI. (Estratto dall Rivista Italiana di Sociologia, Anno 16, Fasc. 5-6, Settembre-Dicembre, 1912.) Rome, Rivista Italiana di Sociologia, Via Venti Settembre, 8. pp. 13.

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Dresden, 20. März, 1913.
Hohe Strasse 24, III.

Sehr geehrte Redaktion!

Das Januarheft Ihrer Zeitschrift enthält eine Rezension meines Werkes: "Lehrbuch der Psychologie," welche mich zu einer Erwiderung notigt. Selbst seit Jahren mit der kritischen Besprechung wissenschaftlicher Werke vertraut, bin ich mir wohl bewusst, dass nicht jeder Tadel eines Buches zu einer öffentlichen Erwiderung berechtigt. Es giebt aber auch hier ein ungeschriebenes Gesetz der Gerechtigkeit und Billigkeit, bei dessen Verletzung dem Angegriffenen das Recht der Verteidigung zustehen muss und das Sie, wie ich hoffe, veranlassen wird dieses Schreiben in Ihrer Zeitschrift zu veröffentlichen. Mein Kritiker hat es sich sehr leicht gemacht. In wissenschaftlichen Kreisen ist es üblich, die Ablehnung eines ernsthaften und wertvollen Forschungswerkes nur so ernsthafter zu begründen, je schroffer sie ist. Mein Kritiker stellt sich ausserhalb dieser löblichen Sitte. Er betont statt dessen die Zugehörigkeit des Verfassers zu einer "technical school," offenbar in volliger Urkenntnis der Technischen Hochschulen Deutschlands, welche die ordentlichen Lehrstühle ihrer humanistischen Abteilungen, wie in meinem Fall, so in der Regel, mit Universitätsdozenten besetzen, und er spricht von des Verfassers Stellung zur experimentellen Psychologie, offenbar ohne die betreffenden Teile des Werkes auch nur oberflächlich gelesen zu haben. Um zu zeigen, wie andere Kritiker urteilen, führe ich den letzten Satz der ersten in einer deutschen wissenschaftlichen Zeitschrift erschienenen Rezension (Literarisches Zentralblatt 1913, Nr. 4) hieran: "Indess sind dies kleine Ausstellungen, die nicht darüber hinwegsehen lassen dürfen, dass hier ein ausgezeichnetes und für den Gelehrten wie den Lehrer in Zukunft einfach unentbehrliches standard work vorlegt.'

Ich hoffe, dass es auch vorurteilsfreie amerikanische Leser geben wird, welche sich diese in der deutschen Wissenschaft bereits anerkannte vollständigste Gesammtdarstellung der Psychologie (kein "textbook" im amerikanischen Sinne) nicht werden entgehen lassen.

In vorzüglicher Hochachtung

PROFESSOR DR. PHIL. THEODOR ELSENHANS.

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THE MEASUREMENT OF ATTENTION[1]

By Karl M. Dallenbach

CONTENTS

I. Introduction

In his *Postulates of a Structural Psychology* (1898) Titchener expressed the opinion that *clearness* is to be considered

[1] From the Psychological Laboratory of Cornell University.

as an intensive attribute of sensation, and that this attributive clearness is the elementary phenomenon of attention.[2] The same view is worked out in detail, and with reference to current theories of attention, in the *Feeling and Attention* of 1908.[3] In the following year, Geissler published "an attempt at a new measurement of attention in terms of clearness values."[4] "A very close parallelism was found to exist between introspectively distinguishable variations of attention and corresponding differences in the precision of work performed at these levels, under the condition that the estimation of degrees of attention was made in terms of clearness and that the work itself was not influenced by anything else but change in attention."[5] The attempt thus yielded a positive result. Geissler, however, worked only with material which was visually presented; and it is clear that the investigation must be carried further into the domains of hearing, touch, and imagery. The present study is an attempt to do for audition what Geissler has done for vision. Since Geissler prefaced his experiments by a fairly full "critical study of previous views and methods,"[6] we may ourselves dispense with any longer introduction. And since this paper deals only with audition, and experiments upon touch and imagery are still to follow, we have thought it best to offer our results by themselves, without criticism of studies which have appeared since Geissler wrote, and without reply to the occasional criticisms of Geissler's method which we have met with. The results which we have obtained confirm those of the previous investigation, so that we find no reason to change our fundamental view of the nature of the elementary phenomenon of attention; and the discussion of minor points of controversy may be postponed until the experimental material covers a wider ground.[7]

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[2] *Philosophical Review,* vii., 1898, 461 f. See also M. W. Calkins, *Introduction to Psychology,* 1901, 137 ff.; I. M. Bentley, *Mind,* N. S. xiii., 1904, 242 ff.

[3] *Lectures on the Elementary Psychology of Feeling and Attention,* 1908, Lects. i., v., vii.

[4] This JOURNAL, xx., 1909, 502 ff.

[5] *Ibid.,* 529. It may be noted that Geissler anticipates an obvious criticism by the statement: "under the same conditions, the introspective [we should prefer to say 'subjective'] estimation of the quality of the work was not as reliable as the evaluation of the degrees of attention."

[6] *Ibid.,* 473 ff.

[7] The writer takes this opportunity to thank Professor Geissler for important suggestions with regard to the method of the experiments reported below.

Observers.—Three observers served in all of the experiments: Dr. W. S. Foster (F), instructor in Psychology, Miss Mabel E. Goudge (G), graduate student in Psychology, and Mr. J. S. Johnston (J), fellow in Psychology. G and J worked without knowledge of our problem. F was familiar with Geissler's investigation, and knew in general the aim of the present study.

II. Methods and Results

A. *Preliminary Training in Introspection*

Our first problem was to familiarise our observers with differences of attributive clearness.

To this end a large number of preliminary experiments was given them. The experiments were of two kinds: with attention directed to the stimulus, and with attention directed away from the stimulus and upon some mental task. In the experiments of the first kind, two metronomes were set going at the rates of 100 and 120 strokes per minute respectively, and the observer, who sat with his back toward them, was instructed to direct his attention to the sounds, and to count the number of sounds between coincident strokes. The observation began with a signal, and ended after a minute and a half, with the word *Introspect.* The observer was then required to describe, in as great detail as possible, the pattern of consciousness during the period of observation. In the experiments of the second kind, the same stimuli were used, but the observer was required to perform some mental task, such as continuously adding, subtracting, multiplying, dividing, reciting, singing, or repeating the alphabet backwards. At the end of a minute, or a minute and a half, the observer was asked, as before, to give as full and complete an introspection as possible. These two kinds of experiments were alternated throughout the preliminary training; four observations were taken in an hour. The observer F gave in all 62 introspections; G, 60; and J, 128. After relatively few trials, the observers were able not only to compare the clearnesses of the stimuli in the two types of experiments, but also to say that the sounds were not always equally clear or obscure during an observation. Thus, F reports, after an experiment in which the task was to add 7 continuously: "The sounds of the metronomes, as a series of discontinuous clicks, were clear in consciousness only four or five times during the experiment, and they were especially bothersome at first. They were accompanied by strain sensations and unpleasantness. The rest of the experiment my attention was on the adding, which was composed of auditory images of the numbers, visual images of the numbers, sometimes on a dark grey scale which was directly ahead and about three feet in front of me. This was accompanied by kinaesthesis of eyes and strains in chest and arms. When these processes were clear in consciousness the sounds of the metronomes were very vague or obscure." Similar reports were made, during the first three weeks of training, by G and J.

When this degree of proficiency had been reached, the observers were asked to estimate, first the larger, and later the finer differences in the clearness of the sounds. They constructed, independently, a rough scale of five or six steps, from very clear to obscure; but they were presently able to assign a percentage value to the clearnesses. At first this gradation was difficult; and the observers, particularly F, felt uncertain of its correctness. As the preliminary training advanced, however, they grew more confident; and toward the end they were able not only to give an analysis of consciousness during the period of an observation, but also to estimate, without difficulty, the clearness or obscurity of the mental processes experienced. At the end of three months, they had worked out (with some suggestion from the experimenter, which, however, bore only upon uniformity of grades) the following scale:

1. 100-90%....................maximally clear.
2. 90-80%....................very clear.
3. 80-70%....................clear.
4. 70-60%....................fairly clear.
5. 60-50%....................fair.
6. 50-40%....................fairly vague.
7. 40-30%....................vague.
8. 30-20%....................very vague.
9. 20-00%....................obscure.

We give a single illustration. F, after an experiment in which the task was to repeat continuously the alphabet backwards, reports: "Repeated alphabet backwards two and a half times. Practice has made this task much easier than at first, so that it is easier for the distraction (the metronomes) to catch attention. Four times for several seconds the sounds of the metronomes were the clearest processes in consciousness, perhaps 75% clear. The repeating process (*i.e.*, the complex of repeating the alphabet backwards, which was composed of visual images of dark grey lines stretching from right to left, eyes following along on lines sometimes with vague visual images of several letters in a group; accompanied by these visual images, or where visual images were lacking accompanied by the kinaesthetic images of eye movements, sibilant auditory images of letters) was only 10-20% clear. Aside from these four times the repeating processes were 85% clear, and the sound of the metronome was 10-15% clear."[8]

At this point we turned to the main problem of measuring attention in terms of clearness; in other words, of discovering how closely the attributive clearness of the processes attended to and of those attended from is correlated with quantitative and qualitative changes of an auditory stimulus of an objectively measurable character.

B. *Preliminary Practice in Observation Under the Conditions of the Main Experiments*

1. *Apparatus.*—Our stimulus, the tone of a Stern variator, varied from 300 to 600 vs. in the 1 sec. The Whipple air-tanks were used to supply the blast. The intensity of the tone was controlled by an air-valve, and the pitch, by the

[8] With this account *cf.* Geissler, *op. cit.*, 510 f.

crank of the variator. Both valve and crank were fitted with large scales and long moving arms, which permitted us to make gross movements in their adjustment.

The Whipple tanks[9] were devised for the purpose of giving a constant air-pressure; but further to insure this result we registered the pressure graphically. Between the air-valve and the variator a T-tube was inserted, the one arm of which led to the variator and the other to a large eosin manometer. A delicate Marey tambour was attached to the manometer, and its writing point rested upon the surface of a smoked drum. When the air-blast was turned on, the pressure was recorded upon the drum. The tanks as set up by Whipple showed a very slight variation, due perhaps to the small size of the intake valve. We therefore disconnected the tanks, and tried them individually. We found that the pressure was now sensibly constant; indeed, between the limits of height 25 and 50 cm., it remained, by our manometer, absolutely the same. During a single experiment the tank was therefore always kept within this central region. Curtains, hung directly over the variator and at other places about the room, eliminated echoes; and thus the constancy of the tone was further insured.

The observer sat about 2.5 m. from the source of sound, his head secured in position by a biting-board, and his right hand resting upon a silent electrical key. In an adjacent room was placed a kymograph with three writing points. One of these points was connected to the observer's key, another to an electrical push-button in the experimenter's hand. The third point, the lever of a Jacquet chronoscope, wrote fifths of seconds between the other two. It was added to give an approximate record of the observer's reaction-times.

2. *Instructions*.—The following instructions were read to the observer:

"You are to sit at the table with your back to the stimulus, your head held firmly in position by the mouth-piece and the biting board, your right arm and hand resting upon the table, and your forefinger, or forefinger and thumb, lightly pressing the electrical key. The experiment will begin upon the signal *Ready, Now,* and will end when the experimenter says *Introspect.*

"The stimulus, which will be the tone of a Stern variator, may vary in intensity or in pitch. The rate of change from one intensity to another, or from one pitch to another, will also vary, the change being made either gradually or very quickly. There are then two points upon which you will report: 1. kind of change, whether of pitch or intensity; and 2. rate of change, whether rapid or slow. As soon as a change is perceived, you will press the key.

"One pressure denotes a change of intensity;

"Two a change of pitch;

"The rate of change must be given in the introspection.

"You are to give your attention to the sound of the variator. At the end of 30, 45 or 60 seconds, as the case may be, you will introspect, and give a detailed description of your consciousness during the experiment."

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[9] G. M. Whipple, this JOURNAL, xiv, 1903, 107 ff.

It will be observed that no mention is here made of the word "clearness." This omission was made advisedly; for we wished, so far as possible, to keep the observers in ignorance as to the subject of the experiment. It is true that *F* knew, in a general way, the aim of this study; but neither he nor the other observers knew the particular phase of the problem that we were studying. We expected, however, in view of the long preliminary training, to receive, without requesting them, detailed introspections upon the relative clearness of the processes in consciousness. In this expectation we were not disappointed; for both G and J continued to estimate the relative clearness of their mental processes in percentage terms, while F, throughout these experiments, used such descriptive terms as very clear, fairly clear, vague, obscure, etc.

In the experiments of 30 seconds, only one or two changes in pitch or intensity were made, while in the experiments of 45 and of 60 seconds, three and four changes in pitch or intensity were made respectively. A 'change' means a variation either of pitch or of intensity; not of both together. The rate at which the change was effected was either rapid or slow. The rapid changes were made as quickly as the adjustments allowed, requiring about one-fifth of a second; the slow changes occupied three seconds.

3. *Series.*—In the series given to the observers all the possible types of change were represented. In the experiments, *e.g.,* in which only one change was made during an observation, the change was in one case of pitch, in another of intensity; in one case it was made rapidly, in another slowly. The standard pitch and intensity were also varied, as was the place of the change within the 30 second interval (near beginning, middle, near end). Again, in the experiments in which four changes were made, the changes of pitch and of intensity occurred an equal number of times in the first, second, third, and fourth places. The rates of change, rapid and slow, likewise occurred an equal number of times in the first, second, third, and fourth places. The series itself varied from four changes of pitch to four changes of intensity.

The series in detail were as follows:

Series I. One change. Duration of experiment 30 seconds.

Variation.	Change of Pitch.	Change of Intensity.	Rate.
1.	0 (350)	1 (l-h)	r.
2.	0 (400)	1 (h-l)	s.
3.	0 (450)	1 (h-l)	r.
4.	0 (300)	1 (l-h)	s.
5.	1 (350-400)	0 (low)	r.
6.	1 (300-400)	0 (low)	s.
7.	1 (500-400)	0 (high)	s.

Series II. Two changes. Duration of experiment 30 seconds.

Variation.	Changes of Pitch.	Changes of Intensity.	Rates.
1.	0 (350)	2 (l-h-l)	s.r.
2.	0 (450)	2 (h-l-h)	s.r.
3.	0 (500)	2 (l-h-l)	r.s.
4.	1 (350-450)	1 (h-l)	s.r.
5.	1 (500-400)	1 (h-l)	r.s.
6.	1 (300-400)	1 (l-h)	r.r.
7.	2 (300-400-500)	0 (high)	r.s.
8.	2 (400-350-300)	0 (low)	s.s.

Series III. Three changes. Duration of experiment 45 seconds.

Variation.	Changes of Pitch.	Changes of Intensity.	Rates.
1.	0 (500)	3 (l-h-l-h)	r.r.s.
2.	0 (400)	3 (h-l-h-l)	s.r.s.
3.	1 (300-400)	2 (l-h-l)	r.r.s.
4.	1 (500-400)	2 (h-l-h)	s.s.r.
5.	1 (450-350)	2 (h-l-h)	s.s.r.
6.	2 (400-350-450)	1 (l-h)	r.s.r.
7.	2 (400-500-450)	1 (h-l)	r.r.s.
8.	2 (400-300-450)	1 (l-h)	s.r.s.
9.	3 (300-400-300-400)	0 (high)	r.s.s.
10.	3 (400-300-400-300)	0 (low)	s.r.r.

Series IV. Four changes. Duration of experiment 60 seconds.

Variation.	Changes of Pitch.	Changes of Intensity.	Rates.
1.	0 (350)	4 (l-h-l-h-l)	s.r.s.r.
2.	0 (400)	4 (h-l-h-l-h)	r.s.s.r.
3.	1 (350-450)	3 (l-h-l-h)	r.s.r.r.
4.	1 (450-350)	3 (h-l-h-l)	s.s.r.s.
5.	1 (500-400)	3 (l-h-l-h)	s.r.r.s.
6.	1 (400-450)	3 (h-l-h-l)	r.s.s.r.
7.	2 (500-400-350)	2 (l-h-l)	s.s.r.r.
8.	2 (350-450-350)	2 (l-h-l)	r.s.s.r.
9.	2 (350-450-500)	2 (h-l-h)	s.r.r.s.
10.	2 (350-400-450)	2 (h-l-h)	s.r.r.r.
11.	2 (500-400-300)	2 (l-h-l)	s.s.r.r.
12.	2 (400-500-400)	2 (h-l-h)	r.s.r.r.
13.	3 (400-300-400-500)	1 (l-h)	s.r.r.s.
14.	3 (300-400-500-400)	1 (l-h)	r.s.s.r.
15.	3 (400-300-400-300)	1 (h-l)	r.s.s.r.
16.	3 (300-400-450-500)	1 (h-l)	s.s.r.s.
17.	4 (450-500-450-400-300)	0 (low)	r.s.r.r.
18.	4 (400-350-450-500-550)	0 (high)	s.r.s.s.

The figures in parentheses under *Changes of Pitch* show the vibration-rates of the tones employed. The letters in parentheses under *Changes of Intensity* show the direction of change, from lower to higher, or conversely. The letters *r* and *s* under *Rates of Change* show the rates at which the changes were effected, *r* signifying *rapid* and *s, slow.*

There were thus 125 changes, 61 of pitch, and 64 of intensity. Of these 125 changes, 62 were made slowly and 63 rapidly.[10]

In the preliminary practice, variations of the above series were chosen at random. For a given experiment, *E* set the variator and the air-valve at the points required. On the signal *Ready, Now, E* released a spring-clip that closed the rubber tube between the air-valve and the variator, and at the same time pressed the push-button held in his hand. At every subsequent change, and at the end of the experiment, *E* again pressed the push-button; so that, by comparison with the time-line and with *O*'s line, the length of the experiment, the times of change, and the time of *O*'s reaction were graphically recorded.

F gave in all 52 introspections; G, 56; and J, 112. This preliminary practice covered the three months February to April, 1912. At its completion we turned to the main experiment, using the Single Task Method.

C. *Single Task Method*

1. *Apparatus.*—The same apparatus was used as for the preliminary practice; but the room, which before was light, was now darkened. The observer was, moreover, enclosed in a muslin booth, which was illuminated from above by an electric light, controlled from the experimenter's desk. The experiments were conducted in darkness, unless flicker was used as a distractor; and the light was turned on at the end of the experiment. *O* was thus able to write his introspections, while *E* marked the record and set the apparatus for the succeeding experiment.

Distractors.—In the preliminary practice the experiments were performed without distraction. In the present series, eight distractors were employed:

1. Flicker (9 rhythms and 4 intensities).
2. Electrical current (3 intensities).
3. Flicker and current (with above variations).
4. Clicks of single metronome (3 rates; 60, 100, and 150).
5. Clicks of metronome and current (with above variations).
6. Clicks, flicker, and current (with above variations).
7. Two metronomes beating at different rates (60, 100, and 150).
8. Phonograph.

(1) *Flicker.*—Behind and somewhat above the observer was placed a second electric light, enclosed within a reflector which directed the light upon a white screen in front of the observer. This light was also controlled from the experimenter's desk; it had four variations in intensity, from very weak to maximally strong. A large cardboard disc., from which were cut four sectors of 30°, rotated before

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[10] The changes are indicated under the separate headings: thus, Series I yields 3 changes of pitch and 4 of intensity; and so forth.

the reflector. Cardboard shields adjustable over the sectors made it possible to obtain nine different rhythms in the flicker. Since there were four variations of intensity, and nine in rhythm of the light, there were altogether 36 variations of the flicker.

The cardboard disc that served as shutter was driven by a motor, which because of its noise was placed in an adjoining room. The motive power was transmitted by a reduction-system of string belts and guttà-percha pulleys to a large wheel, upon the shaft of which the shutter turned. As the light was controlled from the experimenter's desk, this part of the flicker apparatus, since it was noiseless, was started at the beginning of the hour and ran throughout the entire period.

(2) *Current.*—The faradizing current was also controlled from the experimenter's desk. The induction-coil was placed in an adjacent room. The strength of the primary current was governed by a three-way switch connected with a rheostat. This gave three intensities of the induced current. The current ran direct from the coil to the electrodes, which were moistened and applied to the observer's left arm, the one a little below the elbow, and the other at the wrist. During the first few experiments, the electrodes were bound one on each wrist; this plan was, however, abandoned because the strongest current caused a violent contraction of the muscles of the arm and hand, which seriously interfered with the observer's reaction. The contraction was none the less severe after the change to the left arm; but the right hand could now operate the key without hindrance.

(3) *Flicker and current.*—The third distractor was formed by the combination of the first two, and varied within their limits. There were therefore 108 possible variations of this distractor.

(4) *Metronome.*—Two metronomes were placed upon the experimenter's desk, and were there controlled. In the case of the fourth distractor only one metronome was used. The rate of the beat varied from 60 to 100 and 150 strokes in the minute.

(5) *Metronome and current.*—The fifth distractor was formed by the combination of the clicks of a single metronome and the electrical current. It was variable within the limits of these two distractors.

(6) The sixth distractor, formed by the combination of flicker, clicks of a single metronome, and electrical current, varied within the limits of these distractors. In all, 324 variations were possible.

(7) *Two metronomes.*—Both metronomes were set going at different rates, 60, 100 or 150. There were therefore only three variations within this form of distraction.

(8) *Phonograph.*—The phonograph was likwise controlled from the experimenter's desk. Various records were used, including popular and classical pieces, songs, instrumental and band music.

Previous experimenters have found that distractors very soon lose their power of compelling the attention; the observers become habituated. It was for this reason that we selected eight distractors which were capable of variation and extension. During the first few experiments those forms of distraction were employed which we supposed to be the least disturbing. Habituation was in some measure counteracted by increase of intensity, or by change in rhythm of the distractor. Thus in the case of the flicker, and also of the electric current, the weaker intensities were used first, the stronger later.

2. *Instruction.*—The general instruction remained unchanged. The specific directions were as follows:

"The stimulus, which will be the tone of a Stern variator, may vary in intensity or in pitch. The rate of change from one intensity to another, and from one pitch to another, will also vary, the change being made either gradually or very quickly. There are then two points upon which you will report: 1. kind of change, whether pitch or intensity; and 2. rate of change, whether rapid or slow. As soon as a change is perceived you will press the key.

"One pressure denotes a change of intensity;

"Two a change of pitch;

"The rate of change must be given in the introspection.

"You are to give your attention to the sound of the variator, and to neglect as far as possible any distraction, purposive or accidental. At the end of 30, 45, or 60 seconds, as the case may be, you will introspect. In the general description of consciousness, in previous experiments, you have among other things assigned clearness values to the various processes reported. You are now to estimate clearness values only: that is, you are to report, using a scale of 100, the relative clearness of the processes which you observe in consciousness. For example: 'First change, rapid, clearness of so-and-so $x\%$, of so-and-so $y\%$; second change, slow, clearness of so-and-so $m\%$, of so-and-so $n\%$.'"

3. *Number of experiments.*—The experiments by this method were conducted in May and June 1912, and in February and March 1913. Each observer gave in all 86 introspections.

4. *Series.*—The series employed were those outlined above. The order of presentation was determined by lot. Each series was employed twice. Five or six observations, one of which was taken as a control without distraction, were made during the hour. The experiment without distraction occurred an equal number of times in the first, second, ... and sixth places. The distractions were also so distributed that each kind occurred but once in an hour, and as often in the first as in the other positions. During a single experiment the same distractor persisted without change.

5. *Results.*—The Single Task Method was employed, as we have said under 3 above, at two periods. During the interval the Double Task Method was employed. The results of G and F for the two periods of the Single Task Method

agree throughout, and are therefore grouped together. J, however, gave different results in the two periods; and they are therefore considered separately. J_1 denotes the results of the first, and J_2 those of the second period. The observer had for some time been depressed by a visual disability which oculists had failed to overcome, and in the first period was anxiously awaiting the outcome of a new treatment; this is probably the ground of the incapacity for high degrees of attention shown under J_1 in Tables I and II. In the second period, the reason for depression had been removed. It is possible, also, that the practice in concentration gained during the rather exacting observations of the Double Task Method helped the observer to give the improved results under J_2. We cannot offer more than this general explanation of the discrepancy.

In some of the experiments, from the nature of the series, one change was made, in others two, three, and four. In working over the results, each change was considered separately, was judged as a single case and so recorded. Hence in Table I only the *number of such cases* is given. Under *Right* are grouped all the right judgments of kind and rate of change, and under *Wrong* all the wrong judgments. The scale of attention in terms of clearness is arranged above.

TABLE I

NUMBER OF CASES, KIND AND RATE OF CHANGE

Report	Change	O.	Right						Wrong					
			10-9	9-8	8-7	7-6	6-5	5-4	10-9	9-8	8-7	7-6	6-5	5-4
KIND AND RATE	Ps	F	33	14	2									
		G	2	22	9	5	2			2				
		J_1	2	5	5	3		1						
		J_2	9	9	1	1			1		2			
	Pr	F	32	5										
		G	3	18	11	8	1				2			1
		J_1		2	4	1	2							
		J_2	18	5	4	2	1							
	Is	F	13	11										
		G	2	19	6	2		1						
		J_1		3	6	1	2							
		J_2	4	5	3	2	1							
	Ir	F	27	10										
		G	2	13	15	3	3							
		J_1		2	7		2	1		1	1		1	
		J_2	6	3	1						1		1	

TABLE I—*Continued*

Report	Change	O.	Right 10-9	9-8	8-7	7-6	6-5	5-4	Wrong 10-9	9-8	8-7	7-6	6-5	5-4
Kind	Ps	F	1	2										
		G		1	1	4	1			1	3	4	1	
		J_1		1		2	2					2		
		J_2;	2		2	1			1	1				
	Pr	F	4	9	1				1					
		G		2	1	4				1	2	3	1	
		J_1		1	1	6	2	1		1				
		J_2	1	3	2	1								
	Is	F	2	4	1					4				
		G		2	6	3	1					1		
		J_1		1		2	1	2					1	
		J_2	2	1										
	Ir	F	3	6	3				1	3		1		
		G		3	7	3	2					1		
		J_1		1	7		3							
		J_2	3	4	3		1	1	1					
Rate	Ps	F							1	2				
		G		1	3	4	1			1	1	4	1	
		J_1				2				1		2	2	
		J_2	1	1					2		2	1		
	Pr	F	1						4	9	1			
		G		1	2	3	1			2	1	4		
		J_1		1						1	1	6	2	1
		J_2							1	3	2	1		
	Is	F		4					2	4	1			
		G				1				2	6	3	1	
		J_1					1			1		2	1	2
		J_2							2	1				
	Ir	F	1	3		1			3	6	3			
		G				1				3	7	3	2	
		J_1								1	7		3	
		J_2	1						3	4	3		1	1

TABLE I—*Continued*

Report	Change	O.	Wrong 10-9	9-8	8-7	7-6	6-5	5-4
SUBJECTIVE REACTION	Ps	F	1	2				
		G				1		
		J_1						1
		J_2						
	Pr	F		2				
		G						
		J_1						
		J_2						
	Is	F	1	2	1			
		G				2	1	1
		J_1			1		1	
		J_2						
	Ir	F	1	8	1			
		G			3			1
		J_1						
		J_2						
NO REACTION	Ps	F						
		G						
		J_1						
		J_2				1		
	Pr	F						
		G						2
		J_1						
		J_2						
	Is	F	1	4	3	2	1	1
		G				8	6	4*
		J_1				1	2	1
		J_2		1	1	11		1
	Ir	F		1	1	1	1	
		G				3	4	1
		J_1					1	
		J_2				6	3	

* These four cases occurred in the region 4-3 (40-30% of clearness).

The observers' reports may, however, be grouped under five heads: 1. reports in which kind and rate of change are both right or wrong; 2. reports in which kind is judged aright, and rate is wrong; 3. reports in which rate is judged aright and kind is wrong; 4. reports of change in the absence of objective change (subjective reactions); and 5. reports which failed to note an objective change. Under each one of these captions the data are further analysed, according as the objective change is of pitch or intensity, and is made rapidly or slowly. *Ps,* then, means that the objective change was a slow change of pitch; *Pr,* a rapid change of pitch; *Is,* a slow change of intensity; and *Ir,* a rapid change of intensity. In the case of the subjective reaction, the analysis depends upon the rate and kind of change reported by the observer.

In Table II the right cases, the wrong cases and the failures are grouped according to the kind and rate of the objective change. The subjective reactions are grouped as they were reported by the observer. A summary of the entire number of cases appears at the end of the table. It is clear that the greatest number of right cases occurs with the higher degrees of attention, and that the greatest number of wrong cases occurs from one to two steps lower upon the scale of attention. That this ordering is due to attention, and that it is not the result of any specific reaction to one kind or one rate of change, can be seen by referring to the separate captions of the tables. The relation of the right cases to the wrong cases for all changes, whether of pitch or intensity, whether rapid or slow, is remarkably constant.

TABLE II

Number of Cases Grouped According as the Change is of Pitch or Intensity, and is Rapid or Slow, with Summary

Kind or Rate	Report	O.	Clearness						
			100-90	90-80	80-70	70-60	60-50	50-40	40-30
PITCH	Right	F	70	30	3				
		G	5	43	22	21	4		
		J_1	2	9	10	12	6	2	
		J_2	30	17	9	5	1		
	Wrong	F	1						
		G		4	7	7	2	1	
		J_1		1		2			
		J_2	2	1	2				
	Subjective	F	1	4					
		G					1		
		J_1						1	
		J_2							
	No-reaction	F							
		G						2	
		J_1							
		J_2				1			

TABLE II—*Continued*

Kind or Rate	Report	O.	Clearness 100-90	90-80	80-70	70-60	60-50	50-40	40-30
INTENSITY	Right	F	45	31	4				
		G	4	37	34	11	6	1	
		J_1		7	20	3	8	3	
		J_2	15	13	7	2	2	1	
	Wrong	F	1	7		1			
		G				2			
		J_1		1	1		2		
		J_2	1		1		1		
	Subjective	F	2	10	2				
		G			3	2	1	2	
		J_1			1		1		
		J_2							
	No-reaction	F	1	5	4	3	2	1	
		G				11	10	1	4
		J_1				1	3	1	
		J_2		1	1	17	3	1	
SLOW	Right	F	61	18		1			
		G	4	42	18	12	3	1	
		J_1	2	9	11	6	2	1	
		J_2	14	15	4	3	1		
	Wrong	F	7	15	4				
		G		5	7	7	2		
		J_1		2		4	3	2	
		J_2	5	1	4	1			
	Subjective	F	1	10	1				
		G				2	2	1	
		J_1			1		1	1	
		J_2							
	No-reaction	F		1	1	1	1	1	
		G				8	6		4
		J_1				1	2	1	
		J_2		1	1	12		1	

TABLE II—*Continued*

Kind or Rate	Report	O.	Clearness 100-90	90-80	80-70	70-60	60-50	50-40	40-30
RAPID	Right	F	46	29	2				
		G	5	32	28	15	5		
		J1		4	11	1	5	1	
		J2	25	8	5	2	1		
	Wrong	F	3	6	1				
		G		5	10	7	2	1	
		J1		3	9	6	6	1	
		J2	4	7	6	1	2	1	
	Subjective	F	2	4	1				
		G			3			1	
		J1							
		J2							
	No-reaction	F	1	4	3	2	1	1	
		G				3	4	3	
		J1					1		
		J2				6	3		
SUMMARY	Right	F	222	108	9	1			
		G	18	154	102	59	18	2	
		J1	4	29	52	22	21	7	
		J2	84	53	25	12	5	1	
	Wrong	F	12	28	5	1			
		G		14	24	23	6	2	
		J1		7	10	12	11	3	
		J2	12	9	13	2	3	1	
	Subjective	F	6	28	4				
		G			6	4	4	4	
		J1			2		2	2	
		J2							
	No-reaction	F	2	10	8	6	4	2	
		G				22	20	6	8
		J1				2	6	2	
		J2		2	2	36	6	2	

Inspection shows, further, that the subjective reactions occur for the most part under a relatively high degree of clearness, and conversely that the failures, the no-reactions, occur under a relatively low degree of clearness. We may infer that a subjective reaction is, under our conditions, a less serious error than a no-reaction. Emphasis was laid, throughout the experiment, upon reaction to change of the tone of the variator, and the observer was 'set' for change; it is consequently but natural that he should sometimes 'imagine' a change; whereas failure to notice a change objectively presented argues a definite lapse of attention.[11] In view of these considerations, we have ventured to 'weight' our results as follows:

A right or wrong judgment of Kind and Rate counts as.... ±2.0
A right or wrong judgment of Kind counts as............ ±1.0
A right or wrong judgment of Rate counts as............ ±1.0
A Subjective reaction counts as........................ —2.0
A No-reaction counts as................................ —2.5

In Table III the results appear as thus weighted.

TABLE III
WEIGHTED SUMMARIES

O.		Clearness						
		100–90	90–80	80–70	70–60	60–50	50–40	40–30
F	Total right Total wrong	222.0 20.5	108.0 68.5	9.0 19.0	1.0 8.5	 5.0	 2.5	
G	Total right Total wrong	18.0	154.0 14.0	102.0 30.0	59.0 54.5	18.0 35.0	2.0 13.5	 10.0
J_1	Total right Total wrong	4.0	29.0 7.0	52.0 12.0	22.0 14.5	21.0 20.5	7.0 7.5	
J_2	Total right Total wrong	84.0 12.0	53.0 11.5	25.0 15.5	12.0 47.0	5.0 10.5	0.5 3.5	

The crest of the curve of right judgments now falls, with F, 1 place; with G, 2 places; and with J, 2 and 2 places respectively to the left, that is, above the crest of the curve of wrong judgments; and we have evidence, once more, that introspectively distinguished variations of clearness are closely paralleled by corresponding differences in the accuracy of the judgments passed.

The relation of the report to the kind and rate of the objective change appears in Table IV.

[11] Of the 1400-odd cases reported in these tables only 61 were subjective. We regard this small number (about 4%) as evidence both of the quality of our observers and of the accuracy and reliability of our apparatus and method.

TABLE IV

RELATION BETWEEN THE OBSERVER'S REPORT AND THE KIND AND RATE OF OBJECTIVE CHANGE, EXPRESSED IN NUMBER OF CASES

O.	Kind								Rate							
	Pitch				Intensity				Slow				Rapid			
	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.
F	103	1	5		80	9	14	16	77	10	7	12	80	26	12	4
G	95	21	1	2	93	2	8	26	80	21	5	18	85	25	4	10
J_1	41	3	1		41	4	2	5	31	11	3	4	22	25		1
J_2	62	5		1	40	3		23	37	11		15	41	21		9
Total	301	30	7	3	254	18	24	70	225	53	15	49	228	97	16	24

R., right report. S., subjective report.
W., wrong report. N., no report.

Here we see, in spite of individual differences, first, that a change of pitch, under the conditions of our experiment, is more compelling than a change of intensity; and, secondly, that a rapid change of stimulus is more attractive than a slow change.

Our observers are more correct as regards changes of pitch than as regards changes of intensity. F, J_1, and J_2 report more errors for intensity than for pitch. G, on the other hand, reports more errors for pitch than for intensity. This difference, however, is more than offset by the greater number of cases in which she failed to observe a change of intensity. A like failure to apprehend changes of intensity is evinced by F, J_1, and J_2. F and J_1 noted "change" for every variation in pitch, and J_2 for every variation but one; while they failed to observe "change" in 16, 5, and 23 cases, respectively, of change in intensity.

Slow changes are, as Stern has noted, "less likely than rapid to cause a reaction of attention." This rule is borne out, in a measure, by our results. The objective change was neglected 44 times when it was made slowly, and only 24 times when it was made rapidly. Moreover, the change when rapidly effected was reported correctly 228 times, and when slowly effected, 225 times. On the other hand, more wrong reactions were made to rapid changes (97 reported as slow) than to slow (53 reported as rapid). The law, however, rests upon a fairly large body of experimental results.[12]

(1) *The promptness of voluntary action, i. e.,* the time of a simple reaction, was first used to express degrees of attention by Obersteiner in 1879.[13] He employed two distractors, an induction current and a musical box, and found that the observer's reaction was slower under distraction. He therefore assumed that "this retardation stands in inverse proportion to the intensity of attention." "The differences in the reaction period," he continues, "which may serve directly as the measure of attention, vary in different individuals, and in the same individual under different conditions."[14] Many investigators[15] have used the reaction method to measure the attention. Their results show, for the most part, that reaction-time increases with distraction, though there are those who deny the correlation. Cattell,[16] for example, found that there is no corresponding lengthening of the reaction-time with reduction of attention; and Geissler writes that "the final outcome of the reaction experiments, as used for the measurement of attention, has been on the whole negative; it has been impossible to establish a positive correlation between high degrees of attention and short reactions, and between lower degrees and correspondingly lengthened reactions."[17] In his own experiments, however, Geissler found a remarkably high correlation between "the observers' estimates of attentive concentration and the calculated quickness and accuracy of their results,"[18] and remarks that "with all three observers there is a perfect correlation between their best attention and their shortest time, and between correspondingly lesser degrees and longer times."[19]

Our apparatus was arranged, as we have said, to measure roughly the reaction-time of our observers. The average and the mean varia-

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[12] G. E. Müller, *Zur Theorie der sinnlichen Aufmerksamkeit,* 1873, 125 ff.
A. Pilzecker, *Die Lehre von der sinnlichen Aufmerksamkeit,* 1889, 20 f.
W. James, *Principles of Psychology,* I., 1890, 416 f.
L. W. Stern, *Psychologie der Veränderungsauffassung,* 1898, 211 ff.
W. B. Pillsbury, *Attention,* 1908, 30.

[13] H. Obersteiner, *Brain,* I, 1879, 439-453.

[14] *Op. cit.,* 444.

[15] G. Buccola, *Rivista di filos. scientif.,* I, 219 ff.
G. S. Hall, *Mind,* VIII, 1883, 170-182.
W. Wundt, *Grundz, d. physiol. Psychol.,* I, 1874, 749 f.; II, 1887, 293 f.; III, 1903, 441 ff.
E. J. Swift, this Journal, V, 1892, 1-19.
W. James, *Principles of Psychology,* I, 1890, 425, 427-434.
S. E. Sharp, this Journal, X, 1899, 356.
A. Kästner and W. Wirth, *Psychol. Stud.,* III, 1907, 361-392; IV, 1908, 139-200.

[16] J. McK. Cattell, *Mind,* XI, 1886, 242.

[17] *Op. cit.,* 498.

[18] *Ibid.,* 508.

[19] *Ibid.,* 514.

tion of the reaction-times in the total number of experiments,[20] irrespective of the rightness or wrongness of the judgments, appear for each observer in Table V.

TABLE V

AVERAGE REACTION-TIME IN SECONDS AT THE DIFFERENT LEVELS OF ATTENTION

Observer	Clearness											
	100–90		90–80		80–70		70–60		60–50		50–40	
	Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.
F	.9	.48	1.1	.55	2.0	1.1	1.0	.0				
G	.5	.3	1.9	.93	2.0	1.03	1.9	1.05	2.1	1.07	4.0	.0
J_1	.3	.1	1.1	.4	1.3	.9	1.3	.45	1.8	.66	1.6	.8
J_2	1.5	.7	1.8	.8	1.8	.83	2.3	1.2	2.3	1.9	2.6	.0

Av., average reaction time. m.v., mean variation.
Where the mean variation is given as 0, only one experiment occurred under that rubric.

There is evidently a positive correlation between attention, introspectively estimated in t rms of the attributive clearness of mental processes, and rate of reaction. In the whole table there are but four cases in which a lower degree of clearness gives a shorter average time than the next higher degree. Three of these may be summarily dismissed because they are the averages of too few cases: 1, 5, and 1 respectively. The fourth case, that under the fourth rubric of G, cannot be so disposed of. Neither can we consider it as an effect of practice, for this was minimized by the long preliminary series, while the several component reactions were also well distributed throughout the experiment. It is, perhaps, worthy of note, that the mean variation is high. In any case the exception is not of sufficient weight to affect seriously the coefficient of correlation as figured by Pearson's familiar "product moments" method.[21] Calculation yields the following results:

O	Correlation[22]	P.E.
F	0.95	0.038
G	0.76	0.118
J_1	0.94	0.032
J_2	0.98	0.048

This correlation is surprisingly high, when it is remembered that no emphasis was laid, in our instructions, upon the observers' reaction.

[20] The subjective reactions and the failures to react, *i. e.*, the "no-reactions," of course are not considered here; neither case gives a reaction-time.

[21] G. M. Whipple, *Manual of Mental and Physical Tests*, 1910, 27 f.

[22] In computing this correlation, the data under the fourth rubric for F, and the sixth rubric for G, J_1, and J_2, were omitted, for the reason that they represent too few cases: 1, 1, 5, and 1, respectively.

Moreover, no one of our observers, so far as we know, was aware that the reactions were being measured.[23]

(2) *The mean variation* has frequently been proposed as a measure of attention. A small mean variation would correspond to a high degree, and a large variation to a low degree of attention. Obersteiner was probably the first to suggest this correlation. He showed not only that the reaction-times were longer, but also incidentally that the mean variation was greater, in experiments made under distraction.[24] Later, Friedrich remarks: "It is tempting to fix definitely the somewhat unsettled concept of attention by making it proportional to the measure of precision, *i. e.,* to the reciprocal value of the average error, so that a small average error should correspond to a high degree of attention and, conversely, a large average error to a low degree of attention."[25] Although his results show close agreement with theory, he nevertheless is careful in his interpretation of them. He thinks that only "in the case of the simplest mental processes, which are as homogeneous as possible and but little subject to practice, may one assume that the average error is essentially dependent upon the degree of attention." Other authors, however, have been less cautious, and have insisted that attention may be measured by mean variation.[26]

Our own results tend to confirm this view. The increase of the m. v. in Table V is not due in any measure to fatigue or practice. It is not due to fatigue, for only five experiments of 30 to 60 seconds were conducted during an hour; and it is not due to practice, for this was raised to a maximum by the long practice series. In the entire table, with the exception of the levels at which only one experiment is reported, there are but two cases where a lower degree of attention has a smaller m. v. than the next higher. The mean variations for F and G increase uniformly with decrease of the attentive level. Those for J_1 and J_2 are less regular. Nevertheless, the lower degrees of attention still show a greater m. v. We seem justified then, even on Friedrich's principles, in drawing the conclusion that degree of attention can be introspectively estimated in terms of clearness.

(3) The *relation of the reaction-times to the kind and rate of the objective change* appears in Table VI.

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[23] In our case, therefore, the reactions were not known to be reactions, *i. e.,* were not made under the *Aufgabe* of reaction. It is possible that a "reaction" of this sort indexes attention, whereas a formal and set reaction, so understood by the reactor, is too complex a matter to serve as an index.

[24] *Op. cit.,* 446, 447.

[25] M. Friedrich, *Philos. Stud.,* I, 1883, 73.

[26] H. Griffing, this JOURNAL, VII, 1895, 235.
A. Oehrn, *Psychol. Arbeiten,* I, 1895, 92 ff.
V. Henri, *L'année psychol.,* III, 1896, 245.
J. J. van Biervliet, *Journ. de Psychol.,* I, 1904, 230.
A. Binet, *L'année psychol.,* XI, 1905, 71.
W. Peters, *Arch. f. d. ges. Psychol.,* VIII, 1906, 403 ff.

TABLE VI

RELATION BETWEEN THE REACTION-TIME IN SECONDS AND THE KIND AND RATE OF THE OBJECTIVE CHANGE

Observer	Kind				Rate			
	Pitch		Intensity		Slow		Rapid	
	Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.
F	.80	.28	1.27	.76	1.30	.60	.76	.42
G	1.70	.79	2.40	2.14	2.50	1.13	1.50	.75
J_1	1.18	.37	1.50	.58	1.60	.63	1.00	.35
J_2	1.41	.72	2.45	1.21	2.56	1.08	1.18	.56

Av., average reaction-time. m.v., mean variation.

The average reaction-time to a change of pitch is, under the conditions of our experiment, much less than that to a change of intensity; and, further, the average reaction-time to a rapid change is shorter than that to a slow change. This relation is constant for all observers. There is, to be sure, great individual variation, but this tallies in general with the character and quality of the observer's report. F, *e. g.*, gives an exceedingly low reaction-time, but, on the other hand, he makes few errors, and his attention is most frequently judged to be of the highest degree.

(4) A comparison of Tables IV and VI shows that there is a close *relation between reaction-time and accuracy of report; i. e.*, the shorter the reaction-time, the greater is the probability that the report is correct. Of the kinds of change, pitch has the shorter reaction-time, and also the greater number of right cases and the smaller number of failures. Of the rates of change, the rapid has the shorter reaction-time and also the greater number of right cases and the smaller number of failures. It would seem then that there is a direct relation between the quality and character of the report and the reaction-time.

This relation becomes apparent at once if the reaction-time is compared directly with the reports. Table VII shows the average

TABLE VII

THE REACTION-TIME IN SECONDS AND THE MEAN VARIATION OF THE RIGHT AND THE WRONG REPORTS

Observer	Right		Wrong	
	Av.	m.v.	Av.	m.v.
F	.93	.50	1.2	.63
G	1.96	1.06	2.3	1.23
J_1	1.28	.62	1.45	.60
J_2	1.71	.81	1.80	1.05

Av., average reaction time. m.v., mean variation.

and mean variation of the reaction-times for both the right and the wrong cases. It confirms the conclusions drawn from the comparison of Tables IV and VI.

The average reaction of the right reports for all of our observers is shorter than that of the wrong reports. While the difference is very small for J_2, it is so marked in the cases of J_1, and especially of G and F, that we may conclude with Whipple[27] that there is a close correlation between rate of judgment and character and quality of report.

(5) The *effect of the distractors* is shown in Table VIII. The average clearness of the focal processes (the sounds of the variator), its mean variation, and the number of cases are given for each distractor.

TABLE VIII

THE AVERAGE CLEARNESS OF THE AUDITORY SENSATION AS AFFECTED BY THE DISTRACTORS

Observer		Distractor								
		0	1	2	3	4	5	6	7	8
F	Av.	89.5	85.7	88.6	86.4	85.8	87.5	88.8	87.0	77.5
	m.v.	3.6	4.4	3.9	3.6	7.3	5.3	4.1	3.8	12.1
	No.	38	22	21	15	25	24	30	27	26
G	Av.	85.0	73.2	80.3	70.6	76.0	65.8	71.0	74.5	57.8
	m.v.	4.9	8.3	3.3	7.6	7.4	7.3	6.5	8.9	8.8
	No.	29	23	31	27	25	31	23	25	34
J_1	Av.	73.7	72.0	58.7	62.1	70.0	63.3	58.0	67.6	54.0
	m.v.	5.1	4.8	13.1	9.1	5.1	9.0	8.2	9.0	8.7
	No.	14	10	8	13	8	7	14	14	9
J_2	Av.	94.0	84.0	77.7	80.0	74.0	79.6	75.7	73.0	60.0
	m.v.	1.5	8.0	11.8	9.0	9.0	10.5	8.5	10.5	14.6
	No.	11	15	9	20	18	15	17	13	15

Av., average clearness of tone. m.v., mean variation. No., number of cases. 0, normal conditions. 1, flicker. 2, current. 3, flicker and current. 4, single metronome. 5, metronome and current. 6, metronome, flicker and current. 7, two metronomes. 8, phonograph.

[27] G. M. Whipple, this JOURNAL, XII, 1900-01, 433 ff. Whipple gave his observers two successive tones, the second of which varied between the relations equal, greater, or less by eight vibrations per second from the first. The second tone was judged in terms of the first. He divided the observers' judgments into three classes according as the report was immediate, slow, or deliberate. He defined the "immediate" as the judgment made without conscious comparison; the "slow," as the judgment made with conscious comparison; and the "deliberate," as the judgment in which the decision was the result of internal debate. He found that a much greater percentage of the immediate judgments was correct than of the slow or the deliberate.

It will be seen that the number of cases under the several distractors varies. This fact appears to confute the statement (made in the discussion of the method) that the distractors were used an equal number of times. But the discrepancy is explained by the nature of the series. In some, only one change was made, in others 2, 3, and 4. The distractors were to be employed an equal number of times, as many times first as last, etc., so that their order had to be prearranged. The order of the series, however, was not predetermined. Though every series was used an equal number of times, choice was made at haphazard. The series with the large number of changes thus accidentally fell to some distractors more frequently than to others. Still, on the whole, the variation in number of cases is slight and of little consequence.

There is wide variation in the effect of the distractors both upon the individual observers and upon the same observer at different times. F was little affected by any distractor save the phonograph. This result agrees closely, as we have said, with the general character and quality of his work. His attention during the experiments was higher, he made fewer errors, the reaction-time of his judgments was quicker, the mean variation smaller, than those of either of the other observers. The phonograph served to distract his attention; but even with it attention might be as high as under normal conditions. He could disregard it provided that unfamiliar pieces were played. When familiar records were played, his attention was "compelled" by them, he was "unable to attend from" them.

The phonograph proved to be the most efficient distractor for all the observers; and attention under normal conditions without distraction proved to be highest for all observers. This is shown in Table VIII; and also, perhaps more clearly, in Table IX, in which the effectiveness of the distractors is arranged in ascending order from least effective (normal conditions) to most effective (phonograph). In other respects, however, there is little or no agreement. G, J_1, and J_2 show, unlike F, a wide distribution of effectiveness; while F's average attention for all distractors, with the exception of the phonograph, lies within four degrees of clearness and is very nearly as high as under normal conditions.

TABLE IX

ORDER OF THE EFFECTIVENESS OF THE DISTRACTORS FROM LEAST TO GREATEST

Observer	Order								
	1	2	3	4	5	6	7	8	9
F	0	6	2	5	7	3	4	1	8
G	0	2	4	7	1	6	3	5	8
J_1	0	1	4	7	5	3	2	6	8
J_2	0	1	3	5	2	6	4	7	8

0, normal. 1, flicker. 2, current. 3, flicker and current. 4, single metronome. 5, metronome and current. 6, metronome, flicker and current. 7, two metronomes. 8, phonograph.

That the effect of the distractors is different for the same observer at different times is shown by J. Though his results for the two periods of the experiment show a gradual change in the effectiveness of the distractors, maximal attention under normal conditions, and minimal attention under the distraction of the phonograph, they can in no other detail be compared. In the first period the degrees of clearness of the focal processes ranged from 73.7 to 54.0; in the second period they ranged from 94.0 to 60.0. An explanation has been suggested above.

Table IX shows, further, that there is no uniform gradation of the distractors. Thus the flicker (no. 1), which next to the phonograph proved to be most effective for F, was only moderately effective for G, and had the least effect of all upon J. And so on with the rest.

It has usually been supposed that attention is best under slight distraction. Titchener says: "It has been shown experimentally that we attend best under a slight distraction,"[28] and Geissler finds that "the results of the second group of experiments showed plainly that even the most complex combinations of distractors, after a few days' work, were unable to induce great variations of attention. Instead, toward the end of the whole group, most of the normal series were actually performed at a slower rate than the distraction series."[29] Hamlin also gives a similar report. She used adding as a distractor, and remarks that "the subjects usually found that it acted as a spur rather than as a check to the attention."[30] Our results show, on the contrary, that attention is best under normal conditions; the distractors lower the attention. The advantage of the normal series is, in the case of F, not very great; but it is uniform, and in the cases of the other observers well-marked. Under the conditions of our experiment, therefore, the observers attended best under normal conditions.

Owing to the precautions that we had taken, the effect of *practice* and *habituation* in these experiments was practically negligible. In the first place, the work was divided into two parts, separated by a period of nine months; in the second place, a large number of distractors were employed, and these were capable of wide variation in intensity and in rhythm; and thirdly, as the experiments advanced, the intensity and rhythm of the distractors were proportionally increased and complicated. Habituation was therefore reduced to a minimum.

In choosing the distractors, our ideal was that of Drew, "to arrange a series of tasks of increasing degrees of complexity which should from the normal make ever greater demands on the mind until the attention should pass from a fully concentrated to a completely distracted state."[31] This is the principle laid down by Stumpf in his *Tonpsychologie*,[32] and by Titchener in his *Psychology of Feeling and Attention*.[33] The results show that we were not successful in obtaining such a series of graded distractors. There appear to be four main reasons. (1) A change in the stimulus may cause the corresponding conscious processes to rise *involuntarily* in clearness. It "catches" the attention. The tone may be comparatively obscure

[28] *Psychol, of Feeling and Attention*, 1908, 203.
[29] *Op. cit.*, 513.
[30] A. J. Hamlin, this Journal, VIII, 1896, 49.
[31] F. Drew, this Journal, VII, 1895, 533.
[32] C. Stumpf, *op. cit.*, I, 1883, 73-75.
[33] E. B. Titchener, *op. cit.*, 1908, 277, 278.

just before the moment of change, but at and during the change it may be maximally clear. (2) There are, in many of the distractors, brief *moments of interruption*, during which the change may occur and the reaction may be performed as if under normal conditions. (3) The distractors *vary in effect from individual to individual.* What may be a graded series for one observer would affect another very differently. (4) The distractors *do not even affect the same observer* in the same way from day to day. Factors are here involved which are subjective in nature (mood, general organic tonus, etc.), and which it is therefore extremely difficult to bring under objective control.

D. *Double Task Method*

1. *Apparatus.*—The Single Task Method involved only the higher degrees of attention. Under the instructions, this could hardly have been otherwise; with attention directed *to* the sounds of the variator and *from* the distractors, the tones would necessarily be of the higher degrees of clearness. It was incumbent upon us, therefore, to extend the work by some method which should induce the lower degrees of attention, and thus to discover if the conclusions so far drawn hold when the lower levels of attention are involved.

Geissler gives four essential requirements for such a method.[34] The efficiency of the work performed must depend as exclusively as possible upon the degree of attention given to it, and as little as possible upon such factors as practice, fatigue, mood, etc. The performance should never become entirely automatic or habitual. It must require absolutely continuous attention, so that a momentary lapse should at once manifest itself in a momentary reduction of the quality of the work. And the execution of the work must easily submit itself to a scale of quantitative gradation.

The method that seemed best to fulfill these requirements was the Double Task Method. This seems to have been first used by Loeb in 1886,[35] but since that time it has been employed successfully by many experimenters.[36] It requires the

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[34] *Op. cit.*, 515.
[35] J. Loeb, *Arch. f. d. ges. Phys.*, XXXIX, 1886, 592-597.
[36] A. Binet *Rev. philos.*, XXIX, 1890, 138-155.
H. Münsterberg, *Beitr. z. exper. Psychol.*, IV, 1892, 200 ff.
W. G. Smith, *Mind*, N. S., IV, 1895, 50-73.
F. Drew, this JOURNAL, VII, 1895, 533-572.
V. Henri, *Année psychol.*, III, 1896, 232-278; VII, 1900, 250 ff.
J. C. Welch, *Am. J. Phys.*, I, 1898, 283-306.
W. McDougall, *Brit. J. Psychol.*, I, 1904, 435-445.
W. Wirth, *Psychol. Stud.*, II, 1906, 30 ff.
A. Kästner and W. Wirth, *Psychol. Stud.*, III, 1907, 361-312; IV, 1908, 139-200.
S. de Sanctis, *Zeits. f. Psychol.*, XVII, 1898, 205-214.
L. R. Geissler, *op. cit.*, 515-529.

observer to divide his attention, and to perform simultaneously two different mental tasks.

The first task which we selected was essentially the same as that of the previous experiment,—judging and reporting the changes of an auditory stimulus. The recording apparatus, the Stern variator, and the apparatus for the control of the tone, were the same as before. The second task consisted either of counting discrete objects, or of continuous adding.

In the counting of discrete objects, no. 12 BB shot were first used. Since the right hand rested upon the electrical key, the counting was done with the left. The shot proved to be too small to be picked up one by one and thus counted; and an apparatus was constructed whereby they rolled, as they were counted, down an inclined plane to a common receptacle. The rolling, however, made a slight noise; this gave rise to a distraction which under the conditions of the Double Task Method was objectionable; and accordingly a felt pad was substituted for the box. But now the observers had trouble in separating the shot, in moving them rapidly over the surface of the felt; so that, even under the best conditions, accurate records were seldom made. We therefore abandoned the shot in favor of small corks. These permitted of gross movements, could be taken up separately as counted, and were noiseless. They were placed upon a felt pad in front of the observer, and were transferred as counted to a felt-lined box which stood a few cm. to the left.

In the continuous adding, five series of thirty figures were selected to constitute five different degrees of difficulty. The easiest contained all the figures from 1 to 9; with the exception of figure 3, which occurred six times, each integer appeared three times in the series. In the second series all the figures from 3 to 13, with the exception of 10, occurred three times. In the third series all of the figures from 13 to 23, with the exception of 20, occurred three times. In the fourth series the following figures 23 to 27 were used once; 33 to 37, 43 to 47 were used twice; and 53 to 57 once. In the fifth and last series the figures 63 to 67 were used once; 73 to 77 and 83 to 87 twice; and 93 to 97 once. By using a different starting-number, the effect of practice and memory of previous results were entirely eliminated. The starting-number varied in regular order from day to day between the odd numbers from 3 to 25.[87]

The numbers were presented visually by an exposure apparatus which was constructed from a kymograph drum. This was slowly revolved by the motor which, in the Single Task Method, actuated the flicker. The drum was concealed behind a neutral-gray screen which stood in front of the observer at a distance of about 30 cm. A rectangular slit 1 x 3 cm. was cut in the center of the screen directly in the observer's line of regard. The apparatus was so arranged that the numbers appeared from above. The rate of presentation was variable as slow, moderate, and rapid. It was controlled from the experimenter's desk by a three-way switch, and was governed by increase or decrease of the strength of the electrical current. The rate of presentation was also controlled by the spacing of the

[87] *Cf.* Geissler, *op. cit.,* 504.

figures upon the drum, which was constant throughout an entire experiment. In Series I the figures were single-spaced; in Series II, doubled-spaced; and in the other series, III, IV, and V, triple-spaced. The exposure slit was illuminated by an electric light which was fixed just above and somewhat behind the observer. The illumination was constant and uniform, and the observer's head cast no shadow upon the visual field.

A slight shift of the drum upon its shaft made it possible to expose a number-series of any degree of difficulty in the rectangular window. The easier series were first shown, variation occurring only in the starting-number and in the rate of presentation. As the observers became practised in addition, the more difficult series were gradually introduced. We sought to keep the mental task of such difficulty that a high degree of attention was required, and that a lapse in attention should manifest itself directly in the quality and character of the work. Great care, however, had to be exercised not to increase the difficulty of the series and the rate of presentation beyond the limits of the individual observer. F was the only observer to add successfully series V, even when presented at the slowest rate.

2. *Instruction.*—As a control, every third experiment was conducted under the normal conditions of the Single Task Method; *i. e.,* the sole task was observation of, and response to, the changes of the auditory stimulus. Five experiments were usually made during an hour. They began at the signal *Ready, Now,* and the observer directed his attention as instructed, either to the tone of the variator, or to the counting of the corks, or to the adding of the figures. The instruction was as follows:

"In this experiment you are to record all changes of intensity and of pitch as in the previous experiment, by one and two pushes respectively upon the key, and are also in the subsequent introspective reports to give the rate of change.

"You are to direct your attention (to the tone of the variator,) (to the counting of the corks,) (to the adding of the figures). At the end of the experiment, which may run for thirty seconds, you shall (give the number of corks you have counted,) (give the sum of the figures you have added,) answer the following questionary:[38]

1. How much attention in terms of clearness was given:
   a. To the auditory stimulus?
   b. To the other required task,—if there was one?
   c. To any other sensory or ideational processes which may have entered consciousness during the course of the experiment?
2. What affective mood prevailed during the experiment?
3. Have you any comment to make?"

[38] *Cf.* Geissler, *op. cit.,* 519.

These instructions were read to the observer before each experiment.[39] The three kinds of experiment, in which the attention is directed to different tasks, will henceforth be represented by the numerals 0, 1, and 2. 0 refers to the normal or check series, in which attention is directed to the sound of the variator; 1 refers to the Double Task Method, in which it is directed also to the counting of the corks; and 2 refers to the Double Task Method, in which it is directed also to the adding of the figures. These three kinds of experiments are taken an equal number of times, and occur as many times in the first place as the second, third, fourth, and last places.

3. *Number of experiments.*—The experiments were made during the autumn of 1912. The same observers who had taken part in the other series were, fortunately, available. F gave in all 152 introspections: G, 150; and J 151.

4. *Series.*—The series of the Double Task Method differed from those of the Single Task Method in two respects. First, the time of the experiments was reduced uniformly to 30 seconds. This reduction was thought advisable, since the shorter period tended toward accuracy in the introspective reports, and since it tended also to rest the efficiency of the work performed upon degree of attention, and as little as possible upon fatigue. Secondly, the number of changes within a single experiment was reduced to two. The four, and even the three changes which were introduced in the Single Task Method had there proved to be a source of difficulty; and we could not anticipate their successful use under the new conditions, in which the attention was divided between two tasks. In four of the series but one change was made. This precaution was taken in order that the observer might not become habituated to two changes. In all other respects, the series agreed with those of the Single Task experiment.

[39] The importance of repeating the directions is not to be overlooked. On one occasion, the observer was merely directed to "count corks" and not as usual to "direct attention to the counting." The effect on the experiment was plainly shown in the report and introspection. The report was correct, and the clearness was maximal. The introspection read: "At first attention divided equally between tone and counting, both on upper level. On the whole the tone was slightly clearer. Eyes right; turned toward source of sound. These two processes, tone and counting, on upper level for short time. Later they rapidly fluctuated; now the tone was clearer, now the counting."

In detail, the series were as follows:

Series	Time of change	No. of Pitch changes	Pitch of tone	No. of intensity changes	Intensity of tone	Rate of change
I	10″	1	400–450	0	h	r.
II	15″	1	350–450	0	l	s.
III	20″	0	450	1	h-l	r.
IV	25″	0	300	1	l-h	s.
V	5″–10″	2	500–400–500	0	h	r.r.
VI	5″–15″	2	400–500–400	0	l	s.s.
VII	5″–20″	2	500–400–300	0	h	r.s.
VIII	5″–25″	2	300–400–500	0	l	s.r.
IX	10″–15″	1	400–450	1	h-l	r.r.
X	10″–20″	1	500–450	1	l-h	s.s.
XI	10″–25″	1	300–400	1	l-h	r.s.
XII	10″–28″	1	500–400	1	h-l	s.r.
XIII	2″–28″	0	300	2	l-h-l	r.r.
XIV	15″–20″	0	500	2	h-l-h	s.s.
XV	15″–25″	0	400	2	l-h-l	r.s.
XVI	15″–28″	0	450	2	h-l-h	s.r.

SUMMARY

Kind of change	No. of changes	No. slow changes	No. rapid changes
Pitch	14	7	7
Intensity	14	7	7
Total	28	14	14

5. *Results.*—In working over the data, the observers' reports were grouped as in the other experiment. It soon became apparent, however, that this classification was not entirely adequate. There were occasions when the observer reported "change" without giving either kind or rate. Such cases did not occur with the Single Task Method. We dispose of them by grouping them under a new heading, as *Fact of Change.*

The results appear in Table X. The data from the check or normal experiments are not here considered. Only one task was set in those experiments, and the attention was directed to the tone of the variator; consequently, the clearness of the auditory processes was nearly always maximal, and the reports were nearly always correct; the results add nothing to those of Tables I and II (Single Task Method), and are therefore omitted here. Our object in giving the normal series was, it will be remembered, not to confirm the results of the Single Task experiment, but merely to afford a means whereby the observer could, from time to time, compare the clearness of the auditory processes in concentrated and divided attention.

TABLE X

NUMBER OF CASES, KIND AND RATE OF CHANGE

Report	Change	O.	Right									Wrong								
			10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0	10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
KIND AND RATE	Ps	F	4	13	6	2								1					2	
		G		1	1	1			1							7	7	2	1	
		J						5	6	5								1	3	2
	Pr	F	4	10	4									1						
		G		1	1	17	7	2										1		
		J						10	15	6	1								1	
	Is	F	1	3	7							1	1		1					
		G		1			1	1								4	5	1	2	
		J						5	4	2	1									
	Ir	F	5	11	4	4							1			1				
		G			3	1		1	1							2	2	3		
		J						5	4	2	1									
KIND	Ps	F		3	5	1	3						1	1						
		G				4	8	3							1		1			
		J						1	6	7	1							4	4	
	Pr	F		5	8	7	1							1						
		G				1	1	1							1	1	2			
		J						3	9	5										
	Is	F		11	4	1	1							1						
		G			3	2	1	1		2										
		J						1	3	7										
	Ir	F			6	1							1	2						
		G			1					1					3	5	2	1		
		J						1	3	14									1	
RATE	Ps	F		1	1								3	5	1	3				
		G				1		1							4	8	3			
		J							4	4							1	6	7	1
	Pr	F			1								5	8	7	1				
		G				1	1	2							1	1	1			
		J															3	9	5	
	Is	F			1								11	4	1	1				
		G												3	2	1	1		2	
		J															1	3	7	
	Ir	F		1	2									6	1					
		G				3	5	2	1					1					1	
		J								1							1	3	14	
FACT OF CHANGE	Ps	F			1															
		G				1	1	1												
		J							1											
	Pr	F			1															
		G						1												
		J																		
	Is	F	1																	
		G						1	2											
		J							1		1									
	Ir	F		1																
		G					1	1												
		J																		

TABLE X—*Continued*

Report	Change	O.	Wrong 10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
SUBJECTIVE REACTION	Ps	F									
		G									
		J									1
	Pr	F									
		G									
		J									
	Is	F		1		1					
		G									1
		J							1	2	
	Ir	F	1		1	1					
		G									
		J								1	
NO REACTION	Ps	F									5
		G						1	2	1	7
		J									4
	Pr	F									2
		G							1	2	5
		J								2	2
	Is	F				1			1	4	16
		G						2	4	7	13
		J								2	16
	Ir	F								1	12
		G						1		5	10
		J								3	11

Table X gives only the number of cases. A comparison of the right and wrong cases, the subjective reactions and the failures is more clearly presented in Table XI. This table shows that a lower degree and a greater scope of attention were uniformly, in the case of all observers, induced by the Double Task Method. F's introspective estimations cover nearly the entire attentive scale, though there is nevertheless a marked tendency toward the higher degrees. This distribution agrees closely with his introspections, for he reports that the clearness of the auditory processes rises when a change is perceived; as a change is perceived, the processes immediately become clearer, the judgment of kind and rate is made, and then the auditory stimulus again lapses into obscurity.

G likewise covers the entire scale. Her results are very regular, and show a tendency towards the central part of the scale. J's estimations, unlike those of the other observers, are limited to the lower degrees of clearness. His attention upon the secondary tasks was maximal; and his work in them was, throughout, of the highest order.

TABLE XI

NUMBER OF CASES GROUPED ACCORDING AS THE CHANGE IS OF PITCH OR INTENSITY AND IS RAPID OR SLOW, WITH SUMMARY

Kind or Rate	Report	O.	Clearness								
			10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
PITCH	Right	F	8	31	23	10	4				
		G		2	2	23	16	6	1		
		J						19	36	23	2
	Wrong	F		1	4					2	
		G				2	8	10	3	1	
		J							5	8	2
	Sub-jective	F									
		G									
		J									1
	No-reaction	F									7
		G						1	3	3	12
		J								2	6
INTENSITY	Right	F	6	25	21	6	1				
		G		1	7	3	2	3	1	3	
		J						11	18	28	1
	Wrong	F	1	3	3	1	1				
		G				3	11	9	5	2	
		J							1	1	1
	Sub-jective	F	1	1	1	2					
		G									1
		J							1	3	
	No-reaction	F				1			1	5	28
		G						3	4	12	23
		J								5	27
SLOW	Right	F	5	17	15	2					
		G		2	1	2	1	2	1		
		J						9	18	14	
	Wrong	F	1	15	10	3	4			2	
		G			3	6	20	16	3	5	
		J						2	11	17	4
	Sub-jective	F		1		1					
		G									1
		J							1	2	1
	No-reaction	F				1			1	4	21
		G						3	6	8	20
		J								2	20

TABLE XI—*Continued*

Kind or Rate	Report	O.	Clearness 10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
RAPID	Right	F	9	22	11	4					
		G		1	4	22	13	7	2		
		J						15	19	9	2
	Wrong	F		6	15	8	2				
		G			1	1	3	3	4	1	
		J						4	12	20	
	Sub-jective	F	1		1	1					
		G									
		J								1	
	No-reaction	F								1	14
		G						1	1	7	15
		J								5	13
SUMMARY	Right	F	28	95	70	22	5				
		G		6	14	50	32	18	5	3	
		J						54	91	74	5
	Wrong	F	2	25	32	12	7			4	
		G			4	12	42	38	15	9	
		J						6	31	46	7
	Sub-jective	F	2	2	2	4					
		G									2
		J							2	6	2
	No-reaction	F				2			2	10	70
		G						8	14	30	70
		J								14	66

Table XI shows further that the greatest number of right cases occurs in the higher degrees, and conversely that the greatest number of wrong cases occurs in the lower degrees of clearness. This relation between the right and wrong reports is more clearly shown in Table XII, in which the reports are weighted.[40] The same system of weights is here employed as in the Single Task Method. A single addition is, however, made: half a point is added to the right cases for all reports grouped under *Fact of Change*.

[40] We again call attention to the "subjective" reactions. Out of 1160 cases reported under this method only 22 (about 2%) were "subjective."

TABLE XII

Weighted Summaries

O.	Report	Clearness								
		10–9	9–8	8–7	7–6	6–5	5–4	4–3	3–2	2–0
F	Right	28.5	95.5	71.0	22.0	5.0				
	Wrong	4.0	27.0	35.0	18.5	7.0		2.5	16.5	87.5
G	Right		6.0	14.0	50.5	33.0	20.0	6.0	3.0	
	Wrong			4.0	12.0	42.0	48.0	32.5	46.5	89.5
J	Right						54.0	92.0	74.5	5.0
	Wrong						6.0	33.0	69.5	91.5

The crests of the curves of right judgments now fall with F, 7 places; with G, 5 places; and with J, 3 places, respectively, to the left of the crests of the curves of wrong judgments. The crests of the latter curves lie uniformly, for all observers, in the lowest degree of clearness, from 0 to 20. This result, as is shown by the preceding tables, is due to the great number of changes which were unobserved and during which the tone of the variator was very obscure. The general relation holds, however, even if the no-reactions are not considered: in that case, the crests of the curves of wrong reactions still fall 1, 2, and 1 place for F, G, and J respectively below the crests of the curves of right reactions. The results thus substantiate those of the Single Task Method, and confirm our conclusion that a close parallelism exists between the introspectively distinguished variations of attention and the accuracy of the work done,—provided, first, that the estimation of the degrees of attention is made in terms of the attributive clearness of the processes attended to; and, secondly, that the work itself is not influenced by anything other than a change in the attention. That this second requirement is fulfilled under the conditions of our experiment, and that our results are due to attention and not to any other influence, such as a specific reaction to a particular kind and rate of change, is not to be doubted. It is clearly shown in Table XI that the relation holds irrespectively of the kind and rate of the change. The crests of the "right" curves lie from one to two, three and four steps above the crests of the "wrong" curves.

It might be objected that the precision of the work is no more dependent upon degree of attention than upon duration of attention. That the *moment* of change is significant, an objector might say, cannot be questioned. Sometimes a change will occur when the addition is easy, or a cork has just been deposited within the box. Under such conditions a change may be freely attended to, and consequently both the degree and the duration of attention will be increased. If, on the other hand, a change occurs at some critical point in the counting or adding, though it may be perceived as clearly as in the former case, yet the duration of attention will necessarily be shorter; and consequently the precision of the work (judgment of kind and rate of change) will be decreased. In other words, the accuracy of the report will vary directly with duration of attention. This, indeed, might be a formidable criticism if the facts were found to justify it; but they are not. We might, in any event, raise the question whether attention, under the unfavorable conditions of the second case given above, could be as high as under the favorable conditions of the first. We might also point out that our observers were instructed to estimate the *clearness-values only*, that a long preliminary and practice period was allowed in order that they might accurately estimate the attributive clearness of mental processes attended to and attended from, and lastly that their introspective estimation of clearness-values was entirely independent of duration of attention. But we need only refer to the results of the Single Task Method. There, the observers were directed to attend *to* the tone, and *from* the distractors. They were not instructed to react as quickly as possible, but only to respond "as soon as a change is perceived;" there was no pressure upon them to hasten their report.[41] If the duration of attention varied at all, it varied quite independently of, and apart from, the clearness of the mental processes during the change, and the accuracy of the reports made after the change. As now the results of the two methods agree very closely, we may conclude without hesitation that degree and not duration of attention is responsible in our experiments for precision of work done.

It is apparent, however, from the preceding tables, that there is a difference in the *accuracy of reaction to the various changes*. The relation between report and objective change in the Double Task Method is shown in Table XIII.

This table shows, first, that a change of pitch is more compelling than a change of intensity; and, secondly, that a rapid change is more attractive than a slow. The results are uniform for all observers. With changes of pitch, more reports were correct, fewer were wrong, fewer subjective errors were made, and fewer objective changes were unobserved, than in the case of intensity; and likewise, though not so markedly, more rapid changes were reported correctly, fewer wrongly, fewer subjective errors were made, and fewer

[41] The phrase "as soon as" was not felt as a temporal pressure; for, as we have said, no one of the observers knew that the "reactions" were being measured. Had they possessed this knowledge, the instructions might have received a temporal interpretation. In fact, their only concern was to report correctly.

rapid changes were passed over unobserved, than was the case with slow changes.

TABLE XIII

RELATION BETWEEN THE OBSERVER'S REPORT AND THE KIND AND RATE OF OBJECTIVE CHANGE, EXPRESSED IN NUMBER OF CASES

O.	Kind								Rate							
	Pitch				Intensity				Slow				Rapid			
	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.
F	76	7		7	59	9	5	35	39	35	2	27	46	31	3	15
G	50	24		19	20	30	1	42	9	53	1	37	49	13		24
J	80	15	1	8	58	3	4	32	41	34	4	22	45	36	1	18
Total	206	46	1	34	137	42	10	109	89	122	7	86	140	80	4	57

R., right report. S., subjective report.
W., wrong report. N., no report.

(1) The interval between the moment of change and the observer's reaction was measured, as before, in fifths of a second. The *average reaction-time* was computed for each degree of attention as estimated by the clearness of mental processes. This time, together with the m. v. and the number of cases at each level, appears in Table XIV.

TABLE XIV

AVERAGE REACTION-TIME IN SECONDS, MEAN VARIATION, AND NUMBER OF CASES AT THE DIFFERENT LEVELS OF ATTENTION

O.		Clearness								
		10–9	9–8	8–7	7–6	6–5	5–4	4–3	3–2	2–0
F	Av.	1.13	1.22	1.46	1.30	2.10			2.30	
	m.v.	.48	.50	.66	.45	1.36			.30	
	No.	13	54	44	13	6			2	
G	Av.		1.30	.95	1.83	2.17	4.00	4.00	5.80	
	m.v.		.30	.27	.73	.77	1.30	.60	.13	
	No.		3	7	23	18	3	2	3	
J	Av.						1.10	1.32	1.61	1.10
	m.v.						.52	.63	.87	.20
	No.						24	46	48	4

Av., average reaction-time. m.v., mean variation. No., number of cases under rubric.

The number of cases in this table does not agree with that shown in Tables X and XI, although the table includes every reaction-time at our disposal. The difference is due to three causes. First, the subjective reactions and the failures gave no reaction-times. Secondly, the delicate writing-point of the Jacquet chronometer not infrequently bound on the smoked drum. Since the apparatus was in another room, we had no intimation of this defect until the experiment was ended. Though such cases could be (and were) used when we were concerned only with a correlation between the introspective variation of attention and the accuracy of the reports, they were useless for the correlation between introspective variation of attention and rate of report. And thirdly, the observers under the Double Task Method did not always react as soon as the change was perceived and judgment made. They were not instructed to react immediately; and if a change occurred at a critical point, they might voluntarily delay their reaction until a more favorable opportunity to react presented itself. Such delayed reactions were noted in the observer's introspections, and are naturally omitted from the table.

The table itself corroborates Table V; there is a positive correlation between variation of attention and rate of reaction. The reaction-times in the higher degrees of clearness are shorter, and the mean variations are smaller, than in the lower degrees. The numerical expression of this correlation (for length of reaction-time) and of its probable error is as follows:

O.	Correlation [42]	P.E.
F	0.88	0.068
G	0.59	0.253
J	0.60	0.248

(2) Table XI shows that there is a positive correlation between variation in attention and accuracy of work performed *irrespectively of the kind and rate of the objective change.* Nevertheless, we have seen that changes of pitch, and rapid changes, are more compelling

TABLE XV

Relation Between the Reaction-time in Seconds and the Kind and Rate of the Objective Change

O.	Kind						Rate					
	Pitch			Intensity			Slow			Rapid		
	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.
F	1.29	.61	71	1.52	.65	48	1.78	.98	58	.99	.34	61
G	1.91	.75	42	2.28	1.40	15	2.73	1.01	18	1.68	.76	39
J	1.24	.53	73	1.54	.85	48	1.93	.82	49	.96	.28	72

Av., average reaction-time. m.v., mean variation. No., number of cases.

[42] In computing this correlation we omitted the data for F under the 8th rubric, for G under the 2d, 6th, 7th, and 8th rubrics, and for J under the last rubric. If these data had been considered in the computation the correlation would have been much greater. They are, however, the averages of too few cases: 2, 3, 3, 2, 3, and 4, respectively.

then changes of intensity or slow changes (see Table XIII). In Table XV, in which the relation between the average reaction-time of the observers' reports and the kind and rate of the objective change appears, this result is further confirmed.

The average reaction-times and the mean variations are uniformly, for all observers, smaller for pitch than for intensity, and smaller for rapid changes than for slow. These results agree with those of the Single Task Method.

(3) Table XIII shows the greatest number of right cases under the rubrics *pitch* and *rapid,* and conversely the greatest number of wrong cases under the rubrics *intensity* and *slow.* Since Table XV shows that the former rubrics have a more rapid reaction than the latter, it would appear that the *reaction-time for the right answers was faster* than that for the wrong. This inference is borne out by Table XVI.

TABLE XVI

THE REACTION-TIME IN SECONDS AND THE MEAN VARIATION OF THE RIGHT AND THE WRONG REPORTS

Observer	Right			Wrong		
	Av.	No.	m.v.	Av.	No.	m.v.
F	1.26	63	0.52	1.51	56	0.79
G	1.56	29	0.70	2.45	28	1.46
J	1.26	59	0.59	1.46	62	0.73

Av., average reaction-time. m.v., mean variation. No., number of cases.

(4) The *effect of the secondary tasks upon attention* is shown in Table XVII. The average clearness of the sounds of the variator, the mean variation, and the number of cases for each task are given.

This table shows the effect of the division of attention. The fact that F and J have less changes under 0 than under 1 and 2 is due to the nature of our procedure. In some series, only one change occurred during the period of observation; and since the order was haphazard, these series might fall to one kind of experiment more frequently than to another. The difference, however, is small.

The average clearness of the auditory processes in the normal experiments is, for all observers, uniformly higher, and the mean variation is correspondingly lower than in the Double Task experiments. The secondary tasks are, on the average, very effective, though the large mean variation shows that there were cases in which the auditory processes were normally clear. This result is borne out by the introspective reports of the observers. There were many cases in which the change compelled the attention, so that the auditory process momentarily became maximally clear, and the task of counting or adding became obscure. The table shows, further, that the task of adding was uniformly more efficient than the task of counting. The larger size of the mean variation may be ascribed in the light of the introspective reports to the more frequent occurrence of "critical points" in the adding. It will be remembered that the rate at which the corks were counted was governed entirely by the observer, while the adding was objectively controlled.

TABLE XVII

THE AVERAGE CLEARNESS OF THE AUDITORY STIMULUS AS AFFECTED BY DIRECTION OF ATTENTION ON THE PRIMARY OR SECONDARY TASK

Observer		Attention directed to		
		0	1	2
F	Av.	88.5	70.3	53.4
	m.v.	4.5	13.1	29.5
	No.	87	98	100
G	Av.	85.0	48.4	35.1
	m.v.	3.1	12.5	17.7
	No.	93	92	94
J	Av.	92.1	25.5	21.8
	m.v.	2.8	7.3	8.9
	No.	82	100	101

Av., average clearness of tone. m.v., mean variation. No., number of cases under rubric. 0, attention directed to tone of variator. 1, attention directed to counting of corks. 2, attention directed to adding of figures.

TABLE XVIII

COMPARISON BETWEEN THE AVERAGE CLEARNESS OF THE MENTAL PROCESSES INVOLVED IN COUNTING CORKS AND THE DEGREE AND ACCURACY OF THE WORK PERFORMED

Observer		Clearness of Mental Processes Involved in Counting Corks			
		100–90	90–80	80–70	70–60
F	Av.	44.0	36.0	34.2	27.0
	m.v.	5.1	3.2	2.2	0.0
	W.	4	8	3	1
	No.	16	21	8	1
G	Av.	32.6	28.6	23.5	21.0
	m.v.	1.7	2.7	2.8	0.0
	W.		6	2	1
	No.	9	33	4	1
J	Av.	65.0	61.3	58.4	
	m.v.	3.5	3.5	2.8	
	W.	6	9	2	
	No.	17	29	5	

Av., average number of corks counted. m.v., mean variation. W., number of experiments in which the corks were incorrectly counted. No., number of experiments.

(5) Tables XVIII and XIX show the *relation between the character and quality of the work performed, and the attention* as estimated in terms of attributive clearness. Since the tasks occupied the entire period of observation, the clearness of the processes involved in them was obtained as the average of the period.[43]

F and G it should be remarked, counted the corks singly; J counted them in pairs.

The results under Av. and W. confirm our previous conclusions. When the mean variation is considered, however, a discrepancy appears. F and J show a greater m. v. for the higher degrees of attention than for the lower; G alone follows the rule. We can only suggest that the irregularity is due to the small number of cases.

TABLE XIX

COMPARISON BETWEEN THE AVERAGE CLEARNESS OF THE MENTAL PROCESSES INVOLVED IN CONTINUOUS ADDING AND THE ACCURACY OF THE WORK PERFORMED

Observer	Report	Clearness of Mental Processes Involved in Continuous Adding			
		100–90	90–80	80–70	70–60
F	Correct	20	6	1	
	Nearly correct	14	3	1	
	Failures		4	2	
	No. cases	34	13	4	
G	Correct	9	12		
	Nearly correct		18	2	
	Failures		4	4	1
	No. cases	9	34	6	1
J	Correct	13	5	2	
	Nearly correct	5	6		
	Failures	6	11	4	
	No. cases	24	22	6	

In Table XIX the correct additions are grouped under the heading *Correct.* Those in which there was a mistake of one digit, as 164 for 165, or 219 for 229, are classified as *Nearly correct.* And lastly those cases in which the error was greater, or in which the observer lost count of the numbers, are brought together as *Failures.* The degree of attention is determined, as in the previous table, as the average clearness of the mental processes involved in the adding during the entire period of observation.

F and J give the results that we have learned to expect: G's showing

[43] This average, though not asked for in the instruction, was voluntarily estimated introspectively by both F and G, and the value was taken that they ascribed. J gave no such estimation. His average was computed from his estimations prior to and between the moments of change. We may add that the same data were at hand (or the same plan was followed) for estimating the clearness of the mental processes at times when No Reaction was made to an objective change (see Tables I and X).

is less regular. During maximal attention, however, all of her additions were reported correctly. In absolute number, it is true, more correct answers were returned in the next lower degree of attention; but at this level we also find not only 18 cases in which the answer contained a slight error, but also 4 cases of failure. In view of the small number of cases, we may be fully satisfied with the general confirmation of our earlier experiments.

(6) The large number of introspective reports obtained during these experiments, nearly 1400 in all, should yield information concerning the *number of levels* of attention. Titchener, in his *Psychology of Feeling and Attention,*[44] reviews and discusses the literature, and concludes that " a diagram of consciousness would show . . . a two-level formation. The surfaces are not smooth; the upper certainly, the lower probably, is creased or wrinkled." These creases or wrinkles correspond to the finer differences of attributive clearness at the two main levels of consciousness. Since this review, two important contributions to the subject have been made. Wirth[45] finds, under certain conditions, that there are more than two levels of clearness in his own consciousness, and maintains that at any moment of attention all possible degrees of apperception may be represented. Geissler suggests that there are two more or less distinct types of observers: those for whom the dual division is the most natural and most common; and those who as a rule experience several levels of clearness.[46] Only two of his observers reported a multi-level formation, and he was therefore very cautious in drawing his conclusions. " There seem," he says, " to be two types of the attentive consciousness, the dual division and the multi-level formation." Later, in a review and criticism of Wirth's results, he expresses the opinion that Wirth, and probably also Wundt, belong to the latter type.[47]

Our own results throughout corroborate Titchener's original conclusions.[48] All of our observers reported, without exception, the dual division: a clear focus and a vague background, which varied reciprocally.[49] In not a single instance was the multi-level formation even hinted. The distinction of the two types is, then, apparently, a true individual difference, and is not dependent upon external conditions.

The records thus show to date three observers of the multi-level type: Prof. W. Wirth,[50] Prof. M. Bentley,[51] and Dr. H. M. Clark;[52]

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[44] *Op. cit.,* 220-242.

[45] W. Wirth, *Phil. Stud.,* XX, 1902, 493 f.

[46] *Op. cit.,* 528 f.; *cf.* Titchener, *Text-Book,* 1911, 302.

[47] This Journal, XXI, 1910, 155.

[48] *Op. cit.,* 220-242; also *Text-Book,* 276 ff., 290.

[49] The values of the upper and lower levels of attention did not in every case total 100. The discrepancy, however, was never very great, varying only about 5 degrees on the one side or the other. There was some individual variation among the observers. F was ordinarily about 5 *per cent.* short in the total; he never, however, made an error greater than this. G's totals were on the average 5 *per cent.* too large; frequently they were correct; at times they were 10 *per cent.* too large; the value never fell below 100. J's results almost always totaled 100.

[50] *Cf. op. cit.,* IV, 1908, 139 f.; *Phil. Stud.,* XX, 1902, 493; and L. R. Geissler, this Journal, XX, 1909, 120-130.

[51] Geissler, *op. cit.,* 527 f.

[52] *Ibid.,* 528.

and eight observers of the dual type: Prof. E. B. Titchener,[53] Prof. W. H. Pyle,[54] Dr. T. Okabe,[55] Dr. A. de Vries,[56] Prof. L. R. Geissler,[57] and our observers, Dr. W. S. Foster, Miss M. E. Goudge, and Mr. J. S. Johnston.

III. *Summary.*

The general results of our study may be summarized as follows:

(1) Attention may be measured introspectively in terms of attributive clearness. For introspectively distinguished variations of attention (*i. e.*, clearness) are closely paralleled by corresponding differences at the same level in accuracy of work performed, in rate of reaction, and in degree of precision as expressed by the m. v.

(2) Under our conditions, the time of reaction, as is shown throughout by the high coefficients of correlation, serves accurately to measure the attention.

(3) Changes of pitch and rapid changes are more compelling than either changes of intensity or slow changes. The accuracy of judgment for change of pitch and for rapid change is greater than that for change of intensity or for slow change; and, furthermore, the reaction-time to the former changes is smaller than that to the latter.

(4) A close correlation exists between accuracy and rate of report.

(5) Under our conditions, distraction, no matter how slight, tends to lessen the degree of attention.

(6) The difficulty of obtaining a graded series of distractors is very great. In our experience, the action of the distractors is not constant, but varies from day to day, and from observer to observer.

(7) There are two types of the attentive consciousness: the dual division, and the multi-level formation. These types represent true individual differences, and do not depend upon external conditions of observation.

(8) In the dual-division type of attention, the levels vary reciprocally.

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[53] E. B. Titchener, *Lectures on the Experimental Psychology of the Thought-processes*, 1909, Lect. I.; also *Psychology of Feeling and Attention*, 220.
[54] Geissler, *op. cit.*, 527.
[55] *Ibid.*, 527.
[56] *Ibid.*, 527.
[57] *Cf.* this JOURNAL, XXI, 1910, 154 f.

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[1] In compiling this list, the writer has tried to make a beginning toward a comprehensive bibliography of *rhythm*. In order to make the list ultimately as complete as possible, he hopes that those who are interested in the subject will call his attention to any additions or corrections to be made in the bibliography. As a rule, text-books and other systematic works are cited only when they offer original contributions, either by way of theory or by way of experimental investigation, to the general topic. Books and articles, whose titular reference carries only an implicit connection with the subject, are not cited unless they are quoted by some investigator in the field of *rhythm*.

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CLINICAL NOTES ON THE EMOTIONS AND THEIR RELATION TO THE MIND

By George Henry Taylor

In forming a judgment of the emotional factors in a child or a man, it is necessary that every manifestation of emotion should be noted by the observer. His method of examination must be as thorough as that of a trained physician examining a case of illness where doubt exists as to its cause, or of an alienist when determining the sanity or insanity of a patient. A spot diagnosis by a highly trained intelligence is frequently correct, but it is obviously liable to error. A candidate, when examined by the modified Williams' Lantern, may, through a mental lapse and not because of color blindness, name a color wrongly, but he usually at once corrects his error. He may name a red-green, or a green-red; he will not, in my experience, through a mental lapse, name a red-white, or a green-white. This evidence amounts to a suspicion of color blindness, but it would be unjustly used if a candidate were condemned by it without other evidence being considered. The type of person I refer to never repeats his mistake, and shows by the body of evidence in his answers that he is not color-blind. He is never, however, in my opinion, a person with a keen appreciation of color. A person may misname an acquaintance without confusing his identity. He is at least not as liable to misname his wife or his child or his occupation. In the same way a misnaming of red, white or green by an engine driver has a greater significance than the same mistake under similar conditions of examination would have if made by an uneducated candidate for employment.

It may here be mentioned that a mental condition which is the cause of an error, and the trail of a man's life is dotted with errors until it ends, varies little as to the effect, although the consequences which flow from such error may be widely different.

With the average man who lives by labor, a struggle for existence concentrates his mental processes upon the safeguarding of himself and the persons who enter into the orbit of his emotions. An ordinary man or woman is in sympathy with the emotions common to his or her class, and will frequently resent an emotional appeal which is outside of his or her environment. Among the excitant causes of emotions concerned with the appeal of color, I include certain conditions of sound. The appeal of conduct, or moral sense, I believe to be a recognition, in my mind, of the social obligations under which we exist, and the ability to restrain the appeal of emotions when they are in conflict with this recognition. As "the old order changeth, yielding place to new," what may constitute an active emotion in one phase of development, may in another phase pass entirely out of the emotional orbit of an intelligent and educated person.

There is in certain persons, consciously, and in others, unconsciously, a degree of imitation in emotional appeals. The mummer's attitude is largely present in ordinary life. The majority of men keenly resent a finding which exhibits a defect in one or more of their special senses, and as a consequence, a number of educated persons fail to recognise a well defined defect in themselves. I have conversed with a man who professed to a keen delight in color, and found him by examination to be a red-green blind. I have also lived with a person carefully trained in music to interpret the composition of certain composers on a piano, but who in a harsh voice would sing out of tune, and exhibit uniformly in expression and conduct a real indifference to the emotions of color and music.

In the evolution of emotion there is in the mind a dream condition when emotions at rest or in slight vibration become active. They appeal to the mind for recognition, and finally for expression. In rare beings there are periods in which these conditions are fully met. When the appeal of an emotion can survive criticism, it is gradually absorbed by the mind, and may in time, and under certain conditions, cease to vibrate. Without this preliminary stage of vibration, the conception of a newly born truth is incomplete. A purely scientific process can investigate and criticise, it can take to pieces with infinite care and precision, but it is prone to resent a truth if presented in the vibratory stage of an emotion. The mind seeks a "dry light" and the exclusion of emotion, and may to this extent be prejudiced and narrow in its view.

The stimulus an emotion can produce upon the mind, varies in its intensity at different periods. Every emotional person will recognise this varying condition in himself,—how at one time the mind is in sympathy with an appeal, and at another is comparatively indifferent, or even resentful. The appeal of emotions to the mind may be divided into two degrees:—

(1) In which there is a preliminary stage of active vibration.

(2) In which the preliminary stage of vibration is slight.

The appeal of a truth in its vibratory stage is essentially an appeal to the young. The deep roots of conviction in a matured mind are rarely disturbed by such an appeal.

The inherited emotional factors in a human being are the emotions concerned in the protection of life, and the necessary association with this of the evolution of intelligence, on the one hand, and the emotions which have their origin in sex, on the other hand. If one of these is defective, then the fundamental emotional factors in the unit are incomplete. An evolution in the emotions which originate in the sense of sex, and among these I include the emotions of color and musical sounds, can only become coherent to a highly endowed and critical intelligence when such intelligence has itself reached an evolutionary stage, capable of interpreting such appeal.

In illustration of my attempt to give a definition of Moral Sense in the foregoing, I described the effect of education upon an average citizen. It should however, be noted, that in a percentage of persons the place of an appeal to the intelligence in regard to conduct is taken by a purely emotional condition. It is in this type that inheritance of color and of musical sounds is found. The emotional type is in marked contrast to the intellectual type I then described. The manifestation of moral sense through the emotions may find expression by different channels. Amongst the poor and uneducated in whom the moral sense generally finds its medium through emotion we have this type in its most perfect form. Music, color, and the emotional qualities which take the form of a keen sympathy with, and entrance into the pathos of life, is expressed in a type approximating Christ, the great exemplar of emotional morality. Emotional morality is in fact independent of any high intellectual development. Indeed a reference to the intellect in criticism of such emotional manifestations appears to be evidence in the unit, of a deficiency in the supreme sex appeal—a manifestation of absolute emotion—consequently what is termed genius in a painter, a poet or a musician, is a condition in which an appeal to reason is of small value.

It is interesting to note the practical demonstration of this idea in the history of emotional interplay on the various priests of Christ. A sincere and highly intellectual priest tortures or kills persons whom he regards as a menace to his faith, whilst others of the priesthood in the same period regard such acts with abhorrence. If a person criticise these two types without prejudice, he must, I think, recognise on the one hand a reasoning man, and on the other an emotional being who, resenting cruelty, can only appeal to humanity through his own medium of emotion. On a somewhat similar plane is the modern social reformer who, without regard to the suffering and destruction he causes, would precipitate revolution to consummate reasoned idea. Contrasted with this is the man who, resenting violence and cruelty, strives through an appeal to humanity, that is through the stimulus of the emotions, to bring about the same result. The genius—offspring of emotion—is rarely found amongst those who have accumulated wealth. It is the appeal of the tragedy and pathos, the comedy and beauty of life which brings him into being and equips him with the power to love, and to express love to humanity through music and painting, conduct and teaching.

An analogy on these lines can be made between a sex dream, and a nightmare. One is a delirium of sex, the other a riot in the intelligence. There is no fear in the one, fear is the prominent phenomenon of the other. Fear is an intellectual appraisement, whilst love is the emotional manifestation of sex. These appear to be the conditions of a supreme intellectual and sex appeal—and "Love casteth out Fear." Under such a stimulus must Beethoven have composed his "Kreutzer Sonata" on the one hand, and Poe his "Raven" on the other; the one a purely emotional outpour, the other a piece of intellectual artistry.

The creative musician, the painter either in words or upon canvas, and that still rarer form of emotional type, the Christ-like man, all make their supreme appeal through emotion. It is, I believe, in the last analysis, a sex appeal, purged of animalism, and is nearly always an appeal from a man, and through men to women and children. The mind of man, taking mind in this sense to include both intellect and emotion, may for the purpose of demonstration be compared to a straight line. At one end of this potential line is reason, at the other, the emotions having their origin in sex. The emotional and intellectual conditions of a commonplace man in a civilized community (and the large proportion of men are commonplace) differ in education and environment more than in intellectual or emotional inheritance. He stands within a limited orbit somewhere about the middle of this line. If we judge the value of men by an economic standard, then it is probable in this assessment that a hod-carrier is of greater value to the community in which he lives, than is an emotional genius. It is simply a question of standards. At the reasoning end of the straight line I have described, sex would, in such an economic standard, be regarded as a function with periods of excitement in the unit. The male mind, apart from lust, would then appraise the value of a female as a child producer and mother. This appraisement would differ in values though not in principle when made by a brute type on the one hand, and a high intelligence on the other. The appeal of moral sense in the ordinary man I have described, is very little more than a recognition of danger from the standpoint of law or social convention. If he recognise a danger signal he will frequently disregard it, provided he can do so with safety and advantage to himself.

A RAPID AND ACCURATE METHOD OF SCORING NONSENSE SYLLABLES AND WORDS[1]

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In all memory work, the determination of the subject's retentive capacity is undoubtedly the chief factor. When we determine retentiveness by the *time taken to relearn* after a certain interval has elapsed, the method is easy, since the only measurement we have to consider is *time,* i.e., we compare the subject's *time of relearning* with his *time of initial learning.* When, however, we desire to ascertain the subject's retentiveness, say after an interval of one week, by requesting a reproduction without a fresh presentation, the determination is more difficult since our only method then is to determine the subject's retentiveness by the amount and nature of the material that he has actually been able to reproduce, i.e., to recall.

Where logical or meaningful material (e.g., a passage of prose or poetry) is used, the scoring of the work reproduced offers fewer complications than does the scoring of an attempted reproduction of a set of nonsense syllables or words. The latter may not necessitate so much time; but the difficulty of dealing with the errors, omissions, insertions, etc., makes an accurate scoring exceeding difficult since it is difficult to determine, from the reproduction, what associations were originally formed, because the material is less logical in character.

In 1908 I started experiments with digits, words and nonsense syllables in which it was necessary to determine the amount retained by each subject after an interval of one week had elapsed. Digits are so simple in character that they offer but few difficulties; but although many methods were tried for nonsense syllables and words, none of the methods seemed satisfactory. The widely different marks obtained with the various methods of scoring in general use, show that some of these methods must be erroneous. For nonsense

[1] The method as set forth in this article refers only to words and nonsense syllables of three letters.

syllables, probably the most accurate of the methods tried was that of Ebbinghaus; but even here not only was the method long and laborious, but the scores obtained were frequently such that I felt quite sure, from a general survey of the attempted reproductions and from a comparison with the subject's retentiveness for other material, that the scores as given by the Ebbinghaus method were too low.

In the following pages I have attempted to describe a method which I have found to be most satisfactory for both nonsense syllables and words. Its chief advantage lies in the speed with which each subject's score can be obtained after one has become familiar with the method. Probably the best way of obtaining an understanding of this method is to examine a few typical cases.

At the end of this paper will be found copies of the reproductions as handed in by three subjects in a class in experimental psychology. The nonsense syllables and words are given just as they were written, with the errors, omissions, dashes, etc.

The original list of nonsense syllables and words was as follows:

VUS	TUB
YIF	PIN
MAV	HEN
JEP	BED
VOB	LID
FEG	GEM
WOF	BUD
TIB	CAR
NUZ	MAT
BOF	ROD
JED	JUG
KIB	FOG
VEL	LAD
ZID	SOD
BOL	PEN
SEF	CAT
YAB	RAG
KUV	BOX
TEF	NET
NAD	GUN

Briefly stated, the method for nonsense syllables was as follows: Each correct letter, provided the syllable was in the correct position,[2] received a score of one, and the syllable received an extra score of one for being in the correct position. Thus a perfect syllable in the correct position received a mark of 4, while a syllable correct in itself, but not correct in position, received a score of only 3. If the position were correct and the syllable had two of the three letters correct[3] it was scored 3. If two of the three letters of the syllable were correct but the *position* of the syllable itself were not correct, either relative or absolute, it was not scored at all. Therefore, unless position is correct, the separate letters do not count unless *all* are correct.[4] It must be remembered that, as before said, if a syllable is correct, but is not in the correct position, it gets 3, and only 3, counts, since each syllable that is in the correct position and also correct in itself receives a count of 4. Therefore, the highest count obtainable for a list of 20 syllables would be 80. This method of scoring can be made clear by examining a typical case. Take, for example, the list written by J. McH., as given at the end of this paper. *VUS* gets 4 counts, since it is correct in everything. *VIF* gets only three counts, since, although its position is correct it starts with V instead of Y. *JEP* gets 3 counts; had it been in the correct position it would have received 4, since when a syllable is correct, except that it is in the wrong position, it is credited with 3 counts,—one for each letter. *RIL* receives no score at all, since there was no such syllable. *BOV* receives a score of 2, for it contains all the letters that occur in *VOB* and besides is in the correct position, i.e., where *VOB* should be. *SIR* receives no score at all. It is quite likely a pure guess, and put down merely to secure correctness of position for the two following syllables. We are led to believe this when we perceive that the next two syllables, *WOL* and *TID*, have two letters correct in each, and their positions are also correct.

The subjects were told to draw a line under the last syllable they wrote if they felt sure that it *was* the last syllable. In this way the last syllable was given a score of 4 if it was the correct syllable and was also underlined, even if it was

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[2] "Correct position" here, as with digits, may mean correct *relative* position or correct *absolute* position. A syllable is in the correct *relative* position when it is preceded by the correct syllable, or by a syllable of which two letters are correct, provided that these letters themselves be in the right order.

[3] Provided these two letters themselves were in the correct order.

[4] When, however, all three letters were written but not in correct order, i.e., the letters reversed,—the syllable received a score of 1, and if the position was correct, a score of two.

not preceded by the correct syllable;—it was given a score of 4 since it had the correct absolute position.

With words a method similar to that used with nonsense syllables was employed.

A score of 1 was given if the position, whether relative or absolute, was correct. Here also correctness of the relative position was determined by the preceding word. An extra count was given if any two letters[5] were correct, provided that the position of the word was correct. If the word itself was correct it received still an extra count, thus raising the count to three. Therefore, the highest count obtainable for any word was 3, and therefore, the highest score obtainable for the 20 words was 60. This method, like that for the nonsense syllables, can best be understood by an examination of one of the subject's papers. Take, for example, that of the subject reproduced at the end of this article. Let us see how we arrived at the total score of 15. *TUB* received a score of 3 since it is in the correct position and the word itself is correct. *PEN* receives a score of 3, one because two letters of the word are correct, one because the word (as far as the two letters are concerned) is in the correct position, and still an extra mark being given since the word itself is one occurring in the list.[6] *CAT* receives a score of 3,—one because it contains two correct letters, 1 because it is the correct word itself and another count because it is in the correct position, being preceded by the word *PEN*. *MAN* receives no count at all, there being no such word. *PIN* receives a count or 2,—1 because it contains two correct letters and an extra count because it is the correct word. It cannot receive a count for position, neither the relative nor the absolute position being correct. *RUG* receives no count at all. We are tempted to give it a mark of some sort on account of its close similarity to the word *JUG* but this would be precarious as there are many words that rhyme with *JUG* and many such words might have been written at random. We are also tempted to give the word *RUG* a score of 1 as it is similar in meaning to the word *MAT*. This also would be unwise since many words might have been given,

[5] The same rule was used here as in the case of the nonsense syllables and the two letters themselves had to be in the correct order.

[6] This is an exceptional case. With the list in question it is the only word with which such a case can occur, for the reason that, though in one sense the word is not in the correct position, yet two letters of it are, it being in the place that *PIN* should be. Obviously, cases similar to this occur very rarely and would not occur at all if the list of words used did not contain two words so nearly alike as *PEN* and *PIN*.

that in the examiner's mind might have been considered similar to the word *MAT*. In fact in the case of this particular subject we have proof that the word was not put down for this reason as the word *MAT* itself is mentioned as the next word. Therefore, taking everything into consideration, I considered it safest not to give any credit whatsoever for such words. The next word *MAT* gets a score of 2,—1 because it has two letters correct and an extra 1 because it is the correct word itself. *RAG* gets a score of 2 for the same reason. The next four words, *WIT, RAT, BOY* and *RUN,* receive no credit whatsoever, there being no such words.

A score of 1 was not given a word having two letters correct unless the *position* of the word itself was correct. Otherwise the word *RUG* would receive 1 count since it contains the letters *UG*. Had *RUG* been preceded by the word *ROD* it would then have received 2 counts instead of none at all,—1 count for having two letters correct, and an extra count for being in the correct position.

To make this method of scoring still plainer, we shall examine another paper,—that of subject M. K., also reproduced at the end of this article. The first word *TUB* is given 3 counts, it having 2 letters correct, it also being the correct word, and also being in the correct position. *HEN* is given a score of 2, it being the correct word but not in the correct position. For like reasons *JUG* is scored 2. *RAT* receives no score at all although it has two letters, *AT,* that are correct (they being also in the word *CAT*). The word, however, is not in the proper position either relative or absolute and hence can receive no count at all. Words of this kind receive a score of 2 or nothing, for reasons given in detail under nonsense syllables. The fairness of this rule is made clear when we realize that had the word *RAT* been preceded by the word *PEN,* the chances of *RAT* having been a mere guess would be greatly lessened. *TAN* receives no count at all. To the next word, *MUG,* one is tempted to give a score of 1, since it contains the two letters *UG* which are also contained in *JUG*. It would have received credit for these two letters had the word been preceded by *ROD*. Not being preceded by *ROD* it is given no count at all. That this is perfectly fair is in this particular case very conveniently shown by the appearance later on of the word *RUG* which, although there is no such word, is given a score of 2, it being preceded by the correct word *CAT*. The two letters that are correct in this case are *R-G* and although separated by the wrong vowel *U* they are in the proper order. *PEN* receives a score of 2, it having two letters correct and also being the correct word itself. *BED* receives a score of 3, 1 because it contains two correct letters, 1 because it is the correct word itself, and 1 because it is preceded by the correct word. In this case the "preceding" word is not wholly correct but it contains two correct letters, and thus *BED* gets a higher scoring than it would have received had it been preceded by the word *AXE,* for example. The last word *GUN* receives a score of 3, it being in the correct absolute position for the reason that it is underlined, this proving that the subject knew that it was the last word.

Reproduction of Nonsense Syllables and Words
By J. McH.

VUS....	TUB...
VIF...	PILL..
JEP...	RAG..
RIL	CAN
BOV..	———
SIR	———
WOL...	———
TID...	BAR..[7]
	DOG
	SUN
Total Score for	FLY
Nonsense Syllables	———
18	———
	MAT..
	BAG
	BOX...
	TOP
	LID..
	MAN

Total Score for Words, 16.

Reproduction of Nonsense Syllables and Words
By A. F.

VUS....	TUB...
YIF....	PEN...
MAV....	CAT...

[7] *BAR* receives a score of two although it is neither preceded by the correct word nor is the word itself correct. The correct word here is *CAR,*—and *BAR* receives a score of 1 for having two letters correct and another score of 1 because the word is in the correct absolute position.

JEB...	MAN
VOS...	PEN..
WEF...	RUG
FEG...	MAT..
TIB....	RAG..
NUZ....	WIT
LOD..[8]	RAT
GER..[8]	BOY
KUL	RUN
YAB...	

Total Score for Nonsense Syllables 39

Total Score for Words 15.

Reproduction of Nonsense Syllables and Words By M. K.

VUS....	TUB...
YIF....	HEN..
TIB...	JUG..
BIF[9]	RAT
JEB...	TAN
———	MUG
———	CAT..
———	RUG..
NAB...	PEN..
———	BED...
	GUN...

Total Score for Nonsense Syllables 17

Total Score for Words 19.

[8] *LOD* and *GER* both receive a score of 2 notwithstanding that they have only one letter correct, i.e., the vowel. As said on page 527, even though a syllable has two letters correct, if it be not in the correct position (either relative or absolute) it receives no score at all. When, however, the *absolute* position is correct (as it is in the above case) *each letter that is correct is scored.* Therefore, each of the above two syllables receives a score of 2,—1 because it is in the correct absolute position, and 1 for having a correct letter.

[9] *BIF,* though similar to the tenth syllable *BOF,* can receive no score, it being in neither the correct *relative* nor the correct *absolute* position. It has, however, two letters that occur in *BOF,* for which it may have been mistaken. *JEB* (*JED*) on the other hand receives three counts, it being in the correct relative position with reference to the preceding syllable *BIF* (*BOF*). It will thus be seen that in cases like this, what one syllable loses it gives to the other.

CHARACTERISTIC DIFFERENCES BETWEEN RECALL AND RECOGNITION

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The more obvious practical importance of *recall* in daily life seems to have led the greater part of experimental work on memory in this direction, to the comparative neglect of the not less interesting process of *recognition.* The recent work of Müller, Strong and others has called attention to certain instructive and hitherto unreported differences between the two processes. Thus Müller reports that retroactive inhibition fails to appear if partially learned material, followed by some different task, is merely recognised. But in the case of reproductive memory the addition of the incidental task is found to bring about a considerable reduction of recall efficiency. Strong finds that whereas, in recall memory "with increase in the length of the series there is much greater corresponding increase in the time or energy required for its mastery," in recognition memory the results indicate "that the number of stimuli in the series affects the results almost in direct proportion to the increase." Kirkpatrick finds that, under given circumstances, about twice as much can be recognised as can be recalled.

It is probable that a more careful study of the phenomena and laws of recognition will throw light on various other processes concerning which there is still much to be learned. Feelings of identity and of familiarity are fundamental in many intellectual operations, so fundamental indeed that one is tempted to classify them as ultimate experiences which cannot be further analysed. Since this short paper is intended to be suggestive rather than final, no attempt will be made to summarize previous work on recognition. The results to be reported were secured, for the most part, in experiments performed for other primary purposes. They are thus, in most cases, incidental results, but are perhaps for that very reason even more suggestive than if they had been deliberately secured from the point of view in which they are now considered.

Schematically, at any rate, the difference between recall and recognition seems to be a rather simple matter. Recall is that aspect of memory process in which a *setting,* a background or association-cluster, is present in clear consciousness, but a desired *focal element* is missing. Thus I try to recall the name of Byron's hero in "The Prisoner of Chillon." The memory of the theme, the aspect of the castle, the pillar in the dungeon, the beauty of the lake and mountains, constitute a clear setting, but the focal element, the name, is missing. When it appears it probably comes in fragments,—first the form or rhythm of the word, then various letters or syllables, one fragment dragging in the others and being assisted in this by various features of the general setting. The following series, resulting from an actual attempt to recall this name, shows the way in which the unitary focal element is finally constituted by its various fragments:

*B*alboa
*B*omb*ard*o
L*ombr*oso
Bazzadof
*Bar*b*ad*oes
L*ombard*o
*Bonav*entu*r*a
*Bonav*ent
Bonivar
*Bo*l*ivar*
Bonivard (correct)

Recognition is, schematically, just the reverse of this process. In recognition the focal element is present, in the form of sensation, image, or feeling, and the question is whether or not this element will recall a more or less definite general setting or background. Such experiences as those indicated by, "Where have I seen that face before?" "Have I ever heard this sermon?" "Whose voice is that?" etc., illustrate this situation. It is indeed often true that the setting need not come into clear consciousness in order to determine the outcome of judgment or the feeling of recognition. At least it need not become as clear as the recalled element in reproductive memory, in order to produce the feeling of familiarity. But the mere feeling of familiarity represents only a partially complete recognition. The completeness of the recognition will depend on the clearness or briskness with which the setting or certain features of the setting happen to be revived. James has vividly described the way in which vague marginally revived processes may reveal their presence, character and behavior by the production of a resultant feeling (familiarity, recognition, novelty, intention, etc.).

It is often said that recognition is an important part of recall memory, giving warrant to the correctness of the recalled element. This may often be the case, but it is not necessarily so. Items may be correctly recalled but not recognised as correct and rejected. Moreover every case of recognition presumably involves recall or tendency to recall on the part of the setting. The ordinary act of "memory" is said to be complete when focal element and setting belong together, that is, mutually recall or sustain each other.

If this schematic distinction between setting-element and element-setting is a correct one, it at once becomes interesting to picture some sort of neural counterpart of the two processes; to conceive in neural terms the differences in the behavior of the two aspects of memory. Why, for example, does retroactive inhibition affect the one process and not the other? Why does increase in series length influence the two processes in different degrees? Why is more material recognizable than can be recalled? Nor are these the only questions to be raised, for there are many other empirical differences between recall and recognition. Some of these further differences it seems worth while pointing out, even though the data on which the comparisons are based are so incomplete as to be suggestive only of qualitative rather than of quantitative differences.

1—Determination to Remember

As is well known, mere repetition, without the purpose or intention to retain, does not by any means guarantee the subsequent ability to recall. In fact the determination to learn is one of the most important conditions of reproductive memory. Rather curiously the influence of purpose or intention in the case of recognition seems to be much less than in the case of recall. The results of the following experiment suggest the striking difference between the two cases.

In an experiment performed primarily for another purpose than the study of memory, each of five observers went through a form of the "opposites" test from 60 to 75 times. The stimulus card bore 50 adjectives and the task was to speak the opposite of each word in turn, as quickly as possible. One or two trials were made each day, the same list being used but the words occurring each time in a new and random order.

After 60 to 75 trials had been made, each observer was asked to recall and write down all the pairs of opposites that had been used. Fifteen minutes were allowed for this test, but the number recalled after the first three minutes was also noted.

After this test a list of 100 pairs of opposites, containing 50 new pairs along with the original 50, was presented. Each observer was now requested to identify the 50 pairs used in the experiment. In this case all observers completed their selection in three minutes or less.

The following Table gives the number of correct recollections and recognitions for each observer, and the averages of the five observers. Only slightly more than half (28.8) of the 50 items were recalled in a quarter of an hour, about half of these being reproduced in the first three minutes. In the case of the recognition test, however, practically every item is correctly identified within three minutes or less.

TABLE 1

INFLUENCE OF INTENTION ON RECALL AND RECOGNITION

Observer	From a total of 50 original items		
	Number recalled		Number recognized in 3 min. or less
	In 3 min.	In 15 min.	
L	19	38	50
G	14	27	49
H	10	27	49
R	12	25	50
P	19	27	50
Averages	14.8	28.8	49.6

Here we have a case of purely incidental memory, there having been no determination whatever to memorize the list of stimulus words. As a result, no doubt, of this absence of intention, the 60 to 75 repetitions failed to make sufficient impression to make possible more than 30 per cent recall efficiency. But in spite of the absence of intention to remember there is practically 100 per cent recognition efficiency, and the indications are that this efficiency would have been found long before the last repetition. It is not possible, on the basis of these figures, to say that recognition is not influenced by the presence or absence of the determination to remember, but it is clear that it is much less influenced by this factor than is the process of recall.

2.—Value of Repetitions

The above result suggests that a given number of repetitions of the material has greater value for recognition than for recall. It is, of course, a matter of common experience that a single presentation may suffice to enable recognition but be quite insufficient to make recall possible. It would be of interest to determine more or less precisely the equivalence of repetitions in the two cases, for different materials and observers. The following results are suggestive of the sort of differences revealed by experiments designed for this purpose.*

Fifteen items were presented visually, at intervals of two seconds, to each of five observers. Each observer, immediately after the completion of the series, was requested first, to recall and reproduce or describe as many as possible of the fifteen items, and then to select, from a set of thirty items, the fifteen just presented. This method thus gives an approximate measure of the tendency to perfect recall on the one hand and the tendency to perfect recognition on the other hand, resulting from the single presentation. The series was then presented a second time and the recall and recognition test repeated. This process was continued until both perfect recall and perfect recognition were achieved, and the number of repetitions required in the two cases was thus determined. Four different sorts of material were used,—nonsense syllables, simple geometrical forms, pictures, and words (nouns and adjectives). Table 2 gives, for each observer and for each sort of material, the number of repetitions required for perfect recall and for perfect recognition, and the ratio of recall to recognition.

The results are rather interesting. When the material is quite devoid of sense or meaning, as in nonsense syllables, there is very little difference between the number of repetitions required for complete recall and the number necessary for complete recognition. As we pass from nonsense syllables through geometrical forms and simple pictures to common nouns and adjectives, the sense or meaning of the material becomes more and more definite and the possibilities

* These experiments were performed by Miss Edith Mulhall, Barnard, '13.

of associative setting increasingly richer. As this happens we find a correspondingly greater difference in the effect of repetitions for recall and recognition. The recall-recognition ratios increase from 1.2 through 1.4 and 1.9 to 2.2 respectively. This change in the ratios is furthermore due entirely, in these instances, to the increasing ease of recognition in the case of meaningful material. The average number of repetitions required for complete immediate recall changes very little. In general then, with meaningless material repetitions are equally effective for recall and for recognition. But with increase in the meaningful character of the material this influence becomes relatively greater in the case of recognition, until, with nouns and adjectives, it is more than doubled.

TABLE 2

EQUIVALENCE OF REPETITIONS FOR RECALL AND RECOGNITION

Material	Process	Repetitions for different Observers					Averages	Ratio of Recall to Recognition
Words....	Recall.........	3	3	5	3	6	4.0	
	Recognition....	1	1	2	2	3	1.8	2.2
Pictures..	Recall.........	3	3	3	2	4	3.0	
	Recognition....	2	1	2	1	2	1.6	1.9
Forms....	Recall.........	4	3	4	4	3	3.6	
	Recognition....	3	2	3	2	3	2.6	1.4
Syllables..	Recall.........	5	5	7	6	5	5.6	
	Recognition....	3	3	6	6	5	4.6	1.2

3.—Degree of Assurance

Watt remarks, "unfortunately, however, no very reliable test of recognition is known. A learner can either recall a word or he cannot. . . . But he may say he recognises a word without either being sure that he does so, or without really recognising it at all." And, "Correct and sure recall ought to bring with it the assurance of its correctness."

But does not recognition also bring with it the degree of assurance of its correctness? All that is necessary for a reliable test of recognition is a situation in which the chances of accidental correctness are known. This situation being given, it is no difficult matter to test the accuracy of recognition for various individuals, materials, and conditions.

TABLE 3

Correctness and Assurance in the Recognition of Different Materials

Syllables					Geometrical Forms				
Obs.	A	B	C	D	Obs.	A	B	C	D
1	72	65	72	60	1	97	72	50	60
2	78	50	75	..	2	85	..	..	..
3	72	63	65	44	3	84	65	55	33
4	81	52	44	..	4	95	70	..	..
5	76	51	67	..	5	90	50	50	..
6	89	63	50	50	6	90	75	75	..
7	67	63	56	..	7	90	75	67	..
8	63	27	33	..	8	64	55	75	(100)
9	89	76	63	55	9	82	67	58	(100)
10	75	50	..	50	10	87	50	..	..

Words					Pictures				
Obs.	A	B	C	D	Obs.	A	B	C	D
1	90	100	72	50	1	100	84	67	72
2	68	53	25	..	2	89	..	..	..
3	71	60	83	50	3	89	81	77	..
4	91	60	..	..	4	95	80	33	..
5	92	..	..	..	5	98	..	..	..
6	95	57	33	..	6	96	63	50	..
7	87	50	71	42	7	100	..	..	..
8	89	60	60	50	8	96	75	..	..
9	90	80	30	..	9	93	100	..	..
10	80	85	66	..	10	93	100	..	..

Averages of the 10 Observers

Material	A	B	C	D
Syllables	76.2	56.0	58.0	52.0
Forms	86.4	64.0	61.0	50.0
Words	85.3	67.0	55.0	48.0
Pictures	93.0	83.0	57.0	....
Grand Averages	85.2	67.5	57.8	50.0

A test of the accuracy of recall requires only simple enumeration of correct reproductions and their proper relative evaluation. A test of the fidelity of recognition requires more elaborate statistical treatment of the data, perhaps, but the two methods are not unequally reliable. Just what degrees of assurance recognition does bring, and just how correctness varies with this confidence, has been but little investigated.

The figures on page 538 resulted from experiments on this point. Syllables, geometrical forms, advertisements (picture and reading matter), and words were used, and the attempt made to measure the curve of forgetting for recognition, by testing at various intervals after the original presentation. This method was soon seen to be inadequate for the primary purpose of the experiment, since at each test the original material was again seen (though along with other material, to be sure) and the various items chosen at a given trial reinforced by the mere fact of their having been thus selected. But from the point of view of assurance and correctness the experiment afforded ample material at each trial. Ten observers were used, and the degree of assurance indicated in the case of each selection by grading the confidence of the identification as A, B, C or D. The Table gives typical results, showing the total correctness of each degree of certainty for intervals covering a period of twenty-one days.

Several suggestions are afforded by the Table.

a.—The correctness for C and D degrees of assurance (slightly certain and mere guess) is about the same for all four kinds of material. The mere guesses show just the chance relationship (50%, since the items were selected from a larger group containing twice the original number), and the C judgments some 8% higher correctness.

b.—The A and B judgments have higher and higher validity as one passes from nonsense syllables through forms and words to more complicated and meaningful material (advertisements). With syllables, A and B judgments are about 66% correct, with forms and words about 75%, and with advertisements about 88%.

c.—In these latter cases the C and D judgments are less used than is the case with less meaningful material. The four kinds of material show a regular progression in the number of observers not using the C and D degrees of confidence. The A and B judgments are thus more often correct in spite of the fact that more of the items are reported with these high degrees of assurance.

Strong has studied recognition for advertisements and for words, and finds varying percentages of correctness for different degrees of confidence as the length of series is increased. The same investigator has also considered some of the statistical difficulties involved in the correct evaluation and scoring of data secured by the method of selection.

4.—Influence of Primacy and Recency

That primacy and recency of impression influence the accuracy of recognition in much the same way that they influence recall, is seen from the following experiment. Fifteen pages, each containing an illustration along with reading matter, were arranged serially on a table. Twenty observers were allowed to begin at one end of the series and inspect all the items, from left to right, for a short period of time. These fifteen items were later presented along with an equal number of new items, and each observer attempted to identify the original set. The following Table gives the per cent of times that the items in the fifteen different positions were identified.

TABLE 4

INFLUENCE OF PRIMACY AND RECENCY ON RECOGNITION

Position	Per cent. of times identified	Averages of groups of 5 positions
1	84	
2	36	
3	44	
4	36	
5	20	44
6	12	
7	16	
8	4	
9	8	
10	20	12
11	4	
12	20	
13	28	
14	16	
15	56	25

Quite as in the case of the reproduction of simple material after serial presentation, we find primacy and recency both effective, and the former, under the conditions just described, considerably more influential than the latter.

5.—Recognition Span

The fact that there is a more or less definite reproductive "memory span," which varies with the individual, with the material, and with other conditions, is familiar. Aside from this elementary span, with perfect reproduction, only a limited number, from a larger number of presented items, can be reproduced, and this number varies not only with individual and with material, but also with such factors as series length, time interval, etc. Much the same thing holds for recognition, although but little evidence for it has been presented. Strong finds that, "When five advertisements are successively exposed 86% can be recognised immediately after, while only 47% can be recognised from 150 advertisements similarly exposed. The per cent of correct recognitions decreases as the length of the series increases," and "This decrease is possibly faster at first and then steadily becomes less as the series are increased in length." The following results also bear on this question of immediate "recognition span."

TABLE 5

Per cent. of 15 advertisements immediately recognized, average. of 25 observers	.78
Per cent of 25 normal advertisements, average of 20 observers	.76
Per cent. of 25 geometrical forms, average of 20 observers	.62
Per cent. of advertisements when original copy is retained but the cut is changed	.56
Per cent of advertisements when original cut is retained but the copy changed	.43
Per cent. of times substitution of a new cut is detected	.26
Per cent. of times substitution of new copy is detected	.17

The fidelity of recognition clearly varies with the type of material. It also depends on the integrity of the original items, and this fact seems to indicate that the recognition is not entirely of the item as a whole, but is conditioned by specific details in the total composition. Experimental variation should show in any given case just which details are most important.

6.—Recognition after Longer Intervals

Similar differences in fidelity of recognition for different sorts of material are found when a longer time intervenes between the original presentation and the occasion of identification. The following results are from an experiment on the curve of forgetting for recognition, and show also the decrease in per cent fidelity which results when the number of items in the original series is increased.

The nonsense syllables show a considerable loss, both from increase in series length and from increase in interval. Words show a smaller loss, although loss is present from both

TABLE 6

PER CENT. OF CORRECT RECOGNITIONS, WHEN CHANCES OF ACCIDENTAL CORRECTNESS ARE AS 1:1. AVERAGE OF 10 OBSERVERS

	Advertisements	Forms	Words	Syllables
After 2 days				
Series of 15 items....	93	85	78	75
Series of 25 items....	94	75	74	60
After 14 days				
Series of 15 items....	80	81	77	66
Series of 25 items....	80	80	72	56

causes. Forms show no loss due to increased interval, and only after the shorter interval (two days) is there loss due to increased series length. Advertisements, on the other hand, show no loss whatever as a result of increase in series length, but lose appreciably with increased interval.

7.—*Individual Differences*

Individual differences in fidelity of recognition are apparent in any experiment with this process. The range of these differences may be roughly indicated by the ratio of best to poorest in any given group of observers. Using college students, the range is usually about as follows.

TABLE 7

RANGE OF INDIVIDUAL DIFFERENCES IN FIDELITY OF RECOGNITION

Advertisements, 1st set	2.1 to 1
Words	2.1 to 1
Mutilated advertisements	2.1 to 1
Geometrical forms	1.9 to 1
Advertisements, 2nd set	1.8 to 1
Syllables	1.5 to 1
Forms, 2nd set	1.4 to 1
Simple pictures	1.1 to 1
Average	1.8 to 1

The ratio of about 2:1, which is found in these cases, is found in so many experiments on various sorts of mental capacity that it cannot be entirely without significance. Professor Cattell calls attention to numerous cases in which this ratio of best to poorest is present. It should not of course be taken to indicate any necessary distribution of capacity among human beings in general, for the range here is much greater than that represented by this ratio. What it does seem to indicate is that, when individual observers are chosen from a social group which is formed on the basis of some more practical consideration,—as college students, engineers, 8th grade children, etc., a range larger than this will not usually be found. This seems to mean that a ratio of two to one, as between best and poorest, includes those individuals who can satisfactorily and practically find a permanent place in the given group. A more extreme variation, either above the best or below the poorest, is sufficient to place the individual in another social group, and he is not likely to be included among our observers unless special care is taken to find him.

Summary

1. A closer study of recognition than has been made heretofore will probably contribute much to our knowledge of other processes as well.

2. A schematic account of the mechanisms of recall and of recognition seems to involve a common neural pattern, operating in reverse "directions" in the two cases.

3. Purpose, intention, and similar determining tendencies, are much more effective in recall than in recognition.

4. The value of a single presentation is greater in recognition than in recall, and the difference between the values of repetitions becomes still greater the more meaning the material possesses.

5. Recognition is based on varying degrees of assurance, and the degree of this assurance is a fairly definite index of the accuracy of the recognition. Assurance varies with the individual, the material, the length of series, the integrity of the items, the time intervening since original presentation, etc.

6. Assurance and correctness are higher the farther the content is removed, in character, from nonsense material.

7. Primacy and recency influence recognition much as they do recall, and, under certain conditions at any rate, primacy is stronger than recency.

8. A definite "recognition span" may be found, which will vary with numerous individual, material and technical conditions.

9. When the attempted identification is not immediate but takes place at longer intervals after original presentation, various sorts of materials show characteristic differences in relative recognisability and in effect of increased interval.

10. The influence of increased series length varies with the material, and stands, in general, in inverse relation to the meaningful character of the material.

11. The range of individual differences in fidelity of recognition within a given socially selected group is similar to that found in the case of other individual differences.

12. The indications are that a range of about 2:1, as between best and poorest, includes the limits of variation which common experience imposes on the selection of a practical or social group.

13. Other interesting facts concerning recognition have been suggested, such as the failure of retroactive inhibition, relatively small influence of increase in series length, etc.

A NOTE* ON THE RELATION AND ÆSTHETIC VALUE OF THE PERCEPTIVE TYPES IN COLOR APPRECIATION

By E. J. G. Bradford

Abstract

The preface of this note consists of a brief statement of certain experimental results bearing on the order of preference for certain colors, together with the reliability of this preference order, and on the frequency of occurrence of the perceptive types among university students. An analysis of the four principal perceptive types suggests that there are two distinct types within the one which has been called "Associative," namely the 'Sensational-Associative' and the 'Emotional-Associative.' Physiological factors are advanced as likely to affect the perceptive type of an individual. The æsthetic value of the types is considered on the basis of the above mentioned analysis.

The substance of this note deals with certain general considerations concerning the 'perceptive' types and their relations. These considerations have arisen incidentally in the course of some experimental work; hence only the briefest summary of the methods and results of the experiments is here necessary as an introduction to the theoretical matter which follows it.

The Experiments

The Subjects.—These consisted of twenty-six university students, eighteen of them graduates; thirteen from each of the Arts and Science faculties.

Method.—The subjects were presented with a set of fifteen rectangular pieces of paper each about 30 square inches in area. These papers were numbered in order to avoid difficulties arising from individual differences in color naming. The colors were arranged in a row and the subjects were

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* From the Psychological Laboratory of King's College, London, England.

instructed to write down the numbers of the colors in order of preference. When this had been done the next instructions were given, namely to write down against each the reasons for liking or disliking that color.

The Results

Preference Order.—In order to ascertain the average preference order the following procedure was adopted. The fifteen possible positions of a color were divided into five groups whose limiting positions were 1-3, 4-6, 7-9, 10-12, and 13-15. The median position for each color was found, these medians were then grouped together according as they themselves fell within these same limits. The fact that no color has a median position lower than 10.2 confirms the general impression of the subjects, namely that on consideration hardly any colors are really disagreeable when taken by themselves.

		15	
		11	
	1	10	3
	14	5	8
	13	2	12
9	6	7	4

1 3 6 9 12 15

In the above polygon the columns are proportional to the number of colors whose median positions fall between the limits mentioned. The colors were as follows: No. 9, dark blue; No. 1, saturated green; No. 14, chocolate brown; No. 13, pale blue; No. 6, slate grey with bluish tinge; No. 15, saturated crimson; No. 11, pale green; No. 10, coffee brown; No. 5, bluish green; No. 2, ink red; No. 7, cinnamon brown; No. 3, pale pinkish brown; No. 8, bluish green; No. 12, pink; No. 4, yellowish green. The figures inside each column of the polygon are the numbers of the colors falling in that group, in order of preference reading from the top downward.

An inspection of the polygon will show that loss of saturation tends to lower the position of a color *e. g.,* dark blue No. 9 comes within the first group while No. 13 comes within the second; Nos. 14 and 10, 7 and 3 are further instances. The admixture of a small proportion of another color has a similar lowering effect, *e. g.,* Nos. 5 and 1, also 14 and 7. Regarded objectively, the first eight colors in the preference order are pure, while the last six seem to be adulterated with some foreign element.

Cohn had previously put forward the view that increase of saturation tended to make a color more pleasing,[1] although Major[2] obtained results which were antagonistic to those of Cohn. The above results tend to confirm Cohn's results although the details of the method used were different.

Reliability of the Preference Order.—Three subjects were again tested for their order of preference after considerable intervals, in order to determine the constancy of their preferences. A second estimate was obtained after an interval of fourteen days and a third estimate after a lapse of twelve months.

Subject	Correlation coefficients between		
	1st and 2nd	1st and 3rd	2nd and 3rd
H. S.	$.81 \pm .06$		
E. B.	$.94 \pm .02$	$.87 \pm .04$	$.88 \pm .04$
L. S.	$.94 \pm .02$	$.81 \pm .06$	$.83 \pm .05$

The formula used was $\rho = 1 - \dfrac{6 \Sigma d^2}{n(n^2 - 1)}$

Differences of Perceptive Type.—In order to see if training influenced the mode of color appreciation, the subjects were classified according to their faculty. The total number of answers given by the thirteen subjects in each group were classified and the results were as follows:

Group	Physiological answers	Objective answers	Associative answers	Character answers
Arts.......	59	34	35	35
Science.....	40	48	40	32

Although the differences under Physiological and Objective answers are big, they can hardly be regarded as significant since in neither case is the mean greater than twice its probable error.

[1] J. Cohn. Gefühlston und Sättigung der Farben, *Philos. Stud.*, XV., 1900, 279-286.

[2] D. R. Major. "On the Affective Tone of Single Sense Impression," *Amer. Jour. Psychol.*, VII., 1895, 57-77.

[The value of applying mathematical methods to experiments dealing with æsthetics is questioned by some workers in this field. However, if averages are employed, there should be some check on them before they are made the basis of any conclusions. Take an example from some fairly recent work. Winch[3] bases his conclusions regarding the color preferences of school children, on averages which at first sight appear large but which on investigation fail to reach the required standard of significance, in other words they are less than three times their probable error.]

The 'Perceptive' Types.—A psychosis may be regarded either from its cognitive or affective aspect, and this is most certainly true of those psychoses which give rise to an appreciative judgment or opinion. The perceptive types in æsthetic appreciation fall naturally into two groups according as the sensational or emotional elements play a predominant part in deciding the subject's attitude towards the color. Sensational elements are to the fore in cases where the subject gives an Objective or Physiological answer, whereas it is the emotional elements which predominate when Character and some Associative answers are given. This difference will be made clearer by considering each type singly.

In what may be called the Physiological psychosis there is a complex of sensational elements (visual, tactile, kinæsthetic and visceral sensations and images) together with their emotional accompaniments. These sensational elements are purely personal; they are identified with the subject's own bodily sensations and are characterised by a minimum of objectivity. The sensational character of the Physiological answer does not depend necessarily on the absence or weakness of the emotional elements but rather on the fact that the subject has not reached a sufficiently advanced stage to enable him to differentiate the emotional from the sensational elements in consciousness, and hence to give a bias to either one.

There seems to be no *a priori* reason to assume that the emotional elements are stronger in a psychosis which leads to a Physiological answer, than in one which leads to a Character answer; the change from one type to another depends rather on an increasing power of introspective analysis than on an increase in the strength of the emotional elements. Introspection in the former type makes evident the

[3] W. H. Winch, Color Preferences of School Children, *Brit. Jour. Psychol.*, .III, 1909, 42-65.

sensational elements which are far more easily noticed than are the emotional complexes which form the basis for the latter type of judgment. Differences in the strength of these emotional elements are due to temperament, and temperament, it is generally admitted, is not in the normal subject liable to change during a lifetime; yet subjects who give two types of answer to the same color and who seem satisfied only when two answers have been given can hardly be expected to have changed temperamentally during so short a space of time. For example a particular shade of yellow is called sickly when it is obvious to an observer that the subject who gives this answer, is strongly affected by the emotion of disgust. Another subject when presented with the same color might say that for him this color typified the emotion of disgust, a sort of hypostatised "disgust," a Character answer. Why this difference of answer when both subjects experience the same emotion? It may be argued that the word 'sickly' is more often used by the first subject than is the word 'disgust' and hence a difference in type is really only a sign of a difference between the affective vocabularies of the two subjects. The fact that the word sickly is more commonly used by the one subject than by the other may be taken as good evidence that this subject is limited by his inability to differentiate the sensational from the emotional elements, and that it is this lack of ability in differentiation and not his vocabulary that is the prime cause of a difference of type. Character subjects are often in difficulty when asked to express this appreciation in words, those who can do so are registered as of the Character type, those who cannot, 'break down' to a lower type because of their inability to find a word quite suitable to their state of mind; not being able to fix an emotional complex with a word, they fall back on the more easily defined sensational elements.

The above mentioned differentiation may be accomplished in either of two ways, which result respectively in two types, the Objective and the Associative. The Objective subject separates these elements by totally neglecting one of them. The sensational elements (color sensations) seem to be attracted from the self to the object and, as it were, replace that object and thus are regarded as being quite impersonal; the object thus becomes of no intrinsic value so far as the judgment is concerned. With the Associative on the other hand, the association of mental elements with the object tends rather to draw the object towards the self, and no 'depersonation'[4] of the elements results. The object as such becomes an integral part of the complex which determines the appreciation, and in some cases the introduction of the object seems essential to the creation or augmentation of the emotional tone.

Answers which are classified as Associative, really differ very much among themselves when examined introspectively. There appear to be two types grouped together under the head "Associative." The distinction between them depends on the predominance of the sensational or the emotional elements respectively. For example, a red color will suggest a 'red flag,' because of their common sensory element, by a process somewhat akin to complication, *e. g.*, a red flag is more than a red patch; it has substance, and hence it implies touch sensations or at least a combination of perspective and color sensations. On the other hand it may be that the red excites the same emotional complex as does the 'red flag,' in this case the emotional elements predominate and although it may not be strictly correct to say that they act as the medium of association, yet they do seem to favor that particular association. (There seems to be some justification for this last assumption when abnormal subjects are considered, for it is well known that the memory of an alternating personality is dependent on the emotional tone of the subject at that particular time.) These two forms of the Associative type may be called the Sensational-Associative and the Emotional-Associative; the former might possibly develop into the latter because the introduction of the object might tend to reinforce the emotional elements in consciousness, and thus reinforced the subject would more easily distinguish them.

That some colors are more potent than others in arousing the emotions is undoubted, and it is these colors which first enable the subject to differentiate out the emotional elements which once noticed, as it were, almost in vacuo, will be more apparent when encountered in less easily analysed complexes. This provides one way in which advance from a lower to a higher type can take place. However, the same color is not equally potent for all subjects, probably because of the differences in sensitivity of the color apparatus of the eye.[5] For

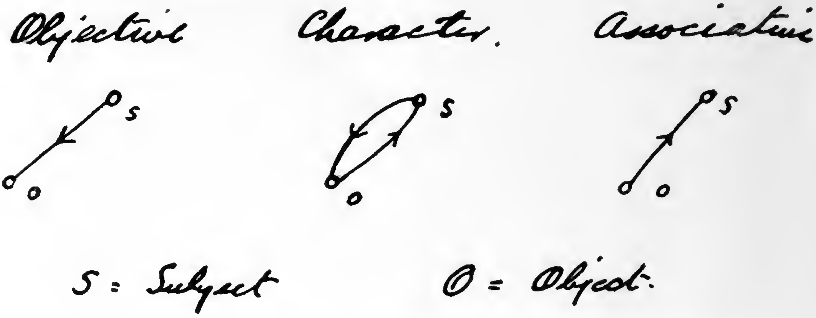
[4] Bullough has suggested the term 'exteriorisation' as a label for this process. It is admittedly a rather barbarous term but it is difficult to find another which is equally suitable unless "depersonation," which term is here used.

[5] Since the above was written it has been shown by experiment that fairly large differences of sensitivity to color exist. (See L. R. Geissler, Experiments in Color Saturation, *Amer. Jour. Psychol.*, XXIV., 1913.)

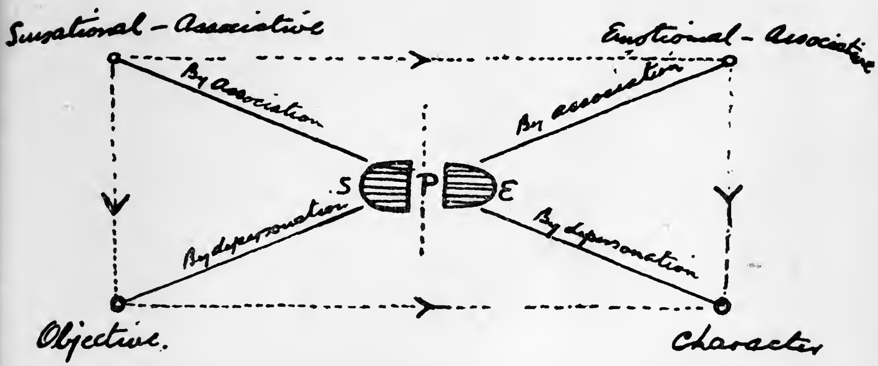
instance a red which is styled "glaring" by one subject, may by another be called "cosy," because the red apparatus of the one is much more sensitive than that of the other. This difference in sensitivity may even be so great as to change the type of answer, thus a yellowish green might be "washy" for one subject, and "sickly" for another, because the one was more sensitive to the green than the other. The one would see a weak *green*, the other would see an impure *yellow* and not a green. It may be noted in passing that this same factor is probably largely responsible for individual differences in color naming. Past experience is another factor which influences the cognition of a color, especially those who are of the Associative type would tend to be influenced in this way. The effects of this factor are seen in such associations as 'green—vegetation' and 'yellow green—quinine' which also afford a good example of how closely connected the Perceptive types actually may be. The association 'green—vegetation' probably would be given by a subject in whose consciousness the sensational elements predominated; introspection alone could decide the validity of this assumption; whereas the association 'yellow-green—quinine' undoubtedly would be accompanied by emotional elements of far greater strength than those accompanying the former association. This second association shows the transition stage between the Associative type and the Physiological, from which by a process of association the mental content is pushed into the objective zone. The quinine association has a minimum of objectivity, it is still somewhat personal, a characteristic of the Physiological type, while the vegetation association is marked by far greater objectivity, in fact the personal element seems almost absent.

The Character type brings us to a stage where the emotional elements have successfully undergone the process of 'depersonation' either directly from the Physiological or indirectly by means of association. The Character type is like the Objective in that the process of depersonation releases certain mental elements from the self, but it differs in that it is the emotional and not the sensational elements which are thus liberated. For example, a pinkish buff, such as No. 3, by a Physiological subject would be called 'sickly,' his attention being largely to his own person rather than to the color. But an Objective subject would style it 'indefinite or impure, satisfactory neither as a pink nor as a buff,' thus showing how the sensational elements, color sensations, are considered as being intrinsically a part of the color. The Character subject would say that it suggested 'baseness, meanness or fawning,' thus showing that this subject sees in the object a certain emotional tone which is really part and parcel of his own consciousness. Besides the similarity due to depersonation of the mental elements the Character and Objective types both have the characteristic of evaluation though their standards be quite different, the former has an ethical and the latter an hedonic standard.

Physiological subjects react to a color with motor manifestations, *e. g.,* facial expressions, and these seem to be the sole effect of that confused complex of sensational and emotional elements of which the Physiological psychosis is composed. The conative tendency of an Associative subject seems to be entirely satisfied by the uncritical acceptance of some object appropriate to the state of mind induced by the color presented. The conative tendency in the Objective subject is not satisfied by the analysis of the sensational elements; he must proceed to the simple judgment of good, bad or indifferent. Character subjects take up an attitude of evaluation towards the emotional elements ascribed to the color, and by a process of hypostatisation are led to react towards the color as if it were a person possessed of these same qualities. For instance the subject when in the presence of a person who is 'hard and keen' has a particular emotional disposition in a nascent condition; a color which is 'hard and keen' causes him to feel sympathetically 'hard and keen' besides feeling somewhat affected by the previously mentioned disposition. Thus there is first a movement towards objectivity followed by a movement towards subjectivity comparable to the two processes manifest singly in the Objective and Associative types respectively.



To summarize, it may be said that the four types of appreciation all develop from the Physiological by the application of two different processes to the two species of mental elements found in consciousness.



The above diagram shows graphically the relations between the various types.

S = Sensory elements. E = Emotional elements.

P = Physiological.

The dotted lines are to show that the Character type may be approached directly from the Physiological or indirectly through the Objective or Associative channels. The justification for this assumption is that certain Character subjects gave two reasons for appreciation, sometimes the second reason was Physiological, sometimes Objective and sometimes Associative; showing that in some cases at least the character attitude was nearer to the Associative and Objective than it was to the Physiological.

The Æsthetic value of the types. It is rather difficult to classify the types according to æsthetic value because both the attitude of the subject and the content of his consciousness are involved.

It seems safe to say that the Sensational-Associative type has no value at all, since it is characterised neither by its emotional content, by the differentiation of mental elements, by abstraction, by depersonation nor yet by the fusion of the differentiated elements with the self.

The Physiological type is characterised by a psychosis in which the emotional elements are present but remain undifferentiated.

The Objective type, so far as mere attitude goes, is closer to that of æsthetic appreciation than are the previous types, in that it is characterised by an initial depersonation but the process has been applied to the wrong material.

The Emotional-Associative has a claim on the ground of the content of its consciousness; yet the interposition of an object as a means of releasing this content from the self seems hardly to be the correct method to lead to the attitude of æsthetic appreciation.

The Character type alone has any claim to the title 'æsthetic;' and even it lacks that relation of fusion between the self and the object of appreciation, which partly at least depends on the emotional sympathy between the object and the self. It is certain that the Character type is the most highly developed of the types. The Objective and Emotional-Associative are partially developed and hence can fairly be said to rank next. The Physiological represents the crude undeveloped content of experience, while the Sensational-Associative can hardly lay claim to be called 'appreciation' at all.

The order which suggests itself from these considerations is the following:

1. Character.
2. Objective.
   Emotional-Associative.
3. Physiological.
4. Sensational-Associative.

Reading downwards from those of most to those of least æsthetic value.

THE COMPARATIVE VALUE OF VARIOUS CONCEPTIONS OF NERVOUS FUNCTION BASED ON MECHANICAL ANALOGIES*

By Max Meyer, University of Missouri

The word "mechanical" in the title of this paper is used in its widest sense, as meaning anything machine-like. The advantage of mechanical analogies in our conceptions of the functioning of the nervous system consists in their forcing us to think in quantitative terms. We psychologists, and also the physiologists and zoologists who are interested in our problems, have been accustomed too long to think exclusively in qualitative terms. Imagine an engineer designing (and true understanding is equivalent to designing) a machine by thinking in such purely qualitative terms as this: the rise of temperature in the boiler is "instinctively" or "reflexly" followed by a rotation of the fly-wheel; each movement of the crank through one hundred and eighty degrees "associates itself" with one movement of the valve gear; the strokes of the piston-rod are "inhibited" by a raise of the damper lid on the flue, etc. The engineer can design a machine only by thinking of a fly-wheel of a definite shape and weight, of a piston-rod of a definite length, of a piston of a definite cross-section and length, of a boiler of a definite size, of a definite degree of temperature within it, of a definite quantity of coal, etc. We shall never understand the function of the nervous system unless we are willing to think of it, too, in quantitative terms. We can use quantitative terms equally well in either of two ways, by means of geometry, and by means of arithmetic and algebra. A quantity or a quantitative relation can be defined equally well in a graph and in an equation. A description of a nervous function which employs neither a graph nor an equation, but humors the reader who dislikes mathematics by confining itself to qualitative terms, is not worth the paper on which it is printed.

This problem has stood in the center of my interest for more than ten years, although I have expressed myself on it

* Paper read at the meeting of the Western Branch of the American Psychological Association, Easter 1913, at Evanston, Ill.

only during the last five. But others have been at work on this problem, too. You have undoubtedly heard of Uexküll. He does not, however, offer much of interest to the psychologist because he has devoted himself chiefly to the explanation of the behavior of such animals as insects and still lower forms, whose nervous systems are undeveloped in comparison with that of man. His mechanical analogies are, in my opinion, neither applicable to higher functions, as, for example, to learning,—the central fact of psychology,—nor are they simple enough for the demands of science, nor are they always easily translatable into anatomical and physiological terms. I agree, on the whole, with Jennings who expressed the following opinion: "It seems that, even for practical purposes, the author (Uexküll) has overestimated the value of a rather gross 'Anschaulichkeit.' The bringing in of machine-like structures,—tubes, valves, etc.,—that confessedly do not exist, seems rather to confuse than to aid the mind. It is not possible to conclude directly from the properties of the assumed machines as to what physiological properties one will find, for the parallelism is far from complete, so one must try to keep the system of machinery separate from the system of physiological facts; there are two systems to grasp instead of one."[1] Let me say at once that I did not derive any of my ideas on this subject from Uexküll.

Mr. Nathan A. Harvey, of Ypsilanti, Mich., has been working along similar lines. But the chief problem of psychology, that of learning, became a stumbling-block to him. He announces his renunciation in the following words: "There is one thing that is not perfectly plain in this scheme: No explanation can be thought of, at least by the present writer, that would account for the shifting of the dendrites in the process of attention. Why should the dendrites move? Here is a place where no answer can be given, and when a person points out that this involves some supernatural agency and cannot be accounted for in terms of pure physiology, we have to acknowledge that we see no way of accounting for it."[2]

Quite recently Mr. S. Bent Russell, an engineer of St. Louis, has offered some interesting ideas on this subject. The

[1] H. S. Jennings, The Work of J. von Uexküll on the Physiology of Movements and Behavior. *Jour. Comp. Neurol. and Psychol.*, XXIX, 1909, 330f.

[2] Nathan A. Harvey. A Device by which Physiological Concepts may be employed in teaching Psychological Functions, *Western Journal of Education*, IV., 1909, 182.

last part of this paper shall be devoted to a comparison of the functional concepts as published by Mr. Russell[3] and those published by myself in my book on the Fundamental Laws of Human Behavior (1911). In some respects I agree with Mr. Russell; in others I disagree. Whatever our differences may be, however, I wish to express the opinion that Mr. Russell has done science an invaluable service by the publication of the paper in question. He describes in all structural details a hydraulic machine which, if inserted in a steamship, would reveal among other possibilities the one described in the following tale.[4]

A canal, large and deep enough to carry a steamboat, has the shape of the numeral 8, or, more strictly speaking, of two circles touching each other. At the point of contact, where the waters belong to the one circle as well as to the other, a steamboat is floating, in the direction of the tangent of both circles. On the deck of the steamboat there are three levers, No. 1, No. 2, and No. 3. No. 1 starts the propeller driving the boat straight ahead. No. 2 starts a second propeller which, serving instead of the rudder, gives the boat a turning movement to the left. No. 3 starts a third propeller which gives the boat a turning movement to the right. On deck, before the three levers, stands one of those psychologists whom I criticized in a paper read before this association a year ago,[5] who believe that a machine can change its ways of working only if a ghost is kind enough to step in, disconnect something, and connect it with something else, —in other words, who believe that "experience" without a ghost is unthinkable. It is a fine Sunday morning, and our experimental psychologist begins his pleasure drive by pulling lever No. 1. But soon he observes that the boat is straightway approaching the dividing point of the canals which threatens destruction. He pulls lever No. 2; the boat swings to the left and follows the circular route of the canal without touching the shore. After a while the opposite point of the circle is reached. It is lunch time. Our psychologist pushes both levers, No. 1 and No. 2, back, and the boat stops. After lunch, to continue his trip, he again pulls lever

[3] S. Bent Russell, A Practical Device to Simulate the Working of Nervous Discharges. *Journ. Animal Behavior,* III, 1913, 15-35.

[4] The tale is not given in Mr. Russell's paper, but, I hope, will appeal more strongly to the imagination than Mr. Russell's equivalent, but somewhat dry, description in engineering terms.

[5] Max Meyer, The Present Status of the Problem of the Relation between Mind and Body. *Journ. Philos. Psychol. and Sci. Meth.,* IX, 1912, 365-371.

No. 1. Suddenly he remembers that No. 1 serves the forward propeller, and looks ahead to see if he is dangerously near the shore. To his amazement he finds that the boat is following the circle properly. Not only is the forward propeller working; the propeller turning to the left is working too, although no one has pulled lever No. 2. The steamboat has an engine which "remembers." After some time the point of departure is reached. Lever No. 1 is pushed back, the boat stops in its original position, and our experimentalist goes home.

The following Sunday we find him again on deck. He pushes lever No. 1 and the boat starts. But the engine no longer "remembers." After so long an interval as a week it "forgets." The boat moves straight ahead. Our adventurer pulls lever No. 3; the boat swings to the right and follows the route of the other canal. Lunch time comes again. Both levers, No. 1 and No. 3, are pushed back, and the boat stops. After lunch the experimenter pulls lever No. 1. The engine "remembers." The boat does not go straight ahead, but follows the curving of the canal to the right. The forward propeller and also the propeller turning to the right both work, although lever No. 3 was not touched again. But after a week the engine "forgets" this experience, too. Nothing, however, prevents it from relearning.

Now, this is not altogether a fairy tale. This wonderful engine (which, after all, is no wonder) is described in sufficient detail in Mr. Russell's paper and, in summary form, it is spoken of on page 30 in the following words: "Figure 6 and table III. show a duplex converging gang. In this arrangement the first key rod (or sensory terminal) *SS* is known as the station key. *MR* and *ML* are opposite movements. If *SS* and *SL* are habitually struck in succession, except when *SS, SR* and *SL* are struck in succession, the device will become "trained" so that when *SS* is struck the movement *ML* will result. On the other hand if *SS* and *SR* are habitually struck in succession the key *SS* will when struck give the opposite movement *MR*." Nothing, except the limitations of your pocketbook, prevents you from having such a pleasure yacht built for your own use in vacation hours. You simply have to follow the specifications given by Mr. Russell in that part of his paper which is devoted to their exposition, beginning on page 21 with the heading "Operation" and ending on page 30 with the words just quoted.

In the pages from 31 to 35,—the closing section of his paper,—Mr. Russell gives specifications for hydraulic machines capable of still other varieties of learning by experience, in fact, of most of those varieties which I have enumerated in my book on Human Behavior. And nowhere in his specifications is any ghost to be found. The specifications contain objective realities exclusively.

What, now, is the value to pure science of these pages, from 21 to 35, of Mr. Russell's paper? Of course, their value to the millionaire who desires to own a pleasure yacht which remembers and forgets its experiences, is obvious, but merely practical. Their value to pure science, as I see it, consists in the fact that we have here a demonstration of the possibility of an "organism," capable of learning and forgetting, which obeys no ghost whatsoever, but only the laws of mechanics. This possibility has been denied by those psychologists, led at present by William McDougall, who have written "Interactionism" on their standard. They might perhaps, in order to save their position, reply that S. Bent Russell has proved this possibility only for a mechanical, but not for a biological organism. But is this not a purely technical evasion of the point at issue? We have always accepted almost like a dogma,—and shall continue to do so,—that what is possible in "mechanics" (taking this word in a narrow sense as the science of matter and motion, but including hydromechanics) is possible in physics-chemistry, the larger field of science; but not the reverse. And we further believe that what is possible in physics-chemistry, is possible in biology; although the reverse may not be true. If it is proved that a mechanical organism can learn and forget without the interaction of a ghost, we have no right to assert that a biological organism can not.

Is Mr. Russell's demonstration of a learning and forgetting machine also valuable to science in suggesting new experimental work in physiology, anatomy, zoology? Here, unfortunately, its value appears very limited. The objections raised by Jennings against much of the theoretical work of Uexküll, apply with equal strength to this of Russell. What suggestions does the neurologist receive from a multitude of terms like these: spur valve, pawl, pawl spring, ratchet valve, rocking finger, finger lever, plunger, dash pot barrel, balanced slide valve, key rod, bell crank, suspender link, coupling gang, etc.? I am far from asserting that none of these terms is translatable into neurological terms; but I do not doubt that most of them are untranslatable.

Let us now turn to a consideration of the first pages, not referred to as yet, from 15 to 20, of Mr. Russell's paper. I propose to compare very briefly our quantitative concepts of nervous function. As previously explained, the geometrical drawings are an essential part of this concept. I represent the nervous system as consisting throughout of arches, parallel or superposed, each arch consisting of a rising, a connecting, and a falling line. I have also devised a consistent method of lettering; but this concerns us less, at present. Each line represents a neuron. This does not mean that in the anatomy of an animal it may not denote several neurons. But no fewer than three can compose an arch. It appears immediately to the eye that some arches are reflex arches, others, the higher arches, are "nerve centers." Regarding the resistance of each neuron as originally the same, it appears immediately to the eye that certain points (sensory and motor) are "corresponding," that is, connected by a nervous path of the smallest possible (length, and therefore) resistance, and that other points are connected only by way of superposed arches, by paths of greater resistance,—how much greater is again immediately clear to the eye.

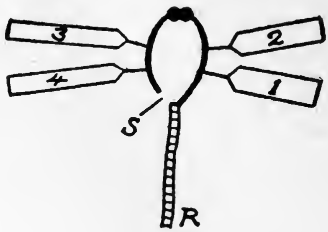
Mr. Russell uses a different kind of diagram. Where I use an arch of three lines as representing a reflex arc, he uses an angle of two lines. Where I use an arch of three lines as representing a superposed nervous arc, he uses a diagram of four lines. He seems to prefer this method of drawing the diagram on account of its showing all the sensory points on one (the upper) side of the diagram, and all the motor points on another (the right) side. I am not certain that this is an advantage. But this method, in my opinion, has the decided disadvantage that one cannot build up systematically an unlimited number of higher and higher nerve centers without making the figure confusing, for all the higher centers of different level would be mixed up among themselves and with the reflex arcs. In my method all the arcs representing the same nervous level, that is, the same closeness or remoteness of connection of a sensory with a motor point, appear clearly to the eye as standing on the same level in the diagram. Having counted the number of neurons from one of these arcs to the sensory or motor periphery, we know it for all other arcs on the same level. Counting, indeed, is not necessary even in the first place, since my method of lettering gives the answer without counting. It is not strange, then, that Mr. Russell, in his stream diagrams, restricts himself to one level above the reflex arcs. That the application to animal behavior demands the removal of such a limit of the number of levels, I have shown in my book, for example, on page 44.

Mr. Russell seems to believe (p. 19, and also the last paragraph of his paper) that he has given an explanation of the mechanism of the association of ideas. I do not concede this. In order to give such an explanation, it is necessary to make first a definite assumption with respect to the nervous correlate of a mental state. I have made such an assumption (on pages 233 ff.) and I have given the explanation: Mr. Russell has not.

A similar remark may be made with respect to what Mr. Russell says on page 33 about satisfaction: "This mechanism . . . illustrates . . . satisfaction, showing how associations may be reinforced or stamped in." I have stated the nervous correlate of feeling on pp. 238-239 of my book.

Mr. Russell, throughout his paper, uses the term "susceptibility" in a sense entirely different from that in which I have consistently used it. I mean by (positive and negative) susceptibility (p. 115) of a neuron its ability to respond to a flux by an increase of conductivity, to lack of function by a gradual return to the former conductivity. Mr. Russell, however, does not use the word susceptibility in this sense of "being capable of a gain or loss in conductivity," but in the sense of a synonym for conductivity itself. He uses, in addition, capacity as a synonym for the same term, conductivity. It is my opinion that no synonyms are needed for conductivity, and that the use of either susceptibility or capacity in this sense is confusing.

To make plain the different ways in which the nervous processes streaming through the diagram influence each other, Mr. Russell uses the analogy of a hydraulic machine consisting of fifty or a hundred parts, of some of which I have quoted the names. These parts of the machine, essential for its functioning, mean practically nothing to the student of neurology, suggest no new experiment. Let us compare the mechanical analogies which I have used. I assume that the point of contact between the ends of two conductors serves (normally) as a one-way valve (called check valve by the engineer). The translation into neurological terms is simple. We assume that the so-called synapse, much abused in psychological theory, when normally functioning, permits the passage of the nervous excitation in one direction only. I further make an assumption about the relative susceptibility in lower and higher arches, which hardly needs a translation into, but is practically stated in, neurological terms. This assumption directly refers to certain known facts of neurology and animal behavior[6] and suggests further experimental work in these sciences. I also introduce (and this is practically all the mechanical analogism which I use) the two concepts of deflection and overflow. The latter means this. Think of a metal wire of a given electrical resistance. If its temperature rises, its resistance becomes greater; and the rise of temperature may be due to the current itself. Suppose something similar holds true for a neuron carrying a nervous excitation,—there you have the translation, simple and direct, into neurological terms. The consequence would be of the nature of the function which I have called overflow, that is, certain rather long paths would carry a greater flux than was to be expected from the original computation of the resistances. And this plays a great rôle in the explanation of what I çall instinct.



Deflection of a stream by another, stronger stream, drawing the weaker one into itself, is easily demonstrated by means of an apparatus which anyone can build out of two glass T's, a glass jet, a few feet of rubber tubing and a bottle of ink, the whole to be connected with the water faucet. I do demonstrate this, for the sake of "Anschaulichkeit," to use Uexküll's phrase, every semester to my students. The translation into neurological terms is self-evident, if we are willing to think of the nervous process as a streaming of ions and of the nervous architecture as corresponding to the general features of my diagrams. Let no one think that the experimental proof of such a nervous "deflection" is a doubtful possibility of a remote future. The deflection was demonstrated ten years ago by a brilliant experiment of Uexküll.[7] In our figure is seen the anterior part of the nervous system of a worm. It is artificially severed at *S*. 1,

[6] *Cf. Human Behavior,* 113-115.
[7] J. von Uexküll, *Ergebnisse der Physiologie,* III, 2, 1904, 10.

2, 3, and 4 are muscles which have a connection of low resistance with the ventral cord, but a high resistance connection with the head ganglion. If the head ganglion is stimulated, the muscles 1, 2, 3, and 4 respond, but weakly, owing to the high resistance of the nervous paths. If at the same time the ventral cord is stimulated at R, 1 and 2 respond strongly, of course, but 3 and 4 respond still more weakly. And if the stimulation of the ventral cord is made very strong, 3 and 4 no longer respond to simultaneous stimulation of the head ganglion at all. That is, the whole nervous process coming from the head ganglion, instead of partly going in the direction of 3 and 4, is now forced to join entirely the stronger nervous process coming from *R* and having its motor outlet in 1 and 2.

If you prefer a demonstration taken from the study of behavior, to an experiment made on a nerve preparation, think of all those facts talked of in the psychological text-books under the head of "attention." Why is it that the enthusiastic chess-player, walking home and at the same time occupied with his next move, suddenly stops in the middle of the street? Because the complex nervous process of preparing for his next move is so much stronger that it deflects the weaker nervous process which controls the muscles of the legs and thus keeps it from sending to these muscles the proper innervations.

I believe, then, to be justified in saying: The mechanical analogies which I have used are few. They are simple. Their significance is comprehensive. And they are easily translatable into physiological terms.

AN INTROSPECTIVE ANALYSIS OF THE ASSOCIATION-REACTION CONSCIOUSNESS

By Emily T. Burr and L. R. Geissler

The present study was begun in the Psychological Laboratory of Cornell University during the Summer Session of 1910 and later continued by one of the writers at Columbia University. In the mean time the other published a preliminary report of some of the results obtained at Cornell,[1] and the present paper is to complete the account of the work.

In the earlier experiments an auditory presentation of the stimulus word was used; but this was later replaced by visual exposure, and the Hipp chronoscope took the place of the stop-watch. Otherwise the experiments were carried on in the familiar way. At the beginning of a series the observer had to choose between two pictures, or two stories, or two boxes containing a collection of articles, to familiarise himself carefully with the one selected, and then to seat himself before the apparatus. The material consisted of four pairs of short stories, four pairs of pictures, and two pairs of boxes. The members of each pair were as different as possible. For instance, a picture of a stone-quarry was paired with a picture of family life, and a story of hardship and tragedy was paired with a humorous, inconsequential tale. In the top of each of the boxes was placed an envelope containing written directions regarding the careful examination of the box. With each box was connected some joke or surprising incident. For example, the observer was told to unwrap a certain small package in one of the boxes and to examine carefully the lobster, which turned out to be an elongated convex mirror in which the observer saw his face reflected. Another box contained live flies in a small bottle, which was so arranged that the observer inadvertently let them out. Likewise the pictures and stories were selected on the basis of the strength of their emotional appeal, in order to make the complex as intense and vivid as possible. The observers were in each case comfortably seated, generally separated from the experimenter by a screen, and the room was so arranged that their attention was not distracted.

The first instruction given to the observer was: "Choose one or the other of these two materials (pictures, stories, or boxes), examine it carefully, and do not let the experimenter know which one you have selected." Immediately after the examination of the selected material the following instruction was given: "I am going to show you (or pronounce to you) one by one, a series of words, and you are to give as soon as possible, in response to each, a word that is associated in your mind with my word; but do not, if you can help it, give one that is connected with your choice. Then I want you to

[1] L. R. Geissler, A Preliminary Introspective Study of the Association-Reaction Consciousness, this Journal, xxi, 1910, 597-602.

describe as clearly and concisely as you can your state of mind and its contents from the time of the ready-signal until you gave your associated word." To this was later added: "Give the mental events in their temporal sequence and try to give more details of the background of consciousness." The experimenter avoided all questioning and other suggestive remarks.

The observers at Cornell University were Miss E. Gunning (*Gu*), then Inspector of Schools at Amsterdam, Holland; and Messrs. G. C. Basset (*Ba*), R. Hugins (*H*), then Assistant in the Psychological Laboratory, Dr. T. Okabe (*O*), late Fellow in Psychology, and Dr. L. R. Geissler (*G*), then Instructor in the Department. At Columbia University the observers, with the exception of one senior in the Law School, were students in the Department of Psychology accustomed to serve as subjects in experimental work. They were Misses Ellison (*E*), Boshitsky (*Bo*), and Kupers (*K*), and Messrs. B. K. Fiske (*F*), and Myers (*M*). Altogether about one thousand association-reactions were obtained.

The quantitative results do not present anything new. Occasionally an irrelevant stimulus is followed by a delayed association, which is usually due to the fact that too many ideas rush through the background of consciousness and interfere with one another and with their articulation in a reaction. Such cases—they also occur where no instruction to conceal anything exists—are not so frequent as to influence to any great extent the average association-time for irrelevant stimuli. The longest reaction-times occur invariably with critical words; and their mean variation is considerably larger than that for irrelevant words. Critical stimuli were sometimes answered by quick and insignificant associations; but if followed up by one or two more critical stimuli, the complex could no longer conceal itself, and was manifested either in delayed or in significant associations or in both together.

Of much greater importance are our qualitative results as revealed in the introspections. They have led us to the conclusion that what is usually spoken of as the concealing of a complex is but a special case of a consciousness under negative instruction, the only essential difference being a greater degree of emotional vividness and strength in the complex. Consciousness under a negative instruction differs, as one of the writers has shown in a previous article,[2] from a consciousness under a positive instruction in that the former involves a stage of suppression and a conflict of at least two determining tendencies, while the latter requires only one such tendency. In order to prove our contention we may now compare the consciousness while trying to conceal a complex in the association-reactions, referred to hereafter as A.R. experiments, with the consciousness under a negative instruction, abbreviated into N.I. experiments.

The first similarity lies in the nature of the *Aufgabe* itself; for "not to betray oneself" is only one particular negative instruction. It does not even matter whether linguistically the instruction is formulated in a positive way; its intent is accepted negatively. When we remember how early and how frequently in childhood the instruction "don't" occurs, we need not be surprised at the readiness to respond to negative instructions. They are, furthermore, sanctioned by the principle of mental economy; for it is much simpler and easier to exclude one impossible way of action than to include all the possible

[2] L. R. Geissler, Analysis of Consciousness under Negative Instruction, this JOURNAL, xxiii, 1912, 183-213.

ways. Now it will be remembered that our particular instruction in the A.R. experiments accidentally emphasised somewhat the negative aspect by its linguistic form. The N.I. experiments, which were carried on a whole year later, were suggested by Langfeld's first article,[3] and their instructions were taken from his work, which *G* did not happen to see until his own preliminary report was published. It will be seen, therefore, that the two investigations were completed independently of each other, with no initial intention in *G*'s mind to compare the two consciousnesses. In fact, the similarity became evident only while the results of the second investigation were being prepared for publication.[4] We wish to emphasise this fact in order to exclude the possible suspicion that our first instruction was purposely framed in the negative form in order to produce an artificial similarity. But, even aside from this fact, it is self-evident that in any Freudian complex of concealment a negative task of suppression is involved, whether it be set by conventional rules of society or by the individual himself in accordance with his own nature, his own ideals and determinations. In all these cases the underlying thought is: I must not or I ought not to do so-and-so. Since, then, here as well as in our A.R. experiments the virtual instruction is 'not to betray oneself,' and in the N.I. investigation the actual instruction was 'not to name the picture or object exposed,' it should not be surprising that the consciousnesses involved in these cases are similar in all essentials, as the following descriptions and illustrations will show them to be.

Some of the introspections on the fore-period read, in the case of *O* for example, as follows: "My bodily posture was stiff and quiet; I held my breath, kept my right ear turned toward the experimenter, and had many organic sensations." At the signal 'Ready' he tended to assume an attitude of indifference, in which there was no particular attempt to control the mental processes (no one content of which was very vivid), and he found it impossible to localise the organic complex and the strain sensations. *Ba* says that "the effort to fix the attention" is accompanied by a strong tendency to assume some specific bodily attitude. Generally, therefore, at the signal 'Ready' he sits with his head resting on his right hand, his right elbow on his knee, his brows contracted, his jaws set, and his lips stiff. A very similar description is quoted in *G*'s preliminary report,[5] and the accounts of the other observers do not differ essentially from these examples. *H* experienced also many verbal ideas, as "I'm ready for the stimulus, let it come," or "I'm on my guard, I won't let her catch me this time," and the like. The women observers speak with greater emphasis of the "intense excitement" during the waiting period, and of "fears of self-betrayal" present in verbal form and "strongly unpleasant." In these descriptions we do not find a literal repetition of the instruction, but it is nevertheless represented either in a modified verbal form or in the whole bodily and mental attitude toward the experiment. There is one difference between these fore-periods and those of the N.I. experiments. In the latter, the fore-period became more and more mechanical, poor in conscious contents,

[3] H. S. Langfeld, Suppression with Negative Instruction, *Psych. Bull.*, vii, 1910, 200-208.

[4] L. R. Geissler, this JOURNAL, xxiii, 1912, 212 says: "A comparison of the consciousness under N.I. with the consciousness of the hidden complex is reserved for a future occasion."

[5] L. R. Geissler, this JOURNAL, xxi, 597.

and vague and faint in their degree. In the former, no such degeneration was noted, presumably because of the element of uncertainty, since not every stimulus was relevant, so that attention was kept alert. The same effect would probably have been produced in the N.I. experiments by the introduction of blank stimuli.

It had been found in the N.I. investigation that the mid-period could be subdivided into three stages, those of recognition, of suppression, and of suspense or search. The same thing is possible in the present work. In the first stage the stimulus-word is received and as a rule carefully examined as to its connection with the chosen material. For example, *K* says: "My first thought after any stimulus is given me is of the instruction. When I see that no connection can exist between the stimulus-word and the experimental material, I can find a reaction and give it with a deep sense of relief. When I realise that the stimulus is a critical one, I often have unpleasant blood pressure in my face, a palpitating feeling, and I say to myself, 'oh, that's dangerous.'" Similarly *Gu* says: "I try each word to see if it be a safe stimulus." *O* finds that he has mentally gone over the entire material, point by point, and when he realises that there is "no danger" in the stimulus, he assumes a relaxed attitude and the formation of a reaction is automatic. *H* says to himself in internal speech such phrases as "detective on my track, I mustn't incriminate myself" or "now we are on the path of the story, I must be careful." *E* seems to place herself each time in the situation suggested by the stimulus; she reads the story or event into every stimulus-word, and if it is not a critical one she usually has a verbal idea of "I'm all right, perfectly safe," and the tense strain and kinaesthetic sensations give way to "a sense of relief and peace." It is unnecessary to multiply the examples. The recognition of the irrelevancy of a given stimulus-word usually leads at once to an indifferent association.

As soon, however, as the connection between the stimulus and the chosen material is discovered, a stage of suppression sets in, or, as one observer says, "there is great difficulty in retaining a 'give away' reaction-word." This is to us the most important similarity between the two kinds of consciousnesses to be compared. *Gu* says: "The recognition that the stimulus is a critical one makes me strive to get away from the experimental material as quickly as possible," and likewise *E* writes that when a critical word appears she tries to get away from the influence of the dangerous material in order to find some non-incriminating connection. The other observers tell of similar suppressions of incriminating associations. *H*, for example, says in one case: "The knowledge of the picture seems to possess me like an obsession, and I can get no other thought." In other words, we have here the same two phenomena as in the N.I. experiments, on the one hand the failure of a negative instruction to keep forbidden ideas entirely out of consciousness, and on the other hand the successful avoidance of their motor discharge, that is, the inhibition of the reaction of articulation. Although some of the present introspections are not as detailed and technical as those in the N.I. experiments, it is nevertheless possible to divide most of these descriptions of the suppression into the two classes of attitudinal and ideational suppression that were found in the N.I. investigation. The attitudinal suppression, for example, is very strikingly intimated by *K* when he says that "the whole state of inhibition is something like the feeling one has when told by a doctor to say 'ah' while he holds down the tongue with a spoon." Another illustration is given by *Ba* who describes the suppression as a "tendency to hold the breath, a rigidity of the muscles of the throat, accompanied by a fixed staring of the eyes," while four observers speak of their tongue as involved in the inhibitory process or mention tight closing of the eyelids, frowning and squinting. As examples of the ideational form of suppression we may quote *Ba*'s words: "Oh, I can't say this, for the stimulus is connected with the story;" and *K*'s introspection after the critical stimulus 'execution:' "In the background of consciousness there seemed to lurk, with uncompromising persistence, that danger reaction 'guillotine' and the verbal idea 'if you don't give that it will be hard to find another reaction.'"

The realisation of the connection between the stimulus-word and the selected material is described in most cases in terms of visual and verbal imagery. This realisation is, of course, one of the inessential differences between the two kinds of mid-periods compared, and is due to the peculiar experimental condition of distributing significant words among irrelevant stimuli. Another difference in the two stages of suppression is the stronger unpleasantness accompanying the effort to suppress an incriminating idea. *G* has described the feelings or emotions which in his case characterise the presence of a complex as "excitement and anxiety on the one hand, and relief on the other."[6] Other observers speak of "an indescribable state of anxiety," of "effort and strain," of "a feeling of helplessness," of "a feeling of breathlessness and a wild desire to escape from all ideas suggested by the critical word," of "a feeling of despair," and the like. The exaggeration of this emotional aspect in Freudian complexes, especially in subnormal minds, seems to have obscured a more critical and analytical view of the true nature of the consciousness of concealing, and to have prevented psychologists from seeing it in its normal aspects. It seems to us that the emotional feature is essential neither to the act of concealing or suppressing nor to the original complex itself which sets up the determination to conceal or to suppress. Whether the additional emotional aspect is responsible for most of the abnormal consequences of suppression is another question; but it seems to us that most of the complexes of normal everyday life are not marked by very strong feelings. The main emphasis must be put on the stage of suppression itself, and its conscious and (hypothetical) neural mechanism has been fully described in the previous article on consciousness under negative instruction.

It must be likewise understood that the stage of suspense or search, the last of the three stages of the mid-period, is only a by-product of the peculiar experimental conditions which require an association-reaction in the shortest possible time-interval. This requirement is a new instruction in itself, which sets up a separate determining tendency and which is interfered with in its normal course by the preceding stage of suppression. This stage of suspense is described by our observers in cases where no ideas will come into focus as "a groping attitude," "a floundering of ideas;" there is "a feeling of the awful passage of time," "a search for new connections," a situation in which "nascent processes seem all in a jumble, so that no one idea can be seized," a state in which "the light has been taken away and left a gray voidness," or "a period filled with vague, indefinite thoughts" for the expression of which the observer

[6] *Op. cit.*, 601.

is "unable to get any words, a stunned, helpless feeling." One typical concrete description of this whole stage of suspense may be quoted from *H,* who says "after the critical word 'sofa' was rejected, one verbal idea after another came. I felt each one, but could not form the corresponding words. Two or three seemed to come almost simultaneously, and it was hard to say which idea came first. A mood of indecision marked the entire period. I mentally compared the images, one with another, and the more I did this, the greater became my anxiety over the extremely long time. I was trying to form a reaction, yet I could not select any one of these images." Frequently the stimulus word is mechanically repeated several times in internal speech until a safe association arises.

The after-period in these experiments is again like that in the N.I. reactions, in that it contains the same kind of feeling of satisfaction and the same kind of self-justification or self-criticism with regard to the nature of the given response. *O* says, for example, "After having given a response, I try to analyse it again with reference to the significance of the stimulus word;" *E* says that she has "a feeling of justification after having spoken a word that could have no reference to the critical material;" and others make similar assertions. This shows that, as in the case of the N.I. experiments, "the mental processes of the after-period are still under the influence of the determining tendencies set up by the instruction."[7] How much, in fact, the whole consciousness is under their influence is easily seen from the external behavior of the observers during a critical experiment. The experimenter often noted contracted eye-brows, tightened lips, squinting of the eyes, raising and bowing of the head, clenching of the fist, restless movements of the feet, uneasy stirring of the body, deep breathing, and quick short laughs seemingly of relief that a successful response had been made.

We have now shown how close the parallel is between the consciousness of concealing a complex in the association-reactions and the consciousness under negative instruction, and we hope that this comparison has thrown some new light on the two phenomena. Whatever complications may enter into the consciousness of a Freudian complex, as met in everyday life or even in subnormal minds, they do not seem to us to be of such a nature as to obstruct seriously the subordination of the particular effort to conceal under the general task not to do a certain thing. In conclusion it may be pointed out that our results furnish fresh evidence for the fruitfulness and reliability of the recently much debated psychological method of introspection.

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[7] *Ibid.,* 204.

"THE FEELING OF BEING STARED AT"—EXPERIMENTAL[1]

By J. E. Coover, Ph.D.

Summary

Experimentation with ten normal reagents to the extent of one hundred guesses each, as to whether they were being stared at during a fifteen-second interval, results in an astonishing approximation to the probability figure when the events are controlled by chance—namely, 50.2 per cent of right cases.

Introspective analysis reveals subjective factors that control the guessing and that confer upon guesses a strong feeling of certainty. These factors are sufficient to account for the commonness of the belief, and they are of the nature of incipient hallucinations and motor automatisms.

Introduction.—Titchener in an article in *Science*,[2] some years ago, stated that students in his junior classes believed they could feel anyone staring at their backs, that laboratory experiments proved the belief to be groundless, and that the belief is to be accounted for by *1.* a natural nervousness resulting from anxiety about how one's back looks; *2*, inhibition, due to the dictates of good-breeding, of the natural impulse to turn around to see if some one is staring; *3.* some one sitting behind, whose attention was attracted by the signs of nervousness, is caught "staring." It occurred to the writer, who had undertaken a general research upon the telepathic problem, that it would be of interest to know *1.* whether the belief above referred to is common with our students, also, *2.* whether it is shared by the men as well as by the women, *3.* what statistical results would yield in the case of a fairly large number of guesses by reagents who believe in the reliability of the feeling, *4.* and what factors in the experience in guessing would correlate with the greater degrees of certainty with which the guess is delivered. These factors, obtained from the introspections, would seem to be not only the essential processes at work when the reagent has 'the feeling of being stared at,' but also the elements of experience accountable, in part, for the belief that the feeling may be relied upon; they would then furnish an explanation supplementary to that given by Titchener in general psychological terms. In case the

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[1] This is a part of the experimental work carried on in "Psychical Research" (endowed by Thomas Welton Stanford of Melbourne, Australia) in the Department of Psychology, Leland Stanford Jr. University, during the year 1912-1913. Acknowledgments are hereby made to Dr. F. Angell and Professor Lillien J. Martin of the department for courtesies extended to the researcher.

[2] E. B. Titchener, "The Feeling of being stared at." *Science,* viii., 1898, 895-897.

'feeling' is instinctive, as is suggested by the phylogenetic origin proposed by Titchener, these factors would serve to set off the feeling, to develop it in definiteness, to make it frequently active, and to give confidence in its reliability.

Replies to a questionnaire from 146 students out of a class of 169 in General Psychology, showed that 68 per cent have "the feeling of being stared at, with the conviction that the feeling can be (more or less) relied upon." Students of both sexes share the belief; 47 per cent of the writers were men and 60 per cent of them stated the belief.

That this class was not exceptional in possessing the belief is shown by 95 returns from another class of 102 in psychology (Mental Hygiene): 86 per cent of these students shared the belief; 54 per cent of the writers were men, of whom also 86 per cent stated belief.

Method.—Ten students, ranging from sophomores to seniors, from the class in General Psychology, who held the belief, made one hundred guesses each. The experiments usually occupied the reagent three or four laboratory hours, given on as many days, one week apart. The conditions usual in the psychological laboratory, of quiet, regularity of time and procedure, etc., obtained.

The interval of 15″ (20″ for the first five reagents) for forming the guess was begun by a pencil-tap by the experimenter and ended by two taps. After announcing aloud "yes" or "no" the reagent wrote out introspections involving *1.* condition of the mind during the interval; *2.* the kind, vividness, temporal course, and spatial attributes, of the imagery; and *3.* the grade and reason (if the grade was high) for the felt certainty of the guess. In order to avoid coincidence of similar mental tendencies in forming the series of experiments, the experimenter made up the series from a dice-box as guessing progressed. Upon recording the last guess he shook the box containing a die; if an odd number of spots were cast, he prepared to stare; if an even number, he prepared not to stare; and tapped off the interval in all other respects uniformly. Single experiments did not follow at a higher rate than one *per* minute; at the beginning of each reagent's work, until he became fairly critical in introspecting, they occupied from three to ten minutes each.

The reagent sat comfortably, with eyes closed and shaded with one hand, and with his back toward the experimenter. He knew that the series of experiments was controlled by the die-spots. When the experimenter stared, he 'stared hard' during the whole interval, 'willing' strongly that the reagent 'feel' it.

For reagents I-V the conductor of the research acted as experimenter; for reagents VI-X respectively there was for each a different experimenter, but of the same sex. Reagent I was the only man in the group.

Results.—The following table gives the results of both groups of reagents, and the averages.

TABLE I

Reagent	Not Staring Total	Not Staring Right	Staring Total	Staring Right	Total Right
I	51	28	49	22	50
II	45	25	55	26	51
III	62	23	38	20	43
IV	53	24	47	25	49
V	51	28	49	27	55
	—	—	—	—	—
Avg.	*52.4*	*25.6*	*47.6*	*24.0*	*49.6*
VI	45	23	55	33	56
VII	56	21	44	23	44
VIII	48	25	52	26	51
IX	55	26	45	24	50
X	52	22	48	31	53
	—	—	—	—	—
Avg.	*51.2*	*23.4*	*48.8*	*27.4*	*50.8*
Grand Avg.	*51.8*	*24.5*	*48.2*	*25.7*	*50.2*

Of the 1,000 guesses, 50.2 per cent were right (P.E., 1.78; M.V., 3.10); 47.3 per cent of the guesses when the experimenter was "Not Staring," and 53.3 per cent of the guesses when he was "Staring," were right. The die-spots came even 51.8 per cent of the 1,000 throws, conditioning this *per cent* of "blank" experiments.

Since six of the reagents guessed "yes" in excess of "no" (III. 18 times, IV. 8, VI. 10, VII. 16, IX. 6, and X. 22), while but three guessed "no" in excess of "yes" (I. 10, II. 8, VIII. 2), resulting in a general average of 6 "yes" guesses per 100 in excess of the "no" guesses, the excess of 53.3 per cent right guesses "when the experimenter stared" over the 47.3 per cent of right guesses "when the experimenter did not stare" is without significance; if half of the excess of "yes" guesses is deducted from the "Staring" experiments, the 53.3 per cent is reduced to 50.2 per cent. The total right guesses for each reagent is the significant figure. The limits are 43-56 and deviate from probability about equally; this size of deviation could be expected 322 times in 1,000.

Considering that theoretical probability is 50 per cent; that our result of 50.2 per cent ± 1.78 falls between it and the experimental probability found by Quetelet[3] in 5,460 drawings from equal numbers of white and black balls (white balls 50.48 per cent); and that an experimental series of our own (frequency of odd numbers on the dice) for the same number of experiments gives 51.8 per cent; we may conclude that no cause besides chance has been found working toward right cases.

There are other ways in which the results may be distributed to show that there is no conspicuous "bunching" of right cases in any of the rubrics, and that therefore the consistency of mutual support adds to the certainty that there has been no influence beyond chance operative toward right guesses.

In some of the experiments, the distance between the experimenter and reagent was varied for the purpose of finding the influence of

[3] Quetelet, *Lettres sur la théorie des probabilités,* p. 57.

distance upon any factor above chance that might be found to be working for right guesses. The following table gives the gross averages and the per cents of right guesses for the various distances in meters.

TABLE II

First Group

Distance	1	2	3	4.6	6	10 meters
No. Guesses	80	140	80	60	50	80
Per Cent Right	46.3	49.3	55	45	54	51.3

Second Group

Distance	2	4	6 meters
No. Guesses	160	100	20
Per Cent Right	45	53	50

But, as Venn says, anything may happen in a chance series, and it may be charged that all the guesses given with a low degree of certainty (a feeling that the guess stands a small chance of being right), by a freak of chance, may have run greatly under the probability-figure for right guesses, and may thus have counteracted in our final per cent for each reagent the influence of a force working for right guesses to be found in those guesses given with a stronger feeling of certainty.

A tabulation of right and wrong guesses under their correlated grade of certainty (recorded in the introspections), however, shows no significant advantage on the part of any reagent for his more certain over his less certain guesses. The following table shows a total of such values from reagents whose grading was definite.

TABLE III

Guesses Given with Various Grades of Certainty

Grades	A	B	C	D	"Pure Guess"	Total
No. Guesses	15	332	264	61	36	708
Right	10	166	129	33	22	360
Per Cent Right	67	50	48.8	54	61	50.8

It seems pretty clear that, if there is a capacity to be aware of being stared at, it is not, as Richet thought of telepathic phenomena, shared to a slight extent by normal persons, but must be confined, as James suspected,[4] to subjects whose sensibilities have been augmented beyond a "critical point" through hypnosis or other abnormal conditions.

Our reagents who had more or less confidence in their ability did not under the favoring conditionss of our experimentation prove their power. Their belief must be largely based upon those subjective factors which enabled them to deliver some guesses with a strong feeling of certainty, and partly perhaps upon undue consideration of cases in which they have "verified" their feeling by catching the starer.

[4] *Proceedings Society for Psychical Research,* 1896-7, 12:4.

Qualitative Results.—Introspections show in what manner guesses are determined, and reveal the factors of experience that contribute to the guesses a feeling of varying grades of certainty that the guess is right.

Certainty is contributed to the guess by (1) some attribute or content of the imagery, (2) kinaesthetic sensations or images, or (3) inferences from sound sensations resulting from the experimenter's manipulation of apparatus, etc., or from other subjective processes.

(1) When the content of visual imagery involved the attitude of the experimenter, it determined the guess according to whether the experimenter was looking straight ahead or looking away. When this imagery was vivid, or if it appeared with facility (liveliness) and at the beginning of the period, or was persistent or recurrent, the guess was given with a feeling of greater certainty. (Reagent I said, "when the direction of the look is seen from the face only, I give the guess Grade C; if from the eyes, Grade B or A.")

The visual imagery may be weak, and when it appears at all be accepted as a sign of being stared at. For Reagent II, visual imagery of the experimenter or of a school-room, in which she first experienced vividly the feeling of being stared at, yielded a "yes" guess.

Sometimes the content of the visual imagery was probably suggested by auditory impressions of the experimenter's movements when the latter were not pronounced enough to be singled out for "inferences" as treated in Section (3).

Those who depended largely upon visual imagery were Reagents I, IV, V, VIII, IX, and X.

(2) Some reagents were much occupied with kinaesthetic impressions during the interval. Thus for reagents III, VII, and VIII, the most characteristic cue for a highly graded guess was an almost irresistible impulse to turn around, or a tension of muscles in the neck and shoulders; for X it was the kinaesthetic sensation in the right temple; sometimes the kinaesthetic impressions were not localised, but were indicated by "a feeling of restlessness;" Reagent VIII also speaks of a "feeling of discomfort" with a "desire to turn."

The imagery of Reagent V involved a visual or at least a spatial element consisting of an imaged straight line or beam from the experimenter's eyes to the back of her head; and a marked kinaesthetic impression, leading to "yes" guesses given with a high degree of certainty, was tension of the eye-muscles toward this line. "Attention and eyes drawn toward line," was a frequent introspection for guesses given with a higher grade of certainty. She also has clear visual imagery of the experimenter either accompanying this kinasthetic impression, and including the "line," or of the experimenter's face turned away. The visual element gave way, in the course of her experimentation, to the kinaesthetic as a guide to the more certain guesses. Reagent VII also mentions this "line" in her visual imagery of the experimenter, and speaks of a "feeling of connection."

The kinaesthetic impressions involving restlessness, desire to turn, strain in the neck-muscles and in the eyes, were shared by other reagents who had other modes of imagery from which they made their guesses; as, V, VIII, IX.

More subtle kinaesthetic imagery was sometimes evidently of influence in determining the guess. Reagent IX "felt like answering a call of her name;" and VIII recorded a "feeling of being alone" which was a positive determinant for a "no" guess; and of a "feeling of being criticized," or a "feeling of nearness to the experimenter," both of which yielded "yes" guesses.

(3) Inferences were sometimes drawn by the reagent from sounds of the experimenter's manipulation of apparatus or his conduct of the experiment. After shaking the dice-box, the experimenter waited until the second-hand was coincident with a five-second dial-mark before he tapped. Reagent I noticed variability in the length of this interval, and inferred that longer intervals were caused by preparing "to stare;" and he confidently gave for these cases "yes" guesses; he sought for a basis for inference when at a point in his series impressions failed to come during the interval. Other reagents noted in the pre-period a sound of movement from the rustling of clothing, and inferred that the head was being raised "to look;" when such impressions came within the interval, the reagent inferred that the experimenter was not looking. Reagent VIII "knew from her movements" the experimenter was not looking, and also inferred that "harder taps" were signals for a "yes" guess.

Even when such impressions are not used in "inferences" they may conceivably influence the guessing by being taken advantage of subconsciously. It is impossible for the experimenter to maintain perfect uniformity in his conduct of the experiment, which involves, among other things, length of the various intervals, breathing, manipulation of the dice-box, intensity and accent in tapping, slight bodily movements, etc. Great effort was made, however, to maintain uniformity, and this may in part account for the lack of an excess of R judgments.

Inferences may also be based upon hypotheses, and depend in their outcome upon subjective conditions; *e.g.*, Reagent VII inferred from internal distraction that E was not staring, or the distraction would have been overcome; and entire absence of impression was inferred to mean that none was sought to be made.

Other tendencies were also noted: "What did I answer last" influenced Reagent II, who was obviously endeavoring to keep positive and negative guesses about equally frequent. She also occasionally made up her mind, Marbe-fashion,[5] to say "yes" next time; but since the series was not voluntarily made by the experimenter, coincidence due to like tendencies of the two minds was excluded. And when she was "tired and bored" she wanted to say "no," as a general protest to further experimentation.

Conclusion.—Our conclusion, with respect to normal reagents, is (1) that the belief in "the feeling of being stared at" is quite common (shared by over half of the university students); (2) that experiment shows it to be groundless; (3) that there is an explanation supplementary to that mentioned by Titchener (nervousness, attracting attention, turning, catching the gazer) for the existence of the belief, lying chiefly in *attributing an objective validity to commonly experienced subjective impressions in the form of imagery, sensations, and impulses.* This is a tendency which, under favorable conditions, works itself out in Hallucinations and Motor Automatisms, and it seems to be a common trait in normal adults.

[5] K. Marbe, Ueber das Gedankenlesen und die Gleichförmigkeit des psychischen Geschehens. *Zeitschr. f. Psychol.*, lvi., 1910, 241-263.

PROJECTION OF THE NEGATIVE AFTER IMAGE IN THE FIELD OF THE CLOSED LIDS

By Frank Angell

In the preceding numbers of this Journal, the writer together with one of his students set forth the results of an investigation on the apparent distance of the negative after-image in the field of the closed eyes. Up to the time of printing, the writers had found no material bearing on the topic save casual references by Fechner and Hering. The fated article which, of course, had a Teutonic existence somewhere, has turned up in v. Graefe's *Archiv* for 1885 with Dr. G. Mayerhausen as sponsor and as title "Ueber die Grössenverhältnisse der Nachbilder bei geschlossenen Lidern" (*Abteilung* 2 S. 23-24). The results differ so widely from those published in the Journal as to call either for explanation or discussion or both.

Experimenting on himself, Mayerhausen finds that the negative after-image in the darkened field appears as large as the inducing object when the latter is set at a distance of two meters. In the case of the Stanford observers, three in number, unacquainted with each other's findings, the region of equality lay inside of eighteen centimeters from the cyclopean eye.

Mayerhausen's method was as follows: As inducing objects he used four white discs, 1, 2, 4, and 8 cm. in diameter respectively, and developed after-images from them at ten different distances from the eye, beginning with five meters and running down to 10 cm. When the after-image was at its maximum clearness, he opened his eyes and compared its diameter with the readings of a centimeter scale held in his hand. In this way, he made eighty readings, viz., two for each size after-image at each of the ten distances. The writer humbly submits that this is a less trustworthy procedure than that described in the Journal where the after-image with closed eyes was compared with the image projected on an adjustable screen until a region was found where the two images showed no appreciable difference. Within this region, after-images were induced and their size measured by projecting them on a ruled screen. The equating of the two forms of after-image was almost as accurate as if the images had been actual cardboard disks, successively compared. The estimation of the projected image was harder but nevertheless the size of the mean variation of the readings (from 4% to 10%) for the three observers showed that the judgments were based on actual if rough measurement.

Mayerhausen combines all his figures into a composite curve in which the abscissal distances are the distances of the inducing discs from the eye and the ordinates are the ratios of the diameters of the inducing discs to the estimated size of the corresponding after-images. The curve indicates a very rapid falling off in the size of the after-images from 10 to 50 cm., and a gradual decrease up to the farthest distance used, viz., five meters. At 30 cm. the image is more than twice as large as the disc itself; at 100 cm. it is 1.17

times as large and at 200 cm. as stated above, image and disc are equal.

Of course the obvious answer to the discrepancy of results is that the methods were different. Accordingly the next thing to be done was to try Mayerhausen's way of measuring after-images with a centimeter rule, though instead of the hand we ventured to use a tripod placed at a convenient distance. The results of these measurements are given below:

Observer *W. T. R.*, assistant in laboratory experiments in preceding investigation on after-images. May have looked for small images, but was simply given directions for repeating Mayerhausen's experiment. Each figure is the average of eight readings:

Distance of square from observer		1 m.	2 m.	3 m.	4 m.	5 m.
Estimated size (in mm.) of image of	8 cm. disc	12.6	9.8	6.7	4.8	2.8
	4 cm. disc	6.6	4.8	3.0	2.3	1.5
	2 cm. disc	3.0	2.2	1.7	1.1	1.1
	1 cm. disc	1.8	1.1	1.–	0.5	0.2

Observer *F. A.* Was observer in preceding investigation, anticipated relatively small size of images. Experimented only once with the 3 cm. and 4 cm. discs. Was usually unable to dectect the after-images from a one cm. disk placed at 3, 4 and 5 meters, and not alway when placed at 2 and 3 meters. When seen, the image was roughly estimated as somewhere near one millimeter.

Distance of 4 cm. disc from observer.	1 m.	2 m.	3 m.	4 m.	5 m.
Size of after-image in mm. (4 trials)	5.10	2.3	1.53	1.2	1 and less.

Observer *G. T.*, advanced student in Psychology, had not served in previous investigations and was unacquainted with its results. Experiments only with 1 cm. disc.

Distance of 1 cm. disc from observer.	1 m.	3 m.	5 m.
Size of after-image in mm. (5 trials)	2	1	0.5

These figures accord with the general experience of the laboratory. In the introductory laboratory course, the students are usually unable to find, at the outset, the after-image of the 1 cm. disc placed at a distance of 40 to 50 cms. as they usually look for an image which approximates the object in size instead of the minute figure of 2 or 3 mms. which they finally detect.

It is at this point that Mayerhausen's figures are wholly inexplicable to the writer. It is conceivable that with the larger discs one can imagine a greater or less distance of projection and so perhaps a greater or less size. But the images from a 1 cm. disc placed at 4 to 5 meters are simply minute gray points. If one uses a black disc on a white ground the after-image will be a point of faint light, not unlike a star of small magnitude. But Mayerhausen estimates the size of the after-image from the 1 cm. disc placed at a distance of 5 meters as from 4 to 6 mm. which is larger than our estimations for a 4 cm. disc acting from a distance of 2 meters. At the outset, however, Mayerhausen instances an experiment which he seems to consider eliminates the factor of distance in explaining the size of projected after-images. This crucial experiment he describes as follows: "If, in the after-image experiment, I fixate a distant wall, and while fixating, slowly push in a sheet of paper between my eyes and the wall (say at a distance of 30 cm.), the image now appearing on the paper will retain exactly the same size which it formerly had on the wall." "Daraus geht zur Genüge hervor," he continues, "dass bei gleich bleibender Fixation es für die Grösse eines Nachbildes absolut gleichgültig ist, wo die Projectionsfläche liegt." This is surely a feeble lever for overturning so well-grounded a structure as the theory of the relation of size and distance of projection of after-images. For it is evident that what we have in this experiment is a blending of the wall and the paper to a common surface which is projected to the distance of either wall or paper. If the after-images of two discs say of 5 cm. diameter, placed 5 or 6 cms. apart, are projected on the wall and then the paper is pushed slowly in until its edge falls between the images of the disc, one may get two sizes of disc images, one corresponding to the distance of the wall, the other to the paper. In fact, Mayerhausen's 'blend' experiment is evidence against his argument, for the sizes of the after-images will correspond either to the supposed distance of the wall or of the paper, and to no other distance.

As distance of projection is not for Mayerhausen the main factor in determining size, convergence must play the chief rôle, and accordingly he concludes that equality of after-image and inducing object at the distance of two meters, is owing to the intersection of the optic axes at this distance. When the eyes are closed, he assumes that convergence at 2 meters forms, at first, a sort of state of provisional equilibrium, which passes over into parallelism of the axes in sleep (*op. cit.*, S. 25). He further assumes that when the object fixed is at a distance, say, of one meter, and the eyes are then closed to develop the after-image, the stronger convergence does not pass over into the state of equilibrium, but a lesser relaxation follows which in his case multiplies the size of the object by 1.17, though, of course, the retinal image is doubled in diameter.

It does not appear that Mayerhausen investigated the movements of the closed eyes when developing after-images. Assuming that the distance of projection depends on convergence, he concludes that the motions must have taken place to produce the results indicated by his figures.

But it certainly is a matter of no great difficulty to acquire the power of converging the eyes at will when closed or when in a dark room and when the trial is made it will not be found to have much influence on the size of the after-image. Hering says that he can alternately change parallelism of the optic axes for the greatest possible convergence without changing the apparent size of the after-image in the field of the closed eyes,[1] and the writer has not been able to note any regular change in the size of the after-image when repeating the experiment. It even makes no difference to the apparent size and position of the after-image if one eye is displaced by pressing on the bulb, a condition which, of course, with open eyes produces double-images.

It is unfortunate that, in this and other psychological investigations, observers obtaining such different results from the same problem can not come together and compare methods of work. The discrepancy is probably owing to some unnoted difference in the details of the operations which joint investigations could bring to light,—to the advancement of knowledge and the saving of printers' ink.

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[1] E. Hering, *Beiträge zur Physiologie,* I, 138.

PROFESSOR MARTIN ON THE PERKY EXPERIMENTS

I have received the following letter from Professor Martin:

STANFORD UNIVERSITY, CAL., July 21, 1913.

DEAR PROFESSOR TITCHENER,

In mentioning only the three reagents who took part in Series IIb and IIc, in your recent discussion (this JOURNAL, xxiv, 124) of the investigation regarding the difference between memory and imagination images, found in my work *Die Projektionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder,* you certainly give an incorrect impression as to the data upon which I base my impression regarding the Perky investigation.

Series II, IIa, IIb and IIc were all made with the purpose of getting at the difference between the images of memory and imagination. In Series II and IIa in which the results (see p. 78) as well as those of IIb and IIc do not confirm those of Perky and in which I naturally place more confidence, on account of the wider experience of the reagents, not alone as *Versuchspersonen* but as *Versuchsleiter,* the following *ten* members of the Psychological Institute of Bonn University acted as observers (see p. 8): *die Doktoren Baumgarten, Behn, Bühler, Morisse, Rieffert, Schanoff, Professor Girgensohn; die Herren Kemp, Köhler und Silberstein.*

I shall, of course, be very glad if you will publish this letter in THE AMERICAN JOURNAL OF PSYCHOLOGY.

Very sincerely yours,

LILLIEN J. MARTIN.

I should be sorry to give an incorrect impression of Professor Martin's work,—though I can hardly have done so to a careful reader,—and I therefore add a word of explanation. Series II and IIa were completed before Professor Martin became acquainted with the Perky experiments, and were made by her own method of projection (*Z.,* lxi., 1912, 398). Series IIb and IIc were made with direct reference to Perky, under conditions which, in Professor Martin's judgment, approximated as closely as possible to those of Perky (*ibid.*). I open my discussion with the words: "I do not intend to offer here any general criticism of Professor Martin's recent work on imaginal complexes . . . . I defer . . . . discussion until further experimental evidence, for or against, is forthcoming. It is my present purpose to examine only a small part of her published work . . . . undertaken as a test of certain results obtained by Perky in my laboratory." And I say later: "We are now taking up the whole problem afresh, with special reference to Professor Martin's method of projection."

In the letter printed above Professor Martin *has not said a word in reply to my criticisms of Series IIb and IIc:* I may take it, then, that she admits the justice of these criticisms. She confines herself to a mention of Series II and IIa, that is, of experiments *performed by a method which I expressly declined to criticise until I had experimental data at command.* E. B. TITCHENER.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXII. The Effect of the Interval Between Repetitions on the Speed of Learning a Series of Movements.

By Mildred Browning, Dorothy E. Brown, and M. F. Washburn

The fact was discovered by Ebbinghaus and thoroughly established by Jost that verbal material is learned with fewer repetitions if an interval is allowed to elapse between successive repetitions; if, that is, the repetitions are 'distributed' rather than crowded together. Now such a process of learning series of words or nonsense syllables involves two factors which may or may not obey the same laws. One factor is the establishment of associative dispositions, whereby one member of the series recalls the idea of other members. The other factor is the establishment of a habit of movement on the part of the articulatory muscles. These factors may be briefly called the associative and the motor factors in learning, and one of the most fundamental problems in the psychology of learning concerns their mutual relations.

In the present study the intention was to set a learning task in which the motor factor should play a larger rôle as compared with the associative factor than is the case in the learning of verbal material, and to see whether the law of distributed repetitions held good for such a task. If we wish to acquire a habit of movement involving the larger muscles of the body, shall we do better if we go through the movement a number of times in immediate succession, or if we allow an interval between performances?

Our problem was constructed as follows. On a sheet of cardboard a rectangle was drawn about thirty inches long by eighteen inches high. This rectangle was divided into eight compartments by lines drawn upon it, four compartments in the upper row and four in the lower row. Each compartment had a single letter printed in it, differing from the letters in the other compartments. The cardboard thus prepared was laid upon a table before the observer, whose eyes were closed. A pack of cards was placed in her hand. This pack was composed of cards, each one carrying a letter corresponding to one of the eight letters of the cardboard diagram. There were sixteen cards in the pack, hence, each letter appeared on two cards. The cards were numbered on the back. The observer, always with eyes closed, sat holding the pack, her hands above a mark on the table against which the center of the diagram rested. The experimenter took hold of her right wrist, and moved her right hand, to the time of a metronome, in such a way as to sort out the pack of cards correctly on the diagram, putting each card in the compartment bearing the letter which corresponded to the letter on the card. After each movement the observer's hand was brought back to the mark in front of the center of the diagram. Thus the observer's hand was guided by the experimenter through a series of sixteen movements. The exact sequence of these movements had been carefully planned and was indicated by the numbers on the back of the cards, while the letters on the front guided the experimenter in the sorting. The observer, of course, keeping her eyes closed, knew nothing of letters or numbers; she simply felt herself being put through a series of movements, and formed some visual images of the position of her hand. She was now caused to go through this same performance repeatedly until she could sort the cards correctly without having the experimenter guide her at all. The experimenter, while still keeping her hand on the observer's wrist, gradually lessened the amount of her control as the observer came to 'know' the movements, until finally a correct sorting was made without contact from the experimenter. A record was kept of the number of repetitions required before this point in the learning was reached.

Each observer learned two such series of sixteen movements. With one series, the repetitions followed immediately one upon another without interval. To enable the repetitions to be made in such rapid succession, thirty packs of cards, exactly similar in arrangement, were provided, so that a fresh pack was put into the observer's hand as soon as she had finished a sorting, the experimenter merely sweeping the pack previously used from the board without stopping to rearrange it. The movement series was always learned in fewer than thirty sortings. A second series was learned with the repetitions at intervals of one minute. This allowed time for the experimenter to pick up and rearrange the cards between sortings. During the one-minute interval the observer's attention was distracted to prevent recall of the movements.

It is evident that one such series of sixteen movements may be a good deal easier to learn than another. If, for instance, the movements come in a sequence that can be easily visualized, the series will be readily learned, In 1911-1912 Miss Mildred Browning made under Professor Washburn's direction a set of experiments on the plan described above, using always the same series of movements, which we may call Series I, for the 'no interval' experiments, and another series, II, for the 'one minute interval' experiments. Great care was taken in the construction of these series to make them of equal difficulty. The results showed that the one minute interval produced decidedly quicker learning than the absence of interval. Since, however, it was always possible that Series II was easier than Series I, Miss Brown in 1912-1913 repeated the experiments, using two new series, A and B. No particular care was taken to make these two of equal difficulty, but in half the experiments Series A was the 'no interval' series and Series B the 'one minute interval' series, while in the other half the conditions were reversed. We also took precautions to eliminate the effect of practice, by making with half the observers experiments in the order 'no interval—one minute interval,' and with the other half experiments in the reverse order. There were twenty observers, of whom twelve had had considerable practice in psychological experimentation. Each observer learned two series. It will be seen that the special practice conditions were almost perfectly uniform, a condition which is not often realized in so small a number of experiments.

The results are stated in the following table. The letters in parenthesis after the numbers are the observers' initials. 'I' means that the series was the first one learned, 'II' that it was the second.

Series A.

With Interval of 1′ I.	II.	Without Interval I.	II.
13(Bu)	5(T)	8(R)	8(B)
11(Q)	11(H)	4(C)	7(P)
4(Wi)	4(Su)	17(S)	6(Th)
5(Wa)	13(Ro)	11(Mc)	7(A)
5(E)	5(L)	6(Ba)	12(Sa)
Av. 7.6	*Av. 7.6*	*Av. 9.2*	*Av. 8*

Series B.

With Interval of 1′ I.	II.	Without Interval I.	II.
11(B)	19(R)	13(T)	19(Bu)
12(P)	3(C)	30(H)	24(Q)
11(Th)	7(S)	13(Su)	19(Wi)
13(A)	18(Mc)	28(Ro)	16(Wa)
11(Sa)	10(Ba)	26(L)	23(E)
Av. 11.6	*Av. 11.4*	*Av. 22*	*Av. 20.2*

It appears from these results (1) that Series B was harder to learn than Series A, and (2) that an interval of one minute is more favorable to learning of this kind than no interval between repetitions. The average number of repetitions in all the 'no interval' experiments was 14.8; the average number in all the 'one minute interval' experiments was 9.6. It further appears (3) that the advantage of the one minute interval over no interval is more marked for the more difficult Series B. The difference between the averages for Series A was only one repetition, while for Series B it was 9.5 repetitions. While there was a good deal of variation in the results from individuals, it seems (4) that on the whole fewer rather than more repetitions were needed to learn a series when another series had been learned just before. The average number of repetitions tends to be a little smaller when the series is the second one learned. This indicates that practice was stronger than any tendency to interference of the older habit with the newer one.

Of course the learning of such series of movements involves both associative and motor factors. In the learning of the easy Series A, particularly, our observers reported that visual imagery helped them. It would not be proper to say that while Jost's experiments proved the value of distributed repetitions in the formation of associative dispositions, ours suggest a similar law for the formation of habits of movement. Jost's material involved motor as well as associative processes and ours involved associative as well as motor processes. We can, however, claim to have shown the probability that a certain degree of distribution of repetitions is favorable to learning in a case where the motor habits are not habits of articulation. And since any habit of movement would involve both associative and motor factors, we may say that there are indications of the validity of the law of distributed repetitions for habit formation in general. The significance of the fact that the advantage of the interval is less marked in the case of the easier series is doubtful. An easy series of sixteen movements, such as the one used here, involves in its learning a good deal of visualization. The pattern of the movements is acquired and held, at least for the easiest parts of the series, as a whole simultaneously present in consciousness. Succession in time, on the other hand, is the special characteristic of processes with a strong motor element. It is possible that the law of distributed repetitions is a motor rather than a purely associative law, and that its validity in the learning of verbal material is due to the motor or articulatory factors in such learning.

XXIII. A Suggested Coefficient of Affective Sensitiveness

By Helen Clark, Neida Quackenbush, and M. F. Washburn

It is a curious fact that in experimental studies of individual psychology no attention, so far as we are aware, has been paid to the characteristic which we shall call affective sensitiveness. By this term we mean a tendency to strong affective reactions, whether of pleasantness or unpleasantness. Evidently affective sensitiveness may be general or special. If it is general, and marked, the individual possessing it will tend to strong likes and dislikes whatever the nature of the material presented for affective reaction. If it is special, he will show sensitiveness to one kind of material and relative indifference to another kind. Hitherto, the only consideration has been whether an individual liked one kind of material and disliked another: our problem is to study his tendency to be strongly affected, in either direction, by one kind and to be left indifferent by another.

In attempting to find a means of measuring this character of affective sensitiveness, we proceeded in the following way. Forty pieces 2.9 cm. square of colored paper, chosen at random from the series of Bradley colors, were numbered on the back. A series of forty nonsense syllables, each composed of an initial vowel and a final consonant, was prepared and each syllable was numbered. The observer was then given the following instructions: "You will be shown successively five colors; then you will have pronounced to you five nonsense syllables; then you will be shown five colors, and so on until you have seen forty colors and heard forty syllables. As you look at each color, you are to express your judgment of its pleasantness or unpleasantness by speaking one of the numbers from 1 to 7, 1 meaning very unpleasant, 2 moderately unpleasant, 3 slightly unpleasant, 4 indifferent, 5 slightly pleasant, 6 moderately pleasant, 7 very pleasant. As you hear each syllable pronounced, you are to judge its pleasantness or unpleasantness in the same way. You will be given a 'Ready' signal before each color is shown and before each syllable is pronounced. Your eyes are to be closed except when you hear the signal 'Now,' when you are to open them and look at a color."

The colors were laid one at a time on a sheet of white paper on the table before the observer, who reported her judgment at once. The syllables were pronounced with as nearly as possible the same force and distinctness. The same order of colors and syllables was always followed. At the end of an entire series free introspective comments were made by the observer.

The results were treated in the following way. For each observer, we counted the number of 'indifferent' judgments made upon the colors, and also the sum of the numbers of 'very pleasant' and 'very unpleasant' judgments. We then divided the number of 'indifferent' or 4 judgments by the number of 'very pleasant' or 7 judgments plus the number of 'very unpleasant' or 1 judgments. The quotient was a number which varied inversely with the affective sensitiveness of the observer to colors: it was larger the more indifferent the observer was to this kind of material. We could just as well have inverted the fraction and obtained a number which would vary directly with affective sensitiveness. Either number furnishes a good index of this particular character. Of course if either the numerator or denominator happens to be zero, we can hardly conclude that the affective sensitiveness is infinitely great or small, but where the numbers have a finite value the index would seem to be roughly serviceable. A similar index was reckoned for each observer's judgments on the nonsense syllables.

Those observers for whom the ratio of indifferent judgments divided by extreme judgments was 1.5 or more were rated as decidedly indifferent to the kind of material used. Those for whom the ratio was .5 or less were ranked as decidedly sensitive. If an observer's ratio fell between these extremes (which were not, of course, held to with mathematical rigidity), she was classed as fairly sensitive.

There were seventy-seven young women observers. Among them, the largest sub-group was formed by those who were very sensitive to both the colors and the sounds. This was characteristic of twenty observers. Next in size came the group who were very indifferent to both: of these there were fifteen. There were thirteen who were very sensitive to colors and fairly sensitive to sounds; eight who were very sensitive to colors and very indifferent to sounds, seven who were fairly sensitive both to colors and to sounds, five who were fairly sensitive to colors and very sensitive to sounds, five who were fairly sensitive to colors and very indifferent to sounds, two who were very indifferent to colors and very sensitive to sounds, and two who were very indifferent to colors and fairly sensitive to sounds.

From these figures, it is clear in the first place that colors provoke a strong affective reaction in more observers than articulate sounds do, under the conditions of our experiment. The total number of observers who were very sensitive to colors was forty-nine; the total number who were very sensitive to sounds was twenty-seven. The total number who were very indifferent to colors was nineteen; the total number who were very indifferent to sounds was twenty-eight.

Secondly, it appears that there is a tendency for observers who are either very sensitive or very indifferent to one kind of material, to have the same affective attitude toward the other kind. The two largest groups were those of persons who showed great sensitiveness to both colors and sounds and of persons who manifested great indifference to both. There is a greater probability that an individual will display general affective sensitiveness, if we may call it general when we have used only two kinds of material to test it, than that he will show specialized sensitiveness to either colors or sounds. It must be remembered that the general situation is a part of the source of the affective reaction. There are probably people who cannot get any strong affective experience in the constrained and artificial surroundings of the experiment, while others are less embarrassed by their environment. Since the situation is practically the same with both kinds of material, it probably tends to make the affective response for colors similar in intensity to that for sounds in the case of many observers.

Thirdly, there exists a small but interesting group of observers who are strongly stirred affectively by one kind of material and left markedly indifferent by the other kind. As we should expect, there was more specialized sensitiveness to colors than to sounds: eight persons were very sensitve to colors and very indifferent to sounds, while in the case of two only was the relation reversed.

What makes an observer give strong affective reactions to one kind of material and weak ones to another kind? Has this characteristic anything to do with imagery? We should not expect that the frequent use of a certain kind of imagery, or the capacity to experience it with especial vividness, would produce any especial affective sensitiveness to sense-impressions of the corresponding modality. We do not need to recall color imagery in order to find a color pleasant or unpleasant. On the other hand, the reverse causal relation might very likely exist. A person who had strong likes and dislikes for the sense-impressions of a given modality would naturally tend to give more attention to them, and this might result in more frequent and more vivid imagery belonging to the modality in question.

We must leave for further investigation the problem of the characteristics which are correlated with specialized affective sensitiveness. So far as our results go, they indicate an entire lack of correlation with either the type of imagery most useful in memorizing, or the type occurring most readily and vividly as an illustrative accompaniment to reading. All our observers who showed specialized sensitiveness, as well as some who were very sensitive and some who were very indifferent to both kinds of material, were subjected to the following tests from Fernald's "The Diagnosis of Mental Imagery" (Psych. Monographs, vol. 14, no. 1): the rhyming tests (page 28), the similar spelling tests (page 28), the test for memory of words similar in sound but spelled differently, and that for auditory memory of words spelled alike but differing in sound (page 29), and the reading tests for illustrative imagery (pages 139-143). In each of these tests, some of the observers who did best were especially sensitive affectively to the corresponding kind of sense impression, while others who did equally well were especially indifferent.

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Note.—1901 (1) *Gustav Theodor Fechner* and 1909 (5) *Festrede* have been transferred, apparently without change save that of title, from W. Engelmann to A. Kröner.

BOOK REVIEWS

The Dynamic Foundation of Knowledge. ALEXANDER PHILIP. London, Kegan Paul, French, Trübner & Co.; New York, E. P. Dutton & Co., 1913. pp. 318.

The thesis which the author of this volume endeavors to maintain and to elaborate is that the world of sensible experience is to be interpreted in terms of a potency or potencies of which sensible objects are the transmutations. The key to our philosophical problems is to be sought neither in sense nor in thought, but in our own individual activity. Taking this activity as our starting point, we are enabled to get beyond our conscious selves, and yet we do not transcend experience. The Real can be found only in "the exertional element of our life, and that element being also that which interacts with and which participates in our environment, seems obviously the element for or by which we derive the knowledge of the independently real. In our exertional activity we are linked and intermingled with the dynamic system which constitutes our environment. Community of knowledge is rendered possible by and only by this participation. When the phenomena of sense can be accounted for as transmutations of a real energy or potency, then and only then can their appearance be explained by a theory consonant with the requirements of an intellectual metaphysic" (p. 64).

On this basis the proper procedure is a preliminary study of the body. The dynamic activities pertaining to the body are discovered by us through the stresses resulting from the limitations and opposition of the environment. These stresses are reported by the nerves to the sensorium, which is the common point of origin of the nerves of the special senses as well as the ultimate terminus of the afferent and excitant nerves of touch and motion. If the sensible impression is not transmitted beyond the sensorium, it is represented merely by a sensation. But the cerebrum may be stimulated, with the result that ideative cognition of the external is induced, the sensation being the material upon which it operates. On the basis of this distinction the author recognizes two kinds of activity, motor activity and the activity of thought. The former is conditioned by space, and hence the axioms of geometry have an *a priori* warrant. The activity of thought is not thus conditioned and hence it is bound by no *a priori* law save that of contradiction.

These axioms or *a priori* conditions, however, are not to be conceived in the manner of Kant, but are conditions laid down by the nature of power, potency or energy, which is the true and fundamental postulate of philosophy. This power, when it takes the form of movement, operates under the appearance of tridimensional space; when it takes the form of thought it operates in accordance with the law of contradiction. The rather obvious inference that if space is merely an appearance, contradiction must likewise be merely appearance seems to be left unnoticed by the author. Nor is he willing to concede that the limitation of thought to the material of sense rules out the concept of power or potency. "Cerebral activity is adapted to represent, though it is not confined to representing, the dynamic process; and in such representative activity the object is not sensation but motor activity. Thoughts, or ideas, as Plato called them, when they represent the process of reality, do not do so by reproducing in fainter form the fleeting sensations which are the mere accompaniments of the obstruction of motor activity. What cerebral activity does is to reproduce or rather represent the dynamic activity in which sensation arises" (p. 187).

The merit of the volume, as it seems to the reviewer, lies in its recognition that the traditional apparatus of sensation and thought is inadequate. Hence the attempt is made to find in power or potency a category that will remove the difficulties. But the author's account of the individual experience which arises in response to the obstruction of motor activity lands us once more in the time-worn categories of sensation and thought, and confronts us anew with the familiar puzzle of representationism. The things of sensible experience *represent* "the result or supposed result of action;" and similarly the activity of thought represents the dynamic activity in which sensation arises. "The mariner observes his position at noon. It is a static fact, but it is determined by dynamic changes; and is not its object to determine the course of his vessel? What is true of the visible is equally true of the tangible, of the audible, and of the whole sensible world. All tactile resistances in some way represent dynamic action" (p. 67). This *might,* of course, be interpreted in the pragmatic sense that objects of sense embody or reflect an adjustment that is completed and that their *meaning* is exhausted in adjustments which are to be made at a future time. But the author does not follow this road. The potency with which he deals is a reality or entity in an absolutistic sense, and like every other ultimate it is under no particular obligation to make itself intelligible.

How the fact of activity becomes a matter of knowledge is not made very clear, even though a considerable portion of the chapter on *activity* is devoted to this question. It is obviously not a fact of sense-perception, for if it were, there would be no justification for the condemnation of sense-experience as purely phenomenal. But neither is it an intuition. The question is left unanswered, unless we regard as answers certain *nichts sagende* statements such as the following: "It is by and in the obstructions of my exertional activity that its forms are defined. Sensation seems to be the medium by which both Discourse and Exertion are rendered knowable" (p. 25).

Waiving this point, however, we find further perplexities in store for us when we attempt to consider the nature of this potency. It is not merely the absence of description but the absence of coherence that is likely to trouble the reader. The author at the start makes appeal to the prevalence of universal change, apparently on the basis of observed fact, and concludes from this that we do not observe the real but only the transmutations of the real. Where observation fails to establish this change, physics and chemistry help out with their description of the processes involved in perception and in the fact perceived. Having thus compelled observation to commit suicide by means of its own category of change, the author proceeds to include space and time among the changes which the real undergoes. We are thus—to paraphrase a recent writer—treated to the spectacle of seeing a man carry a variety of things into a house and finally carry the house itself in. "The ascertained laws of energetic transmutation involve the dynamical relativity of Space and Time, which are quantifying ratios, but not at all the categorical continent of Experience. The antinomies of temporal succession, the dilemmas of Zeno, disappear when we envisage the fact that the energetic transmutation is in essence timeless and inextensive" (p. 196).

It does not seem worth while to follow out in further detail the windings of the argument. The absence of any serious attempt to define the fundamental concepts of the book—sensation, representation, knowledge, independence, and change or transmutation—gives little promise that any genuine contribution is to be expected. And it may be added that a better understanding of the present-day discussions which the author dismisses so complacently with a flourish of rhetorical phrases is more to be desired than additions to the long list of sins that are committed in the name of philosophy.

University of Illinois. B. H. Bode.

The Learning Process. By S. S. Colvin, Ph.D. New York, the Macmillan Co., 1911. pp. xxv, 336. Price, $1.25, net.

In order to be of value from the point of view of the teacher, educational psychology must be neither so speculative and general that it offers no better aid in the solution of practical problems than does common sense, nor so abstract and technical that its facts have no relation to the everyday work of teaching.

Unlike many books on the subject, the *Learning Process* can escape both of these criticisms. "The point of view," says the author, "is a thoroughgoing functionalism and pragmatism." There are no long discussions of the relation of body to mind, the structure of the nervous system, the nature of mind, and so on. After a general consideration of the fundamental elements of the learning process, the author goes directly to the concrete facts. The chapters on habit, sensation and perception, imagination, memory, association, and the transfer of training are clear, condensed accounts, drawn chiefly from experimental works, with frequent summaries and constant interpretation to hold the facts together. Each chapter has a direct and evident bearing on the chapter on application which follows it. These latter chapters are perhaps the distinguishing feature of the book. The recommendations and rules contained in them are many, and they are definite, concrete, and detailed. The chapters on transfer of training, for example, are followed by the application of the given principles to such questions as the disciplinary value of the various studies, the elective system, pure *versus* applied science, the importance of ideas and attitudes in the formation of habits; the chapter on memory, by application to questions of learning by wholes or by parts, distributed learning, 'bunching' the school program, short and unrelated courses, the value of the recitation and of examinations, modes of presentation, aids in learning, etc.

The chapters on attention and interest and on the higher thought processes, and a part of the discussion of reflex action and instinct, are rather more classificatory, theoretical, or formal than the rest, and the generalizations in them find less evident and less extended application. It seems probable that some of these generalizations cannot be specifically applied; if so, a portion of the material might have been omitted (for example, the discussion of attributive clearness, and the structural and logical account of concept and judgment). The statement (p. 140) that "Ebbinghaus attempted to exclude all associative factors in his learning," and that "he really tested not the loss of memory as such, but merely the fading of the memory after-image" is surely inaccurate.

The book as a whole is a decided step in the right direction—away from generality and technicality, and towards concrete facts and their specific application. It is probably the most practical text-book of educational psychology that has yet appeared. W. S. FOSTER.

An Elementary Study of the Brain, Based on the Dissection of the Brain of the Sheep. By EBEN W. FISKE, A.M., M.D. Illustrated with photographs and diagrams by the author. New York, The Macmillan Co., 1913. pp. vi, 133. Price, $1.25, net.

The book is an elementary laboratory manual for a special course in biology. The first two chapters orient the student in the comparative (phylogenetic and ontogenetic) anatomy of the brain. The third to ninth chapters contain directions for the dissection of the sheep's brain and descriptions of the structures to be studied. Physiological and psychological aspects of brain-formations find mention in various places, but they receive greatest attention in the summary, chapter X. Here we find a discussion of the central representation of the senses in the lower animals and man; the direct and indirect paths of conduction from the sense-organs to and through the cortex are traced; and the human brain is broadly interpreted in terms of its history and functions. One can only wish the chapter were less brief and schematic.

The author follows a good rule in giving the etymology of the technical terms which beset the beginner in brain-anatomy and brain-physiology. Unfortunately, however, he has not submitted his derivations to a philologist, with the result that he often falls into error. *Protoplasm* should be derived from Gk. *proto-*, first, and *plasma,* something formed. *Ontogenetic* comes from Gk. *on* (*ont-*), being, and *genetikos* adj. of *genesis,* generation. There is no Latin word *bi* for two, or *quad* for four; and the forms *bigeminus, quadrigeminus* are themselves Latin. The Gk. infinitive *phyein* is transitive; and the forms *hypophysis,* undergrowth and *epiphysis,* excrescence are already Greek. *Pellucidus* is a Latin adjective. *Cineria* should be *cinerea. Arachnoid* represents the Gk. *arachnoeides,* not *arachne. Chiasma* is itself a Gk. word, and there is no Gk. verb *chiozein.* The Gk. word *eidos* does not mean *like,* and the word *arche* does not mean first. Instances of this sort could be multiplied, and show a carelessness that is sadly out of place in a scientific manual.

The simplicity and clearness with which the difficult subject is presented are worthy of praise. The photographs and diagrams are numerous and excellent. There is a bibliography of twelve titles.

W. S. FOSTER.

Palaeolithic Man and Terramara Settlements in Europe. By R. MUNRO. Being the Munro Lectures in Anthropology and Prehistoric Archaeology in connection with the University of Edinburgh, delivered February and March, 1912. New York, The Macmillan Co., 1912. pp. xxiv-507.

The first and larger portion of this volume (pp. 1-287) deals with the material remains, and the culture and civilization, of the palaeolithic races of Europe. The object of the Munro foundation is "to popularize prehistoric methods and research, and to stimulate the inquiring faculties of intelligent persons to pursue the subject on account of its inherent interest to the civilized races of the present day." The style of the lectures is therefore popular, and the illustrations are lavish. We begin with introductory chapters on the "epochs" of the Stone Age, on the methods of cave-exploration, and on chronological problems; then follow discussions of the fossil remains of palaeolithic man found in the various European countries, with a special chapter on *Pithecanthropus erectus.* The sketch of culture and civilization deals, in the main, with the artistic phase of prehistory, though we read incidentally of tools, weapons, clothing, cooking, etc.; other sides of folk-psychology, such as the question of the domestication of animals, the evidence of magical and religious practices, and so forth, are left for later lecturers. In a chapter on the transition from the palaeolithic to the neolithic status, the author makes less than certain other authorities have done of the completeness of the change: "I am not aware," he says, "of any evidence which actually negatives the idea that the Quaternary men of Europe survived till the arrival of the Neolithic tribes, and that both races continued to live amicably in the same neighborhood and ultimately amalgamated with each other."

The lectures furnish a very useful compendium of information,—though the reader will wish, at times, for a more incisive handling of detail, and for the banishment of references from text to footnote. The material is brought up to date: it is not the author's fault that no account is given of the Piltdown skull, probably the most important discovery of the kind hitherto made in England (*Nature,* December 19, 1912).

The remainder of the book (pp. 291-476) has a more restricted topic: it deals with the *terremare* or "marl beds," first found in the valley of the Po, which proved on examination to be the remains of villages of the Bronze Age, groups of huts supported on piles and fortified by moat and earthern rampart. The *terramaricoli* were apparently lake-dwellers who had crossed the Po and carried the habit of pile-construction with them. Dr. Munro traces the relation of these invaders to the Neolithic hut-builders and cave-dwellers of the same region. His view is that the original invasion occurred from the Danubian valley, by way of Croatia, Carinthia and N. E. Italy; that the newcomers reached the Po valley in the period of transition from Stone to Bronze (certain settlements in the Lake of Garda were inhabited up to the beginning of the Iron Age); that they moved westwards along the left bank of the Po, and crossed it, to found *terremare,* near Vadiana; and that presently, moving south, they took possession of the native villages and occupied their hut-shelters, thus giving up the *terramara* habit. A concluding chapter describes structures analogous to *terremare* in other European countries.

These lectures, which were also delivered on the Dalrymple foundation at the University of Glasgow, are again abundantly illustrated, and are accompanied by a complete bibliography.

The Physical Basis of Music. By A. Wood. Cambridge, University Press; New York, G. Putnam's Sons, 1913. pp. iv-163. Cambridge Manuals of Science and Literature No. 55. Price, 40c., net.

This little book gives a clear account of the elementary physics of sound, so far as concerns the production of musical tones. For psychology it relies wholly upon Helmholtz' *Sensations of Tone,* and must therefore be pronounced out of date.

Qu'est-ce que le raisonnement? Par EUGENIO RIGNANO. Scientia, XIII., 1913. pp. 30-57.

In this article the author confines himself to the analysis of certain of the simplest and most common forms of reasoning, reserving for future contributions the study of the evolution of reasoning and the development of its higher forms.

From the analytical study of a series of examples he reaches the conclusion that reasoning consists in the imagined execution of a series of observations or experiences which might have actually been carried out,—e. g., the author, not finding his umbrella in its usual place, thinks momentarily that he may have left it in one of the places where he was on the previous day; but he then reasons that as he did not change his clothing, although it was raining heavily when he came in, this could not have been the case. Here the reasoning is simply the mental representation of a series of experiences, which might have occurred.

In the somewhat different type of geometrical reasoning as, for example, the proof that the sum of the interior angles of a triangle equals two right angles, the actual process of placing the angles in juxtaposition can be carried out by cutting a paper triangle, but is valid only for the particular triangle. Precisely this is accomplished by the imagination. We simply transfer in thought the angles to a common vertex and mentally apply a previous experience to the situation and in making use of an empirical result already obtained we obtain a general value, not obtainable by the actual experiment.

From numerous examples thus analysed, it appears that reasoning is nothing more than a series of representations in thought of operations or experiences either actual or possible.

If the true nature of reasoning is not clearly comprehended, the perfect agreement of the results of the logical process with results actually observed may cause a feeling of surprise and wonder, but if we recognize that the logical process, reasoning, is only a series of experiences, which are all, theoretically at least, susceptible of being carried out but which, in order to save time and energy, are simply limited to the thought, the wonder ceases.

In other words, the intermediary results of all reasoning, even that which develops by means of the most complicated symbolism (to be discussed by the author in a forthcoming paper), have all a concrete symbolism, i. e., they represent the respective empirical results of the different phases which succeed each other in the series of operations or thought experiences.

From this nature of reasoning arise certain advantages and disadvantages. It is evident that in reasoning there is an enormous economy of time and energy as compared with actual carrying out of the experiences.

There are moreover an infinite number of experiences, which though theoretically possible could never be carried out in practice. Reasoning can thus accomplish a far greater number of experiences than would be materially possible and, moreover, can give a more general result as e. g. in the solution of the problem of the value of the interior angles of a triangle.

But there are also disadvantages, which arise from the risk of error which inevitably exists from the very nature of the reasoning process. When the complexity of the process passes a certain limit in consequence of the multiplicity of the experiences mentally accomplished, then it becomes impossible to follow in thought all the factors and their reciprocal effects which are involved and consequently one or more of them is forgotten. Moreover since we are compelled to use symbols, verbal or otherwise, in our thinking, a source of error is introduced which renders absolute confidence in purely mental results impossible.

But on the other hand, the sterility of pure reasoning affirmed by some authors and the assertion that nothing is contained in the conclusion which was not in the premises is untrue, as may readily be shown by the many new facts in science discovered purely by reasoning. The new combination in the imagination of experiences already known leads to the discovery of absolutely new results, to a new truth which is contained in the combination of facts, but which exists in neither of the facts taken singly. It is this new mental vision created by imagination which constitutes the new fact, the conclusion. But it is also true that though reasoning is quicker and therefore more productive, actual experiment may furnish better conditions for the discovery of new facts, because of the insufficiency of the imagination and because in some cases the observation of all that actually happens gives adequate data.

The fecundity of reasoning depends upon the fact that the imagination is not only reproductive but also productive, i. e., it may combine elements given by experience in a manner entirely different from anything already observed in the past.

For these new combinations the affective intensity directed toward the end to be attained is of supreme importance. An analysis of this dynamic aspect of reasoning shows that it consists in an interest which operates for the exclusion not only of all other affectivities but of memories connected with them. It also directly evokes all memories, facts, experiences, and knowledge associated with the affective tendency which is active throughout the whole reasoning process. But this affective evocation is not sufficient in all, especially in new cases, which must proceed by the method of selecting, from the multiplicity of acts imagined, those particular ones which are suited to the end to be attained. It is precisely this triple form of activity, i. e., exclusion, evocation and selection according to the affective tendency which is the essence of teleological thinking.

Simple association which suffices to explain the evocation and succession of ideas is not adequate to explain the directed association which constitutes reasoning. There is needed in addition the affectivity for the end in order to maintain coherence during a long process of reasoning.

There is also present in the reasoning process a secondary affective tendency which consists in the fear of omitting some of the possible actions and reactions to which the object under consideration might hypothetically be subjected, and this exerts an influence on the process of recall. Illogical thinking is, in fact, due to the forgetting or displacement of some factor necessary to the correct result. This phase of thinking the author proposes to discuss in a future paper on the pathology of reasoning. THEODATE L. SMITH.

In the Shadow of the Bush. By P. A. TALBOT. New York, G. H. Doran Co.; London, W. Heinemann. 1912. pp. xiv., 500. Price $5 net.

This book reports the nature and nurture of the Ekoi, a forest or 'bush' people of Southern Nigeria and the Cameroons. The Ekoi are mainly of Bantu stock, and are found to the number of some 20,000 in and about the Oban district; across the German border they number from 6,000 to 17,000, according as certain tribes are or are not included in the count. The author, who is an official of the Nigerian Political Service, has traveled some 1,700 miles annually in the district since he entered upon his duties in 1907, and has made notes of things as he came across them. The result is a book of real interest and value, but a book which has also the defects of its origin. It is cast in the form of an itinerary, with continual interruption by folk-tales, and with special chapters devoted to religion, magic, government, etc.; so that the reader who tries to hold the narrative as a whole is reminded of *The Shaving of Shagpat.* Moreover, certain important questions receive somewhat casual treatment: the status of totemism, e. g., and the evidence of a matriarchal condition (p. 97). These points must be mentioned; but the criticism does not reflect upon the author, who has taken entirely the right course. "Primitive races, the world over, are changing so rapidly that it seemed well to place on record . . . habits and customs [which], at first in everyday use, showed signs of becoming things of the past. . . . Written in the depths of the Bush, far from every book of reference, . . . this book claims nothing save that it strives to tell the story of a little-known people from a standpoint as near as possible to their own."

The two most striking features of Ekoi life are the organisation of secret societies and the universality of magic. "The whole country is honeycombed with secret societies," of which the Egbo Club is the most powerful. This is a men's club, which has its house in every village, under native rule usurped practically all the functions of government, and possesses a very ancient (partly totemistic) ritual. There are also women's societies, to whose ceremonies men are not admitted; only in exceptional cases, or by a sort of inferior membership, are the societies of the one sex open to the other. Even the children have their mimic Egbo Clubs, to say nothing of 'age-classes' or Junior Republics (p. 283). As for magic, it is "the key-note to which the lives of the Ekoi are attuned." They are animists of the most thorough-going kind; "not only great trees, but the smallest plants possess a soul, and can feel pain when plucked" (p. 287); they reverence ancestors; they have two or three 'deities,' and the author thinks that he has found "traces of an older, purer worship;" but the mainspring of conduct is the juju—which may mean almost anything uncomprehended and mysterious, from a sort of demigod to the 'mana' of herb or stone, including also the manifold means whereby these forces may be influenced or controlled. "Ancestor worship, nature-jujus, secret societies, the principal events of life, and the commonest actions of the day, all blend inextricably in a complicated ritual." Juju dances, emblems, posts, trees, stones,—juju rites, revocations, 'sendings,' 'medicines,'—jujus of good and of evil, of protection and of fear,—accompany the reader throughout the book. The author gives some curious instances of the effectiveness of a juju; for the most part he notes them without comment, but in one case he explains that the "strong-smelling pitch used to 'renew the power' of the juju may offend the nostrils of the keen-scented" leopards against whom it was directed. On pp. 85 ff. is a strange story of the death of a chief owing to the shooting of a buffalo (the chief was a 'buffalo-soul') ten miles away.

The Ekoi show a good deal of artistic feeling in the forms of their domestic pottery, and are extremely musical. They have, besides a variety of drums (xylophone and regular types), a 'harp,' an instrument corresponding to the Malagasy *valiha* (played by two men, the one of whom "strikes the strings with two slender wooden sticks, while the other touches it here and there with a small closed calabash, with which he makes occasional 'runs' by drawing sharply up and down"), the *okankan* made of two flat bells apposed, rattles, etc. The author gives instances of the drum-language, which Retz—in the adjacent Cameroons—was the first European to master; we regret that he does not allow more space to the subject. He also gives instances of the *nsibidi* or pictorial sign-language, the secret of which was at first jealously guarded (p. 39), but which he has managed, at least in some measure, to interpret.

The book is profusely illustrated by photographs, and by figures in the text. A colored frontispiece shows an Ekoi girl in 'fatting-house' costume. The series of plates representing styles of head-dress (pp. 318 ff.) is especially good. Appendices deal with tabus, clubs, language (grammar, vocabularies, etc.), anthropometrical data, natural history, etc. The author has great sympathy with the people, and has learned to respect them; his work has evidently been a labor of love; and he is to be sincerely congratulated on the amount of first-hand information that he is able to impart.

BOOK NOTES

Stammering and cognate defects of speech. By C. S. Bluemel. Volume 1, The psychology of stammering; Volume 2, Contemporaneous systems of treating stammering: their possibilities and limitations. N. Y., G. E. Stechert & Co., 1913.

English writers have long wanted a comprehensive work upon this interesting subject, and we seem to have it in these volumes. The writer discusses mental types, eye- and ear-mindedness, the visual image, brain, relations between mental imagery and voluntary speech, impairment of brain centers, aphasia, stammering, mental confusion, fear and auto-suggestion, respiration, vocalization, articulation, verbal exercises, mechanical appliances, psychological methods, stammering schools and specialists.

Personality. By F. B. Jevons. N. Y., G. P. Putnam's Sons, 1913. 172 p.

This is an attractive book by an author who has been profoundly touched by the questions which he here discusses,—personality and impersonality, psychology and personality, personality and change, personality and individuality. Bergson and James have evidently most influenced the writer in the preparation of this work.

Educational Psychology. Volume I. The original nature of man. By Edward L. Thorndike. N. Y., Teachers College, Columbia University, 1913. 327 p.

The title of this work is most challenging. It is the original nature of man which we want to know. The author has treated the subject with freshness and originality but nevertheless with an eye largely turned to practical applications of the doctrines he discusses to education. A fuller notice it is hoped will follow in these pages.

Man and his forerunners. By H. v. Buttel-Reepen. Authorized translation by A. G. Thacker. London, Longmans, Green & Co., 1913. 96 p.

This eminent authority discusses the earliest traces of man; man before the ice age; the glacial and early stone age; the Neandertal race; higher races of the ice age and the Sussex find; men, pre-men and apes; the close of the ice age. The work is illustrated by seventy cuts.

The Canada porcupine: a study of the learning process. By Leroy Walter Sackett. Behavior Monographs, Volume 2, Number 2, 1913. Boston, Henry Holt & Co., 1913. 84 p.

This interesting article is based on the study of sixteen porcupines begun at Clark University three years ago. The author has here made a contribution of value to the subject of animal psychology.

Le langage graphique de l'enfant. Par GEORGES ROUMA. Deuxième éd. Bruxelles, Misch & Thron, 126 Rue Royale, 1913. 283 p.

This is the second edition of the author's comprehensive work upon the subject, which along with that of Kerschensteiner occupies the very first rank.

Fortschritte der Psychologie und ihrer Anwendungen. Hrs. v. KARL MARBE. I. Band. Leipzig, B. G. Teubner, 1913. 133 p.

The chief feature of this number is an exhaustive article by Jakob Stoll on the psychology of defects in writing.

On the relation of the methods of just perceptible differences and constant stimuli. By SAMUEL W. FERNBERGER. Psychological Monographs, Vol. XIV, No. 4, Jan., 1913. Princeton, N. J., Psychological Review Co., 1913. 81 p.

Das Seelenleben des Kindes. Ausgewählte Vorlesungen von KARL GROOS. 4th ed. rev. Berlin, Reuther & Reichard, 1913. 334 p.

A clinical manual of mental diseases. By FRANCIS X. DERCUM. Phila., W. B. Saunders Co., 1913. 425 p.

Das Gedächtnis. Die Ergebnisse der experimentellen Psychologie und ihre Anwendung in Unterricht und Erziehung. Von MAX OFFNER. 3d ed. rev. and enl. Berlin, Reuther & Reichard, 1913. 312 p.

Die agrammatischen Sprachstörungen; Studien zur psychologischen Grundlegung der Aphasielehre. Von ARNOLD PICK. I. Teil (Monographien aus dem Gesamtgebiete der Neurologie und Psychiatrie, hrs. von Alzheimer und Lewandowsky, Heft 7). Berlin, Julius Springer, 1913. 291 p.

Denkökonomie und Energieprinzip. Von P. GABIUS. Berlin, Karl Curtius, n. d. 208 p.

Summaries of laws relating to the commitment and care of the insane in the United States. Prepared by JOHN KOREN for The National Committee for Mental Hygiene. N. Y., Nat. Committee for Mental Hygiene, 50 Union Square, 1912. 297 p.

Allgemeine Psychopathologie; ein Leitfaden für Studierende, Ärzte und Psychologen. Von KARL JASPERS. Berlin, Julius Springer, 1913. 338 p.

Twenty-eighth annual report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution. 1906-1907. Washington, Govt. Print. Office, 1912. 308 + 35 p.

Über "meteoristische Unruhebilder" und "Unruhe" im Allgemeinen. Von MAX LÖWY. (Sonderabdruck aus der Prager Medizinischen Wochenschrift, Vol. XXXVII., Nr. 24, 1912. 106 p.)

The functions of the cerebrum. By SHEPHERD IVORY FRANZ. Reprinted from The Psychological Bulletin, Vol. X, No. 4, pp. 125-138. April, 1913.

The psychology of the personal interview; its relation to moral development through penal institutions. By RUFUS BERNHARD VON KLEINSMID. A paper read before the decennial convention of the Religious Education Association, Cleveland, Ohio, March, 1913. 11 p.

Eugenics and the state. By RUFUS BERNHARD VON KLEINSMID. A paper read before the Cincinnati Academy of Medicine, May, 1913. Reprinted from the Lancet-Clinic. Indiana Reformatory Printing Trade School. 11 p.

The buried animal suture. By HENRY O. MARCY. Reprinted from the Lancet-Clinic, Nov. 16-23, 1912. 32 p.

The selection of stimulus words for experiment in chance word reaction. By ELEANOR A. McC. GAMBLE and ALBERTA S. GUIBORD. Reprinted from Westborough State Hospital Papers, Series I., 1912. p. 91-109.

Saggi di psicologia sociale. GUALTIERO SARFATTI. Reprinted from "Psiche," Anno II, N. 2, Marzo-Aprile, 1913. Firenze, 1913. 47 p.

Der Ursprung subjektiver Kombinationstöne. Von JOSEPH PETERSON. Reprinted from Annalen der Physik. Vierte Folge. Band 40. 1913. Leipzig, J. A. Barth. 2 p.

L'Année psychologique, fondée par Alfred Binet, publiée par Henri Piéron. Dix-neuvième année. Paris, Masson et Cie, 1913. 515 p.

Proceedings of the Society for Psychical Research, Part LXVII, Vol. XXVI, July, 1913. p. 375-544. Glasgow, University Press.

Proceedings of the American Medico-Psychological Association at the Sixty-Eighth Annual Meeting, held in Atlantic City, N. J., May 28-31, 1912. Pub. by American Medico-Psychological Association, 1912. 511 p.

Internationale Zeitschrift für ärztliche Psychoanalyse. Hrsg. von SIGM. FREUD. I. Jahrgang, 1913, Heft 3. Mai. Leipzig, Hugo Heller & Cie, 1913. p. 205-310.

The moral education of school children. By CHARLES KEEN TAYLOR. Preface by Arthur Holmes. Phila., C. K. & H. B. Taylor, n. d. 77 p.

Dr. Motora and modern psychology. Published by the Memorial Meeting for Dr. Motora. Kodokan, Tokyo, 1913. 470 p. (In Japanese.)

Return of Frank R. Stockton. Stories and letters which cannot fail to convince the reader that Frank R. Stockton still lives and writes through the instrumentality of Miss Etta De Camp. N. Y., Macoy Pub. & Masonic Supply Co., 45 John St., 1913. 314 p.

Miss De Camp was a teacher and in 1909 felt a thrill from her shoulder to her finger-tips and her pencil began to move. After a number of sittings it began to write, and it finally appeared that she was the medium through whom Frank Stockton, who died years ago, had chosen to communicate further of his stories to the world. They are written and printed under the direction of his ghost and a number of letters of direction from him are printed. Miss De Camp had read Frank Stockton's stories years before, but had almost forgotten them, but here prints seven that were given to her automatically. They are somewhat in Stockton's style, problematical, inconclusive and weird, particularly one which describes the experiences of Mike O'Flynn, who saw his own funeral, rode on the hearse, saw his body buried, crouching over the grave the first night, and finally vanished.

The psychoneuroses and their treatment by psychotherapy. By J. DEJERINE and E. GAUCKLER. Authorized translation by Smith Ely Jelliffe. Phila., J. B. Lippincott Co., 1913. 395 p.

Dr. Jelliffe has done a real service in translating this important work of the great French specialists, who have seen more clearly than anyone else in our day the hygienic value of normalizing the emotional nature. Their therapy does not consist chiefly, like that of Dubois, in straightening out the intellectual processes but in adjusting the sentiments and emotions. To our thinking the very best part of the work consists in the first chapters, which show mental effects of disorders of the digestive, urinary, genital, respiratory, circulatory, cutaneous and neuro-muscular systems. In very many of these disorders the writers find a genetic factor. The book is of great value not merely to physicians but to psychologists.

A scout of to-day. By ISABEL HORNIBROOK. Boston, Houghton, Mifflin Co., 1913. 290 p.

This attractive little book is a story for boys by the author of "Camp and Trail" and other stories, which shows the purpose and value of the Boy Scout Movement. One of its interesting features is the song for the Scouts, which is being adopted for use by some troops. As the book is a story it can not be reviewed here at any length but it should prove interesting to all Scouts and all adults interested in the Scouts.

PSYCHOLOGY AND PHILOSOPHY

The following announcement is taken from *The Journal of Philosophy Psychology and Scientific Methods* for June 5, 1913:

The subject of "The Standpoint and Method of Psychology" has been selected as the topic for the joint discussion of the American Psychological Association and the American Philosophical Association at its next meeting at New Haven. Professors Edward G. Spaulding and Howard C. Warren, of Princeton University, suggest the following formulation of the problem, which, it is hoped, will serve as a starting point for further formulations and discussions:

Data of Psychology.—Should psychology study unit-beings (selves, mind, consciousness), or inner states (*e. g.*, sensations feelings), or inner processes (*e. g.*, sensibility, affectivity, association), or certain relations between unit-beings and their environment (*e. g.*, reflexes, instincts), or several of these?

Method of Research.—Should the psychologist obtain his data mainly by self-study (introspection by himself and others), or by studying the motor reactions of organisms? If both methods be admitted, what is their relative importance?

Philosophy of Psychology.—Does a systematic psychology depend upon a specific world-view. or can it be developed, as are physics and biology, without a definite philosophical basis? In the latter case, do the results of empirical psychology compel us to adopt some specific philosophy?

Note.—The question of the nature of consciousness, sensation, introspection, etc., should be discussed only in its relation to the standpoint that is taken concerning the above positions.

A frank discussion of fundamental issues, by men who are pursuing different lines of research and have been trained in different disciplines, will often clear the air of needless misunderstanding, and may contribute more positively to the advancement of science. Nor can anyone be more welcome in such a debate than the philosopher, the 'spectator of all time and of all existence'; for he brings an historical perspective which the man of science too often lacks, and he weighs hypotheses with an impartiality which the special student too rarely attains.

It is, then, because I am in harmony with the spirit of the Note printed above that I am moved to enter a protest against the form which has been given to the subjects proposed for discussion. No one, not even the philosopher, may legislate for a growing science, and say what its data and methods 'should' be. Psychology makes its way through the tangle of experience by what methods it may, and gathers as data what facts it can; a new method may enlarge its scope, a novel observation may open up a whole field of work. Only when a science is perfect, and the life has gone out of it, can its data be circumscribed and its methods defined. Psychology as a science is still in its childhood, while its task is as immense as that of physics, the mother of the sciences; it appeals to all the temperaments, and satisfies the most diverse interests of man; to direct it by 'shoulds' and 'should-nots' would be, in my judgment, to hamper its growth and to check useful effort.

E. B. TITCHENER.

INDEX OF SUBJECTS

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(The names of authors of original contributions are printed in SMALL CAPITALS.)

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