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The Psychological Review.

A PRELIMINARY STUDY OF THE SIGNIFICANCE OF PARTIAL TONES IN THE LOCALIZATION OF SOUND.

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An adequate theory of the localization of sound must take account of three general problems: (1) The physical conditions (extrinsic to the organism) upon which localization depends must be determined; (2) the precise physiological processes involved in such localization must be discovered; and (3) the psychological activities which are concerned must be analyzed and described. Up to the present time no theory has dealt exhaustively with all of these considerations, and the psychological problem has often been practically disregarded.

The extensive experimentation of recent years has rendered it essentially certain that the most important precondition on the physical side of sound localization is found in the relative amplitude of the sound-waves distributed to the two ears.[1] It is also known that, in distinction from their amplitude, the composition of the sound-waves is sometimes of significance in localization. The evidence bearing on this point, however, is lacking both in definiteness and in detail.

The varying intensity in the stimulation of the two end organs with the resultant effects upon the cortex and other ganglionic centers has often been regarded as a sufficient and

[1] For discriminating criticism of important views of modern scientists, see Pierce, 'Studies in Space Perception.'

self-evident basis for an explanation of the physiological facts concerning localization. Wundt has advanced the idea that tactile nerves are stimulated by the movements of the tympanic membrane and thus contribute to the localization processes. He has made a similar suggestion with reference to stimulations of the tensor tympani muscle.[1] This type of view has been rigorously criticised by Stumpf, who emphasizes, among other difficulties, the undoubted fact that we can correctly localize two simultaneous sounds.[2] E. Mach early suggested the theory that the external ears act as resonators modifying the quality of sounds heard from different directions, and affording thus a criterion of direction.[3] Theories like those of Preyer and Münsterberg have attempted (thus far with limited success) to make the semicircular canals responsible for the physiological phenomena.[4] So far as these latter theories emphasize the release by sound stimulations of quasi-reflex movements of localization, they point to an important and genuine feature of such processes, whether their conception of the physiological mechanism involved be accepted or not.

On the psychological side various factors have been described as contributing to localization: *e. g.*, (1) the immediate consciousness of position in an auditory space; (2) the consciousness of positional relations gained by visual and other supplementary imagery; (3) the consciousness of tendencies to movement on the part of the head and eyes; and (4) apparently, at times, the consciousness of cutaneous sensations from the shell and membrane of the ear and possibly the tensor muscle.[5] Criticism has been much devoted to combating the frequent and careless assertion that we are conscious of the intensity of the

[1] Wundt, 'Grundzüge der physiologischen Psychologie,' Vol. II. (1893), pp. 93 ff.

[2] Stumpf, 'Tonpsychologie,' Vol. II., pp. 53 ff.

[3] Mach, 'Bemerkungen über die Function der Ohrmuschel,' *Archiv für Ohrenheilkunde*, Vol. IX. (1875), p. 72.

[4] *Cf.* Preyer in *Pflüger's Archiv*, Vol. XL., p. 586; Münsterberg, *Beiträge zur Psychologie*, Heft 2, p. 182.

[5] The general psychological question of the reality of an auditory space has been luminously discussed by Stumpf, and in some of its more empirical features is best treated by Pierce. *Cf.* Stumpf, *loc. cit.*, and also 'Ueber den psychologischen Ursprung der Raumvorstellung,' Leipzig (1873); Pierce, *loc. cit.*

sound heard by each ear and that we in this way localize the source of the sound upon the side most intensely stimulated. The fact is, of course, that we are conscious of one sound and one intensity only, and this is referred to some specific spatial position. But the details of the strictly psychological portion of our general problem have been, perhaps, most often honored by neglect. The recent paper by E. A. McC. Gamble is a notable exception.[1]

So long ago as 1875 Lord Rayleigh had made observations upon the localization of tuning-fork tones, which led him to surmise that differences in the quality and timbre of sounds, as heard by the two ears, were of quite as much significance for localization as the mere differences in the intensity of the fundamental tone.[2] In 1879 S. P. Thompson, discussing experiments of his own with the pseudophone (*cf.* the similar observations of Weber, *Berichte der Gesellschaft der Wissenchaften* [Leipzig, 1851], p. 29 — M. and P. Cl.), came to a like conclusion, which he formulated in a later article somewhat more explicitly.[3]

Despite the instructive character of these investigations, it must be admitted that the conditions which were employed are somewhat unnatural, and that in so far, therefore, they jeopardize the scope of the inferences which may be confidently based upon them. In certain of Lord Rayleigh's experiments, for example, two tuning-forks were struck on different sides of an observer, and then, one of them being stopped, the position of the other was estimated. Evidently the conditions produced by sounding two tones and then suddenly subtracting one are very different, both neurally and psychologically, from those arising when a tone is heard as it originates from some single source. In Thompson's interesting experiments an artificial pair of pinnæ were used, enabling an exaggeration of the effects of reflection, etc., produced by the natural pinnæ. That the localization of sounds could in this fashion be disturbed in certain definite ways

[1] Gamble, PSYCHOLOGICAL REVIEW, Vol. IX. (1902), pp. 357–373.

[2] Rayleigh, *Transactions of the Musical Association* (1876) ; also *Philosophical Magazine* (5), Vol. III (1877), p. 456.

[3] Thompson, *Philosophical Magazine*, January–June (1882), p. 415 ; *ibid.* (5), Vol. VIII. (1879), pp. 385–390.

is not surprising, nor is it remarkable that tuning-fork tones should show much less liability to modification in this manner than more complex sounds. But, in the nature of the case, such observations cannot furnish a complete chain of evidence as to the differences characterizing the localization of these various kinds of sounds under normal conditions. Notwithstanding the limitations upon the implication of these experiments, they certainly constitute presumptive evidence in favor of the belief that quality changes are of genuine significance for sound localization, especially when taken in connection with such observations as Mach's,[1] and such mathematical deductions as Lord Rayleigh has made, showing that, save for a few positions, there is an extremely small difference in the intensity of the stimulation of the two ears by the fundamental tone of a sound.[2]

Pierce has demonstrated that localization in the median plane, which is notoriously uncertain and inaccurate, can be vastly improved, when complex sounds are used, by learning to note the modification in tone-color, or timbre, which is connected with different positions in this plane.[3] He has also made tests with organ pipes and tuning-forks, which suggest that auditory judgments of distance are affected by tonal complexity, the richer tones tending to be judged nearer than those more nearly pure.[4] In a paper published in 1865, Mach remarked a contrary fact and promised further communications upon the subject, which I have, however, been unable to find.[5] Bloch made certain observations, which appear to agree with Pierce—the fuller, richer tones being judged nearer.[6] On the other hand, the computations of Grinwis, showing the relative intensity of the components of a complex sound for various

[1] *Loc. cit.;* also 'Bemerkungen über den Raumsinn des Ohres,' *Poggen. Annalen*, Vol. CXXVI. (1865), p. 331 ; *Sitzungsberichte der Wiener Akademie*, Vol. L. (1864), 'Ueber einige der physiologischen Akustik angehörigen Erscheinungen.'

[2] *Loc. cit.*

[3] *Loc. cit.*, pp. 92 ff.

[4] *Loc. cit.*, pp. 163 ff.

[5] *Poggen. Annalen, loc. cit.*

[6] Bloch, 'Das binaurale Hören,' Wiesbaden, 1893.

distances, furnish a theoretical confirmation of Mach's view.[1] The issue is really somewhat ambiguous. Richer sounds may ordinarily be judged nearer than those more nearly pure. The upper partial tones of a complex sound may be relatively more prominent when the sound is heard from a distance, and still the total sound effect be poorer and less full than when the same sound is heard near at hand.

The extended experiments recently carried out in the Psychological Laboratory of the University of Chicago showed conclusively that persons totally deaf in one ear could localize sounds of sufficient complexity with considerable accuracy, whereas approximately pure tones could not be localized at all.[2] The localizations were evidently based upon the modifications which the partial tones of complex sounds undergo, when the position of the sources of the sounds is changed relative to the ear. The introspective evidence offered by the observers in these tests confirmed perfectly the objective results in pointing to this explanation. It is interesting to note in passing, in connection with Pierce's observations upon median plane localization, to which reference has already been made, that in the Chicago experiments certain of the persons deaf in one ear distinguished front and back with distinctly greater accuracy than the normal subjects. It will be remembered that for normal persons the intensity criterion is for points in the median plane peculiarly ambiguous. But quality differences are relatively reliable, if one has learned to employ them.

The observations reported in this paper constitute an immediate outgrowth of these several previous discoveries and form an effort to begin the systematic investigation of the part played by the partial tones in the localization of sound. The positive outcome of the work thus far is largely methodological in character and bears most immediately upon points (1) and (3) mentioned in the opening paragraph, and indirectly upon point (2). For reasons which will presently appear, the work is necessarily extremely slow, and the results already attained do not

[1] Grinwis, 'Ueber cylindrische Schallwellen,' *Poggen. Annalen*, 1877, Beibl. 8, p. 443.

[2] Angell and Fite, PSYCHOLOGICAL REVIEW, Vol. VIII. (1901), pp. 225, 449.

seem to warrant a more pretentious title than that I have chosen. The probability of unavoidable delay in the completion of the observations is the justification for publication at this time. Moreover, the implication of the experiments successfully executed seems altogether definite and distinctly significant for the theory of auditory localization.

A solution of the problem in hand evidently involves certain indispensable conditions which the apparatus was designed to meet. There must be (1) a series of sounds of controllable intensity, including (2) at least one approximately pure tone. It must be possible (3) to produce these sounds at any position relative to the observer without his previous knowledge of their location. It must be possible (4) to have all the sounds given at equal distances from the observer. It is desirable also (5) that the distance should be capable of variation, although the present paper does not deal with experiments in which changes of this kind were employed. Not least in importance, as dearly bought experience has taught me, is (6) the absolute prevention of the reflection of the sounds. Working under expert assurance that reflection could be eliminated by properly arranged draperies, I wasted much valuable time indoors, with the result that often tuning-fork tones, when opposite one ear, would confidently be localized as opposite the other. My failure may have been wholly due to unskilful devices on my part, but I certainly question very seriously whether experiments with tuning-forks can be satisfactorily carried on save in the open air. With many kinds of sounds this consideration is of minor consequence. I may mention in connection with these indoor experiments the interesting effects of fatigue which were repeatedly apparent. If one ear were fatigued for a tone, and within a few moments both ears were permitted to receive the sound, the latter would often be confidently localized as opposite the unfatigued ear, or sometimes as in the median plane, depending on the degree of the previous fatigue. Thompson, in the paper already mentioned, remarks a similar phenomenon, but much less extreme than in my observations.[1]

[1] *Philosophical Magazine* (5), Vol. XII., p. 351.

Apparatus and Procedure.

To meet the conditions named, work was carried on outdoors on windless nights—a deplorably infrequent circumstance in Chicago — rendering the observations very protracted. A

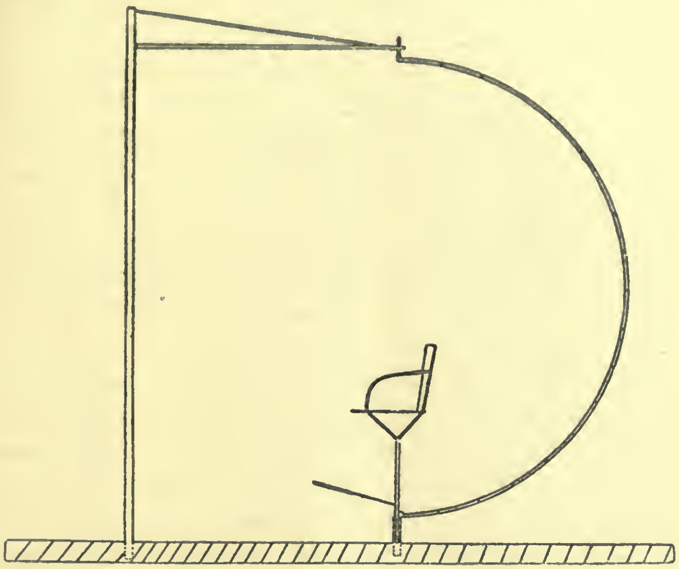

Fig. 1. Platform with Semicircle and Chair.

narrow platform was erected, upon which was mounted the apparatus shown in the accompanying diagrams (Figs. 1 and 2). The upright support (Fig. 1) carries a strong light semicircle

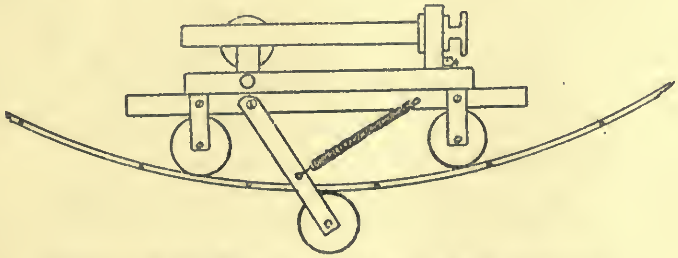

Fig. 2. Carriage with Tuning-fork, Resonator removed.

of steel with a radius of four feet, which is so arranged as to permit a metal carriage mounted on soft rubber rollers (Fig. 2) to travel up and down upon it. The semicircle is marked off in degrees, so that the position of the carriage can at any time be determined. The carriage is held at any desired height by friction screws acting upon the rollers. The semicircle is very accurately hung and revolves noiselessly. The chair shown in the cut is adjustable in height, and when in use is put at such a point as will bring the line joining the observer's ears into the equatorial plane of the sphere determined by rotating the circle. The chair is mounted on heavy felt cushions which insulate it from any sound-waves which might be transmitted through the semicircle. The platform under the chair is marked off like a compass, so that any position of the circle can be determined. Upon the carriage are fastened the various instruments used for giving the sounds. These are so adjusted that at whatever position the sound is given a constant phase is presented to the observer.

The sounds employed were as follows: From (1) a tuning-fork of 1,000 vs.; (2) a stopped pipe of 768 vs.; (3) a reed pipe of 768 vs.; (4) a bell with a fundamental tone of approximately 2,048 vs.; and (5) a noise made by a telegraphic sounder. It would have been desirable to work with tones which were all of like pitch, but this was out of the question for the time being. It will be observed, however, that all the tones are within the middle range of the musical scale, and that they are quite close together in pitch, two of them being of identical vibration rate. The sounder and bell were operated by closing a noiseless electric contact. The two pipes were controlled by blowing through rubber tubing. The fork requires a somewhat more detailed description. It should be said that the intensity of all the sounds was kept as nearly constant as possible, and that the intensity aimed at was such as to render all of them perfectly distinct, without their becoming unpleasant.

The arrangement for the tuning-fork constituted the most elaborate and most troublesome technical part of the problem. To secure as nearly pure a tone as possible a carefully constructed resonator was made and mounted over the fork. The fork was supplied with a magnet between the tines, and this magnet was then connected with the circuit of an interrupting fork of just one half its own fork's rate of vibration. This is the device employed by Helmholtz in his celebrated experiments upon vowel sounds.[1] By bridging the spark in the driving fork one secures a tone in the second fork free from all accessory noises of interrupters, hammers, etc. In my experiments the driving fork was kept where it could not be heard, in a house at a distance from the experimental platform. That I thus secured an absolutely pure tone is, perhaps, more than can be confidently asserted. Resonator analysis failed to detect any tone apart from the fundamental, and, so far as concerns my observers, it can be positively stated that they were utterly unable to discern any complexity in the tone. The tone of the stopped pipe was not to them noticeably complex, so that they could confidently detect the overtones, and yet it was not so perfectly pure as the fork. It had the muffled effect characteristic of such tones. All the other sounds were noticeably complex.

My observers sat in an erect position, with eyes closed, but without a head rest. Previous experiments had led me to fear the effect of such a rest, when working with tones of the present character. My subjects were instructed to eschew all tendency to head movements while making their localizations, and I watched them as closely as possible to detect any such movements. Light, open arm rests enabled them to retain an accurate sense of their general bodily orientation and, after a little practice, readily to assume and retain the correct position. They were trained in the nomenclature employed to designate the various positions on the sphere, and in cases of any doubt they were asked to open the eyes and point. Needless to say, on such occasions precautions were taken to move the semicircle and carriage first, so that their position during the experiment should not be thus discovered. The sounds were given for periods of three to four seconds. This time was hit upon as the result of actual experiments made to determine that duration of the stimulus which would permit clear perception, without any feeling of stress or haste, and at the same time avoid tedium and the confusion sometimes caused by wandering attention. The several tests with different sorts of sounds were made as nearly comparable as possible by using the same

[1] Helmholtz, 'Sensations of Tone,' translated by Ellis, 3d ed., p. 399.

positions. This was, of course, not known to the subjects, who were given no indication of any sort as to the point from which they might expect the next sound. Moreover, the order was altered in which the various kinds of sounds were given at the several positions employed.

Of the three men who served as reagents for me one had had no previous practice in such observations, one had had a moderate amount, and one was extensively drilled. The results gained from all of them agree thoroughly in their fundamental implications, although there is naturally some quantitative variation. I place most confidence in the results of the most experienced observer, and I shall devote myself mainly to his reactions. This is the more warranted by the relatively small number of experiments I have succeeded in making under reliable conditions — some four hundred only. The accompanying table (Table I.) exhibits compactly the results of this observer's localizations:

Table I.

(Reagent, J. B. W.)

Average Error in Degrees.	Sounder.	Reed Pipe.	Bell.	Stopped Pipe.	Tuning Fork.
Longitude,	2	9.5	5	30.5	53
Latitude,	7	4.5	11	13.5	41
Total,	9	14	16	44	94

Results.

We may say at once, that under such conditions as these — *i. e.*, entire freedom from reflection — there is never any confusion of points in one lateral hemisphere with points in the other, save when one approaches very near to the median plane. Even then this form of error is extremely rare and probably attributable to wandering attention, to accidental suggestion from some extraneous source, or to some similarly irrelevant circumstance. The case of pure tones formed no exception to this rule, and the theories which make the intensity of the stimulation of the two ears fundamental in the explanation of localization, are at least correct so far as concerns the assignment of a sound to one of these hemispheres or the other. Sounds originating in the median vertical plane are also correctly referred to this plane.

When one scrutinizes the table further, however, it becomes clear that within the lateral hemispheres accuracy of localization appears to be a function of the sound. The average error in localizing the tuning-fork is 94°, which is more than a quadrant. With the stopped pipe the error is less than half as large as this, while with the bell and reed pipe it falls to less than a sixth, and with the noise is at its minimum, showing less than a tenth of the error with the fork. I lay no great stress on these precise figures, yet I have no question but that they indicate the intrinsic nature of the differences in the capacities of localizing these different forms of sound. Certainly the objective record was perfectly confirmed by the subjective assurance of the observers and their promptness of localization. Moreover, when, as in certain special experiments, the sounds were repeated two or three times in quick succession with a very brief duration for each stimulus, the accuracy of the localizations with the *complex sounds* was distinctly improved. This procedure seems to have the effect of making the quality differentia more noticeable than when the sounds are more continuous.

The comparison of the errors in latitude and longitude is not entirely free from ambiguity, because no points nearer than 45° to the poles were actually employed for giving the sounds. The observers did not know that this was to be so, but it makes comparison relatively unprofitable. In the case where localization is most accurate, the errors in latitude are notably larger than those in longitude, as one might anticipate from the standpoint of the intensity theory. In the tuning-fork case it would seem that mere chance might in the main be accountable for the results, with a single exception to be mentioned in the next paragraph.

The longitudinal regions immediately opposite the ears show fewer errors and errors of smaller amount in the localization of the pure tones, than do the regions in front and behind this. Indeed, the most striking difference in the localization of complex and simple tones is to be found in the ascription of the exact location of sounds to the various points in these lunes diagonally in front and behind. This is in accord with Steinhauser's computations upon the effect of intensity.[1] In the vertical plane, in which lies the line joining the ears, the localizations of pure tones are apparently relatively accurate save as regards height. This constitutes the exception above mentioned and seems to agree with Lord Rayleigh's observations and mathematical calculations, showing that the objective differences in the intensity of the sounds reaching the two ears, which is always relatively small under normal conditions, becomes rapidly less as we move away from the line joining the two ears. With the most experienced of my observers the average error of localization in latitude is nearly four times as large as that of longitude in this region.

Taken in their entirety the experiments seem to indicate that even with pure tones, intensity differences alone are sufficient to enable our confident and correct assignment of such sounds (1) to the median vertical plane, (2) to the lateral hemisphere from which they may chance to come, and (3) the further less accurate and less confident determination that certain sounds of this character belong to the vertical transverse plane of the head. But accuracy of localization as regards altitude in this transverse plane and accuracy in the several regions between this plane and the median plane — accuracy such as is commonly possessed, involving an average error of 10° to 25° — is apparently dependent upon tonal complexity and the modifications in timbre which complex sounds undergo through the changes in the intensity of their partials, when heard from different directions. Localization within the vertical median plane is inaccurate with all sounds, but most inaccurate with pure tones.

The matter can be put diagrammatically as in the accompanying cut, which represents the sphere within which the observer sits. (Fig. 3.) Sounds in the planes *FUBD* and *LURD* can, as the intensity theory requires, be localized with considerable accuracy as regards the *plane* to which they

[1] Steinhauser, 'Theory of Binaural Audition,' *Philosophical Magazine* (5), Vol. VII. (1879), pp. 181, 261.

belong. The exact *point* in the plane from which they originate is relatively uncertain, when intensity is the only available criterion. The experiments seem to show with some definiteness that, as we pass from one of these planes to the other, inaccuracy of localization rapidly increases, unless there be definite qualitative differences in the successive sounds. Without such qualitative variations the lune *UEDG* is subject to persistent confusion with the lune *UHDK* and the several points in each lune respectively are subject to gross confusion with other

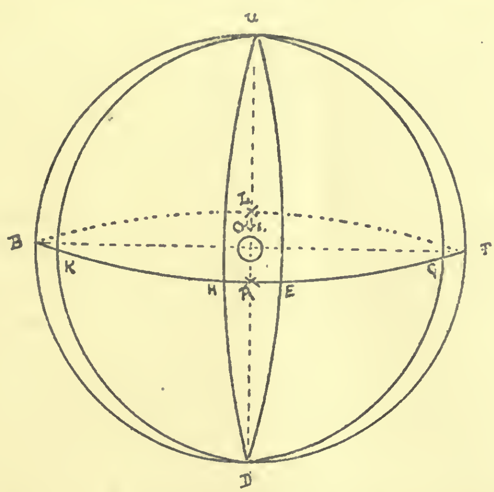

FIG. 3. *F*, *B*, *U*, *D*, *R* and *L* indicate, respectively, front, back, up, down, right and left.

points in the same lune. Whether the confusion of points in the upper with points in the lower hemisphere is in the case of pure tones notably different in quantity or other characteristics from the confusion of such points with others in the same hemisphere, it is not at present possible to say.

These statements concerning localization as a function of tonal complexity must not be understood as meaning that we are reflectively conscious of this local sign of direction involved in the changing quality, or timbre, of the tones. Sometimes this is noted, but it is not in any way necessary that it should be. Whether or not conscious experience teaches us in childhood to discriminate these varying sensations as having a varying spatial significance, is a question of genetic psychology with which it is not possible here to deal. Certainly as adults we make the localizations in an almost reflex manner. But the basis of the localizations is found in these symbols reported in consciousness as differences of quality, to which we have come to attach certain space values.

My subjects used much visual imagery in their judgments. My best trained observer seems in his localizations to be conscious of little else beyond such imagery and an occasional tendency to move the head in the direction of the sound. For him to localize a sound means chiefly to get a visual image of the sounding object in the position where he supposes it to be.

The work thus reported opens up the problem which I hope subsequently to work out in more detail. Much fuller observations along the line already pursued are required to permit more confident and inclusive conclusions. Differences in the localization of pure tones of widely varying pitch must be investigated, for the sound shadows involved with such tones and the diffraction experienced by them vary very considerably. It will be necessary to study more carefully the number and character of the partial tones concerned in the differences we have noted. This involves the whole question of relative intensity and pitch in the partials. All one can say at present is that with sounds of medium pitch such accuracy of auditory localization as our common everyday experiences reveal, seems immediately connected with the presence of distinguishable (though not necessarily noticed) partial tones. When such partial tones are absent or very inconspicuous, gross inaccuracy of localization is at once apparent. Detailed information relative to the localization of very high and very low tones is still to be secured. The effect of the duration of the sound upon localizing deserves closer inspection. In connection with several of the points just mentioned the peculiarities of auditory judgments of distance, as distinct from direction, also require more exhaustive investigation than they have as yet received.

In conclusion I wish to express my sincere obligations to Mr. J. B. Watson and Dr. M. L. Ashley, who have given me unsparingly of their time and assistance. I am also indebted to Dr. Warner Fite for assistance in the construction of the apparatus, and to Professor E. W. Mahood for service as reagent.

THE AFFECTIVE QUALITY OF AUDITORY RHYTHM IN ITS RELATION TO OBJECTIVE FORMS.

BY PROFESSOR ROBERT MACDOUGALL,

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It is difficult, in any appreciation of concrete rhythm, to determine that part of the total pleasure received which is due to the rhythmical form in which the material is cast; for the æsthetic delight which is connected with such an experience is the resultant of a variety of factors, not all of which are purely rhythmical. To make this analysis is, indeed, commonly impossible. The mood is unanalyzable, the object of enjoyment simple. It is the whole experience which is agreeable; and though we are able to say that several sources must combine, since there is present a variety of factors, each of which is a known source of pleasurable stimulation, it does not lie within our power to refer each increment to its separate cause; for the contribution of each factor varies with the aptitude of the individual æsthetic subject, and with the mood and direction of attention at the moment.

The phenomena proper to rhythm are identical in music and verse. The characteristic differences between the two lie solely in the nature of their secondary factors. The rhythms of music are expressed in forms which have rich and pleasing qualities—*tones;* those of verse are supported by forms which arouse varied and beautiful images—*articulate speech.* In the former the immediate sensuous quality of the sound predominates, the ideas suggested are secondary, obscure or lacking; in the latter the images which the words call to mind absorb attention, while the musical qualities of the voice are usually meager or unheeded. It is, therefore, a natural result that in music the formal conditions of rhythm are faithfully observed, while in verse they are transgressed constantly and with freedom.

The description of auditory rhythm as an experience affectively toned involves (*a*) a determination of its purely psychological factors — the modes of feeling which rhythm produces; (*b*) the relation of such modes to types of physical change in the bodily organism, by which the expression of rhythm is characterized; (*c*) the external conditions of pleasurable and painful feeling in rhythm, namely, the elements and relations of the objective rhythm forms which give rise to æsthetic satisfaction and dissatisfaction.

This paper deals with the third of these topics. In regard to the first, a few brief observations. Two factors are unquestionably present in our enjoyment of rhythm. There is what has been called the organic sensation of rhythm, the feeling of response, of pleasurable activity, which is not confined to rhythms actively expressed, but permeates also the appreciation of rhythms which are simply apprehended. There is also the form of the rhythm, which is an ideal content. It is impossible to give expression to the full æsthetic meaning of rhythm in a statement which excludes either of these factors.

The pleasure which such forms of experience afford us is not dependent solely upon an intellectual perception of rhythmical relations, else the peculiar temporal limits imposed upon the forms of succession which support this impression would be irrational; for the formal similarity of sequences which pass in a flash or drag out through hours, to those which we call rhythmical, may intellectually be very readily apprehended, but it is impossible for such measures to give rise in our consciousness to the effect which we call an impression of rhythm. If this were so, in the second place, the impression of rhythm should arise from the apprehension of those visual symbols which represent to the eye musical phrases and metrical speech. But such is not the case; the rhythm is felt by us only when, in some form, we translate those symbols into tones, words or representative movements. The rhythm may be uttered, or sub-vocalized, or the auditory sensations indicated may be called up in imagination as inner speech or song; but in some way, primary or secondary, a sensuous content must be added to the intellectual apprehension of the relations which the auditory sequence presents before any realization of its immediate rhythmical quality can arise in us. And thirdly, under such a conception, the expression in vocal or other motor terms by one who attends to a sequence objectively given should add nothing to the æsthetic effect which that rhythm produces; since it contributes in no way to the formal perfection of the rhythm. Only in the case of one whose auditory or visual perception, or capacity for ideal representation, was undeveloped, could such accessory activities be conceived to add to the pleasure derived from the rhythm; and this solely in virtue of the clearer comprehension of the rhythmical form derived from the kinæsthetic feelings involved in beating out or singing it. Such an hypothesis is self-contradictory, since, if the ideational content be in question and not the feeling of pleasure in the activity itself, the process cannot be conceived to clarify the rhythmical structure, because an apprehension of that ideal form must have been the prerequisite for giving expression to the rhythm.

Nor, on the other hand, can we refer the pleasure of rhythmical apperception or activity wholly to the sensuous feeling and organic reverberation aroused. In some way, clearly or dimly, the form of the rhythm is always felt. The pleasure derives not from the quality of the individual elements and the contrast of weak and strong stimulation or motor discharge alone, but involves also the fact that the rhythm is characterized by formal unity, that it possesses a beginning, a climax, an end, as individual and definite as the quality of the single beat or the constitution of the unit group which enters into it; and this sense of the complete formal sequence is present from the beginning and pervades the whole experience of rhythm. Curtail the series, and its fragmentariness is immediately felt as an imperfection of the rhythmic form; add redundant elements, and the overstepping of the natural rhythmic close is felt in the same immediate way; introduce incongruous forms of temporal or intensive relation, and the discrepancy jars upon the æsthetic consciousness as a violation of the sequence which the rhythmical formation demands. A rhythmical series uncompleted or wrongly executed may haunt the mind for hours or days, until satisfaction is obtained at last by striking the final note or singing the phrase in correct time.

Of these two factors — sensuous feeling and ideal form — the emotional effect of rhythm is due chiefly to the former. Its fervor varies directly with the richness of the sensuous impression and the predominance of the organic reverberation which is aroused. The rhythm which does not move one leaves one cold. Its most intense emotional effects are characterized by violent and widespread rhythmical movement. The ideational content does not in such cases manifest any appreciable development; on the contrary, it may suffer a distinct decline. The apprehension of rhythmic form is an abstract conception, as in that of other formal or mathematical relations. The beauty or dignity of its proportions may arouse our delight, as may a geometrical solution, but this feeling of pleasure is distinct in its origin and nature from that which is characteristically aroused by the reception of a rhythmical impression.

The objective factors upon which the character and intensity of the pleasure in a rhythmical sequence depend may be summed up under the following heads:

1. The absolute rate of succession among the elements of the rhythmical sequence.

2. The absolute intensity of these constituent elements (whether auditory or reactionary).

3. The relation of the absolute rate of succession to the prevailing emotional mood of the moment.

4. The number of elements of which the elementary rhythmical unit is composed.

5. The structural complexity of such units as dependent upon the differentiation of their components.

6. The proportions of the various temporal and intensive values within the rhythmical unit.

7. The form of succession which the elements of the unit group present.

8. The temporal and intensive differentiation of successive rhythm groups.

9. The combination of successive groups into higher rhythmical unities.

To these must be added as sources of gratification in every actual experience of rhythm the following two extraneous factors:

10. The musical quality of the sounds which support the rhythm.

11. The secondary associations which the tones or words of the rhythmical sequence arouse.

1. The absolute rate of succession.

As the very appearance of the rhythmical impression depends upon conditions of absolute rate of succession among the elements which support it, so within the range of temporal relations capable of giving rise to that peculiar experience, the character of the pleasurable impression varies with every change of rate. It is impossible without qualification to say that a progressive variation in one direction is characterized by a continuous increase of pleasure, while that in the opposite direction is marked by a decrease; but it can be said that the varying rates at which the elements of the rhythmical sequence succeed one another have each its peculiar affective overtone, qualitatively unlike those of slower or more rapid rates.

Certain factors which, in a general sense, are elements of the æsthetically agreeable impression do thus increase and decrease as the speed rises and falls. With increased rapidity the definition of the rhythmical sequence, both as regards the segregation of the groups from one another and the differentiation of accented and unaccented elements within the group, becomes more adequate. Secondly, as the rate increases, the limits of the individual group and of higher rhythmical syntheses include a larger and larger number of elements. And finally, in connection with the latter set of changes the differentiation of successive rhythm groups, temporally and intensively, grows more marked. Nevertheless it cannot be said that as the speed rises the pleasure in the rhythmical impression increases. Rhythms of slow tempo and simple phrase may, at any given moment, be vastly preferred to rapid and complexly coördinated forms. The element of absolute rate does not stand alone; its influence can be interpreted only in connection with other simultaneous variants.

Upon conditions of rate depend finally those characteristic qualities by which we describe the various types of familiar rhythm as gay, sprightly, restful, solemn. These qualities are not attributed to the rhythmical sequence in virtue of secondary associations; they are immediate affections of consciousness due to the pure rhythmical form of the sensuous impression. The rhythm *is* lively or restful; it does not remind us of exciting or quieting things. The quality in virtue of which such terms are applied to rhythmical sequences is describable only in terms of the whole constitution of the rhythmical impression; it is not the rate alone, but the forms of the unit-groups and their succession as well, which produce this peculiar effect; but it is due to the rate, since the whole latter group of changes, except rhythmical melody, varies in dependence upon the rate at which the elements succeed one another. Every rhythmical sequence has such a characteristic quality of gracefulness, dignity, lightness, decision, and the like. The slow are solemn, noble, decided; the rapid are gay, light, graceful. These characters will be mentioned in connection with the number and form of the rhythm group; here it is necessary only to point out that they are fundamentally connected with the factor of absolute rate among the elements of the sequence.

2. The absolute intensity of the rhythmical elements.

The changes in affective overtone due to variations in intensity are much less striking than those which follow upon changes in absolute rate of succession. The same rhythm endures through all degrees of intensive change, while with relatively slight variations in rapidity the affective impression is altered completely. The two phases are comparable with those aspects of sensation indicated by the technical terms 'quality' and 'intensity.' Intensive variations, therefore, are less significantly related to the æsthetic impression produced by the rhythm than are changes in absolute rate. The subordination of this factor has been noted by both Meumann and Ettlinger.

Nevertheless, such intensive changes have specific effects upon the quality of the æsthetic impression, and from moment to moment in our experience may affect it to the extent of determining its sign as positive or negative in a scale of æsthetic values. In extreme ranges of intensity this divergence is most striking; the quality of a rhythm given in faint, subdued tones is almost incomparably different from that of one of the same structure and tempo expressed in loud and ringing sounds. But within its middle ranges variation in the intensity of the elements is much less readily noticeable, and affects the æsthetic character of the impression in a far weaker degree, than does that of absolute rate. The typical effect of slight intensive changes is to produce a differentiation of phases between the louder and fainter groups of sounds as factors in a higher rhythmical unity. The variation in intensity brings about a new æsthetic construction of material, but is not apprehended in its proper quality at all. This effect is analogous to the disappearance of perceptional differences in the elements of a repeated iambic sequence to which Mach has called attention.

3. The relation of absolute rate to the prevailing emotional mood of the moment.

There is no tempo which can be called pleasing, none which can be characterized as displeasing, apart from the prevailing mood of the moment.[1] It must be congruous with the feeling of the subject in whose consciousness the rhythm appears, if it is to awaken pleasure. In virtue of changes in the subjective attitude, the affective character of a rhythm may be entirely transformed, and that which at one occurrence gave peculiar delight may later become intolerable.[2]

[1] With the exception of such rates as directly interfere with the distinction or force of the rhythmical impression. Such rates are those which lie near to the upper and lower limits of the series which is capable at all of supporting the rhythm experience. These are always less pleasing than rates in the middle of the scale.

[2] James notes this connection between varying moods and specific rhythmical tempos in the following remark: "There is a certain emotional *feeling* accompanying the intervals of time, as is well known in music. *The sense of haste goes with one measure of rapidity, that of delay with another;* and these two feelings harmonize with different mental moods."

But in so harmonizing, the impressions of slowness and haste themselves disappear, and they do so because in each case the periods of objective and of natural organic rhythm have approached each other. The interval of 0.62 second which Vierordt found 'adequate' in the production of an agreeable effect can be interpreted only as the resultant of a plurality of essentially different rates, or as characteristic of a single mood which prevailed during his experiments.

This dependence of particular rhythm forms upon the mood of the moment

These changes in æsthetic quality are not attributable to secondary associations which connect the rhythm in question with former scenes and experiences, the memories of which are incongruous with the present mood of feeling, though the intensity of emotion may be greatly increased by such factors. It is the naked quality of the rhythm which is momentarily displeasing, not that of the associations which it brings to mind. The explanation which I have to offer is speculative. The rhythms which we prefer when fresh, gay, lively, hopeful, when energy is abundant and the amount of movement large, are those which are characterized by rapid tempo and complex integration; they are, in correspondence with the description of our moods, light, graceful, vivacious measures. When we are fatigued, depressed, weak or melancholy, the rhythms we choose are slow of tempo and simple in their construction and sequences; they, analogously, are called restful, solemn, dignified, and the like. In the one case we are attracted by, and spontaneously give expression to, rhythms which involve rapid motor discharges having a wide range of differing intensities and complexly integrated into groups and sequences. In the other case we prefer such as require only slowly repeated discharges which have a narrow range of intensive variations, and present the simplest types of sequence and combination. The conditions of the rhythmical impression are so related to these general subjective conditions that it pleases or fails to do so according as its tempo (together with such other factors as depend upon this) is congruous or incongruous with the normal rate and intensity of motor discharge at the time; and according as the comprehension of the rhythmical form makes or does not make too great demand upon the fatigued or enfeebled attention process. The capacity of any rhythm to please is dependent

for the appeal which they make to the æsthetic sensibility is constantly commented upon by those who take part in such investigations as the present. Comparing two simple forms. one subject says: "These are about equally attractive. In a quiet mood I should enjoy the first; if I were nervous I should enjoy the second." Another says of a certain rhythm: "It indicates hurry to my mind, and would probably, under conditions of mental excitement or great stimulation, be very agreeable. The agreeableness of various rhythms seems to depend upon my state of mind as influenced by other causes."

upon the relation of the attention necessary to apprehend and reproduce it, to our own stock of vigor and mental control at the moment. We dislike those to which we cannot adapt ourselves with success and ease. Dr. M. K. Smith has commented in an interesting way upon the results of such enforced rhythmical adjustment by suggesting that certain forms of social discontent may possibly be attributed to the constant subordination of movements on the part of workmen in various industries to periods imposed by the mechanisms which they operate, which contradict the natural rhythms of the human organism and thereby produce irritable and inharmonious discharges.

4. The number of elements of which the rhythmical unit is composed.

With this factor the characteristic affective differences in the rhythmic impression are commonly connected. Increase in the number of such constituent elements makes the sequence gay, light, sprightly, cheerful; decrease in number renders it solemn, restful, stately, noble. While the number of elements is unquestionably one factor in the production of such impressions of quality, it is only one of a group which combines to this end. The simplicity or complexity of the rhythmic structure is coördinated with the factor of number in affording a justification for the terms stately and noble, sprightly or graceful. The characteristics of smoothness, buoyancy, insistence, boldness, weight, are dependent upon the position and intensive relations of accented and unaccented phases within the group, not upon the number of elements or the tempo directly. Of all measures the spondee is most grave, sober, dignified; the number of constituent elements is small, the proportion of these within any given series which receive emphasis is large, and the tempo, in consequence of this fact, is slow.

The differentiation of trochaic and iambic forms, or of dactylic and anapæstic, from this point of view, is difficult. In each of these pairs the number of elements is identical, and either may be uttered in slow or fast time, thereby transforming the affective quality of the experience and giving to it at will a grave or gay character. At the same time, the absolute rate at which a rhythmic sequence is uttered stands in intimate relation to the form of structure involved. All double or all triple measures have not the same natural tempo. The rapidity with which it is felt to be appropriate that the sequence should be uttered is affected both by the distribution of intervals within the group, and by the intensive relations of its accented and unaccented phases.[1] The first of each of the above-mentioned groups has customarily been described as bold, flowing, buoyant, while the latter is grave, insistent, weighty; but these descriptive phrases have their origin rather in the position of the accented element of the foot and its proportional time-value than in the number of syllables of which the rhythmic unit is composed. This descriptive classification has been extended — and with equal justification — to prose utterances.[2] Not only the number and relation of elements in the simple group affect the æsthetic quality of the rhythm, but the coördination of these groups in higher unities as well. Dipodic structures are sprightlier than simple sequences, short and common meters than long meter.

5. The structural complexity of the rhythmic unit as dependent upon the differentiation of its components.

The satisfaction which any rhythmical sequence affords is dependent upon a process of individualization which penetrates every part of its structure and gives to each element a functional uniqueness. This differentiation marks the temporal as well

[1] The average duration of the trochaic measure was found in the writer's experiments to be greater than that of the iambic in the ratio 1.000 : 0.791. The temporal values of dactyl and anapæst were almost identical, but the difference which does appear indicates a greater rapidity in the natural tempo of the anapæst than of the dactyl. The amphibrach is marked by a still slower tempo than the dactyl. The series of proportional values is as follows: Dactyl, 1.000; anapæst, 0.997; amphibrach, 1.039.

Ettlinger had previously pointed out this relation of the trochaic and iambic forms, and of series in which the one or the other predominated. He remarks that the Greeks, in virtue of this characteristic difference, called the trochaic '*hesychiastic*' or restful, and the iambic '*diastaltic*' or stirring, and notes the fact that Bach used the former in the choral, the latter in the gavotte.

[2] For the application of concepts of constancy and proportion to prose writing see Schurman, 'On the Length of Prose Sentences,' Univ. of Nebraska Studies, Vol. 1.

as the intensive aspects of the series. It is a factor of which the appreciative subject is not specifically conscious, nor can it be observed by introspective analysis. Its presence imparts distinction, force and character to the rhythm; its absence is marked by monotony and insipidity in the sequence. The notations of music and the so-called poetic forms are never adhered to by the singing or speaking voice, and these departures from the written scale are not made in subservience to the expression of transient emotion, but are constant and universal. The neglect of observing these finer gradations of intensity and duration imparts an inflexible and artificial quality to the rhythm, which is the chief source of the harshness and lifelessness of mechanically produced music. There is properly no reduplication of parts in rhythm. As many temporal and intensive values exist as there are elements in the group. If three members compose it, each of these differs characteristically from both the others in its force and in its duration (or in that of the interval which follows it). Position and function give a different dynamic value to every constituent of the group, the form of which arises not from a separation of its elements into two groups, accented and unaccented, but from the integration of elements each of which is individual and unique.

6. The proportion of the various temporal and intensive values within the rhythmical unit.

These differentiations are not made at random. The unit of structure — and the whole rhythmic sequence as well — manifests definite proportions both of its intensive and temporal values, the maintenance of which is an indispensible condition for the production of an agreeable æsthetic effect. This principle of proportion involves not less than three factors: intensity, duration and position, with their inter-relations. First, the æsthetic effect depends upon the maintenance of just proportions of intensity between accented and unaccented elements. Undue disparity destroys the rhythm, inasmuch as that accented element which is too greatly differentiated from the unaccented members of the group fails to be coördinated at all, and is regarded simply as an unrelated interpolation. This is especially true when the exaggerated element is a secondary variant from a well-established series of accents, but it holds for simple groups as well.

Of proportions among the various temporal intervals which the group presents we cannot speak absolutely, since the values of these are dependent upon factors of stress or intensity in the preceding reactions.

As regards position, in so far as we can speak of the structure of rhythmical units apart from the form of the whole sequence in which they find place, the initial and final members of the æsthetically satisfactory group are characterized by force, the median element by weakness. Almost all our poetical measures present either initial or final stress; median accentuation is practically unknown. In music the initial stress is observed without specific direction.

But the æsthetic adjustment of relative intensity in initial, median and final members of a group cannot thus be considered apart from the whole rhythmic sequence of which it forms a part; for the specific differentiation of such elements depends upon their position in the verse or phrase of which they are components. The form of the dactyl, for example, may change, as the wave of movement passes along the line, from that in which the final member is the weakest of the group to that in which it is scarcely less forcible than the initial accented element. The production of the perfect æsthetic effect depends upon the maintenance of those particular intensive proportions which are characteristic of each position in the whole series.

The proportions due to inter-relation of the three factors mentioned above involve connections between duration and intensity, between duration and position, and between intensity and position. Of position and duration it can be said, as regards the internal arrangement of the group, only that the values of the intervals must be such as the relative intensity of the preceding reaction demands, and that in the course of the rhythmical sequence they must change as the relations of the reactions vary in dependence upon change of position in the whole series.

In connection with an investigation of the constitution of simple rhythm forms reported in a supplement[1] of this journal, the quantitative determination of the relative temporal values of accented and unaccented elements necessary to afford æsthetic satisfaction was taken up at a single point. Two rhythmical forms were selected for experimentation, the trochaic and the dactylic. The intensive relations of the rhythmical elements were constant throughout the experiment. The hearer listened for ten seconds to the series of sounds, when the stimulation ceased. Stoppage was always made coincident with the completion of the rhythmical unit. The duration of the single experiment was determined on the basis of reports by the subjects concerned as to the time necessary to apprehend the rhythm clearly and to appreciate its æsthetic worth.

The æsthetic valuation of the various rhythm types afforded by these changes in the durational values of accented and unaccented elements within the group was made by reference to a numerical scale, in which the line of indifference was represented by the number four, and agreeable or disagreeable impressions by departures from this point toward theoretical extremes of seven and one respectively.

Six persons in the case of the dactylic form, five in that of the trochaic, took part in this investigation. The quantitative results are given in the following tables. The figures at the

TABLE I.

(TROCHAIC FORMS.) — ∪

Acc. Elem. Unacc. El.	1.000 0.565	1.000 0.636	1.000 0.714	1.000 0.800	1.000 0.895	1.000 1.000	1.000 1.118	1.000 1.250	1.000 1.400
Subject A.	4.0	4.0	4.0	3.5	3.5	3.0	5.0	5.0	5.0
Subject F.	4.5	4.5	4.0	4.0	4.0	4.0	4.5	4.0	4.0
Subject M.	5.0	4.0	4.0	4.5	4.0	3.6	4.0	4.5	5.0
Subject N.	5.1	5.4	5.1	5.1	4.5	3.7	5.0	4.0	6.0
Average.	4.65	4.47	4.27	4.27	4.00	3.57	4.62	4.37	5.00
Subject H.	3.0	4.0	5.5	5.5	5.5	4.5	4.5	4.0	4.0
Total Average.	4.32	4.38	4.52	4.52	4.30	3.76	4.60	4.30	4.80

These quantitative results are represented in the curves given below. In the upper drawing the curve of subject H. is represented separately from the rest; in the lower the curves for all subjects are combined in one.

[1] *Monograph Supplement*, No. 17, being 'Harvard Psychological Studies,' Vol. I.

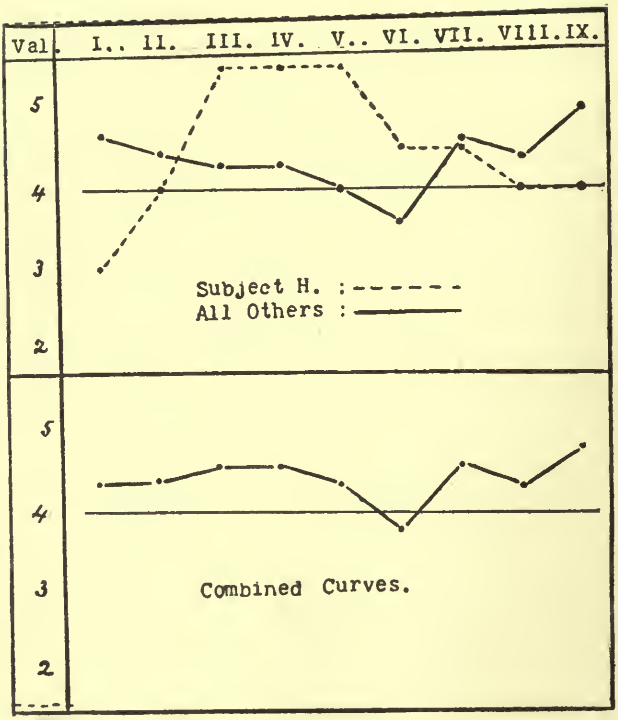

Chart I.

Table II.

(Dactylic Forms.)

Acc. Elem. 1st. Unacc. 2d. Unacc.	1.555 1.000 0.444	1.444 1.000 0.555	1.333 1.000 0.666	1.222 1.000 0.777	1.111 1.000 0.888	1.000 1.000 1.000	0.888 1.000 1.111	0.777 1.000 1.222	0.666 1.000 1.333
Subj. A.	4.0	5.0	4.0	4.0	4.5	4.3	3.9	3.5	2.5
" C.	6.0	6.0		5.0	5.0	3.0	2.0	2.0	3.0
" E.	4.0	4.0	5.0	3.5	4.0	4.0	4.0	4.0	5.0
" J.	4.5	5.0	5.0	5.0	4.8	5.0	5.0	5.0	5.0
" N.	4.9	4.2	5.0	4.8	5.1	4.5	4.0	3.8	4.7
Average.	4.68	4.84	4.75	4.46	4.68	4.16	3.78	3.66	4.04
Subj. H.	3.0	2.5	3.0	5.6	5.2	5.3	1.7	2.5	2.0
" M.	3.0	2.5	3.3	3.4	4.5	4.0	3.5	2.5	3.0
Average.	3.00	2.50	3.15	4.50	4.85	4.65	2.60	2.50	2.50
Total Av.	3.84	3.67	3.95	4.48	4.76	4.40	3.19	3.08	3.27

These quantitative results are represented in the curves given below. In the upper drawing the curves of Subjects H. and M. are presented separately from the rest; in the lower the curves for all subjects are combined in one.

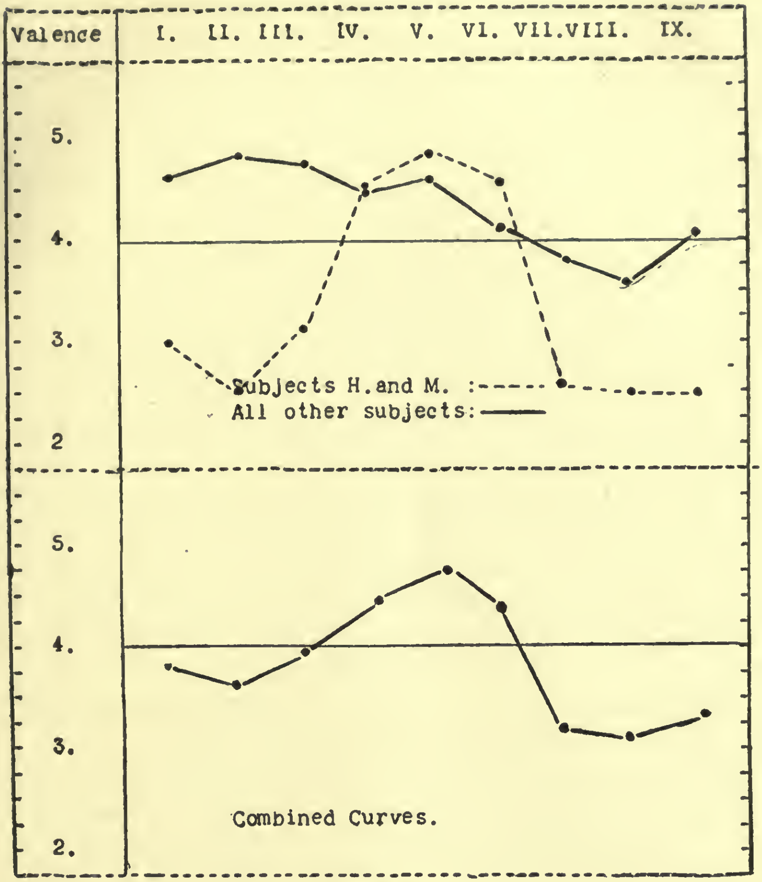

Chart II.

heads of the columns represent the relative temporal values of the intervals; those in the body of the table represent averages of five judgments upon each individual rhythm form.

In the case of each rhythmical form the reports are divided into two sub-groups on account of certain divergences, toward the extremes of the series of values, from the common type of preference which the median range exhibits. In the case of trochaic measures the divergent group consists of one member; in that of the dactylic, of two members, one of whom is that subject who showed divergence in the preceding group. In the case of these two subjects the characteristic preference for preponderating time-values in the accented over the unaccented intervals persists only within stricter limits than in that of the remaining members of the experimental group. Further differentiation is accompanied by either the appearance of a distinct feeling of disagreeableness or an inversion of the type of rhythm by which an anapæstic form is substituted for the original dactylic.

Æsthetically agreeable rhythmical forms are characterized by non-uniformity in the values of their constituent intervals. The most clearly marked tendency in the estimation of such temporal disparities is to prefer those types in which the interval following the accented element exceeds that which follows the unaccented. The appreciation of forms in which this relation is reversed shows more irregularity. In dactylic rhythm this arrangement of intervals produces a uniformly disagreeable impression. In the trochaic form, on the contrary, the highest positive value appears at the extreme of the descending series, with a second high node adjacent to the point of temporal uniformity. These inverted types have a strikingly different affective quality from those in which the accented interval preponderates; they produce a brilliant, staccato effect, and it is from this novel quality that their agreeableness arises. The upper and lower halves of the scale should be regarded as embodying characteristically different types of rhythm, depending upon specific relations of magnitude among the temporal values of their respective intervals. Within each half of this scale certain proportions are felt to be just and correct in the distribution of these time values; and any departure, of excess or defect, from this ideal relation appears as a mutilation of the proper æsthetic effect of the rhythm. But when one has passed, in either direction, through the zero point of temporal uniformity, and the relations of magnitude in accented and unaccented intervals are inverted, one no longer receives the impression of a distorted rhythmical form which involves a still further departure from the original type, but of one which embodies a different principle of arrangement in virtue of the novel æsthetic effect produced.

7. The form of succession which the elements of the unit group present.

Not only are certain limits prescribed to the absolute amount of variation which may be comprehended within the rhythmical group, and a law of elementary proportion thereby established among the values of its constituents, but the group as a whole must also possess definite proportions in both its intensive and temporal characters. Every unit group and every formal sequence has a characteristic rhythmical melody. The definition of elementary proportions does not describe the nature of the group as a specific rhythm type, for the series of relations cannot be read indifferently in either direction. The form of succession among the elements is of fundamental significance. The same series of intensive or temporal qualities which give a rhythmical grouping having a certain characteristic quality when read in one direction, produce a totally unlike æsthetic effect when read in reverse order. In what this difference consists, one may find it difficult to say, but the unique quality of each form is a matter which immediate experience never leaves in doubt.

The resolution of the factors of this experience is speculative. I should describe it as due to the formal succession of organic tensions involved in the representation of the various rhythmic forms — in other words, to the melodic relations of a series of strain experiences.[1] We may suppose the same simple relations to obtain always between elements of duration, between those of stress, and in the connection of the members of these parallel series with one another. In that case we shall have the conception of definite simple quantities entering unchanged into different melodic series; but such an incorporation is itself a determinant of the nature of the individual element. The æsthetic quality of the single note in music is absolutely dependent upon its formal relation to preceding and succeeding tones in the melody of which it forms a part, and there can be

[1] This view is, in general, in accord with Ettlinger's theoretical discussion, in which the experience is reduced to tendencies toward movement and rest oscillating about points of equilibration in the series. I should prefer, however, to couch the statement in less conceptual and intellectual terms.

as little doubt that the characteristic quality which a specific strain experience possesses depends in a similar way upon the form of the whole series of tensions into which it enters. The value of each (hypothetically) simple quantity in such a series of kinæsthetic impressions undergoes transformation with every incorporation into a sequence having a new form of succession. Upon such formal relations with a series of organic tensions depend the characteristic qualities of the various common rhythm types. These have already been considered in § 5 of this paper. The specific qualities of lightness and weight, of liveliness and solemnity, of buoyancy, insistency or dignity there described are due to the form of the succession of motor impulses and organic tensions severally involved.

The conception of simple, unchanged quantities entering in various orders into characteristically different rhythm types, which the preceding statement has assumed is, however, unquestionably false to the facts. One does not get a trochee by reading an iambus backward, nor an anapæst by inverting a dactyl. The formal succession affects the elements of the series not only by the establishment of laws of melodic relationship, but also by transforming the character of each individual component itself. The relations both of intensity and duration presented by the trochee are different from those which obtain in iambic rhythms, and likewise in triple rhythms the whole series of relations which each type presents is unique and not convertible into other types. This is one of the factors of difficulty in passing readily from one type of rhythmic apprehension or expression to another, which has been largely overlooked in discussions of the matter.

Further, the two series of values, intensive and temporal, influence each other at every point. Not only does the intensively higher element attract to itself a proportionately greater time value, but that factor which is temporally extended is also marked by an (unconscious) increase of stress. We come thus to the conception of groups of elements organized in definitely configured types, and sensitively responsive in every part as the basis of our æsthetic valuation in rhythmical sequences.

8 and 9. The temporal and intensive differentiation of successive groups and their combination into higher rhythmical unities.

These are discriminable aspects of a process which is concretely unitary; they may therefore be considered under a single head. The introduction into a rhythmical sequence of variations from the typical figure is primarily connected in consciousness with a feeling of monotony in the unbroken series, and a desire to avoid the æsthetically displeasing quality which it in consequence possesses. Much less frequently is the motive recorded by the reactor a striving after more definite coördination of the series by the combination of simple groups in higher synthesis. Yet there is every reason to believe that the origin of the feeling of monotony is connected with the absence of those means of grasping larger sequences of elements which the differentiation of alternate measures and the introduction of specific points of phrase-initiation and finality afford; and that the peculiar satisfaction experienced in such differentiated sequences is immediately related to the increased definiteness thereby imparted to the form of the whole sequence.

The delight in such mixed measures is universal, and the tendency to transform the succession of uniform rhythm types by the creation of larger figures is strong and persistent. In the writer's experiments, whenever the subject was left free to choose the form of the sequence, he interrupted the repetition of identical groups by the introduction from time to time of a variant form. Even when required to continue an unvaried rhythm such departures were often introduced. When given a rhythm group which was to form the first half of a composite measure and asked to supply a second, metrically equivalent to it, these reactors—with one exception—differentiated the two subgroups. With the dactylic | ♩ ♩ ♩ | is made to alternate the syncopated form | ♩ ♩ 𝄽 |, and the like. Of such departures from the prescribed rhythm type the reactors wrote in the following manner:

> "I resort to this form occasionally in order to relieve the monotony of the continuously repeated simple form."

"I try to introduce either a time variation or a difference in pressure" (*i. e.* either a variation in the distribution of temporal relations within the group, or a differentiation into major and minor phases).

"The first measure [continuous iambics] grew tiresome very quickly and ceased to have any quality. The second [iambics with major and minor phases] was, however, distinctly pleasing."

"It seems that to break the monotony after about three [groups] is necessary."

Of four forms arranged in order of complexity one reactor says: "I liked the last combination better than any of the others, though I don't know why."

Such remarks occur in all the subjective reports upon this part of the work.

When different types of rhythmic structure were compared together in pairs, those forms preferred in every instance (with the exception of certain judgments of one reactor) were the more complex, those rejected the more uniform.

All such forms of differentiation derive æsthetic significance from their function as a mechanism of higher synthesis, a method by which unity is given to larger groups of elements as phrases in the rhythmical composition, or a definite configuration imparted to the sequence as a whole. Upon the adequacy of this process and the character of that melodic form which the total series of elements thereby receives, depends more than upon any other one factor the quality of the rhythmical sequence and the delight it affords as a psychological experience.

There remain to be pointed out only certain secondary factors of æsthetic pleasure which enter as inseparable elements into every concrete experience of rhythm, and to the existence and importance of which attention was called at the beginning of this paper. They may be summed up under two heads: *musical quality*, and *capacity for arousing secondary associations*.

The simple musical quality of the tones in which rhythm finds its expression is an immediate source of intense sensuous pleasure. Every rhythmical series of sounds, therefore, becomes a means of greatly increased delight when its component elements are characterized by purity and richness. But the term 'musical quality,' as here employed, involves much more than this. No tone in such a sequence stands alone.

Not only is it rhythmically integrated with preceding and following tones in smaller and larger groups, but it is also qualitatively related to those adjacent members of the series, and depends for its immediate sensuous effect upon the relation in which its quality stands to theirs. It is an element in a melodic as well as in a rhythmical sequence. This functional connection with other tones in a melody reinforces the pleasure-giving capacity of every tone in the series, and greatly increases the æsthetic worth of the whole experience. This melodic element penetrates not only all musical sequences, but the vast majority of utterances of the speaking voice as well; so that in every rhythmical sequence, with the exception of percussive instruments, its presence is to be reckoned with. Thirdly, the musical quality is enriched in still another way by the combinations of tones in harmonies when melodies are complicated by the union of voices or instruments.

The value of the secondary associations of rhythm is still less determinable than that of æsthetic tonal quality, the part they shall play still less predictable. Their forms are two, emotional and intellectual. The emotional associations of rhythm are voluminous and intensely moving. The emotions here in question are not hedonic qualities of the direct sensuous effects of rhythm and musical tones, which may be in the highest degree emotional. They are evoked through memories and ideas awakened by the melody or rhyme which the subject hears. This form is chiefly characteristic of music. In poetry the pleasure of the rhythm is constantly subordinated to the stream of images aroused by the articulate sounds which support the rhythm. The formal perfection of the rhythm is here so constantly broken by the demand for the fulfilment of laws of logical arrangement and expression that the pleasure arising from it is much less important than that which characterizes musical appreciation. It is only when the sense of the verse is momentarily forgotten, and one gives himself up to the music of its flow, that the factor of rhythm becomes pronounced in the æsthetic apprehension of poetical expression. The æsthetic effect of pure rhythm has nothing in common with either the significance of ideational series or the beauty of musical tones.

Its own proper qualities are intense and complex, but these can never be discriminated in the ordinary experience of music and verse. It is only when the intervals of a rhythmical series are defined by limiting stimuli of the barest sensorial quality, and varying only in regard to the intensity of the motor discharges which they originate, that it becomes possible to determine the relation of rhythmical forms to elements of æsthetic value. The attempt to do this in connection with several simple types of variation was made in the investigation reported in the present paper.

DISCUSSION AND APPARATUS.

A STUDY OF THE EARLY COLOR SENSE.

In this article I shall give the results of experiments which I carried out on my son during the first year of his life. The results deal with three problems in genetic psychology: (1) The existence of color sensations and their relative attractiveness to the child; (2) the child's visual estimation of distances before he can walk; (3) the way in which the hands are used in reaching before the education of their use has begun. In this paper I shall confine myself within the limits of my experiments and shall not refer to incidents dealing with the color sense recorded in my notes, though these confirm the results. The methods used in experimenting on his color sense were three in number and were arranged to fit in with his mental development. The methods of experiment will be given first in this paper and to each will be attached a short name so that in discussing results they may be easily referred to.

First or 'Eye Method.'—For testing by this method some cards were prepared, rectangular in shape, six inches long and four inches wide, and on these were pasted the colored papers used in the experiments. Two of the cards so prepared and of different color were held with their long edges in contact exactly opposite the middle line of his face and about one foot in front of it. The cards were provided with tags at the back so that they could be held without my hands being seen. The proper distance at which the cards should be held was taught by experience. As soon as he fixed his eyes on the edges of the cards that came in contact they were slowly moved apart and the one he followed with his eyes noted. The cards were then reversed in position, the experiment repeated and noted as before.

Second or 'Grasping Method.'—This was carried out by using colored crochet-work balls two inches in diameter. The colors of the balls matched the colors of the papers as far as they could be obtained to do so. As with the cards in the previous method, two of the balls were offered to him at one time. The one he reached out his hand toward and touched was noted, the position of the balls reversed and the experiment repeated. These balls gave an exactly similar appearance from all sides and thus were better than ribbons would have been.

Third or 'Reaching Method.'—The child sat in a chair having a tray in front of it during the series of tests by this method. For the previous ones he had been comfortably seated on his mother's lap. On to the tray was fixed an apparatus of neutral color for the purpose of holding the cards. Apart from the colored cards the apparatus had no attraction for him. By means of this apparatus cards similar to those used in the '*eye test*,' only reduced in size to three inches long and two inches wide, could be set at a known distance from his eyes and level with them. It was possible also to set the cards to either side of him, but all the experiments dealt with in this paper were made by placing the cards exactly in front of him. To obtain a result a card was placed on the apparatus at a certain distance and the child's reaction to the stimulus afforded by the cards recorded.

The first method was devised by Professor Earl Barnes, the second was an adaptation from his suggestions, balls being used instead of ribbons. The third method is due to Professor J. Mark Baldwin, who has published the results of experiments made on his daughter by this method in his book on 'Mental Development.'

The eye method enabled me to start my experiments on the child's 124th day, he then being exactly 4 months old according to the calendar. As far as I know this is 5 months earlier than any published series of experiments have been started. Before this date repeated attempts had been made to start my tests, but the child was unable to respond to the stimuli offered by the colors till then. The second method was useful when he no longer was content to sit and look at objects, but must touch them for himself, while at the same time he was still too young to start the third method. The reaching method enables us to investigate minutely, not only the child's choice of colors, but his visual estimation of distance, and the tendency, if any, to specialize in the use of either hand. For the purpose of this study I shall include under the term colors the following: Blue, green, yellow, red, dark red, brown, black, white and newspaper.

By the first method I carried out two series of tests, in the first of which all the colors were used. Each color was used with the others 6 times, making a total of 48 tests with every color, this being 216 tests in all. This series commenced on the 124th day and finished on the 155th day. The results of this series will be found in the first column of Table I. The colors in this table are arranged in order of preference, the favorite being placed at the top. The number at the

TABLE I.

	1 Eye.	2 Eye.	Grasp.	1 Reach.	2 Reach.	Total Reach.
Number possible.	48	48	36	105	70	175
Newspaper.	45			74	56	130
Red.			29			
Yellow.	29	32	19	73	54	127
White.	25	24				
Red.				72		122
Blue.	24	22	18	70	51	121
Black.		22		68		
Red.	22	20			50	
White.				66		
Green	22		6	65	49	114
White.					47	
Black.	18				46	114
White.						113
Dark red.	17					
Brown.	14			61	46	107

1 reach = 1st series reaching method, etc.

Total reach = total of reach method.

NOTE.—Each line contains only color named opposite it.

side denotes the times each color was preferred, while the total number possible is placed at the top of the column. In the second series, carried out by the eye method, only five of the colors were used. Each pair of colors was used as a test 12 times, giving again a total of 48 experiments with each color. The second column in Table I. gives the results of this second series. The third series of tests were carried out by aid of the grasping method. Unfortunately, I could only obtain 4 colors, and even then the red was not a satisfactory one, which probably accounts for its place on the list. Each pair of balls was offered 12 times, making a total of 36 tests with each ball. This series, the results of which will be found in the third column of Table I., occupied from the 189th to the 203d day. For the fourth series of experiments the reaching method was used, and this series lasted about 9 weeks, and consisted of 105 experiments with each color separately, a total of 840 in all. The results of this series will be found in column 4 of Table I. It should be borne in mind that in the reaching method we have a single color presented separately to the child, while in the previous method a contrast is made by offering two together. The fifth series of tests was really a continuation of the fourth, but the experiments by the third method were divided into two large series so that a comparison of the results might be made. Column 5 of Table I. is a record of this fifth series, in which each card was tested with 70 times, making a total of 560 experiments, lasting over a period of about 6 weeks. Column 6 of Table I. is obtained by adding the figures of columns 4 and 5, and is thus based on 175 tests with each color, which is a fairly large number and ought to give reliable results. The experiments were terminated on the child's 322d day. The tests by the reaching method had lasted from commencement to close 99 days, giving an average of 14 experiments each day. As regards the experiments, it may be said that the conditions necessary to prevent fatigue, etc., as laid down by Professor Baldwin in his book, were all strictly and carefully observed. From a perusal of Table I. it will be seen that the colors were preferred in the following order: Newspaper, yellow, red or blue, white or green, black, with brown last.

Having arrived at a conclusion as regards the general order of the colors, we shall proceed to study the results in greater detail. Fortunately, we have the tables given by Professor Baldwin in his 'Mental Development' to compare with the results now before us. Very early in the course of my experiments a table was prepared showing the results up to that time in an exactly similar form to the table provided by Professor Baldwin, so that both results could be placed side by side. Table II. is composed of the figures of my results placed under

TABLE II.

Distance, Inches.	9	10	11	12	13	14	15	Total.	Ratio A/N
	R A	R A	R A	R A	R A	R A	R A	R A N	
Blue,	0-1	0-4	0-5	1-3	2-4	1-5	3-1	7-23-30	.77
	0-4	0-6	1-2	1-2	1-2	1-3	2-2	6-21-27	.77
Red,	0-1	0-3	2-2	1-4	1-7	1-7	5-1	10-25-35	.71
	0-5	0-4	0-4	2-0	3-1	2-2	1-3	8-19-27	.70
White,	0-0	0-0	0-0	0-1	0-5	1-1	3-0	4-7-11	.63
	0-4	0-4	1-2	1-2	0-4	1-2	4-0	7-18-25	.72
Green,	0-0	0-1	0-1	2-1	1-4	1-2	2-0	6-19-15	.60
	0-5	0-4	2-2	1-1	2-2	0-3	3-1	8-17-25	.68
Brown,	0-1	0-2	2-1	3-2	0-3	3-1	2-0	10-10-20	.50
	0-5	0-5	2-1	2-0	1-2	2-1	3-1	10-16-26	.61
Totals,	0-3	0-10	4-9	7-11	4-23	7-16	15-2	37-74-111	.66
	0-23	0-23	6-11	7-5	7-11	6-11	13-7	39-91-130	.70
Ratio R/n,	0	0	.30	.38	.14	.30	.88	Total { .33	
	0	0	.35	.58	.38	.35	.65	.30	

The upper line opposite each color represents Professor Baldwin's figures, the lower line contains my figures.

those copied from the book I mentioned. The first horizontal line at the top contains the distances at which the cards were placed in the experiments. The line immediately below contains the letters R and A, which stand for Refusals and Acceptances, respectively. When he refused to reach for a card it was placed among the R's, while if he did reach after the card it was put among the *A*'s. The first vertical column on the left contains the names of the colors used, each one having two horizontal lines of figures adjacent and belonging to it, the top one being figures given by Professor Baldwin, while the lower one contains mine. In the last vertical column but one are placed the total number of *R*'s and *A*'s beside the total number of experiments got by adding these two (*N*) for each color at all distances. The ratio A/N is got by putting the total number of *A*'s over the total number of experiments, this being found in the last vertical column. In the horizontal line marked 'totals' are the total number of *R*'s and *A*'s for all the colors added at each distance, and below this is the ratio R/n, worked out by putting the total number of *R*'s over the total number of experiments for each distance.

It will easily be seen without going into details that there is a fairly close agreement between the two sets of figures, and in the ratio R/n line both show a break in the increasing number as we move from nine inches to fifteen inches, and that break occurs in both at thirteen inches and fourteen inches. My figures were obtained from the complete results of all tests that I made up to the time the table was made, except that experiments which I had made with colors not used by Professor Baldwin were omitted as they were of no use in making a comparison. Bearing in mind the fairly close agreement of our work, we must at the same time remember that the experiments were made under different circumstances. Professor Baldwin's were from experiments spread over six months, and these were apparently sprinkled among many other similar experiments with other objects in view. My figures are based on tests lasting up to that period only sixteen days, and I was experimenting with the colored cards alone. To save my results from the criticism of being hastily arrived at I may mention that not even on the most favorable day did my number of experiments exceed twenty-eight, and then they were always divided up with several hours' interval between, so that no sitting was longer than fourteen experiments; while Professor Baldwin allowed forty experiments in one day. At the end of each sitting our child was always left, like Oliver Twist, wishing for more. Another difference is that Professor Baldwin's experiments were on his daughter, while the subject of ours was a son. Again, the figures would naturally differ, from the fact that the colors used in experimenting are probably different, owing to no comparison having been made. My papers were without gloss and agreed well with the surface of blotting paper used by Professor Baldwin. I have made this comparison to prove that with care the third method is capable of producing a similar result with two children, and it would be extremely interesting to have several complete sets of experiments tc compare. If we had a few sets of experiments it might be possible to build up a theory of the color sense, which it is impossible to do on these two sets alone.[1]

Having pointed out the agreements in our figures it now remains for me to show the weakness of the tables as presented in Table II. From one point of view the close agreement of the results is purely accidental; not that the method itself leaves anything to accident, but the basis for the table does, if not carefully arranged. It will presently be seen that neither of our sets of figures presents a really valuable result, owing first to the small number of experiments involved and secondly to their incompleteness. It will be seen on referring to Table II. that neither of us had any *R*'s to record for the nine-inch and ten-inch distances, and in the 200 tests which I subsequently made at these distances it was the same — so that these distances are termed safe distances. The ratio A/N is worked out by putting the number of *A*'s over the total number of tests with each color, so that the larger the proportion of safe-distance tests such a total contains the larger will be the ratio A/N, from this reason alone, that all the tests offered at that distance are certain to be accepted — every color presenting a stimulus to cause the child to reach over these distances. In looking at our figures we find that Professor Baldwin made three safe-distance tests with brown out of a total of twenty, while white has none at all. Though in the instance just taken to eliminate the safe-distance tests would not affect the sequence of the colors, we can easily see that it might did they run each other close. Besides, it is an example of what occurs throughout both our sets of figures and detracts some value from the tables. To make a table on which we can depend, the same proportion of tests at each distance to the total number for the color must be taken. It is simpler, however, to do as I have done, and take the same number of tests for each color at each distance. The number I used was twenty-five, this giving a total of 175 experiments with each color, or 200 at each distance, or 1,400 experiments in all. The results of these 1,400 tests are arranged in Table III. and this shows no break in the continually increasing ratio R/n which is shown by Table II. Beginning at eleven inches with .07, we rapidly rise as the distance increases till we reach .89 at fifteen inches. The

[1] My attention has been called to a note by Professor Baldwin in *Science*, July 7, 1893, making certain corrections in his figures which, however, only increase the agreement with mine.

TABLE III.

Distance, Inches.	9	10	11	12	13	14	15	Total.	Ratio, A/N
	R A	R A	R A	R A	R A	R A	R A	R A N	
News,	0–25	0–25	1–24	4–21	6–19	11–14	23–2	45–130–175	.742
Yellow,	0–25	0–25	0–25	6–19	10–15	12–13	20–5	48–127–175	.725
Red,	0–25	0–25	0–25	4–21	10–15	19–6	20–5	53–122–175	.69
Blue,	0–25	0–25	2–23	4–21	8–17	19–6	21–4	54–121–175	.691
Green,	0–25	0–25	3–22	6–19	12–13	16 9	24–1	61–114–175	.651
Black,	0–25	0–25	2–23	7–18	10–15	18–7	24–1	61–114–175	.651
White,	0–25	0–25	2–23	6–19	11–14	19–6	24–1	62–113–175	.645
Brown,	0–25	0–25	4–21	8–17	12–13	22–3	22–3	68–107–175	.611
Totals,	0–200	0–200	14–186	45–155	79–121	136–64	178–22	452–948–1400	.670
Ratio R/n	0	0	.07	.22	.39	.68	.89	.32	

small difference in the ratio A/N for the colors shows how carefully the experiments need to be made to avoid errors, and, but for the fact that the order of preference as given by this method compares well with the order given by the first and second methods, I should not have so strong a faith in the results.

Fig 1.

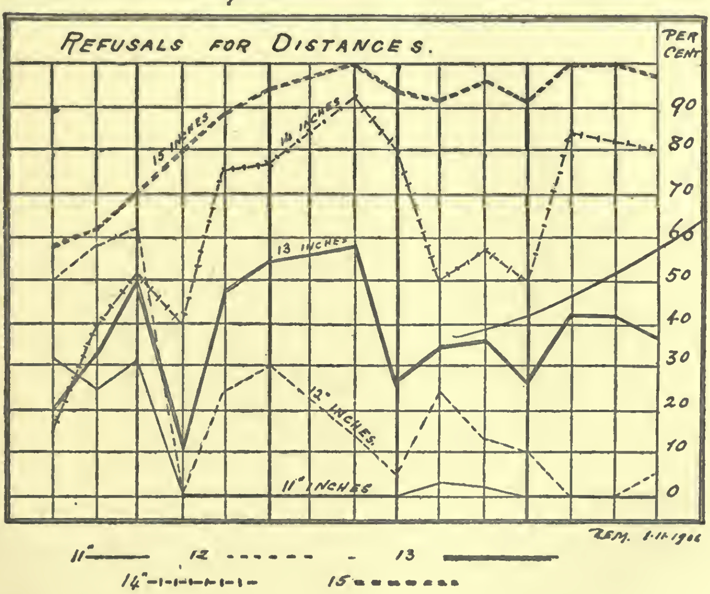

FIG. 1.

We will next consider the way in which the refusals to reach for the cards were spread over the distances, and we will try to see at what rate the child learned to discriminate between them. Fig. 1 is a chart showing the refusals for the distances and the way in which they increased or diminished as time went on. Each section between the thick vertical lines represents a complete series of 200 tests, there being five of these series. The first line in each section is marked with the proportion of refusals for one third of the series of 200, while the second line bears the proportion for two thirds of that number; the last or boundary line bears the points for the whole series. Thus every section is complete in itself, a fresh start being made each time a new series of 200 was started. Each section was made separately from the others, so that not only could the progress during each series be seen, but also each series could be compared as a whole with the preceding and succeeding ones. Lines drawn through the points on the boundary lines will give curves showing the progress made

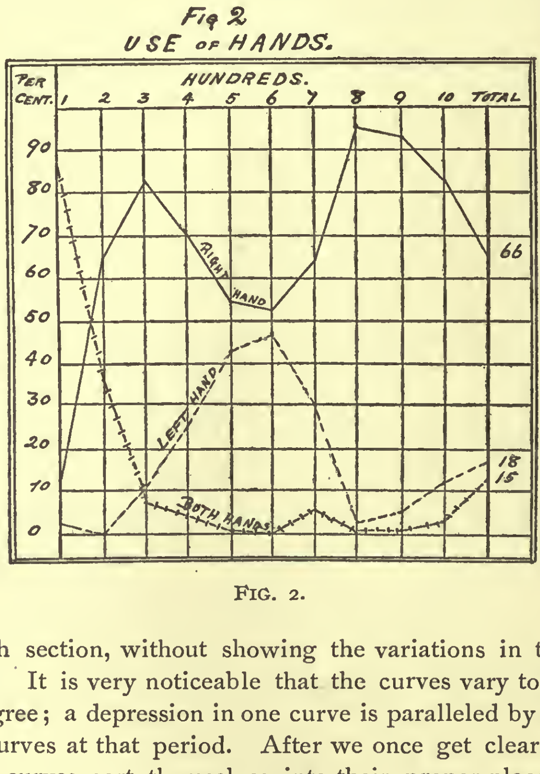

FIG. 2.

during each section, without showing the variations in the sections themselves. It is very noticeable that the curves vary together in a marked degree; a depression in one curve is paralleled by depressions in all the curves at that period. After we once get clear of the first section the curves sort themselves into their proper places, and this chart shows how Table II. has a break in the ratio R/n, while Table III. has none. The experiments used in making Table II. are part of the first section in this chart, while the figures for all five sections are used to make Table III. It was during the carrying out of the experiments represented by the first section that the child was educated into the meaning of inches. After this he had only to learn to more surely inhibit useless movements

The last part of our study will deal with the way in which he used his hands in reaching out for the colors. I did not begin to keep a record of the way in which he used his hands till I experimented by the third method. In the early experiments he used both hands together more frequently in reaching for the cards, and out of the first hundred *A*'s no less than 86 were made in this way.

In Fig. 2 we have a chart in which the first 9 vertical lines each represent 100 *A*'s, the tenth represents the odd 48, and the last the total of 948. As each 100 *A*'s were completed the total number of right, left, and two-handed efforts were worked out, thus giving a record of the tendency he had to use either hand. If we look at the chart we find that both hands were more frequently used together at first, and that shortly afterwards one or the other hand was used alone and the two-handed efforts almost eliminated. The right hand soon grew in power, and during the third hundred reached its first apex in

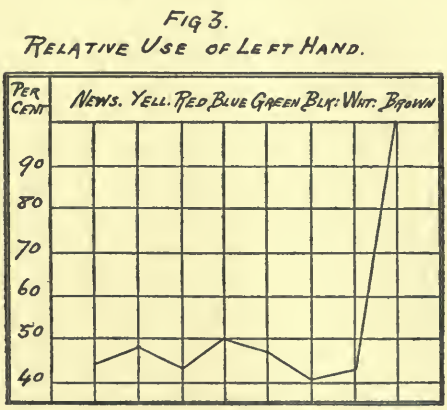

FIG. 3.

the curve. Slowly, however, the left hand is urging its claim in the experiments, and it grows so steadily in the child's preference as to almost equal the right hand during the fifth and sixth hundred *A*'s. From the period represented by the sixth line the left hand loses ground, and ultimately only 6 per cent. of the efforts are with the left hand. The right hand having overwhelmed its competitors—the left hand and both hands together—now makes about 92 per cent. of the effort. The period of greatest use of the left hand was during the

40th and 43d weeks. This coming in and going out of the use of the left hand was also found by Professor Baldwin (see his 'Mental Development').

We have lastly to study the effect of the colors and of the distances on the preference he had at times for the left hand. We will deal first with the effect of color. I have drawn out a chart based on the proportion got by putting the number of times the left hand was used over the total number of *A*'s for that color; by so doing I have deviated from the method pursued by Professor Baldwin in working out the same question. He takes the number of left-handed efforts and places them over the whole number of experiments. The reason why I have taken the *A*'s alone instead of the total number of experiments is that there was probably a latent impulse to the use of one hand or the other, and therefore the *R*'s are uncertain factors which cannot be counted. When I had worked out a chart by the method described above I found it an ungainly one on account of it being necessary to deal with three places of decimals to show the effect of the color stimulus on the use of the left hand. I therefore made a new chart (Fig. 3) by taking the color with the highest ratio of left-handed efforts as represented by 100 and working out the others relatively to it. In Fig. 3, brown—which had a ratio of .379—is taken as 100, the other columns are then represented by 44, 48, 43, 50, 46, 41, 44, starting from the left of Fig. 3. From this we are obliged to conclude that the colors do not draw out the left hand in a direct proportion to their attractiveness to the child.

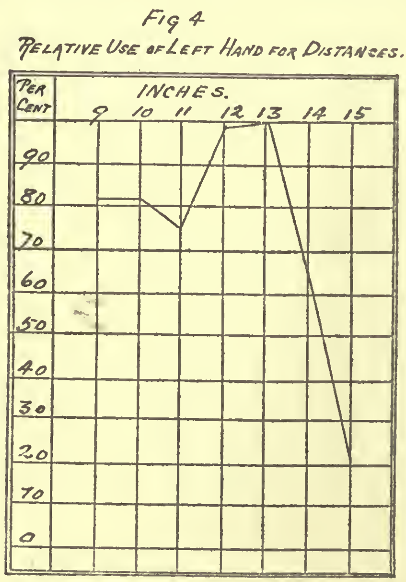

FIG. 4.

It might, perhaps, be objected that the experiments with brown were made during the period of the greatest use of the left hand. This was not so, as the experiments for every column were equally spread over the whole period covered by the tests, and the number with one color never exceeded the number with any other color by more than three.

Pursuing a course, in dealing with the effect of distance on the left hand, similar to that which gave us Fig. 3 for the effect of colors, we get Fig. 4. It will be seen from Fig. 4 that the proportion of responses with the left hand increases up to the 13-inch distance, which is represented by 100, and then declines rapidly. From this it is seen that the effect of distance is not exactly what we should expect, but the fact that various colors were the stimuli probably renders this last question too complex to be answered by the experiments we have been dealing with. To answer this question properly it appears best to take a large number of experiments with a single object.

RUFUS E. MARSDEN.

EXPERIMENTS IN THE PERCEPTION OF THE THIRD DIMENSION.

I have frequently had opportunity to experiment at random on the perception of solidity, and as the results exhibit some bearing upon the study of the so-called motor factors in perception these results may be described.

The experiment which I have tried many times, and especially when travelling, which affords good opportunities for the experiments, consists in testing both binocular and monocular vision in the way described below.

It is perhaps often remarked that favorable conditions will enable the observer to see in the opposite direction from which the objects are situated the images of any given thing, as if seen through the solid matter toward which the observer is actually looking. Changes of adjustment will affect the results somewhat. That is, adjustment to the distance necessary to fuse images from the real objects in the actual field of vision will prevent the perception of objects with reflected images from the opposite direction. But adjustment to the apparent objects will prevent the perception of the real objects in front of the eyes, so that the effect is actually to destroy for purposes of actual perception the real world at which we look, and we see the imaginary world through it, as if vision could perform the miracle of the Röntgen rays. In this condition a very curious phenomenon occurs. Let me describe a concrete case.

Suppose yourself facing the direction in which the cars are moving, and the light effects are favorable to your perceiving objects reflecting their images from the left to the window panes to your right, so that you apparently see the objects, say houses, to the right on the landscape or other houses at which you are actually looking. These houses may appear to be variously located to other observers. But for me there is a perfectly uniform action in this respect. If the eyes are adjusted to fuse the images of the houses on my left I see them *through* the landscape or houses toward which I am looking. I may even so concentrate attention as to become absolutely oblivious of the real houses, and the only world that appears to have any reality at all is the imaginary houses or object at which I am apparently looking. Only when I change the adjustment to the images of the real houses or adapt attention to them do I become conscious of them at all, except as vague images in the field of vision. Now what I always notice in the experiment is that the localization of both the real and imaginary world, is definitely related to the different degrees of convergence necessary to determine fusion. The real world, if situated so that this fusion occurs with a greater degree of convergence than the imaginary requires, appears nearer. If not nearer in fact the imaginary world appears nearer. But take the former condition. We have localization proportioned in some way to the degree of convergence, not at which it is at the time, but at which it must be for fusion of images outside the given degree for actual fusion. Thus, although the motor conditions are the same for both perceptions the localization is different. I have called attention in experiments made years ago to this phenomenon. What I wish to note now is a different fact, though it has the same meaning. I have described the binocular effect, and this indicates a difference of perspective for the real and imaginary worlds. But if, while the eyes are adjusted binocularly for the fusion of the images in the imaginary world, I close one eye, the localization, while I remain perfectly still, is the same for both the real and the imaginary worlds. That is, they are seen in the same plane, when monocularly seen. The perspective which is so clearly seen in binocular vision to be different for the different sets of images is now seen to be the same for both. Whether the motor conditions have been changed or not makes no difference in the explanation of the phenomena. If those conditions are altered by closing one eye, and the convergence is probably altered somewhat, the result remains the same. If they are not altered the result also remains the same.

But now, if we move the head from side to side, while keeping one eye closed and using only monocular functions, thus imitating the effects of motion in the cars, we shall notice that the perspective of binocular vision springs at once into effect again. That is, the real and the imaginary worlds seem differently localized and no longer to lie in the same planes. It is to be noticed, again, that there is no difference of motor conditions for the perception in this case. The motion of the head can hardly be appealed to, as this does not ordinarily affect the results of vision. The adjustment of the monocular system remains the same for both the real and imaginary worlds, and yet the difference of localization is as distinct as in binocular perception. We have a parallax of motion, as we may call it, but this represents only the difference in velocity of the images on the field of vision, caused by the movement of the head, while the muscular adjustment for the focusing of the eyes on the objects is not of the kind to account for the difference of perspective, as that muscular condition remains common to the perception of both sets of images. The deciding characteristic is the parallax of motion, and the effect carries no appearances of being a mere inference. It is as distinct and fixed as any form of localization is, where inference is not suspected or believed. The effect thus has every credential of an organic function of some sort whether we choose to regard it as sensory or motor. It represents in all its characteristics a constitutional function. At the same time, as described and as it always occurs with me, I find no such coördination with what I have understood as 'motor' influences that I could treat it as so caused or as so represented in nature.

In the study of the bearing of such phenomena as I have described on the motor theory of such perceptions I wish to call attention to what seems an inherent difficulty in modern theories couched in the term 'motor.' To call space perception or any other product of similar action 'motor' may be clear or obscure according to the understanding of the term 'motor.' In contrast with sensory functions the term originally represented a wholly unconscious process, and so indicated all the difference from the sensory that the unconscious stands for in comparison with the conscious. 'Motor' thus stood for the initiation of muscular action, a function that never is conscious except in deliberate volition and even then only to the extent of the fiat which finally realizes itself in motion. In the application of the term 'sensory' thus contrasted we were to understand the actual facts of consciousness connected with our primary experiences. Thus the 'sensory' was definitely what we knew and experienced in sense perception. The 'motor' was what we did not know, but what we inferred took place in the central system somewhere. It was of an impulsive character and accounted for the various movements of the organism whether consciously or unconsciously initiated. Now in our modern enthusiasm about 'motor' explanations for various phenomena we constantly forget two things. First, the fact that 'motor' and 'sensory' come to us with an implied contrast in their relation to consciousness, so that an appeal to the 'motor' invariably suggests an unconscious fact in the result which we are explaining. Secondly, the fact that we do not adequately distinguish between this traditional use of the term 'motor' to denote the unconscious agencies connected with the muscular system, and the application of the term to denote the 'consciousness of motion,' which is a sensory fact and not 'motor' in the old sense of the term. This modern use becomes identical with the 'sensory' and not contrasted with it. Consequently there is little excuse for the controversies which have centered about the various problems of space perception or any other question associated with the so-called 'motor' activities. 'Motor' to describe an activity or function wholly below consciousness is one thing, to describe a fact of which we are conscious is another and very different thing separated from the former by all the diameter of a contradiction.

To make our problem concrete. In the traditional conception of the term, to call the perception of space, or the third dimension, a 'motor' phenomenon would be to deny its sensory character and to take it out of the field of consciousness altogether. Here was where Berkeley created difficulties in his theory. The observer never detected any muscular sensations in his visual act except when straining the eyes and this indicated no apparent effect on space perception. All that he could do was to say or imply that the 'sensations' were unconscious, which was to take the phenomena outside the field of legitimate study. But for us to say that such perception is a 'motor' phenomenon and mean by it that it is either constituted by or caused in connection with the sensations of motion, is to conceive the phenomenon as a sensory one and not to contrast it with the sensory at all. The controversies which have centered about the problem have been caused by the abandonment of the original meaning of the term 'motor' while retaining its opposition to the sensory after that change This is a frequent vice of our psychologists and it leads to both their misunderstanding of the problem as conceived in relation to historical questions and their being misunderstood by others. What we want is a clearer definition of what we are talking about. I have not yet seen any such careful account of terms with a view to avoiding misconception and controversy, and I know no term more in need of careful explication than the term 'motor.'

With the differences of meaning attached to it, I think the reader can anticipate the solution to the issue about the 'motor' perception of space. If we mean by the 'motor' theory that the function determining space perception is of the order which initiates muscular action, we have an impossibility in the simultaneous localization of different objects in different planes. The 'motor' or muscular functions are seen, in such phenomena as I have described, to be the same or common, but the localization different. The localization varies when the 'motor' action does not, so far as that is conceived in the mechanical terms for which muscular action stands. The 'motor' theory thus seems to have an irrational application. But, on the other hand, if we mean sensations of motion when we say 'motor' and refer space perception to these as its coincidental factor, there will be less difficulty in accepting the doctrine. It is quite apparent in the experiments which I have described that the sensations of motion are the variants with the differences of effects and we may well attribute this difference of effect to the action of those conditions which discover the existence of motional effects in consciousness as the varying factors in the phenomena to be explained. But if we are to import into the issue the implications of contrast with the sensory which the term 'motor' so often has, we only introduce confusion into the problem. We have in fact a case where, when that contrast is excluded, there is identity with the sensory theory at large instead of opposition, so that all the controversies connected with the matter are largely the proverbial dispute about words, except that the illusion is not wholly verbal.

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DR. BOSANQUET ON IMITATION AND SELECTIVE THINKING.

In my note in the November number of the REVIEW, discussing Dr. Bosanquet's criticism, I expressed the intention of continuing the topic — especially with reference to his criticism of the theory of selective thinking. The criticism in question occurs in his discussion — which was taken up for reply in my earlier note — in the PSYCHOLOGICAL REVIEW for July. Having in mind the general considerations advanced, as to the need of a thoroughgoing application of the genetic method, I make the following points, dealing rather with the adequacy of the theory of selective thinking, the necessity of some such genetic account being assumed.

On p. 386 of his article, Dr. Bosanquet makes the criticism of

[1] A continuation of the discussion in the November REVIEW.

the theory of selective thinking which is embodied in the following words: "The prerogative of reason really is to ascertain without trial and error; that is, without actual working upon things, what adaptation is workable in a given situation. I will take calculation as a very simple instance. When I have calculated how many trees are needed to plant an acre of ground at a given distance apart, the adaptation is fully determinate. There is no room for variation or for selection. Selection of 'actions which work in a given situation' * * * omits the essence of calculation and inference, in short of reasoning * * *. The actions which work, workable combinations, will include the results of theories which work, that is which agree with experience in any form, practicable or cognitive."

This line of criticism is aimed — as I take it — at all views which would attempt to prove that in any act of thought the entire process of organization is one of trial and error; that is, against any view which holds that there is no logical process, or process of inference, in the case of organization in question, which is not then and there due to selection. Such a theory is far from my meaning. In each case of progressive organization — and it is such organization which I have called 'systematic determination,' to which the theory of selective thinking addresses itself — there is the established 'platform' of accomplished organization, and with it the further incorporation of new items or thought-elements into the system. The theory is limited, as far as its application to a particular case is concerned, to the process by which these new elements are secured and assimilated. It leaves open, on the other hand, the question as to the nature and origin of the accomplished platform of organization upon which the process in question proceeds. If we for the moment admit that the incorporation of new elements in the system can only be by a process of thinking which is in some sense 'selective,' we should then have left over the question as to those cases of logical process in which there are no new accretions to knowledge, but simply — as in a case of calculation — the running-through of processes of analysis, substitution, etc., according to normal mental rules.

When we approach this question, there are certain broad and general distinctions to be made. We may ask, first, as to the empirical or genetic growth of these mental rules in the individual — a question which brings up the issue as to whether these modes of organization do not themselves reflect earlier processes of selective thinking, which have become, through association, synergy, etc., self-repeating and in a logical sense analytic. The latter term, 'analytic,' has been used by Wundt and others to mark the distinction which I am here making (compare the article by Urban in the PSYCHOLOGICAL REVIEW, July, 1897). We may, after adequate investigation, come to the conclusion that the entire furniture of logical procedure in the individual reflects the continued action of selective processes in his individual growth. Or, on the other hand, we may reach the conclusion that there are certain constitutional modes of organization and rules of mental procedure (*i. e.*, Dr. Bosanquet's teleological processes) which are given or implicit in all cognitive experiences as such. In this latter case, the question arises: what shall we say of this saving remnant of logical process, which, in terms of the conclusion just supposed, cannot be accounted for as due to any sort of individual selection?

Having now put these alternatives, as I conceive, in the form most favorable to Dr. Bosanquet's contention, I may say that in my personal view this last conclusion is the correct one so far as individual thinking is concerned. We have, first, a large body of empirical or derived knowledge systems which are due to gradual accretion through the process of thinking in some sense selective; second, we have, at the same time, systematic determinations of knowledge in which, along with this empirical process of selection, certain organizing rules are operative which themselves have not been formulated through the selective experiences of the individual; and third, that there are certain normal processes in a large sense analytical (calculation, inference of certain types, axiomatic argumentation, etc.), which, whatever be their origin, are in substantial independence of the particular character of the contents of experience which are organized in and by them. These last mentioned elements in our knowledge, I prefer to call 'constitutional' rather than to use a more logical or metaphysical term. They represent the constitution of the thinker, and must be set down as in each case seeming to be given in the knowledge processes in which they show themselves.

The criticism of Dr. Bosanquet simply charges upon the theory of selective thinking the failure to account for these last mentioned aspects of organization. So far as knowledge in general is concerned, I fully admit this criticism as applicable to the processes of the individual thinker. But it will be observed that the topic which we are discussing, that of the book on 'Social Interpretations,' is *social organization*. Now, social organization is, to my mind, a typical case of progressive determination by gradual accretions of content to the individual's experience. Its most general mode, the self-thought, is a growing empirical content.[1] The process of social organization which

[1] It is a question, however, whether the categories of unity and identity which characterize the self-thought are not constitutional.

the individual goes through is, in the terms of my theory, tentative imitation with equally tentative selection. But this is empirical process through and through; it is a case in which one would make least reservation in regard to the adequacy of the selection theory in view of the presence of analytical logical processes. Dr. Bosanquet's criticism, therefore, so far as it brings out the inapplicability of the selection theory to such logical processes as calculation and deductive inference, does not apply to the application of it which I make when considering social organization.

I have, therefore, to admit that, in this particular case, he is quite right in saying that, on my view, 'actions which work, workable combinations, will include the results of theories which work, that is, which agree with experience in any form, practical or cognitive.' For all thinking, whether social or not, which involves confirmation — whether it be by experimentation, by simple observation, by induction, etc. — all such theories, so far as they issue in progressive or synthetic determination of knowledge, fall in the class which the selective thinking theory aims to cover.[1]

It is quite true, however, that in the address on Selective Thinking ('Development and Evolution,' Chapter XVII.), the theory is stated as applicable to cognitive process in general, and consequently, apart from the merits of the theory as applied to the case of social organization, it must defend itself with reference to the larger issue which is raised under the second and third heads mentioned above; that is, we should still have to ask whether the established platform, or system of organization which precedes the incorporation of new selective elements, is itself the work of still earlier selective processes, and then we should have to go on and ask whether the residue of process, which seems to be purely analytical and deductive, can, in any sense, be brought under the selection view. So Dr. Bosanquet is quite right in asking for an interpretation of the sentence, "thoughts are the counterparts of former adapted actions" (p. 387), which he quotes from the address mentioned.

[1] That the general position here taken is essentially the same as that of the first edition of the 'Social Interpretations' (written before the address on Selective Thinking) may be seen from the following quotation from that work (1st ed., p. 122; 3d ed., 131): "The general conclusion is, therefore, that there is a great sphere of truth, of selective thinking, of inventions judged true, of mental constructions believed, in which the criterion of selection is all along availability for imitative social assimilation in the growth of the thought of self; and unless in some spheres we be able to find other compelling criteria of truth, we shall have to say the same of all selective thinking." (And in a footnote—"This last expresses the probability, in my personal view.")

Interpretating the sentence — as he does quite properly — as meaning "thought is a phase of adapted principle, carrying on in a new situation the idea which gave a successful conclusion before," he criticises it, saying: "But in this case thought depends upon previous thought of the same kind; the variation becomes predetermined; selection vanishes and with it the prerogative of actual working of things as a test. For all thought whatever is a principle at work in the sense in which calculated action is so, and whether the conditions to be met are materially present, or only present in knowledge, can involve no differences of any kind to the nerve of the process. It is only in trial and error that practice on material things has a prerogative over inference. When we come to calculation and reasoning, the especial import of material action disappears and it becomes one among many forms of cognition, all of which are tested by working in the wider sense." And he actually draws the conclusion in the paragraph which follows, that in all thinking — such as in deciding upon the plan for a new house — we should have to actually perform the experiment; that is, 'to be consistent, you should try the combination in actual bricks and mortar.'

My reply to this is in general that it fails to be in any thorough sense genetic. It fails to allow that past acquisitions, which have themselves been selected, may be so fixed and habitual that the selective processes which they originally embodied are obscured. The realization of a principle at work — to use Dr. Bosanquet's phrase — may be through a series of selective accommodations, and the result at each stage may appear to be a given or original plan of organization when, in fact, it has grown up through a series of selections. In each such case we have to ask, however, what sort of process has actually led to the determination of the principle at the particular stage, and open the question as to whether there is not in the determination a certain amount of 'relative suggestion' due to new selections. We are thus driven back from stage to stage of mental progress, the formal, schematic, or logical ingredient being reduced to its minimum as the elements of the combination are found one by one to have been fixed by actual processes of selection. There is thus, in each case, fusion of what seems to be logical process with its empirically selected content; and we push back the final question to this: What are the original and constitutional modes of organization of our knowledge *when the individual begins his personal career, and which, in the growth of experience, are so and so determined?*

The statement of this problem, in the matter of knowledge, may be illustrated by the latest development of biology in connection with the corresponding problem of the relation of congenital and acquired characters. The old theories were those of Preformism on the one hand, and Epigenesis on the other hand. It was the question in particular cases either of 'congenital characters' or of 'acquired characters,' and one must find one or the other. Recent progress in biology has made it clear that such a thorough-going antithesis is mistaken; that there are no adult congenital characters as such; also, that there are no acquired characters as such; but that all characters include a certain constitutional rudiment or *Anlage*, *whose development is essentially conditioned upon the influences of the environment from the start.* The larger part of the character — whatever that character be — is in most cases a joint product, and the question is that of determining how, in a particular case, the two aspects are conjoined.[1]

This, in my view, is substantially the present state of the question historically known as that between the *a priori* and the empirical in thought. It is no longer an antithesis between two elements set over against each other in the structure of knowledge. It is now a question of *the interpretation of a living organic process which is experience*, *but which is also function.* We have a growing structure or system of knowledge. It grows by selective assimilation of new elements of content. What are the conditions which determine the mode of organization at each stage of its growth? This is the genetic question — the question which the theory of selective thinking attempts to answer. There remains, however, the question as to what is the original nature, reduced to its lowest terms, of the mental function which takes on these modes in a progressive way in actual experience. The answer to this last question gives a theory of the constitution of knowledge — the endowment factor.

Having admitted so much — that the tracing back of the processes of knowledge to their simplest forms lands us in the recognition of some sort of constitutional endowment — one is, of course, not able to shirk the question as to what this endowment means. And anyone who leans toward a thoroughgoing genetic point of view raises the further inquiry as to the racial evolution of endowment. Here the ground becomes more or less familiar when looked at historically, and the two theories in the field are, of course, those of 'race-experi-

[1] Perhaps I may be allowed to refer to my own statement in exposition of the view of the 'Organicists' in 'Development and Evolution,' pp. 34ff.

ence,' on the one hand, and 'variation,' on the other — that is, *if one be an evolutionist at all.* Once accepting the genetic point of view in race-evolution, we find reinstated for the successive stages of racial progress the same question as that discussed above for the individual's progress; namely, how is it that successive forms of mental organization can have been produced in connection with the corresponding evolution of physical forms? And it is a commonplace to say that the selection theory, in the sense of current Darwinism, provides a way of accounting for this racial progress, as the selective theory of thinking aims to provide an account of the individual's progress. All the way through, however, from the human mind back, let us say, to the protozoan mind, the genetic psychologist, like the general biologist, aims to provide a theory of what he sees without being called upon to suggest a philosophy of the nature of the reality whose successive determinations he sees. The philosophical problem for the evolution series, as for the series representing the individual's development, remains over. But whatever its solution be, it should not be allowed to intrude itself upon the genetic psychologist, or upon the biologist, who pursues the problem through both series with view to the most adequate statement of the facts, and formulation of the methods of their organization.[1]

[1] We should, I think, expect that the completed genetic survey would afford most essential data for sound philosophical construction; and there are signs of the recognition of this resource — signs that the genetic method is forcing its way into metaphysics. In the recent 'Outlines of Metaphysics' of Prof. J. S. Mackenzie, we find the explicit declaration of such a platform and intention, but little first-hand psychology, and further a certain helpless deference to the traditions of a school. On the other hand, in the recent work 'Foundations of Knowledge,' by Prof. A. T. Ormond, we must recognize a book of very different character. It is, in my opinion, a very able piece of constructive work, in which the attempt is seriously made, and with understanding of what the attempt really means, to treat of the origin and development of knowledge with view to the interpretations of epistemology and philosophy. (But that it is *caviar* to the philosophers of the classical logical method may be seen in the review of it by Prof. D. G. Ritchie in *Mind*, January 1892, p. 92, who misses the meaning of the book in all its large features, and devotes pages to fretful pedantry over certain misplaced commas, and Greek accents gone astray. This seems, however, to be a chronic ailment of Professor Ritchie, and does not weigh for much in a country which holds Bosanquet, and Bradley, and Ward, and Stout.) Ormond's grappling with the problem of the antithesis between fact and worth, science and value, is a forerunner of the vital constructions of near-by philosophy, and his attempt to exploit the æsthetic category — a lead which, I think, when fully followed out, will yield the profoundest interpretations of experience — must have its due influence on future thought. (Cf. these things with the harmless dead-level of Professor Ritchie's writings, as for example the title essay of his 'Darwin and Hegel').

That such a distinction of problems, as between the provinces of science and philosophy, is necessary, may appear more clearly when we take the point of view of psycho-physics and consider the development of mind and body together. The postulate of psycho-physics is that whatever is going on at any time in consciousness, something is going on at the same time in the brain. As soon as we come to make this position definite, it becomes clear that we must admit the existence of this two-fold process, both in the factors of experience corresponding to content, and in those corresponding to organization or form. It is equally necessary, for example, that we ask both for the physical process in the case of the sensations which are united in the perception of an object, and also for the physical process of *the function of union itself, which constitutes the object as one.* In other words, there must be a scientific side, an empirical side, to the categories of organization, as well as to the material which is organized in the categories. And to say that certain modes of organization, or laws of thought, are constitutional, and in their original character logical, does not in any way exempt them from this sort of scientific construction. For example, suppose we take the case just cited of what we call 'unity' in mental construction of any sort. It is possible to assume — as the older logical psychology did, and as I suppose Dr. Bosanquet would now be disposed to do — that all thinking involves unity as a constructive principle, and that no mental wholes of any sort are possible except through the use of this category. But it is possible, also, while fully admitting the logical importance of this element in actual constructions, to ask for the genetic processes by which the mind maintains unity at any grade of its construction.[1] Is the unity, let us say, of an individual object accompanied, on the physical side, with some sort of general sensory process by which more partial sensational processes are held together? If so, then on the mental side we recognize in the feeling, or more generally the consciousness, of unity, something going along with the possible separate states of consciousness corresponding to the particular processes of sensation involved. This means that we have, in so far, not only determined the physical process underlying mental unity, but we have also characterized *the consciousness of unity* along with the other elements which go into the perceptual psychosis as a whole. Suppose, on the other hand, we believe, as the present writer believes, that the consciousness of unity is, so far as the physical processes are

[1] Of course I mean not merely de facto unity, but *consciousness* of unity — not merely 'psychological,' but '*psychic*' unity.

concerned, not a sensory process but a motor process. Then we have a different genetic account of the mechanism and possibilities of this category throughout all the constructions of knowledge. On this theory, as I hold it, unity is the mental reflection of motor synergy — the unity of more partial and partially adapted motor processes in a larger whole of adjustment or accommodation. This reflects itself in consciousness as what we call the form of 'unity in variety,' or the 'sameness in difference' of which the philosophers of Dr. Bosanquet's school make so much. It gives us what I believe has never been given before, a legitimate genetic and psycho-physical account of the antithesis between the unity of a mental whole and the variety of its detailed contents. Moreover, just in so far as this unity results[1] in a prospective motor adjustment, by which particular more detailed adaptations become effective in the treatment of experience, just in so far the whole is reflected in consciousness as what we call teleological — as realizing an end. We thus have, from the point of view of science and of the actual processes involved, a genetic account of the teleological wholes which Dr. Bosanquet emphasizes in his criticism of the theory of selective thinking.[2] Apart altogether from the question of adequacy of such an account — a point which cannot be argued in detail in this brief paper — what I should say is that such an account, whatever its detail, in nowise contradicts or interferes with the teleological interpretation of the movement of thought, as Dr. Bosanquet and Dr. Stout depict that movement; and this may be taken, I think, as an example of the sort of supplementary treatment which logical psychology should anticipate and welcome from genetic psychology.

It is astonishing to me, after what I have been accustomed to consider the thoroughgoing reconciliation of logical and genetic points of view in the work of Hegel, that thinkers whose views derive so much inspiration from him should not be able to accept this position. Many of the disciples of Hegel, and indeed also of Aristotle among the moderns, are not as thoroughly genetic as were their masters. There is growing up to-day, however, a new sense of the importance of the problem which is really at the bottom of our differences of opinion here — the problem as to how, if we give naturalistic science its full

[1] I say 'results' because it *is*, as a mental construction or ob ect, a result even *before* active realization — it is a synergy of revived processes.

[2] I do not see how the fact that the details are altered in the new organization affects the validity of the theory, except on the doctrine of extreme atomistic association. The larger whole of motor processes is an essential reorganization, and we should expect the partial contents to be liable to paring-down and reducing changes.

rights, we can still conserve the newer modes and values which arise as the material of science is progressively organized in more advanced and complex stages. How, in short — to modify the famous question of Kant — *how is experience of new reality possible?* —that is, how can experience go forward by successive applications of certain categories and principles, and issue in *essentially novel modes?*

This, I take it, is one of the two burning questions which genetic science is making it possible and most urgent to ask.[1] And I think if we are to have a philosophy of the sort hinted at above, which is based on genetic no less than quantitative science, and includes the formulas of both, we must somehow answer this question before we can proceed very far. Much fear of that bug-bear called 'naturalism' — especially in England — arises here. It proceeds by the definition of naturalism as a view which recognizes only mechanical interaction, and it then tests the right of science to interpret all forms of organization on the basis of such a naturalism. Now, apart altogether from the criticism of this use of the term naturalism — a use of the term in my opinion quite unjustified — we may say that even though such a naturalism carry its interpretations through the whole range of phenomena, nevertheless there still remain other possible interpretations, and among them the genetic, of the same or other aspects of the same phenomena. This is too large a question to discuss in a page, and I have sketched in the recent volume, 'Development and Evolution,' Part III., what is there called the theory of 'genetic modes,' as a tentative line of direction in approaching this problem profitably.[2] If we go to the

[1] The other is that of the interpretation or meaning of the entire genetic series as dealt with by science.

[2] The following quotations may serve to show what I believe to be the correct relation between the two interpretations of fact which are represented respectively by quantitative science, on the one hand, and that sort of science, on the other hand, called genetic, which recognizes the production of new synthetic modes or phases of reality. "How then can there be any such thing as a phase of reality which is not subject to plain statement under natural law? This question represents a very common criticism of all thoroughgoing statements of mental evolution. It rests on the mistaken view that a statement of the historical career of a thing can ever be an adequate statement of its nature; in other words, that the origin of the categories of thought can tell what these categories will do—what their function and meaning is in the general movement of reality. * * * The writer has developed a view of mental development, which not only makes each state of it a matter of legitimate natural history, but goes on to say that the one process of motor adjustment is imitative in type. What could be a more inviting field for the criticism: imitation is mere repetition [so Dr. Bosanquet charges upon my view]; how can anything new come out of imitation? Not only is consciousness merely repeating the relationships

bottom, I think we can 'hold up' nowhere short of the position of certain recent writers (for example, Münsterberg) who hold that the right of naturalism to the treatment of every describable thing, must be admitted, but, at the same time, the equal right of a science which deals with other modes of organization, with meanings and values, over the whole field, is equally to be admitted; and that the problem

already present in nature, but the development of consciousness itself is merely a series of repetitions of its own acts. * * * The counter question may be put: Why cannot anything new come out of imitations? Why may not the very repetition be the new thing, or the condition of it? To deny it is to say that by looking at the former instance, the historical, after its occurrence, you can say that that occurrence fully expresses mental behavior. On the contrary, the prospective reference gained by the imitation may bring out something new; the repetition may be just what is needed to develop an important stage in the career of mental reality. In itself, indeed, an imitation is no more open to the objection we are considering, than any other kind of mental behavior, and it is not allowed that imitation is no more than repetition — though, of course, in certain cases it may be no more. * * * It does not follow that because a product — one of the categories of organization, such as design, the ethical, etc. — is itself a matter of gradual growth, its application to reality is in any way invalidated. * * * The fallacy is just the assumption that reality is finished; that categories of retrospective reference exhaust the case; that the series of events, which are sufficient ground for the origin of the category, might also be sufficient evidence of its validity. That there is a sharp contradiction, therefore, between a doctrine of derivation from experience and application beyond experience" (*Development and Evolution*, pp. 283–6).

"I contend that absolutely new and unheard phases of reality may 'arise and shine' at any moment *in any natural series of events — constituting new genetic modes.* Considering the origin and nature of the categories of thought, whatever our theory of the method of their genesis may be, we find that they are modes of function selected for their utility as furnishing interpretations of experience. It is evident then that it is impossible to discount or deny by their use any modes of existence or reality *which they do not interpret.* * * * The very origin of the categories which we use in science, restricts their application, since there may be other types of experience which are so far untouched and which might be construed only under other categories" (*ibid.*, pp. 305–306).

"We cope with the new by a tentative outreach toward it, armed with our categories of description and historical interpretation. In so far as these are adequate, they reflect earlier stages in the unfolding of the same system. But the 'arming' is inadequate for full interpretation, since the arms are forged in the fires of the past. The ideals, the values yet in process and always to be in process of achievement, have their impelling power from the very experience that knowledge and life are functions of a genetic process of which our formulated realities are passing phases. This might be carried out in a philosophical view of reality — a theoretical doctrine of metaphysics — but that is not my intention here. The only safe course for science, however, is to recognize these things. Genetic science is competent to make the reservation always, in the presence of each of the applications and explanations of exact and numerof philosophy — of the metaphysics of the future — is that of finding some final mode of interpretation in which these two world-comprehensive points of view are to be finally reconciled and harmonized.[1]

In the preceding remarks, I have gone a good way afield from Dr. Bosanquet's more detailed criticisms of the theory of selective thinking; but when we come now with these general remarks in mind to take up the points which he makes, I think we shall find that each of them illustrates a phase of this larger issue. For example, on pp. 387–88, he speaks of the teleological aspect of a whole of mental construction as in some way contradicting or invalidating the process of construction given by the theory of selective thinking. I should say, not so. On the contrary, I think that a whole of thought which illustrates and accomplishes a mental end is not only not exempt from the requirement of being psychophysical and phenomenal, but that its only possible realization in experience is through such processes — through traceable genetic processes. We must ask for the genetic side of the constitution of a teleological whole. Furthermore, to cite another instance, he asks: 'Why is material action an excellent test of thoughts?' And his answer is: "Because, in assignable respects, it is a very strong case of thinking with verification, but if you make thought borrow its unity and consistency, having none in itself, from what is found to succeed in action, then I no longer see why we should respect these qualities or treat them as guides to truth or reality. Action no longer reveals a principle that pervades experience." On the contrary, I should say that this completely inverts the requirements of genetic science in favor of a philosophical assumption. If a 'principle that pervades ex-

ical science, that *it is a cross section, not a longitudinal section, to which the quantitative and analytical formulas apply;* or that if they apply throughout a serial process — as in a series of successive transformations of energy — *that* is proof that the process in that case *is not a genetic one.* It is the genetic aspect, we must hold in such cases, which has escaped the formula. The success of the quantitative and analytical method is itself the evidence that no really genetic movement has occurred; in other words, that only those aspects of things have been observed which illustrate the repetitions, not the adaptations, of nature" (*ibid.*, pp. 307–308).

And in stating positively what is called a 'postulate' of the theory of genetic modes, this formula is advanced: "That series of events only is truly genetic which cannot be construed before it has happened, and which cannot be exhausted by reading backwards after it has happened" (*ibid.*, p. 311). But it is maintained that while such a series cannot be *exhausted* by reading backwards, it can, nevertheless, always be *interpreted* by such a process of reading in the categories of exact science.

[1] Personally, I think the æsthetic category goes further in covering the 'multitude of sins' of naturalism than any other!

perience' is not to be revealed through conscious process, how short of a miracle can it be revealed at all? If it is to be revealed, it must be in interpretations or generalizations of actual empirical processes. Now I do not say that thought 'borrows its unit and consistency from action, having none in itself.' This would commit me to a dualism so essentially logical that, *me judice*, science should have none of it. On the contrary, I should say that the unity of 'thought in itself,' *is* the conscious side of the unity or synergy of material actions—that so far from expecting unity and consistency to serve as guides to truth or reality, on the contrary what we mean by truth or reality is just the presence of the unity and consistency of material action. The logical dualism between reality and the thought construction which is supposed to lead to it is mythical. This question is discussed in the address on 'Selective Thinking' and it is also treated with similar results by Simmel in his paper, quoted there. It is one of the results of a genetic theory of thinking that these dualisms between hypostatized realities on the one hand, and mental processes logically divorced from such realities on the other hand, are once for all disposed of. If some mental experience is to 'function as universal,' to use Dr. Bosanquet's own expression, why should it not be a mental experience which has for its physical counterpart the synergy of adapted action? This illustrates the possibility, as said in the broader arguments above, not only of finding genetic processes accompanying the abstract and universal ingredients of our knowledge, but of finding ourselves also compelled to readjust our earlier logical distinctions in view of the results of researches in psychological genesis.

One further word: Why do we need 'thought-in-itself'? Is it not a sort of superstition that 'thought-in-itself' is better than thought in action, or thought in an empirical or genetic series of some kind. Is not to require it really to make a judgment of value, a *Werthurtheil*, to express a preference, and then to refuse to be thoroughgoing in one's science for fear in the end this philosophical judgment of preference may lose its prestige?

In conclusion, I may express the hope that Dr. Bosanquet will give us his further mind on certain of these matters; particularly should I, for one, value his opinion as to the scope of genetic psychology, and also as to the treatment of the successive grades of organization which I have called 'genetic modes.'

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A SOUND PERIMETER.

Recent studies in auditory space perception have shown that the power to localize sounds rests, to a great extent, upon secondary factors. What unaided introspection would lead us to consider direct acoustic sensory data, exact experiment often reveals to be only associations or the result of subconscious influences of some sort. In future experiments more attention must be paid to the elimination or control of these associations and suggestions. Within the last few years, much good work has been done in the study of the localization of sound, but all with crude and often inadequate apparatus. None of the sound cages, or substitutes for the same, which have been used, could have been operated without giving suggestions that would tend to invalidate the results. Only those who, like the writer, have been engaged in these experiments, can fully appreciate this criticism. Results have been obtained at the expense of wasted time and patience in the effort to conduct the experiments on such plans that the shortcomings of the apparatus might be overcome.

In order to be adequate for most purposes, the apparatus for the producing and registering of the sound which is be to located should permit, among others, the following variations in the stimulus without giving any suggestion or counter-suggestion to the observer: (1) the direction of the stimuli from the middle of the aural axis, (2) the intensity of each of the stimuli, (3) the distance of one stimulus, (4) the number of stimuli to be given simultaneously or in succession, and (5) the order and frequency of stimuli from a given position.

The sound perimeter shown in Fig. 1, has been designed to meet these requirements. It consists of a system of telephone receivers so mounted and connected as to make the above-named variations possible. The main frame is made of iron tubing and braced in such a way as to afford the maximum rigidity with a minimum of material which might reflect sound. The receivers through which the stimuli are produced, are mounted on movable arms, which may be denoted as *A*, *B*, *C*, and *D*, respectively. Arms *A* and *B*, each representing an arc of 135° of a circle whose radius is one meter, are so mounted on a common center at the top that they may swing in the same course, describing a part of the surface of a sphere one meter in radius. Each of these arms carries a pointer, which moves under the circular scale placed above the bearings. This scale is graduated in five-degree units and marked with large figures, which may be read from the experimenter's position behind the tablet on the main support of the frame. The two arms are mounted on a common axis, but they turn on independent bearings, so that there is no friction between

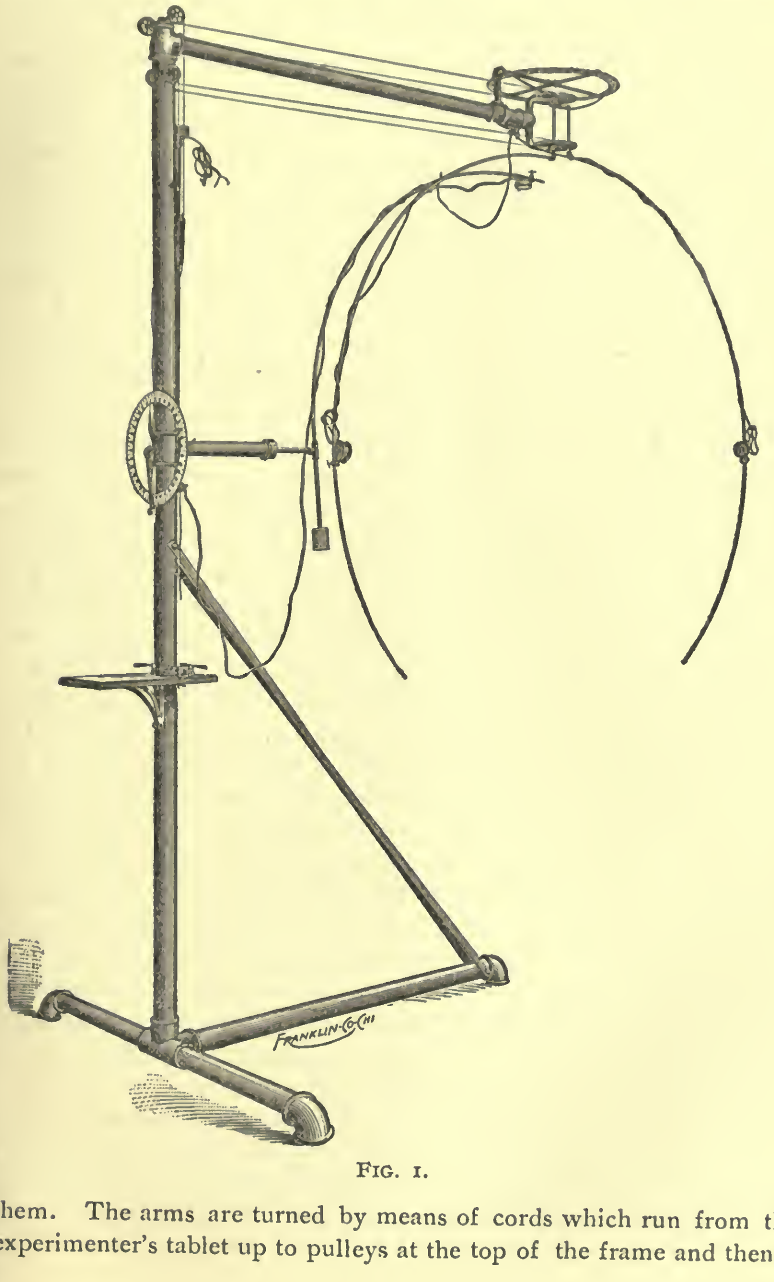

FIG. 1.

them. The arms are turned by means of cords which run from the experimenter's tablet up to pulleys at the top of the frame and thence.

to wheels mounted on the upward projections of the arms. There are two of these cords for each arm; pulling one cord turns the arm to the left, and pulling the other turns it in the opposite direction.

The third arm, *C*, turns in the surface of the same sphere as the other two arms, but is mounted on the side and counterbalanced, so that it may be turned readily by means of the crank which is seen directly above the tablet. The pointer on the crank runs over a circular scale which is graduated in five-degree units, in the same manner as the scale for arms *A* and *B*. The axle which carries this arm may be drawn back through the frame so that the arm may pass the other two arms without striking at the top, and so as to be out of the way when not in use.

Arm *C* may be removed by pulling the axle out after detaching the crank, and arm *D*, a straight rod, will fit in its place of support. In Fig. 1, arm *D* is seen only in part, being stored away on the side of the main upright of the frame. This arm carries the receiver on one end, and is graduated in centimeters for guidance in the adjustment of the distance of the receiver from the center of the sphere. The arm is three meters long and slides freely in the horizontal direction.

For ordinary purposes only one receiver is needed on each arm, but it is evident that any number of receivers desired may be mounted on each arm for the purpose of special experiments. The receivers are clamped by a thumb screw and may be placed in any position on the arms. In order to eliminate conduction along the arms, the receivers are insulated from their clamps by means of soft rubber.

Soft and flexible wires run from the receivers to terminals on the surface of the frame and permanent wires are laid from these terminals, inside of the frame, to the tablet. The same circuit is used for arms *C* and *D*, as they are never used simultaneously. There is a knife switch for each circuit on the tablet and all the circuits are completed through the same battery and mercury key (not shown in the cut). Thus, when the key is pressed, a click will be heard in a receiver if the switch in its circuit is closed; and if two or three switches remain closed at the same time, the current is distributed equally to the corresponding receivers and the clicks will occur simultaneously in all.

Resistance may be put in the main circuit or in one or more of the branch circuits, as the needs may be, to vary the intensity of the click. If a dry-battery is used it may be fastened to the frame and then the apparatus will be complete without any further accessories.

To vary the quality of the stimulus, tones of different pitch may be substituted for the click. For that purpose it is necessary to have electric tuning-forks of the desired pitch in a distant room and to complete the perimeter circuit as a shunt through the fork. The tone will then be heard in the receiver whenever the key is pressed.

The center of the sphere described by the arms is 1.73 meters above the floor. A high, adjustable stool is placed under this center and adjusted for the observer so that the center of the observer's head occupies the center of the sphere. If a head rest is used great precautions should be taken to prevent disturbing effects. It is best not to use any head rest, but to check the position of the observer frequently by putting arms *A* and *B* at opposite points and sighting across. The height is determined by reference to the axis of arm *C* or arm *D*.

The scheme for numbering the points on the scale is of considerable importance. That plan has been adopted which students tend to follow spontaneously when asked to describe the location of a point in space. In this there is no number higher than 90. The upper scale gives the reading for horizontal planes and the side scale for vertical planes. The nomenclature adopted may be described without any diagram. The horizontal scale has two zero-points, one in the median plane in front and the other in the median plane behind; *i. e.*, every point in the median plane of the head is at 0° with reference to the horizontal plane of space, and degrees are counted toward the right and toward the left from the median plane both in front and behind. In the vertical scale, the two zero-points are at the level of the ears; *i. e.*, every point in the horizontal plane through the ears is at 0°, and degrees are counted upward and downward from this level. This gives a simple and natural nomenclature for direction, *e. g.*, a point is 'in front, 15° left and 25° up.' The upper scale may be turned so that this system will correspond to any desired position of the observer.

This apparatus will favor the use of the method of right and wrong cases and the method of minimal changes, in which it is not necessary for the observer to estimate degrees. However, it is sometimes advantagous to allow the observer to indicate the direction with a pointer; the experimenter may then swing the perimeter arms to such a point and read off the result on the scales.

This brief statement, supplemented by the figure, may suffice to give a general idea of the apparatus. Its special merits are, that it enables the experimenter to stand in one place throughout complicated series of experiments and operate all the parts of the apparatus without giving any suggestion by movement or delay, that the movable parts of the apparatus are made to act without sound or jar, and that it makes it possible to vary, measure, and control the essential factors.

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PSYCHOLOGICAL LITERATURE.

The Founder of Mormonism. A Psychological Study of Joseph Smith, Jr. I. WOODBRIDGE RILEY, with an Introduction by Professor G. T. LADD. New York, Dodd, Mead & Co., 1902. 8vo, pp. 446.

The attention of psychologists should be drawn to this study of the originator of a curious and influential religious movement still operative in the United States. The volume is the Doctor's dissertation (Yale) of a careful student, equally conversant with the literary, historical and psychological aspects of the topic. The story is an extremely complicated one, and many of the essential facts are most difficult to discover. As the result of a painstaking search through a great mass of contemporary records, Dr. Riley has succeeded in presenting a consistent—though inevitably in parts a conjectural—reconstruction of the sources and nature of the writings and doings of Joseph Smith, Jr. The keynote of the explanation is a psychological one; that Smith was a neurotic, with distinctive epileptoid symptoms, given to the seeing of visions, steeped in a superstitious form of narrow religious dogma; that his associates and first converts were precisely of that easily suggestible type whose lack of education and religious fanaticism would make them credulous believers of such pretences to revelation and divine authority as Joseph Smith put forth; that the 'Book of Mormon' and many of the revelations that formed the early history of the Church of Latter Day Saints were dictated by the author, Joseph Smith, while the latter was in a quasi-hypnotic condition self-induced — the writings and the account of their production bearing evidences of automatic writing and of the processes observable in 'crystal-gazing.' The force of this argument can be appreciated only in its cumulative mass, and that requires a fair-sized volume to set it forth. The story is confessedly an imperfect one; and further research is not likely to fill in the regrettable gaps in the evidence; still, the internal circumstantial evidence distinctly contributes to the acceptability of the theory which Dr. Riley maintains. Analogical reasoning occupies an important place in the argument, and resemblances to recognized results of the same forms of individual and collective psychoses as those involved in the origin of Mormonism, are constantly cited. It is somewhat surprising that Dr. Riley's diligent use of recent psychological materials has not brought to his attention the case of the 'revelator' of 'From India to the Planet Mars,' so carefully detailed by Professor Flournoy. In both cases we have the same fantastic explorations of the imagination; the same assumptions of narrative, incidents and style apparently foreign to the subject's normal intelligence; the same invention of fictitious names, persons, places and things; the same possibility of tracing many of the incidents and details of the document to authentic experiences of the subject, but experiences which may have been subconsciously realized and are certainly recorded while in an abnormal state of dissociation; the same periods of incubation preliminary to the further development of a new stage in the automatic revelations; the same participation of the suggestions of others and of the clever adaptation to actual circumstances and incidents, in the subsequent revelations; and so on. The striking differences between the two cases are external and not psychological. In the one case, the revelations are given out as real and inspired, find a congenial soil in which to flourish, and so attain practical significance. In the other case they remain the purely personal expression of a luxurious imagination. Undoubtedly the primitive state of the psychology of abnormal mental states at the time of the miraculous discovery and transcription of the 'golden plates' of the book of Mormon had something to do with the ease with which such a propagandum was disseminated. But we must remember that the kinds of persons who would become converts to such systems are not likely in any community to regulate their beliefs by scientific principles. Recent movements of comparable type in very differently situated communities have revealed the widespread existence of the proper psychological soil for the flourishing of similar delusions. The comparative study of such movements, most of them with a religious tendency, is likely to contribute to their understanding. Dr. Riley's presentation of Mormonism's founder is a notable addition to the collection of psychological documents of this type. J. J.

Development and Evolution. JAMES MARK BALDWIN. New York, The Macmillan Company, 1902. Pp. xvi + 395.

In this volume Professor Baldwin undertakes, from a decidedly genetic point of view, the discussion of a series of problems of vital interest both to the psychologist and the biologist. The result is a work in which thinkers in both of these subjects will find much food for reflection and investigation. Though written by a psychologist, the work may be said to have even greater value for the biologist than for the student of mental phenomena, as disclosing the attitude of recent psychology towards biology and the limitations both inherent in the subject and of the biologist's own making.

The work consists of a number of articles that have already been published in various journals since 1895, worked into a whole with considerable material not hitherto published. All of the chapters bear more or less directly on the subject of organic selection and orthoplasy. For the purpose of making the treatment of these subjects as complete as possible, the various articles of Osborn, Lloyd Morgan, Poulton, Headley and Conn, bearing on the same matter, have been reprinted in a series of appendices. Although this involves a good deal of repetition, as the author admits in the preface, the great advantages of the method are apparent. In these days of scattered publication every psychologist and biologist will be glad to find all of this material in such accessible form. The work thus becomes, as the author claims, a sort of handbook of the theory of orthoplasy.

The work is divided into three parts, which deal respectively with 'The Problem of Genesis,' 'The Method of Evolution,' and 'Criticism and Interpretation.' In the first part we find some valuable definitions of terms, interesting discussions of psychophysical parallelism and of certain conceptions of moment in the comparison of mental and biological phenomena, and a criticism of the theory of preformism from the standpoint of accommodations.

The terms 'development' and 'evolution' are employed in the title and throughout the book in the sense of 'ontogenetic' and 'phylogenetic' development respectively. Mental phenomena as viewed subjectively are called 'psychic,' as viewed objectively 'psychological.' A series of terms in 'nomic' (bionomic, psychonomic, socionomic) are introduced to designate the agencies which condition biological, psychological and sociological phenomena. It is stated that the term 'bionomic' gave the cue to establishing this distinction and the other terms above mentioned.[1]

[1] In a recent article in *Science* the present writer advocated the use of the term 'ethological' in the place of 'bionomic,' although no reference was made to the latter term because he desired to leave the botanists in possession of the term 'œcology,' and because he wished to have a better term for the use of zoologists. The following reasons may be assigned for avoiding the use of the term 'bionomics': First, there is absolutely no reason to suppose that the botanists will use the term 'bionomics' as the term 'œcology' is thoroughly established in their science. Second, while 'taxonomy' and 'taxonomics' mean the same thing, 'bionomy' and 'bionomics' do not, at least according to the

In his discussion of psychophysical parallelism Professor Baldwin recognizes two leading principles: "First, the principle of *equal continuity*, to the effect that there can be no breaks in either series of changes, the brain changes or the conscious changes, without a corresponding break in the other; in other words, if one of the series be continuous, the other must be continuous also." "Second, the principle of *uniformity*, to the effect that the sort of modifications which are associated one with another in brain and mind are always the same; that is, if a certain brain process be correctly hit upon as essentially associated with a certain conscious state, then the concomitance of these two terms may be looked for on all other occasions of the occurrence of either of them. This formulation is, it is easy to see, absolutely necessary to any science of psychophysics at all." A discussion of the theories of heredity is adroitly avoided and is, indeed, quite unnecessary to the argument. "The question of the 'continuity of germ plasm' may be decided one way or the other — either for or against the actual transmission of an identical substance — without raising the question of a corresponding transmission of anything psychological. For if there be breaks in the psychological series at those nodal points at which generation succeeds generation, there are also, by the principle of equal continuity, discussed above, breaks in the psychophysical, and we may find the psychological series beginning again at the appropriate point in the development of the organism of the new generatian — the point at which the psychophysical again begins. In other words, the advantage gained from the psychophysical point of view is that if there be no gaps in one of the series,

dictionaries, and there is a chance for confusion here. Third, 'bionomics,' in the sense of 'ethology,' is hardly used outside of England, and even in that country it does not seem to be in very general favor. Thus Dr. Bather and Dr. Sharp inform the writer that the Royal Society recommends the use of 'ethology' as the proper heading for works on habits, etc., in the 'International Catalogue of Scientific Literature,' and the latter gentleman calls attention to the use of this term for some years past in the 'Zoological Record,' and expresses the opinion that it will be generally adopted. Fourth, 'bionomics,' in the sense in which it is used by Professor Baldwin, emphasizes the environmental factor in adaptations, whereas 'ethology' emphasizes the side of the organism, the 'true set of forces whose play is conditioned' by the environment. This is no unimportant distinction in these times of piping physical and chemical dogmatism in biological science. Fifth, the uniform use of 'nomic,' as suggested by Professor Baldwin, breaks down in the well-established term 'taxonomic,' which does not refer to anything conditioning something else (unless we distort Professor Baldwin's meaning in this case to imply that the structures of organisms condition their position in the classification) but is used in the sense of the word *νομος* in 'bionomy.'

we may either assume them filled up by theoretical parallelism with the other series at these points, at which it has no gaps, or we may — if we deny continuity to either — make gaps in the second series in correspondence with the gaps found in the first. We have in any case, in short, either a psychophysical fact, or we have not: if we have, then either series is sufficient to carry us over the critical point; if we have not, then the break in one series is sufficient evidence of a corresponding break in the other also. The principle of parallelism assumed, we claim once for all the right *to neglect the relation of the two terms, mental and physical, in all circumstances whatsoever.*" Thus the same set of formulas will cover both sets of facts, and we are at liberty to use "the mental facts as antecedents of the physical facts, often the physical as antecedent of the mental, and again, often the psychophysical as antecedent of either or both."

The second chapter, on 'Comparative Conceptions,' shows that Professor Baldwin has none of the squeamishness of many psychologists about adopting a thoroughgoing genetic point of view in psychology as well as biology. In previous works (notably in his 'Mental Development of the Child and the Race') he had extended the principle of natural selection to the selective adjustments effected by consciousness from over-produced functions (movements, dispositions, etc.) on the basis of Spencer and Bain's theory of 'surplus discharges.' In the present work this principle finds further application and emphasis. "A completed view of the psychophysical accommodation requires (first) natural selection, operating upon (second) variations in the direction of plasticity, which allows (third) selective adjustment through the further operation of natural selection upon the organism's functions."

The section on 'Correlation of Characters' is of considerable interest, although dealing with a notoriously obscure and refractory set of phenomena. The important conclusion is reached that "the psychophysical question, when put in a particular case, is really one of determining the correlation of the characters which consciousness shows with those of the organism as such. And it follows that, wherever a mental character enters into the complete carrying out of a physical function, it must have its place assigned to it." * * * "Furthermore, we may say that no physical character which has mental correlations is completely understood until these latter are exhaustively determined, and also that no mental character escapes physical correlation. Recent researches in the psychological and physiological laboratories is establishing many such psychophysical correlations: that of emotion with motor processes, of attention, rhythm, and the time-sense with vaso-motor changes, that of mental work with nervous fatigue, and so forth through all the main problems of this department. All this affords, in so far, at once illustration and proof of the general formula of psychophysical parallelism." According to Professor Baldwin, it is only recently that the idea of selection from congenital variations has been brought over from biology into psychology. Here, too, is seen an instance of the close correlation of the mental and physical. "Natural selection has worked upon correlated psychophysical variations — not upon organic variations merely. In other words, it has been the psychophysical, not the physical alone, nor the mental alone, which has been the unit of selection in the main trend of evolution, and nature has done what we are now urging the science of evolution to do — she has carried forward the two series together, thus producing a single genetic movement. It would have been impossible for mind to develop by selection with reference to utilities for which the necessary organic variations were not present; and so also it would have been impossible for the organism to evolve in ways which the consciousness of the same animal forms did not support and further. There could not be independence; there must be correlation." The fact of correlated variation is also applicable to organic and mental variations in different individuals. As an excellent example of this, Professor Baldwin calls attention to the theory of sexual selection, in which a correlation is assumed between the organic ornamentation of the male on the one hand and the mental apprehension and sexual impulse of the female on the other. The very interesting results of Groos's study of play in animals are also utilized in this connection.

In the third chapter the reader's attention is directed to the two leading principles to which the work is devoted. These are best stated in the author's own words, even at the risk of overdoing the number of quotations in this review. These quotations should be prefaced by the remark that Professor Baldwin, like some other recent authors on evolution, understands by 'modifications' the 'somatogenic variations' of biologists, and by variations proper the 'blastogenic' or 'germinal variations.' The first principle is that of organic selection. "This position is the general one that it is the individual accommodations which set the direction of evolution, that is, determine it; for if we grant that all mature characters are the result of hereditary impulse plus accommodation, then only those forms can live in which *congenital variation is in some way either 'coincident' with or correlated with the individual accommodations which serve* *to bring the creatures to maturity.* Variations which aid the creatures in their struggle for existence will, when definite congenital endowment is of utility, be taken up by the accommodation process, and thus accumulated to the perfection of certain characters and functions. The evolution of plasticity, on the other hand, could only itself have taken place by the coöperation of accommodation using the variations towards plasticity already present at each stage, and thus saving and developing such variations. This gave an even higher platform of variation from which steady refinement of plasticity and its accompanying intelligence was all along possible. *Organic selection becomes, accordingly, a universal principle, provided and in so far as, accommodation is universal.*

"Accommodation, therefore, when all is said, is a positive thing, a vital and mental functional process supplementary to the hereditary impulse. It must be considered a positive factor in evolution, a real force emphasizing that which renders an organism fit; whereas natural selection, while a necessary condition, is yet a negative factor, a statement that the most fit are those which survive. If it be true that those variations which can accommodate, either very much or very little, to critical conditions of life are the ones to survive, and that such variations will be accumulated and will in turn progressively support better accommodations, then it is the accommodations which set the pace, lay out the direction, and prophesy the actual course of evolution. This meets the view of the Lamarckians that evolution does somehow reflect individual progress; but it meets it without adopting the principle of Lamarckian inheritance.

"The second general position advocated, on the basis of facts, in the following pages is that of Social Heredity or Social Transmission with Tradition. This too falls into place in our general theory of determination. If accommodation is a fact of real and vital importance, then some natural way of regulating, abbreviating and facilitating it would be of the utmost utility. If animals were left to constant experimentation each for himself, they would die, as we have said above, before they made much development. We find that an important function of consciousness is that it enables them to profit by experience. By memory, association of ideas, pleasure and pain motivation, they abbreviate, select and handle experience to the most profit.

"But there also arises an additional resource — and certainly a very important one — by which *they are enabled to profit as well by the experience of others.* So soon as animals can use their native impulses in an imitative way, they begin to learn directly, by what may be called 'cross-cuts' to a desirable goal, the traditional habits of their species. The chick which imitates the hen in drinking does not have to wait for a happy accident, nor to make a series of experiments, to find out that water is to be drunk. The bird deprived of the presence of others of its kind does not learn to perfection its proper song. All the remarkable accommodations of an imitative sort, so conspicuous in the higher animals, enable them to acquire the habits and behavior of their kind without running the risks of trial and error. Calling this store of habits of whatever kind 'tradition,' and calling the individual's absorption of them and his consequent education in tradition his 'social heredity,' we have a more or less independent determining factor in evolution. For these accommodations are the cream of the needs of life, they represent the essentials of education, the *sine qua non* in an animal's equipment; so the accommodations which must be reproduced in race evolution, as adaptations which the species must effect, are in these lines. The influence of organic selection is, therefore, exerted to determine, by the selection and accumulation of variations, the congenital equipment which most readily utilizes and supplements these traditional modes of behavior. The two factors work together and for the same general result.

"There is, therefore, in tradition a further determining factor. Natural selection plays about it to fix a requisite function here, to eradicate what is unnecessary and nonuseful there — in short, by its omnipresent operation on this character and on that, to perfect the individual for the most adapted life."

In concluding the first part of the work Professor Baldwin stops to advance a serious objection to the theory of preformism and at the same time to answer one of the objections that have been made to the principle of organic selection. It has been claimed by the preformists that the accommodations of the organism are sufficiently accounted for by the natural selection of the congenital variations that make for plasticity. There is, therefore, no reason to emphasize the modifications themselves. The author meets this objection by showing that it is the actual development which dominates and determines evolution, not the variations by which certain functions are merely rendered possible. "Let us say, for instance, that the female bird has a certain capacity for preferential choice among possible males. This means nothing unless she actually makes a choice. Then the physical characters of the offspring vary according as this male or that is chosen, and these go down to posterity. It is the result which is the evolution, and it is conditioned upon the use made of the endowment."

With the exception of its last chapter, the second part of the work, on the 'Method of Evolution,' consists of a collection of articles that have been published in *Science*, the *American Naturalist* and the author's 'Dictionary of Philosophy and Psychology.' This is the most valuable portion of the book, as it teems with interesting suggestions and sound comments on current biological theories. The first chapter is devoted to a criticism of the late Professor Cope's well-known article in the *Monist* (July 26, 1895), and points out some of the fallacies of Lamarckism. The following passage anticipates the consideration of instinct to which the two succeeding chapters are devoted: "The only apparent hindrance to the child's learning everything that his life in society requires would be just the thing that the advocates of Lamarckism argue for — the inheritance of acquired characters. For such inheritance would tend so to bind up the child's nervous substance in fixed forms that he would have less or possibly no plastic substance left to learn anything with. Such fixity occurs in the animals in which instinct is largely developed; they have little power to learn anything new, just beause their nervous systems are not in the mobile condition represented by high consciousness. They have instinct and little else."

Professor Baldwin's discussion of instinct from the standpoint of organic selection is particularly valuable to the comparative psychologist. With the inheritance of acquired characters the origin of instinct from 'lapsed intelligence' is also of necessity repudiated. Nevertheless the author recognizes the basic value of intelligence, which he uses in a very liberal sense as the equivalent of consciousness — but shows that this very assumption enables us to dispense with the Lamarckian factor. Intelligence "is itself a congenital variation; but it is also the great agent of the individual's accommodations both to the physical and to the social environment." And he remarks further on by way of summary: "1. We reach a point of view which gives to organic evolution a sort of intelligent direction after all; for of all the variations tending in the direction of an instinct, but inadequate to its complete performance, *only those will be supplemented and kept alive which the intelligence ratifies and uses for the animal's individual accommodations.* The principle of selection applies strictly to the others or to some of them. So natural selection eliminates the others; and the *future development of instinct must, at each stage* *of a species' evolution, be in the directions thus ratified by intelligence.* So also with imitation. Only those imitative actions of a creature which are useful to him will survive in the species; for in so far as he imitates actions which are injurious, he will aid natural selection in killing himself off. So intelligence, and the imitation which copies it, will set the direction of the development of the complex instincts even on the Darwinian theory; and in this sense we may say that consciousness is a factor without resorting to the vague postulates of 'self-adaptation,' 'growth-force,' 'will-effort,' etc., which have become so common of late among the advocates of the new vitalism.

"2. The same considerations may give the reason in part that instincts are so often coterminous with the limits of species. Similar creatures find similar uses for their intelligence, and they also find the same imitative actions to be to their advantage. So the interaction of these conscious factors with natural selection brings it about that the structural definition which characterizes species, and the functional definition which characterizes instinct, largely keep to the same lines."

It is the opinion of the present writer that however valuable the questions of imitation and social heredity may be in the case of man and the higher animals, their unguarded introduction into a general discussion of instinct may lead to some confusion, for it is evident that the vast majority of the lower animals, *e. g.*, hundreds of thousands of species of insects, each with its own more or less sharply defined instincts, are neither gregarious nor social, nor ever even see their parents nor witness the activities of other members of their own species. Hence there can be absolutely no imitation or social heredity in the development of these instincts which are often, nevertheless, extremely complicated (*e. g.*, in the ant-lions, yucca moths, solitary bees, wasps, etc.). In these cases, as Professor Baldwin would probably admit, we must rely on natural selection — or to put it in his terms — the absence of imitation would permit the full incidence of natural selection, *i. e.*, imitation could not act as a screen to protect the organism while it is acquiring the congenital basis of instinct.

Space will hardly permit of an adequate consideration of Professor Baldwin's views on orthoplasy, which is another name for the principle of organic selection involving the repudiation of Lamarckism and the thoroughgoing appreciation of the interaction of modifications and natural selection in producing determinate evolution. This theory certainly holds out the hope of being able to pilot us safely between the Scylla of inherited somatic characters and the Charybdis of imaginary germinal architectonics. It is, moreover, perfectly logical in construction and therefore of considerable value as a hypothetical explanation. Its heuristic value is another matter, and it is here that the biologist will probably demand further light from Professor Baldwin and the other supporters of the principle. Apart from the difficulty of distinguishing very satisfactorily between modifications and variations, it is not at all easy to see how we are to go about testing the hypothesis of organic selection by observation or experiment. This is, of course, no argument against the truth of the hypothesis, but it is nevertheless a serious defect and one which it shares with nearly all the various selection hypotheses. There is also considerable doubt concerning the universality of organic selection, especially in cases of 'passive' characters, like coloration, sculpturing, pilosity, etc., which are nevertheless the very things that the biologist who is trying to work from the relatively solid basis of specific variations, desires most to explain. This seems to be admitted by Professor Baldwin when he says: "Mimicry and those anatomical and structural characters in which the element of function is much reduced, seem to be explained by natural selection with little supplementing from other factors." Still, it may be asked: Why may not natural selection suffice also in other cases of reduced function, especially when we consider the marvellous perfection of adaptation in mimicry and passive structures and the occurrence of determinate evolution in these cases also? A similar question may be asked in regard to the highly perfected instincts of the non-social insects alluded to above.

In his consideration of the objections that have been urged against organic selection Professor Baldwin expresses the belief that the principle should not be restricted to coincident variations but include correlated variations as well. This is supposed to meet the objections of Plate ('Ueber Bedeutung und Tragweite des Darwin'schen Selectionsprincips,' 1901,) which appear to be known to the author only from the report in the *Année Biologique* for 1901. The gist of Plate's argument that 'the blastogenic variations are too small in comparison with the functional adaptability of the organism to have selective value' does not seem to be squarely met by Professor Baldwin, and, if the present writer correctly grasps his meaning, he considerably weakens, at this point in his work, the value of his principle from the standpoint of determinate evolution by too much stress on the obscure possibilities of correlative variation.

The third part of the volume contains considerable new material, embodied in the chapters on the 'Struggle for Existence and Rivalry,' 'Lamarckian Heredity and Teleology' and the 'Theory of Genetic

Modes.' All of these chapters, and especially the last, have a decided bearing on current tendencies in biological science, and will be read with approval by those who are able to appreciate the exaggerations and limitations of the neovitalistic and biometric movements and the current physico-chemical dogmatism in physiology. The last chapter also contains an able criticism of Pearson's views on the subject of history and of Prof. T. H. Morgan's somewhat vacillating opinions on organic regeneration. The chapters on 'Selective Thinking' and 'The Origin of a "Thing" and its "Nature"' are more abstruse and involve questions of more especial interest to the psychologist.

Although the nature of the subjects treated in the volume necessitates careful reading, and sometimes frequent reperusal, no biologist nor psychologist who reads the work will be able to lay it down without a feeling of gratitude to Professor Baldwin for the many helps and suggestions which it contains. Greater familiarity with the details of biology would perhaps have enabled the author to enliven the articles with more richness of illustration and have thereby extended the good influence of the work to that greater body of biologists who fail to appreciate the extent to which psychology is destined to illumine many of the dark recesses of their science.

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ANTHROPOLOGICAL.

Reports of the Cambridge Anthropological Expedition to Torres Straits. Vol. II., 'Physiology and Psychology.' Part I., 'Introduction and Vision.' W. H. R. Rivers. With an Appendix by C. G. Seligmann. Cambridge, The University Press, 1901. 4to, pp. 140.

The Color Vision of the Natives of Upper Egypt. W. H. R. Rivers. Journal of the Anthropological Institute, Vol. XXXI., July–December, 1901, pp. 229–245.

Although the 'anthropology' of the philosophers has usually been equivalent to psychology (in a broad and somewhat obsolescent sense of the latter term), it will not be disputed that the modern science of anthropology has chiefly embraced studies in the physical structure and development of man, in the history and distribution of races, and in comparative culture and linguistics, together with a certain amount of archæology. Logically, however, the comparative study of the mental processes of peoples forms as legitimate a branch of anthropology as the study of their mental products, and in point of fact a renewed interest in the psychological problems of comparative anthropology has recently stimulated systematic attempts to collect trustworthy data available for exact inductive interpretation. Field work in this direction has scarcely begun, but the time is ripe and the opportunity fast fading away under the glare of spreading civilization. Experimental psychology has evolved methods and devised tests, which, if not calculated to discover the innermost secrets of the soul, are nevertheless available for extensive application in the field, and adequate to the definite settlement of numerous important questions.

Psychologists, in congratulating their industrious fellow-workers, the anthropologists, upon adding another side to their already many-sided labors, cannot but congratulate themselves as well upon the promise with which such investigation is fraught for psychology.

Dr. A. C. Haddon has rendered a signal service to two related sciences by the excellent provision made for the psychological examination of a primitive people — the Papuans are among the 'lowest' of the races — in connection with the Cambridge Anthropological Expedition to Torres Straits in 1898. It is intended to publish a series of Reports in six volumes, the second to be devoted to 'Physiology and Psychology.' Of this volume, Part I., by Dr. W. H. R. Rivers, has appeared. The 'Introduction' (pp. 1–7) defines the work as 'an attempt to study the mental characteristics of the natives of Torres Straits and the Fly River district of British New Guinea by the methods of experimental psychology.' The chief part of the work was done on Murray Island, where a disused missionary's house was fitted up as a laboratory. Dr. Rivers was here assisted by Messrs. W. McDougall and C. S. Myers, and elsewhere by Mr. C. G. Seligmann.

The difficulties to be encountered in thus dealing with a primitive people have been abundantly recognized by Dr. Rivers, who was able to overcome many of them, as his account of the circumstances shows. Appeals to the vanity of the natives proved especially efficacious in enlisting their interest. The smallness of the mean variations in most of the quantitative records and the general consistency of the results[1] tend to confirm the favorable impression of the trustworthiness of the observations in the minds of the experimenters. The tests were carried on at intervals through several months, many being repeated upon the same individual. The records are usually in the form of averages with mean variations — these determinations for both the individual and the group. In some cases the median is also given. The bear-

[1] Attention is called to the fact that the procedure was with these natives ideally '*unwissentlich*' in almost all cases.

ing of age and sex was considered as far as possible, although it was not always possible to learn the age.

In the following account of the results on vision no attempt at complete summarization can be made of a monograph which is itself a summary of a varied series of tests; besides, the inconclusiveness of many of these renders the records unfit for quotation apart from the accompanying discussions and qualifications in detail. While many of the results are extremely suggestive—even tantalizingly so—most of them require verification by further experiments. At this juncture it should be remarked that as a contribution to the empirical study of methods there can be no doubt of the value of this research. Extreme care is given throughout to the consideration of sources of error which are recognized when not eliminated. Historical summaries of the previous work in the several departments facilitate interpretation of the new facts. I can only quote a few of the more striking results and indicate the upshot of our author's conclusions.

The results fall under four heads: I., 'Physical Characters and Diseases of the Eyes' (pp. 8–11); II., 'Visual Acuity' (12–47); III., 'Color Vision' (48–96); IV., 'Visual Spatial Perception' (97–132). An Appendix, by Mr. Seligmann, deals with 'The Vision of Natives of New Guinea' (133–140).

The almost universal pigmentation of the conjunctiva, the prevalence of opacities of the cornea, of pterygium and pinguicula, the smallness of the pupils in general, are the main points under I. The second and third sections concern us more; the fourth might concern us most of all if only it led us anywhither unmistakably.

Snellen's E-test gave the most satisfactory results on visual acuity, though the ordinary letter test-types of Snellen, Snellen's counting test (No. LIV.) and Guillery's test (visibility of black dots on white ground) were used. The tests were made in the open air, and ordinarily with both eyes open. Beginning at a distance of fifteen meters from the type, the subject was asked to hold a board stamped with the letter E in such a position that the E should resemble the position of the letter indicated on the chart. The distance at which letter No. 5 [1] was read correctly nine times in ten gave the measure desired. Learning by heart seems to have been ruled out in these tests. The reliability of the method is evidenced by the agreement of results obtained on different days, which shows that differences in illumination were not a significant factor.

[1] *I. e.*, the letter which subtends an angle of five minutes at a distance of five meters.

In general, 'visual efficiency' (*Sehleistung*) rather than 'visual acuity' (*Sehschärfe*) in Cohn's restricted sense was measured by these tests, but the latter term is here, as commonly, used to cover both functions.

The average determination for the 169 individuals tested in the Torres Straits and the Fly River region taken together, was 10.6/5, with a median of 11/5. This shows a superiority to that of the Heligolanders tested by Cohn, the only results comparable to these, the averages being 2.12 and 1.77 respectively. We can hardly speak of the average acuity of civilized races, seeing that tests have usually been made upon special classes or by means and under conditions which render them incomparable with the present series.[1] The facts, 'so far as they go,' seem to show that in a state of nature there is no marked sexual difference in visual acuity — but they go a very little way. In Torres Straits 'a very distinct and progressive decrease of visual acuity from the age of thirty-five onwards' is recorded, which is partly attributable to pathological changes less common among Europeans, whose acuity is said to decline at about the age of fifty. While a slight hypermetropia is considered normal for savages as for children, and some facts suggest this condition among the Murray Islanders, Dr. Rivers' results were not decisive. Myopia was extremely rare — a circumstance which would tend to raise the average visual efficiency. "Several cases of astigmatism were discovered in Murray Island." A few experiments suggest that the Papuan eye adjusts itself to the dark more quickly than that of a European. The just perceptible difference of brightness was measured by the use of Masson's discs. While the threshold was higher than that previously recorded for Europeans, results quite as good were obtained, by the same method and with the same discs, from Englishmen, after Dr. Rivers' return. There is thus no evidence of a superior sensitiveness to brightness on the part of these savages.

The conclusion of the whole matter of comparative visual acuity in primitive and civilized peoples (assuming the doubtful right thus to lump diverse racial groups)— so far at least as this research goes— is that, while there is an average superiority in the savage's favor, this is, after all, but slight, especially when the cases of myopia, etc., are excluded, so slight indeed as to render the common report of travelers untrustworthy or as to require that the traditional interpretation be altered. Practice and special knowledge of familiar objects are the two principal factors suggested, in support of both of which Ranke's

[1] The standard $V = 1$ is of course subnormal.

interesting observations are cited.[1] The apperceptive factor, illustrations of which are familiar, invites further study, both qualitative and quantitative.

The special richness of their vocabulary, no less than other obvious signs of familiarity with natural objects, proves the natives to be close observers of nature. In this connection, Dr. Rivers offers a highly interesting suggestion which invites searching analysis and inductive investigation, and which, if true, might prove to have important practical bearings. According to this suggestion, the predominance of minute attention to objects of sense hinders higher mental development. Although the mind must be fed of the senses, it may be overfed and starved through its very gluttony! "If too much energy is expended on the sensory foundations, it is natural that the intellectual superstructure should suffer." (Let our systematic naturalists take warning!) Ranke's unfortunate experience in losing his power to enjoy scenery in the large after living with South American natives and acquiring their habits of particularized attention, together with his still more unfortunate failure in the effort 'to devote attention to the more serious problems of life,' is cited in support of a causal connection between the alleged sensory plethora of the savage's mind and his lack of any considerable æsthetic appreciation of nature.[2]

[1] *Correspondenz-blatt. d. deutsch. Gesellsch. f. Anthrop.*, S. 113, 1897.

This opens a field for fruitful experimentation. One gathers from Dr. Rivers' discussion that while the average acuity of the savage is naturally but little higher than that of his civilized brother, by the constant practice which his mode of life affords he may acquire a superior acuity. Whether this is so in general, or whether practice improves only the ability to descry certain special objects, would be an interesting problem for study. Ranke evidently meant to assert increased general acuity by practice in accommodation for distant objects.

[2] The general question of the relation of the data of perception to the higher mental syntheses is thus revived and set in a new light. Mr. Galton's results on mental imagery in relation to maturity and intellectual occupation with abstract thought are suggestive in this connection. The economy and increased scope of symbolic thinking render it advantageous in evolution, while it implies a correlative neglect of sensory minutiæ. There is some reason, however, to doubt the alleged contrast between the typical savage mind, immersed in a sensuous medium by which it is pressed if not oppressed from every side, and the free mind of your *fin de siècle* philistine, soaring in the dry atmosphere of abstract thought high above the diversified surface of things, and contemplating the whole as might a bird possessed of the requisite apperceptive faculty. Not that Dr. Rivers asserts such a contrast baldly — indeed many of his conclusions counteract, if they do not nullify, this doctrine. But it seems worth while, in view of the suggestion under review, to bear in mind how insidiously, yet how completely withal, we may be deceived by appearances in inter-

Perhaps the section on 'Color Vision' comprises the most interesting results of the investigation.[1] Results of actual tests of color vision were compared with the color nomenclature. The bearing of this on the one-time active theory of Gladstone, Geiger, Magnus, *et al.* is such as to breathe into it a new life and so to give it a 'fighting chance' again.[2]

Holmgren's wools, supplemented by others, were matched by over two hundred individuals. The descriptions of the tests should be read in full for their suggestiveness on many points of method and of theory. Transient comparisons were noted as well as definite decisions. Red, pink and yellow were sorted normally, but green was frequently compared with blue and blue with violet. The tendency[3] to match differently sensed colors having a common name in a limited color vocabulary, especially with the paler 'cold' colors, was noted, along with other sources of error more easily to be guarded against. The tendency to match saturations rather than tones, depending in part upon the linguistic factor, introduced another complication.

While there may have been yellow-blue blindness in a few cases, the facts do not compel such a conclusion. Among 152 individuals tested, including 130 males 'with the just possible exception' of one man, 'there was not a single case of red-green blindness.' The ab-

preting the minds of peoples having a mental tradition wholly different from our own, correlated it may be with a markedly different environment. (Professors Franz Boas and Livingston Farrand are inclined to the extreme view of an essential uniformity of psychic constitution and even of psychic capacity as between the so-called 'higher' and 'lower' races. See some weighty considerations relative to this point in a paper by Professor Boas on 'The Mind of Primitive Man,' in *Science*, Vol. XIII., No. 321, February 22, 1901, pp. 281 ff.) This perceptual enslavement to the particular objects of immediate utility in the daily routine, on the part of a people without the intellectual leisure which comes of a specialized civilization and a comminuted industry requiring little attention to details in general on the part of each, but a limited attention to specific details only, and without the necessity of dealing with the larger relations implied by such a civilization and the concomitant competition of wits, may possibly serve to differentiate mentally the 'primitive' from the more 'advanced' type. But of course no one will pretend that persistent inability to see the woods for the trees is a defect confined to savage peoples.

[1] See an excellent account of these and other results in relation to the whole question of 'Primitive Color Vision' by Dr. Rivers in *Pop. Sci. Monthly*, LIX., May, 1901, pp. 44–58.

[2] Many of the criticisms of Virchow and others will of course continue to hold against the theory, which must ever appear ludicrously weak in its Gladstonian form.

[3] Mr. Seligmann notes the absence of any such marked tendency in British New Guinea.

sence of Daltonism in this race is further supported by results obtained with Lovibond's tintometer. Comparative tables of the threshold values for saturations of red, yellow and blue (when correctly perceived four times in five) reveal some interesting differences between the Murray Islanders and Englishmen. To the Murray Islanders red is most easily recognized, next yellow and finally blue (averages for 17 natives: R = 17.6, M. V. 7.66; Y = 26.5, M. V. 9.71; B = 60.0, M. V. 16.5). Yellow takes precedence with the English, while red and blue differ relatively little (averages for 18 persons: R = 31.7, M. V. 22.5; Y = 20.5, M. V. 8.11; B = 36.4, M. V. 15.13). The greater variability of the English arises chiefly from cases of red-weakness. No indication of color-blindness was given with the tintometer, nor even any insensitiveness to red.

It should be remarked, however, that among eight Lifuans (of the Loyalty Islands), three were distinctly red-green blind. There are other considerations which reveal the utter futility of all attempts to generalize the color vision of 'savages' *vs.* 'Europeans.' Dr. Rivers suggests that the prevalence of color blindness may prove to be an ethnic character of taxonomic significance.[1]

In the absence of a perimeter, quantitative observations of indirect color vision were not made, but, by means of rough tests made by moving black cards with colored patches, several interesting features were brought to light. There was, *e. g.*, little difference between the red, yellow and blue fields, although the blue tended to be largest. The green field was relatively small. The color fields seemed generally smaller than in Europeans. The ready recognition of blue was not only conspicuous but in conflict with the results of the other tests and the defective nomenclature to be noted. This discrepancy is attributed to the influence of the pigmentation of the *macula* in direct vision. The large size of the red field and the high sensitiveness for red among the savages seem at first blush to bear adversely upon the atavistic theory of color-blindness.

Contrast (with a color wheel and Meyer's experiment) was apparently less vividly perceived by these natives than by Europeans. Tests for color preference proved unsatisfactory. The probable influence of factors summarized by the word 'tradition' seems to me to invalidate conclusions drawn from Sunday costumes, in which black clearly predominated with the men, while red seemed to take the lead among the women.

Not least interesting are the observations collected upon the color

[1] Cf. the case of the Egyptians below.

vocabularies, obtained by requiring Rothe's papers, various wools and other colored objects to be named. The formation of color adjectives by reduplication from the names of natural objects is very interesting. Thus three words used for green in Murray Island emphasize bile, leaf and pus respectively. The relationship between words for different shades of the same color, and other linguistic features, cannot be discussed here, but only some of the general results of psychological interest. Three color vocabularies of the Torres Straits and the Fly River district agree in having relatively definite and well established words for red and yellow. All three agree in having each a definite word for green, which is not used so unanimously, however, as the words for red and yellow. The Kiwaians have no word for blue; the Murray Islanders, lacking a native word, have modified and reduplicated the English form; in the Western Tribe of Torres Straits a native word occurs, which may be used for green as well. Australian tribes were also examined to some extent, with the result that a still more primitive condition was discovered, in which there were definite names for only red, black and white.

In general there was found among all these tribes an undifferentiated vocabulary for blues and the adjacent hues, which was loosely applied. (Blue sky, green sea, dirty water and black night were denoted by common names.) *What is more to the point, there was an undifferentiated color sense to correspond, as shown by the wool tests, tintometer, etc.* This correspondence lends some justification to the fundamental assumption of the Gladstone-Geiger school. It must not be supposed, however, that the correspondence was at all exact. The vocabularies probably fail to express all the differences actually perceived. Indeed the tribe having 'the most incomplete and indefinite color vocabulary met with in British New Guinea'—the Sinaugolo—was 'specially quick and correct' in matching wools (Appendix, p. 135).

There can be little doubt, thinks Dr. Rivers, that blue is a darker or duller color to the Papuans than to us. But he admits that physiological insensitiveness is only one factor in determining the defective nomenclature, and cites evidence corroborative of the theory which insists on the relation of the distribution of natural pigments to the rise of a color nomenclature, while suggesting other factors, among which may be mentioned the alleged absence of æsthetic interest in nature, with her beauty of earth and sea and sky.

Visual space perception was examined in various ways. Binocular vision was tested with a form of Hering's 'fall experiment,' in which shot varying in size was dropped before or beyond a fixation point consisting of a bead suspended in a cardboard tube at a distance of two feet from the observer's eyes. Sixteen individuals gave definite results demonstrating ordinary binocular vision. They were right 'nearly every time' with two eyes, while 'as often wrong as right' with one eye.

Papuans saw double images; coördinate eye-movements were observed in a single Papuan baby, aged twelve hours; etc.

The results obtained from dividing lines and estimating lengths and from the illusion tests do not lend themselves to summarization, nor can much importance be attached to them. Dr. Rivers' discussion is, however, instructive throughout and his meager results should be provocative of renewed and more extended investigation. While certain constant errors appeared which differed quantitatively from those obtained with English students or were of opposite nature, it is of interest that these differences tend to disappear when comparison is made between Papuan and English *children*. This comes out, *e. g.*, in the overestimation of vertical as compared with horizontal lines, an illusion which has probably been tried by Dr. Rivers on a larger number of individuals than by any one else. The Murray Islanders are more susceptible to this illusion than English students. More interesting perhaps is the result that by all groups (twenty Papuan men, twelve boys; fifteen English students, twelve children) the vertical was made shorter when drawn on the end of the horizontal than when drawn through the latter to make a cross, and shortest when drawn in one direction from the middle of the horizontal. A similar result emerged from a rough examination of 112 English school boys.

To the Müller-Lyer Illusion (Heyman's device) the Papuans were distinctly less susceptible than Europeans, as shown by the several statistical values taken together. All things considered, the Papuans were but slightly less constant and accurate in their observations than the English. Dr. Rivers thinks that the savages gave more exclusive attention to the lines themselves, the English to the figures as a whole. This will be seen to harmonize with other apparent facts supporting the view that the minds of savages are chained to particulars of sense, the latter being subordinated to the higher functions of apperception in civilized races. Other geometrical illusions from the Milton-Bradley set of 'Pseudoptics' were tried on the natives, who were also questioned *ad hoc* upon common phenomena of nature.

It is hoped that Dr. Rivers will have further opportunities for studying these Papuans until, for this group, statistical data of unquestionable significance be obtained, and that others will be stimulated to prosecute like investigations on other primitive groups. Enough has been said to indicate how futile it is to treat all savages as if they constituted a homogeneous class.

The last remark will be strengthened by a survey of the second paper, in which Dr. Rivers reports some results obtained, in part by Mr. D. Randall-MacIver, in larger part by himself, from the natives of Upper Egypt, who were principally typical *fellâhin*, speaking Arabic.

Without attempting any detailed comment upon their color vocabulary and the variability of usage, I proceed at once to summarize the main points, largely in the author's own words. "There was a very definite word for red, *aḥmar*, which was not only applied to objects which we should definitely distinguish as red, but also to colors such as orange, purple, violet and brown, which contain a red element. There was a somewhat less definite term for yellow, *aṣfar*, which was also used for orange and brown and was occasionally applied to green and faint red. The word for green, *akhḍar*, was still less definite, being very often applied to blue, violet, gray and brown. There was no definite word for blue," etc. The same word was indiscriminately used for blue, black and other dark colors. "The nearest approach to a word for blue was *läbänî*, milk color," used also for green, gray, brown. From all this "a decreasing definiteness in the nomenclature for color as one goes from red through yellow and green to blue, was as marked in these peasants of Upper Egypt as it is in the Papuans of Torres Straits and in so many other savage and semi-civilized races." "More variety in the terms applied to brown than to any other color," is noted, "over twenty different terms being given to brown papers and wools."

An interesting feature of the nomenclature was "the tendency to use words denoting differences of color-tone for differences of shade, *i. e.*, of luminosity" — "a peculiarity which is far from being associated * * * with absence of the color sense" as independently tested, yet which is analogous to the Homeric practice upon which Gladstone rested his hypothesis.

Color vision was tested with Holmgren's wools, Nagel's circles of colored dots and Lovibond's tintometer. These *fellâhin* reacted to the wools in all essentials precisely as had the Papuans of Torres Straits. A similar insensitiveness to blue was apparent. The resemblance between pink and violet, which were often compared, may arise from this insensitiveness, which suggests a defect in Holmgren's test for ethnographic purposes, because 'a confusion of colors which in a European certainly means insensitiveness to red, may in other races be due to insensitiveness to (or lack of interest in) blue'; further, such linguistic defects as have been hinted at 'may be of influence in the process of matching, and may lead to confusions in this process which are in Europeans characteristic of color-blindness.'

To give some figures: Of 83 male natives examined, 4 (*i. e.*, five per cent.) were certainly color-blind, while others were doubtful and several gave indication of red-green weakness: this equals or exceeds the European average and contrasts strikingly with the condition found in Torres Straits. The results with the tintometer showed that the *fellâhin*, like the English but unlike the Papuans, were somewhat less sensitive to red than to yellow, but were relatively insensitive to blue, like the Papuans and unlike the English.

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NEUROLOGY.

A Case of Astereognosis resulting from Injury of the Brain in the Superior Parietal Region. WILLIAM H. TELLER, M.D., and F. X. DERCUM, M.D. Journal of Nervous and Mental Disease, Vol. 28, No. 8, August, 1901. Pp. 459–461. With discussion by DRS. CHARLES W. BURR, JAMES H. LLOYD, and WILLIAM G. SPILLER, before the Philadelphia Neurological Society, 25 February, 1901, reported in the same number of the Journal, pp. 468–470.

Taken together, these reports of cases and of the opinions of competent specialists constitute an excellent account of the condition called astereognosis, which consists in an inability to recognize objects by the sense of touch, that is, to feel the shape of objects by the muscular, joint and dermal senses.

The patient in this case was a negro aged twenty years. He was brought into the hospital after lying unconscious for a week from a blow on the head with a baseball bat. There was then an area of depressed skull in the left parietal region; complete paralysis of the right leg, right arm, right side of face and tongue.

Following the operation the facial paralysis soon disappeared; he could move his arm thirteen days after the operation, and two days later his leg, since which time voluntary control over his body has gradually improved.

His gait is slightly hemiplegic; he sways when standing with eyes closed; there is marked ataxia of movement of right arm, and decided hypesthesia on right half of body.

"With eyes closed the patient fails to recognize any object placed in his hand. * * * Slight hypesthesia to pin points, loss of space-sense, loss of the sense of location, hypalgesia and diminution of the pressure sense in the right wrist, hand, and especially the fingers, are observed. The perception of heat and cold is preserved, save that the responses are somewhat slow and the acuteness of perception slightly diminished. The knowledge of the position of the fingers in the right hand is also greatly impaired. Astereognosis is complete. * * * This case is extremely interesting, because the brain tissue was evidently injured in the region of the superior parietal lobule, especially in the region posterior to the motor area. Indeed, the conditions present in this case are *almost such as could have been desired in an experimental research in cortical localization.* The lesion appears to have involved the motor region but little as the resulting terminal hemiplegia is exceedingly slight. The astereognosis, on the other hand, is the most striking feature of the case."

GEORGE V. N. DEARBORN.

TUFTS COLLEGE.

Ueber die Wirkung des Eucain B auf die Geschmacksorgane. ARTHUR FONTANA. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, XXVIII., 253–260.

Signor Fontana, a medical student at Turin University, adds the results of a series of experiments with eucaine B to the scanty data thus far obtained in regard to the effect upon the taste-organ of the various anæsthetics employed in therapeutics. Of these substances, the effect of cocaine and of gymnemic acid (a drug derived from the East Indian *gymnema sylvestre*) had alone been tested. Eucaine B is the hydrochloric acid salt of benzoylvinyldiacetonalkamin. As a material for taste experiments it possesses over cocaine the advantages of lower price and of slighter toxic properties and of furnishing solutions which can be sterilized by heat without decomposition and which retain their properties for a considerable time. Signor Fontana has made with this substance a series of experiments upon himself with control experiments upon Dr. Kiesow and upon four other persons. The anæsthetic was in aqueous solution and was applied with a brush of about 6 mm. in diameter to the tip and to the forward part of the sides of the tongue. The intensity of the anæthesia was measured by the number of brush-strokes, and by the saturation of the solutions, which were of five-tenths of one and two per cent., and of complete saturation. The test stimuli regularly employed were graduated series of aqueous solutions of sulphate of quinine, cane sugar, common salt and tartaric acid. Three drops at a time of the material at a temperature of 15°–18° C. were applied to the tongue with a pipette.

Signor Fontana's results are as follows: Eucaine B, like cocaine, tends to raise the intensive limen for all four of the taste-qualities. Its effect, like that of cocaine, is far more marked upon bitter and sweet than upon salt and sour, and is most marked upon bitter. The degree of effect of eucaine B upon the three qualities less implicated can be stated in each individual case. Fontana does not compare eucaine B with gymnemic acid. In this connection, however, one may recall the showing of Dr. Kiesow that gymnemic acid, like cocaine, affects sweet and bitter much more than salt and sour, that, unlike cocaine, it affects sweet more than bitter, and that its influence upon bitter is much less intensive than the effect of cocaine upon sweet. Thus in eucaine B and gymnemic acid we have two substances which may eliminate one quality and yet at the same time may produce slighter and mutually comparable effects upon the other three.

Fontana tested his sensibility for the different qualities three minutes and five minutes after the application of the anæsthetic and again at intervals of five minutes until the normal limen was restored. From these trials it appears that the effect of eucaine B is less lasting than that of cocaine.

E. A. McC. Gamble.

Wellesley College.

EXPERIMENTAL.

Ueber den Einfluss der Farbe auf die Grösse der Zöllnerschen Täuschung. Vittorio Benussi. Zeitschrift f. Psychologie u. Physiologie d. Sinnesorgane, Bd. XXIX., Heft 4 u. 5, S. 264–341; Heft 6, S. 385–433.

As is indicated by the title, the purpose of the experiments was to determine the effect of color (and brightness) upon the Zöllner illusion. The apparatus consisted of a perpendicular frame, covered, usually, with white cardboard, and having the figure drawn upon this as a background. The single main-line was fourteen centimeters long, with transversals five and one-half centimeters long, and crossing at an angle of twenty degrees. At the upper end of the main-line a thread was attached, which ran over the top of the background, and was weighted behind in order to keep it drawn tightly. The test consisted in adjusting the position of the thread so as to appear to be in line with the main-line of the figure. The amount of deflection necessary to produce this appearance was read off from a millimeter scale at the top of the frame over which the thread passed. Seven one-colored figures were used (red, yellow, green, blue, violet, gray, and black), and forty-two combinations of these into two-colored figures, in which the transversals and main-lines were colored differently.

In one-colored figures the illusion varies with the brightness difference between the figure and background, being weakest for the light gray and strongest for the black. The other one-colored figures come in between the black and white in the order yellow, red, green, violet, and blue. In the two-colored figures a darkening of the transversals and an increase in the brightness of the main-line result in a greater illusion. The illusion also varies directly with the brightness difference between the figure and background, decreasing with decreasing brightness difference. The illusion was also found to be reduced to a minimum in those two-colored figures which have a maximal brightness difference between main-line and ground and a minimal brightness difference between transversals and ground. In colored figures, then, the force of the illusion depends upon the brightness difference between the figure and background and the color quality of the components of the figures. The equations and laws for this brightness difference between the figure and background are worked out to a very considerable length.

It was found that, in order to produce a maximal illusion, the transversals must cross at an angle of from twenty to thirty degrees; they must be relatively long and close together; there must be a maximal brightness difference between the transversals and background and a minimal brightness difference between the main-line and background.

In determining whether or not the illusion depends upon eye-movement, it was found that there was practically no difference in the results when the eyes were fixated upon the point of junction of the thread with the main line and when they moved freely. Eye-movement alone was regarded as insufficient to account for the illusion. (The author assumes that steady fixation eliminates eye-movement; it would seem that this assumption is not entirely accurate.)

With reference to the nature of the illusion, the hypothesis that it is an illusion of judgment is dismissed as unsatisfactory. In support of this statement experiments were made with the two parts of the regular figure united by means of the haploscope. If the illusion were really one of judgment, it would not be affected by this method. The results of over fifty-five hundred separate determinations demonstrated that the illusion is decreased when the two parts of the figure, the main-line and the transversals, are united by the haploscope, the illusion, however, still varying with the brightness difference between the figure and background. MABEL CLARE WILLIAMS.

UNIVERSITY OF IOWA.

Untersuchungen über die sogenannten Aufmerksamkeitsschwankungen. E. WIERSMA. Zeitschr. f. Psy. u. Phys. d. Sinn. Bd. 28, H. 3–4. Pp. 179–198.

In this article Dr. Wiersma continues his researches upon the attention waves and extends them to include the effects of changing conditions upon the capacity for perception as measured by the time of visibility of a faint stimulus. Again the three senses, sight, touch and hearing, were investigated, and each with respect to the effect of the time of day, of mental and physical fatigue, and of the use of alcohol and sodium bromide.

The change during the course of the day was found to be dependent upon whether the individual was a morning or evening worker. For a morning worker the period of greatest perceptive capacity was at noon, and the early morning hour was considerably better than the evening hour. For the evening worker, on the contrary, there was a gradual increase in capacity from morning to evening. The experiments were taken three times a day only, between nine and ten in the morning, two and three in the afternoon, and seven and eight in the evening, so that the exact point of change between increasing and decreasing efficiency could not be established. It is suggested that the difference in type is rather a matter of habit than inheritance, for experiments upon nurses who had no fixed periods of mental work showed no such marked influence of the time of day as did the experiments upon the academic men.

Bettmann's results were confirmed in the experiments upon the influence of mental and bodily fatigue. The period of visibility of the minimal stimulus was decreased by both mental and physical work, adding or walking for two and a half hours, although the decrease was more marked for the mental than for the physical work.

Ten c.c. of alcohol had from the beginning a marked effect in decreasing ability of perception, while three grams of sodium bromide had just as marked an effect in increasing it. The effect of the bromide was also found to persist for three days after the drug had been taken.

Dr. Wiersma's experiments show that the attention wave furnishes a very delicate and convenient method of measuring mental effectiveness under many influences, and suggest that further investigations along the same line should prove fruitful. W. B. PILLSBURY.

CHILD STUDY.

The Observation of School Children. JOHN A. HANCOCK. Pedagogical Seminary, September, 1901. Pp. 290–340.

This article professes to be an outline of a course in the experimental study of children for use in training and normal schools. The whole field of child study is covered, in a way, but the classification employed is careless; *i. e.*, we find the sub-title *motor tests* under the title *instructions for eye examinations*. Quite a full account of how and what to do in the way of physical measurements is given; some space is devoted to methods of observing fatigue and nervous disorders; mental ability is treated under the heads of memory, association and discrimination, followed by an elaborate statement of the questionnaire method taking up nineteen pages, about three-fifths of the entire article. The presentation of the questionnaire method is merely a formulation of questions implying the results of the principal researches of that character so far published from Clark University. One can not but regret that this method is emphasized so disproportionately to the more direct quantitative tests of mental ability. The whole article is little more than a catalogue of what has been done and shows lack of sifting and critical selection in respect to contents; it is as if one should take up all the recorded attempts at problems in physiology, and base demonstration experiments upon them without attempting to organize the material into a scientific presentation of the subject. Of this the author himself seems aware, and the chief value of the article is that it truly represents the chaotic state of this phase of genetic psychology. The time is ripe for a master mind to seize upon the heterogeneous mass of material at hand and give us a science.

CLARK WISSLER.

NEW YORK UNIVERSITY.

ETHICS.

Die Entwicklung der Ethik. Vortrag gehalten vor der Aristotelian Society of London. DR. JAMES LINDSAY. Uebersetzt von Ludwig Busse. Zeitsch. für Phil. u. phil. Kr., April, 1901, pp. 190–210.

The author expressly limits his accounts of the development of ethics to its last stage, during which 'the moral consciousness, under the influence of philosophy and humanistic tendencies, attains to its maturity.' But the address in fact pays but scant attention to the development of ethics prior to the middle of the nineteenth century, two sketchy pages containing all that is said. And the ethics of the last fifty years is not considered developmentally. Dr. Lindsay in fact has two main purposes in this article, to sum up what has been done and left undone for ethics by studies owing their inspiration to the evolutionary point of view, and to insist with fervor that ethics must rest on metaphysics and theology as its only secure foundation.

The author is at pains to be fair and open minded in his estimation of evolutionary ethics, and most of his positive and negative criticisms are just, though they have been made before. But in some of his statements, *e. g.*, when he says that scientific ethics does not esteem itself a normative science (p. 203), and when he repeatedly declares that the value of moral principles is wholly unaffected by their origin and history, he voices his own views, or at most those of his school, and not, as he would have us believe, the views all authorities have come to accept. Moreover, Dr. Lindsay fails to mention the most important services of evolutionary ethics. He seems quite unaware that when morality is observed and its achievements noted, first in its simplest initial form, and then as each successive complication appears down the course of its history, nature presents us, as Professor Dewey has well shown, with analyses and syntheses quite similar to those performed in scientific laboratories, and of like scientific value. And, returning to the point mentioned above, it is precisely in throwing light on the function and value of moral principles that evolutionary ethics performs its greatest service. Dr. Lindsay would hardly maintain that his rôle in life and his value as a man depend wholly on what he has been and done during the last week. And yet he would have us judge morality by its 'present,' which in relation to its life history is shorter than a week in relation to his.

The plea for a metaphysical ethics is based essentially on the existence of metempirical facts of fundamental moral importance, and an ethics that omits these from its foundations is declared to be inadequate. And inadequate a naturalistic ethics undoubtedly is. But Dr. Lindsay forgets that his metempirical 'facts' are far from being fully authentic or even generally accepted. He might have been less ready to scorn naturalistic ethics as a torso, had he reflected that his metaphysical statue may possibly be but a mass of crumbling dust.

S. E. Mezes.

University of Texas.

NEW BOOKS.

Das psychotische Moment. M. FÜHRMANN. Leipzig, Barth. 1903. Pp. 95.

Abhandlungen zur Physiologie der Gesichtsempfindungen. H. 2. J. VON KRIES. Leipzig, Barth. 1902. Pp. 197.

A collection of papers reprinted by Professor von Kries from the Zeitschrift für Psychologie.

Esquisse psychologique des peuples Européens. A. FOUILLÉE. 2me éd. Paris, Alcan. 1903. Pp. xix + 550.

The Mind of Man. A Text-book of Psychology. G. SPILLER. London, Sonnenschein; New York, Macmillans. 1902. Pp. 552. $2.75.

L'Etica evoluzionista; studio sulla filosofia morale di Herbert Spencer. G. SALVADORI. Turin, Bocca. 1903. Pp. xv + 479. L. 10.

Aristotle's Psychology. Translations of the De Anima and Parva Naturalia, with introduction and notes. W. A. HAMMOND. London, Sonnenschein; New York, Macmillans. 1902. Pp. lxxxvi + 339. $3.00 *net.*

Hegel's Logic. An Essay in Interpretation. J. G. HIBBEN. New York, Scribner. 1902. Pp. x + 313.

Studies in the Cartesian Philosophy. N. SMITH. London and New York, Macmillans. 1902. Pp. xiv + 276. $1.60 *net.*

Investigations in the Department of Psychology and Education of the University of Colorado, Vol. I., No. 2. ARTHUR ALLIN, Editor. University of Colorado, Boulder, Colorado. 1902. Contains an important paper on 'The Survival Values of Play,' by H. A. Carr, which will be summarized in a later issue.

Les Obsessions et la Psychasthénie, I. PIERRE JANET. Publications from the Laboratory of Psychology of the Clinic of the Salpêtrière (3d series). Paris, Alcan. 1903. Pp. xii + 743. Fr. 18.

The following publisher's note (condensed) gives a description of this important volume. We translate it as a preliminary notice. "M. Janet gives in this volume a new application of the method used by M. Ribot and employed by himself earlier in his work on 'Neuroses and Fixed Ideas.' The method consists in drawing from psychology all the light which it can afford on the classification and interpretation of the facts of mental pathology, and in seeking reciprocally, in the abnormal alterations of mind, those natural data of observation and experiment which further the analysis of human thought. It thus has equal interest for psychology and for medicine. Among the phenomena which are treated in this study are the following: obsessions, abnormal impulsions, mental diseases proper, the doubting mania, ticks, emotional agitations, phobias, and certain detailed phases of disease, such as neurasthenia, unusual feelings of strangeness, and derangement of personality. M. Janet applies to those having the affections which come under these heads the term 'scrupuleux,' because a certain 'scruple' constitutes one of their essential characters, and also by the more precise name of 'psychasthéniques,' which appears to him to sufficiently describe the enfeeblement of the psychological functions which are involved. The volume consists of two parts. The first is descriptive and analytic, under the title 'Analysis of Symptoms.' In it we find the treatment of obsessions, various uncontrollable agitations, psychasthenic stigmata, etc. In the second, which is more historical and general, the author gives us general studies on the weakening of psychological tension, the evolution, diagnostics, and treatment of psychasthenia and its place among nervous conditions. By the comparison of the different symptoms in one book, he makes a contribution to the study of diagnostics, prognostics, and treatment of the affections included, which play an important role in nervous pathology; and further gives a psychological analysis of these different phenomena so far as he has been able to discover among them recognized characters and to arrive at an interpretation which allows the grouping of the greatest possible number of facts under general conceptions. The volume, like the preceding one on Fixed Ideas, will be followed by a second part, published in collaboration with Professor Raymond, which will comprise clinical observations on a great number of diseases and will include psychological and clinical descriptions and documents affording the justification and the proofs of the interpretations presented in this volume by M. Janet."

The Jewish Encyclopedia, I. A–Apo. Is. Singer, Man. Editor. New York, Funk & Wagnalls. 1901.

A work of encyclopedic character, devoted to the history of religion, literature, and customs of the Jewish people from the earliest times to the present day — prepared by a corps of scholars and specialists under an editorial board, Dr. Isidore Singer being the managing editor. This encyclopedia promises to be, judging from the first volume, an extremely comprehensive and valuable work. It falls to our province to notice only the philosophical and psychological articles, together with the biographies of philosophical thinkers. These we find, as represented in the present volume, to be thorough and accurate. We commend the encyclopedia in general terms, at the same time reserving for a later notice, after subsequent volumes appear, a more detailed appreciation of the philosophical articles. Among the biographical articles, beginning with the Jewish writers and men of genius of the earliest times and coming down to those of the present, we notice the inclusion of contemporary writers, both in England and America. The long list of Alexanders, for example, includes the name of our esteemed contemporary at Manchester, Professor Samuel Alexander.

J. M. B.

Les obsessions et les impulsions. PITRES and RÉGIS. Bib. intern. de Psych. Expér. de Toulouse. Paris, Doin. 1902. Pp. 434. 4 fr.

We have from time to time listed in this department the successive issues of the International Library of Experimental Psychology, edited by Dr. Toulouse. The separate volumes, of which some forty are projected, are now rapidly appearing and we have nine of them already upon our table. While expecting to notice, in considerable detail, the individual volumes, it may be well to call the attention of our readers to the enterprise as a whole, to the comprehensiveness of the undertaking, and to the high character of the separate volumes.

J. M. B.

Ethics of T. H. Green, Herbert Spencer and J. Martineau. H. SIDGWICK. London and New York, Macmillans. 1902. Pp. xli + 374.

These lectures of Professor Sidgwick have been edited by Miss E. E. Constance Jones, of Girton College, Cambridge, from manuscript which was not prepared for publication by Professor Sidgwick, and, consequently, did not have his final revision. They are complete, however, as he delivered them, and constitute a most interesting and valuable critical volume from the pen of Professor Sidgwick. The essay on Green is perhaps the most unsympathetic of the group, Sidgwick seeming to have some difficulty in entering into the spirit of the Oxford philosopher. On the other hand, that on Martineau seems to the present writer illuminating and valuable. Mr. Spencer is treated in ten lectures in considerable detail and with that quality of judicial fairness which we are accustomed to expect from Professor Sidgwick when at his best. This part of the volume — that devoted to Herbert Spencer — seems to be a very valuable addition to the critical literature of evolution ethics in general. One reflection forces itself upon the mind as we follow Professor Sidgwick: it is that whatever we may find in the criticism made by other writers, of systems with which they do not agree, in the way of lack of information and inadequate representation of the works criticised, such faults did not attach to the work of this critic. The moderation, balance, and reserve of Professor Sidgwick's mental appreciations show themselves here in all their force. J. M. B.

L'Année Philosophique. F. PILLON. 12th year, 1901. Paris, Alcan. 1902.

In this volume M. Pillon continues his annual critical summary of the literature of French philosophy for the year 1901. The volume also contains a list of the writers referred to, which constitutes a selective bibliography of French philosophical literature for the year.

Outlines of Psychology. W. WUNDT. Translated by C. H. Judd. Second edition, revised from the fourth revised German edition. New York, Stechert. 1902. Pp. xxii + 390. $2.00.

In the translator's preface to the English edition, the translator points out by page citation the most important additions to the text which have been made in the work as contrasted with the first English version. Among these additions are various literary citations which Professor Wundt himself inserted in the fourth German edition. These constitute a select bibliography made by the author. The translator adds that he has not found it necessary to make any significant changes in the terminology adopted for the earlier edition. A brief notice of the first English edition is to be found in an earlier number of this REVIEW (V., 1898, 208). J. M. B.

Crimes et Anomalies mentales constitutionnelles. A. FOREL and A. MAHAIM. Paris, Alcan. 1902. Pp. 300.

Mainly a report on certain French medico-legal cases with citation of 'expert' opinions.

Philosophie générale et Métaphysique. Vol. I. of the Proceedings of the International Congress of Philosophy, Paris, 1900. Paris, Colin. 1900. Pp. xxii + 460. 12.50 fr.

This is Vol. I. of the detailed proceedings of the congress. Subsequent volumes are to be devoted to Ethics (II.), Logic and History of Science (III.), and History of Philosophy (IV.). The whole will constitute a very valuable collection of papers covering the entire field of philosophic thought and serving as a worthy monument, in this particular branch, of the great Exposition of 1900.

J. M. B.

Talks to Students on the Art of Study. FRANK CRAMER. San Francisco, Hoffman-Edwards Company. 1902. Pp. 309.

This book is by the author of that interesting study 'The Method of Darwin,' published several years ago. This volume is devoted, as its title indicates, to talks upon certain psychological topics which are most vital to true pedagogical method, such as habit, observation, attention, memory, etc. The book seems to us to be sound and also interesting — one which the teacher will find instructive and helpful.

J. M. B.

The Faculty Corner. Papers by the Faculty of Iowa College. Grinnell, Iowa. 1901. Pp. 208.

This book is of interest to students of philosophy because of its tribute to the late Professor James Simmons, to whom it is dedicated. A paper called 'Education According to Nature,' written by Professor Simmons, is included in it. Other essays, to which it may be well to refer, are on 'The Usefulness of Art,' 'The Study of Society,' 'The Elements of Enjoyment in Music,' 'Utility as a Criterion of Being' — all by professors of Iowa College.

J. M. B.

The Founder of Mormonism. I. W. RILEY, with an introduction by Professor G. T. Ladd. A psychological study of Joseph Smith, Jr. New York, Dodd, Mead & Co. 1902. Pp. xix + 426.

The Force of Mind, or the Mental Factor in Medicine. ALFRED T. H. SCHOFIELD, M.D. Philadelphia, Blakiston. 1902. Pp. xiv + 309.

Personal Idealism; Philosophical Essays by Eight Members of the University of Oxford. HENRY STURT, editor. London and New York, Macmillans. 1902. Pp. ix + 393.

A volume devoted to what the authors call 'Personal Idealism,' from the general tendency of all the essays to emphasize personality as a fundamental category in philosophical construction. The essayists comprise Stout, Schiller, the editor, and others less well known. We hope to print a review later.

The Philosophy of Religion in England and America. ALFRED CALDECOTT. New York, Macmillans. 1901. Pp. xvi + 434.

A general classification, résumé, and criticism of writings on the philosophy of religion. The author's scheme is extremely comprehensive, dealing not only with works purely philosophical and psychological (the latter less effectively), but also with those which may properly be called theological. It covers ground which has not been gone over in the same systematic way before. We notice particularly the attempt to include American writers as well as English, the appendices being devoted especially to three American writers, who were evidently unfamiliar to the author at the time his main manuscript was composed. J. M. B.

Nineteenth Annual Report of the Bureau of American Ethnology, Parts I., II., 1897–1898. J. W. POWELL, Director. Washington, Govt. Pr. office. 1900. Pp. 1160.

This report contains a long essay on 'Esthetology' (the 'science of activities designed to give pleasure') by the lamented Director Powell, which will be valued as one of his latest writings. Dr. W. J. McGee contributes a paper on 'Primitive Numbers,' and Dr. Cyrus Thomas one on 'Numeral Systems of Mexico and Central America.'

The Principles of Logic. H. A. AIKINS. New York, Holt, 1902. Pp. x + 489.

L'Association des Idées. E. CLAPARÈDE. Bib. Intern. de Psych. Expér. Paris, Doin, 1903. Pp. 426. 4 fr.

An Essay on Laughter. J. SULLY. London and New York, Longmans, 1902. Pp. xvi + 441.

Can Telepathy Explain? M. J. SAVAGE. New York and London, Putnams, 1902. Pp. xvi + 243.

In their advertisement the publishers say: "Dr. Savage * * * here states a great number of well-authenticated instances of apparently spiritistic revelation and communication." In the preface Dr. Savage says: "I frankly confess that I am strongly inclined to hold the belief in continued personal existence as capable of proof and in the possibility of at least occasional communication." In the appendices he gives 'some [favorable] opinions of well-known men'; 'a partial list of names favoring * * * occasional communication' (good, bad and indifferent, *e. g.*, Joseph Jefferson, Queen Victoria, and 'the parents of General Grant'! Where is that humorist, Mark Twain?); 'a partial list of books * * * nearly all in favor of the spiritistic theory' (*e.g.*, 'Planchet, or the Despair of Science,' 'Gates Ajar,' and 'Fact and Fablein Psychology'!) J. M. B.

Annual Report of the Department of the Interior: Commissioner of Education, I. Washington, Govt. Ptg. Office. 1902. Pp. cxii.

This report contains the usual summary of educational progress for the year, by Commissioner Harris, and a valuable list of publications of the Bureau of Education from 1867 to 1902 (with indications as to which of the titles are out of print). The body of the Report is made up of documents relating to education at home and abroad.

Lehrbuch der Psychologie. F. Jodl. Second Ed., 2 vols. Stuttgart and Berlin, Cotta'sche Buchhandlung, 1903. Pp. xx + 435, and x + 448.

NOTES.

We are informed by Professor W. R. Sorley, of Cambridge University, that Professor Robert Adamson has left a considerable body of manuscripts, which he is editing for publication. He expects that there will be two volumes, chiefly psychological, entitled, 'The Development of Modern Philosophy.'

We notice the appearance of the first number of the new *Hibbert Journal* — a review devoted to philosophy and the philosophical treatment of religion. Among the coöperators, the name of Professor Howison, of the University of California, appears. The journal is published by Williams & Norgate.

Parts I. and II. have appeared of the *Journal für Psychologie und Neurologie*, which is a continuation, beginning at Volume XI., of the *Zeitschrift für Hypnotismus*. The managing editors are Professors August Forel and O. Vogt. It is published by Barth of Leipsic.

It is announced in the press that Professor W. Knight has resigned the chair of Moral Philosophy in the University of St. Andrews.

The Carnegie Institution has granted $1,600 to Professor E. W. Scripture, of Yale University, for prosecution of researches on the voice.

We are informed that the issue of Wundt's *Philosophische Studien* has been closed with Vols. XIX. and XX., which were dedicated to Professor Wundt on his seventieth birthday. This journal will appear in the future under the title *Archiv für die gesammte Psychologie*, under the direction of a board of editors comprising, besides Professor Wundt himself, Professors Külpe of Würzburg, Kirschmann of Toronto, Kraepelin of Heidelberg, Meumann of Zurich, and Dr. Wirth of Leipzig, Professor Meumann being Editor-in-chief.

Owing to delays in the printing of *Studies from the Harvard Psychological Laboratory* (No. 17 of the Monograph Series of the Review), Monograph No. 19 has been sent out without waiting for the appearance of the preceding numbers. It is expected that Monographs 17, 18, and 20 will appear in due succession; they will be sent to subscribers of the Monograph Series as they are ready.

Vol. X. No. 2 March, 1903

The Psychological Review.

PSYCHOLOGY AND PHYSICS.[1]

BY PROFESSOR E. C. SANFORD,
Clark University.

The honor that you have conferred upon me in electing me to the presidency of the Association is one of which I am very sensible, and one that I shall not misprize to the extent of addressing you upon the details of experimental matters with which so far I have been chiefly engaged. I have therefore chosen as my topic some of the more general aspects of psychology and its relations to one of the other sciences, and given my paper the title that stands upon the program.

I have two chief points to make: (1) That psychological theory is influenced to a large, and perhaps at times to an embarrassing, extent by points of view and forms of expression derived from the physical sciences, and (2) that even in spite of this fact psychological theory is, and perhaps cannot well help remaining, distinctly anthropomorphic in its nature.

First then as to the influence of physics on psychology. A part of this influence is due not so much to the science of physics as to the fact that man dealt long with the outer world before he made much progress in introspection, and even now spends a considerable portion of his time in that sort of commerce. As a result his habits of thought are in large degree objective, and the language that he uses is saturated with physical connotations and metaphors. One need only recall the broadly material character of most of the forms of popular

[1] President's address, American Psychological Association, Washington meeting, December, 1902.

spiritualism to perceive what tricks this may play with the unwary. And indeed it is not easy for even the best of us to keep clear of this inveterate physical-mindedness and the subtle suggestions of language. We help out our thinking by material figures and feel a sort of dumb compulsion to make our psychological theories accord with physical requirements. Ebbinghaus notices the first of these tendencies in his address on the past hundred years of psychology before the last International Congress.[1] After describing the older psychology as distinctively 'mechanistic,' he continues: "Mental phenomena are connected with material processes; in order, one may suppose, that the difficult exposition of the first may take its bearings from our familiarity with the second, in other words, from the natural sciences. Of these Physics and Chemistry first won an assured completeness of content and method, and thus many physico-chemical analogies came to be influential in the older psychology. The return of images passed for a phenomenon of like nature with inertia, association was set parallel to attraction, the ego became an aggregate of ideas, spatial perception a chemical combination of sensations." This state of things continued in the ascendent, though not uncriticised, well on into the century, as witness the works of the two Mills, and the Herbartian mechanics of ideas; and even now "the color theory of Helmholtz and Wundt's space theory are comprehensible only on the basis of an essentially physical manner of thinking. Perhaps the last remnants of such manipulation of things by means of mechanical categories is even yet not overcome, but in essentials one may venture to say that Psychology has outgrown it. We realize that the soul is connected with the body, in particular with the nervous system. If material analogies and explanations are used for mental combinations they are drawn from the special elaborations of physico-chemical processes that we call physiological or biological. Here chiefly it is the notion, originated by Darwin and Spencer, of a development of the mental life, both as a whole and in its single aspects, like that of biological evolution, that has become the controlling and fruitful one for us."

[1] 'IVe Congrès international de Psychologie,' Paris, 1900, pp. 58 f.

Ebbinghaus is right, no doubt, and most psychological investigators are now sufficiently independent, at least in the immediate conduct of their investigations and in the extraction of their first generalizations. In the discussion of psychological theory, however, and especially in the borderland between Psychology and Philosophy, the influence of other sciences has not wholly disappeared, but on the contrary seems often to remain to make trouble for those who cross the border from the psychological side. I have in mind particularly the doctrine of the psychophysic parallelism and the assumptions upon which it rests. I do not propose to discuss this in detail — especially as to-morrow's program promises the treatment of it by several of our colleagues most expert in such questions — but I would ask you rather to review the field with me from an exclusively psychological point of view, and to see whether a restatement of the problem in purely psychological terms does not throw some light upon it.

Before beginning, however, let me beg your indulgence while I try to anticipate two possible misconceptions. First, in what I am going to say I have no thought whatever of disparaging the other sciences or of belittling their importance or even their helpfulness in psychological investigation. On the contrary I hold them in the highest estimation, and believe the legitimate debt of psychology to them to be enormous. The criticism, if any is implied, lies against psychology and psychologists, not against other science or scientists. And second, I have no thought of recommending a new point of view from which to prosecute psychological investigation. Quite the contrary. I do believe, however, that a purely psychological point of view is a valuable one to take upon occasion in considering questions of psychological theory and of the interrelations of Psychology and the other sciences. As a traveler through the woods may now and then find it advantageous to climb a tree and look about him to discover the general direction of his path, or even for the mere enjoyment of the aspect of things from a new point of view, and yet would not, except under dire necessity, attempt to continue his journey like a monkey or squirrel direct from tree to tree, so I beg you to take for a few moments a position from which things may be viewed in somewhat different relations from that in which they appear in the paths of ordinary reflection and investigation.

Our common working position is that of a double series of phenomena, a physical series in the world about us and a psychical series within us. Höffding divides the universe into two parts and gives one to Physics and one to Psychology.[1] "These two provinces," he says, "include everything that can be the subject of human research;" and psychologists generally, when in a working and not in a critical or contentious frame of mind, would raise no objection. When, however, we take the exclusively psychological point of view, we shall want to say that Psychology, as the science of conscious states and processes, has to do with all experiences of every sort whatsoever. Even those aspects of experience that we commonly call physical, if they are noticed at all or become in any degree matter for reflection, take their place *ipso facto* among conscious experiences and thus become appropriate matters for psychological investigation. From this point of view Höffding was much too modest; he should have claimed the universe entire. The other sciences then become but separate departments of psychology, sciences of special aspects of conscious experience. Physics is engaged in discovering the uniformities running through the group of psychical experiences that have to do with bodies in movement and at rest, with heat, light and electricity; Chemistry in discovering those that have to do with the composition of bodies, the nature of the elements and the laws of their composition; Biology with the group of experiences connected with living organisms; and similarly with all the other sciences of nature. Mathematics and Logic are still more intimately psychical, dealing as they do with certain actual or possible forms of thought in symbolic representation. All the phenomena dealt with by each and every one of the sciences, their laws and general formulas, and the ultimate philosophical conceptions to which they lead, are one and all conscious experiences and as such fit matter for psychological examination.

[1] 'Outlines of Psychology,' London, 1893, p. 1.

This is not idealistic metaphysics, though I confess that there are certain resemblances, and that an idealistic philosophy follows as naturally from an exclusive contemplation of psychical facts as a materialistic one from a like concentration upon the facts of the physical sciences. It is not metaphysics at all, but a simple description of the facts. Conscious states exist; they are all of them, and no others, the subject matter of psychological science, at least in the broad sense in which we are now using the term. What I am saying holds equally well with materialistic, idealistic or dualistic conceptions of ultimate reality. I am dealing exclusively with conscious experience, and such an exclusive point of view is equally possible with all. Furthermore, the legitimate distinctions between the sciences are not obliterated by regarding them in this way. All differences remain as from the beginning except that their terms of statement are changed. Even the difference between Psychology and Physics is not removed, as I shall hope to make clear presently.

But first let us look a little closer into Physics from our ultra-psychological point of view. The conscious experiences with which Physics starts (physical phenomena, as we commonly call them) are percepts or series of percepts belonging chiefly to the sense fields of sight, hearing and touch, including under the latter the kinæsthetic senses as well as pressure, heat and cold — the senses that mediate the 'life of relation' with the world outside our own bodies. For convenience I shall call them the 'physical group' of senses. Certain characteristics of these senses can be traced through all the elaborations and abstractions of physics up to and including the most remote. Taste, smell, pain, the general and organic senses — all having little external reference — are not mentioned at all in physics, or at most incidentally, as supplying certain supplementary data. And even among the physical senses themselves all are not of equal importance. Theoretical physics tends more and more to construe all phenomena in mechanical terms; and sound, heat, light and electricity are studied as varieties of wave motion. The special sense qualities fall into the background and the phenomena are restated in the terms derived from the senses capable of perceiving matter in motion, the kinæsthetic and a part of the dermal senses and vision in its spatial function. No physicist would be materially hindered in the prosecution of researches in any branch of his science by the lack of smell, taste, pain or the general and organic senses. One without kinæsthetic and dermal senses, on the other hand, would find the science itself well-nigh inconceivable. This only means that for physicists as for other men these senses are those of last resort in matters relating to the external world.[1]

But the sensory basis of Physics is not the science itself. The science is the superstructure of generalizations, laws, hypotheses and theories, which have arisen in the effort of physicists to reach a simplified conception of the experiences occurring in the sense fields just mentioned. These in their finished form are generalized verbal descriptions of physical phenomena (*e. g.*, "every particle of matter in the universe attracts every other particle," etc.) accompanied in some cases, perhaps, by a more or less typical image. In this verbal form they are conveniently remembered and serve many useful purposes. They represent nothing more, however, than the final stage of a process of abstraction from phenomena actually observed or reported, and, as Ribot so ably demonstrates,[2] owe what value they possess to the possibility of a return from them to concrete phenomena. The concrete phenomena to which the physicist returns when he is pressed to make his law of gravity concrete are a group or sequence of images derived from the physical senses. He imagines again the falling ball, the swinging pendulum, the moon and planets in their orbits — whatever may at the moment best serve his purpose — or he actually sets up his apparatus and gives ocular demonstration of the law. He drops the ball and measures the spaces passed over in equal times, proving in the particular case the steady acceleration due to gravity. From our ultra-psychological standpoint once more the original phenomena are all of them percepts or series of percepts; and the more or less abstract formulations, leading

[1] Physiology, which is really a special branch of Physics and Chemistry, has to do with these senses and all the rest in a somewhat different way from that which we have been considering; to that I shall refer later.

[2] 'Evolution of General Ideas,' Chicago, 1899, p. 110 and elsewhere.

up to the concise verbal statements of physical laws, are convenient symbols, standing for the original perceptive experiences. A physical law and its possible concrete examples is a sort of fixed associational complex, or, as Charles Peirce might have called it, a habit[1] — or as we might say, a special habit of thought with reference to conscious experience of a particular kind.

This is true not only for the ordinary laws of physics but also for the still more general and abstract principles of the permanence of matter and the conservation of energy, for these differ from the ordinary laws in nothing but their abstractness and in the tremendous sweep of their application. They exist in the mind of the physicist as an abstract form of words. If he is pressed to make them concrete or if he applies them, they show nothing but certain coincidences or groups or sequences of sensory images or percepts. The permanence of matter means only that as a result of a certain series of percepts the same original percepts with which we set out may be reached again. We hand a chemist a silver dollar. He dissolves it in acids and forms a solution from which he presently obtains precipitates. These he treats variously until at length he presents us with a little button of silver and another of copper which we can take to the mint and get recoined as a silver dollar again. In the same way the conservation of energy means that at the end of a certain series of percepts the same grouping of percepts from which we set out may be reached again — the same quantitatively in all particulars of state and arrangement.

And the same is true finally with the ultimate matter and energy themselves. Matter has the qualities of impenetrability and extension and betrays by its conditions and behavior the presence of various forms of energy. These qualities are allowed it because they characterize the original physical percepts from which the concept is derived. They are the skeleton that remains after all non-essentials have dropped away. Even if the concept is still further refined and atoms are conceived as without substance — mere centers of attraction

[1] 'Illustrations of the Logic of Science,' *Popular Science Monthly*, XII., 1878, p. 291.

and repulsion — these very attractions and repulsions are so accorded them that they give rise to the original sensations underlying perceptions of extension and impenetrability. And energy in its various forms: Kinetic and potential, heat, light, energy of molecular condition, radiant energy, electrical and magnetic energy — all of them betray their presence by the changes they produce in matter. In other words they are abstractions from experience of various sorts within the field of the physical group of senses — abstractions that have proved useful in the effort to unify these experiences. Indeed I may conclude as I began, by declaring that from the ultra-psychological standpoint Physics is a most elaborate development of one aspect of the Psychology of the dermal and kinæsthetic senses — a most interesting and valuable study of fixed associative groups of a limited sensory origin.

It is time that I justify my remark that this ultra-psychological point of view does not remove the legitimate differences between Psychology and Physics. The method of Psychology is in its broad outlines not essentially different from that of Physics. Its phenomena are conscious experiences of various sorts, including all those with which Physics sets out, but also along with them experiences involving pain, organic and general sensations, feelings, emotions, memories, images, volitions, processes of reasoning — indeed everything in experience at all. The generalizations of Psychology, like those of Physics, are abstractions of varying degrees of refinement, generally used in verbal form, but convertible on demand into terms of concrete experience. The characteristic difference lies in their standpoints or attitudes. Even in dealing with the same original experiences with which Physics deals, Psychology selects somewhat different aspects and looks for a different set of uniformities. These different standpoints or attitudes are themselves, in psychological terms, differences in the direction of attention with all that implies.

The mighty gap which has sometimes been assumed to separate the realm of Physics from that of Psychology is thus seen to be of no very extraordinary nature. It is an arbitrary separation made for convenience and not worthy to excite special admiration. One might as well express surprise over not being able to pass by insensible gradations from *A* to *not-A*, or from one territorial jurisdiction to another, as over finding no place of transit by easy stages from things physical to things mental.

A second point of difference is the greater variety of experience covered by Psychology, Physics dealing with what we call outer experiences only, Psychology in our present sense with both inner and outer. As I have tried to show, Physics comes at last to work with terms derived exclusively from the kinæsthetic and parts of dermal and visual experiences. Generalizations derived from these sense-fields can never fully explain experiences belonging to other fields, because of the difference in sense modality. One cannot describe or explain a taste in terms of vision nor general sensations in terms of moving particles. The same is the case with the more complex experiences involving sensations not of the physical group.

A third difference — not an essential one, but one fraught with manifold practical consequences — is the greater simplicity of physical experiences. This gives them a great hold upon the imagination and is one of the reasons for the dominating influence of the physical sciences, of which I spoke in beginning.

Psychology itself, as it appears from our ultra-psychological point of view, deserves a little consideration. In many cases Psychology is able to trace fairly well-connected sequences in its special aspect of experience, *e. g.*, in the members of a practiced train of mnemonic associations; but in others it is not able to do so. Among experiences of all grades instances occur in which, so far as we can see, the antecedent condition does not at all account for the phenomenon that develops. Let us take some concrete examples. I see a boy touch a lighted match to a rocket, which presently goes off with a hiss, and leaving a train of fire, bursts in a shower of stars which sink and disappear. In such a series there is no break. The statement from our present point of view would be simply that we had experienced a series of percepts beginning with the touching off of the rocket and ending with the disappearance of the falling stars. The psychical series of experience differs from the physical series only in the aspect regarded, and each member of the series is a sufficient reason for the presence of the next. The case is not so simple when attention is otherwise directed and the new sensation bursts in upon an unrelated series — if, for example, when seated in revery, I hear a sudden explosion out of doors. There is now nothing in the conscious train to introduce the sudden sound; as far as that is concerned, the sound is wholly unaccountable. To leave it unaccounted for is to admit just so much chaos into the mental world, and we therefore relate it forthwith to some other order of occurrences independent of our consciousness, in which it may be lodged safely between antecedent and consequent—in other words, we assume the physical series.

It is not absolutely necessary that we identify this needed independent series with the series coming to us through the physical group of senses — we might assume some sort of an 'unconscious'; but the intercurrent experiences generally come to us through one or the other of the physical senses; the physiology of the senses makes the connection seem close; and we make it readily because at other times we have followed with attention a full series that led up to a similar explosive sound; we have seen the boy touch a match to the fuse of the fire cracker, have seen the spark creep along the fuse and finally heard the report. We assume that something similar has happened again, though we were not there to see, and we say the sound was due to an external stimulus; and, extending the idea, arrive finally at an independent external world constantly in existence and frequently furnishing us with sensations by means of stimuli administered.[1] This assumption is, as I have said, in the nature of a theory or hypothesis, like the atomic theory or the hypothesis of an ether, and has the same sort of justification — the only possible justification, other than immediate conscious experience — to wit, that it harmonizes an immense number of facts and has not met an irremovable exception.

In an entirely similar way, wholly unrelated ideas at times break into our trains of thought or revery. Often in such cases

[1] I am here speaking of the logic of the thing, of course. As a matter of fact the whole system of the outer world is built up and in use in the mind of the child long before he ever introspects or takes consciously any theoretical attitude toward his experience at all.

more careful examination shows some unnoticed association or intruding sensation to account for the break, but there are some cases where no such explanation is discoverable and they remain inexplicable in psychical terms. In such cases we find it necessary to appeal once more to the activity of something outside the conscious series, and we speak of 'unconscious cerebration' or simply of 'the unconscious.' In the case of these higher psychical processes our knowledge is less perfect and the connection with the physical series is less patent; consequently we feel the compulsion less strong to identify our hypothetical unconscious series with the physical series. As our knowledge of cerebral physiology increases we shall probably find the tendency increasingly greater, and we shall speak less of 'the unconscious' and more about 'unconscious cerebral processes.'

Let no one think we have a quarrel against these assumptions. We have none; they are useful and we are forced to make them. We only desire to recall once more that the physical series which they assume is based upon the physical group of sensations and has been elaborated according to the usual psychical processes of perception, association, abstraction and generalization.

What we have just been considering has brought us to the problem of the relation of mind and body, one which I believe has been obscured — at least as far as psychological theory is concerned — by the preponderance of forms of statement and points of view derived from the physical sciences.

The problem as it appears from our purely psychological standpoint is simply this: How can we best utilize the physical series that we have been obliged to assume? And the natural answer seems to be the assertion of some sort of interaction. The physical series must influence the psychical series or it will not fulfil the purpose for which it was assumed. And the psychical series must influence the physical, if we are to explain the cases of voluntary movement, where we can follow distinctly a psychical series leading up to a predictable physical result. For psychology as a natural science, the hypothesis of interaction seems quite sufficient.

That this seems to run counter to the physical principle of the conservation of energy, is in itself no greater argument against the theory of interaction — provided that that theory is otherwise the best from the psychological point of view — than it is against the principle of the conservation of energy itself. That principle was reached from the consideration of a certain group of facts; it is by no means necessary that it should apply when other facts are brought into consideration. It is possible that we may in the end strike some form of conception that shall show that the theory of interaction contains no real denial of the principle of the conservation of energy — perhaps on the lines of the 'genetic modes' of our colleague, Professor Baldwin,[1] or along the line suggested in Dr. Bawden's paper before the Philosophical Association,[2] perhaps in some one or other of the other suggestions that have been made. Bnt in the meantime it seems to me a crying example of the over-reverence for physics of which I am speaking, that a physical principle should be cited as an objection to a psychological one otherwise valuable.

The current answer to the problem is, as everybody knows, not interaction, though that has been supported by the brilliancy of both James and Stumpf, but that of the psycho-physic parallelism in its various forms. That this theory, which is surely not so natural and simple as interaction, should be the current theory of the day is not to be accounted for by reasons lying in the province of psychology. The mere assertion of the parallel (with the implication that the two series are independent) and no further explanation of the nature of the relation, is distinctly inferior to interaction. It makes necessary the assumption of all kinds of unconscious actions, or interferences of some sort, to account for the above-mentioned irregularities of sensation and reflection, while it gets no corresponding advantage. Very often the parallels are not thought of as independent, but a greater permanence and reality is attached to the physical side. It is asserted that there is no psychical action without action in the

[1] 'Development and Evolution,' N. Y., 1902, pp. 300 ff.

[2] 'Proceedings of the First Meeting of the American Philosophical Association, 1902,' *Philosophical Review*, XI., 1902, p. 270.

higher cerebral centers, but the counter proposition, that there is no action in the higher cerebral centers without psychical action, is denied. The currency of the theory in this form is due, I believe, to the glamour of the physical sciences. I agree entirely with our colleague, Professor Münsterberg, that the determination of standpoints on this question rests upon extra-psychological considerations,[1] and I believe that it is in the predetermination of these considerations that the influence of the physical sciences is most exercised.

So much for my first point, to which I have perhaps given too large a portion of my time. My second was that Psychology is an essentially anthropomorphic science (and I should say here, parenthetically, that I am now using the word Psychology in the narrow and ordinary sense, and the word 'anthropomorphic' in contra-distinction to mechanical). I doubt if any one would contest the proposition. Just as Physics retains to the end certain characteristic features of the sense fields in which it originates, so we may expect Psychology to bear the marks of its derivation from conscious experience as we know it. In normal adult psychology the anthropomorphism is too much a matter of course and too little disadvantageous — indeed too fully justified — to be much noticed, but one has only to open his eyes to realize its anthropomorphic character. This indeed might not be worth notice were we not tempted beyond the bounds of introspection to enter upon comparative psychology. Are we justified in using our own psychical experience as a basis of interpretation with reference to the behavior of animals, very little children and the feeble-minded? Were it not better if we declined to go beyond the immediate facts of observation and were content with a purely objective science of animal or child or idiot behavior? Such a course might be possible, but I doubt if any one has ever seriously contemplated it in the case of the higher animals, or could carry it to fruitful results if he should undertake it. Nor would any one seriously propose to treat the behavior of his fellow men in the same way, *i. e.*, to refuse to credit them with conscious experience in the main like

[1] *Grundzüge der Psychologie*, Bd. I., 402 ff.

his own, though this would seem to be required logically, unless one can show an essential difference in the conditions under which he judges of human and animal activities. The fact is that as soon as actions become at all complicated the concept of mechanism breaks down. It does not simplify the phenomena, nor unite them without violence with other groups of phenomena. It loses therefore the only justification which it ever had, namely its usefulness. Another concept and this the anthropomorphic one (*i. e.*, interpretation in terms of human experience) makes the facts observed more intelligible, and enables us to group them in larger unities and thus establishes its propriety. Our justification in assuming a psychic life of some kind in animals or little children or low idiots is of the very same nature as our assumption of a material world which furnishes the excitants of sensation, or of the mechanical principles that govern its phenomena — of the same kind, though less certain of course, because the facts covered are more complex and less easy to observe.[1]

I trust that no one will understand me as holding that because we must cast our conceptions of the psychic life of animals in human terms we need do so rawly, and in the identical terms in which we experience our own. By no means. Exactly as the theoretical mechanics of the physicist is not the mechanics of any particular body, but an abstraction of the features common to all mechanical phenomena, so our ultimate psychoses must be the fundamental ones common to all conscious experiences. Just what these ultimate psychoses ought to be we are perhaps not fully ready to say, but we seem to require a sensory awareness, colored by original liking or disliking and a tendency of these sensory impressions to remain for a brief period. This simple consciousness must also be effective in some way in altering existing tendencies to movement.

How far down in the animal series this shall be assumed to go is purely a question of utility in thinking, and can be settled

[1] It is perhaps worth noting in passing that those who argue from experiments on animals that cerebral changes induce psychical changes, are not in a position to deny the existence of psychical experiences to animals.

only by gathering facts and trying to explain them. It will be conceded that the hypothesis of some sort of mind in animals is required at the upper end of the scale; it is equally conceded that a mechanical explanation may suffice at the lower end of the scale — though conservative biologists would perhaps regard it as probable rather than proved. Usefulness in thinking must decide where the line shall be drawn, or if the two series—psychical and physical—must be assumed to act conjointly throughout.

This assumption of mind in animals almost inevitably draws after it some form of belief in interaction. It is hard to believe that consciousness has not been a helpful possession in the evolution of man and higher animals, and if so it must have made his behavior different from what it would have been if consciousness had not existed. I do not insist upon this point, however, lest some one accuse me of introducing Biology into psychological theory when I cast out Physics.

This anthropomorphic character of Psychology stands in sharp contrast with the mechanical tendency of Physics. In the wider conception of Psychology which I used in the first part of my address, the mechanism of Physics would have to be included as a partial aspect of the general anthropomorphism but with the narrower and current definition of the science the distinction is radical. The ultimate perfection of psychology is reached in an abstract consciousness in which state follows state according to known laws, that of physics in an abstract mechanism in which everything is formulable in terms of moving particles. Neither anthropomorphism nor mechanism, though growing steadily more refined and abstract, can lose its essential quality and yet serve its purpose. The former must carry the marks of its derivation from human introspection; the latter of its origin in the physical senses. For this reason no attempt to explain experience in physical terms can ever be thoroughly satisfying, nor one that tries to get rid of the necessary assumption of the physical series. Any philosophical system that hopes to pass from the ultimate principles of science to a satisfying grasp of all phenomena must treat both these ultimates with equal favor.

A STUDY OF THE RELATIONS BETWEEN MENTAL ACTIVITY AND THE CIRCULATION OF THE BLOOD.

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The purpose of this investigation is threefold. The first problem concerns the changes in the rate and force of heart beat and the vasomotor fluctuations concomitant with agreeable and disagreeable sensations and affective states under conditions of mental acuity and fatigue. The second treats of these differences as connected with processes of intellection during the progression to a state of mental fatigue. The third is a consideration of the vasomotor rhythm, indicated by the Traube-Hering waves, in relation to the acuity of sense perception observed to recur in rhythmic regularity.

In consideration of the admirable historical review of observations relevant to the subject of this investigation made by J. R. Angell and H. B. Thompson,[1] a review of the literature would here be superfluous.[2] The results and conclusions of former investigators will be considered wherever it is desired to make statements of comparison.

I here take the opportunity to acknowledge my indebtedness to Dr. Edwin Grant Dexter, professor of education in the University of Illinois, under whose general direction this study has been made; to Dr. Stephen S. Colvin, assistant professor of psychology in the University of Illinois, and the several students who have acted as subjects for experimentation; and to Dr. John A. Bergström, of Indiana University, under whose di-

[1] Psych. Rev., VI., pp. 32–43.

[2] Publications since the issue of the above study make possible the following additions: Wundt, *Philos. Studien*, XV., 140. Wundt, 'Völkerpsych., I., II., 1900, 40 ff. W. P. Lombard and W. B. Pillsbury, *Amer. Journ. of Physiol.*, III., 186–201. A. Lehmann, 'Die körperlichen Aeusserungen psychischer Zustände, I., Plethysmographische Untersuchungen,' Leipzig, 1899.

rection preliminary experiments on phases of the same problem were made, which furnished the inspiration for the present investigation.

Apparatus and Experiments.

A continuous kymograph, highly adjustable with respect to speed, designed and made in the laboratory of the department, was used for taking tracings. A chronograph indicated a seconds time tracing. Marey tambours, to which were attached inking cups and capillary pens drawn from glass tubing, were used to produce the tracings on white paper. For continuous records this method has many advantages over smoked paper tracings.

Both the time-rate of the pulse and the vasomotor changes in the peripheral blood vessels were recorded by the use of the

FIG. 1. Apparatus used in taking simultaneous sphygmographic and plethysmographic tracings.

plethysmographic tracings, save in a few cases where sphygmographic records were taken directly from the carotid. The air plethysmograph was used. Most of the records were taken from the forearm, but a few were from the foot. For the tests involving fluctuations of attention, a tambour giving the tracing was placed in connection with a press bulb. The stimulations used were visual and auditory. An audiometer was used for the latter as much more satisfactory than the ticking watch; for visual stimuli the Masson disc and geometrical figures were used. Extended experimentation was required before the most suitable speed of kymograph, the most desirable temperature of the room, the best quality of rubber, the proper manipulation of the pens and the most satisfactory position of the body were secured.

The series of records upon which this paper is based consists of the results of extended and complete tests taken from twelve subjects. For the long records the subject remained in the chair with the apparatus attached for from forty-five to sixty-five minutes. For the tests in responses to emotional states and brief intellectual application, the subject was taken at a time of mental acuity, before the prolonged task was begun and again after the completion of the hour's vigorous work.

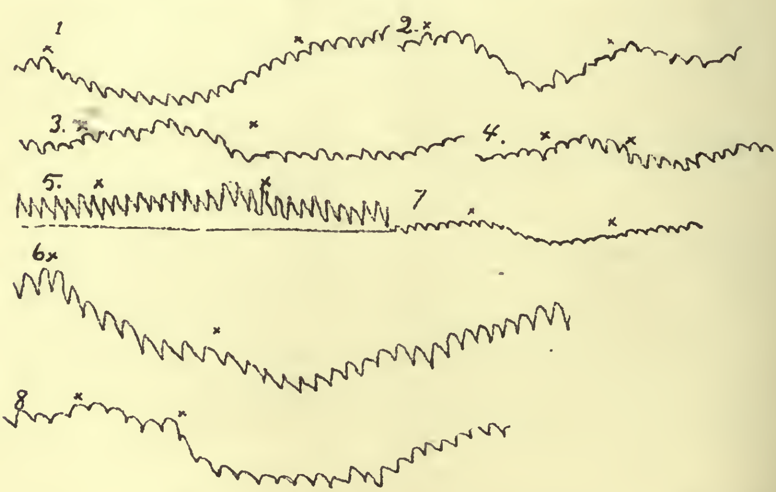

FIG. 2. Curves 1, 2, 3, heliotrope; 4, 5, crab apple; 6, 7, 8, carbon bisulphide.

Explanation of Figures. All curves read from left to right. The application of a stimulus or the solution of a problem begins at the point marked with a cross, terminating at the second cross. A drop in the curve indicates vasoconstriction; a rise, vasodilation.

Many records were taken from the twelve subjects, and also from others, which were not used. Wherever, through any error in adjustment or working of technique, question arose as to the validity of tracings, they were thrown out. Careful tests have assured me that the accuracy of the curves used may be fully relied upon as indicating changes in the rate and volume of the blood flow to the parts used in the experiment and to show the character and amplitude of the capillary pulse.

I. Affective States.

The emotional states here considered were all produced by agreeable or disagreeable odors. Curves 1 to 5 are tracings of agreeable odors; 6 to 10, of disagreeable. All these are plethysmographic except 5 and the upper curve of 9. In these the rate of heart beat during the emotion varies with the vasomotor changes of the peripheral blood vessels. Vasodilation is accompanied by an accelerated heart rate and vasoconstriction by a diminished rate. In every case constriction occurs, although constriction is preceded by dilation in curves 2, 3, 4, 11 and 13. This is most marked in 3, taken from B., and in 9 and 13, from D., these subjects invariably showing this effect, regardless of the character of the emotion. In subject B., constriction was never very evident, either in emotional or intellectual activity.

It has been held by Féré[1] that agreeable experiences are accompanied by dilations of the capillary blood vessels and disagreeable experiences by constrictions. Alfred Lehmann[2] agrees with this, and Wundt[3] has embodied Lehmann's conclusions in his *Völkerpsychologie*. No other investigators have found any foundation for these conclusions. I find that all emotional experiences are accompanied by constriction, either immediately or after brief dilation. For most individuals, normal vasomotor recovery is more rapid after an agreeable experience than after a disagreeable feeling. Compare curves 1 and 6.

It may be noted that the vasomotor activity of the peripheral vessels precedes, in some cases, the constriction or dilation of

[1] 'Sensation et mouvement,' Paris, 1887.

[2] 'Hauptgesetze d. mensch. Gefühlslebens,' tr. by Bendixen, Leipzig, 1892.

[3] 'Völkerpsychologie,' i, I., 1900, 40, ff.

the carotid. (See curves 9 and 18.) This suggests the question of control of the blood supply to the brain, a question as yet having received no completely satisfactory solution. The view of Mosso[4] that the blood supply to the brain is a condition and not a cause of psychic activity finds support in these curves.

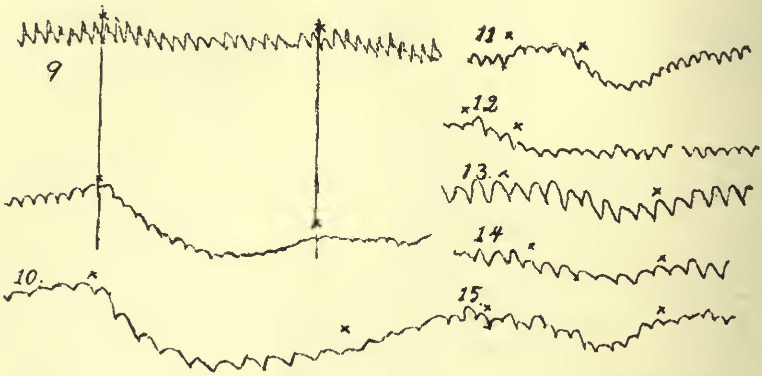

FIG. 3. Curves 9, 10, carbon bisulphide; 11, 13, carbon bisulphide before work; 12, 14, 15, same after one hour's work. Upper curve in 9, from carotid.

This question will be discussed further in the last section of the paper.

Curves 11, 12, 13, 14, 2 and 15 show the effects of fatigue upon the circulatory responses to emotional states. Curves 12, 14 and 15 are tracings for the application of the same stimuli as in curves 11, 13 and 2 respectively, after one hour's rigorous mental activity. The effects of fatigue are shown in the greatly diminished amplitude of pulsations, the diminished heart rate, the diminution in vasomotor response to stimulation, and the greater retardation in the recovery of the vasomotor level.

II. Intellectual Activity.

The tests employed in these experiments for intellectual activity were mathematical problems for the short periods, and memorizing or reading difficult subject matter for the prolonged records. Curves 16 to 21 are plethysmographic tracings for problems in addition and multiplication. In every one of these vasodilation precedes constriction, and in all the heart rate in-

[4] 'Die Ermüdung,' p. 195, ff.

creases during the period of vasodilation. In all excepting 16 the amplitude of the pulse curve is diminished. Curves 19 and 20 were taken from subjects M. and B. respectively, whose records for almost every test given of any kind failed to show any marked constriction, where it was shown being preceded by dilation. This was especially true of B., further shown in curves 27 and 28. It is to be noted, however, that the amplitude of the pulse curve is greatly reduced and the heart rate is quickened, indicating an increased blood pressure.

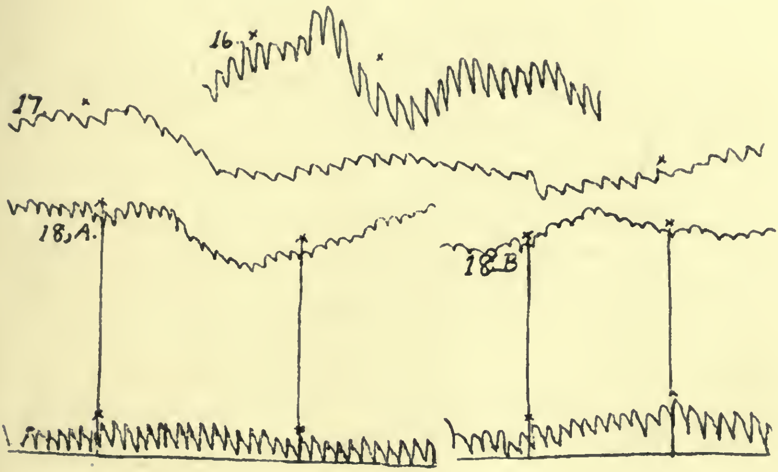

FIG. 4. Curve 16, addition ; 17, multiplication ; 18, both A. and B., addition ; in 18, lower curve from carotid.

Curves 22 to 24 are from the carotid. So also are the lower curves in curves 18, A. and B. All these excepting 23 show vasodilation and all of them show diminished amplitude of pulse curve, indicating an increased flow of blood under a greater arterial pressure. Curves 22 and 24 also show clearly that the dicrotism of the carotid pulse is much less acute. The dicrotic notch is almost obliterated during intellectual activity in curve 22. These results are quite the opposite of those of Gley[1] who found that mental work increases the amplitude of the carotid pulse and accentuates the dicrotic notch. Binet and

[1] 'Etude expérimentale sur l'état du pouls,' etc. Paris, 1881.

Henri[1] agree with the results here given in finding an increased blood pressure during intellectual activity.

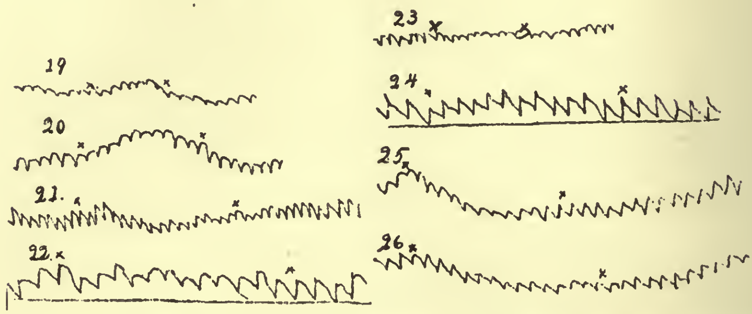

FIG. 5. Curves 19, 20, 21, multiplication; 22, 23, addition, from carotide; 24, multiplication, from carotid; 25, addition, before work; 26, same, after one hour's work.

The tracings for intellectual activity given by Angell and Thompson[2] show far less vasomotor response than do the ac-

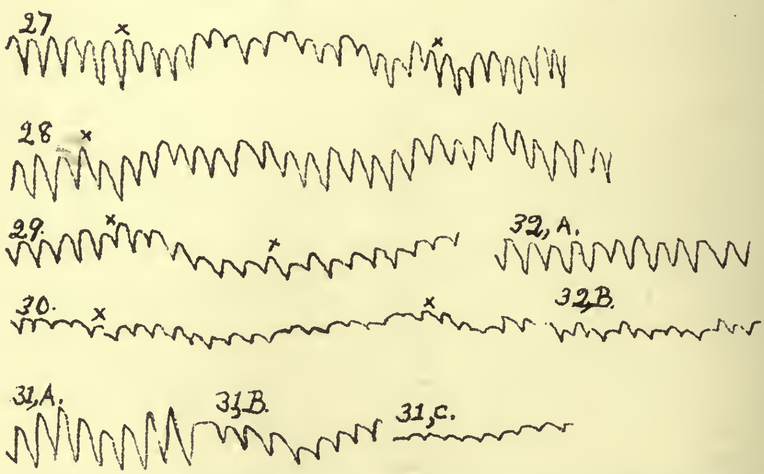

FIG. 6. Curves 27, 29, multiplication, before work; 28, 25, same, after one hour's work; 31, A., before work; B., in fortieth minute; C., in sixtieth minute; 32, A., before work; B., in sixtieth minute.

companying curves. They also point out a regularity and stability in the circulatory processes concomitant with intellec-

[1] 'La fatigue intellectuelle,' pp. 112, 113.
[2] *Ibid.*, pp. 61, 63, 64.

tual activity much greater than that observed for emotional activity.[1] The accompanying curves fail to show any such differentiation. These differences in observations emphasize the fact that there are great individual differences in subjects. The equivocal character of the literature on this subject may be very largely due to the individual variations, no previous investigator having employed a sufficient number of subjects to realize the necessity for caution in generalizing.

Curves 25 to 30 show the effects of fatigue on the circulation in its responses to intellectual activity. Curves 25, 27 and 29 represent short mathematical problems before work was begun by the subjects. Curves 26, 28 and 30 indicate problems of the same degree of difficulty by the same respective subjects after one hour's rigorous mental application. The subjects remained in the chair during the hour with the apparatus attached, a continuous tracing being recorded. In no cases are records here compared which were taken at different sittings. Not only are differences in physical and mental states in the subject so great as not to admit of comparison, but it is not believed that adjustments of apparatus are possible which will insure tracings susceptible of valid comparison as to absolute dimensions. In these three sets of tracings, fatigue is evident in the greater time required for the solution, in the diminished rate of heart beat, in the diminished amplitude of the pulse curve and in the reduced vasomotor response to the intellectual stimulus.

Curves 31, 32 and 33 are plethysmograms, and 34 and 35 are sphygmograms showing the progress of fatigue. Curve 31, A., at the beginning of work, B., in the fortieth minute and C., in the sixtieth minute, indicate the diminished heart rate and pulse amplitude through one hour's continuous work. Curve 31, A., before work, B., in the tenth minute, C., in the twenty-fifth minute and D., in the fortieth minute, show both the diminished pulse rate and amplitude, and the changes in the dicrotic notch through forty minutes of close mental application. Curves 34 and 35 show these diminutions in rate and amplitude, and more clearly the changes in the dicrotism of the pulse through forty minutes rigorous mental work. Fig. 7 indicates clearly the

[1] *Ibid.*, p. 67.

effects of fatigue on both the capillary pulse and the carotid, curves 34 and 35 each being carotid tracings, while 33 is from the forearm. The loss of acuteness in the dicrotic notch is

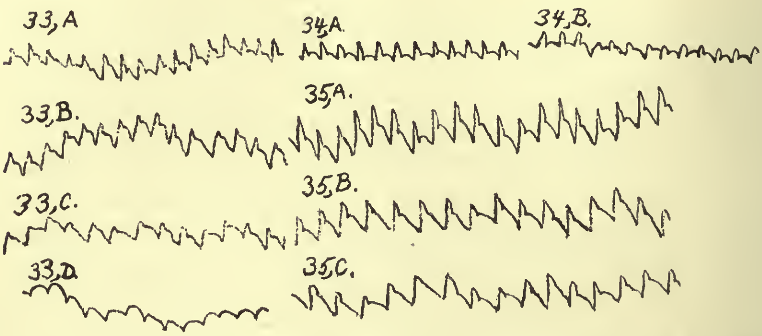

FIG. 7. Curve 33, A., before work; B., in the tenth minute; C., in the twenty-fifth minute; D., in the fortieth minute; 34, A., before work; B., in the fortieth minute; 35, A., before work; B., twenty-fifth minute; C., fortieth minute; 34, 35, from carotid.

plainly evident in both, but the anacrotic effects are most fully developed in the capillary tracings.

Figs. 8 to 11 give reduced records of plethysmographic tracings, showing both the heart rate and the vasomotor fluctuations through periods of from forty minutes to sixty-five minutes. The records show conditions at intervals of one minute. In these figures the numbers at the top and bottom of each line indicate in minutes the progress of the experiment. In every case the upper record in each figure is based upon the rate of heart beat. The lower record indicates the vasomotor fluctuations. The numerals at the right and left of the pulse-rate curves express the number of beats of the heart each minute; those at the right and left of the vasomotor record show the height of the curve in the tracing in millimeters.

Binet and Courtier[1] are the only observers who have made similar tests for long periods of time. Their experiments were with two subjects and covered periods of five hours. Tests were taken at intervals of about one hour, the apparatus being removed after the test and the subject retiring to an adjoining

[1] 'La fatigue intellectuelle,' p. 97.

room. The validity of the comparison of records taken under such circumstances is open to question. In continuous records the fluctuations vary from two to twenty millimeters in vasomotor level. Short tracings taken at intervals of an hour might show either extreme of these variations. The movements to the

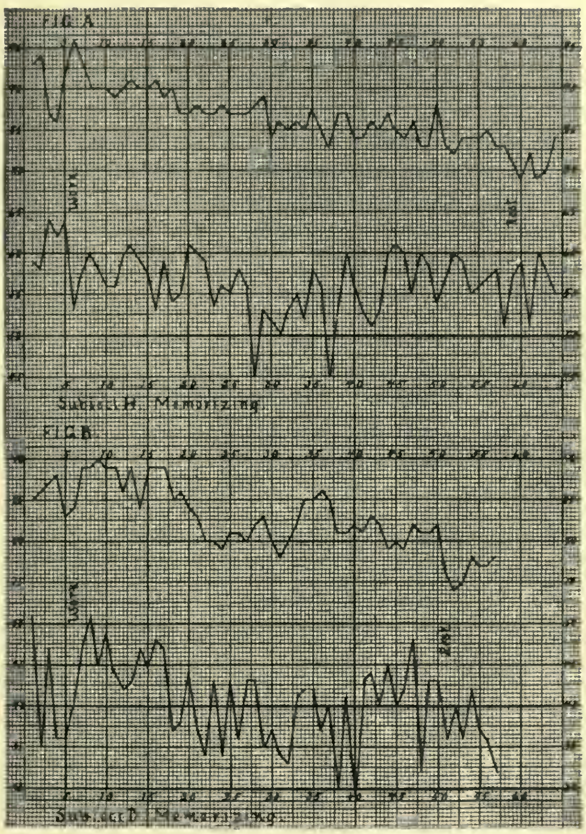

FIG. 8. Prolonged intellectual application. Upper curve in each figure for heart rate, lower for vasomotor changes. Numerals at top and bottom of each figure indicate minutes of progress; at the right and left of the upper record the heart rate per minute, of the lower record the height of the tracing in millimeters.

room and for the adjustment of the apparatus, together with the mental relaxation occasioned by these movements, would introduce possibilities of additional error, all of which are obviated by the continuous records. As to the rate of heart beat, two types of records are found. Those shown in Figs. 8, 9, and curve E, of Fig. 10, indicate the one form in which the rate progressively diminishes under intellectual activity, or diminishes for a considerable time, then remains approximately the same for the remainder of the hour. Fig. 8 shows the former

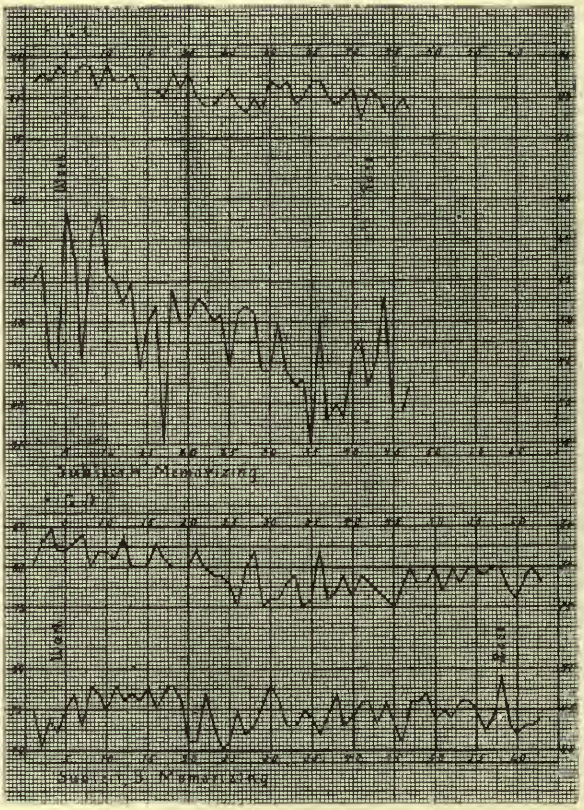

FIG. 9. Prolonged intellectual activity. For explanation see FIG. 8.

condition, curve D, of Fig. 9, the latter. The second type is shown in curve F, of Fig. 10, and in Fig. 11. Here also two forms occur, curve F illustrating the first, a general rise in the rate until the close of the hour. Fig. 11 shows the other form, in which there is an increased rate for a considerable period then a diminution.

In curves A and B, the fall is steady, after the short rise at the beginning, from 94 beats a minute to 80 in the former and from 89 to 76 in the latter. In curve D, Fig. 9, the rate drops from 82 to 75, then at the forty-sixth minute rises and remains

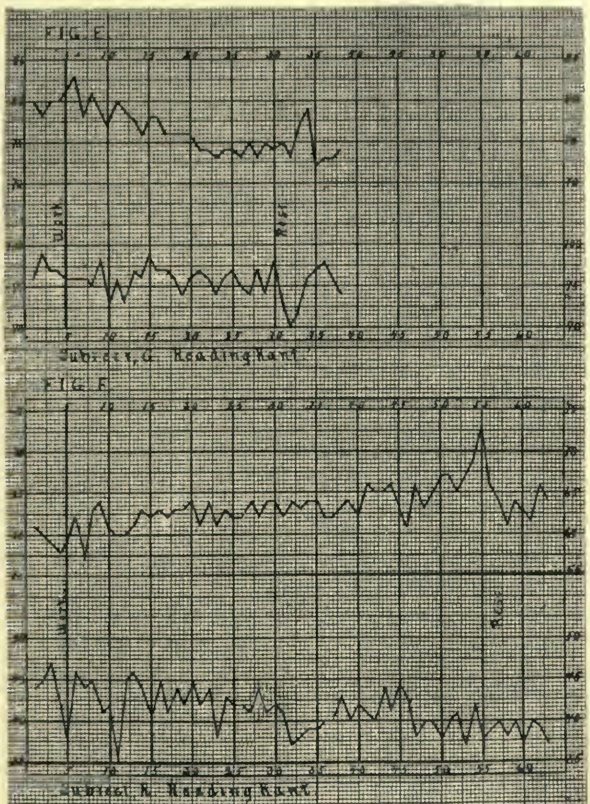

FIG. 10. Prolonged intellectual activity. For explanation see FIG. 8.

at or near 80. In curve F, Fig. 10, the rate rises from 58 to 65, takes a sudden rise to 73 in the fifty-fifth minute, then falls back to 63 at the close of the hour. Curve H, Fig. 11, shows a rise from 81 to 86 by the thirty-fifth minute, then a fall to 81 again by the end of the hour.

The fluctuations in the vasomotor records are much greater than those in the rate of heart beat. The records in Fig. 8 and curve C, of Fig. 9, indicate a type in which the vaso-constriction increases until about the fortieth minute, then slightly diminishes for the remainder of the period. Curve D, Fig. 9,

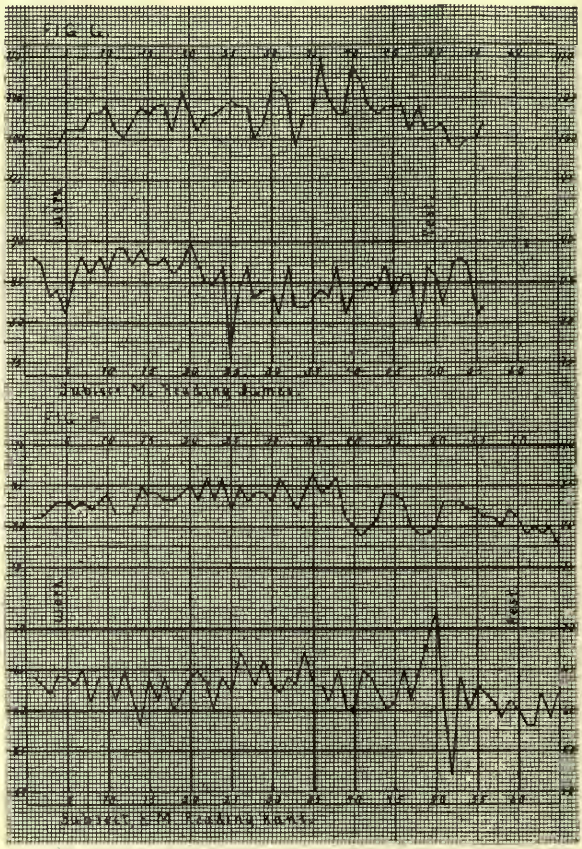

FIG. 11. Prolonged intellectual activity. For explanation see FIG. 8.

and curve H, Fig. 11, show the type in which there is no progressive vasomotor activity. In curve D, after a slight dilation, constriction bringing the curve down to the resting level follows, and slight fluctuations in both directions recur at intervals of from two to four minutes through the hour. In curve H, there is a gradual dilation up to the thirty-fourth minute, then a progressive constriction for the remainder of the period. In curve F, the constriction is continuous and progressive through the hour, the curve falling from a height of 44 millimeters to 37, as measured on the tracing.

Curves A, B and F show a tendency toward recovery in the increased heart rate, and curves A, C, G and H, in vaso-dilation, during the short period of rest recorded at the close of the intellectual labor. But recovery from such prolonged and intense application is too slow to be very markedly evident in the first few minutes of relaxation. Fluctuations in both the records for the pulse rate and for the vasomotor level are rhythmical, the periods varying from one to three minutes. There is also a correspondence observed between the two rhythms. In general, an increase in vasoconstriction for one or two minutes is followed by an increase in the rate of heart beat. This correspondence is confined to short periods of time, however, for most subjects. One record only is found where the parallelism is continuous and progressive through the long period, curve F, Fig. 10, showing this for fifty-five minutes

W. H. Howell[1] found that the blood pressure begins to diminish in a subject sitting for a sleep record as soon as he is comfortably seated, and continues to diminish for from an hour to an hour and one half.

Prolonged intellectual activity produces the opposite result. curves 31, 32 and 33 clearly indicate the progressive increase in blood pressure. How long it might continue to increase it is beyond the province of this investigation to state.

III. The Traube-Hering Rhythm and Fluctuations of Attention.

During periods of rest or after the subject had become absorbed in concentrated study, long undulations, covering the time of two or three respiration periods, appeared with great prominence and regularity in the tracings of a number of individuals. The time of these wave-lengths corresponds fully with the periods of the Traube-Hering blood pressure waves and the changes in amplitude of the parts of the wave indicate the blood pressure changes. Therefore the term Traube-Hering is be

[1] *Jour. of Exp. Medicine*, II., p. 319.

lieved to be valid as applied to these undulations and is used when they are referred to. Types of these waves from five

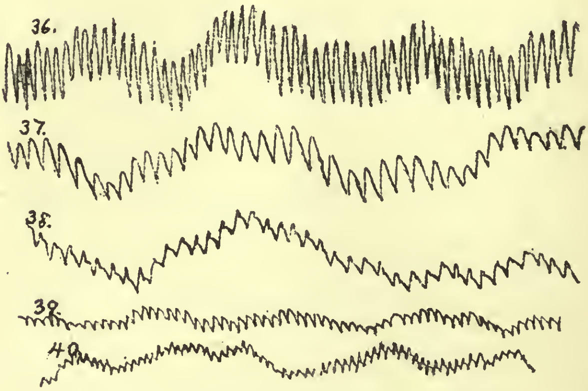

FIG. 12. Types of Traube-Hering Waves.

different subjects are shown in Fig. 12. The parallelism of the waves from the arm and foot is seen in Fig. 13.

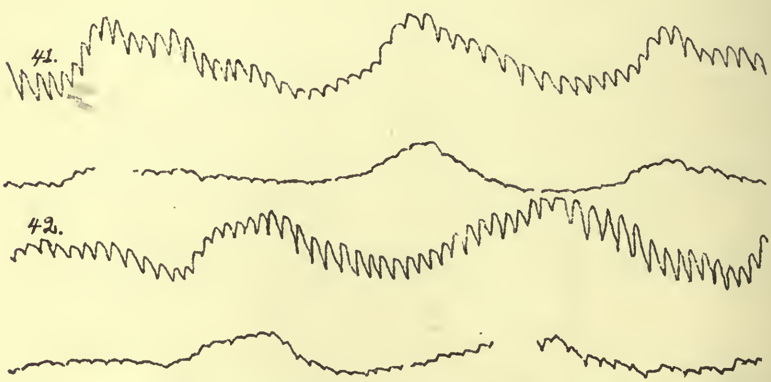

FIG. 13. Showing Parallelism of Arm and Foot Plethysmographic Tracings. Lower Tracing from the Foot.

The agreement of these wave lengths with the periods of rhythmic variations in the acuity of auditory and visual perception observed in the use of the Masson disc or the audiometer suggested that there might be a causal relation, or at least a constant correspondence between the two. Tests were made with the results shown in Figs. 14 and 15. Curve 43 is a tracing recording the appearances and disappearances of the

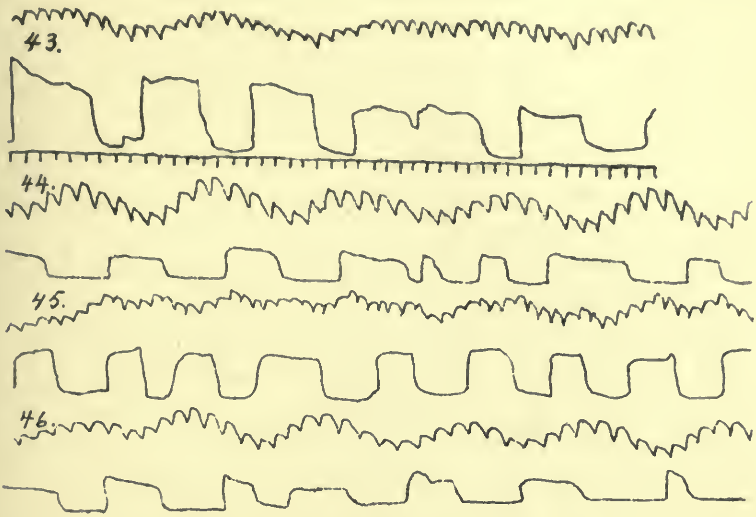

FIG. 14. Parallelism of Traube-Hering and Attention Waves. Curve 43, Masson Disc ; 44, 45, 46, geometrical figure.

gray ring in the Masson disc. Curves 44, 45 and 46 show the changing appearance of a pyramidal figure from convexity to concavity and *vice versa* by three different subjects, each con-

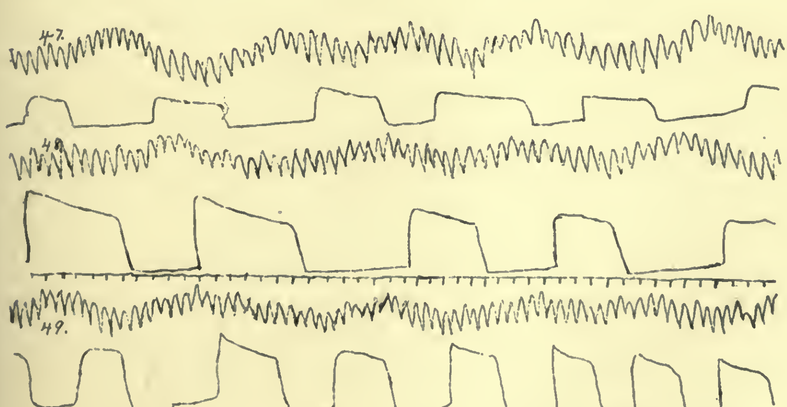

FIG. 15. Parallelism of Traube-Hering and Attention Waves. 47, Retinal rivalry; 48, Audiometer ; 49, Masson disc.

sciously attending to the convex appearance. Curve 47 is a tracing for retinal rivalry, attention being directed to the image on the right eye. Curve 48 is a tracing for the appearances and disappearances of sound by use of the audiometer, and curve 49 a record from the use of the Masson disc. In all of these tracings the crests of the attention waves indicate the appearances of the sensation, the valleys the disappearances.

The possible relation of the rhythmic fluctuations in the intensity of sensations to the Traube-Hering waves was expressed by Roy and Sherrington[1] in 1890, but, hitherto, no experimental evidence has been offered to establish this relation. While the waves do not always fully coincide, in general there is a very close correspondence. The crests of the attention waves are usually coincident with the valleys of the Traube-Hering undulations. Using the audiometer set at the threshold of auditory perception, the minimal sound appears soon after the period of vasoconstriction; it increases until the maximum is reached just after the beginning of vasodilation and is lost near the middle of the curve's ascent.

The most recent and complete investigation of the causes of the Traube-Hering waves is that by Lombard and Pillsbury.[2] Their conclusion is that these waves are caused by a rhythmic activity of the vasomotor center. They found a quickening of the heart during vasoconstriction and a diminution during dilation. But the accompanying curves very markedly show the reverse of this. The quickening of the heart begins at the moment of vasodilation and continues almost to the crest of the wave, when a diminution begins and is maintained to the valley of the following wave. This agrees with the results of Binet and Courtier,[3] who made some observations on these waves in 1895. This also confirms a general observation which is exemplified in Figs. 2, 3, 4 and 5, namely, that when vasodilation precedes constriction, regardless of the mental state accompanying it, the rate of the heart is quickened until the beginning of constriction; and that when constriction is immedi

[1] *Journal of Physiol.*, XI., p. 108.
[2] *Am. Journ. of Physiol.*, III., pp. 201–228.
[3] 'L'année psychologique,' 1895, p. 124.

ately occasioned by a mental state, the pulse rate is diminished until the beginning of dilation. In curves 41 and 42 especially, and in 38 and 39, the diminished amplitude and reduced rate of the pulse are clearly evident during vasodilation. The same may be noted in curves 3, 9, 19, 20 and 24. Bayliss and Hill,[1] in a study of cerebral circulation and blood pressure, conclude that 'cerebral circulation passively follows the changes in the general circulation.' The above facts lend support to this view and to the conclusion that the circulation of the brain is controlled through the action of the vasomotor center acting upon the splanchnic area in governing the rate of heart beat and upon the vasomotor nerves controlling the peripheral circulation.

Why mental states should produce vasoconstriction in some subjects and vasodilation in others can not be answered in the light of present knowledge. In either case the fluctuation in the general circulation provides for an adequate blood supply to the brain—in vasodilation by an increased heart rate and a heightened blood pressure—in vasoconstriction by a diminished volume of blood to the periphery.

Summary of Results and Conclusions.

Both emotional states and intellectual activity are accompanied by change in heart rate and blood pressure in all individuals, and by vasomotor changes of the peripheral vessels in most persons.

In general, a quickened heart rate is concomitant with vasodilation, a slower rate with vasoconstriction.

The only constant variation observed in the tracings for agreeable, as distinguished from disagreeable experiences, is a tendency toward more rapid recovery to the vasomotor level in the former.

Intellectual fatigue is accompanied by a diminished vasomotor response to emotional states, and recovery is much slower than during mental acuity.

Prolonged intellectual activity produces a diminished amplitude of the pulse curve, an increased blood pressure and a diminution in the acuteness of the dicrotic notch. In most sub-

[1] *Journ. of Physiol.*, XVIII., p. 357.

jects a diminution of the heart rate occurs during the progress of an hour's mental activity, but in a few the rate is increased for this period.

In prolonged intellectual application, three types of vasomotor effects are noted: one of progressive constriction; one of no progressive vaso-changes, and one showing progressive constriction for a certain period then continuing at that level or showing slight vasodilation for the remainder of the hour.

A rhythm of from one- to three-minute periods is observed in both sphymographic and plethysmographic tracings. In general there is a tendency to coincidence in rhythm, a vasodilation being concomitant with an increased heart rate.

In most subjects during states of mental regularity and stability, as in repose or continuous intellectual application, the Traube-Hering waves are clearly defined in the tracings.

The pulse rate is quickened and the amplitude diminished during the period of vasodilation in the progress of the wave, the rate lowered and the amplitude increased during vasoconstriction.

These Traube-Hering undulations correspond in wave-length with the fluctuations of acuity in visual and auditory sense perception, the greatest acuity occurring just after the maximum of vasoconstriction.

The correspondence of vasomotor activity and heart rhythm, the response of the splanchnic and peripheral vasomotor activities to cerebral activity, and the evidence that the cerebral circulation is varied in consequence of changes in the systemic circulation, support the conclusion that the blood supply to the brain is controlled through the action of the vasomotor center. This is effected through irradiations to the splanchnic area which governs the rate of the heart beat and to the vasomotor nerves which control the peripheral blood flow.

DIRECT CONTROL OF THE 'RETINAL FIELD': REPORT ON THREE CASES.[1]

BY PROFESSOR GEORGE TRUMBULL LADD,
Yale University.

In the PSYCHOLOGICAL REVIEW for July, 1894, I gave a brief report of certain experiments made with a view to determine the answer to this question: "Can the sensations customarily called 'retinal,' which arise with the eyes closed and motionless, be made to respond to volition with respect to the form and color which they assume?" These experiments were instituted with a graduate class of sixteen members — all of them entirely capable of conducting and accurately reporting such observations. Of the sixteen only four reported no success; nine had a partial but constantly increasing success; while the remaining three attained speedily to very remarkable results. These three soon found themselves able to determine at will, within the limit set by the experiment, the shape of the visual phenomena; and, as well, the color — the color being sometimes so vivid that, on opening the eyes upon a sheet of white paper the proper complementary after-image promptly appeared.

These phenomena which I then regarded as exceedingly suggestive and, when taken in connection with numerous allied experiences, as necessitating an almost-complete change in our theory concerning the functions of the retina and the relation of visual sensations to centrally initiated conative impulses, have, so far as I am aware, received far less attention that they merit. I have myself been unable to pursue further the investigation. But there have since been handed to me three reports of self-experimentation which I consider well worthy of submitting to the Association, with the remark that they are all by trained students and successful teachers of psychological subjects. In this connection I take the liberty to request the mem-

[1] Read before the Amer. Psych. Assoc., Washington, Dec., 1902.

bers of the association to assist in future investigation of the problem by kindly sending to me any material which they may be able to collect.

There follows now a report of the cases:

Case 1. — *Mr. W. P. M., Harvard '97*, who repeatedly succeeded with himself in these two classes of experiment which I describe *verbatim* as given by the experimenter. *Experiment I.:* "By closing my eyes somewhat tightly I could see a number of 'bits' of colored light upon my 'retinal field.' Selecting one of these colors I concentrated my attention upon it, and it seemed to grow of its own accord into a perfect circle. I then willed that this circle should become an ellipse; and without the faintest 'muscular sensations' (except the vague strained feeling of purely mental concentration) the circle 'sagged' into an ellipse; thus

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 3 being exactly the same shape as the ellipse that I had pictured in my mind as the *ideal* end to be attained, in visible actuality upon my retinal field. This experiment has been successfully and easily performed by me a number of different times."

Experiment II. — "Fixing my attention upon one of the colored circles above mentioned I succeeded by an act of will in changing the color of the ring from blue to red. I have also changed a red ring to a yellow or green, according as I desired. There were no muscle-sensations. Having given the '*fiat*' of my will, I would wait quietly, observing the variously colored shapes float fantastically before me until the ring of the old color gave place, as it were of its own accord, to the ring of the desired color. I was not always successful in this experiment; and the length of time which I had to wait was very variable."

Case 2: *Mr. H. D., Yale Ph.D., 1896.* The tabulated results obtained by this experimenter, which are herewith presented, may be profitably supplemented by the following remarks as given in the language of the subject himself.

EXPERIMENTS ON THE VOLUNTARY CONTROL OF THE 'EIGENLICHT.'

First Series. Experimenter H. D.

No. of Experiments.		1.	2.	3.	4.
Date.		Nov. 20.	Nov. 21.	Nov. 22.	Dec. 7.
1. Time taken to produce cross.	A. M.	2–3 min.	Same as Exp. 1.	1½ min.	
	P. M.	1 min.		About 20 sec.	15 sec.
2. How long it could be retained.	A. M.	3 sec.	2 sec.	2 sec.	
	P. M.	5 sec.		10 sec.	5 sec.
3. Modification of shape.	A. M.	Several times.	Bars extending.	Bars extending.	
	P. M.	Bars extending.		" "	Little change.
4. Modification of color.	A. M.	Red.	Red.	Deepening red.	Silvery white to red.
	P. M.	Red.		Red to whitish gray.	
5. Effect of fatigue.	A. M.	Image blurred.	Pain back of right eye; disappearance of image.	Pain in right eye; same as to image.	
	P. M.	" "			Pain as before.

No. of Experiment.		5.	6.	7.	8.	9.
Date.		Dec. 8.	Dec. 9.	Dec. 18.	Dec. 19.	Dec. 20.
1. Time taken to produce cross.	A. M.				1 min.	
	P. M.	20 sec.	10 sec.	5–10 sec.	10 sec.	20 sec.
2. How long it could be retained.	A. M.					
	P. M.	7 sec. (clearly).	10 sec.	12 sec.	5 sec.	About 7 sec.
3. Modification of shape.	A. M.					
	P. M.	At 7 sec. it disappeared.	Cross lost in circle.	Control of shape lost.	Bars extending.	Vertigo set in and cross is lost.
4. Modification of color.	A. M.					
	P. M.	Red to deeper red.	Red to silvery gray.	Red deepening.	Bars extending.	Vertigo set in and cross is lost.
5. Effect of fatigue.	A. M.					
	P. M.	Failure of control.	Pain and then failure of control.	Deepening and vertigo.	Vertigo and obliteration.	Vertigo set in and cross is lost.

Second Series. Experimenter B. Z.

Serial Number of Experiment	I.	II.	III.	IV. a	IV. b	IV. c	V. a
Date.	Nov. 4, 1895, 7–7.15 P. M.	Nov. 5, 1895, 9–9.15 P. M.	Nov. 6, 1895, 3.45–4 P. M.	Nov. 18, 1895, 8.15–8.30 A. M.			Nov. 18, 1895, 7.15–7.45 P. M.
Eigenlicht.							
1. Color.	Golden specks on black.	Yellow broken cloud.	Light yellow clouds.	Golden on black ground.	Vivid red.	Vivid red.	Almost white.
2. Persistence.	Not steady.	Not steady.	Steady.	Steady.			Steady.
3. Shape or distribution.	Circle with outer wavy edge.	Ripples from center to edge.	Rifts of varying size.	Clouded over in streaks.	Clouded.	Clouded.	White flakes on black.
Figure.							
1. Figure desired.	Cross.	Cross.	Cross.	Cross.	Circle.	Two concentric circles.	Cross.
Figure secured.	$\wedge$	$\times$	Cross.	Cross.	Circle.	Two concentric circles.	Cross bar.
2. Time required to produce figure.		1 m.	1 m.	Very short.		Very short.	Few seconds.
Time figure could be retained.			5 sec.		Easily retained.	Easily retained.	Few seconds.
3. Modification of shape, if any.	Second streak came later.	Second bar later and vanishing at once.	Shifting cross bar.				Cross bar shifted position.
4. Color.	Light specks.	Black.	Black.	Black.		Black.	Black.
5. Effect of fatigue.		Black spot in center changing to violet and then to green.	Effacement by bright violet color.	General blur.		Running together of colors.	Shifting colors and shapes.

Second Series.—Continued.

Serial Number of Experiment.	V. b	V. c	VI. a	VI. b	VI. c	VII.	VIII. a
Date.	Nov. 18, 1895, 7.15–7.45 P. M.		Nov. 19, 1895, 11.45–12.10 noon.			Nov. 21, 1895, 12.15–12.30 P. M.	Nov. 21, 1895, 7.45–7.55 P. M.
Eigenlicht.							
1. Color.	Almost white.	Almost white.	Yellow.	Red.	Red.	Diffused green; later red.	Yellow.
2. Persistence.	Steady.	Steady.	Not steady.	Not steady.	Not steady.	Steady at first.	Steady.
3. Shape or distribution.	White flakes on black.	White flakes on black.	In streaks then in clouds.			Breaking into small clouds.	Like stars on black ground.
Figure.							
1. Figure desired.	Circle.	Two concentric circles.	Cross.	Circle.	Triangle.	Cross.	Cross.
Figure secured.	Circle.	Failure.	Cross (large).	Circle.	Triangle.	Cross (small).	Cross (small).
2. Time required to produce figure.			Several minutes.	Several seconds.	Very short.	Short.	Very short.
Time figure could be retained.			Some minutes.			Considerable.	
3. Modification of shape, if any.			Small red cross took place of black one.		Second overlapping triangle.	Disappearance and reappearance of figure.	
4. Color.	Black with violet center.		Black.	Black.	First triangle black, second triangle violet.	Black.	Black (intermittent).
5. Effect of fatigue.	Black rift in center; then violet; then green.		Whole field blurred.		Everything blurred.	Dimming of color.	

Second Series.—Continued.

Serial Number of Experiment.	VIII. b	VIII. c	IX. a	IX. b	IX. c	X. a	X. b	X. c
Date.	Nov. 21, 1895, 7.45–7.55 P. M.		Nov. 24, 1895, 10–10.15 A. M.			Nov. 24, 1895, 10–10.15 P. M.		
Eigenlicht.								
1. Color.	Yellow.	Yellow.	Yellow.	Yellow.	Yellow.	Diffused white.	Diffused white.	Diffused white.
2. Persistence.	Steady.	Steady.	Steady.	Steady.	Steady.	Steady.	Steady.	Steady.
3. Shape or distribution.	Like stars on black ground.	Like stars on black ground.	Fine streaks.	Fine streaks.	Fine streaks.			
Figure.								
1. Figure desired.	Circle.	Two concentric circles.	Cross.	Concentric circles.	Triangle.	Cross.	Circle.	Two circles.
Figure secured.	Circle (large).	Series of circles not concentric.	Cross (small).	Concentric circles.	Triangle.	Cross.	Circle (large).	Number of circles.
2. Time required to produce figure.	Considerable.		An instant.	Very short.		Very short.		
Time figure could be retained.			Only an instant.			Considerable.		
3. Modification of shape, if any.								White center of large ring broke into fine rings.
4. Color.	Yellow with inner and outer bands of black.	Dim black.	Yellow not bright, soon dim.	Black and yellow alternately.	Alternately yellow and violet.	White on black ground.	Black.	White.
5. Effect of fatigue.						Figure grew gradually fainter.		Effort to produce red color in rings broke all into horizontal streaks of faint color.

1. "The color of the '*Eigenlicht*' had (*i. e.*, ordinarily on beginning the experiment) the appearance of a dancing mass of vari-colored dust, red predominating."

2. "Persistence depended upon the physical conditions, and varied accordingly. * * *" "The best results have been secured, uniformly, after a long evening's reading, where there has been continued concentration of attention. * * *" "As soon as attention and will had gained control of the mass, the color seemed to be at their mercy."

3. "The normal appearance of the 'Eigenlicht' is always round. The center of the circle is the bridge of the nose; the radius extends to the corner of the eye and sweeps over the forehead to the other eye. It seems to be very objective and quite dependent" — *i. e.*, on the will.

4. "A triangle is more easy for me to produce than a cross, and a circle easiest of all. * * *" "Generally the cross was projected, when it came, *at once*. Thus it seemed to depend upon the creation of the brain."

This experimenter has frequently testified to me that the control of the *color* of the images obtained in this way is, in his case, usually easy and nearly complete.

Case 3: *Miss B. Z., Yale Ph. D., '97*. The extended series of experiments presented herewith in tabulated form requires no separate remarks except the observation that, in this case, the control of the subject was exercised over the shape of the visual image. No evidence of marked control over its color is presented.

It is undoubtedly too early in the investigation to draw from it with confidence any extended theoretical conclusions. Certain facts of general import seem, however, to be common to all the cases which have thus far been reported; and, also, certain changes in the current theory of color vision to be at least indicated. I append, therefore, to my two reports of, in all, some fifteen cases the following somewhat tentative observations.

1. It seems obvious that the control of the size and color of the images appearing before the eyes when they are closed in the dark grows by practice. As everywhere else, so also here, trying begets habit; and ease and success accompany the

growth of habit. This was certainly so in my own case, when I first obtained the phenomena which led me to propose the problem to others. And I am inclined to think that nearly or quite all the instances of failure to get some access of successful results are due to a lack of persistence and of confidence in the possibility of finally succeeding. But here, as in the case of all our uses of the delicate and complex organic mechanism, both peripheral and central, different individuals differ greatly in their native and acquired aptitudes.

2. It is not as yet quite clear how much of the effect must be ascribed to a *selective* attention, somewhat resembling that with which we watch for, seize upon, and hold in consciousness a word which we will to recall. I am, however, entirely confident that the phenomena cannot be completely accounted for in this way. An exalted, not to say abnormal activity of the color sensations is undoubtedly favorable to obtaining the best results. In my own case, when I was excited and somewhat hyperæsthetic, my experiments were most startling as respects the success attained. Such a condition affords, as it were, an increased abundance of material upon which selective attention may be directed. Yet many of the responses made to the will appear to be almost as prompt as those which concern changes in some portion of the motor organism. Nevertheless, —

3. The testimony of all seems to be perfectly conclusive that the desired results as to the size and color of the images cannot be wholly mediated by the motor apparatus connected with vision by the eye. A certain feeling of strain, such as goes with intense attention to a visual object, is, indeed, a common accompaniment of the first efforts to obtain a circle, cross, or triangle. One is also usually conscious of focusing the eye upon this object. And pain in the eyeball, or back of it, frequently follows prolonged experimentation of this sort. *Squeezing* the eyeball with its own muscular surroundings may result in an increased liveliness of the color sensations. But unconscious and minute movements of the eye can scarcely be held to account for the appearance of the particular shape desired; and as to changes of color, I am sure I should not know how to connect any particular voluntary or involuntary

movement of the eyes with the turning at will of a red cross, or circle, or triangle, into one of green or yellow.

4. These phenomena, especially when considered in connection with numerous others — *e. g.*, certain experiments with color illusions and visual dreams, which I have previously reported to the Association, and other phenomena, which have either been simply observed or more or less thoroughly examined by different students of psychological phenomena — have convinced me that our entire modern theory of vision has gone astray in respect of the functions of nervous elements in the retina. In a word, I believe that the shapes and colors of our visual images are *centrally determined* to a far greater extent than has hitherto been supposed. In my judgment we shall never find the final physiological explanation of our experience with colors in the structure and functions of the retina. Further investigation of the histology of the rods and cones, or of the functions of the two, or of the services rendered by various pigments in conjunction with the retina, will probably not add further knowledge as to the nature or causes of the psychical phenomena. Physical and physiological or chemical theories of the retinal functions are of comparatively little value for the psychology of vision. The fusion, the differentiation, the contrast, the sequence of our color sensations, depend upon complex *cerebral* processes, the nature of which is determined, not simply by the quality and intensity of the stimuli and by the effect of the stimuli upon the peripheral organism, but also in large measure by the associations and habits of those nervous centers of the brain that are correlated with the other senses and with memory, recognition, attention and choice.

The proof that a more '*central*' theory is needed to account for our experience with visual images even of the more simple sort, their shapes and their colors, and that the influence of the retinal elements and their functions has been hitherto greatly overestimated, is by no means confined to phenomena such as I have just described. The remarkable color illusion to which I called attention in the 'Studies from the Yale Psychological Laboratory' in 1898, and color illusions in general, point in the same direction. So also do the phenomena of 'color audition' which, if under this term we include the excitation of color sensations by auditory stimuli, are much more frequent than is ordinarily supposed. An example taken from my own experience will be of interest here. Several years ago I was having some floors in my house scraped with a metal tool in order to prepare them, by removing the old filling and wax, for being filled and waxed anew. I had several times during the noon hour amused myself by using this tool, thus establishing fusions between the sensations of sound which the tool produced and the changed and streaked colored appearance of the floor at which I was gazing intently as I worked. After this preparation of the brain, as I was lying with closed eyes on the bed in the room above, waiting for my vacation afternoon nap, the sounds of the tool used by the workman in the room below promptly and invariably stimulated in the so-called 'retinal field' the visual images of the streaks in the floor, varied nicely so as to correspond with the variations of the auditory stimuli. That is to say, when I heard the sounds of scraping, I at once saw before the eyes the same streaks which my experience had previously fused with precisely similar sounds of scraping.

It is a very common experience with me — and I find on inquiry also with others — to have the fainter or more vivid but appropriate visual images start out in the retinal field, when the eyes are closed on occasion of certain skin or muscular sensations being started; or even when, as we say, thinking vaguely and perhaps dreamily 'about' kinds of transactions that readily admit of schematic visualization. Now these are certainly forms of stimulating the production, and determining the color and shape of visual images, that have nothing whatever to do with the nervous elements or the pigments of the retina. The voluntary control of the way in which colored images shall be stereoscopically united points in the same direction. If one prepares two squares of paper of the same size, but of different colors, persistent practice to obtain control, in many cases, enables one at will, when the two squares are stereoscopically united, to see the combined image with either one of the two colors, or to fuse them in the color which is itself the mixture of the two. Such experiences, I repeat, go a long way toward relieving us from the necessity of any further effort to find in the structure and functions of the retinal elements or in the chemical constitution of the pigments of the retina the account of color sensations, and of the phenomena of their fusion, contrast, and differentiation.

5. I shall perhaps seem to most of you to be extending unwarrantably the conclusions based upon this entire class of facts, if I say that they seem to me to bear important testimony to a certain theory of consciousness and of the nature of man's mental life and mental development. I admit at once, of course, that were these facts left standing alone the charge of gross exaggeration of their importance would be justified. But they are not alone. They are only one group of facts — of countless thousands in numbers and classifiable under a score or more of heads — which, in a way quite satisfactorily prove the truth of the general theory. Negatively stated, no theory, sensational, associational, physiological, or psycho-physical, which regards consciousness merely 'content-wise' and passively, can adequately account for even the simplest of our experiences. Positively stated, consciousness must from the very first and in all its varied forms of manifestation be considered as an active, discriminating, selective, and directive force. Only from this point of view can we explain the evolution of the so-called higher faculties of the mind. This energistic aspect of consciousness, this general faculty which is needed to account for the formation of all the particular so-called faculties of man, is everywhere present for the working psychologist to discover and to trace its historical development, in the individual and in the race. Admitting this theory, more or less of direct control over the shapes and colors of objects appearing in the so-called retinal field seems consistent enough with all that we know about both cerebral physiology and also mental life, from the psychologist's point of view, which is the standpoint of consciousness.

DISCUSSION AND REPORTS.

PROCEEDINGS OF THE ELEVENTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, WASHINGTON, D. C., DECEMBER 30 AND 31, 1902, JANUARY 1, 1903.

Report of the Secretary.

The eleventh annual meeting of the American Psychological Association was held in the rooms of the Columbian University, Washington, D. C., on Tuesday and Wednesday, December 30 and 31, 1902, and Thursday, January 1, 1903.

President Sanford presided and over fifty members of the Association were in attendance at the various sessions.

At the regular business meeting held on December 30, the following was transacted. Election of officers for 1903: *President*, President William L. Bryan, Indiana University; *Members of the Council to serve for three years*, Professor James R. Angell, University of Chicago, and Professor George M. Duncan, Yale University. The following new members were elected: Mr. Joseph H. Bair, Columbia University; Dr. William Churchill, Yale University; Professor S. S. Colvin, University of Illinois; Professor Arthur H. Daniels, University of Illinois; Mr. J. F. Messenger, Pratt Institute; Professor Melbourne S. Read, Colgate University; Dr. Margaret K. Smith, New Paltz, N. Y.; Dr. Helen Bradford Thompson, Mt. Holyoke College; Dr. S. F. Wrinch, Princeton University.

The Secretary reported on behalf of the Council an invitation from the American Association for the Advancement of Science to become affiliated with that association and to be represented by a delegate upon its council.

Upon recommendation by the Council the Association voted to accept the invitation and the President was appointed the delegate to the Council of the American Association.

The Council recommended that the annual dues of the Association be reduced to one dollar. After discussion, a motion to that effect was laid upon the table for one year.

On behalf of the Committee on Bibliography, the Chairman, Professor Sanford, presented the following report:

Report of the Committee on Bibliography, Washington, 1902.

The Committee on Bibliography would respectfully report as follows:

1. That after careful consideration of the questions referred to them, they would recommend that the Association undertake the compilation of a bibliography covering as fully as possible all psychological literature published prior to January 1, 1894, the beginning of the 'Psychological Index.'

2. They recommend that the Bibliographical Committee mentioned below be authorized to purchase, as the nucleus of this bibliography, so much of Professor Leuba's collection belonging to years prior to January 1, 1894, as they may deem advisable, at a rate to be mutually determined, provided, however, that the total sum shall not exceed two hundred dollars ($200), this expenditure being regarded as an equivalent to Professor Leuba for money expended for clerical work involved in making his collection.

3. That the compilation of the said bibliography be entrusted to a committee of five members of the Association (to be known as the Bibliographical Committee) endowed with full power to determine the nature of the material to be included, the method to be followed in collecting, and all other matters connected with the undertaking, except as specified in section 1 above and in the other sections of this report.

4. That no definite action be taken present with reference to publication, in view of the fact that the preparation of a complete bibliography is a matter of considerable time, perhaps several years, and that its ultimate bulk cannot be determined in advance. The present committee is convinced of the advantages of a regularly printed and published bibliography, but is not ready to recommend the Association to take action in the matter at present.

5. That the said Bibliographical Committee keep the said Bibliography, until it is judged complete, in the form of a card index, classified according to the headings at present in use in the 'Psychological Index,' and that they take such measures as they shall deem wise for making its contents from the first as useful as possible to members of the Association, in particular that they consider the feasibility of the plan contained in Exhibit A appended to this report.

6. That for the carrying out of the objects enumerated in the preceding sections they be granted the sum of three hundred dollars

($300) for clerical assistance in addition to the sum of two hundred dollars ($200) mentioned in section 2, making in all a total of five hundred dollars ($500).

7. That the said committee take such means as may seem best to enlist the coöperation of all members of the Association in the pointing out of existing lists of literature prior to 1894 and in contributing references from their own reading.

Exhibit A.

A Plan for Use of the Bibliography while in the Form of a Card Index.

It is suggested that the Bibliography can be made useful from the first if the Bibliographical Committee be instructed to furnish to any member of the Association applying for it a typewritten copy of such section or sections as he may desire at the actual cost of transcription (if the applicant desires to retain the transcriptions), or at one half the cost of transcribing if he returns the copy in good condition within sixty days of its receipt, the Bibliographical Committee (or its chairman) drawing upon the Treasurer of the Association for such sums as are necessary to meet the expense of the transcriptions and returning to him such sums as may be received for transcripts made, the sums drawn not being included in the five hundred dollars mentioned in the body of the report; and nothing in those provisions being so interpreted as to prevent any member of the Association from direct access to the cards of the Bibliography at such times and places as the Bibliographical Committee may specify.

It would be well also if the committee should from time to time publish in the Psychological Review or *Science* statements of the periodicals covered and of the general progress of the work.

The report was adopted by the Association and the Committee consisting of Professors Sanford, Warren, Creighton, Sneath and MacDougall was continued as the Bibliographical Committee of the Association.

Report of the Treasurer for 1902.

Dr.

To balance at last meeting	$1,524.46	
Dues of members	339.00	$1,863.46

Cr.

By expenditures for		
Printing	$19.50	
Proceedings	5.19	
Postage	15.50	
Stationery	8.10	
Clerical assistance	11.27	
Expenses of Committee on Bibliography	7.27	$66.83
Balance		$1,796.63
Interest on deposits (approximate)		175.00
Total		$1,971.63

Audited by the Council.

LIVINGSTON FARRAND,
Secretary and Treasurer.

ABSTRACTS OF PAPERS.

Address of the President: *Psychology and Physics.* By E. C. SANFORD.

(The address appears in full in this number, March, of the PSYCHOLOGICAL REVIEW.)

The Psychology of Weather Influence. By EDWIN G. DEXTER.

The paper attempts to outline the more important results of a series of studies made by the writer on the influence of weather on human conduct. The method followed consisted of the comparison of the average daily occurrences of certain recorded abnormalities of conduct with definite meteorological conditions. The data, gathered principally from records kept in New York city, consisted of the daily records of suicides, of arrests for assault and battery and for drunkenness, and of the deportment of the city penitentiary and certain of the public schools; in all, nearly 500,000 separate occurrences, covering a period of ten years. The meteorological data were taken from the records of the New York Station of the United States Weather Bureau.

The studies showed that the various meteorological conditions directly affected the metabolic processes of life, some increasing the vital energy, others tending to deplete it, and that the resulting mental states were those common to conditions of excessive or depleted vitality.

Low temperatures seemed generally katabolic, with gradually increasing anabolic tendency until a mean temperature of about 85° F.

is reached, when a sudden change takes place and the effect upon available energy is disastrous. Above this point active disorder reaches its minimum, while the death rate, suicide and clerical error increase alarmingly. Low pressure is anabolic, with a reverse for a higher mercury column. A dry atmosphere is exhilarating, while a moist one is depressing. Calms were found to have a tremendous katabolic effect in New York city. For moderate movements of the atmosphere these conditions are quite reversed, while for very high winds we find them again approximated.

The clear, dry day is anabolic in its tendency, while days of an opposite character show a katabolic effect. Deportment is at its best on cloudy days, but suicide showed a peculiar anomaly, being excessive on the bright dry days.

In conclusion two facts may be mentioned:

First, children are more influenced by weather states than adults; second, women seem more sensitive to meteorological conditions than do men.

It may be added that we cannot suppose meteorological conditions to be the immediate cause of many of the abnormalities of conduct which vary with them. Suicide, for example, is more frequent when the barometer is low, yet a low barometer, we could hardly assert, drives men to self-destruction. The only thing supposable is that during such atmospheric conditions the general emotional states are of such a quality that other things are more likely to do so.

Normal Variability in Mental Traits. By EDWARD L. THORNDIKE. (Read by title.)

The Development of Memory in School Children. By JAMES E. LOUGH.

The experiments here reported were made with 682 girls, aged 9–15, in one of the New York public schools for the purpose of ascertaining the variations of memory due (1) to content, (2) to age and (3) to the 'brightness' of the children. The method of experimentation was similar to that employed by Lobsien with school-children in Kiel — lists of ten words were read to a class of children who were then asked to write down as much of the list as they could remember. The lists employed were: (I.) Ten 'color words' (red, green, etc.); (II.) ten 'sound words' (rumbling, chasing, etc.); (III.) ten numbers (28, 93, etc.); (IV.) ten 'touch words' (rough, warm, etc.); (V.) ten 'emotion words' (sad, brave, etc.);

(VI.) ten abstract words (result, direction, etc.); (VII.) ten concrete words (Japan, Dewey, etc.); (VIII.) a second list of numbers. Color words were best remembered (av. 86 per cent.), then followed in order, concrete words (av. 75 per cent.), touch words (av. 70 per cent.), emotion words (av. 68 per cent.), sound words (av. 58 per cent.), abstract words (av. 50 per cent.) and finally numbers (av. 45.5 per cent. for the first list and 44.5 per cent. for the second list). This order was constant save at the age of 9 when emotion words changed from fourth to second place. There was surprisingly little improvement of memory between the ages 9 and 15, the average change being about 5 per cent., the maximum coming at 13 with the single but marked exception of emotion words which reach a minimum at this age. Memory for concrete words shows the greatest improvement (13 per cent.) and memory for numbers the least. In the earlier years the 'brighter' pupils show better memories than duller pupils in corresponding grades, while in the later years the duller pupils show a marked superiority of memory.

Early Music Interests of the Child. By WILL S. MONROE.

The music interests of 161 children between the ages of three and six inclusive were tested along the following lines: (1) Ability to learn the scale; (2) ability to remember the scale; (3) ability to remember simple songs; (4) fondness for music. Ability to learn the scale was least at three years and greatest at six: 29 per cent. of the boys and 47 per cent. of the girls at three, 31 per cent. of the boys and 54 per cent. of the girls at four, 34 per cent. of the boys and 59 per cent. of the girls at five, and 40 per cent. of the boys and 71 per cent. of the girls were able to learn to sing the scale. The girls throughout surpassed the boys in ability to sing the scale.

Ability to remember the scale a fortnight after it had been taught — with several intervening reviews — was represented by 19 per cent. of the boys and 23 per cent. of the girls at three, 27 per cent. of the boys and 33 per cent. of the girls at four, 29 per cent. of the boys and 45 per cent. of the girls at five, and 40 per cent. of the boys and 57 per cent. of the girls at six. The girls surpass the boys in tone memories, although sex differences are less pronounced than in ability to learn the scale.

Ability to learn and remember songs was much greater than ability to learn and remember the scale. At the age of three, 43 per cent. of the boys and 59 per cent. of the girls; at four, 50 per cent. of the boys and 61 per cent. of the girls; at five, 47 per cent. of the boys and 62 per cent. of the girls; and at six, 60 per cent. of the boys and 71 per cent. of the girls remember for a fortnight the songs that have been taught them.

Fondness for music furnishes necessarily vaguer returns, yet the results are suggestive. At three, 33 per cent. of the boys and 53 per cent. of the girls; at four, 31 per cent. of the boys and 60 per cent. of the girls; at five, 23 per cent. of the boys and 65 per cent. of the girls; and at six, 20 per cent. of the boys and 57 per cent. of the girls are reported fond of music.

Experimental Psychology and the Elementary School Curriculum. By Francis Burke Brandt.

The purpose of the paper was to direct attention to the elementary school curriculum as a field for psychological research by offering a report regarding some tentative experiments being carried on in the experimental school established in connection with the Philadelphia School of Pedagogy. The experiments have aimed at the solution of three problems: (1) In the daily time programme, what subjects shall be included as qualitatively coördinate? (2) shall the subjects regarded as qualitatively coördinate be treated as quantitatively coördinate? (3) shall the qualitatively coördinate subjects succeed each other in the daily school programme in any specified order? Several thousand observations have been made on boys while they were engaged in the several school studies, with a view to determining the typical modes of activity characteristic of each school subject. These observations show that the activities involved in all of the heterogeneous and multiform 'subjects' constituting the elementary school curriculum are reducible to five fundamental and clearly differentiated forms, describable as reflectional, observational, conversational, ideational and creational. Repeated study of the typical postures and positions of a class shows that, generally speaking, the emphatic character of mathematical activity is reflectional, that of natural science observational, that of history conversational, that of language (linguistics and literature) ideational, and that of school arts (music, drawing, manual training) creational. Otherwise stated, mathematics exercises brain attention; natural science, eye attention; history, ear attention; language, especially the reading of literature, ear and eye attention; school arts, external expressive activity — the rendering of the inner outer.

Conceiving the curriculum as a highly specialized mode of daily experience and as consisting of the subjects above enumerated as qualitatively coördinate, the second part of our experiment has been to formulate and put to experimental test a school programme in which an equal amount of time is assigned daily to each of the coördinate subjects. Incidentally we are also testing a specified order in which the qualitatively coördinate subjects are made to succeed each other in the daily programme. This programme follows:

A GRAMMAR SCHOOL DAILY PROGRAMME.

Closing Time.	Minutes.	Subjects.	Groups.
9:10	10	Opening exercises.	
10:00	50	Arithmetic.	I. *Mathematics.*
10:25	25	Physiology.	II. *Natural Science.*
10:45	20	Recess.	
11:10	25	Geography.	
12:00	50	History.	III. *History.*

NOON INTERMISSION.

Closing Time.	Minutes.	Subjects.	Groups.
1:35	5	Opening exercises.	
2:00	25	Grammar (spelling, etymology, syntax, composition).	IV. *Language.*
2:25	25	Literature (reading).	
2:40	15	Recess.	
2:52	12	Music.	V. *Arts.*
3:30	38	Drawing, penmanship, manual training.	

Summed up, this programme has been under experimental test for three years. It is an attempt to demonstrate experimentally: (1) That the qualitatively coördinate scholastic subjects suitable for elementary instruction may be adequately and best conceived under five fundamental forms whose appropriateness as subject-matter for instruction is determined in each case primarily by its value as a daily mode of experience adapted and suitable to the psychological nature of the child; (2) that inequitable distribution of time to any one of these five fundamental forms means over- or under-activity of some fundamental side of the child's nature; and (3) that by a specified order of taking up the several subjects daily, the health of the child is properly safeguarded through a rational alternation in the exercise of different kinds of attention.

An Experiment with the Questionnaire. By H. AUSTIN AIKINS.

Ten thousand children in the first eight grades of the Cleveland public schools were asked, 'What is the worst thing you ever saw anybody do?' and 'What is the best thing you ever saw anybody do?' This is a preliminary report on the answers to the first question.

There are several well-marked curves, showing changes in the children's moral interests. As we go up the grades the curves for bad language, stealing and cheating, smoking and chewing, disobedient and disorderly conduct, fall. Those for cruelty, unkindness and mean acts rise. In the higher grades cruelty is mentioned far oftener than anything else.

Analysis of the cruelty answers gives remarkable curves for cruelty to birds and to horses. Birds are mentioned in the first grade about as often as all other animals put together, and in the eighth hardly at all. With horses this is reversed.

So far as there is any difference between the two, boys tend to name masculine vices, and girls feminine. Some of the curves seem to indicate that boys get new ideas or interests about a grade earlier than girls.

The Position of Psychology in the System of Knowledge. By HUGO MÜNSTERBERG.

(The paper appears in full in *Harvard Psychological Studies*, Vol. I., 1903, PSYCHOL. REV., Mon. Sup., No. 17.)

Psychological Method in Ethics. By JOHN DEWEY.

It is commonly agreed that the most generic distinction between philosophy and the sciences is that the former deals primarily with values, and the latter with facts or presented phenomena. Or, in rough but convenient phraseology, the philosophic disciplines are normative, the scientific descriptive. From this general premise it is argued that since psychology is a science concerned with facts and events (states of consciousness, and their physiological correlations), it can have no essential bearing upon ethics, a branch of philosophy concerned with a particular sphere of value. I wish to point out that one may accept both the generic distinction referred to and the placing of psychology as a purely natural science, and yet maintain that psychology furnishes an indispensable phase of method in ethics.

1. While affairs of conduct are primarily matters of value, viz., functions and attitudes, not of mere presentations in the stream of conscious states, and hence not a part of direct psychological data, it yet remains true that every such conduct-value has its signature or correspondent in the body of immediate data or presentations. That is to say, an end or ideal of behavior is not to be sought for as a mere psy-

chological presentation; but since it marks a positive distinction within conscious experience there must be a conscious state which somehow corresponds to it, and, for the psychologist's purpose, *carries* it.

2. The psychologist can study the particular conditions in the stream of presentations of that particular content which represents the having of a moral ideal; and he can trace the influence, in the way of stimulation and inhibition, which such content exercises upon further presentations in the stream. Psychology as genetic is concerned precisely with just such problems of origin and subsequent career; the matter of further career being a strict part of genetic psychology in so far as any one presentation is found to furnish specific conditions.

3. It is practically impossible to see how any control of the interpretation of the meaning to be justly assigned to the category of, say, 'ideal' is to be secured without recourse to just such a device as this. Just because the ideal in actual moral experience is a matter of immediate personal worth, it is unanalyzed. Just because the presentation is not the moral reality, but gives it in an abstract and detached form, it renders the ideal capable of objective analysis and statement, or lends itself to the needs of intellectual control. Normative philosophy is not concerned with a mere reproduction of the original, vital experience. To object to the employment of psychology because it does not constitute or present the value with which it is concerned, would, therefore, so far as its logic is concerned, rule out any philosophic interpretation whatsoever. The only way to have the value is to have it as a matter of personal experience, and that is no more philosophy than it is science. To say then that psychology cannot give the ideal is entirely aside from the point. Philosophy cannot 'give' it either. What psychology can do is to study in a definite and analytic way the meaning of a value as determined by its origin and function in the stream of experience.

4. It is clear that the use of psychology in this way is formal rather than material. That is to say, it does not tell what the concrete ethical ideal is; it tells what any experience must be if it is to be qualified as ideal. It shows the conditions of origin and use to which any qualitative experience must conform if it can be properly characterized as aim, end or purpose. It is quite clear, however, that such determination is not *merely* formal. The very fact that there are certain specific conditions of origin and career to which the candidate for ideal value must submit itself, gives a definite intellectual base line from which to measure the claims of such a candidate. In other words, if it is said that pleasure of perfection and self-realization, or

recognition of duty, is the ideal, we have at once a definite method to pursue. Can they fulfil the conditions of origin and functioning which are demanded? Or, putting the matter more positively, the knowledge of these circumstances of genesis and subsequent career, enable us to delineate the main features of anything which has legitimate claim to be considered as end or ideal. If we assume for the moment that an anticipatory image is the psychological counterpart of the moral ideal, our problem is defined as the discovery of the adequate stimulus and the adequate use of functioning of such an image in experience. Such an analysis carried to its end would certainly leave us with a very positive notion regarding the generic traits of the ethical ideal. The form thus determined is not a form separate from all matter of experience, but is the form or framework of a certain kind of actual experience.

Critique of 'Psycho-physical Parallelism.' By GEORGE TRUMBULL LADD.

In opposition to all forms of the current hypothesis of so-called 'psycho-physical parallelism,' the paper made the following points: (1) All the data for any theory as to the relations of body and mind originate within the unity of the 'stream of consciousness.' The connections between the different items or 'moments' of this stream are not merely those of sequence in time; but they also have the appearance of dynamical connections. (2) Just as apparent as the fact of this unity is the fact of a certain *diremption* accomplished by the activity of discriminating consciousness. Some of the psychoses are ascribed to the *Ego* as their subject, and others are more definitely localized in the organism. (3) These two classes of experiences are now inevitably regarded by the natural 'ontological consciousness' in terms of the interaction of body and mind. (4) So true and inevitable is all this that the very conceptions 'cause,' 'causal relations,' 'causal influence,' etc., originate and receive their chief validation from this experience; and without it, no question as to a theory of the relations in reality of body and mind could ever arise. (5) Judged from the point of view of experience the figure of speech involved in the term 'psycho-physical parallelism' is both inadequate and misleading; it is inadequate, because it utterly fails to emphasize the complicated net-work of interrelations of which we have an indubitable experience; and it is misleading, because it neglects the *dynamical* character of the interrelations. (6) These defects are emphasized anew, when the theory becomes metaphysical and strives to state itself in terms of the 'ontological consciousness.' (7) For purposes of psychological science it is the business of the investigator, assuming the standpoint of the natural dualistic hypothesis, to discover the precise nature and empirical formulæ of the interrelations. But, finally, (8) philosophy is not satisfied to leave the subject in this shape; it seeks a '*uniting bond*' for these, and for all other dynamical connections of our experience. This bond it must find in the Being of the Cosmos, whose being man, with the totality of his nature, shares.

The Theory of Energetics in its Philosophical Bearings. By JOHN GRIER HIBBEN.

The claims of the *Energetiker* as expressed by Ostwald in his lectures on *Naturphilosophie* delivered at Leipzig, in the summer of 1901, may be briefly summarized as follows:

The most universal concept employed in scientific circles to-day is that of energy. All other concepts can be derived from it. Energy can be defined in terms of work, as that which is the result of work, or that which may be transformed into work. Thus the metaphysical concept of substance, and of cause may be expressed in terms of energy. Matter may be expressed in terms of form-energy and volume-energy. Matter, therefore, gives way to energy, and with the passing of matter, the transition from physical to psychical phenomena becomes immensely simplified, for it is possible to conceive of a psychical energy as transformed physical energy more readily than we can coördinate matter and mind. The general position is fortified by three analogies:

1. As a transition is effected from physical to nervous energy, so also a like transition occurs between nervous and psychical energy. It is no more difficult to conceive the one than the other.

2. As only a few crystals under pressure manifest electrical phenomena, so not all energy is accompanied by consciousness but only in the unique case of the central energy of the brain. This relieves the necessity of correlating consciousness with all forms of energy.

3. As a storage battery produces energy out of all proportion to the liberating cause, so also the centrally stored energy produces results out of all proportion to the nervous energy which acts as its liberating cause.

In criticising this theory the following theses were discussed.

1. Its mathematical presuppositions and processes are precariously uncertain, as indicated by Boltzmann and Planck.[1]

[1] See *Annalen der Physik und Chemie*, N. F., B. 57, S. 39, 72.

2. The mechanical or dynamical expression of any physical system does not purport to give an exposition of the essential reality of the phenomena which it describes.

3. The correlation of the various forms of physical energy expresses a quantitative equivalence. But the attempted correlation of nervous and psychical energy is distinctively qualitative.

4. The definition of energy in terms of $\frac{1}{2}mv^2$ represents its essence under space and time conditions. But mental phenomena cannot be brought under such categories.

5. Psychical energy is either the same in kind as nervous energy or it is not.

If it is, it must be a disguised form of mass and velocity relations.

If it is not, it lies outside the initial concept of energy altogether.

6. If we compare the physical world as a whole with the world of thought as a whole, we find the former characterized by a decrease of energy and an increase of entropy. The reverse is true, however, of the world of thought.

7. The concept of energy is not a 'form' of the mind in a Kantian sense, as Ostwald maintains.

8. Ostwald affirms that 'the continuity of experiences in one brain or in one mind' constitutes the consciousness of personal identity.

If this continuity, however, is maintained by the brain regarded as an organ of physical energy, it fails to account for the resulting continuity of consciousness.

If, on the other hand, it is maintained by the subjective thought center, this transcends the fundamental concept of energy.

The Status of the Subconscious. By JOSEPH JASTROW.

The speaker presented a survey of the field of the subconscious emphasizing the position which this topic should occupy in the framework of psychology. The subconscious represents the great other half of the mental processes coördinate in theoretical importance if not in practical effect with the conscious processes. The subconscious participates in every one of the factors that together constitute the onward movement of the mind, and is to be understood through its normal manifestations in every-day thinking. Those topics that contribute more especially to the field of the subconscious are: (1) The distribution of consciousness in ordinary occupations; (2) the phenomena of distraction; (3) dreams; (4) special control over the subconscious as in crystal gazing; (5) automatisms; (6) efforts to retrace trains of thought. On the abnormal side the field includes trance states, the action of drugs, more pronounced automatisms, disorders of personality. The tendency to form a conception of the subconscious too exclusively on the basis of the abnormal was opposed and the importance of the consideration of normal and commonplace characters for the elucidation of the status of the subconscious in psychology was emphasized.

The Apparent Form of the Heavens and the Illusory Enlargement of Heavenly Bodies at the Horizon. By A. H. PIERCE.

1. To assist in determining whether the illusion of the horizontal moon depends upon the apparent form of the heavens, questions were sent out relative to the apparent form of the sky by day and by night, and also to the apparent size and distance of the moon at horizon and zenith. One hundred replies were tabulated. (*a*) In clear weather by day 31 see the dome of the sky flattened, 31 see it hemispherical, 36 see it raised. Two did not reply. In cloudy weather by day the figures are respectively 79, 10 and 7. Seven did not reply. In clear weather by night 24 see the heavens flattened, 18 see them hemispherical, 48 see them elevated. Ten did not reply. There is thus a fair tendency to see the heavens in the form of an elevated dome by night. Forty-eight see the heavens in the same form both by day and night, in clear weather, 14 seeing them flattened, 10 hemispherical and 24 elevated. (*b*) As to the moon, all see it larger at the horizon, of course. For 89 the moon seems more distant at the zenith, for 9 only more distant at the horizon. Two did not reply. The illusion of the horizontal moon and the apparent form of the heavens seem thus to be unconnected matters. This is further shown by the fact that to 75 the moon seems to float. This fact helps to explain certain disparities between perceived form of sky and apparent distance of moon. Three see the heavens raised at night, yet the moon seems more distant at the horizon. Twenty see the heavens flat at night, yet the moon seems more distant at the zenith. Nineteen see the heavens hemispherical at night, yet sixteen see the moon more distant at the zenith and three more distant at the horizon.

2. A comparison of illuminated disks, after the manner of Bourdon, contradicts the results of the latter and shows that at angles of elevation of 0°, 22½°, 45° and 90° equally appearing circles correspond to visual angles in the ratio respectively of 1, 1.4, 1.6 and 3 The raising of the gaze appears therefore to be influential in diminishing the apparent size of objects seen thus.

3. Preliminary determinations seem to show that the moon illusion ceases at about 30° of elevation. Consequently, the cause of the illusion is hardly to be sought for entirely in the direction of the gaze.

Direct Control of the Retinal Field. By GEORGE TRUMBULL LADD.

In addition to the cases reported in the PSYCHOLOGICAL REVIEW for July, 1894, the paper reported three cases of subjects who were able to acquire a marked control over the shape and color of the sensations arising in the so-called 'retinal field,' with the eyes closed and apparently motionless.

On the basis of all these cases, some fifteen in number, the following conclusions seemed warranted:

1. The control of the size and the color of the images appearing projected before the eyes, when they are closed and in the dark, increases with the repeated effort at control. Here, as elsewhere, control grows by practice.

2. A part, but not all of this control, seems to be due to the well-known effects of selective attention. In certain cases, however, the desired change of color seems to follow the conative impulse almost as promptly as do the movements effected by the voluntary muscles.

3. Although certain muscular effects are undoubtedly connected with the strain of attention and the location of the visual image, the shape and especially the color of this image do not appear to be dependent upon the volition *through* the motor organism.

4. These phenomena, when taken in connection with numerous other allied phenomena, indicate the necessity for a more *central* theory of visual sensations. The shape and the color of these visual images are *not* dependent upon the character of the peripheral excitation. Probably the functions of the rods and cones, and of the pigments of the retina have much less to do than has hitherto been supposed with the character of the visual images.

5. In a tentative way we may say that these phenomena favor the dynamical theory of conscious mental life, rather than the theory which emphasizes passively received content.

The Effect of Screens on Acuteness of Vision. By CHRISTINE LADD FRANKLIN.

This paper gave the result of an investigation carried out last summer at the University of Berlin, and already briefly reported on by Professor Nagel in the Proceedings of the *Physiologische Gesellschaft.*

It has been noticed that screens, when held near the eye of the observer or near the object looked at, interfere very little with clearness of vision. On the one hand, veils as worn by women (as Professor Nagel has said) are of very slight inconvenience or they would be discarded. On the other hand, a tarlatan screen stretched across the stage in front of *tableaux vivants*, while it softens the outlines a little, is absolutely not perceived as an obstruction. But there are intermediate points at which the disturbance is much more serious. It was to study the law of these disturbances that the investigation was undertaken. The more detailed results will appear later in the *Zeitschrift für Psychologie u. Physiologie d. Sinnesorgane.*

The Supplementary Image in Recognition. By ELEANOR A. McC. GAMBLE and MARY WHITON CALKINS.

This paper reported the repetition and modification of an experiment performed by A. Lehmann and described by him in the *Philosophische Studien*, Vol. VII. The experiment consisted simply in giving to subjects who were not aware of the point at issue a series of odors and in requiring them (1) to write down, *in order if possible*, whatever the smell brought to mind; (2) to note with a dash each pause in the flow of ideas; (3) to mark the smell as 'familiar' or 'unfamiliar'; and (4) to underscore the name-image if it was suggested.

The main result of the investigation is a negative one. Lehmann's theory of recognition as constituted by the occurrence of supplementary images is disproved by the following facts: (1) Not only recognition but the consciousness of unfamiliarity is often accompanied by supplementary associations clearly due to the quality of the unfamiliar odor. (2) In cases in which the time-order is noted, the supplementary imagery is most commonly recorded as distinctly subsequent to the recognition. (3) Imagery clear enough to be remembered does not always occur in cases in which recognition is well marked. The paper will appear in full in the *Zeitschrift für Psychologie u. Physiologie d. Sinnesorgane.*

The Orientation of Certain Tactual Perceptions. By WILLIAM CHURCHILL.

Weber (cf. *Sitzgsber. d. k. Sächs. Gesellsch. d. Wiss. Math.-phys. Cl.*, 1854, S. 85) once observed that forms such as letters traced upon the skin are variously oriented upon different parts of the body. Although he recorded only one mode of orientation upon each area mentioned, a recent investigation covering thirty-eight observers (including eight blind) has shown marked individual differences.

Considering the experimenter's view-point as the norm and using the same figure in different positions to indicate the amount of divergence exhibited, it may be said that (1) upon *forehead*, *cheeks and chin* two distinct types of observers were found, one whose mode of orientation agreed with that of the experimenter (L being perceived as L), and a second, much more frequent, which perceived the letters in a way analogous to 'mirror-writing' (L as ⅃); (2) upon the *breast*, *abdomen and ventral side of the lower limbs*, in addition to the types just mentioned, a third type was noticed (L as ⅂); (3) upon the *dorsal side*, with the exception of a few instances of curious inversion (L as Γ), all observers coincided with the experimenter in orientation. Visual influence was excluded so far as possible by requiring the observer to render his decision with closed eyes. Of the eight blind observers (six congenital cases) only the two with a record of vision in early childhood exhibited a tendency toward the third type.

The number of normal observers who indicated a decided preference for any one type upon the respective surfaces is set down in the following table. Any observer not giving at least seven out of ten answers in accordance with the same mode of orientation was classed as indefinite.

Surface Stimulated.	Type.				Not definite
	I. L	II. ⅃	Γ	III. ⅂	
Forehead	*2*	*28*			
Cheek, right	13	14			3
Cheek, left	3	24			3
Chin	3	27			
Breast	*3*	*13*		*10*	*4*
Abdomen	*2*	*8*		*12*	*8*
Thigh, right, ventral side	3	10		9	8
Thigh, left, ventral side	4	11		8	7
Shin, right	3	13		7	7
Shin, left	3	11		6	10
Occiput	*30*				
Back	*30*				
Thigh, right, dorsal side	24		2		4
Thigh, left, dorsal side	24		2		4
Calf, right	27		3		
Calf, left	27		2		1

Repetition of the experiments upon fifteen observers proved that the preferences exhibited upon the first occasion were in most cases persistent.

As explanation it is suggested that Type I. indicates a predominance of revived muscular sensations (particularly because so often found upon the *right* cheek in close proximity to the hand customarily used in writing) in Type II. the tactual impressions are related directly to the 'absolute space' of the body; in Type III. visual memories are conspicuously active. Introspection by the observers gave confirmatory testimony.

Perhaps a test of orientation may be of service in distinguishing between the so-called motor type and the strong visualizer.

The Factors of Affective Tone in Simple Rhythm Forms. By ROBERT MACDOUGALL.

(The paper appears in full in the preceding number (January) of the PSYCHOLOGICAL REVIEW.)

Researches on the Voice. By E. W. SCRIPTURE.

Ten Years of American Psychology: 1892–1902. By EDWARD FRANKLIN BUCHNER. (Read by title.)

In the effort modestly to mark the first decennium of the life and work of the American Psychological Association, the paper sketched the features of the earlier American psychology and reviewed the progress made in the establishment of laboratories and the appearance of systematic literature during the ten years preceding the organization of the Association. Special analysis was made of the work accomplished inside the Association from its beginning, with which was contrasted the chief psychological achievements outside the Association. Various suggestions, including that of the establishment of a prize gold medal in psychology, were made with a view to improving the condition of the science in America. The Association has received 283 papers, reports, etc., topically distributed as follows: general (56), sensation (53), genetic, social and individual (41), higher manifestations of mind (39), cognition (34), conation and movement (17), characters of consciousness (13), mental tests (11), sleep, trance and pathology (10), anatomy and physiology of the nervous system (7), and affection (2). Another distribution of this material shows the work of the Association to have been thus: experimental (86), philosophical (34), apparatus (28), theoretical (25), genetic (25), descriptive (23), physical and physiological (20), historical (12), pedagogical (12), comparative (7), miscellaneous (7), discussions (4). Of the 148 members elected during the decennium, 89 have contributed to the activity in the annual meetings, the average attendance at the annual meetings has been almost 35; the average number annually elected to membership is almost 12;

the average number of contributing members is only about 20. The increased scientific sociability of psychology during the ten years was reviewed. The growth of psychology, as indicated by the widespread establishment of laboratories and the rapid multiplication of means for the publication of research reports, and a descriptive account of the chief events and emphatic features year by year concluded the scope of the paper.

A New Field for Psychologists. By JOHN P. HYLAN.

Experimental psychology is the ruling phase of our science at the present time. The recency of this phase has prevented the experimental method from being applied in a comprehensive way to the whole of the subject. As a result psychology has not been developed to meet the needs of every-day life, and has also suffered from the lack of a consistent purpose. The sciences in which we excel have been developed in response to practical needs. May not psychology similarly profit by being given a practical aim?

As a suggestion towards this end may be mentioned the need of a psychology of the moral life. The lack of an adequate method of moral education is shown the most forcibly by the prevalence of crime. A conservative estimate of the annual cost of crime in the state of Massachusetts is $5,151,130, or a cost per capita of $1.84, being two thirds the average cost of education for every person in the United States. In addition to this direct cost to the taxpayers, must be added the support of families deprived of their maintenance through the retention of wage earners in prison, and the loss and destruction of property caused by law-breakers. It is thus to one of practical interests as well as to the philanthropist and scientist that the need of a science of moral culture appeals.

This, however, is not a new field in the sense of not having gained the attention of psychologists before. Several of our leading contemporaries have shown interest in the problem. What we need is the development of practical methods for the prevention of crime, and the regeneration of the criminal type of character when once formed. As a means to this end, individual criminals should be made objects of minute study, and the conditions which attended each step in crime carefully scrutinized. The effects of monotonous work, of fatigue, of amusements, diet, and social influences should receive exhaustive study, and also localities which are especially productive of the criminal character. To this end every house of detention should have a resident psychologist who should command every facility for furthering this study. Charity organizations, university settlements, and school boards should employ expert psychological assistance in detecting criminal influences among certain classes and society at large.

The advantages of such an application of psychology would not lie wholly in the sphere of moral culture, for in this way mental science would gain some of the organization and native completeness that has been sacrificed through the method of analysis.

The Golden Section as an Æsthetic Canon. By WILLIAM A. HAMMOND. (Read by title.)

On the Psychology of a Group of Christian Mystics. By JAMES H. LEUBA.

I. *The Tendencies.*

The extreme sensitiveness of the Christian Mystics to certain organic processes brings with it a deepening of four needs or tendencies. They are (1) the need of mental peace; (2) the need of an affective support; (3) the tendency to organic enjoyment; (4) the tendency to the universalization of action.

Concerning the tendency to organic enjoyment.

If our Christian Mystics usually refuse their body the ordinary satisfaction it craves, it is not because of the unholiness of the pleasure itself, but because sexual passion, even when within legitimate bounds, places one so frequently and, often, so irresistibly, in opposition to the finer moral promptings. But the yearning for organic enjoyment is too strong and in itself too innocent to permit of the sudden elimination of that part of our nature. The Mystics have found a way of gratifying, to some extent at least, the sexual cravings and at the same time of avoiding the moral dangers lurking in the ordinary way of giving them satisfaction. This conciliatory course is erotomania with the idea of Jesus, the Virgin Mary, or God as respondent.

As the enjoyment they secure in this way does not come from practices guilty in their eyes, but, on the contrary, from the vivid realization of the surpassingly great love to them of an absolutely good God, its effect is elevating rather than debasing; it becomes a strengthener of holy resolves; it cements together the tendencies according to the 'will of God' and increases their power.

Concerning the tendency to the universalization of action.

Deep significance is to be attached to the heroism of the unremitting efforts of our Mystics in favor of universalized conduct, *i. e.*, of action proceeding from generalized motives. Their own habit is to designate such conduct as the performance of God's will. What is peculiar to them is that the highest good they recognize has at its service a psycho-physiological imperative mechanism; the 'ought' is backed in them by a compelling motor force.

II. *The Mystical Ecstasy.*

When we want ineffective desires to triumph there is no more powerful means at the command of science than to induce hypnosis and then to suggest the wished-for victory. Now, the Mystics have developed in their endeavor to satisfy their needs a method of worship which places them (while under the control of the God-idea) in a condition of increased suggestibility. This method they call *Orison.* It is, in its lower degree, a meditation upon God and, in its higher, a trance looked upon as union with Him. In the progress of the soul to God the two great periods observed in hypnosis are clearly marked: (1) loss of muscular control, (2) loss of sensory perceptions. It differs from the ordinarily induced hypnosis in that it is dominated by a feeling of love. The mystical ecstasy is then a love trance, more or less profound, in which the idea of God, or of Jesus, or of the Virgin, takes the place of the hypnotizer. What is to be chiefly noticed is that this so-called 'higher' religious exercise of the Mystics gives them at once mental peace, a vivid feeling of the presence of the Great Comforter, the delights of passionate love and, moreover, brings with it a strengthening of all the tendencies that are believed to be in accordance with God's will.

One may generalize and say that in all religions the attitude looked upon as most divine is one of increased suggestibility. The most general and the most significant statement which the psychologist can make regarding the religious attitude *par excellence* is that, among all peoples and at every degree of their development, it is a trance dominated by tendential ideas, not everywhere the same, but expressing always the needs accounted highest by the individual or by the society to which he belongs.

(See the *Revue Philosophique* for July and October, 1902.)

Researches on Methods of Memorizing. By C. N. McAllister. (Read by title.)

Apparatus for Producing and for Recording Serial Stimuli. By James Burt Miner.

Attention was called to the fact that at least a dozen researches on rhythm and time had been carried on in this country alone within the least three years. The investigation of problems of this nature makes desirable a simple instrument for giving electric contacts in series. An inexpensive contact wheel to be rotated by a motor and speed reducer was exhibited. The instrument, when thus connected, gave a half-second interval with an average error of only five sigma in ten reproductions. The length of the interval could be changed by moving the pins mounted on the rim of the wheel. These pins could be easily and exactly adjusted and thrown into or out of operation. The electric circuit was made without the current passing through the wheel. This was done by means of a special contact, consisting of two parallel flat springs attached to the base, and so arranged that the pins on the wheel, as they passed, just touched the nearest spring. The electric circuit was made by the crossing of two lines of platinum, attached to the adjacent surfaces of the springs.

A new recording apparatus was described which utilizes the carbon ribbon of a typewriter in making a record of successive movements. This 'carbon-ribbon kymograph' has been used for registering with electric pens on the common telegraph-ticker tape a time line of seconds and the subject's reproduction of the interval for an hour without interruption. A form of glass pen attached to a tambour was also shown, to be used for writing with ink by capillary attraction. Curves of the blood pressure and pulse had been satisfactorily traced with it. An improved form of a continuously adding finger dynamometer was presented. The instrument weighed only three ounces and was especially convenient for making fatigue tests on school children. The above pieces of apparatus had been developed or improved at the Columbia laboratory under the direction of Professor J. McK. Cattell and used by Mr. Miner in connection with his study of rhythms.

Description of New Apparatus. By E. B. DELABARRE.

1. *A Continuous Roll-paper Smoke Recorder.*— A roll of paper is placed in a smoking-box, underneath the recording table. A ventilating tube, leading into a chimney, prevents escape of smoke. The wick of an oil-stove is raised and lowered automatically, as the apparatus starts or stops. Between the flame and the recording-paper is interposed a continuous band of wire mesh. This moves across and around under the box, passes over a brush to prevent clogging, and effectually prevents scorching of the paper. The paper is led from the smoke-box across the top surface of the table, where records are made. A long weighted cord draws the slack of the paper up across the room, after it emerges from the driving apparatus. Without an assistant, a six-inch record has been made of pulse and respi-

ration curves for a half-hour continuously, before it was necessary to interrupt the process for cutting-off the used paper. With an assistant, the apparatus can be used without interruption as long as desired.

2. *A Cylinder Chronograph.*— A synchronous motor will reproduce in the laboratory the approximate uniformity of revolution belonging to the generating apparatus of an electric lighting station. A cylinder connected with the motor can be used for time-records, with the advantage that the length of line recorded is directly proportional to the time interval measured, and hence the latter can be read off rapidly. A quickly applied correction is then necessary to offset actual variations in speed, as determined by a speed-recorder. Measurements by a tuning-fork show that by this method a very high degree of accuracy is attainable. Records are made either directly or electrically, and either on the cylinder itself or on paper covering it. The cylinder is sufficiently wide to accommodate a long series of records, and is fitted with automatic devices for giving and recording signals, and for readjustment after each record.

3. *An Inexpensive Pendulum Chronoscope.*— A weighted cord swings across a scale divided into Σ intervals; and behind a strip of wood that can be pressed firmly against a cord, interrupting the motion of its upper part but leaving the lower part free to swing on without strain. Visual signals are given by the uncovering of a window in a screen, twenty or thirty Σ after the beginning of the swing. The apparatus involves a certain degree of inaccuracy, but will give all the accuracy needed for a large number of more common purposes.

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Members will please notify the Secretary of any errors in names or addresses as given in the above list.

WESTERN BRANCH OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

The Western Branch of the Psychological Association held its first regular meeting at the University of Chicago on December 5 and 6, 1902. Eight institutions were represented at the session. The program contained papers by Professor Coe of Northwestern University, Dr. Fite of the University of Chicago, Professor Gale of the University of Minnesota, Professor Angell of the University of Chicago, and Professor Jastrow of the University of Wisconsin. The comment and discussion stimulated by the papers was vigorous and suggestive.

The next meeting of the Branch will be held at Northwestern University.

The Executive Committee, consisting of Messrs. Dexter, Coe and Angell, was reëlected, Professor Coe being appointed chairman vice Professor Angell.

AN EXAMPLE OF AN ASSOCIATION THROUGH A FORGOTTEN IDEA.

At the request of Professor Scripture I have written out the following account of a mediate association.

I was sitting in my college room, studying or writing, one afternoon, when I heard for the first time the chimes which had lately been presented to the college and located in the chapel tower. My attention was naturally arrested from the work which I was doing, but instead of my mind dwelling upon the novelty of the chimes or returning to the broken thread of my work, there suddenly flashed into my mind a scene which I could not locate. The picture was that of a red brick building, situated upon a hill, and associated with the picture was the thought of myself as being in a novel, unfamiliar relation to it. The circumstance interested me, and I thought enough about it at the time to make a record of it, and to try to place the scene. On going home and describing the circumstance to my mother, she reminded me that my description corresponded to the location of a large school in a place where I had spent a week visiting when I was ten years old, that is, ten years before the incident. The sound of the college chimes was much the same as the sound of the chimes at the school building, but I did not know or had entirely forgotten that I had ever heard them. But the scene which they recalled was sufficient to enable my mother to identify it with its actuality.

M. E. Alling.

Yale University.

PSYCHOLOGICAL LITERATURE.

Psychologie Économique. G. TARDE. Paris, Alcan. 1902. 2 vols. Pp. 383 and 449.

The title 'Economic Psychology' suggests fields of thought highly interesting to the social psychologist. For recent economic theory, through its studies of value, or by the path of the 'historical method,' has given increasing recognition to the psychological forces at work in the economic field. In the opening chapters of his work, Tarde makes a forcible presentation of the psychological factors which enter into economic problems. The psychology which has been taken into account has been too largely individual psychology. The influence of men upon each other — the field of *inter-psychologie*, as Tarde would designate the science — is much more important and has been much less studied. The economic man is a two-fold abstraction. He is too narrow in interest, and too isolated in his relations, to represent the actual man of commerce and industry. Sentiment, faith, prejudice, determine the individual as really as does egoistic interest, even in economic relations. The forces of group, corporation, vicinage, religious community, are more or less influential in every bargain, or fixation of wage and salary. The sympathetic strike, practiced in 'America, the most utilitarian of countries, we are told,' is an extraordinary example of interest in the lot of others, and of the power of social solidarity. Sacrifices for an idea, for a principle, for a sympathy, have never been more conspicuous than in the field supposedly governed by economic forces.

Moreover, the psychology used has been the antiquated hedonistic psychology of the eighteenth century, which conceived man in terms of sensibility; and in the forces of sensibility the attention has been largely fixed on pain as the constraining motive to activity. Labor has been regarded as the all-important fact, whereas in certain respects leisure is the more significant. It is in the periods of leisure that social union and freedom of individual choice are stimulated. The division of leisure deserves consideration as well as the division of labor. "The less men work, the more they want to consume." Changes in the form of industry produce changes in the state of mind of the laborer; and through the varying psychical qualities of different races, new inventions permit certain races to forge to the front. Thus the present age of machinery puts a premium on the tenacity of attention which is found in the German, English and American, while the more mobile and versatile imagination of the French and Italian workmen, which was of great value when the artistic was prized, is a disadvantage to the worker with a machine.

While, however, the complexity of economic facts and the need of psychology for their explanation are well pointed out, the author's actual success in analysis and interpretation does not meet the anticipations raised by the introduction. For the method pursued is to range the usual economic facts of production, exchange, consumption, etc., under the author's well-known rubrics of 'imitation,' 'opposition' and 'adaptation.' In some cases no further psychological analysis is attempted. Instead we have simply a discussion, ethical or economic, as to the respective merits of individualism and socialism. It may be easily admitted that the economics of fashion and mode present phenomena of imitation, that strikes are a form of opposition, and that exchange and associations may be regarded as adaptations (indeed, what intelligent act is not an adaptation of means to ends?). The question remains whether we have added much to our scientific knowledge by grouping our facts under such general categories. To supplement its general analytic method, modern economics has resorted to the historical method. But unfortunately Professor Tarde gives little or no attention to this method. The economics which he considers is of the older, abstract type, and his psychology suffers correspondingly. If one were to apply the 'historical method' to the explanation of this, he might doubtless find a reason in the fact that French economical theory has not as yet generally responded to the new movement. A psychology which should interpret the actual development of industry and commerce, and of the various forms of economic value would have a more fruitful problem.

J. H. TUFTS.

UNIVERSITY OF CHICAGO.

The Varieties of Religious Experience, A Study in Human Nature. Being the Gifford Lectures on Natural Religion Delivered at Edinburgh in 1901–1902. WILLIAM JAMES. New York and London, Longmans, Green & Co. 1902. Pp. xii + 534.

This volume presents in full measure the characteristic features which have caused Professor James' writings to be so widely read and appreciated — an engaging style, an abounding wealth of concrete material, a complete and sympathetic understanding of the common experiences of life, the touch of nature which makes the author kin to all his readers, and, above all, the power of acute analysis and of bold constructive generalization. The task which he has set himself in these Gifford lectures, is an inductive study of the varieties of religious experience with the purpose of ascertaining their common elements, of assessing their value, and of discovering a basis for their rational justification. He restricts the sphere of his observation for the most part to the human documents of the neurotic type, on the ground that the extreme and abnormal cases bring into high relief the essential elements of religious experience, and that the intense emotional tone of such states of consciousness emits not only heat but light as well. In this connection, he insists that religious experience should be tested not by its origin, whether under normal or abnormal mental conditions, but solely by the three following criteria: (1) Immediate luminousness, (2) philosophical reasonableness, (3) moral helpfulness (p. 18). The utility of a religious experience is after all its supreme warrant.

The author further circumscribes the field of research by the exclusion of institutional religion with its variety of creed, ritual and rubric, and by confining his investigation solely to the various manifestations of personal religion. The definition of religion in the line of this purpose is as follows: "Religion, therefore, as I now ask you arbitrarily to take it, shall mean for us the feelings, acts and experiences of individual men in their solitude, so far as they apprehend themselves to stand in relation to whatever they may consider the divine" (p. 31).

The prevailing types of religious experience fall into two classes, the so called 'once-born,' and 'twice-born' souls. The former are those fortunate spirits to whom religion is a second nature. Wordsworth perhaps had such in mind in his lines:

> "There are who ask not if thine eye
> Be on them ; who, in love and truth
> Where no misgiving is, rely
> Upon the genial sense of youth.
> Glad hearts! without reproach or blot,
> Who do thy work, and know it not."

This Professor James calls the religion of healthy mindedness. The present day representatives of this class he finds strangely enough among the Christian Scientists, inasmuch as they deny the existence of evil, sin or disease, and claim that their religion is one of health and repose. Although not an avowed adherent of the 'mind-cure' doctrines and practice, Professor James is nevertheless most sympathetic and appreciative in his estimate of this new cult. The phase of this movement which appeals to him with special force is that of its close connection with the phenomena of the subconscious life (p. 94 f).

The second class of religious experiences embraces all the 'sick souls' who need a new birth. They present invariably the phenomenon of the 'divided self,' with an accompanying consciousness of friction, struggle and a desire for some form of unification (p. 127 f). As regards this process of unification, the author discusses at length the experience of conversion, in which there is always the existence of transmarginal or subliminal consciousness (p. 189 f). In instantaneous conversions there is evidence of the more intense activity of the subconscious self. This phenomenon is always accompanied by a highly wrought emotional 'faith-state' which is characterized by a sense of peace, consciousness of the possession of truth, and of 'an appearance of newness beautifying every object' (p. 248 f).

Professor James proceeds in the next place to discuss the subject of the fruits of the religious life which appear in their most complete form as the state of saintliness. This type of character is treated in its various phases at considerable length; five chapters are devoted to it, which abound in significant illustrations of a most interesting nature. The characteristics of saintliness are as follows:

1. Consciousness of the existence of an ideal power.

2. A sense of the friendly continuity of the ideal power with one's own life.

3. An immense elation and freedom, as the outlines of the confining self-hood melt down.

4. A shifting of the emotional center towards loving and harmonious affections, manifested in an increase of charity and tenderness for fellow creatures (p. 273 f).

The practical results attending the phenomena of saintliness are summarized as follows: "Economically, the saintly group of qualities is indispensable to the world's welfare. The great saints are immediate successes; the smaller ones are at least heralds and harbingers, and they may be leavens also, of a better mundane order. Let us be saints, then, if we can, whether or not we succeed visibly and temporally" (p. 377).

Turning from the question of the utility of religious experiences which manifest themselves in the various characteristics of saintliness, Professor James next discusses the question of the intellectual justification of such experiences. Such a justification may be either mystical or philosophical. As regards mysticism, the following conclusions are reached:

1. The mystical states carry authority for him who has them.

2. But for no one else.

3. Nevertheless they serve to break down the exclusive authority of rationalistic states, and strengthen monistic and optimistic hypotheses (p. 423 ff).

Concerning philosophy, Professor James insists that its peculiar function is not to furnish religious feeling or even conviction, but its province is critical, and inductive; and as such, philosophy is to be regarded more in the light of a 'science of religions.'

In the concluding chapter (p. 511 ff), the author states his own 'over-beliefs' in the following cardinal points of doctrine:

1. The subconscious self as intermediating between nature and the higher region.

2. The higher region, or 'God.'

3. Influences from the higher region produce real effects in nature.

In the postscript, Professor James explains his general philosophical position as based upon a pluralistic hypothesis consistent with the concrete individual experiences as the basis of religion.

Such being a very brief summary of the contents of this volume, let us turn our attention to some points of criticism. It must be remembered that while the extreme case may throw much light upon the nature of religious experience, nevertheless, the abnormal in turn cannot be adequately estimated save through the light shed upon it by the normal. The type of religious experience must lie, as is the nature of all types, midway between the extremes; and it after all represents the normal man, the great body of sane individuals whose religious experiences are no more connected with pathological phenomena than their experiences of friendship, of patriotism, or of moral obligation. Moreover, when submitted to the test of utility, the extreme case of religious experience by the very excess of its emotional vigor may defeat its own end as a social force. Professor James makes prominent the fact that 'saintly conduct would be the most perfect conduct conceivable in an environment where all were saints already, but in an environment where few are saints, and many the exact reverse of saints, it must be ill-adapted' (p. 356). The religious forces which possess in an eminent degree the capability of adapting themselves to the needs of various peoples and to various times are precisely those forces which emanate from the normal experiences common to the vast number of inconspicuous persons who constitute the median type. These experiences are commonplace; they possess no content of mystic truth; they are accompanied by no dream or trance states, they show neither the heat nor the light of emotional enthusiasm — but they do form a steady stream of influence in every community which makes for the conservation and progress of all forms of social good. But for the historic continuity of such experiences throughout the Christian centuries, the flame of religious devotion would have been extinguished, and the occasional appearance of a hero of saintliness, or of a religious genius would hardly have served to rekindle its glow. As in philosophy the appeal is so often made to the common sense of the plain man, so also in religion the superior court of appeal resides in the common experiences, the commonplace experiences if you will, of simple conviction and quiet devotion.

Moreover, it is obvious that the common type of religious experience is found in such numbers that it can be studied only *en masse.* Now, inasmuch as institutional religion is at once the sphere of its activity and the result of its convictions, it follows, therefore, that institutional religion must be regarded in some measure at least in the final estimate of religious values in general. If there is to be a science of religion, it must deal in its inductive investigations with the various forms of institutional religion. For the critical and inductive method is not only to be applied rigorously to the varieties of personal religious experience, but it should be applied also in such a manner as to embrace the largest possible area. To omit the study of institutional religion, therefore, is to circumscribe the area of investigation in such a way as to cut off much light which might prove most illuminating. In this connection also, if the test of utility is to be applied, the common religious experiences of those who constitute the various sects of Christendom must be reckoned with. Their manifold influence upon the progress of civilization attests their value according to a standard which Professor James in this work specially endorses. Moreover, the collective efficiency attaching to groups of persons with common religious connections and concerted activities must also be considered. The utility of such a social factor is evident.

The extreme case of religious experience, again, cannot be correctly evaluated until we know its effect upon the community, the tribe, the nation, or the age in which it occurs. This effect can be estimated, however, only when we know the general level of religious experience and that in turn can be known only in reference to the prevailing religious tone of the age as evidenced in its religious institutions and movements. Thus the significance of the individual case of personal religious experience can be adequately appreciated only in its general religious setting and historical antecedents. Professor James asserts that 'churches when once established live at second hand upon tradition, but the *founders* of every church owed their power originally to the fact of their direct personal communion with the divine' (p. 30). While it is true that great religious movements may be traced to the personal religious experience of a conspicuous leader of thought, nevertheless, on the other hand, it is quite as true that the leader is to be accounted for in a large measure by the religious atmosphere of the age in which he lives. It is but half the story to state that Luther founded Protestantism; it is also true that Protestantism produced Luther.

Turing now to Professor James' hypothesis of the subliminal region as an explanation of man's seeming consciousness of a higher power, it is insisted that divine influences may operate through this medium. The author is constrained to confess that 'just how anything operates in this region is still unexplained' (p. 270). He regards this region, however, as the probable sphere of mediation between the subjective consciousness and the 'something higher' or the 'something more' which is recognized in every religious experience. It serves to bridge the gulf between the known and the unknown, establishing an unbroken continuity between them. There still remains, however, an outstanding difficulty. This *tertium quid*, this medium of transition and communication, partakes more of the nature of the unknown than of the known. The weakest point in this chain of continuity lies in the passage from the conscious to the subconscious. It does not rationally certify the presence or indicate the nature of the divine.

In the subconscious region there may be the open door through which divine influences operate, as Professor James suggests, but it may be also the region of chimeras and delusion. As an hypothesis, it may serve to explain the divine, or equally well, to explain it away. If we trace religious phenomena to this source, the source itself is still within the shadow. If it were possible to illuminate the field of the subconscious so as to reveal in clear light the forces which are there operative, it would cease to be subconscious. Subconscious states, therefore, cannot be adduced as a *rational* justification of religious experiences. They may perform an extra-rational function, and if we are properly to estimate their evidential value, we should not fail to keep in mind their extra-rational character.

A remaining point is still to be considered. Professor James suggests that not only do we find the foundations of religious experience within the nature of the individual, but also it may be that the object of that experience may differ for each individual; in other words, every man may have his own God, the complement, as it were, of his own religious nature (pp. 525–6). Such a conception would destroy that fundamental unity which is as essential to a science of religion as it is to physical and chemical science. It must be said, however, that Professor James does not subscribe unreservedly to any such doctrine, but suggests it merely as a possibility which at least merits some consideration.

In conclusion, it is a pleasant duty to emphasize the fact that while this volume contains much that may possibly provoke differences of opinion, nevertheless it must be acknowledged that the thought throughout is so fresh, vigorous, stimulating and suggestive, that it will no doubt prove a valuable and permanent contribution to a profounder understanding of religious experience and of human nature.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

The Principles of Logic. HERBERT AUSTIN AIKINS. New York, Holt. 1902. Pp. 489.

Professor Aikins has undertaken a rather difficult task in this little volume. It appears to be a double task. First to write a treatise on logic which shall serve for elementary study in colleges and at the same time to discuss subjects which more properly belong to advanced thought on the subject, and second to deviate somewhat from the traditional conception of logic and its functions. In regard to the former task I do not think that he has improved the subject for college curricula. It is hard enough in any case to get logic respected by colleges, and in the writer's opinion the reason lies in the fact that the science has been made too formal for usefulness, and postponed too late in the course. It ought to follow mathematics immediately to correct the confidence in reasoning which that science inculcates consciously or unconsciously. In that case the simplest formal rules are all that are necessary and the student can then be put to practical examples and made to test his reasoning processes. No discussion of the larger questions of logic and philosophy is necessary. For advanced students of the subject it is different. Professor Aikins hesitates between the two needs and does too much for one and too little for the other.

I do not see that his treatment of conversion and related processes and his view of deduction in any way contributes to a better understanding of logic. It only makes the subject less exact than it really is and serves but to confuse the mind as to the real nature and merits of the traditional logic. The more we discuss about its principles and rules the less likely is the student to accept them for the regulation of practice. All attempts to be original in this science only result in confusion. If the category of quantity is not satisfactory in determining formal logic I am confident that we shall have no definite rules at all by which to measure or determine an agreed standard of correct proof and communication of conviction. Let me take one example to illustrate. On page 194 Professor Aikins gives the following syllogisms with his remarks:

"Five francs are a dollar;
Four shillings are a dollar;
∴ Five francs are four shillings.

The inference is perfectly valid: but if we say in precisely similar form:

"Blades of grass are green:
Frogs are green:
∴ Blades of grass are frogs,

the inference is not valid. The reason, of course, is that the copula 'are' is used in different senses in the two syllogisms."

Now I must contend that in the first instance, according to all intelligible rules of logic the first syllogism is not valid in its inference. You have to supply a special meaning for the terms even to make it appear valid. The formal rule requires us to interpret 'are' according to the simple meaning of that term in all other propositions and not to suppose mathematical equality which it may suggest in such propositions as those in this instance. It is only in formal logic that we can get any general rules whatever. If we are dealing with material logic we have no general principles, except as we can classify them under those of material fallacies, but none whatever would exist for valid reasoning. In fact the difficulty in both Professor Aikins' examples is the same. It is not the truth of the proposition in the conclusion that is concerned, but the validity of the process of getting it, and that must be subjected to the quantitative test, or there is none. It would be merely a matter of individual opinion whether it were true or not and there would be no way to apply *ad hominem* argument.

I would agree that scientific method needs to be considered in the philosophic curriculum, but it ought to be discussed without complicating it with the questions of elementary logic.

J. H. HYSLOP.

NEW YORK.

The Elements of Experimental Phonetics. EDWARD WHEELER SCRIPTURE. Yale Bicentennial Publications. New York, Charles Scribner's Sons. 1902. With three hundred and forty-eight illustrations and twenty-six plates. Pp. xvi + 627.

The matter represented in this volume is of a much greater variety than one expects in a book on phonetics. However, the author is certainly right in expressing his opinion that the science of phonetics cannot be confined to a study of the physics and physiology of speech sounds, and that the problems of speech perception, of the psychology of language, of rhythm and verse, etc., can all be treated by experimental methods and must be included. The book, indeed, includes an enormous amount of details, even the most elementary, of every science in any respect related to the problems of speech production and speech perception. And being written in a rather popular style, without entering into any deeper discussion of contradictory theories of these auxiliary sciences, it seems to be written for any one who possesses as much as a high school education. This fact does not, of course, exclude the book from being used also by more specially trained readers. Indeed the extraordinary number of titles of books and scientific articles quoted by the author seems to prove that he had also such readers in mind. He certainly deserves praise for collecting such an amount of material. If, however, the first class of readers will be much benefited by this material in its present form of representation, seems doubtful to the reviewer. Yet this judgment is entirely subjective, and others may disagree with it.

The reviewer, while reading the book, wondered if the author could actually have read all the books and articles quoted. He feels sure that the author has not mentally digested all of them. To the naïve reader the text gives the impression to be the critical decoction of the numerous articles quoted at the bottom of nearly every page. A critical comparison of the text with the notes, however, and the enormous size of the volume too, make one rather believe that the author wrote the text partly from memory, quoting now and then the sources of his information as he remembered them, and mostly by compiling rather uncritical abstracts made by himself and others; and that, having completed this, he had some librarian distribute the titles of the catalogue of some scientific library over the different pages of the book. A possible explanation of this would be, that the pressure of the Yale bicentennial caused abortion of the product before it was quite mature. A bibliography is a good thing, but it might prove more useful if printed as an appendix than if connected with a text which is not always its progeny. The naïve reader who for further information reads the articles quoted at the bottom of the page, will often waste much time by reading articles insignificant for the problem, and sometimes not finding at all the one which contains the best information; the latter being absent, because its title does not disclose all of its content, the former being present, because it is *entitled* to be. This criticism, of course, is not intended as a reproach to the author, but merely to warn the reader not to expect to find more in the book than it offers, although this latter is a great deal.

Part I.: *Curves of Speech.*—The author explains in detail vibratory movement, how it is produced, influenced by friction, by exterior forces like an electromagnet with an intermittent current, how it can be graphically represented. The phonautograph and manometric flames are then discussed. Both make speech visible, but imperfectly. The phonograph is then described, which is greatly superior as a recording instrument and has the additional advantage of permitting to prove the accuracy of the record by means of reproduction. The different methods of reading the phonograph record are mentioned. A description of the gramophone and of the author's method of enlarging gramophone records follows. The last two chapters of this part are on the analysis of speech curves obtained by the recording instruments mentioned.

Part II.: *Perception of Speech.*—The anatomy of the organ of hearing is illustrated by six very clear-cut figures. Aphasia, agraphia, etc., are discussed, together with the accepted theory of the action of the cortical centers. The perception of musical tones is treated in the way in which this is commonly done. The reviewer was shocked to find here the somewhat sweeping statement: 'To the mind the tone from a violin is just as simple as that from a tuning fork or an organ.' The author seems to have a general tendency to decide difficult theoretical questions by leaving the decision to the common sense. This does not please the reviewer, but it may satisfy others. Further, the perception of speech elements is discussed. More or less different sounds are perceived under certain conditions as like. Language tends to bring into prominence such elements as are necessary for distinction, and to suppress needless distinctions. In a chapter on speech ideas, the phonetic unit is distinguished from the phonetic element: a phonetic unit is a complex sound representing a single idea. It may consist of more than one 'word.' Phonetic elements are the simplest speech sounds which we are able to remember. In the same chapter are discussed visual and auditory lapses, misreadings and misperceptions of spoken words. Another chapter is on the association of ideas. The last three chapters of this part report on the results of experiments concerning association with special regard to speech. A pendulum chronoscope is described, measurements of association time are mentioned, the significance of transposition and substitution of speech elements for the development of language is discussed, methods of memorizing are compared by briefly stating the results of the several investigators.

Part III.: *Production of Speech.* — The first chapter gives a detailed description of the most important apparatus for recording muscular movements in general, particularly of the Marey drum. The second compares breathing under ordinary conditions with breathing during speech. Three further chapters give a very clear description of the anatomy of the vocal organs, including the muscles of the lips, tongue, soft palate, etc., of the action of the larynx and methods of observing it. The action of the vocal bands cannot be regarded as vibratory motion of strings or membranes, but rather of cushions of a highly complicated structure and function. The mathematical laws of vibrations of the air in cavities are mentioned and briefly applied to the cavities of the mouth, as far as this is possible at the present time. The methods of obtaining palatograms (of tongue contacts) are described and a number of palatograms of several languages are reproduced and compared. The author then discusses tongue positions and movements. He emphasizes that one should not overrate the importance of a certain position found in the production of a certain sound. This position may be of much less significance for the production of this sound than the manner in which the tongue moves to this position and away from it. Further chapters discuss the action of the pharynx, nose, velum, lips and jaw, simultaneous and successive movements of the several organs functioning during speech, and vocal control by the nervous system.

Part IV.: *Factors of Speech.* — A careful study of vowel records shows that vowels are produced by air puffs emitted from the larynx, which may be like sinusoids or more like explosive blows. The tone corresponding to the frequency of these puffs, the cord tone, varies during a single vowel in pitch as well as in intensity. The tone (or tones) produced by each puff in the mouth cavity is, according to the Helmholtz theory, an overtone of the cord tone; according to the Willis-Hermann theory its pitch is independent of the pitch of the cord tone. The author accepts the latter theory, stating and comparing the arguments for either theory. He makes the contradictory statement (p. 420) that 'the two forms of treatment imply that the *cavity tone* is to be considered in the one case as a *free vibration* of the air in the cavity, and in the other case as a *forced vibration*.' The cavity tone is never a forced vibration; for a forced vibration of a body (or a volume of air) cannot be called a tone of this body, *e. g.*, the forced vibrations of a sheet of paper which I hold in my hand while listening to an orchestra, cannot be called the tones of the paper, but only the tones of the orchestral instruments. What the author refers to, is that according to the Helmholtz theory the mouth cavities only reënforce overtones of the cord tone, whereas, according to the Willis-Hermann theory the mouth cavities may not only, of course, do this, but also produce tones independent of the pitch of the cord tone. These independent tones then fluctuate very rapidly in intensity, because the blast from the larynx is not constant, but consists of puffs. Forced vibrations of the air in the mouth (*i. e.*, vibrations of periods greatly differing from the natural periods of the vibrating body, the air in the mouth) are a matter of course, but do not enter into this problem at all.

A further statement which does not seem consistent with the author's views is this (p. 426): "The supposition that spoken and sung vowels consist of whispered vowels plus a cord tone is an absurdity. A whispered vowel produced at the same time with a violin note does not become a sung vowel by the addition." This is no argument. The violin tone is by no means identical with the cord tone of the same pitch, since the cavities of the mouth reënforce special overtones of the cord tone, but not of the violin tone which is far away. I have taken the experiment of producing at the same time *very strong* whispered vowels (by very strong expulsions of air) plus weak violin tones and must report that the compound sounds, however imperfect the experiment is, do not seem so unlike sung vowels to warrant the author's judgment of 'absurdity.' The only argument against the above theory, which the reviewer sees, is that in whispering the blast is constant, in speaking or singing intermittent, so that the independent tones of the mouth must be of constant (or gradually decreasing) intensity in whispering, intermittent in speaking and singing.

Liquids and consonants are then discussed. Sound fusion: "Speech cannot be considered as made up of separate elements placed side by side like letters. In the flow of speech it is just as arbitrary a matter to consider certain portions to be separate sounds as to mark off by a line where a hill begins and the plain ends." Progressive change: "The various hypotheses that have been put forth as explanations of phonetic changes might be directly tested by reproducing the conditions and recording the speech results. Thus the hypothesis that the changes known as Grimm's law are the results of increasing rapidity of speech might be tested by recording language spoken at different speeds." Melody of speech: "The present chapter will be confined to a study of pitch in speech." The cord tone alone is treated as the basis of this study. "A vowel is in fact not a melody alone but a harmonized piece of music. Experimental data on this subject are almost entirely lacking." Duration: "The experiments on speech sounds have made it clear that at best the terms *long* and *short* for the vowels or syllables of a word can mean no more than that they are on an average long and short." Loudness: "Among the hundred or so English vowels that I have inspected, I have been unable to find one that can with any close approximation be considered as steady in intensity and constant in pitch. * * * It seems to be the rule in English that a vowel following a pause shall be a rising or crescendo one, and one preceding a pause shall be a falling or diminuendo one." Accent: "The one property that characterizes auditory accent is impressiveness; this may arise from increase in loudness but also from decrease, from rise in pitch but also from fall, from lengthening of the duration but also from diminution — in short from any *change* that produces a mental effect." The last two chapters discuss auditory and motor rhythm in general and speech rhythm in particular. "Since even in scanning the syllables do not have simple relations of length, it is justifiable to conclude that in naturally spoken verse the relations differ even more widely from the theoretical ones. * * * If we assume that the movements of the hand and vocal organs are executed simultaneously, we can conclude from the foregoing experiments that the point of emphasis in rhythmic speech comes before the vowel and before or in the course of the consonant which precedes the vowel. In other words, the point of emphasis in rhythmic articulation lies at the *beginning* of the movement of the vocal organs for the production of the sound."

The relative length of the four parts may be seen from the following: I.–75, II.–112, III.–211, IV.–159.

Three appendices are added: Fourier analysis, studies of speech curves and free rhythmic action.

Although the book is not in all its parts a critical digest of the matter treated in it, but leaves a large amount of the critical work to the reader, it is certainly an extremely useful collection of material to him who knows how to use it.

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Problems of Evolution. F. W. HEADLEY. New York, Crowell & Co. 1901. Pp. xv + 373.

The author of the delightful 'Structure and Life of Birds' appears before the public in the present volume as an ardent and versatile champion of the extreme Darwinian school, and certainly proves himself worthy of being enrolled among the company of English scientists, comprising Wallace, Lankester, Poulton, Meldola, Ll. Morgan, Kidd, Ball and others, who are disposed to stand by the guns of their great compatriot at a time when the biologists of continental Europe 'are half inclined to reject Darwinism' if not 'to scoff at the bridge by which the scientific world made its way to evolution over a sea of difficulties.'[1] We are of course reminded of the fate which overtook physical science during the period of Newton-worship in England, and prediction, based on historical analogy, would probably point to the more independent scientists of the continent as the sources of any further great biological advances in the immediate future. Perhaps the revival of Mendel's work, a return to experimental breeding, from which Darwin himself started, may be a partial confirmation of this prediction.

Mr. Headley has undertaken a task before which a less intrepid writer and one with less English breadth of interests might stand aghast. In the small space of 368 pages he sweeps through the gamut of biology from the mitotic division of the cell to Chinese sociology, of course only *per summa cacumina rerum* and often leaving to one side whole masses of facts that could be used very effectively in support of his views. He often displays a refreshing neglect of the enormous literature on natural selection, Lamarckism, etc., playing about his subject in short, crisp, often delightfully facetious sentences. This is a new departure and one perhaps to be commended in a general work of this kind. Even the old stock of evolutionary puzzles

[1] *Conf.*, *e. g.*, the epithet 'Kleinigkeitskrämer von Down' applied to Darwin by a bumptious German, and the sensational title of a recent work, 'Am Sterbelager des Darwinismus.'

— the giraffe's neck, the stag's antlers, the eyes of the cave-animals, etc.— are garnished with a few fresh phrases when set before the scientific reader for the *n*th time.

The first half of the work is devoted to the problems of animal evolution (including man in his animal aspect), the second to human evolution as such. A brief introductory chapter in the first part familiarizes the reader with a very few of the main facts of cytology and embryology. Then follows a chapter on 'Heredity, Variation and Death,' which contains some valuable suggestions worth transcribing in full: "I believe that the infusorian's senility and death after multitudes of fissions is due to excessive specialization. Weismann himself suggests that the specialization of the somatic cells *may* be the cause of death in multicellular animals, the Metazoa, but he rejects it for what seem to me inadequate reasons. In the Metazoa the specialization of the somatic cells is a great advantage, while heredity is maintained by the only slightly varying reproductive cells. And we can imagine Natural Selection singling out for survival those organisms in which these two opposite tendencies were developed simultaneously; the conservative cells handing on the characters of the race; the progressive and less stable cells changing in the course of ontogeny, so as to meet the most varied demands, and then — a wonderful fact! — when once they have gained the requisite qualities, becoming conservative, and maintaining by non-specializing fission the specialities they have developed. In the one-celled animals, as I have shown, there must be the same tendencies. But the tendency to specialize must soon be fatal. An infusorian's one cell must be a Jack-of-all-Trades, or he will be eliminated. When, after multitudes of fissions, therefore, the infusorian grows senile, I believe it is because of failure to divide into similar halves — that is, to specialization. There is in fact a failure of heredity; some characters are over-represented in one half and under-represented in the other. When fission has been repeated hundreds of times it is far from wonderful if in every individual thus produced some essential character is lacking. This becomes all the more probable when we remember that the life-histories of the Protozoa and the Metazoa seem to follow parallel lines, a point which, as I have already mentioned, has been well brought out by Mr. Archdall Reid. If the cells produced by an amœba's fissions held together there would be formed a Metazoan: the period of fission in the amœba corresponds to the life of the soma of the Metazoa, which life begins with the formation of highly specialized cells. In the Metazoa amphimixis is beyond doubt a return to the non-specializing principle. In the infusorian, then, may we not regard conjugation in the same light? Conjugation is the natural corrective of the inevitable tendency of cells to divide unequally. But in the higher animals this specialization in the soma is an enormous gain. On the other hand, very slight variations are required in the cells set apart for reproduction. For though, if the species is to succeed in the struggle with others, slight changes must constantly be originating in them, yet the animals which grow from them must not deviate *widely* from the type. One of the higher animals is, therefore, a contrivance resulting from natural selection for utilizing the two tendencies of cells — to maintain their characters when they divide, but to vary slightly. To put it more correctly, those individuals, in different parts of which these two tendencies were highly developed, have been selected for survival. But even in the somatic cells, in spite of their returning, when once specialized, to the system of equal fission, so far as equality is possible, inevitable specialization must lead to the same results that we have seen in one-celled creatures: through repeated fissions they will, at last, no longer have that combination of elements on which life depends. So that both in the Protozoa and the Metazoa we may regard fission and over-specialization as the cause of death; amphimixis and generalization as the cause of rejuvenescence. But the multicellular are in a very different position from the unicellular. For the Protozoön there is a vitalizing process possible, which may restore its youth. For the Metazoön, as a whole, there is no such magic possibility, but only for special cells set apart for reproduction; for what we think of as the individual, death is inevitable."

In the second chapter the author trains his guns on the 'Lamarckian Principle.' Indeed 'one of the chief objects of this book is to show that the Lamarckian view has no basis in fact and that it offers no explanation of animated nature.' And it is not too much to say, in language which Mr. Headley uses of one of the worsted advocates of this principle as applied to the development of floral structures, that the bones of Lamarckism 'lie bleaching before the walls.' Apart from freshness of treatment, many of the arguments are not very new. He sets forth the impossibility of conceiving of a process whereby the somatogenic modifications (acquired characters) of an organism can produce corresponding variations in the offspring developing from its germ-cells — perhaps at the present time a strong biological, but by no means a strong logical argument; the fact that the environment is impotent to change the inherent characters of an organism; the impossibility of explaining the colors of flowers and fruits on the Lamarckian principle; the fact that in their hearts breeders of animals and horticulturists are necessarily Weismannians; the fact that growth and development are independent of exercise; the instincts of solitary insects; the skill of the workers among social insects — an argument which is being deprived of much of its force by recent observations on ants and termites.[1]

In brief, the author believes that "when Weismann has been worsted in controversy it has often been through the cumbersomeness of his own armour rather than the skill of his adversaries. Notably has this been so when, arrayed in his elaborate panoply of biophors, determinants, ids, idants, he has tried a tilt with them on the much-vexed question of the reproduction of lost parts. When a cray-fish loses a claw, he proceeds to grow another. The salamander is celebrated for its power of replacing lost limbs. How explain this, he is asked, if the germ-plasm lives apart? Then appear the armies of determinants that he has called into being, and he marshals them, each in its place, not without difficulty. This I cannot help regarding as a self-imposed burden, as I have already said. I wish now to point out that the reproduction of lost parts should be the *bête noir* of the Lamarckian rather than of the Neo-Darwinian. For it is clear, when a lobster replaces a lost claw, that nothing external is the cause of the re-growth; the loss has merely stimulated the organism to put forth a power that belonged to it at birth, viz., the power of replacing a lost limb."

It may be remarked that the author fails to make use of some valuable anti-Lamarckian arguments, *e. g.*, those drawn from the migrations of animals in Professor Brooks' lecture ('The Foundations of Zoölogy,' pp. 108–111), and mimicry and protective resemblance are

[1] Many authors (Forel, Miss Fielde, the reviewer and others) have shown that worker ants (sometimes incorrectly called 'neuters') often lay eggs which develop into males. The reviewer believes that this is of quite frequent and normal occurrence, at least in well-established formicaries, and that it certainly opens a path for the specific characters of the workers to be transmitted through certain males to the eggs of the queens and through these to succeeding workers and queens. In a word, the 'determinants' of the workers must find their way repeatedly into the germ-track (Keimbahn) of the species. Fertile workers among bees have often been observed, and Heath and Silvestri have recently given good evidence to show that a similar condition obtains among the workers and soldiers of termites. It would seem therefore that in the well-known Spencer-Weismann controversy in regard to worker insects, both parties assumed conditions unwarranted by the facts.

not emphasized, probably because these subjects have been more fully considered by other Neo-Darwinians (Wallace, Poulton).

Chapter IV., one of the longest in the book, deals with 'Natural Selection.' In this and the following chapter on 'Sexual Selection,' the author skilfully draws on his extensive knowledge of birds, although much of the material presented is well known to the general reader. Mr. Headley is delightfully frank in dealing with the weaknesses of speculative biologists. We have seen this in the above-quoted passage on Weismann. It is also apparent when he says: 'Correlation, like Mesopotamia, is a blessed word; the evolutionist calls it to his aid when in dire distress.' The author is himself of a decidedly speculative turn and his book abounds in conjectural ways of accounting for the origins of certain adaptations, like flight in birds, lungs in the Amniotes, loss of sight in cave-animals, etc. At the present time such disquisitions have little more than rhetorical value. They remind us of the fictitious speeches in the Greek historians, and are quite in harmony with the elaborate Haeckelian 'Stammbaum,' with its gnarled and inflexible branches, reproduced on page 139. Still these chapters abound in suggestive paragraphs like the following: "The sudden loss of horns brings out a point to which, I think, attention has never been directed in discussions on pammixis. The evolution of new characters is a gradual process requiring ages of time. Geology shows that the stag's antlers have grown step by step from small beginnings. But they might be completely lost in a single generation. The horns of cattle, though less magnificent, are none the less the slow product of ages of unintermitted selection. But by a sudden freak they disappear utterly in an individual here and there, or leave only a dangling vestige attached to the skin.

"Those evolutionists who love symmetrical theories, mapped out regardless of observed facts, imagine a process of retrogression by which all the stages in the evolution of the most complex animals may be retraced in ordered succession. What actually happens is usually very different. An elaborate organ is suddenly much reduced and mutilated or suddenly disappears altogether. The wing of the apteryx has not become by reversion a reptile's fore-limb, then the fin of a primitive fish, and after these transformations slowly dwindled. Under the downy feathers is a minute avian wing, not a reptilian fore-limb or a fin; the machinery of flight has been simply reduced, and, probably not by slow degrees, reduced almost to the vanishing point, so that loss rather than reversion is the word to describe what has happened. No ancestor of the apteryx had a fore-limb similar to the vestige that survives in the present representative of the line. We have here the result of a tendency that is, I believe, universal in the organic world, of heredity tending to fail and let the complex organism that evolution has built up suffer dismemberment."

Mr. Headley attributes less importance to geographical isolation and more to natural selection in the production of species than did Gulick or Romanes. Occasionally there are obvious contradictions between the facts recorded and the inferences. Thus at page 115 he calls attention to the male stickleback which 'makes a nest and protects the eggs, guarding them from his own cannibal unmotherly spouse,' whereas three pages further on he asks the question: "Why do 'water-breathing' animals, those that for respiration make use of oxygen dissolved in water, all allow the direct and pitiless incidence of natural selection upon their young?" Besides this *lapsus* he appears to have overlooked completely the Embiotocid fishes and the viviparous sharks, brilliant examples of water-breathers that shield their young from the 'incidence of natural selection.'

The principle of 'organic selection' is accepted and succinctly stated in the following words: "A congenital variation, in itself too minute to affect the question of survival, may gain selection value through exercise. The variation having thus been saved by exercise, further variations in the same direction may occur." The examples cited in support of this principle are highly conjectural and, therefore, not very forcible.

The second half of the book contains much interesting material on human evolution. There are luminous, but not necessarily very original, discussions on the evolution of race-energy and physical degeneration (considered under the captions of stature, disease, deaf-mutism, nerve-strain, alcohol), on the possible means of checking degeneration, on the roots of morality and religion and on intellectual development. A very brief summary of the author's main conclusions is given in the opening paragraph of the twelfth chapter: "I have shown or attempted to show (1) that the tendency of civilization is to reduce physical vigor; (2) that among men natural selection often fails to eliminate individuals for conduct that is ruinous to the tribe or nation to which they belong; from this failure of natural selection has resulted the evolution of morality and religion which have stepped in to save the human race from falling victims to their own vices; that now among civilized peoples evolution is working mainly towards a strengthening of the moral virtues; (3) that intellect, though there is no increase in actual brain power, is yet becoming more and more potent, since it has at its command an ever-growing accumulation of knowledge."

The work closes with a timely study of Chinese institutions. The illustrations, confined to the first part of the book, are very meager and taken mostly from well-known text-books of zoölogy.

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Dictionary of Philosophy and Psychology. Written by many hands and edited by James Mark Baldwin, with the coöperation and assistance of an International Board of Consulting Editors. New York and London, Macmillan. 1902. Vol. II., pp. xvi + 892. $5.00 net, per vol.

In the Psychological Review of March, 1902, a review of the first volume of the 'Dictionary' was presented. It was possible at that time to discuss the general plan of the work as announced in the Editor's preface, and to form some preliminary judgment of the way in which the plan had been carried out in the treatment of the topics as far as the term Law. The second volume, which appeared some months ago, contains the text from Law through *Z* and also contains full indexes of the foreign terms. This completes the text of the Dictionary. The third volume is to be devoted entirely to a general bibliography and will contain no further treatment of topics. It is possible, therefore, to review at this time, not only the new matter presented in Vol. II., but also the whole work as far as the text is concerned.

The second volume is a much bulkier book than Vol. I. There are 828 pages of text in Vol. II. as compared with 644 in Vol. I., and in addition, Vol. II. contains 75 pages of indexes. This difference between the sizes of the volumes is due in part to the large number of important topics which naturally fall in the second half of the alphabet.

The articles under P, for example, cover 152 pages. The longest articles under this letter are as follows: 'Patristic Philosophy' (5 pages) by Professor Shanahan; 'Philosophy' (6 pages), by Professor Dewey; 'Probability' (9 pages) by Professor E. W. Davis and Professor F. Franklin; 'Proposition' (9 pages), by a number of writers, but chiefly by Mrs. C. L. Franklin, and 'Psychology' which is given under a series of subtitles and covers nine pages.

The articles under S cover 176 pages. The longest articles are: 'Space' (6 pages), by Professor Gardiner; 'Speech and its Defects'

(11 pages), by Professor Jastrow, with remarks by the Editor; 'St. Thomas and Roman Catholic Theology' (4 pages), by Professor Shanahan; 'State' (5 pages), by Professor Bosanquet; 'Syllogism' (11 pages), by Mrs. C. L. Franklin and Dr. C. S. Peirce, and 'Symbolic Logic' (10 pages), by Professor Couturat, Mrs. Franklin and Dr. Peirce.

Without attempting to describe in detail the scope of each letter, mention may be made of the longer encyclopedic articles which have not been referred to in the lists above given. One of the most valuable articles in the dictionary is the article on 'Vision' (26 pages) written by a number of contributors and presenting very clearly and fully the status of present knowledge on the subject.

The 'Nervous System' is treated in an article 13 pages in length by C. L. and C. J. Herrick. There are also articles on localization, and on the spinal cord, in this volume. When it is remembered that the longest article in Vol. I. was on the brain, we see that the nervous system has been treated at great length in the work. The treatment is, furthermore, very comprehensive and clear. The most obvious criticism on this series of articles is that the distribution of matter under the various titles does not seem to have been very carefully planned. For example, the article on brain frequently overlaps the article on nervous system. To such an extent, indeed, is this true that one cannot read the one article without referring to plates in the other. Furthermore, there is no reference whatever in the article on localization to the work of Flechsig, and the table of reflex centers in the spinal cord is given, not under the title spinal cord, but under nervous system.

'Variation' (8 pages), 'Uniformity' (5 pages), 'Organic Selection' (4 pages), 'Origin versus Nature' (6 pages) and other related topics are very fully discussed by various contributors and the Editor. Under 'Selection' a table is given by the Editor classifying the various forms of selection and stating the essential characteristics of each form.

'Oriental Philosophy' is treated in an article 23 pages in length by Rev. J. E. Carpenter, of Manchester College. In this article Egypt, Babylonio-Assyria, Persia, India and China are discussed. Religion is discussed with reference to its evolution, its philosophy and its psychology by Professors Ormond, Wenley, Baldwin and others.

Other special topics are 'Optical Illusions' (8 pages), by Professor Sanford; 'Time' (6 pages), by Professor Gardiner.

This array of encyclopedic articles in the second volume accounts, as has been said, in some measure for the greater bulk of this volume as compared with the first. But closer examination of the articles convinces one that the plan of the work has undergone an internal as well as external enlargement as later stages have been reached.

Thus, there is a clear disposition to give Greek philosophy a fuller recognition. An article on Schools of Greece has been inserted which reviews in a general way all phases of Greek thought and deals in extenso with the Epicurean and Stoic Schools. It is a very desirable article for such a dictionary. It seems to be somewhat crowded out of place when it has to work its way in under the obscure heading Schools, but it is certainly better late than never. One will regret that the article on Epicureanism in the first volume could not have been correlated in any way with this later article by even so much as a reference forward. And one will be disposed to question a plan of dictionary making which allows two independent discussions of Epicureanism and no special article on Stoicism. But some minor lack of uniformity in the methods of attaining the full purpose of the work is doubtless due to the difficulty of planning in detail for every phase of such an extensive undertaking.

Again, one must note that the second volume has followed the negative rather than the positive method of curing the biography. There are only one half as many biographies in the second volume as in Vol. I. Here again alphabetic conditions explain in part, but only in part, the difference.

The second volume is much freer than was the first from terms which have no direct relation to the main purposes of the book. It was possible to mention at random under any one of the letters in the first volume irrelevant terms, but one will find very few such terms in the second volume.

The complete use of the foreign terms in the Dictionary is now made possible by the addition to this volume of Greek, Latin, German, French and Italian indexes. So far as the researches of the present reviewer have extended, these indexes are complete and accurate. There is need for a similar index of the leading English terms. The dictionary contains a great deal of valuable material which will not be easily accessible to the ordinary student because it is distributed under widely separated headings. Even the cross references between topics do not fully direct the reader in his search for these discussions. An English index like the indexes of foreign terms would do much to remedy this difficulty and would reduce the whole work, encyclopedic articles and shorter dictionary articles alike, to a much more uniform standard of accessibility.

From this description of the second volume and this comparison with Vol. I., we turn now to a more general view of the work as a whole. As pointed out in the discussion of the first volume, the present reviewer must leave the evaluation of the topics outside of the field of psychology and its immediately related subjects to those who are competent to pass judgment in those spheres. It is possible for the psychologist, even though he look through the dictionary in a casual way, to recognize in many of the departments outside of his own clear-cut, consistent lines of thought running through the dictionary. The various articles on the philosophy of religion, on æsthetics, on epistemology, and on other general groups of subjects, all furnish good illustrations of what can be done in the way of preparing critical statements on current topics in these fields.

Turning now to the psychology presented in the dictionary, we have a most valuable and highly instructive exhibition of the present conditions within our science.

In the first place, one becomes conscious during a critical examination of the dictionary that there are many terms which are not capable of any universally acceptable definition. This appears in some cases in the fact that the writers of the dictionary articles have found it necessary to record divergent usages. It appears also in many cases in the fact that the recorded usages do not include those which are to be found in works of standard authority. The terms which belong more especially to experimental psychology are, as one would naturally expect, capable of briefer and more precise definition. The terms which are broad and general in their connotation have not yet been brought to the same clearness and definiteness as the terms of corresponding scope in the physical sciences. Indeed, in the dictionary articles one frequently finds that there is a disposition to try to force agreement in spite of the obvious impossibility of securing agreement. The article becomes in every case of this kind a piece of special pleading. The psychological position of the author is aggressively brought to public attention. All this is legitimate and valuable if the dictionary is to be put on an equal footing with other elaborate discussions of psychology, but it leaves the student without the settlement of many of the problems which a large board of authorities coöperating in the critical sifting of usages was expected to furnish. The dictionary will lose its highest value, will indeed be in frequent cases positively misleading, if it is appealed to as a final authority on current usage of terms, if by current usage we mean the usage of all the leading authorities.

One illustration must suffice to make clear the meaning of these statements. Take the definition of the all-important term mind as it is given for psychological usage. Mind "in psychology" is "the individual's conscious process, together with the dispositions and predispositions which condition it. It is thus the individual's consciousness, with its capabilities; its capabilities including all faculties, powers, capacities, aptitudes, and dispositions, acquired and innate." Certainly this definition with its emphasis upon the term individual is wholly inadequate to cover either the spirit or letter of Wundt's statement, "es ist einleuchtend, dass in ihm (diesem empirischen Sinne) die 'Volksseele' genau mit demselben Rechte eine reale Bedeutung besitzt, wie die individuelle Seele eine solche für sich in Anspruch nimmt." (Völkerpsychologie, Vol. I., Pt. I., page 9.) Or again, to quote from Sully: "In addition to such wide and varied observation of individual minds, the objective study of psychical phenomena should include manifestations of products of the collective mind, that is to say, of the mind of the community or society * * *" ('Human Mind,' Vol. I., page 20). Furthermore, even when the word mind is used to apply to individuals rather than to communities, certainly it is common to emphasize in some sense, not merely the existence of consciousness, but its unity, its organization into a single system. Mind is not to be set over against consciousness as its synonym merely; mind means all of consciousness, but it means especially the combination of all phases of consciousness into one. As Ward has put it: "In psychology, on the other hand, the individual mind may mean either (1) the series of feelings or 'mental phenomena' above referred to; or (2) the subject of these feelings, for whom they are phenomena, or (3) the subject of these feelings or phenomena plus the series of feelings or phenomena themselves, the two being in that relation to each other in which alone the one is subject and the other a series of feelings, phenomena or objects" ('Encyc. Brit.,' XX., 39).

One can hardly refrain from expressing the wish that 'mind in psychology' had been included in the list of subjects to receive more extended encyclopedic treatment.

Recognizing, then, the chief psychological articles as discussions, rather than as final statements of authority, or even as exhaustive reviews of usage, we find it interesting to note the clear tendencies which many of these articles exhibit.

The majority of the elaborate psychological discussions are written by the Editor and Professor Stout. Both writers exhibit a marked interest in the genetic aspects of the subjects which they treat. Indeed, throughout the dictionary one finds constantly recurring evidences of the great influence of the principles of evolution and development on all our present-day thinking. This is perhaps one of the strongest and most important features of the work. The discussion of psychological topics from this point of view promises much for the future of the science. The work that Professor Baldwin and Professor Stout have already done along the lines of genetic psychology is here presented in new and often more compact form than in their earlier writings.

The close relation between psychology and biology in all of its branches, is fully recognized by the dictionary. Indeed, the superficial observer will form the impression, because of the long encyclopedic articles on brain and nervous system and other anatomical and physiological topics, that the work is one in which physiological psychology gets unusually full recognition. This first impression will be somewhat modified when one considers that the purely psychological discussions are much more numerous, though frequently shorter, and that the material on brain and nervous system has been massed at a few points. But even with this mode of distribution clearly in mind, the reader will find no cause to change his view that physiological psychology is adequately treated.

The relation of psychology to the social sciences has been fully recognized. Indeed, under the heading Social Science the Editor has given a table of classification in which the various social sciences are distributed with obvious regard to their relation to psychology and philosophy.

The discussions of abnormal psychical processes, most of which are written by Professor Jastrow, present their subjects from the point of view which Professor Jastrow has always adopted in his writings on these subjects. That point of view can be briefly characterized by the statement that it is itself eminently sane and free from any lack of clearness. There is no tendency in all these articles to depart from the more general mode of thought which is everywhere recognized as appropriate to normal psychology.

In concluding this description of the text of the dictionary it is appropriate to repeat the expressions which are called out by the first volume. The dictionary is a work conceived on a large scale and brought to completion in a manner thoroughly creditable to editor, contributors and publishers. It is the only book of its kind in our language, and it will doubtless exercise a large influence on the future of psychological science and psychological terminology. It is stimulating to students and workers in the fields which it covers, and where one is not fully in accord with the details of the plan on which the book was prepared, or with the conclusions reached by the contributors, he will at least find much valuable material here at his immediate disposal for reflection and for use in balancing his own views.

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GENERAL.

Zur Lehre von den Allgemeinbegriffen. G. STÖRRING. Philosophische Studien, Zwanzigster Band (Festschrift, Theil II.). Leipzig, 1902.

General concepts (Allgemeinbegriffe), or concepts as such, must first be distinguished from general images (Allgemeinvorstellungen). There is really no such thing as a general image, in the usual sense; every image of a triangle must, as Berkeley claims, be either right-angled or oblique-angled, etc.; in other words, the general image, commonly so-called, is really nothing but a single image. A single image may, however, represent a number of completely similar objects; or, if vague, it may be applied indifferently to several dissimilar objects, as when the child calls different objects by the same name, without distinguishing them. But this is quite different from a concept; the latter is applied to different objects which are at the same time distinguished. The question arises, then, what part is played by the single image in the conceptual process? The answer is that its function is representative (stellvertretend). The representative image stands out against other similar single images lying in the background of consciousness. But the first stage of the conceptual process is not the recognition of this similarity but the analysis of content and relations found in the representative image. Then, by association, through a common content, with the other images present, it becomes representative of a class and thus converts the process of analysis into a conceptual process. Since the image must also be differentiated from each image of different content, it is evident that a number of negative judgments are necessary to render the conceptual process complete. But the last is true only of more complex images; the concept of a simple content-element requires merely an analysis of this element out of the whole content; and that of a simple relation presupposes only an identical judgment (A is A) in which the relation is posited. The author concludes with a reply to Volkelt, who claims that a concept can never be complete because completeness would include an intuitive apprehension of the whole content of the object, — the concept indicating merely the *direction* of the thought. According to our author, it contains in itself the complete thought, and the direction referred to is merely that of the development of the image.

Eine Willenstheorie vom voluntaristischen Standpunkte. N. Lossky. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. 30, Heft 1 u. 2. Leipzig, 1902.

The paper begins with a preliminary definition of voluntarism and its method. Voluntarism is the theory which holds that the typical form of conscious process is voluntary activity — that within the Ego there are no static (bleibenden) states, but only purposive activities; its method, therefore, is to take the obvious instances of voluntary activity as its point of departure for the analysis of mental life as a whole. Upon this basis we then distinguish the following: (1) *My* impulse, (2) the feeling of *my* activity, (3) the changes brought about by my activity. (1) We must distinguish between my impulses and those that merely occur in my consciousness (mir gegeben). The latter orginate many of the acts of my own body; for example, the drunkard holding the glass in his hand may drink as the result of an express determination, or merely mechanically, without really taking the matter into consideration. Only those impulses which I will to satisfy are properly mine. But, again, this willing may be spontaneous or forced upon me. We must, therefore, distinguish three grades in the voluntary character of our activities; *my* (spontaneous) decisions, forced decisions, and acts not deliberated upon and hence not the result of a decision. But for a complete definition of what is properly mine it must be noted that the impulses included in the activity are not all clearly analyzed; some of them are below the threshold of consciousness and the total motive, as far as these are concerned, is an unanalyzed impulsive feeling. Ultimately, moreover, no impulse has a separate object of its own; what we discover is that each is but a function in a continuous process of purposive activity. (2) The feeling of activity is ultimate and unanalyzable — not resolvable into either muscular sensations or sensations of innervation. (3) The changes following the impulse also vary in the degree to which they are 'mine.' There is my inner act, in which I create wholly new combinations of images; my partially internal act, in which the new combinations are due partly to external suggestion; and my external act, which means merely that a movement of my body follows the appearance of an impulse. With regard to the last-named, it is the object to be attained, and not the movements themselves, which constitutes the content of the impulse, and therefore the movements as such are not truly mine. All phenomena of consciousness may then be resolved into three classes — 'acts of will,' or my acts, 'acts in me,' the result of impulses opposed to mine, and 'states of consciousness,' not connected with impulse. But, according to the author, this distinction is ultimately a distinction of degree only. Every mental process contains a certain measure of given material. Whether it is 'mine' or simply 'in me' depends upon its nearness or remoteness to the focus of attention. This is determined by the extent to which the given material is acted upon by the impulses which I identify with myself and accompanied by the feeling of self-activity — nothing which enters consciousness being altogether free from these influences. The voluntaristic theory holds then that every conscious process, so far as it is apprehended as mine, has all the essentials of a voluntary act and is determined by the impulses identified with self.

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Some Examples of the Use of Psychological Analysis in System-making. MARGARET FLOY WASHBURN. Philosophical Review, XI., 5, Sept., 1902, pp. 445–462.

Dr. Washburn summarizes, compares and discusses the different methods of analysis and the consequently different conceptions of 'attribute' and of 'element' which underlie the psychological classifications of Wundt, of Ebbinghaus, and of Münsterberg.

The method of analysis adopted by Wundt and by Ebbinghaus may be termed 'psychological'; it is the 'method of calling mental phenomena elementary because they are the simplest phenomena that, as capable of being *experienced apart* from each other, may be attended to separately (p. 453).' Miss Washburn rightly, however, criticises both writers in that they insufficiently distinguish this method of analysis from a second — that employed by the writer of this review,[1] 'the method of calling mental phenomena elementary because they are the simplest phenomena that, being independently *variable*, may be attended to separately.'

On the basis of the first of these principles of analysis Wundt, as is well known, (1) classifies psychical elements as sensations and feelings, and (2) endows these elements with the 'attributes' of qual-

[1] 'An Introduction to Psychology,' 1901, Chap. viii.; this REVIEW, July, 1900, p. 378 *seq.*

ity and intensity. On the first of these procedures, Miss Washburn remarks (p. 454) that while often claiming a psychological ground for the classification, Wundt really emphasizes the epistemological basis of it, in the teaching that the psychical elements correspond 'to the two factors contained in immediate experience, the objective contents and the experiencing subject.' With reference to the second point, Miss Washburn infers (p. 457) "that Wundt means by the attribute of a sensation a character that does not spring from the togetherness of sensations, and that is not even influenced * * * by * * * the mental context." On the basis of the first of these criteria, she agrees, Wundt rightly excludes extension and duration from the class of attributes (she fails here, as Wundt does, to take account of the sensationalistic conception of bare 'extensity'—distinct from form). But Miss Washburn objects to the exclusion of 'vividness' from the class of attributes; vividness, to be sure, is affected by the 'mental context,' but so also, she argues, is the 'quality' of sensations. Wundt's classification of feeling, finally, is subjected to less fundamental criticism; but it is rightly urged that, by his own standards, pleasantness and unpleasantness should be ranked as feeling-qualities rather than as feeling-directions.

Ebbinghaus recognizes three groups of psychic elements, sensations, ideas and feelings. But his 'ideas' are merely 'centrally excited sensations' and have — as Miss Washburn points out — no real right to be classed as distinct psychic elements. Sensations, Ebbinghaus further teaches, have attributes of two sorts: specific and generic. "Specific attributes * * * are those * * * which no two classes of sensation possess in common; they are generally grouped under the two heads of quality and intensity, but these names do not refer to any real sameness among qualities and intensities themselves, only to two kinds of relation to the stimulus. * * * On the other hand, the generic attributes are actually and in themselves identical for different classes of sensations (pp. 458–459)." These generic attributes are: extension duration, movement, change, similarity, difference, unity and plurality.

It is thus evident that Ebbinghaus conceives of the generic attribute as "any conscious content which is so far dependent upon sensations that it arises only in connection with them, and is yet a bit of immediate experience not an artificial product of the transformations of thought." In the opinion of the present writer, Miss Washburn is justified in her basal criticism of this position. All the generic attributes, she points out, "except duration and extension would by most authorities be called independent bits of conscious content (p. 459)." They have, in fact, as good a right as the 'feelings' to be classed as elements of consciousness. A second criticism is implied in the quotation just made. The generic attributes, as enumerated, are extremely heterogeneous in character: both extension and duration should, in fact, be reckoned with the specific attributes.

The analysis at the basis of Münsterberg's psychological classification is of two sorts; and Miss Washburn points out that the two methods are not thoroughly coördinated. Münsterberg employs, first, "as * * * criterion of an element, that it is the final stage of a *parallel* analysis of mental state and physical stimulus" (p. 451). From this point of view he holds that the sensation is the only psychic element, on the ground that "the physical stimulus in the outer world can not be dissolved any further without ending the mental effect." But Münsterberg further uses "the method based on the assumption that because similarity between complexes involves partial identity, all similarity is partial identity and is between complexes" (p. 454). From this assumption he infers that sensations, because similar, are complex not elemental, and that they are made up of elemental 'psychic atoms.' On the first of these methods Miss Washburn remarks: "The notion of what constitutes a physical analysis needs to be made more precise * * * or we have gained nothing by demanding a parallelism (p. 451)." As a matter of fact, she points out (p. 456), the conception of the sensation as the only psychic element is based by Münsterberg on epistemological rather than on psychological grounds. "Nothing but sensations can be allowed in the scheme of elements because only sensations correspond to distinguishable parts * * * of objects; and only by relating our mental states * * * which we alone can know, to objects which are accessible to others, can we accomplish the second main purpose of analysis, besides explanation, namely description." In Münsterberg's second system of analysis Miss Washburn disputes the premise that similarity always is partial identity. The similarity of concepts, she argues, is rightly so described, but absolutely simple sensations as well as concepts may be similar (p. 453).

Münsterberg's classification of sensational attributes ('qualities' as he calls them) is three-fold. He distinguishes (1) content qualities (kind, intensity and independence), (2) form qualities (with the two main heads, space-form and time-form) and (3) value qualities, which "represent epistemologically the attitude of the subject." Miss Washburn holds that "the term 'quality' is not used in the same sense for the three classes. Content qualities," she says, "seem really to be attributes belonging to the sensation element, in the ordinary sense, that of independently variable aspects." Form qualities, on the other hand, described as themselves possessing kind, intensity and independence, are 'as a matter of fact sensational elements.' The value qualities, finally, are closely parallel with Ebbinghaus's group of 'generic attributes.'

Different readers will draw widely different conclusions from Miss Washburn's clear exposition and discriminating criticism of these three theories. To the present reviewer, for example, it seems perfectly clear that the difficulties in the way of the three methods of analysis, employed by Wundt, by Ebbinghaus and by Münsterberg, are avoided by that other method which defines the element as the 'independent variable' and recognizes qualities, intensities and extensities as themselves sensational elements. Similarly, the inconsistencies in Ebbinghaus's treatment of 'generic attributes' and the ambiguities in Münsterberg's enumeration of 'qualities' suggest to the writer the need of a class of 'relational elements'—immediate consciousnesses of likeness, difference, one-ness and the like, the analogues of Ebbinghaus's generic attributes (extension and duration, excepted) and of a group of Münsterberg's value qualities.

But, however the specific deduction varies with the individual reader, the practical outcome of the paper is the same for all—a persuasion of the need for greater uniformity in psychological method and in definition. "The differences," as Miss Washburn says in her concluding paragraph, "in these three systems of psychology are due to different conceptions of the nature of analysis, * * * [of] the relation of an attribute to that on which it depends, * * * of the relation between epistemology and psychology. * * * If agreement could only be reached on these three points, two of which are mere matters of definition, it would certainly be a most important advance in psychological method, and a valuable saving of time now wasted in dispute about words."

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Classification of Psychical Phenomena for Experimental Research. Toulouse, Vaschide et Pieron. Mind, N. S., No. 44, Oct., 1902, pp. 535-537.

This paper proposes a 'practical classification' of psychical phenomena, 'especially intended for experimental work' and completely freed from the traditions of 'classical psychology.' In truth, however, this scheme of classification is based throughout on theoretical considerations. Its recognition of 'sub-conscious' as well as of 'conscious' sensations is mainly founded (p. 536, p. 540 *seq.*) on Leibniz's law of continuity. Its conception of the complexity of sensations involves the doctrine — abundantly refuted by James, by Külpe and by many others — that psychical and physical complexity are completely parallel. Its treatment of 'affinity' — a conception covering what are generally known as association and apperception — is a mere restatement of the dogma of associationism.

In more detail, the writers distinguish the 'internal' and the 'external' sensations as sub-classes of 'the conscious sensation or elementary psychical phenomenon'; and they enumerate (p. 537) as the 'fundamental and irreducible characteristics' of all states of consciousness (1) intensity, (2) affectivity, (3) objectivation, and (4) affinity. It is obvious that these characteristics are utterly heterogeneous. 'Intensity,' moreover, is treated not only in its ordinary meaning of sense-intensity, but also as 'personal intensity,' to include both attention and memory. It is certainly confusing to use the word in both these senses; and, furthermore, the conception of 'personal intensity' is far from clear. There is, of course, a significance to one school of psychologists, in the description of attention and memory as peculiarly personal sorts of consciousness, but Toulouse and his colleagues define personality as 'complete synthesis' or aggregation of ideas, and from this point of view memory seems no more 'personal' than perception.

Opinions may differ on the value of the classification proposed, but it can not seriously be maintained that it offers any new or any consistent principle of division; and it is inconceivable that experimental psychology should be in any special manner the gainer from it.

Einleitung in die allegemeine Theorie der Mannigfaltigkeiten von Bewusstseinsinhalten. GOTTL. FRIEDR. LIPPS. Philosophische Studien, Festschrift, 1902, II. Theil, pp. 116–151.

Lipps sets out from the conception of thought-activity and thought-object (*Denkthätigkeit und Denkgegenstand*), as implied one by the other, and strictly correlative. The term 'thought' is apparently used as synonym of 'consciousness.' He goes on (§ 6) to distinguish the thought-activity as either (1) mere apprehension (*Erfassen*) or as (2) relation (*Beziehen*). Corresponding with these, in the domain of object-of-thought, he names (1) the content-of-consciousness (*Bewusstseinsinhalt*) and (2) substance, the substratum (*Träger*) of cause and effect. These two, however, he insists, are separated not in reality but only through abstraction (p. 132), so that exclusive reality can be attributed to neither one. "Therefore," he says (pp. 133–134), "neither the world of contents-of-consciousness nor the world of substances exists for itself alone, but rather both together, interwoven as they are, form the one real world of the objectively existent."

The most interesting application of this doctrine is to the problem of the classification of the sciences. "The natural sciences (pp. 134–135) belong without question to the doctrine of substances, for every nature-object exists as substance (*besteht substanziell*)," that is, as center of forces. Psychology, on the other hand, in the modern sense of the science of what one feels and knows, is the study not of soul-substance but of conscious contents.

The closing sections of the chapter, which compare the simple with the complex object-of-consciousness, are not very closely connected with the rest. They include (1) a forcible exposition of the truth that simplicity or complexity of the conscious content does not imply simplicity or complexity of the bodily accompaniment; (2) a conventional analysis of the content-of-consciousness into quality and extensity; (3) an attempt — unsuccessful in the opinion of the present writer — to show that simplicity and complexity are merely relative terms and that, consequently, all supposedly simple conscious contents are analyzable. From this follows the possibility of a 'psychical atomism' like that advocated by Münsterberg, who, however, is not named here.

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Wellesley College.

Physiological Factors of the Attention Process. (I.) W. McDougall. Mind, N. S., No. 43, pp. 316–350.

The writer starts on the assumption that the psychological facts in connection with attention are already pretty thoroughly known and that what is now lacking is to supply the physiological basis for those facts. This he believes to be as completely wanting as the psychological factors are supplied.

The earlier pages of the work are devoted to a brief *résumé* of the current theories. Most of this portion of the paper is devoted to a drastic criticism of Münsterberg's *Aktionstheorie*.

The author's own theory is an application of his neurin hypothesis, published in *Brain* last year. Briefly, nervous action is pictured as the flow of a hypothetical fluid, neurin, through afferent and efferent fibers from sense organs to muscle plates. The flow is checked and regulated by the varying resistances of the synapses, which are pictured as valves that work only in one direction. There are said to be four levels on which these connections may be made, although the writer has a most irritating way of naming but three wherever he gives a specific enumeration. The three levels are the spinal and subcortical connections, the points of connection in the sensory areas of the cortex and the association areas of Flechzig. Each higher level is distinguished from the lower by an increase in the number of paths which may be taken, and in the complexity of the connections.

Differences in the direction of the attention mean merely the opening of different paths of connection in the cortex between sensory and motor areas. When an indistinct sound is interpreted as the clucking of a hen one series of connections is open, when later it is found to be the cry of a hawker at a distance there is another set open. All differences reduce to a question of the channel that the nervous impulse may happen to take. Not only attention, but all of what James calls the substantive states of consciousness are explained as due to these higher level paths. The transitive states are provided for by assuming that there are collaterals which connect the various paths of the same level and permit crossing from one to another.

The author promises to discuss the problems as to why the neurin takes the high level paths at all, and why one rather than another, together with the fact of fatigue and of the mutual exclusiveness of the paths, in later articles. Until it is seen what success is had with these crucial questions, it is not possible to criticise the theory, but as it stands it seems very schematic, to have slight basis in fact, and if true to offer no advantages in definitiveness or simplicity over the familiar theories of the day.

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VISION.

Der linear-perspectivische Factor in der Erscheinung des Himmelsgewölbes. GEORGE M. STRATTON. Zeitschrift f. Psych. u. Physiol. der Sinnesorgane, XXVIII., 42–45.

In opposition to von Zehender's contention that the flattened appearance of the clouded sky is explained by the physical facts of the eccentricity of the observer's position within the cloud sphere, Stratton holds that the most important factor in the explanation is visual, and depends on the general laws of perspective.

Die scheinbare Vergrösserung der Sonne und des Mondes am Horizont. EUGEN REIMANN. Zeitschrift f. Psych. u. Physiol. der Sinnesorgane, XXX., 1–38 and 161–195.

The first part of Reimann's paper is historical. Beginning with Aristotle it gives a detailed account of the various hypotheses that have been offered in explanation of the apparent enlargement of the heavenly bodies at the horizon.

The second part is theoretical and critical. At the outset the author takes the discussion onto a higher plane than usual by seeking a quantitative expression of the illusion. Measured in terms of the distance at which a white disk must be viewed in order to appear equal in size to the sun, the apparent size of the sun at the zenith was found to be related to its apparent size at the horizon as 38.11 to 11.47. The ability to explain this enormous difference is made the test of the hypotheses. The change in direction of the line of regard is shown by simple experiments to be altogether inadequate. Comparison of the sun with terrestrial objects is likewise shown to be inadequate. Of the theories depending on changes in the apparent distance, the exaggeration of divided distance and the influence of aërial perspective are both dismissed without discussion as unsatisfactory. The apparent shape of the sky, on the other hand, is accepted as the real ground of the illusion. By measuring the real elevation of the apparent middle point of an arc connecting the zenith with the horizon, it is found that the apparent length of the horizontal radius of the sky is from three to four times the length of the vertical radius. This relation is confirmed by astronomical observations on meteors, and it coincides fairly well with the changes in the apparent size of the sun. It is Reimann's effort to show that this apparent flatness of the sky dome is not a mere illusion which in turn demands an explanation, but a fact based on simple optical laws. In a murky atmosphere, the more distant objects are obscured, as it were, with a gray wall. In the clearest weather the same limitations to vision are still present, only they are further removed and changed in color from gray to blue. This wall is always relatively nearer at the zenith in cloudy weather because of the position of the opaque cloud stratum; in clear weather, not because of any real interference to vision, but because in the higher strata of air there are relatively few particles capable of reflecting light enough to penetrate the denser lower strata. The highest point from which the light of the rising sun is reflected to the earth in sufficient quantity to be visible is about 15 km., while the greatest distance at which mountain peaks are visible in clear weather averages about 50 km. These facts agree both with the apparent relative distances of the zenith and the horizon, and with the apparent changes in the size of the sun and moon, and they are consequently held to constitute an adequate explanation of the illusion. The psychological difficulties involved in the assumption that we have direct visual data for the comparison of such enormous distances are not discussed.

Ueber das räumliche Sehen. E. STORCH. Zeitschrift f. Psych. u. Physiol. der Sinnesorgane, XXIX., 22–43.

From the field of psychiatry, Storch always brings interesting and valuable data to the discussion of psychological problems. But his unusual indifference to the history of his problems, as well as to the results of previous discussions, seriously detracts from the pertinence of his contributions. Storch contends that the monocular visual form yields no definite spatial perception of form, but includes innumerable possibilities, constituting a 'visual concept' which becomes definite only through experience. "Concerning the real distance and size of objects binocular vision can teach us no more than monocular." "Depth exists as sensory stimulus only for touch." The indefinite visual factor must be supplemented by the sum total of the spatial perceptions, which arises through the other senses, and for which the visual factor is a symbol. The Müller-Lyer figure, the Poggendorff, and other illusions are offered as illustrations and proof of the influence of the depth factor in the visual perception of plane figures.

RAYMOND DODGE.

SPEECH FUNCTIONS.

Unfähigkeit zu lesen und Dictat zu schreiben bei voller Sprachfähigkeit und Schreibfertigkeit. C. RITTER. Zeitschrift f. Psych. u. Physiol. der Sinnesorgane, XXVIII., 96–130.

Without entering into the bearings of his case on the problems of the psychology of reading, the author limits himself to a painstaking, if sometimes tedious, examination of what we would call a high-grade imbecile, who presents an unusual form of defective linguistic development. The patient, a healthy man, twenty-six years old, can name most of the letters of the alphabet at sight; but, notwithstanding ordinary schooling, he has never been able to read them combined as words. He can copy without gross errors but is unable to write anything from dictation except his own name. Numerals, however, as our general knowledge of the differences between them and words would lead us to expect, are correctly read as wholes into the tens of thousands.

RAYMOND DODGE.

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Studies of Melody in English Speech. By E. W. SCRIPTURE. Philosophische Studien, XIX., 599–615, 1902.

This contribution of Professor Scripture to the volumes of the *Studien* published in honor of Wundt's seventieth birthday, throws a side light on the author's elaborate investigation of phonetics. Dr. Scripture supplements the important work which he has done in measuring the pitch and intensity variations of speech in curves traced from gramophone plates, by here measuring the record of sentences spoken into a mouthpiece connected with a Marey tambour. Unfortunately we have no record of the same sentences spoken into the gramophone. A comparison of the two methods would offer a valuable check on the reliability of the gramaphone for registering the voice. It is quite likely that the sounds are changed by the resonators of any phonographic instrument. We would like to know if the changes are sufficient to disturb the records for the purposes to which they are put. So far as the cord tones of the voice are concerned, it is probable they are not. We may judge from the present paper that Dr. Scripture found no differences in the records made under the two methods which were sufficiently great to cause him to suspect the accuracy of the results previously published from the gramophone.

"In the melody of speech," Dr. Scripture finds, "we have a partial record of the emotional expression of the speaker." Tables of measurements and plotted curves are given for eleven short sentences which express emotions accompanying command, interrogation and exclamation. In the gramophone records the fundamental form of pitch for the declarative sentence was shown to be a circumflex melody. The variations from this are traced for each of these new sentences. The records are for the voice of Dr. Scripture himself and were taken at the Collège of France, Paris, on apparatus of the Abbé Rousselot. The curves bear out those of the Jefferson toast made on the gramophone in the fact that the variations in tone are found not to be great, rarely reaching an octave. The presence of sonant 'h' as well as a surd 'h' is again demonstrated. Dr. Scripture finds opportunity to criticise certain statements in Sweet's 'New English Grammar' regarding the intonations of the English language. The presence of a circumflex melody in either isolated words or in sentences used to express surprise is shown by the records, while Sweet states that for single words thus used there is a steady rise in pitch and for sentences there is a fall. Emphatic words were also shown to have this rise and fall of pitch. The sentences for which the measurements are given were taken from examples in Sweet's Grammar, so we are making some progress toward an empirical examination of language problems which have been tossed back and forth for years with little hope of solution until they could be taken up in the laboratory. It is pleasant to note that the work of Dr. Scripture on phonetics is being further carried forward, an appropriation having been made by the Carnegie Institute partially to meet the expense incurred.

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SPACE.

Die Dimensionen des Raumes. A. KIRSCHMANN. Philosophische Studien, Bd. XX. (Wundt's Festschrift), I. Theil, S. 310–418.

This essay of more than a hundred pages is the most pretentious contribution to the first of the two Festschrift volumes. The reader is convinced early that this is a careful piece of critical work, displaying the results of wide reading, close thinking and a general overhauling in a refreshingly sane way of some of the fundamental spatial concepts. The style of the article is usually clear and at times forceful, though lapsing occasionally into a diffuseness that detracts much from the power of presentation. The value of the essay lies in the most commendable attempt to reach some sort of clear understanding in regard to the qualitative nature of space, whether this space be that of the psychologist, the philosopher or the mathematician. The discussion that such an attempt involves is both needed and welcome, particularly in view of the fact that the lay mind, and that of some others besides, is so readily mystified by the usual arguments for a fourth dimension, and in view too of the uncritical attitude of many mathematicians who accept with ill-concealed wonder and amazement the flat contradictions between the spatial characteristics disclosed by pure intuition and those arrived at through the current analytic methods. We must frankly express our gratification that the matter has been handled by one whose equipment in the various fields mentioned is sufficient to enable him to speak with authority.

Our author is a thorough-going intuitionist in this space matter. It is of the immediately given space alone that we have any right to speak. ?
The space of our intuition is the sole space that we can attempt to analyze and characterize, from whatever point of view we may make our approach. As the best mode of procedure toward his proposed task the author selects for the main topic of discussion the problem of the dimensions of space. Is the generally accepted tri-dimensional

character of space obviously and necessarily drawn from the nature of space itself, or is it a mere conventionality adopted in the interests of demonstration or quantitative reckoning? This must be answered by every one who strives towards clearness in the use of spatial concepts.

In the first place, one must be clear about the fact that magnitude and extension are two utterly different matters. The latter only belongs originally to the space concept. The former is an imported characteristic, implying first measurement, and then intensity through which alone measurement is possible. Theories of space and theories of magnitude must therefore be kept unmistakably asunder.

The main discussion is preceded by a review of the several motives operative in producing hypotheses of a fourth or of higher dimensions. These are four in number. *First*, there is the mystically minded person who demands a fourth dimension of space for the explanation of such operations as are illustrated by the performances of Slade before Zöllner. A space is desired in which tri-dimensional knots may be united and in which egg-shells may be turned inside out without doing violence to the laws of cohesion. *Second*, psychological doctrines of space perception, though contributing no positive plausibility to the assumption of higher dimensions, seem often to imply that such higher dimensions are perfectly possible. To the partial nativists who hold that the third dimension is the product of inference, or at least a construction not immediately given, and to the empiricist who 'forcibly evolves space from non-spatial elements' there should be nothing impossible or contradictory in the thought of four or more dimensions. *Third*, the attempt of natural science to establish thorough-going causal connections in its world of fact is thwarted in at least one direction. Enantiomorphic forms and movements resist all thought of transformation from one to the other. No simple causal connection can exist between the right- and left-handed movements of the particles of such substances as sugar solutions and oil of turpentine. For only qualitatively similar movements can be transformed into each other. But the assumption of a fourth dimension would furnish an escape from this difficulty. For, by the well-known analogies, the right-handed form need only be turned through the fourth dimension to make it congruent with its left-handed companion. The *fourth*, and most influential motive has sprung from the side of mathematics. The belief in hyper-space has been fostered, if not overtly supported, by the distinctions advanced between Euclidean and non-Euclidean spaces, by the speculations of Helmholtz, Riemann, Klein and others to which expression has been given in the terms curved hyperbolic and elliptical spaces, and by the alleged analogies between algebraic powers and spatial dimensions. The practice of mathematicians to distort terms from their accepted meanings into a terminology applicable to new speculations has done much to introduce confusion and to make the mathematician appear to mean what he would not maintain if cross-questioned.

Clearness, then, and harmony between those fields wherein the concept of space is current can be best secured, the author thinks, by a critique of the definition of dimension. The various meanings of 'dimension' may be briefly passed in review. First, that meaning which is contained in the expression 'an *n*-dimensioned manifold' is properly applicable only to a manifold of independent variables and has nothing to do with the actually given space. '*N*-dimensional space' is only an unsuitable and misleading expression for a manifold of *n* independent variables. — Similarly, that notion of dimension is unsound which springs from the practice of forming a correspondence between dimensions and the powers of variables. For the prosecution of the analogy requires the assumption of perpendicular coördinates, which assumption is not based upon the nature of the immediately given space. — Nor can that definition of dimension be upheld which makes of it a fundamental *direction* in space. For the choice of the directions and their relation to one another are purely conventional matters. Eliminate the condition of perpendicularity from this convention and any number of fundamental directions may be chosen to serve as axes of coördinates. That is, the number of the dimensions of space may be as great as one pleases. For simplicity's sake only are the usually chosen coördinates and dimensions to be preferred. — The only definition that can stand the test of scrutiny is to be couched in terms of *boundaries*. For this is founded upon the space intuition. On this view the dimensions of space are merely the *grades of determinateness possessed by spatial forms*. The order of this determinateness is then expressed by the series — solid, surface, line, point. In a general sense the first dimension is just 'unlimited space extending in all directions,' the simplest and most primary deliverance of our space intuition. If confined within limits, however, the limiting form, or surface, appears as the second dimension. Successive limitations give us then, further, the line and the point, making four dimensions in all. The point is in no way the 'space-element,' nor is it in any sense non-spatial. It is rather the most thoroughly determined of all the spatial forms.

It is clear that no thought of *direction* is contained in this notion of dimension. Accordingly, this point of view is not practicable for the purposes of mathematical analysis. For these purposes the author freely admits that the usual Cartesian coördinates are the most serviceable. Only one must remember that the choice of the directions and their relation to one another are arbitrary and conventional, in no way corresponding to the real nature of space.

As to the number of dimensions, if we use the rectangular Cartesian coördinates, with full conciousness of the conventional character of our procedure, we may be sure that a 'fourth dimension' is an absurdity. If we surrender the rectangular relation and use as axes of coördinates any arbitrarily chosen directions from a given point, we may then raise the number of 'dimensions' to any degree that we choose, while if we adopt the approved conception of dimension given above, we may speak with perfect propriety of four dimensions, but no more. In any case the point to be urged is just this, that whatever notion of dimension we choose to adopt for purposes of analysis or what not, we must make clear to ourselves the relation between this notion and the real nature of space, and be convinced that we have therein no possible warrant for the assumption of a non-Euclidean or an extra-empirical space.

Many interesting and cleverly handled side-issues occur here and there in the course of the essay. These may be passed by here with a mention of one only. The Wundtian doctrine of local signs is frankly confessed to be faulty in so far as it attempts to reduce the spatial to the purely intensive or qualitative. It leaves nothing to be desired, however, in so far as it explains the spatial *arrangement* of our sense impressions.

Ueber die Auffasung einfachster Raumformen. RICHARD SEYFERT. Philosophische Studien, Bd. XVIII., S. 189–214.

The author's former article under this same title was noticed in this REVIEW some years ago (Vol. VI., p. 447). The experiments there described dealt with the *subjective* factors in the apprehension of simple triangular forms. And the conclusion there reached was that the sensations coming from the movements of the eyes constitute the decisive factor in the accurate apprehension of these forms. The present article supplements and completes the other by dealing with the *objective* factors in this process of apprehension. The same ten typical triangular forms were again made use of here, only they were presented under the several conditions secured by heavy or light outlines, together with unemphasized or clearly marked vertical points, and by variations in size, distance, color and illumination. The procedure of the subjects was in all cases to contemplate the figure for a given time and then by free-hand drawing to reproduce it in its actual size. The deviations of the drawn from the actual figures were measured by the sum of the deviations of each angle from its copy.

The results of the experiments may be summarized as follows:

Strongly marked outlines and clearly indicated vertical points are conducive to correct apprehension of form.

A colored form is in general more correctly apprehended than one drawn in the ordinary fashion. Still it is not so much the color itself as the brightness contrast between the colored form and its background that constitutes the advantageous factor.

For the best results size and distance must be so related that the entire retinal image falls within the limits of the yellow spot.

Disadvantageous factors are color contrast between form and background, faulty illumination, fatigue, æsthetic dissatisfaction, and the absence of the advantageous factors noted above.

Certain regularly recurring errors were noted. There was a marked tendency to make all reproduced lines shorter than those of the copy, a tendency springing, the author thinks, from an overestimation of the reproduced lines because of the summation of muscular sensations from eye *and* hand and arm. The vertical dimensions of the reproduced forms were regularly made too small — an expression of the general tendency to overestimate vertical extents.

A. H. Pierce.

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NEW BOOKS.

La Théorie de l'Émotion. W. James. Trans. with Introduction by G. Dumas. Paris, Alcan. 1903. Pp. 158. Fr. 2.50.

Contains translations of Professor James' chapter on 'Emotion' from the *Principles of Psychology* — with the addition of certain passages from his original article in *Mind*, IX., 1884 — and also of the article 'The Physical Basis of Emotion' from The Psychological Review, September, 1894.

Life in Mind and Conduct. H. Maudsley. London and New York, Macmillans. 1902. Pp. xv + 444. $3.50.

L'Année Psychologique. A. Binet. 8e Année, 1901. Paris, Schleicher. 1902. Pp. 757. Fr. 15.

Contains the usual quota of leading articles and reviews of literature for the year 1901, and also the bibliography of the *Psychological Index.*

Addresses and Proceedings of the National Educational Association. Minneapolis Meeting. 1902. Pp. viii + 1021. University of Chicago Press.

The usual *mélange* of things important and otherwise. It is a pity that the more authoritative papers can not be selected out for publication apart from the proceedings, lists of members, etc. J. M. B.

Human Nature and the Social Order. C. H. Cooley. New York Scribners. 1902. Pp. viii + 413.

L'Art et la Beauté, Kalliklès. L. Prat. Paris, Alcan. 1903. Pp. 285. Fr. 5.

A theory of art and beauty put in the form of Platonic dialogues.

Stad Antwerpen. Paedologisch Jaarboek, 1902–1903 . (3d and 4th year.) M. C. Schuyten, Editor. Nederlandsche Boekhandel, Antwerp; also Schleicher, Paris. Pp. 485. Fr. 10.

Each paper in this Dutch year-book of the Antwerp Paedological Society is followed by a *résumé* in French. The book contains abstracts of important publications of the year, as well as original papers.

Five Years' Observations and Experiments (1896–1901), on the Bionomics of South African Insects. Guy A. K. Marshall, E. B. Poulton, and others. From Trans. Ent. Soc. Lond., 1902, Part III., Nov. Pp. 287–584.

Contains a most interesting mass of material, with studies by Marshall and Poulton 'chiefly directed to the Investigation of *Mimicry* and *Warning Colours*,' and an 'Appendix Containing Descriptions of New Species.' In this connection we may also call attention to Professor Poulton's lecture on 'Mimicry and Natural Selection' reprinted from the *Proc. V. Intern. Zoologencongr. Berlin*, 1901 (1902).

Agnosticism. R. Flint. New York, Scribners. 1903. Pp. xviii + 664. $2.

History of the Problems of Philosophy. P. JANET and G. SEÁILLES. Trans. by A. MONAHAN; edited, with an introduction, by H. JONES. London and New York, Macmillans. 1902. Two vols. Vol. I., *Psychology*, pp. xxvii + 389. Vol. II., *Ethics*, *Metaphysics*, *Theodicy*, pp. xiii + 375.

La Logique morbide. I. *L'Analyse mentale.* N. VASCHIDE and CL. VURPAS. Preface by TH. RIBOT. Paris, Rudeval. 1903. Pp. xxviii + 268. Fr. 4.

The first volume of a series to be devoted to what the projectors call *logique morbide* — defined by M. Ribot in his introduction — and sufficiently characterized here by the topics of the volumes proposed, *i. e.*, *analyse mentale*, *syllogisme morbide*, *émotion morbide*, *création intellectuelle morbide.*

J. M. B.

NOTES.

THE Carnegie Institution of Washington has issued a circular announcing the revival of the bibliography of medical literature, called the *Index Medicus.*

WE regret to announce the death of the eminent psychiatrist, Professor v. Krafft-Ebing, of Vienna.

THE publishers, Messrs. Houghton, Mifflin & Co., announce a new edition of the *Cosmic Philosophy* of the late Dr. John Fiske, with an extended 'Introduction' by Professor Josiah Royce, of Harvard University—to appear at once.

PROFESSOR E. F. BUCHNER, formerly of the School of Pedagogy of New York University, has been appointed professor of philosophy and pedagogy in the University of Alabama.

WE note that among the desirable things mentioned by the various authorities in Oxford, when asked for suggestions, a department of experimental psychology was brought forward by 'several.' We hope this desideratum may soon be realized. The suggestion recalls the fact that psychology has recently been recognized by Oxford in certain signal ways. The first American appointee to the Romanes lectureship was a psychologist — not only the first American but also the first psychologist to be appointed. The first honorary degree in science ever given by Oxford University was bestowed upon an American psychologist. In Oxford itself psychology is coming forward in the work of Dr. Stout and in the broad sympathy and direct recognition on the part of the zoölogists — notably Professor Poulton. Indeed, Oxford graduates now resident elsewhere are representing the subject with distinction — witness Alexander, Hobhouse, and Titchener. England seldom remains long in a secondary place in any experimental science; and it is very singular that experimental psychology has had to wait so long for adequate support there. It is undoubtedly due to the difficulty of getting new subjects into the universities. For some time at least none of the Rhoades scholars can work in experimental psychology. Perhaps this is one of the subjects in which the demand created by the Rhoades scholarships may stimulate to the supply of facilities for research in the university.

Psychologists should note the establishment of Research Assistantships by the Carnegie Institution of Washington. Details and conditions of application may be had by request (marked 'Research Assistantships') to the Carnegie Institution, Washington, D. C.

We regret to announce the death, on January 24, of Professor J. O. Quantz, of the chair of Psychology in the State Normal School, Oshkosh, Wisconsin.

PROFESSOR J. MARK BALDWIN has engaged to give a course of lectures on 'Organic and Mental Development and Evolntion' in the Chicago University Summer School, June 15 to July 22.

It may be remarked that the present issue of the REVIEW is increased by twelve pages, beyond the usual maximun, in order to accommodate the Proceedings of the American Psychological Association. We note that a New York Sub-section of the Association has been formed. It held its first meeting in New York City on February 23.

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THE PSYCHOLOGICAL REVIEW.

MIND AND BODY, FROM THE GENETIC POINT OF VIEW.[1]

BY PROFESSOR J. MARK BALDWIN,

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In the discussion of the relation between body and mind, we often find the position advanced that the experience of each is, in its original form, equally 'subjective.' Idealistic analysis of external objects reduces them to sensational and other presentational elements. It thus becomes possible to take what Professor Sanford, in his president's address printed in the last issue of this REVIEW,[2] calls the 'purely psychological'[3] point of view, in the consideration of this time-honored problem.

The direct result of such a point of view is that we have to deal with what may be called psychic or subjective series alone, and the remote consequences, for the theory of knowledge, are the familiar ones common to the different forms of subjective idealism.

Apart altogether from the results which follow for the discussion of the relation of body and mind as indicated in detail below, I wish to discuss briefly the legitimacy of this purely subjective point of view; not so much with reference to the traditional question of the validity or sort of reality attaching to the external world, as with reference to the supposed primacy of the subjective. Does it follow that because the

[1] Lecture read before the Yale Philosophical Club on February 10, 1903, and also before the Princeton Psychological and Philosophical Seminaries.

[2] THE PSYCHOLOGICAL REVIEW, March, 1903.

[3] A better designation would be 'purely psychic'; that is, the point of view of the subject of the experience.

world as well as the self, the body as well as the mind, is a construction from data of presentation — that therefore, the subjective factors are entitled, in some sort, to greater primacy and ultimateness with reference to the universe as a whole than those which we ordinarily denominate objective and external?[1] I propose to look at the question from the point of view of the genesis of the distinction between body and mind; and in the light of our outcome to consider the general question of the relation of the two sorts of reality to each other.

I.

There has been considerable discussion lately as to the rise of this main distinction. As to the outcome, the chief results are, I think, very generally accepted — apart from differences of detail. In the first place, we may say that consciousness, in its earliest experiences, does not have the distinction between the 'inner' and the 'outer,' the self and the world. Its experience is what I shall call in a figure 'protoplasmic'; it is in Ward's phrase 'a continuum,' or in James' phrase 'a buzzing confusion' — the two writers agreeing, in their differences, that classification and differentiation of the sort requisite for the separation of the worlds of the subjective and the external, are absent.[2] Experience of this neutral or protoplasmic sort, when considered as preliminary to the later distinction, I have called, using a term which goes well with 'subjective' and 'objective,' by the term 'projective' — the positive side of the conception being this, that even then a mental content is presented or 'projected' in the field of the respective sense through which it arises.

The beginning of the distinction between mind and body seems to be found — assuming that experience is at first undifferentiated or protoplasmic — in the child's apprehension of its presentations as being respectively of or from *persons*, on the

[1] I use 'external' as opposed to subjective, since the term 'objective,' applying as well to mental objects which are not external, would be ambiguous.

[2] See the joint art. 'Experience,' in the writer's *Dict. of Philos.*, in which James goes further than either Stout or the present writer in maintaining the virgin neutrality of experience of the earliest type, with reference to 'inner' and 'outer.'

one hand, and again *things* on the other hand. If we use the symbol '*Pr.*' for projects, or presentations *not yet distinguished* as external things or as self, we may put it down that the first distinction is thereafter between '*P*,' persons, and '*T*,' presentations which are not persons — understanding, however, that in the child's apprehension this distinction is merely a distinction, not a characterization, and that it comes to him largely through the accommodations of his active life to the varying situations in which he finds himself.

He next, through a process various aspects of which are described by various authors under the terms 'introjection,' 'imitative interpretation,' etc., assimilates his *P's* — his personal projects — to certain other intimate, pungent, highly toned experiences. These are set over against other presentations separated off as '*T*'s,' or thing-projects. The *P*-experiences have a series of aspects which I can not now describe in detail,[1] but which comprise effort, and pleasure and pain, and belong in a mysterious way to the individual himself. Things, he learns, may be left behind, may be shared, may be manipulated: and again things resist, limit activity, stand stubborn — characters, all of them, which the *P*'s do not so evidently show. Here, in this growing distinction, is the germ of the difference between that which may be *left behind and found again* — the external — on the one hand; and the ever present, the always owned — the subjective — on the other hand.

Now what it is essential to note at this stage of the progress of consciousness is this: the *P*-experiences, which by this act of interpretation have become 'subjective,' are at once and by the same act of apprehension, *common to the individual's psychic self* — which I shall call '*S*' — *and to the other self or projective person already presented in contrast to things.* The self-as-subject arises by interpretation of the objective *P*-presentation. This carries with it a further interesting implication.

The child has heretofore apprehended his own body as a thing — a projective mass of sense experiences, like other things. He now distinguishes the *P*-characters from the *T*-char-

[1] It may be said that it is not necessary for our present purpose to describe the actual materials which go into the notion of personality; of that there may be great diversity of opinion.

acters in certain things. Further, he claims the *P*-characters as being his also — as being identical with certain of his own states realized as subjective. So he treats his own body precisely as he treats other objects, and finds his subjective self-part, *S*, distinguished from his mere thing-part, *T*. The thing-part in all persons, in others and also in himself, becomes differentiated from the *S*-part. This thing-part he comes later on to call 'Body,' while the self-part he calls 'Mind.' The *S* is one of a number all of which are Mind, *M*; and the things which go with selves, being equally numerous, are Body, *B*. Here then we have discovered the great line of cleavage or segregation, as between mind and body, in its essential genesis.

The implications at this point are more or less evident. Just as at the earlier stage, in reaching the distinction between the subjective and the external, we found that the genesis of the self essentially implicates other selves, and cannot be apprehended without them, so now, with respect to the relation between mind and body. What I have called the thing-part of the person is realized, as in relation to the self-part, first in the case of other persons; but the relation is necessarily carried over to the private self, who is also considered as mind associated with body. Expressing these implications also in symbols, we may say that M (my mind) is thought only with M' (other minds), and B (other bodies) only with B' (my body), and that when one entire personality is set over against another, M is associated with the B', and M' with B.

If this general line of genetic distinctions [1] be accepted, and we then ask what consequences would seem to follow for the discussion of the relation between mind and body, we may, I think, draw conclusions of some importance. Before proceeding, however, with the discussion it may be well to make precise the symbolism we have been hitherto employing, and to set down the stages of mental growth recognized above, in order to show

[1] In support of such views the reader may consult Royce, 'Studies in Good and Evil' (the chapters on the Self); Stout, 'Manual of Psychology,' Bk. IV., Chap. VII.; Mezes, 'Ethics, Descriptive and Explanatory,' Chap. VII.; Ormond, 'Foundations of Knowledge,' Pt. II., Chaps. VII., X.; and the writer's 'Social and Ethical Interpretations,' Chap. I., and 'Mental Development in the Child and the Race,' Chap. XI.

the sort of opposition or contrast which obtains at each stage between the two terms to the controversy, mind and body. This is done in the symbolic expressions immediately following.

The symbol ( ) is herein suggested to denote the 'becoming' or genesis of the term which follows it, from that which precedes it: it may be called *the symbol of genesis*. Its interpretation in particular cases would be alternatively in one in three modes: genesis which is indefinite as to exhaustiveness in the analysis of A into B, for which the expression is of two equal semicircles, thus $A\,(\;)\,B$; second, genesis expressed as A exhausted in B, but not necessarily B in A, in which case the semicircle toward A is greater than that toward B, thus $A\,(\;)\,B$, third, genesis expressed as A not exhausted in B, in which case the semicircle toward A is less than that toward B, thus $A\,(\;)\,B$. We here have occasion to use only the first of these, the symbol of 'indefinite genesis, or genetic progression.'[1]

Summing up the stages of growth, now pointed out, in symbolic statements, we have certain expressions which I shall call 'Progressions,' in distinction from 'Equations.' The sign for genesis may be rendered by the word 'becomes.'

Progression 1. Pr (projective experience) ( ) $P + T$.

Progression 2. $P\,(\;)\,S + S'$.

Progression 3. $\frac{S'}{T}\,(\;)\,\frac{M}{B'} + \frac{M'}{B}$.

We may characterize these 'Progressions,' respectively, each in view of its place in the entire genetic process, as (1) the 'projective Progression,' which reads, *Projects become Personal-Pr and Thing-Pr;* (2) the 'subjective Progression,' which reads, *Personal-Pr become Subject-self and Object-self;* and (3), 'the ejective Progression,' which reads, *Object-self becomes Mind and Body* — the last alone representing complete dualism of Body and Mind.

II.

So far we have come to a dualistic stage in the development of consciousness, and we have intimated the conclusion that from this stage of interpretation, the two terms, M and B, must

[1] I may emphasize the distinction intended between the values of these signs and those of signs of quantity.

be considered as strictly correlative. The determination, in the course of reflection, of further predicates for either the one or the other, if made at all, should be of such a sort that some further relation between them takes the place of this. This first and most general result requires that we must take either *the original or the derived form of experience* — the earlier stage or the later stage of conscious growth — as the basis of examination and criticism. If we take the early or protoplasmic stage, we lack the distinction altogether; and we have then to say that the question is an impossible one, since the terms of the antithesis between mind and body have purely abstract or logical value. If, on the other hand, we take the later stage, when the distinction has matured, then the two terms, mind and body, *have strictly correlative meanings*, and in recognizing the sort of predicates which consciousness and reflection attach to the one, we are bound, by right of genesis and meaning, to do the same for the other also.

Suppose that, in our further quest, the point of view described above as the 'purely subjective,' be taken; we have then to ask the question as to what form the concepts mind and body take on in the thought of the individual thinker whose point of view it is. We find that to think of body as presentation is, in accordance with Progression (3), to think other minds with it as presentation, and this involves, by Progression (2), thinking of one's own mind as presentation. In other words, it is impossible on this hypothesis to take any other than a purely phenomenalistic or presentational view of both sorts of object, body and mind. The procedure which involves treating other minds as objective phenomena, and at the same time maintaining the psychic point of view with reference to one's own mind, is illegitimate.

Furthermore, to think of another mind as presentation, and to maintain also the psychic point of view, is to think of the other mind as thinking of the thinker as presentation. So there results, at the best, the notion of a series of isolated psychic centers. On this basis no general thought or theory of mind and body could be derived; for to do so would be to desert the strictly private standpoint, and bring back that of phenomenal correlation of body and mind.

If then we intentionally adopt the point of view of correlation we find two alternatives: either we have to consider the relation of the presenting mind to its object, the presented body (including by implication also the body which is associated with the presenting mind) — the relation of M to B (or to B') — or, on the other hand, we have to consider the relation of presented mind to presented body — that of M' to B.

The first of these problems would seem to be the traditional question of the relation of mind to its object: the question of theory of knowledge or epistemology. In so far as it is merely the construction of a mental object which is in question — the sort of function in exercise in the perception of the external object — we have no approach one way or the other to the question of mind and body; for whatever the result, not only does body become experience, but mind, as presenting, is also in some sort experience. And as soon as we reach the apprehension of the presenting self as some form of experience we come to the other alternative — as is symbolized in Progression (3) — that of the relation of mind as presented to body as presented. It may be said that there is a mode of self-experience of which the self is not presented as content. That view represents a later stage of reflection, I think, and it is taken up below (in the next paragraph but one).

The other form of this alternative, however, that of the relation of a mind to its own body, is an interesting case here, for we assume the brain in the body, and by our Progression (3), this B' is an implication of B (the body or brain of another which is actually presented). This is often thought to import into the question some peculiar features, for it is supposed that our knowledge of our own brain is in some way less direct, and hence, in some way, less valid and reliable than that of other brains which we might actually see if they were put before us. I am quite unable, however, to see any force in this position; for the brain in connection with another person's mind is not actually seen, but is only inferred as part of the whole of his organism of which we see the other parts; and exactly the same is true of the thinker's own brain. He does see and touch his own organism in certain features the same as those which he experiences of other persons' bodies, and besides he has from his own body those direct sense-impressions upon the basis of which his original distinction between the subjective and the external is actually based. We may accordingly treat the two cases as really one, and accept the result stated immediately above, to the effect that the case is reduced to that of *the relation of presented body to presented mind.* We have still, indeed, two cases, one of which is that in which the presented mind is actually a different mind and the other that in which the presented mind is the same as the presenting mind, and it may be well before going further to inquire into the meaning of this distinction. It is important to do this because, in the result, we shall find that confusion on this point lends some unclearness to the analysis usually made from the subjective standpoint.

When we refer, as is done above, to the relation of the presenting to presented mind, for the purpose of the discussion of parallelism or of any other general theory, what we mean by presenting-mind-as-itself-presented is the individual mind made the object of its own presentation. We do not mean the mind as simply conscious operation upon the body presented. This latter is very often meant; as, for example, when we are told to take the point of view of the mind's spontaneous perception of body, and ask in what sense the presenting mind is then and there conscious of itself. We must take the meaning to be the individual mind as made the object of its own presentation, that is as an object additional and alternative to body likewise presented as object; because the theory requires that the view reached should cover the case of the relation of another person's mind to his body, and that would mean his mind presented as object to an onlooker in the same sense that his body is presented as object. Psychologically, indeed, the difference is that between unreflective and reflective consciousness. When it is argued that there is a form of apprehension of self as function, not as presentation, and that this apprehension is felt or in some way realized in the act of presenting body as object, we are, as I take it, abstracting from the act of reflection and in so far reducing consciousness to the stage — in the matter of the distinction of body from mind — of our Progression (1), at which the distinction between subjective and external is not yet realized, or is so obscure that the self is not presented as distinct from other subjective selves. This latter stage, that of Progression (2), allows the distinction of self and other-self only because it permits the full and explicit presentation of the experiences common to selves as such.

If this be true, namely, that to take the subjective point of view, in the sense of supposing a conscious self-function over against body-content — that this really requires or involves the spontaneous point of view, then we must allow ourselves only the distinction between mind and body which consciousness has achieved at that stage of its development. Now we find that consciousness has then only projective distinctions, not subject-object distinctions; and if that be the stage for valid interpretation, then the question of the sort of reality attaching to either of them over against the other cannot be put at all.

I wish to insist on this result. It is fundamentally that of the general criticism made on the subjective standpoint at the end of the first or genetic section of this paper above, except that we now reach it by detailed analysis. It brings the direct charge against the subjectivists of confusing the spontaneous with the reflective points of view. Only a genetic analysis of these stages — as shown in the Progressions (1) and (2) — can expose this confusion. If we treat the body-member of the antithesis as being such a product of reflective thought, that its reality, its properties, etc., can be shown to be due to the higher activities of distinction and synthesis which characterize the reflective consciousness, then we must treat the mind, in which this thought of body occurs, as exercising these reflective processes in apprehending body. But if we do this, we are dealing not with mind as spontaneous function but as having itself as object or content of reflection set over against such a body; in other words we must take Progression (3) as the basis of interpretation. If, on the other hand, we treat mind as being the individual's spontaneous function of construction of an object, then the object, body, must be the sort of object, the 'project,' which such a consciousness is able to construct. The fallacy of the subjectivists is in saying that in contrasting body and mind, we may mean a thought of body which is a constructed object subject to analysis, and a thought of mind which is not an object at all. Of course, if such a procedure be allowed, we may at once say: body is a complex of mental states, mind is the agent which has the states—and the conclusions of idealism follow.

But suppose we turn the tables, and commit ourselves for a moment to the diametrically opposite procedure. Suppose we say: mind is the inferred thing which, upon reflection and as the result of objective analysis, we suppose to be the bearer of psychic states; body, on the contrary, is the immediately given, the original mode of sense-experiences of resistance, etc.: hence the primary of body. Mind turns out to be a series of epiphenomena. This is the fallacy of the materialists. Its roots are found just in taking the spontaneous standpoint for one term of the antithesis, body, and the reflective standpoint for the other, mind.

I contend that both procedures — that of the subjectivists no less than that of the externalists — that both are illegitimate. If we interpret mind and body by Progression (1), the antithesis between them does not exist, and we have no data for such a question; if we interpret both by Progression (3), in which the dualism has emerged, we have the question of the relation between presented mind, considered as the object of reflection, and presented body, likewise considered as an object of reflection. This alone is the legitimate form of the scientific and philosophical problem.

III.

This question is that of the relation of the two general spheres of objective construction to each other. This question, as I conceive, demands further genetic analysis, for it involves the determination of the sort of objective reference attaching to each of the two modes of construction, together with their content; also the categories in which each content is organized. In short, if we are to ask as to the reality of mind and body, and as to the sort of real relation which subsists between them, our only possible solution resides in the determination of the nature of the concepts of reality or objective validity attaching in our reflection to each.

This, of course, is a 'large order'; and we cannot hope to develop a theory of reality here. Yet we may assume acquaintance with the alternative historical points of view, and ask what value the genetic approach to the concept of such a real relation gives to either term over against the other. Particularly may we expect light, from the genetic progress of consciousness, upon that possible interrelation of the two spheres of reality which is involved in such a theory as that of 'interaction.' In anticipation, I may say at this point that the considerations which follow are directly opposed to the theory of interaction, and in favor of a form of psychophysical parallelism, finding its philosophical ground in a reflective monism.

When we come to ask as to the genetic procedure in the organization by which the two spheres of reality, respectively designated the mental and the physical, are constituted, we find that the processes called above 'Progressions' are to be recognized. The fundamental ground of the distinction between persons and things resides in the need of classification and reaction upon contents which have such different characters that they cannot be treated without such a fundamental differentiation. I think it is a fair statement of the generally accepted views of the two sorts of science arising from this differentiation to say that physical science is a form of knowledge in which the materials are those which can be organized under the category of 'agenetic,' or what are usually called 'mechanical,' sequence. These materials are distinguished very early in the mental history of the child from the genetic or amechanical sequences, by their character as showing a certain *regularity*, *invariability*, and *reversibility*, which beget in him an attitude of discounting or predicting their occurrence and recurrence. This is the sphere of repetition, and in the reactive life, the sphere of habit. This mass of material is the domain of those retrospective, exhaustively formulated sequences which admit of measurement, and which, in their further pursuit, yield results in quantitative physical science. I do not mean to say that all physical science, all knowledge of the external world, is actually quantitative; but what I mean to say is that this aspect of the world of external objects constitutes it a mode or sphere of reality different from the mental. It is the ideal of physical science to be agenetic and mechanical;[1] this is the category which it exploits. In so far as this world has aspects which cannot be so treated, we find an actual tendency to divorce such aspects from the external and to read them into the subjective group — as, for example, sense-pleasures and pains, so-called subjective sensations generally, and in a less degree, odors and tastes — certain of which, under the various designations 'secondary properties,' etc., are thought to be more loosely attached to external objects and more nearly subjective than other properties. Yet they are apprehended by direct stimulation from external objects, in the same way that other physical properties are.[2]

Such an agenetic form of sequence is what is meant by 'cause and effect' in the physical sciences. As soon as we attempt to make that notion mean more than temporal sequence, by introducing into it the idea of efficiency, we reach the concept of energy, which is quantitatively determined, and liable to the agenetic formula of 'conservation of energy.' This has lead to a monopoly of the concept of cause by the physical sciences and its formulation under such dicta as 'there can be nothing in the effect that is not already in the cause' and the construction of the phrase 'post hoc ergo propter hoc' in the sense 'non propter hoc non effectus.' In other terms, we find the physical formula for cause and effect to be strictly an *Equation*, not a *Progression*, in the genetic meaning of Progression as indefinite inequivalence. The assertion of such an inequality is considered *ipso facto* the denial of causation.

Furthermore, this equality view of causation is dominant even in the attempts at genetic deduction of the category of cause. The experience of effort is looked upon as intensively graded in proportion to the difficulty of the movement accomplished, or to the resistance overcome in the external world.

[1] This means, finally, reducible to motion; and since motion is always reversible (whether the actual reversal is within our power or not) we have *reversibility* as the criterion of a strictly agenetic or mechanical series.

[2] It is interesting to note the development of this sort of construction in the history of the scientific impulse, *i. e.*, a lingering personification of the forces of nature wherever they are mystifying or not subject to regular behavior. The history of science shows the gradual reduction of the personal to the mechanical in external phenomena; but to-day spiritism remains the religion of mystified physicists!

And the next and final step in the 'physicallizing,' so to speak, of the conception, and with it of the experience on which it is based, is that the mind is supposed to be in causal interaction with the body in a way which is expressible in an equation: so much mental expenditure or experienced force equal to so much physical effect or work — muscular work, and, remotely, purely mechanical work.

It suffices, at this point, to make this fact plain — the appropriation of the notion of cause by physical science, and the very general tendency of philosophers to accept it. I shall come again to the point further down in our discussion; here we may for the present rest it, and return to the consideration of the corresponding genetic development of the notion of mind and the categories under which in turn it is constituted.

The beginning of the distinction between body and mind — as shown in our Progression (1) — is the contrast, in presented phenomena, between the regular and predictable, and the irregular and, in a sense, capricious. It is facts of the latter sort that the child finds imitatively available, and in general assimilable to those aspects of his experience embodying his own agency and his private subjective interests. These go on to be organized in the form of self, over against the antithetic organization of the phenomena of body, as sketched in what immediately precedes. For our present purpose — which is not an exhaustive statement, but merely the development of a contrast — the essential character of this type of his experience is its genetic organization. It is in the experiences of impulse, instinct, strain, effort, etc., that his personal self comes up sharply against the regularities and law-abiding changes of the physical world; and his mental growth is a series of achievements by which he reduces the conflicts, and effects a *status quo* for the practice of the essential things of his life. He grows by accommodations, personal concessions, adjustments, which become systematized in a progress of mind more and more complex, and to the onlooker, essentially novel. His instincts — the most mechanical part of him — are broken up; his impulses are inhibited; out of all the conflicts of processes that wonderful endowment of personality, voluntary self-control and determination, emerges and becomes an instrument of foresight and prudent conduct. All this is progress — that is the point; not mere repetition. It is growth, not mere change. It is genesis, not a reversible atomic series. In type, therefore — and this is what the term means — it is genetic, not mechanical, not agenetic.

Mind, therefore, when he generalizes it, is something which is characterized through and through by growth, genetic organization. As he looks upon another person, saying of him, 'body and mind,' by mind besides body, he means a subjective part which is, like his own subjective part, a thing of growth in accommodation and self-determination. And when he says of a person that he has both body and mind, he means what he means also of himself — a being which somehow has two sides, each of them showing a characteristic type of serial change, illustrated in many particular cases. And these two sorts of change, whenever realized in a particular case, *are uniformly and continuously together*.

Now it is in this realm — that of subjective change and determination — that the mental and moral sciences find their data. The stage of reflection shown in the Progression (3) is that of dualism. The *M*-terms are instances of one mode of sequence in phenomena, and the *B*-terms are instances of another. But it is a singular and compelling fact that the *M*-terms are, in their very nature, not capable of formulation in what the writer has elsewhere called 'cross-sections' — that is by the mere analysis of a complex situation into its elements, considered as giving an exhaustive statement in terms of a foregoing or after-following situation. Such cross-sections are possible only of physical sequences. But if they be impossible mental processes, then only 'longitudinal sections' are possible: the actual statement of Progressions, as that term is defined and illustrated above.

It results that the mental sciences must always be unfinished. Their data cannot be formulated in universal statements. Only actual growth can reveal, in each case, the next succeeding mode of organization. Such concepts as we do work out, therefore, as applicable to mental change, are essentially of this 'longitudinal' character — that is they are Progressions. Cause and effect in psychology, for example, can only mean a sequence of indefinite inequivalence between a preceding and a following term — that is, *so far as it is genetic in type*. When we say — or when the self in the case says — that the mind causes the movement of the arm, what is meant is that the experience of the moved arm is the genetic issue of the mental processes in certain directed volitions. We do not mean to say, or we should not, that cause in the physical sense, defined above as motion followed by motion in a reversible series, is all that takes place. We mean the full statement of fact of a connection between changes in the two series, in the form of a regular sequence. If we go on to interpret the sequence by either of the two conceptions of cause, we naturally fall into one or other of the two fallacious interpretations described above.

It is interesting to note that the naïve form of interpretation of the relation subsisting between mind and body is that of genetic or longitudinal sequence, not that of quantitative transverse equivalence, although the reverse is often assumed to be the case. This fact exhibits the obverse side of the movement, spoken of above, by which the notion of cause in the physical sense gradually extends itself to include mental change. We find, in the progress of racial thought, that earliest of all, the changes of nature generally are attributed to personal agencies; that this is gradually limited as natural knowledge advances; that the segregation of phenomena, under the concepts of law and necessity, narrows the sphere attributed to personal agency; until — and this is the last stand of the naïve point of view — it is only in the one case of the relation of *one mind to one body, and that its own*, that such a point of view is still held. In the theory of interaction, the attempt is made to justify this one remaining case.

But those who make the attempt do not see that this is to preserve, in the one case, the point of view which in the progress of racial reflection has been given up in other cases. Such a position is in opposition to the essential progress of physical science. In all other cases knowledge of the physical demands, for the constitution of a lawful universe, that sporadic interferences from the sphere of mind shall be abolished. Such interferences remain as the extreme resort of religious mysticism, by which, indeed, they are called 'miracles.' In such cases, they are, even to religious thought, the exception which proves the rule. They are miracles — phenomena fit to excite wonder — just because they constitute the departure from the operation of the processes natural to the group of phenomena in which they show themselves. But in the case of the relation of body and mind there still remains, in the conception of causal interaction, the confusion of the unreflective and reflective points of view due to the immaturity of the distinction between the two spheres of causation necessary to the consistent development of either sort of science.

The evolution of contemporary thought is making this evident. This appears in the greater difficulty one has in accepting the theory of interaction when the purely objective point of view is taken — that is, when one considers the case, not of the subject's own body, but of that of another. When one takes such an objective standpoint, it is found necessary to preserve at once the integrity of the physical series of brain changes, fulfilling the principle of conservation of energy, and at the same time to allow that it is in some way interfered with by mental agency. The result takes on — apart from the resort of subjectivism — one of two forms: either the express denial that the mind increases or diminishes the amount of physical energy already in play; or, on the other hand, the resort to devices by which the modifications in the quantitative determinations of physical science, due to mental activity, may be compensated for. On the one hand, we have the claim that the mind can direct, or switch off, or hold in the physical energies; or that it can decide as between alternative expressions of physical energy, without altering its quantity — suppositions long since and many times shown to be absurd. As type of the latter sort of devices we have the suggestion attributed to Lotze that the plus and minus increments to physical energy, due to mental interference, will in the long run counterbalance each other, and the actual equilibrium of the physical forces remain after all undisturbed. All such superficial hypotheses seem to me only to keep up the confusion which it is my present purpose to expose.

I may now sum up the results of the foregoing discussion. *En résumé*, therefore, we find that in its actual genesis, the distinction between phenomena of mind and body, considered as distinct types of presented phenomenal change, requires the use of two distinct categories of construction, the genetic and the agenetic. Physical science it is which interprets the agenetic. Its explaining concept of cause is illustrated only and always in transformations of energy. On the other hand, the mental and moral sciences interpret the genetic, in the special realm denominated subjective.[1] They recognize, just by their distinction from the physical, the type of change which issues in progressive organization, and permits only of longitudinal or 'progressive' statement. They contrast the *Progression* with the *Equation*. The attempt is often made to interpret by one of these categories, that of physical energy, the relation between mind and body; this leads to that form of the theory of 'interaction' which accepts the energy view of causation. Mind becomes a form of energy. This, properly speaking, is a materialistic conception. On the other hand, the attempt is made to interpret the relation between mind and body as a case of Progression or genetic change; we then perforce deny that the antecedent brain state in a psychophysical sequence fully determines the subsequent brain state, and thus allow that this case is the one miracle which survives in physical nature. This is the other form of the interaction theory. Each of these attempts makes appeal to a stage of relatively naïve or unreflective consciousness. Both fall into the same confusion; but they illustrate the confusion differently. One maintains the point of view of reflection only on the side of the physical; the other only on the side of the subjective.

We have maintained, on the contrary, that the theory of the relation of mind and body must maintain consistently the opposition under which alone these two concepts develop and have validity; it must be free, in explaining the relation between the two, from the application to it of either category which belongs exclusively to one of them.

[1] The sciences of life lie between. Life processes are really genetic, as is maintained below.

IV.

With so much game in our drag-net, we now have to ask the final question as to the proper apparatus for fishing in the deep seas of the mind's more refined reflection. The problem is this: Can we hold each set of phenomena to its own legitimate construction and at the same time reach a comprehensive conception of the concomitance of mind and body under which the scientific formulas appropriate to each may be given full value?

So far as the formulation reached at the stage of scientific dualism represented by Progression (3) is concerned — the point of view common to psychology and physics alike — it is expressed in psychophysical parallelism; for it is the essence of that theory that it refuses to postulate any positive predicates of the psychophysical relation, and rests content with the recognition of sufficient uniformity and generality to justify investigation by recognized scientific methods in both departments of science.

More positively, indeed, psychophysical parallelism justifies itself genetically at both the stages of mental development shown respectively in Progression (2) and Progression (3). We may take the point of view of Progression (2), and deliberately refuse to allow the validity of the dualism reached by reflection in Progression (3). In that case, the phenomena of personality are simply joint phenomena; neither mind nor body is treated under peculiar categories. The interpretation of a sequence is from one psychophysical term — what we may call a BM term — to another; and we have no possible question of the separate action of brain-states or mind-states as determining subsequent states of brain or mind or both. We then have the simple form of Progression, $BM\,()\,B'M'$, in which one psychophysical term, *taken as a whole*, is considered as the antecedent of another, also *taken as a whole*.

This is to my mind, both in biology and in psychology, the only justifiable scientific method. Its merit is that it compels the equal recognition of the two aspects of phenomena whenever they are both present, and really banishes the futile question of reconciling the terms of a dualism which, so far as concrete phenomena are concerned, is abstract and artificial. I have argued in detail for this conception in my recent work, 'Development and Evolution,' claiming that general biology suffers equally with genetic psychology from the divorce of the facts in these two allied spheres, which the traditional dualism compels. Evolution is psychophysical, not organic and *besides* — or possibly, *not at all* — mental. The psychophysical standpoint is the only valid scientific standpoint for a theory of organic descent, no less than for a theory of individual development; and the two genetic series must be interrelated by some form of 'intergenetic concurrence.'[1] The question, of course, remains over as to the scientific formulations possible to such Progressions; that is as to a method of investigation of successive 'genetic modes' of organization.

If, however, we commit ourselves to a point of view which, as in Progression (3), results in an explicit dualism, then our care must be to reach interpretations which do not invalidate, though they may transcend, this dualism. I have shown above that the current forms of the interaction theory involve a confusion of categories, due to the failure to maintain a consistent level of mental development. The only outlet is to push reflection further, and find a category of experience within which the two forms of sequence may proceed, indeed, as demanded by reflective dualism, while still, at the same time, held in a single thought without conflict. Our question then becomes: How can we satisfy the mental demand for a type of change which shall, at one and the same time, both exhibit the form of 'progression' by successive genetic organizations and also be liable to interpretation in terms of the equations of agenetic science? This to my mind — as I have said in formulating it differently in a recent discussion — is *the* question of contemporary philosophy. I can of course, do no more here than indicate certain seemingly valid approaches to its solution.

In the first place, empirical science must do its utmost in actually finding and examining such complex sequences. But its first qualifications are to be its competency and its fairness in seeing

[1] An expression used in the work cited for the relation existing between racial evolution and individual development, of which the law of 'recapitulation' is the broadest biological formulation.

them when they are found. As a matter of fact they do not lie far out of hand. It was an insight of Aristotle that the phenomena of life contain the great things of philosophy. Life shows both the *dunamis* and the *energeia*, both the dynamic and the static, both the teleological and the mechanical, both the *Progression* and the *Equation*. Biological literature is now-a-days full of the recognition of the concurrent presence in vital phenomena of the two modes of organization; and biologists are wondering whether any theory of the principle of life is possible which is not to commit suicide by cutting its throat on the ragged edge of facts of one or other of these two sorts. Yet we find indications in recent work of ways in which biologists of the two schools, vitalistic and mechanical, are finding points of common ground. The development of the statistical method is showing ways whereby the variations and progressions demanded by vital teleology and genetic process may be allowed, at the same time that exact formulas of distribution are applied to the same cases taken in the mass. In this way, for example, that bugbear to many advocates of purpose in the world, natural selection, becomes consistent with a teleological philosophy. Economists and criminologists are reaching exact statistical formulas for events involving individual desire and choice. It is beginning to be seen, even by the most positivistic science, that genetic processes may be developed in series which also allow statement in cross-sections and equations at each successive stage. All this means that the world is after all one, and that the categories of mental construction, derived in a process of evolution by actual treatment of the world, *cannot finally reflect processes in essential contradiction with each other.*

The needs of philosophical thought, however, are more urgent; and I have no desire to deny the impulses of consistent reflection which are impatient during the slow processes of evolution. Philosophy asks: How can we think reality in one thought? In the terms of our present discussion: How can body and mind, being what we have come to think them to be, live hospitably housed together in one phenomenal group of facts? In answer to this we have all the modes of reasoning of modern philosophy, from the consistent negative processes of abstraction of Hegel, to the logical 'rectification of concepts' of Herbart and Bradley. In opposition to both these tendencies, the present speaker holds that the category of final interpretation must be *a full one*, *not an empty one*, if it is to have concrete significance, and if it is to have the respect and win the adherence of that class of men who make actual contributions to knowledge — the men of science. It is to be sought in the interpretation of *the actual coefficients of the fullest reality of which we can have experience.* The realities of organic life are 'fuller' than those of inorganic nature, because they require just the differential genetic treatment which characterizes the distinction between the Equation and the Progression; because life exhibits ends as well as transformations. The mental life is a form of reality which in turn is fuller than the organic; because it yields the subjective, and requires again another genetic differentiation of experiences into the subjective and the external. If we go further, we find that the highest organization of the mental life is again the fullest, the richest in what we mean by reality in its complex modes. We go from spontaneity to effort, from presentation to reflection, from fact to ideal, from automatism to self-determination. The fullest, not the emptiest, the concrete experience, not the logical universal, is the point of view of most adequate interpretation, because it is just by the processes of expansion and growth that nature makes to us her revelation. We do not realize nature by abstracting from her content. When we speak of a final or absolute experience what we mean, if we mean anything worth while, is an *all-comprehensive and completely-full experience.*

Now — to state a point of view, not to expand or justify it — there is a type of mental organization which is in certain ways 'fuller' than any other, which requires and feeds upon — or to speak philosophically, 'transcends' — the opposition between fact, with its formulations in the Equations of positive science, on the one hand, and purposes, ends, values, and Progressions, on the other hand; it is what is commonly known as *Æsthetic* experience. In the essential union of the two points of view respectively of the 'producer' and the 'spectator' from which a work of art may be approached, we find in our experience the richer whole. In æsthetic contemplation there is the fulfilment at once of the demands for a system of relationships essentially finished and formulated — something completely true — and also the satisfaction of a genetic ideal of perfect outcome — something divinely fair!

I should hesitate to state so bald a preference in philosophy, however, and then leave it entirely without justification, at least so far as defense against possible criticisms brought from the point of view of genesis is concerned; so I shall close with the statement of two considerations which tell with some force in favor of what I have elsewhere christened in advance 'Æsthonomic Idealism.'

In the first place, if we recognize a genetic process in consciousness, we must be consistent in maintaining this or that genetic level without confusion. Failure to be consistent in this respect was our charge against both forms of the interaction theory. It follows that if consciousness goes on to a level in which the mind-body dualism fails, if consciousness itself seems to achieve a union of the terms of the earlier opposition, then it is a fair use of the genetic method to take this higher point of view. I think this is the case; that in æsthetic appreciation we reach a form of immediacy of experience in which the dualism of external and subjective is blurred and tends ideally, at least, to disappear.[1]

In the second place, the problem of validity, which in psychological terms is that of belief, sets the requirement that our final postulate be not, or not merely, a logical abstraction. Logical abstraction is well in its sphere, but through its abuse the whole distinction between mind and body, together with the network of further distinctions which confuse and balk us, have their origin. So far as philosophy — to use the terms employed above — can keep to the actual fulness of experience, as enriched with the gains of its genuine genetic constructions, so far is its understanding of reality also rich and real. Now it is, in my opinion, in the æsthetic category that such genuine constructions reach their fullest development and fruition. In æsthetic

[1] Cf. Ormond, 'Foundations of Knowledge,' Pt. II., Chaps. IX., X., for an interesting exposition of this higher immediacy of æsthetic consciousness.

satisfaction both the scientific impulse and the motor processes of practical life reach their 'end-state' — here they jointly 'issue and complete themselves.'[1]

If I should now add to the count of our catch given above (at the end of the third section of this lecture), I should hold up to view certain small fishes.

First, a method is employed which aims to be strictly genetic, and a symbolism is suggested which may be used in treating all of the problems of philosophy on which genetic analysis is able to throw light. In our fishing figure — this may be likened to a somewhat novel form of net.

Second, I think it has been shown that science demands psychophysical parallelism and is content with it. It may be said by some fishermen here that this is but a sorry flounder! Yes; yet the flounder is undergoing evolution, and he shows several very promising characters. The genetic character of science is being recognized, as equally marked with the quantitative character; and there is hope for a theory of 'correlation' of these characters, which will yield a higher adaptation in the whole realm of science. Psychological parallelism then is, from the point of view of science, our positive catch.

Second, as to philosophy; we have dragged certain monsters which we have had to throw away: Interaction in both its forms — the 'cause' theory and also the 'effect' theory of mind. Having thrown away these unprofitable forms, we have thought we felt the nibbles of a fish of another species named 'æsthonomic,' a variation in the large family of Idealisms; but we have not dared to show him above water — admitting, indeed, that by a great effort we might be able to land him at all!

[1] See the writer's *Dict. of Philos.*, art. 'End-state.'

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY PROFESSOR JAMES ROWLAND ANGELL.

I.

Fatigue; Suggestions for a New Method of Investigation.

BY C. R. SQUIRE.

A period of constructive work upon a scientific problem is usually followed by one of criticism. This period of criticism is, however, the precursor of another productive period. For after flaws in the apparatus or in the method of procedure have been brought to light the attempt to remedy them is likely to follow. When the concepts involved are clarified the problem is rendered capable of more definite statement.

We have reached the second period in the study of fatigue. The attempts of Mosso and Griesbach to establish simple and convenient psychophysical methods for the investigation of fatigue, because of their very clearness and conciseness of statement, have been most open to attack. Müller's[1] criticism of the ergograph, trenchant as it is, will not lead us to discard every form of this instrument, until a better one has been substituted. On the other hand the æsthesiometer will scarcely be considered again for an investigation of fatigue by one who is familiar with the literature on the subject.

There is, however, a numerous class of investigations that have met with scarcely any criticism: These may be classed together, since they attempt to measure fatigue in the gross by ability to add or perform some other associative act of greater or less complexity. Most of these studies have emanated directly from Kraepelin's laboratory or have adopted in some particular

[1] 'Ueber Mosso's Ergographen,' *Phil. Stud.*, XVII., 1–29.

the methods there used. Thorndike's[1] criticism was directed against certain dogmas that have arisen as a result of these investigations, rather than against their method of procedure.

This type of investigation appealed to educators. The method was simple; it could, apparently, be directly employed in the schoolroom upon masses of children. This apparent feasibility, together with the lack of wholesome criticism, gave rise to a considerable number of such studies. The total result is a series of pedagogical maxims as to the relative amount of fatigue induced by the various subjects on the school program, the proper alternation of work and rest, and other maxims of similar nature. Unfortunately these maxims rest upon a very insecure scientific basis. The method of procedure is so gross that the results are not susceptible of an unambiguous interpretation.

In a review of an investigation of this class made upon school children, MacDougall[2] makes the following pertinent criticism: "The test does not get at the phenomenon at all directly or unequivocally. The material from which the results are read is the product of the total set of mental conditions obtaining at the time of the investigation; and the number of errors in any given case will as readily be affected by a feeling of rivalry between the pupils or by a momentary distraction as by the influence of fatigue itself. These influences cannot unconditionally be set down as constant factors, which are therefore eliminable. The anticipation of recess or the conclusion of work may very well be potent in establishing a law of rhythmical increase or decrease in the number of errors, which will well combine with the actual exhaustion effects to produce a curve which does not at all truly represent the rise in fatigue."

The most obvious fallacies that have determined the method as well as the interpretation of results in the majority of these investigations are:

1. That muscular and central fatigue are interchangeable terms; that one can be directly expressed in terms of the other.

2. That fatigue arising from one kind of psychical activity

[1] 'Mental Fatigue,' Psy. Rev., VII., 466–482; 547–579.

[2] Psy. Rev., VI., p. 203.

can be measured by one involving totally different processes. This is one of the most frequent fallacies.

3. That incompetency due to fatigue, and decrease in interest due to the monotony of the task, are one and the same. Few of the investigators discriminate between them.

4. That the number of arithmetical operations, etc., performed during a given time represents the whole of the psychophysical activity, and consequently the percentage of decrease in the number can be regarded as function of increase in fatigue. Leuba[1] pointed this out in his criticism of the Griesbach method.

5. That an experiment can be framed which for an hour or more will be a direct correspondent to the same period spent in the ordinary activities of school.

6. That children are fit subjects for fatigue experiments.

Children are incapable of the prolonged disinterested attention demanded by the fatigue experiment. The only test which can ever be used in investigations upon school children with any hope of obtaining reliable results, must be of a similar nature to Griesbach's. Meumann[2] suggests the use of changes in blood pressure, pulse and respiration as measures of fatigue in such cases. A trustworthy test of this character is highly desirable.

However, it is not the purpose of this paper to criticise earlier investigations; but rather in the light of what has been done to offer suggestions for further study.

What must be demanded of the new method? First, that it make possible the separation of the component elements, fatigue of muscle and central fatigue, and that it select one of these for systematic study. Secondly, that it make use of a definite method of procedure, which shall be carefully regulated, so that all the conditions shall remain constant, or vary in known directions. Thirdly, that the measure and the process measured shall be comparable.

This, as previously stated, is very frequently overlooked in the formulation of fatigue experiments.

[1] 'On the Validity of the Æsthesiometric Method as a Measure of Mental Fatigue,' PSY. REV., VI., pp. 573–598. Cf. also Germann, *ibid.*, p. 599.

[2] 'Entstehung und den Zielen der experimentellen Paedagogik, Deutsche Volksschule,' V.

Method.

The method here described was devised to meet these requirements. It seeks to eliminate muscular fatigue in order to observe the rise and general course of fatigue of attention, exempt as far as possible from peripheral distractions. This, of course, necessitates a device that shall be fine enough to register the fluctuations in the attention during the course of the experiment.

A pattern of ten digits written upon a slip of paper was given to the subject, who was required to memorize it before the experiment began. The subject was then asked to tap the pattern given him for a certain length of time. By the use of the devices of tambour, smoked drum and time-marker all variations from the pattern and all changes in the rate of speed could be read at the conclusion of the experiment and paralleled with the introspective report of the subject.

The next consideration was a means of getting at any muscular fatigue that might be induced by the tapping. The ergograph was used for this purpose. The presumption was, that if a certain muscle or group of muscles could do as much work in a given unit of time immediately after the tapping as before it, the muscular fatigue in that particular case was a negligible quantity. This test would be valueless, of course, unless (*a*) the same muscles were used in raising the weight and in tapping, (*b*) the movements made were of the same character and in the same direction in both instances. Both of these requirements were met, as will be seen.

The reagent sat in a swivel chair, raised so that his left forearm rested comfortably in the prone position on the arm-rest of the ergograph. The second and fourth fingers were clamped to the board, and the third finger was enclosed in a padded metal splint for its entire length. By means of the splint all movements of the third finger both in tapping and in raising the weight were restricted to flexion and extension at the metacarpophalangeal joint. The forward and backward movement ordinarily used in raising the weight was converted by means of a system of pulleys devised by Prof. Angell into the up-and-down movement made in tapping. This fact was accidentally verified. In one of the practice experiments, a disarrangement of the apparatus had delayed the experiment and disturbed the subject. When the experiment was finally begun, the reagent forgot that his finger was attached to the weight and tapped, as he thought, the pattern previously given him. The result was an ergographic tracing of the usual excursion arranged in groups. (A part of this tracing is given below.) A hook attached to the dorsal side of the finger piece that secured the splint and a corresponding S in the end of the wire most remote from the weight made it possible to pass quickly from the ergographic test to the tapping. During the tapping the splint and

finger piece were left exactly as they were when the weight was lifted.

The order of procedure was as follows: The pattern was first given to the subject; when he was confident that he had memorized it, the finger was attached to the weight. At a signal from the operator, the subject, his eyes being closed, began to lift the weight. He continued to do this until he received the signal from the operator to stop. This was given at the end of a minute. No restrictions were made as to the rate at which the weight should be lifted. It was thought that a compulsion to work at any other than the natural rhythm would of itself induce fatigue. The ergographic tracing, together with that of an electric time-marker, was made upon the smoked drum of a kymograph.

After the minimal pause necessary to detach the weight and adjust the lever for tapping, a third signal was given and the subject began to tap the pattern previously learned. The writing point of the time-marker was adjusted as closely as possible under the writing point from the tambour in order to economize drum space. The tapping, except in the practice series, was continued until the drum was filled or time at disposal of the subject at an end. Some of the later series lasted a little over forty minutes.

The kymograph was then quickly adjusted for the second ergographic test. The lever from the tambour was set aside, weight attached, and the tracing taken for the same time and in similar manner to the first.

The objections that have been raised to the use of the ergograph in fatigue tests were either met by some arrangement of the apparatus or were naturally ruled out through the use we made of it. The leverage was kept constant. This was insured by the arrangement of the splint. Müller attacks the ergograph on the ground that it is impossible to isolate a single muscle or even a group of muscles with it and so study their fatigue. This does not vitiate our results, however, since no matter how many muscles or groups of muscles were active, the same muscles and the same groups of muscles were engaged when tapping as when lifting the weights.

In both the lifting and tapping we were able to discern a movement of muscles of the forearm. This would appear to substantiate Müller's statement.

Another objection that has been made to the ergograph by Müller and others with right is, that the necessary tightness of the band around the third finger hinders circulation and consequently induces fatigue earlier than it would otherwise arise. We were also able to rule this out in nearly every test. The pull of the weight which ordinarily tends to displace the finger piece when the movement is backward and forward, served in this case with the up-and-down movement to hold the finger piece in place. The bands were consequently kept much looser than would have been possible with the usual arrangement.

Moreover, the ergographic test was never continued to the fatigue point. We were not concerned with the curve of muscular fatigue nor with the absolute amount of work done, but with the relative amount before and after the tapping which constituted the fatigue test proper. In the first trial series thirty seconds was taken as a unit for the ergographic test; it was found, however, with the weight of 800 grams which was used throughout the experiment, that one minute was not long enough, even when followed by the tapping, to induce muscular fatigue. (See table.) It was consequently taken as the unit in all the series that followed.

The tapping in the first trial experiments was continued for twenty minutes, although incipient fatigue was evidenced some time before the end of the twenty minute series; this time, however, was too short to give a fair expression of the course of fatigue. In some of the series the tapping was continued for forty minutes. It was impossible to regulate the time of tapping exactly and get the maximum duration, since we were not always equally successful in economizing drum space. One subject had but an hour at his disposal; when there was a delay in starting the experiment, it was frequently necessary to stop the tapping at the end of thirty minutes. In most cases fatigue of attention became evident after fifteen minutes.

Although all movements tend to become automatic, the experimenter had hoped that a pattern of ten digits would present difficulties enough to prevent its mechanical reproduction within the short space of time given to the experiment. Here a difficulty arose. One subject in particular manifested a very strong tendency to reduce the whole process to automatic action. To obviate this difficulty the subjects were asked to give the series in alternation with the series multiplied by two; on the next day the pattern given was varied in two additional series. The series were made more and more complex until the test consisted of the pattern and four variations. Then the order was reversed, with three, two and one variations, until finally the subjects were simply asked to tap the series memorized. The patterns were as follows:

July 7th	4 4 2 1 3 2 2 4 3 1 then
Series multiplied by two, or	8 8 4 2 6 4 4 8 6 2 then
Ser. multiplied by two minus one, or	7 7 3 1 5 3 3 7 5 1.

July 8th	3 2 2 4 3 2 4 4 2 3 then
Series minus one, or	2 1 1 3 2 1 3 3 1 2 then
Series multiplied by two minus two	4 2 2 6 4 2 6 6 2 4 then
Ser. multiplied by three minus three	6 3 3 9 6 3 9 9 3 6.

July 9th	1 1 4 2 1 3 2 2 1 3
Series plus two	3 3 6 4 3 5 4 4 3 5
Series multiplied by two plus one	3 3 9 5 3 7 5 5 3 7
Series multiplied by two plus three	5 5 11 7 5 9 7 7 5 9
Series multiplied by two minus one	1 1 7 3 1 5 3 3 1 5.
July 10th	3 1 2 1 4 3 4 1 2 1
Series multiplied by three	9 3 6 3 12 9 12 3 6 3
Series multiplied by three plus two	11 5 8 5 14 11 14 5 8 5
Series multiplied by two plus three	9 5 7 5 11 9 11 5 7 5.
July 11th	2 1 3 4 1 1 2 4 2 3
Series multiplied by three minus one	5 2 8 11 2 2 5 11 5 8
Series plus three	5 4 6 7 4 4 5 7 5 6.
July 12st	3 1 1 2 4 3 3 1 3 4
Series multiplied by two	6 2 2 4 8 6 6 2 6 8.
July 14th	2 4 2 1 2 4 1 3 3 2.

The subjects were: Miss H., a principal of a ward school in Chicago; Mr. B., a teacher of mathematics, and Mr. K., also a teacher of mathematics. None of these had previously had much practice in introspection. This lack of previous practice was, however, more than counterbalanced by their interest and the pains which they took to follow all directions given.

After five practice series had been taken, it was evident that the tendency to automatic action in two of the subjects would make it impossible to study the fatigue of attention if the simple pattern alone were reproduced. The course of procedure just described was then begun. The subjects had by this time become familiar with the apparatus and the general course of the experiment.

The hours of experimentation were: *B*, 8–9 a. m.; *H*, 10–11:15 a. m.; *K*, July 1 and 2, 3:30 to 4:30; after that 1:30–3.

In the time at our disposal we could only hope to demonstrate the feasibility of our method.

It now remains to be seen whether the results warrant the assumption that the method above described meets our requirements. Do the results indicate that it was possible to regulate the experiment so that we are able to disregard the element of muscular fatigue? The following table answers this question:

TABLE I.

	H.				*B.*				*K.*			
	Number.		Distance.		Number.		Distance.		Number.		Distance.	
	I.	II.	I.	II.	I.	II.	I.	II.	I.	II.	I.	II.
July 1	46	53	167	206	38	40	147	180	60	61	211	246*
" 14	42	49	147	187	39	42	152	174	54	60	225	220
" 2	50	54	100	108	40	37	137	142	51	68	214	227
" 12	40	47	175	186	35	42	140	164	52	61	246	284
" 7	37	48	116	133	36	36	181	206	63	48*	172	194
" 11	37	47	126	160	36	40	151	183	56	54*	212	270
" 8					34	42	178	228	64	56	280	265
" 10	38	45	156	148*	40	38*	150	164	45	49	196	205
" 9					35	37	133	151	53	53	175	154

This table records the number of times the weight was raised by each subject before (I.) as compared with the number of times after (II.) the fatigue experiment; the distances through which the weight of 800 grams was raised in the two instances are also compared. The results are grouped with reference to the complexity of the series. It can be seen that muscular fatigue was a negligible quantity in most of the series, for with a very few exceptions the weight was raised a *greater* number of times and through a greater distance after the fatigue experiment than before it. All of the subjects at one time or another reported that the excursion they would have made in the second test was cut short by the limitations of the apparatus. This difficulty was never experienced in the test preceding the fatigue experiment. The objective difference in ability to raise the weight in the two instances was thus really less than the subjective, as reported by the subjects. They found relief in the raising of the weight after the strained attention required in tapping. It was undoubtedly this feeling that brought about the constant difference in favor of the second test.

A decrease in the number of times the weight was raised is more frequent than a decrease in the distance through which it was raised. Kraepelin believes that the first factor is conditioned by central fatigue; the second alone by muscular. Our results do not substantiate this theory, however, since there was evidence of central fatigue, when there was a marked increase in the number of times the weight was raised.

The introspective reports of the subjects explained nearly all the exceptional cases.

These cases were found in nearly every instance to be due to some change in the conditions of the experiment, most frequently to a disarrangement of the apparatus. If we discard all those series — marked in the table with an asterisk — in which some disturbance in the regular conditions of the experiment was noted either by the subject or the observer, our results surely warrant the conclusion that this method gives us a means of eliminating muscular fatigue; and that the errors made in the production of the series tapped were not due to muscular fatigue. Our results seem to indicate that central fatigue does not decrease ability to do muscular work.

Thorndike[1] with the dynamometer could find no decided decrease of physical power after continued mental effort.

Bolton[2] found that "Certain forms of mental activity, probably such as are associated with *motor* excitations, appear to increase the height of the ergographic curve, although they undoubtedly produce central fatigue." He says again, "Dennoch beweisen unsere Versuche mit vollster Bestimmtheit dass es nicht angeht, aus dem Sinken der Muskelkraft irgend welche Schlüsse auf den Grad der geistigen Ermüdung zu ziehen, um so weniger als Oseretzkowsky auch nach dem sehr ermüdenden Auswendiglernen eine erhebliche Zunahme der Muskelleistung fand. Wahrscheinlich sind die gegenseitigen Beziehungen sehr verwickelte. Bevor also Massenversuche angestellt werden, ist es unumgänglich nothwendig, mit grösster Sorgfalt der Wechselwirkung zwischen geistiger Thätigkeit und Muskelleistung im einzelnen nachzugehen, damit wir erst ein richtiges Verständniss dafür gewinnen, was wir eigentlich messen."

We quote Bolton at length as he here emphasizes the necessity of a method of procedure similar to that followed in this experiment, *i. e.*, the elimination of one factor in order that we may more accurately measure the other.

It is, however, surprising that one is capable of such prolonged and fairly rapid contraction of the muscles of the finger as is necessitated in the tapping of these patterns for forty minutes, without inducing well-marked muscular fatigue.

[1] 'Mental Fatigue,' PSY. REV., VII., 578.

[2] 'Ueber die Beziehungen zwischen Ermüdung, Raumsinn der Haut und Muskelleistung,' *Psy. Arb.*, IV., 187, 232.

Take one record, that of *B* on July 14, for illustration. We find that the pattern was repeated one hundred and twenty-three times before the close of the series. The pattern when completed requires twenty taps. If we neglect those taps that were repeated and omitted—the one will probably nearly balance the other—the total number of taps made by *B* in that record was 2,460. This is by no means the greatest number of taps made during any one series, for the complex patterns required a greater number of taps for the complete pattern and were frequently repeated quite as many times as the one taken for illustration. When we turn to the table, it is seen that there is no evidence of muscular fatigue in the ergographic record taken after this tapping.

Moore[1] says that he once made 480 taps, although he was sure that he could not have made one hundred more at the close of any record. In his records fatigue was manifested as early as the seventieth tap. The early rise of fatigue in his case may have been partly due to the arrangement of the apparatus; the tapping may have been violent and at a speed exceeding the natural rate of the subject. (While it is not the aim of this paper to discuss the question of the rise of muscular fatigue, the query suggests itself whether tapping could not be prolonged indefinitely, if circulation were unimpeded and the natural rate of the subject maintained, without giving rise to muscular fatigue.)

The record of *H* for July 2 is free from any trace of fatigue. Her rate was very slow; she followed the rhythm of the time-marker, one tap a second. At this rate she was able to give series after series without making an error. The rate here is much slower than that of the other subjects and also than that of her own subsequent records. In her introspective report she notes that this series was not so fatiguing as that of the day previous; because of its variety it was more interesting and therefore easier to attend to.

Is not the early rise of fatigue in Moore's experiment due in part to the direction of the attention upon the contraction of the finger and the sensations involved? In our problem the atten-

[1] 'Studies of Fatigue,' *Studies from Yale Laboratory*, III., 92.

tion was necessarily directed upon the pattern and the associations called for. Is it possible that the central excitation retarded the rise of muscular fatigue? However that may be, muscular fatigue did not play a noticeable part in these experiments and can be disregarded when we attempt to explain the irregular and inaccurate tapping.

It will surely be apparent at once that our second requisite of a satisfactory method was fully met; viz., that it was possible to regulate the method of procedure so that the conditions should remain constant or vary in known directions.

It can also be seen from the description of the method that the measure and the processes measured are comparable, since by the terms of the experiment the one is contained in the other. The measure of fatigue was the increase in the number and duration of errors and irregularities during the production of the memorized and associated series.

The errors and irregularities were of various sorts; taps added or omitted, groups added or omitted, groups broken by long pauses, whole groups or series repeated, irregular succession of groups in which there was no trace of the pattern given and at times the whole series of ten digits was omitted.

None of these inaccuracies or irregularities as such can be considered indices of fatigue; lack of practice would also cause irregularities; but irregularities due to this cause would *decrease* as the series were repeated. Irregular following upon a long period of fairly correct tapping, *i. e.*, after practice has had its maximum effect, can hardly be regarded otherwise than as a pure fatigue phenomenon, unless it be due to lack of interest and consequent inattention. Lack of interest could not be advanced as an explanation in the case of these subjects.

As fatigue enters, recall is halting and often fails, the associations weaken. As the associations fail, some appear to hold together better than others.

K in a certain series had firmly associated the combinations 5, 8, 5 and 3, 6, 3. As he became more and more fatigued, he interspersed these with groups from all the series regardless of order of groups or series. Again, the first five or six groups of the first series were more closely associated than the last members of this series; so were the last five or six groups of the second series, than the first members of that series. As the experiment was continued and fatigue became apparent, he frequently omitted the intervening groups, although he never failed to give these in their correct order. This is doubtless explained by the fact that all of the subjects tended to divide the series of ten digits into smaller series. Thus the groups memorized together were undoubtedly recalled together.

The results show that:

1. Gross irregularities become more frequent as the series are continued.

2. The increase in inaccuracy and irregularity is not continuously progressive, but shows fluctuations.

3. While the complex series, as we should naturally expect, show a greater number of errors at the beginning of the record than the simple series, the proportion of increase as the record proceeds is really less than in the case of the simple series.

Certain of the complex series have not only a relative advantage over the simple with regard to the increase in inaccuracy, but an absolute. The number of errors was least for *B* when one variation of the original series was introduced; for *H* and *K*, when the original series had two variations added.

Scripture[1] found that "The fatigue in reaction time increased with the complexity of the adjustments required for perceiving the stimulus. There is least fatigue where only an effort of attention is involved, more when the act of accommodation is added and still more when the act of convergence is added."

The two instances, though they might appear comparable at first sight, are not. An experiment in reaction time by the very conditions of the experiment keeps the attention on the alert; each stimulus gives a new impulse. With the tapping the control was central; no external stimulus served to reinforce the central mandate. Therefore, the greater the monotony, the greater the strain on the attention and the more fatiguing the series.

H says: "At times the deadly monotony of the tapping

[1] 'Researches in Reaction Time,' *Yale Studies*, IV., 12.

nearly overcomes me." At such times peripheral stimuli tend to force themselves upon the attention and lapses occur.

The most complex series were also extremely fatiguing; the associations were not so readily made; there was also a greater number of things to be memorized. When the pattern was *slightly* varied, the demands upon the attention were not so great as with the simple or very complex series.

The effectiveness of the simple as compared with the complex series in producing fatigue, varied with the individuals. *H* always found the simple series very fatiguing because of their monotony; while *B* says: "The simple series produce less fatigue. They become in part automatic."

With the most complex series the subjects frequently reported a feeling of relief as they returned to the original pattern. One variation of the original series was at times apparently easier to give than another just preceding it or following it. This might very well be expected, since more associations were required in some cases than in others, etc. To double a series would naturally be less difficult than to double and then to subtract one.

A general periodicity in the number of errors was to be observed. A similar phenomenon was frequently noted in the height of the excursion of the writing point. Such records began with a maximal excursion, which seldom lasted beyond the second rotation of the drum, a period of about five minutes' duration. After this the excursion grew smaller quite rapidly. Then followed frequent fluctuations in the height of the excursion, although the maximal height was never reached again.

The more complex the series, the less marked was the rhythm above noted. In these series the excursion throughout the record was slight and quite even. This phenomenon was not frequent enough to make a detailed study of it possible.

However, it is possible by means of the irregularities in the tapping to follow the general course of the fluctuations in the attention.

The facts brought out by the records are:

1. The duration of the fluctuations increases as the series progress.

2. Their frequency increases also, although not continuously.

3. Their duration and frequency are less in those complex series which give the advantage of variety without calling for too many and too involved associations.

A period of prolonged inattention or inaccurate and irregular tapping is always followed by one fairly free from fluctuations. Kraepelin[1] and Voss[2] have noted the same phenomenon and have given it the name of '*Müdigkeitsantrieb*.' This is especially noticeable in *K's* record.

It was impossible to rule out all distractions arising from external stimuli. Those most frequently noted were:

1. Click of the time-marker, strong tendency to follow its rhythm especially in the early series. "After the first tap there was a tendency to wait for the time-marker to click before making the next tap. This gave an opportunity for the attention to wander, especially toward the sound of the marker, and often resulted disastrously."

2. The sensations resulting from the impeded circulation consequent upon the binding of arm and finger.

3. Constrained position of the body. *H* resented the fact that she was obliged to close her eyes. This resulted in a distraction of the attention.

4. The pounding of the workmen outside, mentioned only by *H*.

5. The click of the metal finger piece, which was heard when the splint became disarranged, served at times as a distracter, at other times as a stimulator.

The fluctuations, however, were not dependent upon external causes. Neither were they subject to voluntary control, as the following report shows: "Could anticipate lapses of attention but not ward them off. The feeling was not similar to that experienced in muscular fatigue. Merely a sense of the inevitableness of the impending lapse. When the lapse came, it was not recognized as such until after the mistake had been made. This, of course, called forth effort to prevent further

[1] 'Ueber Ermüdung und Erholung,' *Psy. Arb.*, I., 678.

[2] 'Ueber die Schwankungen der geistigen Arbeitsleistung,' *Psy. Arb.*, II., 409.

blunders, which were generally prevented, but a sense of strain was always felt before the work would run smoothly again. Moments of especial clearness were observed when I could remember the series for quite a distance ahead. These may be the crests of the attention wave. They were not subject to control."

These are not the normal fluctuations of attention, which are present even in the most intense attentive state. Several important differences in the normal and the fatigued attention wave are to be noted. (1) The normal fluctuations are much shorter in duration. (2) The normal are more regular in their order of occurrence. It is only necessary to observe the records near the close of a long and difficult series to note the great irregularity of the fluctuations in a state of fatigue. (3) The lapses are more intense and recovery more difficult in the fatigued state. All of the subjects report occasions of total blankness. *K* experienced them most frequently as times when he is 'entirely at sea as to which of the series was being tapped.' *B* says: "Sometimes I have felt completely lost, utterly in the dark for an instant. Recovery in this case, although rapid, was with unusual effort." (4) When the subject is fatigued, voluntary control is weaker; he is not able to shut out distractions that would not trouble him ordinarily. The simple series gave more occasion for fluctuations of this nature than did the complex. The ease of the series and the monotony of the task favor mind-wandering in the most conscientious subject. *K* compares this state to that experienced when fatigued by adding long columns of figures. With the complex series, peripheral distractions are not so frequent a cause of fluctuations. In these series, lapses are due to difficulty in making proper associations and to halting recall of groups and of series. "The interval which was required to decide what series should come next was often long."

What is the cause of these fluctuations? It is most probable that they have much in common with fluctuations of attention under normal conditions. In case of fatigue, however, innervation is accomplished with greater difficulty and is therefore accompanied by sensations of bodily strain or a 'feeling of effort' as reported by the subjects. Thus the period of uncertainty is lengthened and an external stimulus such as the click of the apparatus may serve either to enforce or distract, according to the disposition of the individual.

A decrease in rate of speed has been regarded as indicative of fatigue; some experimenters have used it alone as a measure of fatigue.

Gilbert[1] in his experiments upon school children found that the loss of speed in tapping for 45 sec. at six years was 21.4 per cent.; at fifteen years 12.7 per cent.

Moore[2] found that fatigue lengthened the time, although the increase was not a steady one but showed fluctuations. Oehrn[3] repeats the statement frequently made by Kraepelin. "Two factors influence the duration of psychical processes. Practice produces an increase in their rate; fatigue a decrease." Scripture[4] found in his experiment on reaction time that a lengthening of the average times appeared sooner than an increase in their mean variations.

We were at first inclined to accept the current opinion, but our results, although most certainly indicative of fatigue, do not show any regular decrease in the rate of tapping.

The series were divided into five-minute sections, and the average duration of each of the patterns during the successive five-minute periods was found. Often there was an increase in the rate of speed as the tapping continued, although there was well-marked fatigue. On the other hand, while the size of the M. V. decreased for a time with practice, it gradually increased toward the close of the series.

It is quite as likely that decrease in rate of speed is due to a feeling of weariness, of tediousness. As that is an index of fatigue, the first is often confused with the latter. While this decrease of rate may be expressive of fatigue, it is not always and necessarily so. A certain irregular and fitful tapping at increased rate is quite as much an index as decreased speed.

[1] 'Mental and Physical Development of School Children,' *Yale Studies*, II., p. 40.

[2] *Op. cit.*, p. 95.

[3] *Psy. Arb.*, I., p. 92.

[4] *Op. cit.*, p. 19.

Some of the testimony clearly cited is in perfect harmony with this presumption. Gilbert found the decrease in speed greatest in the case of the young children who are less able to resist a feeling of tediousness, and less capable of a prolonged voluntary effort, than are adults. Our subjects were unusually free from such an influence; interest in the problem kept them at a point of the highest possible efficiency.

In our opinion the size of the M. V. is a much more accurate measure of fatigue than the average rate of speed. It can be regarded as the coefficient of irregularity, and together with the purely qualitative measure — the degree of accuracy — gives us a fairly reliable standard, when the method of experimentation does not take into account the duration and frequency of the fluctuations of attention, which after all furnishes the most accurate measure.

In our previous discussion we have taken it for granted that there is a state of fatigue, a condition of real incompetency. The fatigue induced by these experiments was not always general, or marked by incompetency in *all* activities of the day, during the hours that succeeded the experiment.

While the possibility of a state of general fatigue is conceded by nearly all investigators, there does not seem to be any recognition of the fact that real fatigue may exist without being general; that there is a state of special fatigue.

Kraepelin[1] distinguishes between '*Müdigkeit*' and '*Ermüdung*.' MacDougall[2] distinguishes between 'weariness' — personal and fluctuating and dependent upon a superficial act of attention — and 'fatigue' — a deep-seated nervous phenomenon. These classifications are helpful in calling attention to the fact that all which passes under the name of fatigue is not genuine fatigue, but is often a feeling of boredom with a distasteful task. However, if any piece of work is continued long enough, there arises a condition which we may well call special fatigue or real inability to continue the special activity at anything like the highest possible efficiency. It is a state of nervous exhaustion, though not so wide spread as when fatigue is general. Possibly a totally different activity could be engaged in directly

[1] *Op. cit.*, p. 623.

[2] *Op. cit.*, p. 203.

after the subject had experienced special fatigue without a noticeable failure in either quality or quantity of work.

Thorndike errs in failing to recognize special fatigue. We cannot measure ability for one kind of work by a test which involves other mental processes. It is not surprising, when we consider this fact, that Thorndike found school children were able to do as good work after school as before. "Why I do less work of some special sort after being engaged in it for a long time is not that I am not able but because I don't feel like it," is only partially true. If the work were strenuously continued for any considerable length of time, there would be real inability to do that *special* work effectively; although there might not be a decrease in the ability to do other work, which would be the case if the fatigue were general.

This method enables us to distinguish between general and special fatigue. It should also be possible by gradually lengthening the series to ascertain for each of the individuals the point at which special fatigue became general.

In these experiments we have been chiefly concerned to find a satisfactory method for the study of the fatigue of attention. The method could be used, however, to investigate the effect of fatigue upon recall. As we used it, it is not adapted to the investigation of the effect of fatigue upon the accuracy and readiness with which associations can be made. We found that the variations of the original pattern after a few repetitions were memorized as series; the associations or arithmetical operations called for by the terms of the experiment were omitted and the reproduction of the series became simply a matter of recall. However, the relation of fatigue to association can be investigated, as will be seen, by a slight alteration in the original method.

By a modification of the experiment, it would be possible to determine the effect of fatigue upon the perception and the time interval between stimulus and perception. Muscular fatigue might be eliminated as it has been in this experiment. In place of a memorized series the subject could be required to repeat the stimulus given by the experimenter, or still better by some apparatus arranged to give any desired number of stimuli in any desired order of succession. The fatigue experiment in this case would be one in simple reaction time carried to the fatigue point and beyond.

This modification, with another slight variation in the method of procedure, would enable us to study the effect of fatigue upon the ease and accuracy with which associations are made. It would simply be necessary to require the reagent to make some definite associative reaction instead of giving a repetition of the stimulus. The ergograph furnishes us with a fairly reliable means of eliminating muscular fatigue in any of these cases.

II.

Meaning in Memory and in Attention.

BY KATE GORDON.

I. Memory as Dependent upon the Complexity of its Content.

It would require no small degree of skill and patience to give an adequate summary of all that has been well done and well said upon the subject of memory and its laws. Without attempting that, we may take it for granted that one or two tendencies are sufficiently apparent. In classifying the laws of memory it has generally been held, by recent writers at least, that there are, in the last analysis, two mutually irreducible principles of association. One principle stands for a connection between things whose first conjunction was fortuitous and whose bond is purely mechanical; the second represents a connection between objects which seem peculiarly fit to go together or which have some intrinsic relation quite apart from simple contiguity. In Wundt,[1] for example, we find the following: "Fasst man so * * * alle Associationen als Resultanten elementarer Verbindungsprocesse zwischen einfachen Empfindungen oder relativ beschränkten Empfindungscomplexen auf, so sind nun an und für sich nur zwei solche Elementarprocesse möglich," * * * *i. e.*,* * * die Verbindung gleicher Elemente, und die Verbindung solcher, die durch gemeinsames Vorkommen in einen

[1] Grundzüge d. Phys. Psy., vol. 2, p. 468.

functionellen Zusammenhang getreten sind. Wir wollen diese beiden Formen der Elementarverbindung die Gleichheitsverbindung und die Berührungsverbindung nennen." These two forms are elsewhere called intensive and extensive, or internal and external association. Aschaffenburg[1] quotes in his work a number of proposed classifications which adhere to this same two-fold division, for example, that of Trautscholdt as follows:

I. *External Association* (*i. e.*, by contiguity).

A. Of simultaneous ideas.

i.—2.—etc. (subordinate divisions).

B. Of successive ideas.

i.—2.—etc. (subordinate divisions).

II. *Internal Association* (by similarity).

In the other schemes of division which Aschaffenburg quotes (Kraepelin, Wahle, *et al.*) and in his own, the distinction of inner and outer association is maintained apparently as ultimate. The reaction against the extreme form of associationism as held by Mill, Bain and others is represented in Stout[2] by the great importance which he attaches to the *conative unity* of consciousness. Stout also seems to uphold the classification of associations as external and internal. He calls them respectively the 'general unity and continuity of consciousness' and the 'conative unity and continuity of consciousness,' meaning, apparently, accidental and intrinsic connections between psychic elements. In Mr. James's categories of frequency, recency, vividness and emotional congruity,[3] we recognize, in the frequency and recency, the external or mechanical conditions of remembrance, and in the vividness and congruity, the intrinsic conditions which stand for the amount of attention and interest bestowed. James's analysis of the 'similarity' association, however, tends to reduce the usual sharpness of the difference between contiguity and similarity, inasmuch as he points out that similarity is a case of partial identity, which brings it back to terms of the contiguity of elements.

[1] 'Experimentelle Studien u. Associationen,' *Psych. Arbeiten*, Vol. I., p. 209.

[2] 'Manual of Psy.,' pp. 71 ff.

[3] 'Psychology,' Vol. I., p. 577.

If we accept the division of associations into external and internal, we may say that the greater part of the experimental work in memory has been engaged with the external or mechanical factor. The experiments of Wundt and Ebbinghaus, for instance, deal mainly with the influence of repetition and of time-intervals upon memory. The question in this type of experiment seems to be this: given a certain content, by what device can it best be impressed? In the experiments reported below the object was to study the nature of the content itself as affecting association, to find out what sort of material was impressive in itself. If we may say that the essential fact about memory is that earlier conscious process functions by representation in later conscious process, then we may call the test of effectiveness or goodness of memory the likelihood of recall which a given content enjoys. The question here at issue is: what effect, if any, does the value of a psychical object have upon its chances of reproduction? The experiments were devised under the assumption that meaning or value is to be defined on the functional side in terms of attention and interest, and on the structural side in terms of complexity; that the more complex a content is, the greater the numerical aggregation of elements, *i. e.*, the greater the number of its differentiæ, the more it means.

Among the writers who have recognized the quality of the object as a factor in association are the following. Külpe [1] says, in his chapter entitled 'Incentives to Reproduction and Liability of Reproduction,' that the liability of reproduction depends upon the nature of the conjunction of sensations in consciousness. Besides spatial and temporal colligation, he there mentions as important (*a*) the remoteness of impressions from each other in space and time. "The greatest liability of reproduction is correlated with a direct contiguity or succession." (*b*) The more a content differs from its surroundings, the easier will be unification and recall. (*c*) The existence of a name for the complex makes it more effective for central excitation.[2]

[1] Külpe, 'Outlines of Psy.,' pp. 199–202.

[2] *Cf.* also Lehmann, *Phil. Studien*, Vol. V., p. 135 and F. Angell, *Phil. Stud.*, Vol. XIX., p. 1.

He says, further, that the more individual the quality of the connected content, the stronger is the liability of reproduction. "The simple sensation, which * * * can occur in the most various connections, does not as a rule possess any considerable effectiveness for central excitation. But more complicated processes, ideas, are often quite individual in character, etc." Kennedy[1] writes: "The accuracy of recollection is largely conditioned by the character of the content to be remembered." There are [he is summarizing the doctrine of others] two general ways in which a thing may be remembered. (1) By immediate recall, as when fine shades of color are reproduced just as they were in reality. (2) By mediate recall, in which some third factor enters. This is the way in which complex material is remembered; it is fixed by our concepts or classifications of it (*i. e.*, by translations or indirect means). Attention, repetition, rhythm and the general character of the object—these, he holds, govern the 'reception of the object.' "In general we may say that the reception which an object gets into consciousness depends very largely upon the sort of object it is." The idea expressed by the two men just quoted is, of course, commonly remarked, but experimental evidence has been rather more meager.

A research was conducted by Binet and Henri[2] upon 'La mémoire des mots' and 'La mémoire des phrases.' The authors here distinguished the memory of sensations from the memory of ideas. They used as content to be recalled, first, lists of figures (memory of sensations), then lists of isolated words, and finally words in rational combination, *i. e.*, sentences (memory of ideas). They found that words were recollected more easily than figures, and that among such isolated words those were most likely to be recalled which were most intimately connected with the subject's own habits. Sentences were better remembered than an equal number of disconnected words. It should be remarked that although Binet and Henri were working in this last research upon the memory of *ideas*, they appeared to make the exact verbal reproduction the test of correct

[1] 'On the Experimental Investigation of Memory,' PSY. REV., Vol. 5, 483, 488.

[2] *L'Année Psy.*, Vol. 1, 1–23, 24 and 25.

memorizing; substituted words or equivalent expressions were not accepted as being equally good as the precise phraseology of the original material. Miss Calkins[1] and Mr. Kirkpatrick[2] have severally made experiments comparing the memorizing of the abstract (*e. g.*, of words) with that of the concrete (as of pictures). They reached the same general conclusion, namely, that 'the superiority of concrete (objects) to verbal, as memory material, is under-estimated rather than over-estimated even by its most strenuous upholders.' Particularly was this true in the case of delayed as distinguished from immediate recall. W. G. Smith[3] has studied the relation of attention to memory. Smith's experiments illustrate the effects of the distraction of attention from the memory process. Various forms of distraction were used which proved progressively engrossing, with the result that memorizing became poor in proportion as attention was withdrawn. Smith's general contention is that contiguity is a merely formal factor and that the real explanation of effective memory lies in attention and interest. Bigham and Münsterberg[4] have made investigations (with which those of Cohn[5] also agree) showing that the memory of words is more certain if they may be both seen and heard by the subject — that the apprehension by several avenues is better than by one. Miss Calkins in another investigation[6] says that the readily associated objects are the 'interesting' ones; and interest she analyzes (in agreement with James) into the primacy, the frequency, the recency and the vividness of the content presented. The outcome of her work shows that the material which does sustain these relations is that which is best remembered. Margaret Keiver Smith[7] in a study of 'Rhythmus und Arbeit' has shown the effect of variety in rhythm upon association. Nonsense-syllables were learned by heart, the number of necessary repetitions being the test of readiness of memory. It was found

[1] Calkins, PSY. REV., Vol. 5, 451.

[2] Kirkpatrick, PSY. REV., Vol. 1, 602.

[3] 'The Relation of Attention to Memory,' *Mind*, N. S., Vol. 4, 47.

[4] PSY. REV., Vol. 1, 34.

[5] *Zeits. f. Psy. u. Phys. d. Sinnesorgan.*, Vol. 15, p. 161 ff.

[6] PSY. REV., Vol. 3, p. 32.

[7] *Phil. Studien*, Vol. 16, p. 197 ff.

that, other things equal, the syllables which were presented in simple rhythms were harder to acquire than those which came in the more complex rhythms. Thus, syllables were learned more readily in the dactylic than in the iambic meter. Allin[1] is also an advocate of the general standpoint which grounds effectiveness in association upon attention and interest.

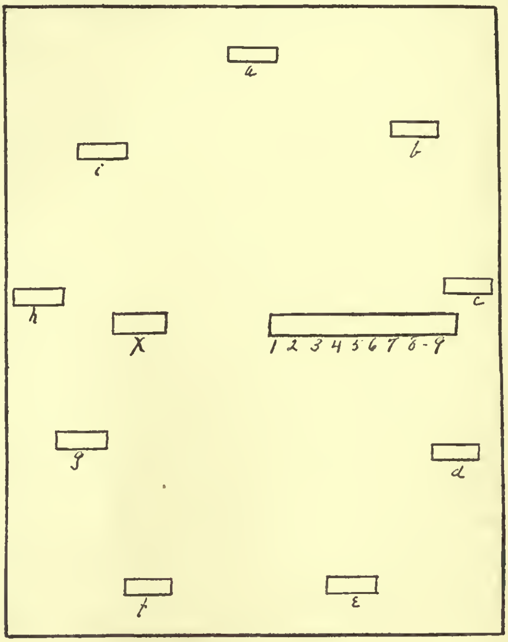

FIG. 1.

S represents a black screen 22 by 28 inches in which various small openings were cut and behind which a revolving wheel carried the printed syllables.

Case I. The syllables appeared one after another at *x*.

Case II. Syllables were shown one after another along the line 1-2-3-4.

Case III. They appeared along the same slot but in a haphazard order, *e. g.*, 5-3-1-9-6-8-7-4-2. This order would remain constant during the learning of any one series of syllables, but was changed every time a new series was begun.

Case IV. Syllables were shown in regular succession around the circle *a-b-c-d*.

Case V. They appeared in irregular succession, as *e. g.*, *f-g-c-e-h-i-a-b-d*.

It may be in place to mention here one or two opinions on the nature of the memorizing of a complex content. Kennedy, as above quoted, distinguishes mediate and immediate recollec-

[1] 'Grundprincip. d. Association.'

tion, and says that the memory of complexes is mediated by some third factor intervening between the two primary members of an association. Scripture[1] and others allow the legitimacy of such a description and have shown that association between two objects may take place by means of a third object which is not consciously recognized by the subject as having performed that office. Cordes,[2] however, explains 'mittelbare Associationen' as cases in which the A-phenomenon (or first member) calls up as a B-phenomenon (or second member) a complex idea. The part which is afterwards analyzed out and called the third or mediating term, is, he says, just as much a part of the B-phenomenon as is the element which stood in the more prominent place in consciousness.

The general plan of the following experiment was to select the simplest feasible material for memorizing and to compare the time taken to learn this with the time needed to learn a content into which two or three complications upon the simple first form had been introduced. The first member in each comparison was a series of nine nonsense-syllables, *e. g.*, vax, tir, etc., printed on slips of white paper and shown one at a time through the same opening in a black screen, x, Fig. 1. The complication of this situation took three forms.

I.

In the first form of the experiment the content was complicated by adding to it a spatial differentiation. The list of nonsense-syllables, instead of appearing in the same place, x, appeared not only in temporal succession but also in spatial succession. The nature of this displacement was varied so that there were in all five different cases to be compared.

Each series was repeatedly shown to the subject until it was learned by heart. Thus the number of repetitions stood as the test of the difficulty of a series. After an interval of three days the same five series were learned over again in the same way, thus giving a test of their effectiveness in delayed recall. In order to distribute evenly the effects of fatigue and of practice

[1] 'The New Psychology,' pp. 201–202.

[2] Cordes, 'Ex. Untersuchungen,' *Phil. Stud.*, Vol. 17, pp. 73–77.

during a sitting, the five sorts of series were presented from week to week in revolving order: *e. g.*, (1) I., II., III., IV., V. (2) II., III., IV., V., I., etc. In the subjoined tables the Roman numbers stand for the five kinds of space relation, the Arabic numbers represent the repetition necessary for correct reproduction. Each of these numbers is the average from five tests.

FIRST LEARNING.

I.	A.		B.		C.		D.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	4.75	.75	5.4	.72	4.2	1.04	7.0	2.00
II.	4.25	.75	4.6	.88	4.6	.48	8.4	2.08
III.	4.50	1.00	5.2	.64	5.4	.88	7.8	2.56
IV.	4.75	.375	4.0	.40	4.2	.84	8.4	.72
V.	6.50	1.50	4.8	.64	4.2	.64	8.0	1.60

SECOND LEARNING—THREE DAYS AFTER.

I.	3.00	.50	2.8	.64	3.0	.40	3.6	.72
II.	3.75	.375	3.2	.64	3.0	.40	4.2	1.44
III.	3.50	1.00	3.6	.48	3.6	.48	4.4	1.68
IV.	2.50	.50	3.0	.40	3.0	.40	4.4	1.16
V.	3.75	.75	4.0	.80	3.2	.96	6.0	1.60

	E.		F.		G.		H.		I.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	5.2	1.04	6.2	.84	15.0	2.80	11.5	.87	5.2	.64
II.	4.4	.64	5.6	.72	11.00	2.0	9.0	3.50	4.6	1.52
III.	4.8	.96	8.0	2.00	16.00	5.2	10.5	2.00	4.2	1.04
IV.	4.0	.80	6.0	1.20	18.40	4.88	8.0	.50	4.0	1.20
V.	7.2	1.84	8.4	1.68	17.6	2.48	11.0	2.00	4.0	.80
I.	3.8	1.04	4.8	.64	9.2	2.56	4.75	.37	3.0	.40
II.	3.0	.40	4.0	.80	7.0	2.80	3.75	1.12	2.6	.48
III.	3.6	.72	4.4	.88	6.4	.84	5.00	1.00	3.0	.40
IV.	3.0	.40	4.4	.88	7.4	2.08	3.75	.75	2.6	.48
V.	4.2	.64	5.4	.88	7.8	1.04	5.75	1.75	3.4	.72

	B′.		E′.		F′.		General Average of all the Subjects.
I.	3.8	.64	5.6	1.52	5.4	.72	7.05
II.	3.8	.96	3.8	.64	4.6	.48	6.14
III.	4.0	.80	4.0	.40	5.2	.32	7.11
IV.	3.2	.64	3.2	.32	4.0	0	6.60
V.	4.2	.64	5.8	1.04	5.6	1.28	7.70
I.	2.6	.48	2.6	.72	3.4	.48	4.06
II.	2.8	.44	3.6	.48	3.2	.32	3.80
III.	3.0	.40	2.8	.64	4.2	.72	4.08
IV.	2.6	.48	2.2	.32	2.8	.32	3.63
V.	2.8	.44	2.8	.64	4.0	.80	4.61

The capitals A, B, C, etc., stand for the different subjects. The right hand column of figures under each subject shows his average variation.

The tables lettered B′, E′, F′ represent a second series of experiments performed with B, E, and F, respectively. They show that the same tendencies which were apparent after five weeks' practice also persist at the end of ten weeks. The above data are not, of course, extended enough to warrant very detailed conclusions, but they point on the whole toward the uniform preference for the two forms which offer both complexity and regularity. The fourth form, which gives the best combination of individuality with unity, was easiest for nearly every person. The introspective evidence of the subjects agreed very closely with their results. Without any suggestion from the operator the following points were volunteered: that the fourth form was easier because each syllable had a place of its own: that it was pleasanter to anticipate a series in that arrangement than to keep the eyes fixed on one point as in the first form.

2.

The second part of the experiment was arranged to compare (I.) the learning of syllables printed in black letters upon white paper with (II.) the learning of syllables printed in black but each upon a slip of different-colored paper. Thus a series

FIRST LEARNING.

	J.		K.		L.		M.		N.		General Average.
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	
I.	5.9	2.30	6.75	1.25	5.4	1.28	8.33	1.77	3.83	.83	6.04
II.	5.3	1.22	6.25	.87	4.4	1.52	8.33	1.33	4.16	.83	5.62

SECOND LEARNING—THREE DAYS LATER.

I.	2.7	.56	4.75	.75	2.6	.48	4.33	1.00	2.66	.50	3.40
II.	2.3	.62	4.00	.50	2.0	.0	3.16	.80	2.50	.50	2.79

would consist of *vax* on a blue slip, *sed* on yellow, *ver* on rose, etc. No color was ever associated with more than one syllable. The subject was not required to remember or even to notice the colors — they were simply there to help or hinder as they could.

The syllables in both these cases, I. and II., were shown in temporal succession but without spatial displacement.

Thus four out of five did their best work with the colored backgrounds. Here too the subjective evidence agrees. In several instances the color was definitely ascribed as the cue by which the syllable was recalled.

3.

In the third form of the experiment, the simple series I. was contrasted with a series II., which combined both space and color differences. The first set was shown at *x*, Fig. 2, and the second set down the line 1-2-3-4.

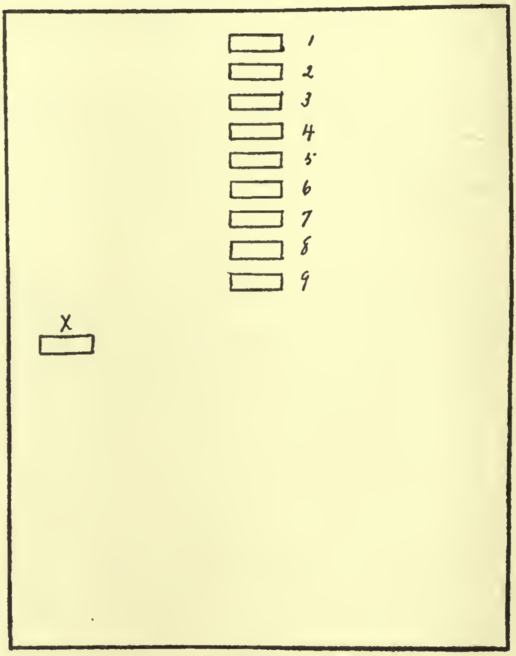

FIG. 2.

The results were as follows:

FIRST LEARNING.

I.	O.		P.		Q.		R.		S.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	15.0	2.8	3.6	.48	3.16	.55	7.0	2.00	3.83	.55
II.	12.4	2.48	3.2	.32	4.16	.28	6.2	1.44	3.83	.55

SECOND LEARNING.

I. II.	11.0 9.2	1.2 4.6	2.6 1.6	.88 .48	2.33 2.33	.55 .42	4.6 4.2	.88 .64	1.66 1.16	.53 .33

.	T.		U.		V.		W.		X.		General Average.
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	
I. II.	4.33 4.50	.66 1.00	5.2 5.2	.58 .84	2.66 3.00	.66 .33	5.5 4.2	.75 .62	7.0 6.0	2.0 1.5	5.728 5.274
I. II.	2.83 2.66	.55 .44	2.83 3.83	.94 .66	1.83 1.50	.27 .50	3.25 3.00	.37 0	4.25 3.25	.87 .75	3.709 3.264

The results of all three forms of the experiment indicate that the complex series has the advantage in each case. Not only is this true for the first acquisition, but it holds as well for the second learning. We should have, therefore, to modify somewhat an opinion which is often expressed, namely, that the more difficult a thing is to acquire in the first place the more readily it is regained at a later time. There must, of course, be an early limit to the arbitrary complications which can be introduced in the above way; for as soon as we take a content which cannot be immediately apprehended as a whole, we shall have a division of attention. 'Greater complexity' in a content does not mean a greater number of different objects in juxtaposition; it means greater variety in unity. A complex content is one which, while it represents many possibilities of distraction, *i. e.*, stands for much past experience, may yet be immediately taken in as a whole. Each member of a series, then, must have unity as well as variety, and when we reflect that the combinations used in the tests — a syllable, a color and a point in space — were purely arbitrary and perfectly new to the subjects, it seems somewhat remarkable that there should have been any uniformity at all in the results. The general conclusion to be derived from the three experiments is this: *that the most complex series of syllables* (the one with space and color) *was learned more easily than the simplest series; and that both elements in the complication* (as proved in part 1 of space, and in part 2 of color) *contributed to this result.*

II.

Attention as Determined by the Complexity of the Presented Content.

In order to examine the assumption that attention or interest is centered in a complex as opposed to a simple object, several tests were arranged in which a complex and a simple content bid against each other for attention. Experiments relevant to this situation have been made by Mrs. Hill.[1] Her method was this: two cards which first had like figures on them were laid before the subject and he was told to select one by taking it in his hand. Slight differences were then made in the two cards, *e. g.*, a small cross (x) was put in the corner of one card and again the subject made a choice. Her conclusion was that whatever attracted attention was most likely to determine choice in its favor. The general tendency was for attention to go to the card which had had the mark or color added to its other content. Münsterberg[2] has performed experiments to compare the judgment of time-intervals as dependent upon the 'filling' of those intervals. He found that in the case of two equal time-intervals that one was judged shorter which was filled with the more complex content.

The statement is frequently made that, when two fields compete for attention, the intricate or interesting one is preferred. The object of the following tests was to demonstrate as much in the instance of simple geometrical figures. The question as to which of two fields makes the stronger appeal to interest may be divided into two inquiries, namely, which attracts attention first, and which holds attention longer when it is attracted. The distinction is of course a purely practical one. The first point — the attraction of attention — was tested in this way. The subject was asked to fixate a spot marked midway between two simple designs and then to turn the eyes to either side he chose. In order that the first glance might be a real choice he was told that he might look at either picture but at *only one*. The cards were at such a distance apart that a vague impression of the figures could be got in peripheral vision when the

[1] *Am. Jour.*, Vol. 9, p. 587 ff.

[2] Psy. Rev., Vol. 1, pp. 51–56.

eyes were centered on the point between. A few tests were given to each subject in which the two figures were just alike, and in no case did there appear to be any noticeable preference for looking to the right or to the left. Tests were then given in which ten pairs of pictures were shown, each pair consisting of one simple and one somewhat more complicated figure, as in Fig. 3.

The more intricate designs appeared sometimes on the right and sometimes on the left, but not in any regular alternation or

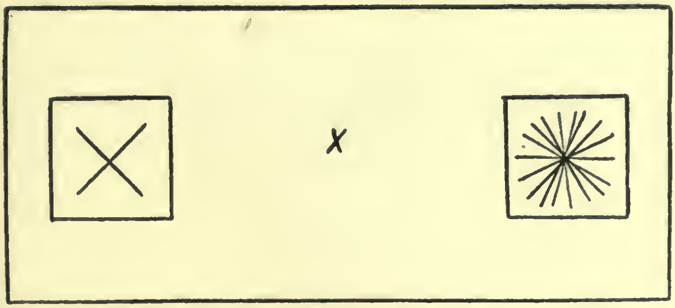

FIG. 3.

rhythm. The question then stood: out of these *ten* possible choices how many times would the subject be attracted by the complex figure?

Out of ten persons, nine chose the complex the great majority of times. Thus, *A* chose the complex nine times out of ten, or 90 per cent. of the time.

A.	*B.*	*C.*	*D.*	*E.*	*F.*	*G.*	*H.*	*I.*	*J.*
90%	100%	60%	90%	90%	90%	10%	100%	100%	100%

The introspective evidence was expressed in terms of 'interest' and 'curiosity.' The subjects were sometimes able during the few seconds of fixation on the mid-point to take in the simpler figure, and the tendency then was to look at the doubtful or indefinite one. The attraction was the indistinctness or the problematic character of the picture. One subject showed a marked exception to the others: *G* almost invariably looked at the simpler figure. His explanation was that the simpler one made a sharper contrast with the background.

2.

The second point — the holding of attention — was measured in this way. The subject was told to let the eyes oscillate between the two fields, lingering as long as he cared to on either side. These eye-movements were watched and recorded by the operator on a smoked drum, the time being kept by a seconds marker. There is a chance for considerable error in this method, inasmuch as the oscillations are often very rapid and some time is taken up by the operator in catching the eye-movements and responding to them upon the electric key which was used to make the records on the drum. The results represent the percentage of cases in which the complex object held attention for the greater part of a given time. Thus, *A* looked at ten pairs of pictures, and each time she looked longer at the complex one of the pair than at the simple one.

	Complex.	Middle Case.	Simple.	
A	100%			
B	60	30%	10%	Only nine tests made with
C	60	30		*C* and *D* instead of ten.
D	70	10	10	
E	100			

The left-hand column gives the percentage of cases (out of ten) in which the complex figure held attention longer; the middle column shows the percentage of cases in which the time was evenly divided between the simple and complex, and the third shows the times when the simpler design was dwelt on longer.

3.

A different plan was also used for trying whether attention would be held any longer by a more intricate geometrical figure. Drawings were taken such as are frequently employed to illustrate the fluctuations of attention, *e. g.*, the flight of steps. The subject was made to watch first the simple outlined book-cover *a*, Fig. 4, and to record, by pressing a rubber bulb, the changes in attention. He was next asked to look at a more complex diagram *b*, Fig. 4, and to register in the same way the fluctuations which these blocks sustained. The records were traced on a revolving drum, the pressure on the bulb sending

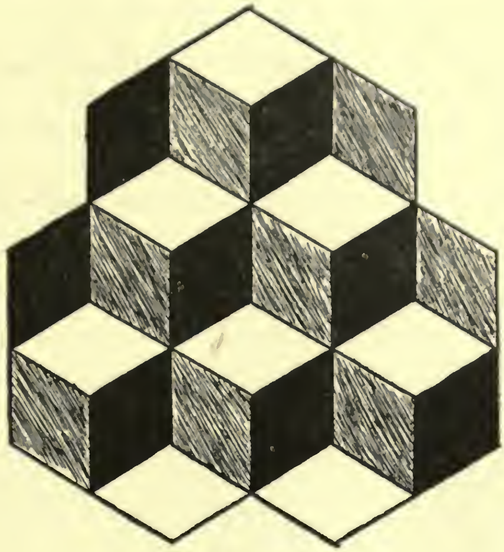

FIG. 4*a*.

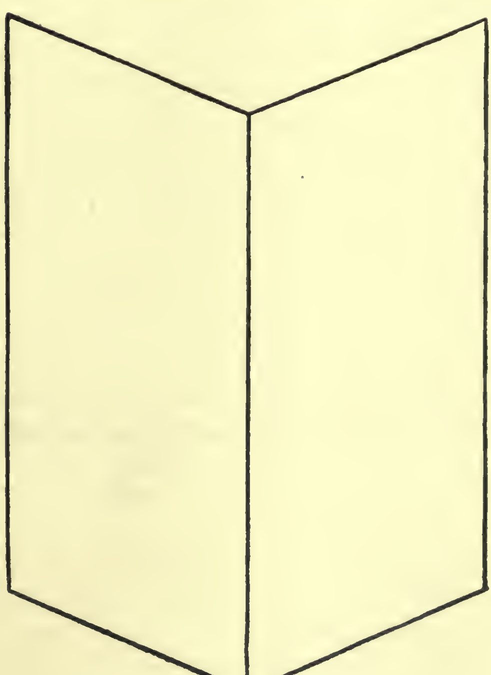

FIG. 4*b*.

the pointer up, and the rate of rotation was kept by a seconds marker so that the results had this form (Fig. 5).

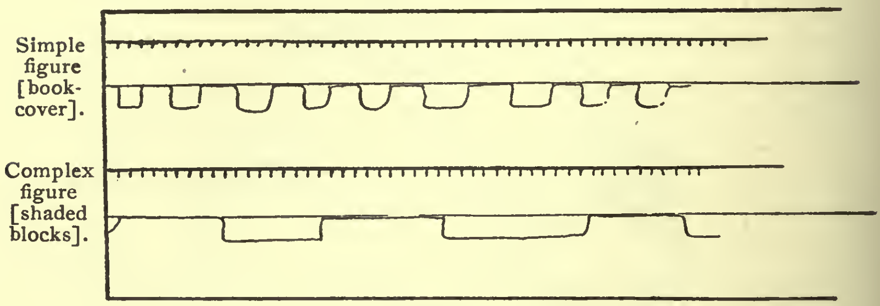

FIG. 5.

The numbers below stand for the number of fluctuations occurring within a given time, *e. g.*, to 100 changes with the simple figure there were only 50 with the complex, which means that the complex one held attention for twice as long as the simpler one did.

	A.	*B.*	*C.*	*D.*	*E.*	
Simple book cover.	100	100	100	100	100	Fluctuations.
Complex shaded blocks.	50	70	35	35	20	"

The actual number of seconds during which the pictures remained constant, was approximately 4, 5 or 6 for the complex, and 2 or 3 for the simple.

To restate the general point of view which the above experiments in memory and attention serve to illustrate, we may consider the relation of meaning as a principle of association to the two principles commonly advanced, namely contiguity and interest. A content has complexity or meaning according to the amount of past experience it represents, or, in proportion to the appeal it makes to habits of mind already fixed. Simple contiguity is the initial type of meaning — its lowest form — a mere chance mechanical juxtaposition: the connection between two things which have an intrinsic interest in each other, or 'necessary' bond, represents the highest or last form of meaning. These two conditions of memory are indispensable to one another. Two contiguous things would never be perceived as contiguous unless we *attended* to them together, had some slight degree of *interest* in them as together. On the other hand any two things which are intrinsically germane are by that very fact contiguous in consciousness. It seems, therefore, not inappropriate to call contiguity and interest the two extremes of the process by which significance is attained,[1] and to suppose that many repetitions of a thing in our progressive experience may transform the 'merely' contiguous into the 'conatively' or intrinsically contiguous. To a living organism repetition is never mere iteration; but the effects of successive presentations are cumulative. There is added, for example, the emotional element of familiarity. Or, beginning at the other end, we see that discrimination and association go inevitably together (two things held in mind as different are by that act associated), and hence anything which has many differentiæ — much meaning — has many associations, *i. e.*, has been present many times in past experience. The greater its intrinsic appeal, the more must an object have been *worked over* in experience.

On the structural side, then, meaning is contiguity often repeated; on the functional side, it is attention or interest. Repetition is the means to the acquirement of significance, and the fact of internal interest in a content stands for the significance acquired in the past. The two factors are complementary; for with richness of content fewer repetitions are needed to make a lasting impression, whereas a poor material must be repeated many times.

III.

An Investigation of the Process of Judgment as Involved in Estimating Distances.

BY M. L. ASHLEY.

I.

Nature of the Experiment.

It is a well-known psychological fact that our judgments are often due to habits or influences of which we are not at the

[1] Of interest in this connection are a few paragraphs by Stout on the 'acquirement of meaning,' 'Manual of Psy.,' Bk. I., Ch. II.

time fully aware. It is less generally recognized that we may mistake the factor upon which the judgment is really based, and least of all is it commonly supposed that our judgments may rest upon elements which we imagine ourselves to be consciously and definitely neglecting. That such is nevertheless the case is shown by the experiment we are about to describe.

The object of the experiment was twofold. In the first place, it was thought that, if by some means the visual and the tactual-motor factors, upon which we mainly rely in our estimates of depth, could be brought into conflict, the resulting judgments might serve to indicate the respective rôles of these factors in our spatial judgments formed under normal conditions.[1] In the second place, since the judgment would be formed under conflicting tendencies, one might reasonably expect to obtain certain data which would be of value in bringing to light in some degree the psychology of the judging process itself.[2]

In order that these purposes of the investigation might be accomplished, it was essential that the observer should be ignorant of the real situation and should suppose that an object which he reached for and touched was at the same distance from him as an object which he saw. In fact, the problem was to find what effect would be produced on one's judgments by a discrepancy, either recognized or unrecognized, between the visual and tactual-motor distances of objects whose distances were supposed to be the same. Indeed, in most of the experiments the object touched and the object seen were supposed to

[1] Somewhat similar investigations have been made to determine the comparative accuracy of touch and vision, as, *e.g.*, 'A Comparison of Sight and Touch,' by Bowditch and Southard, *Journal of Physiology*, III., pp. 232–245. 'The Perception of Space by Disparate Senses,' Jastrow, *Mind*, XI., p. 539 f. But these experiments were concerned with spatial estimates of the visual and tactual-motor factors when employed separately. Another related experiment is that of Stratton on 'Harmony of Touch and Sight,' *Mind* (N. S.), VIII., p. 492. In this case we have mainly, as in a number of similar investigations, the formation of new habits and coördinations, rather than the direct measuring of one factor against the other.

[2] It may be said of such experiments as K. Marbe's 'Experimentell-psychologische Untersuchungen über das Urtheil,' that they deal with cases of comparatively simple and direct reaction, in which conflicts are not clearly brought out.

be one and the same. To meet these requirements the following apparatus was constructed:

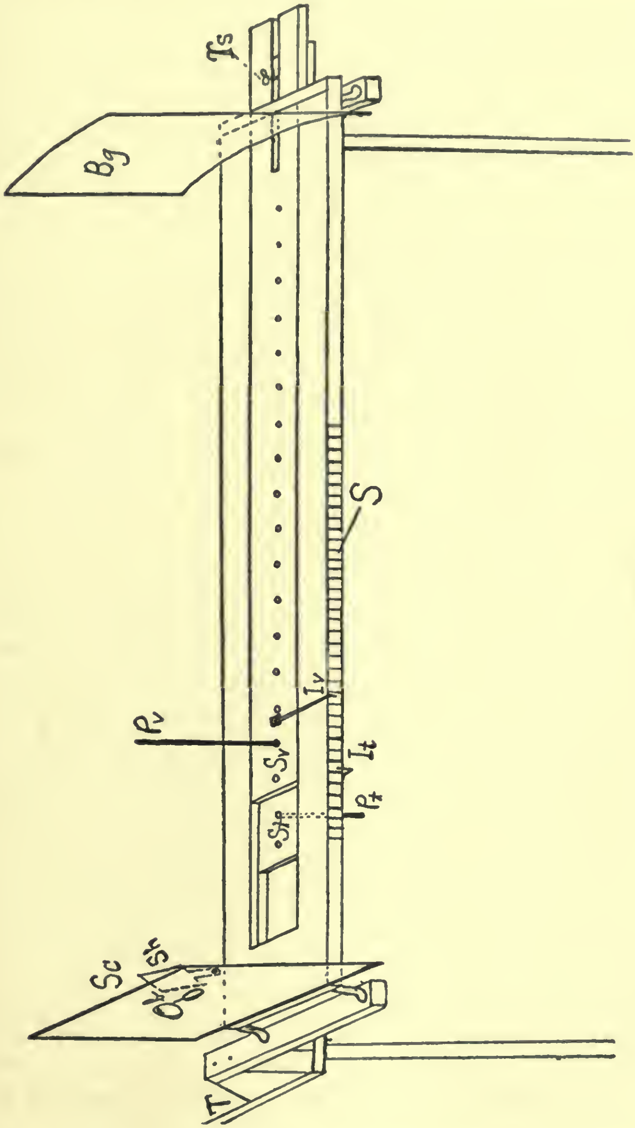

FIG. I.

Two narrow strips of hard wood were fitted one above the other in a groove in such a manner that they could slide back and forth either together or independently of each other. Small holes, two centimeters apart, were bored through the two strips, through which a long pin could be pushed, so that it could be seen from above and touched from below. Or, two pins could be used, one of which, *Pv*, would be carried by the upper slide *Sv*, and the other, *Pt*, by the lower slide *St*. *Ts* is a thumb-screw which passes through a slit in *Sv* and clamps *Sv* and *St* together when desirable. The framework with the slides in place is mounted on a table, *T*, in such a manner as to allow freedom for the arm in reaching. At the front is a screen *Sc*, with a narrow opening *Ob*, through which the observer looks with both eyes at *Pv*. *Ob* is provided with a shutter *Sh*. The upper slide is furnished with an index *Iv*, and the lower slide with an index *It*. These indices indicate on the millimeter scale *S* the distance either pin has moved. *Bg* is a black cloth back-ground. The arrangement of the apparatus in the diagram (Fig. 1) shows the lower pin the nearer of the two to the observer.

II.

Experimentation and Results.

In order to determine the degree in which one's spatial judgments could be influenced by the abnormal conditions employed, it was first necessary to find within what limits the movements of the pins could be detected under normal conditions. Since each pin could be moved separately, or both could be moved together, either forward or backward, six difference thresholds would be required for each observer for the distance which was to be employed as a reference point, or standard, from which the pins were to be moved. As a convenient standard distance a point on the scale was chosen and maintained throughout the experiments, which brought the visual pin forty-two centimeters from the eye of the observer.

To obtain the threshold for vision alone the pin *Pv* was exposed a sufficient length of time for the observer to get a clear impression of its location; then the shutter was closed, the pin moved and the shutter opened, about three seconds having been required to change the position of the pin. The observer then judged 'nearer,' 'further,' 'same,' or 'uncertain.' These judgments are indicated in the tables by *n*, *f*, *s*, and *x* respectively. To find the threshold for the tactual-motor side alone, the shutter was kept closed throughout. After reaching and touching the standard, the observer lowered his arm while the pin *Pt* was being moved. He then raised his arm and after reaching and touching the pin, judged as in the case of vision alone. Practically the same time interval was required for moving the pin as in case of vision. Where vision and touch-movement were combined, the observer saw and touched the pin at the same time. The shutter was then closed and he lowered his arm. The pin having been moved, he again raised his arm and the shutter was opened when he was about to touch the pin, in order that he might receive the tactual-motor and the visual impressions as nearly as possible at the same instant. The time occupied for the shifting of the position of the pin and the form of the judgments were the same as before.

The thresholds were determined mainly by the method of right and wrong cases, the amount of change which was correctly judged in seventy-five per cent. of the cases being taken as the threshold. The unit of change was one half of a centimeter for vision alone or for vision and touch combined; for the tactual-motor form alone the unit employed was one centimeter. During the determination of the thresholds as well as in the subsequent experiments, the observer sat at such a height that his line of vision fell upon the black background and well above the slides which carried the pins. This precaution was taken to prevent his receiving any assistance through comparison with other objects.

When two pins are employed, as in this experiment, there are ten ways in which they can be placed at unequal distances from the observer, who supposes he sees and touches the same pin, while in reality he touches one and sees another. (Only in the exposures employed as standards were the two pins actually at the same distance from him.) These ten positions for the pins are shown below. Suppose the observer to be located at the left of the pins. Where a pin has been moved, its position in the standard will be indicated by a dotted line. Arrows show the direction of movement from the standards which are figured directly above in each case. These varieties are arranged and numbered in the order in which they were meant to be given, though it was not found convenient to maintain this order strictly throughout the experiment.

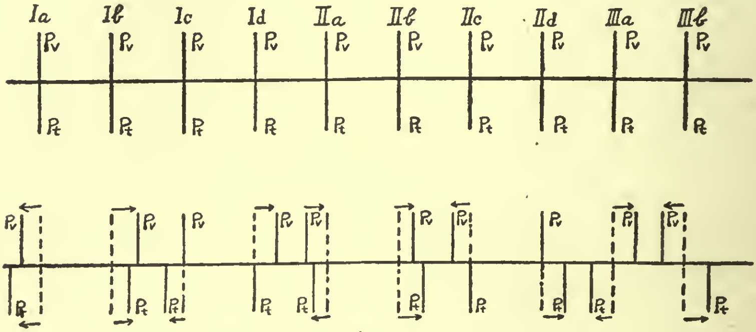

FIG. 2.

It will be noticed that in *Ia*, *Ib*, *Ic* and *Id*, *Pt* is always nearer to the observer than *Pv*, while in *IIa*, *IIb*, *IIc* and *IId*, *Pv* is nearer than *Pt*. In *IIIa* and *IIIb*, *Pv* and *Pt* have moved in opposite directions and their positions in *IIIa* are the reverse of those in *IIIb*.

Since the main object of the experiment was to study the conflicts brought about between the spatial impressions received from *Pv* and *Pt*, the combinations *Ia*, *Ib*, *IIa* and *IIb* were not given so much attention as the others and were omitted after a few trials. Accordingly, it will suffice for our purpose to show in tabulated form the results obtained under *Ic*, *Id*, *IIc*, *IId*, *IIIa* and *IIIb*. These results appear in the following tables, which are numbered in accordance with the previous description. The errors in the judgments will be more apparent if the threshold tests are compared with them, and readily to permit this a table of thresholds will be given first.

THRESHOLDS.

	A.	*B.*	*C.*	*D.*	*E.*
Pvn	1.5	2	1	1	.5
Pvf	2.5	4.5	1.5	3.5	1.5
Ptn	3	3	4	2	2
Ptf	2	2	3	2	2
P(tv)n	1.5	2	2	1	1
P(tv)f	1.5	3	1.5	2.5	1

Pvn, *Pvf*, *Ptn*, and *Ptf* signify that *Pv* or *Pt* has been moved nearer or farther away. *P(tv) n* and *P(tv) f* mean that hand and eye are combined and that the movement is nearer

Ic

Ptn.	*Pvs.*	*A.*	*B.*	*C.*	*D.*	*E.*
1	0	x	n	n	s	s
2	0	x	n	n	n	n
3	0	x	s	n	n	n
4	0	x	n	n	n	s
5	0	x	n	n	n	f
6	0	x	n	n	n	n
7	0	x	n	n	n	n
8	0		n	n	n	n
9	0		n		n	n
10	0		n		n	n
11	0				n	n

Id

Pts.	*Pvf.*	*A.*	*B.*	*C.*	*D.*	*E.*
0	.5	n	f	n	s	f
0	1	f	f	n	s	s
0	1.5	f	f	s	f	f
0	2	x	f	n	s	f
0	2.5	f	f	f	x	f
0	3	f	f	f	n	f
0	3.5	f	f	f	s	f
0	4	f	s	f	f	f
0	4.5	f	f	f	f	f
0	5	f	f	f	f	f
0	5.5	f	f	f	f	f
0	6	f	s	f	f	f
0	6.5		f	f	f	f
			f		f	f
			f		f	f
					f	f
					f	f

and further respectively. Throughout the tables the vertical columns headed *A*, *B*, *C*, *D*, and *E*, contain the results obtained from the respective observers. In the table for thresholds

IIc.

Pts.	*Pvn.*	*A.*	*B.*	*C.*	*D.*	*E.*
0	.5	x	s	n	n	s
0	1	n	s	n	s	n
0	1.5	n	s	n	s	n
0	2	n	s	n	x	n
0	2.5	n	s	n	n	n
0	3	n	s	n	n	n
0	3.5	n	n	n	n	n
0	4	n	s	n	n	n
0	4.5	n	n	n	n	n
0	5	n	n	n	n	n
0	5.5	n	n	n	n	n
0	6	n	n	n	n	n
0	6.5		n	n	x	n
0	7		f	n	n	n
0	7.5		n		n	n
0	8		n		n	n
0	8.5		n			

IId.

Ptf.	*Pvs.*	*A.*	*B.*	*C.*	*D.*	*E.*
1	0	x	s	n	s	n
2	0	x	s	f	f	f
3	0	x	f	f	s	f
4	0	x	n	f	f	f
5	0	x	f	f	f	f
6	0	x	s	f	f	f
7	0	x	f	f	f	s
8	0	x	f		f	f
9	0	x	f		f	f
10	0	x			f	f
11	0	f				
12	0	x				
13	0	x				
14	0	x				
15	0	x				

the extent of the required movement for each observer is given in centimeters. The letters *Pvn*, *Pvf*, etc., indicate the kind of change for which the threshold was obtained. In the tables

IIIa.

Ptn.	*Pvf.*	*A.*	*B.*	*C.*	*D.*	*E.*
.5	.5	x	s	n	f	n
1	1	x	s	x	f	f
1.5	1.5	f	s	f	x	f
2	2	x	s	f	f	s
2.5	2.5	f	n	f	s	f
3	3	f	s	f	f	f
3.5	3.5	f	n	f	x	n
4	4	f	n	f	f	f
4.5	4.5	f	n		n	f
5	5	f			f	n
5.5	5.5	f			n	f
6	6				n	f
6.5	6.5				s	f
7	7				n	f
7.5	7.5				n	f
8	8					
8.5	8.5					

IIIb.

Ptf.	*Pnv.*	*A.*	*B.*	*C.*	*D.*	*E.*
.5	.5	n	s	s	n	n
1	1	n	s	n	n	n
1.5	1.5	n	s	n	n	n
2	2	n	s	n	s	n
2.5	2.5	n	s	f	n	n
3	3	n	n	f	x	n
3.5	3.5	n	n	f	s	n
4	4	n	f	f	x	n
4.5	4.5	x	s	f	f	s
5	5		s		f	n
5.5	5.5				f	n
6	6				f	n
6.5	6.5				f	n
7	7				f	f
7.5	7.5				f	n
8	8					
8.5	8.5					

Ic, *Id*, *IIc*, *IId*, *IIIa* and *IIIb* the first two vertical columns headed *Pvn*, *Pvf*, etc., contain the extent in centimeters of that particular form of change which the observer judged 'nearer,' 'further,' etc., as the case might be. *Pvs* and *Pts* indicate that *Pv* and *Pt* were stationary.

We see in the table for thresholds that the lowest thresholds were obtained for *Pvn*, while for *Pvf* a much greater change was required.[1] The reverse was true in case of *Ptn* and *Ptf*. The thresholds where eye and hand were used together fall between those in which either eye or hand was used alone. It would appear that in this last case the poorer criterion was not discarded in favor of the better, but entered as a hindrance, and the result was really the combined effect of the two. It should be remarked in this connection that where the change was great, the eye and hand seemed to reinforce each other when the movement for each was in the same direction, and that in such cases there was a marked increase in the observer's assurance.

It was not possible to obtain the same number of judgments of each kind from all of the observers, because the nature of the experiment was such that, if at any time the observer had sus-

[1] This result is in general agreement with those obtained by Wundt, Arrer and others, though the thresholds are rather higher. M. Arrer, 'Ueber die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung. *Philos. Studien*, XIII., 116–161, 222–304.

pected that the pin he saw was not the same one he touched, his subsequent judgments would have been valueless. Accordingly, when the extent of movement had been sufficiently increased for marked perplexity to be manifest, it became necessary to change to some other form of the experiment. It would hardly be expected that all of the observers could be brought to the same point before recognizing the discrepancy between the two pins.

The tables show that the several observers, despite certain irregularities, were influenced in very different degrees by the visual and the tactual-motor factors. Thus, in table *Ic* (*Pt* nearer and *Pv* stationary) it appears that *A*, who is of the visual type, was in doubt when *Pt* alone had changed its position, while all of the other observers judged more in accordance with the movement of *Pt*. Again, in table *Id* (*Pt* stationary and *Pv* further), *A* judged 'further' quite regularly. In tables *IIc* (*Pt* stationary and *Pv* nearer) and *IId* (*Pt* further and *Pv* stationary), *A's* preference for the visual factor is clearly shown. In tables *IIIa* (*Pt* nearer and *Pv* further) and *IIIb* (*Pt* further and *Pv* nearer), *A* appears to have followed mainly the pin *Pv*. In tables *Ic*, *Id*, *IIc* and *IId*, *B*, *C*, *D* and *E* are shown to have judged for the most part according to the pin which really moved, whether it was *Pv* or *Pt*, though in tables *IIIa* and *IIIb*, *E* seems to have been influenced more by *Pv* than by *Pt*.

An examination of the tables shows further the results of conflicts which were brought about between the visual and the tactual-motor factors. The subjective effects of such conflicts were various. Sometimes the observer seemed to feel no difficulty and to be quite unaware that his grounds of judgment really conflicted. Sometimes he would experience considerable perplexity, but would be at a loss to account for it. In other instances he would assert that he judged by eye or hand almost exclusively, or, again, that he combined the two, while his results indicated clearly that in reality he did not recognize the bases on which his judgments rested.

To make this clearer we may refer to specific instances. In the case of *A's* judgments in table *Ic* (*Ptn* and *Pvs*) the effect of the tactual-motor factor is evident, though throughout the whole experiment *A* supposed her judgments to be almost wholly controlled by vision. At no time was the difficulty understood, though the perplexity and disappointment at not being able to tell whether the pin had moved or not was clearly shown in the tone of voice in which the judgments were given. It will be seen that in every instance reported in this table *A* was unable to detect any change in the position of the pins, though in some instances the pin *Pt* had actually moved more than twice the distance which could be detected in the threshold determinations in which *Pt* alone or both *Pt* and *Pv* approached (*Ptn* and *P*(*tv*)*n*). The same holds true in a still more marked degree in the experiment reported in table *IId*. Here, though previously able to detect a change of two or three centimeters, an increase of fifteen centimeters in the distance of *Pt* did not lead *A* to judge it further, when *Pv* was stationary. In tables *IIIa* (*Ptn* and *Pvf*) and *IIIb* (*Ptf* and *Pvn*) *A* mainly followed the visual factor, but the results of conflict appear in a few instances, particularly in the last judgment in table *IIIb*.

Though often erratic, *B* judged mainly by the hand. Throughout the tables his judgments are more irregular than *A's*. *B* was inclined to ignore the eye in cases of contradiction and realized, when the discrepancy was considerable, that the visual and the tactual-motor factors did not agree. Tables *IIIa* (*Ptn* and *Pvf*) and *IIIb* (*Ptf* and *Pvn*) give clear cases of conflict between the two elements, in which *B* seemed to follow now one and now the other.

Observer *C* said that his judgments were mainly of an immediate muscular sort wherever the tactual-motor factor was available. He could judge very well by the eye, he thought, but preferred the hand. As his judgments were given with great care, his results and comments are the more significant. Some of his last judgments given in table *Id* (*Pts* and *Pvf*) were ventured with considerable hesitation. Near the end of the series in table *IIc* (*Pts* and *Pvn*) he said that he was laying a good deal of stress on the hand, while, as the table shows, *Pt* was really stationary throughout. Thus, when *Pv* had approached as far as seven centimeters, he seemed to think it was *Pt* which had given him the main evidence of approach. After the last judgment in table *IIIa*, which was given as 'nearer'

(*Ptn* 4.5 cm. and *Pvf* 4.5 cm.), he said he was combining the two elements, and while he thought *vision* the more important factor, he did not disregard the hand. According to his threshold for *Ptn*, he would have confidently judged *Pt* alone to be nearer. At the end of the series of judgments given in table *IIIb* (*Ptf* and *Pvn*), *C* said he judged mainly by touch, and remarked that the tactual object seemed further than the visual one. It appears that at about the middle of the series, where, according to the thresholds, the conflict should have made itself felt, he changed from the visual to the tactual-motor factor as a basis for his judgments, but did not notice the discrepancy between the two pins till near the end. It will be noticed that at the end the pins were actually nine centimeters apart.

Observer *D* placed most reliance on the hand. She remarked once early in the series of table *Ic* (*Ptn* and *Pvs*) that the tactual object seemed nearer than the visual one, but she appeared to disregard this occurrence afterwards. Near the end of table *Id* (*Pts* and *Pvf*), *D* remarked, after having given a judgment 'further' with some hesitation, that the hesitation arose from using hand and eye together, since the visual estimate came more slowly; then, in reply to a question, *D* said that there was *no disagreement* and that *the same result would have been obtained from either alone.* These introspective observations were the more astonishing because *D* had but little faith in the eye compared with the hand as a means of estimating distance. In spite of the fact that *Pt* remained stationary, *D* said the visual estimate of 'further' came more slowly, that there was no disagreement, and that either alone would have given the same result. We have here a clear case of error as to the source from which the judgment was really obtained, and the ascribing to tactual-motor elements of an impression really originating in visual processes.

In table *IIc* (*Pts* and *Pvn*), in the case of the second of the judgments *x*, *D* said that the object as compared with the standard looked the same and felt further. *D* remarked, in addition, that both eye and hand were employed, and that it was not so hard to use both as it had been before. We see, therefore, the same sort of error that was noticed in table *Id* (*Pts* and *Pvf*).

In connection with the results given in *IId* (*Ptf* and *Pvs*), *D* said that in case of any apparent difference at first, the visual object was always made to agree with the tactual, but that the *same final result would have been attained with either alone.* In table *IIIa* (*Ptn* and *Pvf*), *D's* judgments are very irregular, and at the end vision and touch could not be made to agree. The same disagreement was experienced near the end of the series in table *IIIb* (*Ptf* and *Pvn*), though in table *IIIb* the tactual-motor side appears the more influential of the two.

In *E* we have a pronounced visual type. In *Ic* (*Ptn* and *Pvs*), *Id* (*Pts* and *Pvf*), *IIc* (*Pts* and *Pvn*), and *IId* (*Ptf* and *Pvs*) the hand did not appear to play much part and no disagreement was noticed. In table *IIIa* (*Ptn* and *Pvf*), however, this manner of judging seemed to be reversed in a few instances. At the end of another series like *IIIa* and of equal length (not appearing in the tables), in which the first judgment was 'nearer' and all the rest 'further,' *E* said that where the change was great, he tended to judge by means of the hand and had lately been doing so. It is seen, however, that while the pin was being judged further, *Pt* was constantly coming *nearer*. In the last judgment but one in table *IIIb* (*Ptf* and *Pvn*), in which *E* judged 'further,' he noticed the disagreement between the two objects.

In addition to the results which appear in the tables, a few others were obtained from the same observers, which did not differ materially from those tabulated. Other observers also took part in certain phases of the experiment with like general result. In one instance the observer, although he knew the nature of the apparatus, was misled into confidently judging the visual pin nearer, when only the tactual-motor one had approached.

While the experimentation was rather too limited in respect to both the number of observers and the number of judgments obtained to justify any sweeping generalizations, the results are not without significance in regard to certain aspects of our spatial judgments, and are at least suggestive as to the judging process itself. It appears that with persons of either pronounced visual or motor type the corresponding psychical factor tends in judgments of this kind to be emphasized to the neglect of the other in case they are combined. Whether ultimately the visual factor depends on the tactual-motor factor for its real significance, and whether, if cases of marked discrepancy between them were long continued, the visual would yield to the tactual-motor, remains a question. Such a view certainly has much in its favor, and it is not improbable that forms of experimentation could be devised which would go far towards deciding the matter.

The outcome of the experiment certainly adds evidence in support of the view that the bases of our judgments may be in a great measure subconscious, in the sense that they are not explicitly recognized. The introspective testimony given by the observers indicates that, as was mentioned at the beginning, the judgment may be the product of tendencies or habits which we not only do not ordinarily recognize, but which we can not correctly evaluate when conflict brings them to our notice. The most striking of the experiments show that we may be predominantly influenced by factors of which we are clearly conscious and which we definitely suppose ourselves to be disregarding. In fact, the judgment may apparently be viewed as the product of more or less conflicting habits, in which the form it finally takes is a sort of resultant of various forces; and the sharper the conflict among these forces, the greater may be one's sense of uncertainty and distrust as to his correctness.

I am under obligations to those who kindly gave their services as subjects in the experiment. For them the work must have been somewhat uninteresting, inasmuch as they were necessarily kept in ignorance of the real conditions under which they judged and of the real purpose of the investigation. I am indebted to Professor J. R. Angell, not only for various helpful suggestions, but also for considerable assistance in constructing and testing the apparatus.

DISCUSSION AND REPORTS.

AFTER IMAGES AND ALLIED PHENOMENA.

I have a frequent experience in connection with after images which it may be worth while to record, especially as others may be able to repeat or confirm my observations. What I have to remark is not the result of deliberate experiment at first, but is a spontaneous occurrence and what follows it is the consequence of experiment.

I have in my life often experimented with after images, and perhaps this fact makes me susceptible to them and to the observation of them when they occur without the effort to produce them. However this may be, I often notice an after image of a bright object in the field of vision when I am not trying to produce it. It of course arrests my attention and I immediately turn to observe it. As usual it quickly fades. I then try to reproduce the after image by experiment and as generally fail as I try. No amount of effort will reproduce it as before. I may obtain a faint one, but usually can obtain none at all. But the interesting phenomenon in connection with the spontaneous after image that arrests my attention is the fact that I have uniformly observed that it occurs only when I am in a state of abstraction. Thus if I am looking at a lamp or bright ring and at the same time not thinking of the object on which vision is actually fixed, the after image is almost certain to occur with great distinctness if I happen to turn the head to one side and the background is favorable. If I try to repeat the after image by looking purposely at the light, I utterly fail. The production of it seems to be related in some way to the connection between fixation and inattention. It may be worth studying in this connection the influence of attention upon the action of chemical forces in the retina. Of course something of this kind may already have been done, but if so it has not been my fortune to see it, as my studies have not enabled me to keep abreast with the scientific and physiological side of this matter. But the phenomenon which I have just described certainly suggests a possible relation between attention and the amount of chemical action in the retina.

There is another phenomenon which is possibly connected with related functions. When mentally preoccupied and having the eyes fixated on a given point or object I often notice the disappearance of a part of the indirect field of vision. I have tried to see whether it might not be due to the falling of the object on the blind spot, but uniformly discover that it is not, as the disappearing object may be on the side of the retina opposite the blind spot. On careful experiment and observation I find that the disappearance is directly related to the degree of abstraction, and that I can reproduce it artificially, if I am successful, as I sometimes am, in effecting the abstraction necessary and at the same time the proper adjustment of attention. It is difficult to produce the artificial abstraction required, but when I am successful I effect the disappearance of the object, which immediately reappears the moment attention is given to it without altering the fixation of the eyes. The effect seems to be that of making clear an actual impression, while attention in the previous experiment seems to destroy an after image. Why is this the case? I of course have no answer to this question. It is simply an interesting phenomenon to find the fact, which is apparently the converse of the first experience described. In the former, concentration of attention is conducive to the appearance of after images, and in the latter this concentration tends to extinguish real impressions. The latter may be a normal *retrécissement du champ visuel*, but why the former should not also illustrate the same fact is the phenomenon of interest. JAMES H. HYSLOP.

THE EARLY COLOR SENSE — FURTHER EXPERIMENTS.

In a discussion of the results arrived at in my article on the early color sense, PSYCHOLOGICAL REVIEW, Vol. X., No. 1, p. 37, Dr. Edridge-Green has said that the results are entirely due to the illuminosity of the experimental color cards and not to the colors themselves. His theory is that children are color-blind during the first years of life and that a child sees 'all objects as they are seen in a photograph, that is, in different degrees of black and white.' Also in Professor Chamberlain's comprehensive work entitled 'The Child,' in the Contemporary Science series, on page 79, we find that Garbini divides the development of the child according to the evolution of the color sense into six periods: "The fourth period being the 'Chromatic' period—from the sixteenth to the twenty-fourth month of life. The child continues to have more and more delicate photoæsthetic and visive perceptions and begins to have first chromatic perceptions — red and green." As all the experiments recorded in my article were concluded before the child was twelve months old, according to this theory also the results were due to some stimulus other than that of color.

In a discussion which followed a paper I read on this subject before the London branch of the British Child-Study Association, Dr. Edridge-Green said that children could not perceive colors till they could name them successfully and correctly. Allowing for an instant that my experiments give a result due entirely to illuminosity, they then show a wonderful power of remembering and discriminating between the various and often only slightly varying degrees of illuminosity. If the child's power of associating words heard with things seen is strong enough to enable him to name colors when first perceived, it must also be strong enough to enable him to associate the names of colors with the different degrees of illuminosity to which the results are said to be due. But it is the weakness of this power of associating words heard with things seen which spoils the color-sense experiments of Preyer, etc.

With the idea of finding out if illuminosity does play a very great part in obtaining the results by the methods I used, the following experiments were carried out. I obtained a dozen cards of different grays without gloss and laid them out on a table in a room previously arranged for this experiment. Then I took my yellow, blue and red cards and had the light gradually shut out of the room till one of the three colored cards could no longer be seen as such. Stopping the darkening of the room at this period I picked out the gray corresponding to it under these conditions. More light was shut out by my assistant till another was not visible as such, and a gray again matched with it. After I had got the colors matched in this manner my wife, who was assisting me, went through the matching process exactly as I did, and her results compared exactly with mine.

From the matched colors and grays I prepared test cards by cutting a piece about 1″ long by ½″ wide, of very irregular outline, and pasting it on the corresponding gray. If he were color blind to all colors, as assumed in theories mentioned in first part of this article, he should not be able to pick out this irregular patch of color on the gray. But as soon as I showed him a test card his eyes were immediately fixed on the colored patch, and he made an effort to pick it off. These experiments began on his 492d day (16 months old). On the 498th day I tested him in the same way, except that the colored pieces of paper were now only ⅜ of an inch square and stuck in various positions on the gray. Sometimes the color would be in the middle and sometimes in the corner of the gray card.

In each experiment he was immediately successful. No more was done in this way till the 616th day, when we had a new carpet laid in one of our rooms; this carpet had a pattern of such a nature that large pieces of it were of the same decided color. I cut some small squares from the colored papers nearly ½″ square, and placed them on the three principal colors while he was out of the room. When he came in I picked up one piece and told him to find some more. He started eagerly to look for them and as soon as he saw one would pick it up with delight. Altogether he picked up 12 pieces, being four different-colored papers on each of 3 different colors in carpet, in this way. This test occurred to me because I noticed that he would find the smallest object whenever it differed in color from the ground on which it rested, and of course sometimes the illuminosity of object and ground must be equal, thereby rendering the object invisible if theories quoted are correct. On the 617th day I carried out more similar tests, with the same certain results. Then I cut some elliptical-shaped colored papers (size, 4″ x 2½″) and pasted small triangular pieces (⅜ of an inch side) on them. I do not mention the colors used, as without seeing them it is hard to judge of their suitability, but it is open to others to check my results on other children. The difference of colors is not so important in these experiments, if the child sees the applied color each time a test card is offered, as it only requires a large number of combinations to make sure that the illuminosity of the applied color does agree with the illuminosity of the ground color in some cases. And if no failure is reported as long as the colors differ, it is a legitimate conclusion that the results are not due to the illuminosity but to the colors themselves. These last test cards gave the same result, and the child immediately saw the small piece of applied color. On the 618th day I used pieces of wool 1″ long, laying them on the carpet while he was absent. As before, I showed him what to find and he only had a difficulty in finding one piece. Though we had laid these pieces of wool down we ourselves had a difficulty in finding the piece that puzzled him for a time, the reason being that the colors were so much alike. He was 21 months old when these last experiments were finished, and I think it only needs some observer to experiment on the earlier months in this way to disprove more thoroughly than I have been able to do, the theories that suppose a child to be color blind.

I would suggest that cards could be prepared by printing small patches of color on different-colored grounds so that the experiments would cover a wide range. We all know how our color sense is ever developing; but should we know our children to be color blind we shall confess with sorrow that we have misunderstood them to a greater extent than we ever dreamed in our most abject moods. And we should feel that it must indeed be a colorless world for them.

RUFUS E. MARSDEN.

MENTAL IMAGERY.[1]

Dr. Slaughter's method was to 'ascertain as nearly as possible the exact behavior of the image during a certain interval of time which after trial was fixed at ten seconds.' Figures drawn on cards were used as stimuli. The subject was allowed to fix his gaze on the figure for an indefinite time. At a signal he closed his eyes. Five seconds later he was told by another signal to watch his imagery carefully and to remember the behavior of it, and, after ten seconds of such introspection, he recorded his results. For stimuli designed to evoke visual imagery various figures were used, such as a black square on a white card, with other geometrical forms, playing cards and printed letters. From these tests and from reports of blindfolded chess players he infers that the inner or imagery visual field is contracted, that only a small portion of what was really seen can be reproduced at any one moment in visual imagery. For the purpose of arousing motor images the subject was (supposedly — there is no definite statement) shown cards with, *e. g.*, a picture of a pendulum or of a circle with a ball on the edge. He was told to get a motor image of this, *i. e.*, to imagine the pendulum swinging and to imagine the ball as rolling around the circle. It is not clear from the article what he asked his subject to do, whether to visualize motion or, in some part of his body, to motilize (so to speak) the specified motion. His subjects here report an actual eye movement in themselves, but this is not either motor or motion imagery. Auditory images were called for by the presentation of a tuning fork, two tuning forks with a beat, slowly dripping water, quickly dripping water, a waterfall, ticking of a watch and whistling of wind. The dermal, gustatory and olfactory images studied were those of plush, clammy hand, hot water, plunge into cold water, the four tastes, and ammonia and alcohol. He states that in all the tests with the exception of the card series (that is presumably the playing cards) drawn figures were used instead of call words (p. 529). It is difficult for the present writer, who considers himself at least moderately skilled in mentally representing his objective experiences with considerable fidelity to the quality of the original sensation, to conceive how, *e. g.*, a clammy hand could be drawn on a card

[1] 'A Preliminary Study of the Behavior of Mental Images,' J. W. Slaughter, *Amer. Jour. Psych.*, October, 1902, pp. 526–549.

without the words 'clammy' or 'this hand is clammy' either printed or called; to say nothing of presenting as an auditory stimulus the line drawing of a waterfall or of two tuning forks with beats! The method used for the study of visual images consisted in presenting something to the actual vision of the subject for purposes of making test conditions and of having them as simple and definite as possible. These were then voluntarily recalled as visual images and their behavior described. The auditory images (as we learn from p. 541) were evoked by call words, but we must object here that if this is the case the study of his subjects' auditory images is not analogous to that of their visual images. The parallel to presenting real sights to the eye is presenting real sounds to the ear; and, in the case of motor and motion images, is the presentation of real motion, *i. e.*, putting his subjects in a swing or on a rocking horse or giving them a good shaking by the shoulders for motion imagery; for motor imagery getting them to do some unusual thing with their arms or legs; and, for olfactory, gustatory, and other images, giving them real sensations belonging to these different sense qualities and asking them on some subsequent occasion if they have observed any mental resuscitation of the originals. I say on some subsequent occasion, because the quality of some of the last-mentioned sensations is, in my own case at least, likely to remain as an after image or memory image (motor, *e. g.*, the rocking of a vessel experienced by one sometimes after disembarking, which has the quality of an after image: objectivity, non-subjective determination, etc.). If this research had been consistently carried out even with call words as stimuli, it would, to be strictly consistent, have thrown out the study of auditory images entirely.

The doubtful value of Dr. Slaughter's experiments is plainly shown in his discussion of his subjects' auditory introspections, *e. g.*, he says that his subject St. 'really has better auditory images than K. and is strongly motor while K. is strongly visual'; and 'when the object stands out complete except for the sound, and the whole situation is arranged so as to point to it, it may seem present as a matter of course whether it actually appears or not, and may seem as clearly distinguishable as any of the other qualities. It is similar to the case of the blind spot in the field of vision.' I think Dr. Slaughter has been misled here by a false analogy. A sense quality is a definite complete and always distinguishable mental phenomenon, and a subject if sufficiently trained in introspection can always say whether he has it or has it not, or feels doubtful about it. It is beyond the province of any one else to tell him that he has it not, when he says he has it.

If therefore the subjects of Dr. Slaughter say they had auditory imagery, he can hardly presume to say that they did not. He says he knows his subject St. had no auditory imagery because St. said he had the other kinds, *i. e.*, the presence of visual imagery is sufficient proof that the other kind did not exist, in spite of the statement of the subject that it did.

To examine in detail some of the verbal reports of the introspection of the subjects, let us take those concerning auditory imagery. After the call word (?) 'tuning fork,' subject St. "Felt tension in tongue but could not really get sound. Had sort of humming located in mouth fairly continuous. No visual image of fork. No kind of outside setting." The only words here that can, by any twist, be made to describe auditory imagery are the continuous humming; but he puts it in his mouth! The only way I can mouth the conception of humming is by means of motor imagery. True auditory humming is imaged in terms of pure sound, and as I wrote these words I mentally heard a boys' glee club humming a part of a song. There is evidently no clear statement on the part of St. that he has any auditory imagery at all, and yet St. is the subject who 'really has much better auditory images than K'! Let us follow St. through several other experiments. In 'Two tuning forks with beats' St. says, "Visual image of two forks. Oscillation of attention between forks that really seemed external and humming in the head. Beats were only rhythm put in by emphasis in humming. Visual image had no setting, seemed just in front in air. It fluctuated considerably." There may have been real auditory imagery here. To 'Slowly dripping water' subject St. says, "No visual imagery. Felt distinct movement in throat." This is real objectified sensation, and not imagery of any kind. "Rhythm intervals about a second long. Word 'drop' was repeated with the rhythm." No description of auditory imagery here! When he says 'drop' was repeated, he fails to tell us how. It might have been auditory; but if so, why did not he say so? It might have been motor, but he does not say that. For 'Quickly dripping water' he says: "Chief part of whole complex was movement in throat." To 'Waterfall' he says: "Visual image of waterfall and water falling over. Movement in throat muscles." And so on. No claim to have had auditory imagery either of ticking of watch or of whistling of wind, unless his statement about the wind that it was 'of whistling around corner of house but no visualization of house' may be taken as a description of auditory imagery. On the other hand, subject K., the inferior aurilizer, disclaims throat muscle sensations and visualizations in several instances and uses expressions descriptive of real auditory imagery such as 'got sound image,' 'got sound first,' 'sound image very distinct,' 'auditory fairly constant,' 'sound image first,' 'strong wheezy whistle,' etc., though he is constantly describing visual motor and verbal concomitants. Are these concomitants what Dr. Slaughter means by the 'behavior' of the auditory image? In that case, if a pure unaccompanied auditory image existed in his subjects, even for a moment, they would not have any behavior, and so would not come within the scope of his article. It seems, however, that this study of mental images should as stated (p. 526) be 'a study of particular images, if not in their relations, at least *in situ*,' but in the 21 introspections offered us as studies of auditory images, there seem to be only 11 real examples. These, to be sure, are studied *in situ*, or rather all except a few words of the reports are a description of the *situs* and not of the image itself.

Dr. Slaughter remarks on the auditory part of his study that "we are dealing with a vastly more complicated set of conditions than in any of the experiments [visual, motor, motion] previously considered. Images of a visual character are possessed of a certain degree of independence, and the conditions of their maintenance are chiefly, for introspection at least, to be found within themselves. Again, motor images, when taken in isolation, merely require a partial repetition of the original movement or impulse to that movement. But apparently in the case of auditory images, the conditions both of obtaining and holding them have to be brought in from outside. In other words, the study of auditory images is chiefly one of association, both of ideas and sense elements" (p. 543). To these remarks I should like to object: (1) that an auditory image is a psychical phenomena as simple and elemental as is the sensation of a real tone produced by a tuning fork, and (2) that its behavior is not its concomitants. These may or may not influence its maintenance in consciousness, but can not affect its character. Again, let me urge that if motor images, 'when taken in isolation' (where's the behavior gone?), require (for their maintenance, supposedly) a partial repetition of the original movement, then during the time occupied by their partial repetition, the image being comparatively so faint, can not be detected in the mental complex; and that it is tautologous to say that otherwise they require, in order to be maintained, the impulse to that movement, because the impulse differs from the image itself only in the added element of volition.

As to the possibility of subjecting mental images to experimental conditions in the way Dr. Slaughter attempts, I can not but dissent. Had the experiments been carried out for senses other than sight in a manner analogous to that for visual images, he would have had for instance to present a clammy hand to his subjects, let them hold it for some time, wait five seconds, think hard for ten more, and then verbally report their imagery. The results would have been one of three. Subjects would have felt clammy hand, *i. e.*, the imagined touch of it on their own palms or knuckles, as the case may be; or would not have felt it, or they would not have known whether they felt it or not. Any one of these is a direct simple answer, and any psychologist ought to be available as a subject for such research.

I should like to defend my own method as explained in my monograph, and to say that had it been used by him he would probably have found some others of the ten types mentioned by me. What I reported was what I mentally saw, heard, touched, felt, tasted and smelt in various qualities (all imagined, however). Dr. Slaughter says of my observations of my own imagery, 'The faultiness of the method is evident after a direct examination of the images.' I do not know whether he means by this (1) that an examination of the types of images I reported shows that my method of observing them was faulty, that the lack of experimental conditions surrounding the introspections was at fault, or (2) that my method of using these results was faulty (wherein I would with all humility agree with him). If it was the first, I can only say that having frequently caught myself paying close attention to the various imagery concomitant with silent reading, I allowed the reading to lapse, as it were, and entertained the fleeting images uninterruptedly, much as one reading at a window might lay aside his book and look out at a passing military procession. This I would continue for an average of seven and a half minutes, writing down any word that came into my head, that seemed to describe the image then occupying the foreground of my mental content, and ignoring the other images until they came to the fore. I wrote down words also that did not describe images but were part of the imagery. These were classed as verbal imagery (largely auditory).

The records show that, while I made no effort to detain any one image, the average life of any individual one was 8.86 seconds, or very near the time (10 seconds) which he found by trial to be the best. The real issue is whether the method of trying to think in terms of one sense or another according to stimuli presented by an experimenter is better than the method of passively observing the constitution of the stream of thought. I grant that the attempt to follow the lead of the experimentally administered and therefore controllable stimulus is more scientific in a narrow sense, but I doubt that any results can come from experiments of this nature, because his subject St.'s failure to get the desiderated auditory imagery in nearly 50 per cent. of the instances cited shows only that he did not get it that time and not that he could not mentally image a sound. It was probably scared away by the unnatural environment of the experiment. There is no proof of course that St. is not able to have auditory imagery. In fact he may be, as Dr. Slaughter says he is, a much better aurilizer than K. Just here, however, he let K. get ahead of him. It seems anomalous that one should offer as a contribution to the study of auditory imagery the statement that in half the cases there was none. And I should like to protest against this experimental method being substituted for mine with the unqualified remark that mine is faulty. I certainly collected specimens of the species of phenomenon I was studying.

On the whole, Dr. Slaughter's paper seems to me to be an example of not a few that have appeared of late in which the conditions of the experiment are not clearly described; and in the statement of the results the language is not clear, and gives the impression that there are more words than ideas behind them. For instance, in a paragraph on the direction of images he seems to be talking about visual images only, and, if so, it would have been better to say so; and the paragraph is so obscurely worded as to be almost unintelligible. When in the conclusion he says that 'the factors which keep visual images in clear consciousness are their own internal organization combined closely with motor elements,' what can be intended by the 'internal organization' of a visual image other than the image itself; and this is equivalent to saying that the factors that keep a visual image clear are the image and its motor elements; but it is hard to conceive what the motor elements of a visual element of consciousness may be. When he says 'auditory images appear only in connection with an organized associative situation,' what do these three words mean but images of other sense qualities? And if he means to say that auditory images appear only in connection with other images, he says what is not by any means proved; and he helps us not at all, for he does not tell us the nature of that connection. He can not make this statement as a conclusion from his experiments, as he has shown that some of his so-called auditory images were not auditory, but only concomitants, only an 'organized associative situation' mostly motor. When he says that 'images from other sense departments also require such a situation which is in most cases all that appears,' he seems to me to be guilty of a genuine Irish bull! That there is any doubt of the existence of images of other sense departments than visual, auditory and motor, I cannot accept for a moment. When Dr. Slaughter says that 'the real existence of these images is doubtful,' I understand him to mean their existence in other minds as well as in those of his subjects. He intimates as much when he says that 'our subjects failed to manifest such an elaborate equipment' (as the tactile, gustatory, olfactory, thermal, pain and organic imagery mentioned in my monograph), apparently throwing discredit on the truthfulness of my report of my own images. He says: 'One general question bearing upon the work' is 'whether the images obtained under introspective conditions are the same as the normal images of every-day life.' I can not conceive that the normal images can be studied in any other way save in 'introspective conditions,' and it is only in moments of voluntary or involuntary introspectiveness that we become aware that there are any 'normal working images' at all. So that there is no question as to the sameness of the images. The vital question is whether in subjecting or in trying to subject the flow of images to the control of scientific experiment we may not be changing the nature of these images. I believe that we can not change their sense quality. They remain constant in quality, but the machinery of scientific research may cause them to be obscured or driven below the threshold.

WILFRID LAY.

NEW YORK CITY.

ON LAUGHTER.[1]

A friend of mine once spoke of Sully's writings as sane, eminently sane, but dry. In this 'Essay on Laughter,' however, the reader is fairly deluged and swamped with lively adjectives and figures of speech. 'Blithely' tumbles merrily after 'sprightly' on every page, and 'floods of merriment' chase wildly the 'rillets of joy' of less significant dimensions. Irrelevances in conversation, we are informed, are among 'the recognized tributaries of the river of laughter,' and over all the imp of laughter shrieks his way with uncontrollable joy. Therefore we may say sane, eminently sane, but not dry.

This beautiful largesse of adjectives has, however, also a woful aspect. In the modern world of commerce a business manager would hardly accept a report spread out over 450 pages, a report which might easily have been compressed within 100 pages. I see no reason why the

[1] James Sully, 'An Essay on Laughter, its Forms, its Causes, its Development and its Value,' Longmans, Green & Co., New York, 1902, pp. 441.

psychologic public should not be treated in the same concise, business-like manner as is the head of any ordinary business firm or manufacturing plant. Although somewhat diffuse, excessively sprightly, and never deeply original, still Sully's treatment of the subject is by far the best yet published. It is therefore with diffidence and with deference that a few remarks on the subject are here ventured.

So far as ticklishness is concerned, a very important factor in the production of this feeling is undoubtedly that of the summation of stimuli. In a research of Stirling's carried on under Ludwig's direction [1] it was shown that reflex contractions only occur from repeated shocks to the nerve centers—that is, through summation of successive stimuli.

That this result is also due in some degree to an alternating increase in the sensibility of the various areas in question from altered supply of blood is reasonably certain. The connection of tickling with capillary pulsation is therefore worthy of investigation. As a consequence of this summation-process there could result in many cases and in cases of excessive nervous discharge the opposite of pleasure, namely, pain. This would result from long-continued stimulation or from light stimulation whenever the central nerve cells were possessed of little stability or inhibitory capacity, as in sickness, etc. A number of instances have been recorded of death resulting from tickling and there is no reason to doubt the truth of the statement that Simon de Montfort, during the persecution of the Albigenses, put some of them to death by tickling the soles of their feet with a feather. Mediæval justice and the hidden doings of the Inquisition might reveal many such instances if they were investigated. Lauder Brunton suggests that possibly the different effect of a slight stimulus like the touch of a feather, which causes intense reflex action, and of a gentle but steady pressure of the finger, which gives rise to no reflex action at all, may be due to the stimulation by the latter of two sets of nerves which counteract or inhibit each other.[2] It may be that the effect of steady pressure may cause a general diffused hyperæmia, whereas the stimulation resulting in the phenomena of tickling may and undoubtedly does cause a sudden convulsive hyperæmia which entails an explosive motor discharge. This relief of sudden congestion by additional stimulation of other and different nerve endings is observable in the relief afforded by rubbing or stroking a part which has been pinched or bruised, or by scratching an itching spot.

[1] Stirling, *Ludwig's Arbeiten*, 9ter Jahrgang, p. 290; *Sitz. Ber. d. k. Säch. Gesell. d. Wiss.*, Bd. XXVI., p. 439.

[2] Lauder Brunton, 'On Inhibition,' 'West Riding Asylum Reports,' 1874, p. 179, and *Nature*, 1883, Vol. XXVII.

An additional causal factor in the production of tickling may lie in the nature and structure of the nervous process involved in perception in general. According to certain histological researches of recent years[1] we know that between the sense organs and the central nervous system there exist closely connected chains of conductors or neurons, along which an impression received by a single sensory cell on the periphery is propagated avalanche-like through an increasing number of neurons until the brain is reached. If on the periphery a single cell is excited, the avalanche-like process continues until finally hundreds or thousands of nerve cells in the cortex are aroused to considerable activity. Golgi, Ramon y Cajal, Koelliker, Held, Retzius and others have demonstrated the histological basis of this law for vision, hearing and smell, and we may safely assume from the phenomena of tickling that the sense of touch is not lacking in a similar arrangement. The importance of this law, it may be incidentally remarked, is manifest at a glance, for a future science of education. The spread of all methods whereby first-hand information is gained, while empirically found to be eminently satisfactory, is now known to rest upon a scientific basis. The laboratory method, kindergarten and primary object lessons, and constructive work, the use of illustrations in text-books, magazines and newspapers, the stereopticon, etc., etc., may be cited as empirical recognition of this scientific fact.

May not a suggestion be offered with some plausibility, that even in ideal or representative tickling, where tickling results, say, from some one pointing a finger at the ticklish places, this avalanche-like process may be incited from central centers, thus producing, although in a modified degree, the pleasant phenomena in question? It would be in such a case another form of circular reaction.

Among the parts not mentioned by Sully as subject to ticklishness might also be mentioned the palate and the lips or any part rendered more or less sensitive, as in the case of sores. The palate, in many cases at least, may be tickled by having the tip of the tongue pass lightly backwards or forwards over its surface. In certain physical moods such ticklishness with me is almost unbearable. The reactions observable upon the recovery by a limb of its normal condition after having been 'asleep' are identical in some respects with certain phenomena of tickling. A German child remarked in my hearing that champagne 'schmeckte ebenso wie eingeschlafene Füsse.'

[1] Ramon y Cajal, 'Einige Hypothesen ¨ber den anatomischen Mechanismus der Ideenbildung, der Association und der Aufmerksamkeit,' *Archiv für Anatomie und Entwickelungsgeschichte*, Jahrgang 1895, pp. 367 ff.

In visual and auditory perception there may be induced some of the phenomena of tickling. A medical friend of mind informs me that certain notes in deep solemn music affect his epigastric muscles in a sort of shock reaction. The quivering can be induced by false notes at times. As to the deepest causal factor, I should say that tickling is the result of vaso-motor shock. In addition to these cases the phenomena of tickling may be autogenous in nature, that is to say, vaso-motor changes may be induced in the skin without apparent external stimulation. These changes are known at times to produce the phenomena of tickling.

If hypotheses are in order, I might suggest that as the attitude of disgust and dislike may be an incipient act of vomiting or the rejection of unpalatable food, so the smile may betoken an attitude of the whole organism in which the inception of food is the most striking characteristic. These actions which are obviously so useful in matters of food may have become in the course of social evolution associated with other affairs because of their eminently social symbolic value. The lower animals must perforce express themselves somewhat differently because, according to the testimony of comparative anatomists[1] they lack the necessary facial muscles for language and the smile.

The laugh may have another physiological *raison d'être* besides that mentioned by Darwin, Spencer and Sully, of relief of cerebral distension and congestion. Like singing it may be a therapeutic agency in reference to pulmonary exercise, blood-oxygenation and general bodily nutrition. The deep inspirations which the singer and laugher are compelled to make cause a distension of a number of air-vessels ordinarily in a condition of semi- or almost complete collapse. As a result of the laugh the circulation is hurried on through them and the lungs are developed to their fullest capacity. The well-developed lungs, by facilitating the process of oxygenation, favor the nutrition of the body in general. The laugh, it is true, causes 'a cessation of cerebral strain,' but the greatest relief is of pulmonary or vaso-motor origin. The sigh also possesses the same function, but the difference between the sigh and the laugh is the difference between work and play. As a general rule the play activities are more general and involve a greater amount of metabolism. The vitality of play is more intense. As singing has been recommended as a valuable adjunct in the treatment of anæmia and pithinoid chests, so laughter must not be denied its therapeutic and metabolic virtues. Deep inspirations favor

[1] Cited by A. H. Keane, 'Ethnology.'

the flow of blood through the lungs, from the right to the left side of the heart. Thus occasional sighs or laughs, or in other words deep inspirations, interrupt the shallow breathing constituting so-called 'breathless attention.' The shallow breathing leads to stagnation of blood in the right heart, and an occasional deep inspiration is necessary to relieve this. By holding the breath for a moment the stagnation of the blood in the right heart will provoke epigastric pulsation and cause the veins in the head and neck to swell. In cases of death from suffocation or drowning, the right heart is found engorged with blood. Now in most instances of witticism or in joking, although not in all, there is an element of expectation, suspense or inhibited function. The laugh is the rehabilitation of function, the rebound to increased metabolism. This may also explain the easily-excited laugh of those attendants at a funeral or solemn ceremony where the grief is not too intense. Any foolish stimulus may cause the metabolic rebound. A friend of mine once attended an execution. The morning sun was excluded, the shadow and damp of prison walls were everywhere, the usual crowd of curiosity-mongers was present. Upon regaining the open air and sunlight the major part of the crowd burst out laughing with no other external stimulus than the exuberant sunlight.

The mechanics of laughter would also have to take into account the important influence exercised by the diaphragm, the muscular walls of the stomach and glandular activity in the various degrees of the laugh.

Sully mentions the scratching of the head during a state of mental irritation as a well-known instance of the transference of expressive movements from one state of feeling to another, à la Darwin and Wundt. Lauder Brunton explains this habit of the English rustic and similar ones, such as pulling the mustache or beard, or the German habit of slapping the side of the nose with the finger, as a stimulation of some branches of the fifth nerve, thereby causing local dilatation of the cerebral vessels and an added ability to carry on a line of thought. In a similar manner the gustatory branches and the buccal branches of the fifth nerve are stimulated by taking something that has a strong taste, such as brandied cherries. In rural regions peppermint candy is the open sesame of wakefulness in this line. Sucking and chewing and sipping are stimulants greatly increasing the flow of blood through the carotids, as has been determined by experiment. Certain elements of the smoking habit have their *raison d'être* in activity of this sort. The habit many boys have of spitting on their hands and then of rubbing them together before taking a leap is based on the fact that thereby they obtain a sensori-muscular stimulation. Many mental and bodily automatisms usually explained by reference to some general principles such as inhibition ought to be reëxamined with the view of ascertaining the special causation in question.

Some theories die hard. Of no topic in psychology is this more true than in that of the psychology of the comic. In Sully's 'Essay on Laughter', and in the article on this subject by Hall and the present writer, may be found *in extenso* a collection of such metaphysical hard-ridden and hard-pressed definitions. Nor are Sully and other modern writers altogether free from blame in this respect. Miss Calkins in her 'Introduction to Psychology' says that 'virtually all theories of the comic agree in defining the sense of humor as enjoyment of an unessential incongruity' (p. 284). Sully says, "The most promising way of bringing the several laughable qualities and aspects of things under one descriptive head would seem to be to say that they all illustrate a presentation of something in the nature of a defect, a failure to satisfy some standard requirement, as that of law or custom, provided that it is small enough to be viewed as a harmless plaything" (p. 139).

It is a Ptolemaic pastime trying to discover the causes and inner essence of laughter in the objective world, or even for that matter in the world of mental presentations. In the treatment of the emotions no scientific grounds for causal explanation or classification can be found in the objects of the emotions; no more can such be found for laughter, one of the prominent forms of emotion. The real causal ground of laughter is to be found in physiologic processes. A person may laugh when tickled, may laugh from the influence of drugs, may laugh automatically without the presence of mental presentations, may laugh as an exhibition of *bien être*, may laugh at a button on his coat, may laugh when there is only one single presentation in the mental field or when there are two or more. Moreover, these external things are not laughable in themselves. It is our reaction which clothes them with the cloak of humor, gayety, or what-not. In this the comic follows the general law of all emotions, including also under that term the field of æsthetics. These emotional judgments are revelations and judgments of our own selves and characters, rather than of the mountains, sculpture, paintings, or so-called laughable things. Thus the cockney's account of his exploit at a fire (quoted from the London *Times*) would hardly be yours or mine: "'Jump, yer silly fool!' we shouted, 'we've got a sheet!' and he did jump, and there weren't no bloomin' sheet, and he broke 'is bloomin' neck. Larf! I thought I sh'd 'ave died o' larfin'." That which is high tragedy to the gods in the gallery may be comedy to the parquet, and *vice versâ*. "Avast thou wretch!" cries the demi-mondaine actress, "I would rather wear the filthy rags of poverty than don the imperial robes of sin." The artistic part of your nature laughs while your moral nature is full of pity; meanwhile there is joy in the 'nigger heaven' over another sinner repulsed.

Evidently the causal element lies in vaso-motor and nervous processes. The sense of joy present in the feeling of *bien être*, in the witticism, in the mild atmosphere of humor, is evidently due to vaso-motor phenomena and a discharge of surplus-stored energy where the discharge does not involve too much strain, effort or lesion. The laughter as a motor phenomenon may continue automatically, finally producing lesion and pain and in some cases death. In the more highly evolved form of this process, such as in wit, the element of suddenness is paramount, brought about by the coalescing of nervous currents seldom or never associated and by sudden vaso-motor and metabolic changes. In other words, we are dealing ultimately with mild forms of vaso-motor shock. Thus Dr. Edward E. Hale was taken when a boy to hear his father speak on a critical occasion. He was so impressed by hearing the orator cry: "Will any man dare say * * *" that he shouted from the gallery, "No, pa!" Neither of these elements taken by themselves are laughter-producing, neither can the ideas by themselves produce such a result, but the vaso-motor shock and sudden coalescence of nervous currents may excite by association the motor centers to intense activity. The same may be said of other such instances, as for example the following: A clerk in a book store upon being asked if he had William Watson's latest poem, 'The Eloping Angels.' "The Eloping Angels," he said, scratching his head; "No, I don't think so, but we have the sequel to it, 'The Heavenly Twins.'"

In other words, it is not an appeal to our sense of superiority, to our feeling for the ludicrous, to this feeling or to that; the enjoyment we call humor or wit is the result of vaso-motor and nervous changes. The objects of the humor or wit may be numberless, or rather co-extensive with one's experience, but the fundamental or underlying process will be the same. The concept incongruity may therefore be interpreted with more propriety as the unusual. These unusual coalitions of wit and laughter, however, may at times be eminently fitting or congruous.

The laughter induced by nitrogen monoxide or by cannabis indica is probably hyperæmic or congestive in its origin. The *Rausch* in all its forms, æsthetical, political, religious, spirituous, etc., ought also to be treated in this connection. Some psychologist with Atwater courage and with no reputation to lose ought to make a study of the possible individual and social utility of the *Rausch*. The savages, it is well known, induced this intoxication by various means.

Walter E. Roth, in describing certain songs of the northern Australian aborigines,[1] relates an interesting fact concerning the genesis of savage emotion. He says that, "while the songs are in progress, one, two or more men — any that like — will take into their mouths, chew and spit out again, the leaves of the 'stinging tree' (*Lapartea* sp.). What with the pain and irritation so produced, such an individual is speedily aroused into a state bordering on frenzy, when he will commence eating the human excreta prepared for the purpose, will both act and give expression to anything foul and bestial he can think of, do his best to insult everybody present, start chasing the women, and, rushing hither and thither, will finally fall to the ground completely exhausted and collapsed. The mental and physical pain to which the person is thus subjected may be gauged from the fact that it requires some few weeks before he is sufficiently recovered to resume his ordinary routine of daily duties.

A most pernicious doctrine rather prevalent in theories of æsthetics and play is that of self-illusion. One author even goes so far as to say, 'Make-believe, pretence, representation, are of the essence of play, mirth, and art.' It is a case where theory and half-baked analysis run blindly against the facts. The pretence or self-illusion is in the majority of cases quite as illusive as the grin of the Cheshire cat. Sully says 'play is free activity entered upon for its own sake' (p. 146). "Play ceases to be pure play just as soon as the end, for example conquest, begins to be regarded as a thing of consequence to the player" (p. 147). Karl Groos also makes much of this theory, saying, for instance, that 'joy in conquest' is the end of play combats ('Play of Animals,' pp. 291–2).

I do not deny that there are some play-activities into which there enter pretence and make-believe, but it may be remarked at the same time that such plays are very poor play. In studying the phenomena of play two standpoints must be strictly observed, namely, the subjective and the objective. Subjectively the player, if he plays in earn-

[1] Walter E. Roth, B.A., M.R.C.S., etc., in Bulletin No. 4, 'North Queensland Ethnography, Games, Sports and Amusements,' Brisbane, March, 1902, p. 22.

est, that is to say if it is the best type of play, resembles closely in his activity the so-called serious occupations of life. If it were not so it would not be a useful training for after-life. Play is in many ways modeled after social life and is *the* social life for the child. It is desperately real to him, and he wonders often why adults are living such a miserable, artificial life, making money and spending wearisome laborious days for ends which are hardly worth the while. The boy who 'monkeys' or 'fools' at practice games is warned off the field by the coach. No pretence or make-believe is wanted. They play and play to win something outside the play-impulse itself. My psychology students tell me that when they play on the football field all their psychological knowledge about play being a preparation for life drops away from them and they play to win their way to the goal line. It is a serious but withal a joyous occupation to them. Such psychological knowledge may injure to some extent the complete engrossment in these preparatory occupations. The true player drops the word preparatory and simply believes these activities to be serious occupations in which he is tremendously interested. The same is true of plays of a more youthful age. The doll, for the time, is a student in school, is sick, naughty, etc. The tin horse with fore legs longer than the hind legs has longer legs; these crass adults who talk differently are talking of another world of horses. The myths and legends of the child's world are very real worlds to him.

Objectively, of course, we look upon the activities of these early stages of growth as preparatory. To call it a world of pretence is to apply a misnomer and to judge poorly of the value of play. Adults are subject to selection, so also are these preliminary stages, but it ought to be called a propædeutical selection, one by the way not yet recognized by biologists, psychologists or sociologists.[1]

Miss McCracken,[2] in speaking of the poverty-stricken girls of the working classes of a certain city, says very aptly:

"In the first place they have gone to the theatre, and they go to the theatre to see the play; not the players, nor to see how they play the play, nor why they possibly play it thus, nor why they do not play it in some other way ('in any conceivable other way,' as I overheard

[1] A further discussion of this topic will appear shortly under the title of 'Propædeutical Selection.' See also the writer's article on 'Play' in the *University of Colorado Studies*, Vol. 1, No. 1, and Mr. H. A. Carr's paper on 'The Survival Values of Play' in the *Investigations of the Department of Psychology and Education of the University of Colorado*, Vol. I., No. 2.

[2] Elizabeth McCracken, 'The Play and the Gallery,' *Atlantic Monthly*, April, 1902.

a critic murmur at a recent Shakespearean revival), nor what the author of the play meant, nor what he did not mean, nor what he should have meant. They may see all these things; they frequently do see several of them; but they go to the theatre to see the play. It is interesting to remember that in Shakespeare's time the entire audience went to see the play."

Moreover, the only true criterion of play is the performance of an activity with ease and mastery and with the spirit of pleasure. All else is work or indifferently work or play. If this thesis is granted,[1] then play must not be confined to what we may call traditional forms of play, but must be extended even to adult occupations when performed with the spirit of pleasure and with ease and mastery. For these reasons laughter may be classed as a form of play.

One more point only in this discussion. H. M. Stanley[2] and Sully[3] suggest that teasing may well be taken as the starting point in the evolution of play. Taking merely traditional forms of play into account, this *a priori* statement seems to be hardly warranted by anthropological data. The hypothesis seems to underlie this statement that play is a single impulse, a faculty-of-the-mind affair, whereas it is simply protean in its concrete forms. But leaving this point aside, we can safely lay claim to some actual historical data. Buecher, in his 'Arbeit und Rythmus,'[4] clearly proves that many songs, dances, and early forms of literature had their origin in the work activities of early men. It is needless here to repeat the evidence adduced to prove the assertion. Then, again, many ancestral adult activities have been modified to suit childish needs; many present-day adult activities are modified in the same way. But farther back than all this we may go and say that play entered in those species in which parental care began to shield their plastic young from the incidence of natural selection. Then propædeutical selection entered, whereby the preliminary, introductory, educative activities and occupations suitable to the particular species in question survived building and moulding for the larger life of the adult. That joy accompanied such a process we can reasonably believe, taking as an analogy the exuberance and fullness of life of youth wherever we find it.

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[1] See articles quoted above.

[2] H. M. Stanley, discussion of paper by Hall and Allin on 'Tickling, Laughter, and the Comic, etc.,' PSYCHOLOGICAL REVIEW, 1899, p. 87.

[3] Sully, p. 184.

[4] Followed, and to some extent extended by Gummere, 'The Beginnings of Poetry.'

PSYCHOLOGICAL LITERATURE.

Vom Fühlen, Wollen und Denken. Eine psychologische Skizze. THEODOR LIPPS. Leipzig, Johann Ambrosius Barth. 1902.

This sketch treats primarily of the question of feeling, and of thinking and willing only as they are necessarily involved in the discussion of the former. After defining his general position in the introduction, the writer enters upon a comprehensive analysis of feeling, indicating incidentally his views on the apperceptive processes, and somewhat more fully on the relation of feeling to the processes of volition, with a short discussion of the related questions of value and right.

In general, the feelings as subjective are opposed to 'the objective and the qualities of objective experience.' "It is the same whether I say 'I have a sensation' or 'I have a sensation of something objective,' or whether I say 'I feel' or 'I feel myself.' The whole content of sensation constitutes the perceptual picture of the objective world, just as the feelings constitute the 'I' as it is in every moment of my life immediately experienced" (p. 2).

The view of Jodl, Titchener and others, who maintain that there are only two kinds of feeling, pleasant and unpleasant, is combated; and feelings are defined as the immediate conscious indications of the relation in which individual psychic processes or events stand to the whole continuum of mental life (psychischer Lebenszusammenhang) (p. 5). The manifoldness of the feelings is just as great as the number of these relations, which is practically countless. The main part of the discussion is a development of this position.

Three fundamental opposites between feelings are first sketched: (1) Feelings of simple apprehension and apperceptive feelings;[1] the former indicates the relation in which an individual event stands to the whole apprehended content of consciousness, the latter the relation in which an event stands to the group of processes which constitutes the apperceived content of consciousness. (2) Presentative and apperceptive feelings of objectivity, and feelings of apprehension and apperception; the distinction in this second pair is based on the fact that all presentations and apperceptions are of objects, *i. e.*, on the one hand

[1] 'Perceptive und apperceptive Gefühlen,' p. 8.

they are presented objects, on the other they are subjective experiences. This gives rise to the double question: How do objects on the one hand, and their apprehension and apperception on the other, stand in relation to me? (3) Further, the content of my experience is determined on the one hand through my own instrumentality, on the other by objects. This is the basis of the third pair of opposites, viz., the feeling of freedom and the feeling of being determined.

The feeling of objectivity is again divided into subjective objectivity and objective objectivity, the former referring to the feeling which characterizes the simple apprehension of an object by which I find myself creating that object, the latter to those cases in which I find myself conditioned by an object; *i. e.*, there is as it were a claim put forth by the object in its own right to be perceived and I feel myself conscious of that claim. This latter, which is called 'gegenständliche Objectivitätsbewusstsein' (p. 11), is the feeling of reality.

There is a distinction made in the apperceptive processes, analogous to the distinction of freedom and determination in the way in which objects are apprehended. Corresponding to freedom of apprehension is active apperception, in which I turn my attention voluntarily to the object. Opposed to this is passive apperception, in which the object draws my attention to itself.

Returning again to the fundamental feeling, that is, to the general 'I-feeling' (Ich-gefühl), a distinction is drawn between the feeling which characterizes the state in which the attention of the subject is fixedly directed to a certain object, and that which belongs to the state in which the attention of the subject is passing from one object to another, *i. e.*, between what might be called relatively the states of rest and motion. The feeling of motion or change may be called 'Streben, Begehren, Verlangen, Erwarten, Sehnen, sich Besinnen, Wollen, Fürchten, Hoffen' (p. 19). Lipps adopts Streben (conation) to represent in general what any one of these terms might signify. This conation always involves striving towards some end, and meeting the resistance which is opposed to that process. Since in every psychic event there is a tendency towards an end, and resistance to be overcome in its progress, so every psychic event has more or less the character of conation. "And if the capacity to strive be called 'will,' using that word in its widest significance, this means nothing more than the possibility that a mental event may occur, and that resistance will be encountered in the normal progress of the event" (p. 23).

There are two pairs of fundamental opposites in the feelings of conation, based on the general distinctions drawn above: First the feelings of subjective and objective conation; second, the feelings of active and passive conation. Active conation is my striving towards some end; aside from this is a striving 'in me,' not properly 'my striving,' *i. e.*, passive conation. This activity feeling is based on the *interest* which is at the time dominant in me.

By interest, in a general sense, is understood: "All that helps a psychic event to be apperceived — or all the factors of the psychic 'energy' of a process, *i. e.*, there must be something in me which gives an object the capability of affecting me" (p. 31). In a more particular sense, I can speak of my interests, those which direct active conation, and the interests not in this special sense mine, which support passive conation. My interests are my personality, which is the effective factor in controlling the direction of my apperceptive activity. All feelings of pleasure and value depend ultimately on the coöperation of my interests in the apperception of an object. The present ruling positive interest is in a special sense the personality, within the wider total personality. What happens aside from that which issues under the direction of this special personality is only passively mine.

The basis of the feeling of reality has already been indicated, and its development may be passed with the remark that 'the tendency in the object to appear real is at the same time my tendency to have the object appear real to me' (p. 81). Another group of feelings depends on associative preparation, *i. e.*, association has built up an expectation of what will occur under given circumstances. If a strange intrusion occur it excites the feeling of surprise, its unexpected greatness the feeling of astonishment, its suddenness the feeling of fright, etc.

The striving after knowledge, just like all other conation, arises from the accession of a subjective interest to an objective tendency. This is conditioned objectively by logical contradiction, and subjectively by my attribution of a positive value (positives Wertinteresse) to the solution of the contradiction, *i. e.*, the persistence of the contradiction contradicts, its solution corresponds to the nature of my mind. Volition is in a general sense active conation.

"The feelings of pleasantness and unpleasantness are a coloring which all feelings may take on" (p. 141). Pleasure is an immediate conscious symptom that a psychic event finds favorable conditions for its apperception, in the nature of the mind. These feelings are dependent upon two conditions: (1) The readiness in the nature of the mind to satisfy the demands which a process makes on the apperceptive activity; (2) that this readiness have an adequate opportunity to exercise itself. Whatever satisfies these conditions has the characteristic of pleasantness, what transgresses them that of unpleasantness.

Approaching the ethical feelings, self-condemnation and pride are explained by reference to the nature of the structure of the personality. The 'I' is made up of an indefinite number of 'I's,' representing the passing moments of my experience. They are relatively self-dependent, but can be condensed into a single personality like so many layers of a whole. When the I of the present moment does something opposed to the expression of the total personality, there arises the feeling of self-condemnation. The development of the total personality follows the same principle as the development of the knowledge of the laws of nature, *i. e.*, modification in accord with the discovery of new facts. The feeling of oughtness (Sollen) is conation, with the character of objectivity, *i. e.*, a striving which has an objective basis, on the one hand in the claim of the object, and on the other in the positive attribution of value by the total personality. The ideal total personality is reached when all the possible experiences of the self and others, which might make revision necessary, have been added to the individual personality. The individual total-personality stands in relation to this ideal complete-personality, as the 'I' of the present stands to the total 'I.' "I have the highest feeling, *i. e.*, the feeling of unity of the ideal and the present personality, in short, the feeling of moral freedom, when my present personality is directed to the same as that which the ideal personality demands of me" (p. 196).

The name of Professor Lipps to a work on the psychology of feeling is sufficient to justify one in large expectations, and the book sketched above, though in some respects it can scarcely be considered final, amply rewards a careful study. A specially valuable characteristic of the treatment is that, without wandering afield to discuss a legion of theories, it comes directly to the point, and develops its theme logically throughout, bringing the whole into one perspective, so that the different phases appear in a definite relation to one another. Specially valuable is also his treatment of the relation of feeling to will. To say that the terminology is precisely defined, and then used consistently throughout, is only what one would expect from the author, but it is worthy of mention on account of its being a virtue so often wanting.

Professor Lipps has the failing, however, of introducing new expressions to indicate situations for which familiar terminology is quite adequate, and its use would have saved the necessary explanations, and occasionally would have left a clearer apprehension of what is expressed. To cite a single instance: what more is contained in the expression, 'gegenständliche Objectivitätsbewusstsein' (p. 11) than we understand by sense perception, and by the German Wahrnehmung? His chief failing is perhaps the use of terms which belong to presentational processes, to indicate different kinds of feelings, *e. g.*, feelings of apprehension, and of apperception. What more do they explain as to the actual nature of the feelings, than would be expressed by speaking of a green feeling or a white feeling? His varieties of feelings are in a number of cases indefinite just on this account, and would bear a closer analysis. And if, as is stated, 'the feelings of pleasantness and unpleasantness are a coloring which all feelings may take on' (*loc. cit.*), one might ask whether a further analysis would not indicate that the theory combated in the introduction had received a rather too summary treatment. One regrets too that Professor Lipps, in the development of his theory, makes no reference whatever to the value of the work which has been done in the investigation of the physical expression of states of feeling.

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Histoire et solution des problèmes métaphysiques. CHARLES RENOUVIER. Paris, Alcan. 1901. Pp. 477.

The problems of metaphysics and their solution in the French 'neocritical' philosophy were treated in their logical, systematic relations, with only such illustrative historical material as seemed necessary, in the author's previous volume, 'Les dilemmes de la métaphysique pure' (see PSY. REV., IX., 80). In the present volume this historical material is elaborated by a review and criticism of the chief systems of metaphysics as they have successively arisen in what, for want of a better term, we are accustomed to call the history of philosophy. The work, accordingly, is of the nature of a critical history of philosophy; it begins with the conception of the world in remote antiquity and comes down by the way of the great speculative constructions of ancient and modern times to the neocriticism in which the efforts to solve the metaphysical problems are held to reach, at length, a rational and satisfactory conclusion. The author, however, is very far from considering that philosophy moved towards this conclusion, as to a predetermined end, by any inherent necessity. His view of the history of philosophy is the very opposite of Hegel's. The history of philosophy is for him not the self-development of a system of thought, but the empirical succession, with, to be sure, more or less external continuity and logical connections, of warring systems, systems which, as one of his disciples (L. Dauriac) has said, can be classified, like living forms, into different and antagonistic varieties. In treating of the history of philosophy from this point of view Renouvier's chief, one might say his only, concern is with the validity of the conceptions considered. His criticism, therefore, is not what is called 'objective' and historical, as though the historical process furnished its own self-criticism, but logical and pragmatic. He is interested, above all things, in the exposition and defense of his system of thought. Hence his strictures are mainly directed to the exhibition of the inherent vice, as he conceives it, in most other systems, namely, the realization of abstractions combined with insufficiency and contradiction in the grasp and application of fundamental logical principles, defects from which his own philosophy, he claims, is free. This dogmatic interest makes him an unsafe guide on the historical side of his work; his criticisms too often lack the support of textual citations, and the student, in whom an extensive and thorough knowledge of the material is already presupposed, will find much to disagree with in Renouvier's representations. Yet, as Renouvier is unquestionably a man of genius, his remarks are always suggestive and at times profoundly penetrating; they stimulate reflection on the subject in hand even when, perhaps oftenest when, they awaken disagreement or doubt with regard to their historical accuracy. Take, for example, the following concerning Hegel: Both in Hegel and in the Neoplatonists, says Renouvier, speculation ends in the sacrifice of the individual; so that in this respect, Hegel would be reckoned in the Neoplatonic camp 'but for the fact that for him the universal is an abstraction doubly dead, wherein it is the destiny of the individual to be absorbed *without transformation into the divine*' (p. 147). Or this on Fichte: 'In the doctrine of Fichte, whose *subjective idealism* appears at first sight to have nothing to do with nature, we have to recognize in effect a sort of history of the universality of phenomena referred to their principle, which is called, but is not, the ego,' it being, as Renouvier goes on to explain, 'the noumenon of a universal ego, as if this logical essence could exist outside of positive consciousness, a person, God or man' (p. 356). Or, again, this on Kant: 'The whole Kantian doctrine of the *pure reason* tends to views concerning the deity and nature opposed to theism' (p. 338).

Probably the most valuable part of the book to a student of the history of philosophy is that in which the author takes stock of the present condition of philosophy in France (Livre X.). Among other things, as, *e. g.*, an incisive characterization of the intellectual and sentimental temper exemplified by Renan (p. 425), it contains Renouvier's most complete account of the genesis and historical affiliations of neocriticism, particularly in its relation to Hume and Kant, to Leibniz and Descartes. This and the corresponding chapter in the *Dilemmes* contain precious material for the future historian of the philosophy of our time. And it would be quite in the order of the day, when pragmatism and pluralism are in the air and scientific monism and German trancendentalism are alike being put on the defensive by insistent claims of individuality and personal will, for some bright candidate for the doctorate in one of our universities to turn aside for once from the elucidation of the everlasting Kant or the resuscitation of some obscure and well-forgotten ancient and give attention to these Renouverian books, with a view to a critical account and estimate of this aggressive system of phenomenalism conjoined with monadism, of critical rationalism united with rational belief. Whoever attempts this task will find a multitude of problems on his hands, not the least being one of great interest to the theoretical psychologist, namely, the determination of the conception of the soul as the law of phenomena having for its function perpetuity in time and such an original liberty of action as implies real contingency in the objective order of events (pp. 451, 460 ff.).

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ETHICS.

La Morale : Fondement psycho-sociologique d'une conduite rationelle. G. L. Duprat. Bibliothèque International de Psychologie Expérimental. Paris, Octave Doin. Pp. 385.

M. Duprat conceives of ethics, not as a science, but as an art, though an art founded on a scientific study of facts. Having satisfied himself at the outset of his inquiry, that duty essentially demands the most perfect possible coördination of all functions, alike within individuals and among the individuals composing society, he concludes that the chief task of the ethical investigator is to study psychic and sociological activities, for the purpose of detecting defects of coördination, and constructing the individual and social ideals of perfect coördination. "If we discover [constatons] incompatible tendencies, vices, defects and excesses that mar the harmony of the whole and the coördination of individual or collective functions, it is our duty [as ethical investigators] to indicate what should be suppressed, what should be developed, what created, in order that the system may become at once the richest and the most harmonious possible" (p. 57).

With regard to this program two chief doubts suggest themselves. It may well be asked whether the welfare, the real and genuine interest, of the conscious living beings making up society is not of more fundamental moral importance than anything so purely abstract as coördination. Order, coördination, organization, is indeed indispensable for the promotion of the welfare of beings living in close contact with one another, but the intrinsic value of mere arrangement seems to be æsthetic rather than ethical.

And, again, when ethics undertakes to diagnose the ills of the social body, to prescribe remedies, and to set up the ideal of social health, as Dr. Duprat says it should, the particularity of this undertaking may well render it precarious. To be sure, M. Duprat prepares himself for his difficult task by careful studies of the conditions of individual and social action, as set forth by psychology and sociology, and of the trend of social evolution, as set forth by the latter science, upon which he leans heavily. But it still remains true that personal conviction is largely responsible for Professor Duprat's construction of the individual and social ideals offered as the *summum bonum*.

In short, the author, in seeking to work out an account of morality on the theory that ethics is an art, commits himself to undue dependence on knack, skill, insight, and other subconscious, untestable procedures for the attainment of his results. At least so it seems to the present writer, and so, he believes, it will seem to the careful reader of Dr. Duprat's book.

But it should also be pointed out that the book discusses in an interesting way family, industrial, political and educational ideals, considering, among other live problems, ideal family relations, relations of labor and capital, scope of governmental functions, and coeducation.

S. E. MEZES.

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Der aesthetische Genuss. KARL GROOS, Professor der Philosophie an der Universität Giessen. J. Rickersche Verlagsbuchhandlung, Giessen, 1902.

This new work appears in the place of a second edition of the author's earlier 'Einleitung in die Aesthetik.' It differs, however, in so many important respects from what the author calls his Jugendversuch, as to amount practically to a new piece of work. In the first place, the author confines himself to a study of the more general conditions of æsthetic enjoyment and appreciation, reserving for a later work his chapters on the 'modifications of the æsthetic,' and for a still later treatise his study of the processes of æsthetic creation. But in addition to this difference in scope there is a noticeable difference in method. The psychological standpoint is more rigidly observed, and there is a decided redistribution of emphasis, a toning down of those aspects of his earlier work which were distinctively his own and a tendency to take account critically of all the recent contributions to the subject, thus making the later work less individual and more representative.

The most noticeable redistribution of emphasis appears in connection with the rôle given to the concepts of play, inner imitation and illusion. The difference may perhaps be best expressed by the statement that, while these functional attitudes are still conceived to be fundamental as distinguishing the æsthetic from the non-æsthetic, the pleasure values connected with these activities are not given the important weight which they received in the earlier work. On the other hand, much more emphasis is put upon the content side, upon the pleasure values or effectiveness of the direct and reproductive factors, sensational and ideal. The proportionally large number of pages given to the detailed treatment of the sensational and reproductive factors in æsthetic effect, their fusions and colligations, is significant of this change of method.

In his treatment of the direct or sensational factor a large place is given to the muscular and organic sensations, leading even to the claim that 'the specifically æsthetic disposition presupposes a strong motor type, and that with the reduction of the motor elements, there is reduction of æsthetic effect.' In treating of the sensational factor he also makes an important distinction between the *pleasant* and the *intensive* effects, making a basis for such distinctions as that between the beautiful and the sublime. The intensive effects are largely the product of the fusion of intense organic sensations with other content. Although on this point his views are not so definite as one might wish, it is here that the emotional values of the work of art are to be found, it would seem. The chapter on the reproductive factors in æsthetic effect describes the most important conditions of the Verwachsung (this is the author's own term) of sensational and associative factors, and the most important forms of this interpenetration of the two elements. The sphere of associated content is widened to include feelings and emotions, but here again the lack of clear definition of the emotional element is felt.

The play impulse, inner imitation, or voluntary surrender to the play of impressions, and the state of illusion, are still, as we have indicated, fundamental criteria of the æsthetic, but the æsthetic value is conceived to lie not so much in a distinct pleasure value arising from these activities, as in the intensification through them of the pleasure values arising from the greater fusion of the sensational and reproductive factors. The activity of play does indeed have its pleasure value (p. 112) as illusion has its pleasure value, independent of the values of the sensational and reproductive factors; but 'the chief value of illusion (p. 228) lies in the fact that it deepens, indeed makes possible even, the reactive feelings which belong to the content.' It is in connection with the concept of illusion and Scheingefühle that Groos has been most influenced by criticism, notably that of Külpe and Lipps, and has so modified his conceptions as to avoid the serious psychological difficulties to which they gave rise. Moreover, in connection with the study of inner imitation he has introduced the concept of Einfühlung, and his analysis of these phenomena is one of the most important individual features of the work.

Another modification of importance is his introduction of a chapter upon the æsthetic judgment. Distinguishing between the æsthetically effective and æsthetically valuable, or between æsthetic effect and worth, he conceives the latter to be a narrower province within the former. While the normative as such lies outside psychology, the discussion of the motives determining æsthetic value judgments is a psychological problem. Moral and knowledge judgments intermingle with æsthetic and contribute to the total effect. Within the æsthetic judgment itself are motives and postulates which go beyond the merely sensational and reproductive factors, with their pleasure values. The typical, the perfect, the purposeful suggestiveness of human meaning, all enter in to distinguish the æsthetically valuable from the merely effective. Groos does not go beyond the mere psychological description of these motives; he nevertheless furnishes a critical sundering of these judgments which, in turn, through fusion with the sensational and associative elements become factors in the total æsthetic effect.

In general, it may be said that this last attempt of Professor Groos to deal with general æsthetic principles represents a distinct advance in the direction of a general æsthetic theory. His own more specialized work during the years which have intervened since the publication of the 'Einleitung,' together with the careful consideration given to the results of other workers in the field, have made possible a work which, if less individual than the earlier publication, is certainly nearer the ideal of a general scientific treatise. The modifications and redistribution of emphasis which the various elements of his theory have undergone have in no way affected their importance and value, but are only such as a broader view of all the phenomena of the field in question made desirable. In its present form, Groos' view of the entire field of the psychological conditions of the æsthetic appreciation is perhaps as nearly authoritative as any can be in the present state of the subject. WILBUR MARSHALL URBAN.

TRINITY COLLEGE.

Les Obsessions et les Impulsions. A. PITRES et E. RÉGIS. Paris, Octave Doin. 1902. Pp. 434. (Bibliothèque Internationale de Psychologie Expérimentale.)

The authors of the 'Report on Obsessions' before the International Medical Congress in Moscow, in 1897, develop their theses in the present volume into a systematic treatise. So complete a treatment of the subject has, we believe, not before appeared.[1] The work is characterized not so much by the development of novel views as by general soundness of view resting on an extraordinarily large experience. The authors tell us that within some ten years they have personally observed no fewer than four hundred cases of obsessions. A large number of these are here reported. The book certainly conveys a very vivid impression of the facts.

The view taken of obsession is that it is a morbid state fundamentally emotional. It is defined as a morbid syndrome characterized by involuntary and distressing consciousness of parasitic sentiments or thoughts which tend to impose themselves on the ego, developing within the subject, in spite of his efforts to expel them, creating thus a sort of psychical dissociation whose final term is the conscious doubling, or division, of the personality (p. 16). This conception serves to distinguish obsession from the fixed idea. The fixed idea in its pure form is an intellectual phenomenon, gives rise to no distress and revolt, is accepted, for the most part, as real. Pure obsession, on the other hand, is a condition of emotional distress not derivable from any antecedent or accompanying idea, and when an idea is present it is recognized as false or at least pathological (p. 17). The two phe-

[1] Since the above was written, P. Janet's *Les Obsessions et la Psychasthénie*, a still more elaborate work, has been published. Critical notice will appear in a later number of the REVIEW. Janet, while recognizing the relative importance of the emotional theory of obsessions advocated by our authors, criticises it as in certain respects vague and incomplete.

nomena are often, however, associated, and it is admitted that in practice it is not always easy to say which gives rise to which. Obsessions, in the broad sense, are accordingly classified into phobias and obsessions (in the narrower sense), phobias being again subdivided into diffuse and special and obsessions into ideative, impulsive and hallucinatory. This classification of phobias along with obsessions, which departs from the common representation, is based on the view that the two are but varieties or degrees of the same neuro-psychopathic condition and that they differ only in the proportion in which the emotional and ideational elements are combined and in the way in which they severally develop. Obsession, it is maintained, is often only an aggravated or intellectualized form of phobia (p. 66). It is unfortunate that the carrying out of this view, which seems to have much to commend it, should introduce a somewhat perplexing confusion of terminology.

In addition to the description and illustration of the most important forms of phobia and obsession, the authors select for detailed discussion the obsession of blushing, a form of obsession which they first described in 1896 and which they claim had never been seriously and systematically described before. The obsession of blushing (ereuthophobia) is distinguished from the mere morbid and distressing tendency to blush (emotional ereuthosis) by the presence of a fixed idea (p. 180). It is said to be commoner in men than in women. The fact that, once the obsession created, the crises are usually brought on by the idea, might be held to conflict with the view that the fundamental characteristic of obsession is the emotional disturbance. But this is not necessary. What the subjects speak of as the 'idea' of blushing is, properly speaking, a fear. It consists, according to the authors, in a vivid representation, a systematic hypermnesia of affective memory and a spontaneous revival of a previously painful emotion, leading, unless checked, to the reproduction of the original emotion (p. 182). This view is confirmed by the reflex-like suddenness with which the crises come on in certain cases and by the fact that, under favorable conditions of the atmosphere, it is sometimes found impossible to blush, no matter how much the idea of blushing may be entertained. The experiments, therefore, which show the order (1) phobic idea, (2) blushing, (3) distress, accord in general with clinical observation, but they do not prove that the phenomenon is primarily intellectual; if (2) and (3) came first, (1) would doubtless follow. The primary disposition would seem to be regarded as a certain affection of the vaso-motor system; for the authors believe that the tendency to blush habitually precedes the emotional distress and that frequently differences in the intensity of the phobia bear a relation to the disposition to blush. To this extent they accept Lange's view of the importance of the vaso-motor element in the emotional process while refusing to commit themselves to his general theory (p. 195).

In the matter of etiology it is maintained that the habitual predisposing cause is heredity. Usually the obsession develops suddenly on occasion of some accidental event and is most frequently determined by some form of moral emotion. The view that it is exclusively of sexual origin is rejected. The disputed question as to whether it may ever terminate in insanity is decided in the affirmative. The final conclusion as to its nature is that it is not a distinct disease, but a symptom, and this is the general opinion. In the words of the authors, it "can be regarded as * * * one of the elements constituting a special emotional syndrome, the syndrome of acute distress (*angoisse*) liable to present itself in different degrees and under various forms in different neuroses and psychoses" (p. 252). Two chapters dealing repectively with the diagnosis and treatment of obsessions bring this portion of the book to a close.

The last third of the book treats of morbid impulses. In introducing the subject, the authors remark on the prevailing confusion respecting it, some regarding the 'impulsion' as an irresistible or at least imperious morbid tendency, some as a forced act, some both indiscriminately; some, again, consider it as exclusively a conscious obsessive propensity, while others admit that it is sometimes unconscious and automatic. The view of it taken here is that it is simply the tendency to reflex action breaking down the normal voluntary *tonus*, *i. e.*, the harmonious regulation of the instincts by the ego. Hence the term is here used to cover all pathological impulses from the blind, automatic impulse at the one extreme to the obsessive impulse in its most attenuated form at the other. The execution of the act is not regarded as indispensable nor the impulse itself as necessarily irresistible. It is defined as 'the imperious and often irresistible tendency to revert to the pure reflex' (p. 291). Morselli is followed in the description of the general characteristics of morbid impulses as endogenous, imperious, aberrant, involuntary and usually, though not always, conscious. The one selected as the basis of classification is that of constraint. Thus three types are distinguished: (1) pure motor, wholly reflex; (2) psycho-motor, quasi-reflex; (3) psychic, or impulsive obsessions. Morselli is again followed in the classification of clinical types, the chief of which, impulses to suicide, homicide, theft, arson, drink, wandering away and sexual acts, are described seriatim at some length. These types are regarded not as monomanias, but as symptoms or syndromes of a variety of psycho-pathic conditions — degeneration, epilepsy, hysteria, etc. — in each of which they take on a modified form. Among the points of interest in the discussion of the medical aspects of the phenomena, the view taken of the treatment by suggestion may be referred to as indicating the present trend of opinion on this subject. The authors believe that this form of treatment is only applicable in cases of impulsive obsessions and even then only when there is a hysteropathic disposition for hypnotic suggestion to work on; but even in these cases they find the results frequently disappointing in spite of the large promise of the beginning. But the pure degenerate and the victim of constitutional neurasthenia — and these form a large part of the cases — cannot, they declare, be hypnotized at all.

The work concludes with the detailed report of a number of criminal cases in which the authors have given expert testimony in favor of medical treatment in place of the ordinary legal penalties.

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Human Nature and the Social Order. Charles Horton Cooley. New York, Charles Scribner's Sons. 1902. Pp. viii + 413.

It is the author's aim 'to set forth from various points of view, what the individual is, considered as a member of a social whole.' Believing that "'society' and 'individuals' do not denote separable phenomena, but are simply collective and distributive aspects of the same thing," he holds that in current thought, the social and the individual are often opposed in a fallacious fashion, as though two separable entities or forces were under consideration.

Our modern world is particularly liable to emphasize the individual aspect as in some sense more real, or as psychologically primary though ethically inferior. Such abstract views may be classed as: (*a*) *mere individualism*, in which the collective phases are looked upon as quite secondary and incidental; (*b*) *double causation*, in which society and the individual are thought of as separate causes, and socialism is opposed to individualism as a theory of life; (*c*) *primitive individualism*, in which man is conceived to have been originally a mere individual, but to have been undergoing a process of socialization, and thereby of moralization, since the individual in this view is liable to be identified with the bad, the social with the good; (*d*) the *social faculty view*, in which the social is regarded as including only a part of the individual, *i. e.*, certain faculties or emotions. As contrasted with these four views the author holds that individuality and sociality have always existed as complementary aspects, and that 'the line of progress is from a lower to a higher type of both, not from the one to the other'; that 'man's psychical outfit is not divisible into the social and the non-social, but that he is all social in a large sense, is all a part of the common human life, and that his social or moral progress consists less in the aggrandizement of particulai faculties or instincts and the suppression of others than in the discipline of all with reference to a progressive organization of life, which we know in thought as conscience.'

It is evident from the above that the author's program includes both social psychology and ethics. The platform just quoted is of course not peculiar to the author. It has been gradually emerging in the work of criticism and construction in which many have engaged. The interesting and valuable feature of Professor Cooley's contribution is the happy manner in which he has developed and illustrated his thesis. He has had the fortune to have two children so different in their types of reactions as to prevent the hasty generalizations from 'one' to 'all' sometimes met with in genetic psychology, and has added important observations and reflections to this field. He has drawn upon a wide acquaintance with literature for illustrations of the social sentiments, and has indicated ways in which the experimental method might well be applied to this great field of emotional expression. He has an observant eye for significant aspects of human conduct, and sanity of judgment for their interpretation. He has expressed himself with simplicity and directness. The various points of view from which he considers his subject, as suggested by the titles of his chapters, are suggestion and choice, sociability and personal ideas, sympathy, the social self, hostility, emulation, leadership, the social aspect ofconscience, personal degeneracy, and freedom.

Under 'Suggestion and Choice' the phenomena of imitation are discussed. The point is well taken that we must distinguish carefully between the result and the process. The result of a child's efforts may be the production of an act which is like the act of the parent, but the process may not be at all that of mechanical suggestion. Children 'cannot imitate an act except by learning how to do it, any more than grown-up people can, and for a child to learn a word may be as complicated a process as for an older person to learn a difficult piece on the piano.' The two children observed by Professor Cooley showed a marked difference as to imitation. One showed scarcely any tendency of imitation until after the age of two years and a half; the other began to show vocal imitation when a little over two months and continued in the ordinary path. The former reached his results largely by his own experimentation and refused to imitate directly.

The chapter on 'Sociability and Personal Ideas' makes a vigorous statement of the fact that society is fundamentally a subjective reality. "In order to have society it is evidently necessary that persons should get together somewhere; and they get together only as personal ideas in the mind." "Persons and society must, then, be studied primarily in the imagination." "The imaginations which people have of each other are the *solid facts* of society." It seems to the reviewer that the emphasis upon the function of imagination, both in this and the following chapter, is one of the best features of the book. The chapters on the social self may be regarded as in part a supplementation of Professor James' chapter on the self.

The author has a special criticism in several passages upon the popular antithesis between egoism and altruism. It may be readily granted that the phrases as ordinarily employed are liable to criticism in so far as they seem to imply mutually exclusive alternatives. But common life knows perfectly well what it means by selfishness, and no amount of psychological criticism on the terminology will alter the fact that one man has a 'self' which does not recognize the equal or superior claims of others, while another man has a self which does recognize the claims of others to be treated always 'as an end, never as merely a means.' Now, from the individual standpoint one of these selves may be as 'harmonious,' as 'stable' as the other. But they have very different ethical values. I query whether, in his desire to correct a current misuse of terms, the author has done justice to the real ethical difference implied in the terms. Other queries as to details will naturally suggest themselves to the reader, but the book is a decidedly welcome contribution to social psychology.

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NATURE OF CONSCIOUSNESS.

The Psychology of Mental Arrangement. I. Madison Bentley. Am. Journ. Psychol., Vol. XIII., No. 2.

"We may say, in general, that any structure made up of heterogeneous elements — if it be anything more than a mere collection or heap — implies arrangement, plan, pattern, and not simply addition of abstracts units." The question is whether this arrangement or principle of synthesis is to be found without or within the sum of the units. The problem of mental arrangement is, in the last analysis, the problem of interpreting our psychological abstractions in terms of the steps by which these abstractions were first made and have since been elaborated. In the associational and faculty psychology this, only too obviously, never was done. Is it done in the recent psychology which discusses the problem?

This paper aims 'to bring together the various recent contributions to the subject * * * by the Austrian school of psychologists' (Ehrenfels, Meinong, Cornelius, Witasek, Schumann and others). Ehrenfels insists that over and above the elements into which a mental complex can be analyzed there is a factor of arrangement which he calls the form-quality (*Gestaltqualität*). Meinong uses the term funded-contents (*fundirte Inhalte*). Cornelius, on the other hand, starts with 'a big, unanalyzed, undissected mass' (*Gesammteindruck*). Schumann, Stumpf and Stout represent intermediate positions.

The real problem here involved is the fundamental one: When is experience one, and when is it many? Under what conditions do we view consciousness as a unity, and when do we distinguish various aspects or elements? It is essentially the same as the question when mind is to be viewed statically as a thing or entity and when dynamically as a process, a stream, or movement. These are questions concerning consciousness which can be answered only by a functional analysis from within, never by a survey from without. It must be an internal rather than an external type of analysis. The whole discussion shows "how misleading is such a rubrication as 'sense' and 'intellect.' Mind is not so simple as that one can say of a mental formation, 'this is either a sensation that has found its way into mind or a sheer spiritual creation that mind has evolved.'" It is true 'that a complete descriptive account of a mental complex demands more than an enumeration of its constituent elements,' but on the other hand, in these discussions, "where a 'funded' factor has been found necessary, the true nature of psychological elements has not, as a rule, been kept in view," and the concept of 'mental activity,' here as elsewhere, is ambiguous.

One phase of the discussion is the insistence by certain writers on the existence of distinct elements of relation, a consciousness of 'togetherness.' "Is the 'fourness' in the perception of four objects, or the 'squareness' of the square itself, an element?" Is 'liquidity' something more than pressure + temperature? Does the 'fifthness' of the fifth remain the same though the elements be changed? It appears so. Stout says that 'togetherness' or, as he calls it, 'the apprehension of form,' is a distinct kind of consciousness, a 'constituent of consciousness comparable * * * with the perception of red or blue.'

The solution of this problem is to be found in an examination and criticism of our methods of psychological analysis and abstraction. If our method yields us an abstract particular, psychological atoms, then, to offset this, we are compelled to postulate some synthetic principle, some abstract identity. This is the fallacy underlying the uncritical use of such terms as 'mental activity,' 'attention,' and the various phrases by which this Austrian school of psychologists express the important idea of mental arrangement as over against the mental units which result from the analysis of any concrete experience.

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The Unity of Process in Consciousness. HENRY RUTGERS MARSHALL. Mind, New Series, Volume XI., pp. 470–502.

Looking at the world of living organisms, from the standpoint of an outside observer, one 'cannot but be impressed with the evidence that a unity of process exists through all the apparent diversity which he at first observes in the forms of living bodies.' Thus it appears, at least, to the biological investigator whom we may conceive to view this process as "a 'spirit,' if we may use the term, with such full capacity for scientific observation and analysis as man displays, but without any thought that his observations or analyses are aught else than interesting modifications of his consciousness; without any knowledge that his consciousness is related to any human body; and with no notion whatever that any form of consciousness has any connection with animal activities."

'Our spirit' observes that if any bit of matter, whether lifeless or alive, receives a stimulus, it reacts upon this stimulus and becomes a different thing. In the case of living matter, there is, apparently, the capacity to become again what it was before its reaction to the stimulus. But a closer inspection shows that this capacity is never exactly regained. Reaction to a stimulus never leaves the body exactly what it was before the reaction. In the case of the complex organism and environment this is called 'learning by experience.' Probably it is characteristic of all living matter, but escapes observation in the simpler forms because of the inadequacy of our observation. That is, there 'must be a unity of process in all the reactions of living matter.'

Close observation, furthermore, fails to reveal any absolute distinction between the multicellular organism and the single cell. The cell is a complex, a system, a living machine, as truly as what we call the organism; and the activity of any part involves changes in the whole mass. "The activity of the system as a whole is in fact merely differentiated in form by this special activity of the part." Here, again, is evidence of a unity of process in the nature of life as exemplified in animal behavior, a unity which embraces the differences rather than excludes them.

As a test case, let us suppose that 'our spirit' turns his attention to the examination of the reactions of the nervous system of civilized man, which reveals animal behavior in, perhaps, its most complex form. Here, too, a close study shows 'that no reaction in any part of a system of systems can fail to modify in some measure the total pulse of activity in the whole system of systems.' "Stimulation and reaction occur in one and the same act." Stimulus, response, and the intermediate central nervous process are simply phases of an organic circuit. They are not successive stages but coördinate constituent functions in one process. The 'neurergic pattern' which the whole system or circuit presents depends simply upon what aspect is emphatic at the time.

'Our spirit,' however, may turn his attention to apparent variations from this. "He notes for instance in some cases immediacy of reaction to a stimulus, and in others a marked hesitancy." "Under certain conditions of stimulation he sees what appear as trial of one form of activity with failure, then of another with success, and a persistence of the successful form of activity." But here again 'he sees that the basis of the adaptation of the activity of a complex system of systems to new conditions must always lie in the emphasis of some partial activity in some minor system of the great system of systems; and that the capacity to effect this emphasis of a partial activity is one which inheres in all systems, whether relatively simple or complex, and that when effected it must give rise to a variation from the reaction which he has looked upon as typical.'

So far, this unity of process has been found and described by 'our spirit' in purely objective terms, such as the scientific biologist might use. "We may now assume that he suddenly, and for the first time, discovers the startling fact that his own mental states are in some way related to a human body." He first asks of what he is really thinking when he says that his mind has a body. Take a concrete case. "I have a certain spatial presentative experience of an 'object in the outer world' which I call a sharp-pointed pin; and this is presented as approaching what is another 'object in the outer world' held in the same presentation, and which latter I call the finger of this human body. When in this complex presentation the pin touches this finger I experience not only the complex spatial presentation of the two objects in contact, but also what I may speak of as a streak upon the surface of the stream of consciousness, and which I call a painful pricking sensation; and this is a non-spatial presentation, it is not an 'object in the outer world.'" "I observe so many striking facts of this kind, and the evidences of the relation thus suggested are so many, that I am led to assume for the moment an hypothesis which I shall call 'parallelism.'" "If I think at all of the relation between the action of nerve in my body and my non-spatial presentation I cannot, under this hypothesis, assume the occurrence of a non-spatial modification of presentation without also assuming the existence of a coincident 'action of nerve' in my body." "I perceive at once that logically I should expect to be able to assert the reverse of this proposition, that is, I ought also to contend that I cannot assume any action of nerve in my body without assuming also a coincident modification of my non-spatial presentative experience."

There will be forced upon the attention of our introspective psychologist (for such 'our spirit' has now become), the fact of the unity of process in nerve action with which he has already become familiar as a scientific biologist. The continuity in the stream of consciousness is reflected in or is reflection of the unity of process in nerve action. And the neural systems within systems he finds reflected in the fact that consciousness in like manner is a complex system of psychic systems. Moreover, he finds that there is a 'noetic pattern' corresponding with the 'neurergic pattern,' and that he must view this 'noetic pattern,' also, "as a whole pulse of psychic activity, and these especially vivid presentations merely as partial psychic activities which for the moment are especially emphatic." "And thus he sees how what may really be a unity of psychic process, in a complex system of systems, may appear at the first glance to be a diversity of process."

But, is the converse true? Is all nerve action accompanied by a coincident psychic modification? Similar arguments lead to an affirmative answer. 'There is some modification of psychic life in connection with all action of nerve,' and this is 'mentality.' "There must always be coincident a system of mentalities which under certain conditions becomes what we call a consciousness."

This, it seems to the reviewer, is the central problem. What are these 'conditions'? And why call these systems *mental* unless or until these conditions have been fulfilled? The real logic of Mr. Marshall's article seems to be this: In organic function we find and describe unity and continuity of life process, expressed in the idea of growth. The unity and continuity of function in the life process imply orderly and continuous activity with reference to an end. But the idea of end has meaning only in or with reference to conscious experience. Therefore, in consciousness alone is to be found real unity of process. Whether this is idealism or not is an entirely distinct question, depending upon one's conception of the nature of consciousness.[1]

The assumption of unconscious mental states seems only confusing. We have already the convenient category of the neural or the physiological for the unconscious conditions of consciousness. Why then create a realm of the unconscious psychical to account for them? Why would it not be as legitimate to postulate a subphysical world to account for difficulties in physics as to posit a subpsychical realm to account for these unconscious conditions of consciousness?

Two further points: It seems to the reviewer that Mr. Marshall distinctly weakens his argument by his defense of the 'back-stroke' theory of the emotions. His own skilful analysis of the organic circuit seems rather to imply (what he denies) that 'the data for consciousness are in all cases supplied through afferent channels.' But very timely seems the criticism of Mr. Morgan's introduction of 'effective consciousness,' to account for the higher forms of animal behavior. "The principal objections to the view that consciousness at times 'enters in' to guide behavior, and at times does not, lies in its unacknowledged denial of the unity of process in consciousness."

H. Heath Bawden.

Vassar College.

EXPERIMENTAL.

Eye-movements and the Æsthetics of Visual Form. G. M. Stratton. Philosophische Studien, Bd. XX. (Wundt's Festschrift, II. Thiel), pp. 336–359.

The substitution of photographic records of eye-movements for introspective testimony as to the way in which the eyes behave is

[1] See a forthcoming article on 'The Functional Theory of Parallelism' by the present writer in the *Philosophical Review*, where this thought is developed.

without doubt a long step in advance. Photographic methods as applied by Professor Dodge to the measurement of reaction times, and similar methods as applied by Professor Stratton in this investigation to the determination of the direction of eye-movements, show how utterly unreliable are all the statements based on mere introspection. Professor Stratton finds that in following the outlines of simple figures, such as circles and rectangles, and various kinds of curves, the eye does not make a movement corresponding to the line followed, but moves forward in a series of irregular and often wholly ungraceful lines. A circle, for example, is followed by movements that describe a triangle or an irregular closed figure. A rectangle is followed in both curved and unequal straight movements. Graceful curves and ungraceful irregular lines are followed by the eye in movements hardly distinguishable from each other. These facts lead Professor Stratton to the conclusion that eye-movements do not contribute the chief factors to our recognition of æsthetical forms.

Æsthetical form is due rather to a wider process of synthesis. The regular curve is more gratifying to us than an irregular line because its regularity economizes our attention. We can apprehend its purpose or principle as a fixed and unitary formula of direction. The wavering and uncertain purpose of the ungraceful line is by contrast uneconomical of attention. Among the various regular lines, curves have more of the suggestion of life. The graceful flight of birds and the movements of the skater, or similar forms of living movement, come vaguely into consciousness when we feel the beauty of a graceful curve. There may be an organic vasomotor or muscular response as a secondary sensuous basis for this association with living movement.

Æsthetical form is thus seen to be not merely a matter of sensation. It is a matter of general interpretation and involves all phases of mental nature. Indeed, our author finds in his study opportunity to emphasize the relation of the æsthetical to the moral and religious purposes of life.

The theory of æsthetical form here outlined has broad implications which we shall expect to find Professor Stratton working out in greater detail in the future. The present article is exceedingly suggestive in its conclusions; in its earlier experimental part it is a large contribution to our knowledge of eye-movements.

Charles H. Judd.

Yale University.

Die Arbeitscurve. EMIL KRAEPELIN. Philos. Studien, XIX. (Wundt's Festschrift, I.), pp. 458–507.

It is difficult to summarize this article, which is itself a summary of a considerable portion of the research that has been done in Professor Kraepelin's laboratory for something like ten years.

The work that has been especially employed for study is that of adding columns of figures, the successive combining of two numbers serving as a test of the amount of work done. An extended period of work shows marked variations in the rapidity of execution from beginning to end, which are especially striking when plotted into a curve. The work aims to explain these variations. This is accomplished by analyzing the curve into component parts, each of which represents a fundamental process. These processes vary with the individual, so that the methods for analyzing the curve serve also to analyze the subject's mental constitution.

The two most pronounced factors in shaping the curve are fatigue (Ermüdung) and practice (Uebung). Fatigue tends to decrease the rate of working, and hence to make the curve drop from the beginning of the period. With subjects easily fatigued this is often the result, but subjects not so sensitive to this factor show a gradual rise for a considerable time, when finally all alike show a rapid fall caused by an increased difficulty in work as a result of a high degree of fatigue. The gradual rise is due to practice, which tends to make a performance more quickly and easily executed the more it is repeated. This influence overcomes the immediate effects of fatigue, and accumulates during a single working period, increasing also in a marked way the rate of working for each successive period. Although loss from lack of practice is more rapid at first than later, traces from it have been found to remain for months; so that with frequent repetitions the rate of adding will constantly increase until a stage is finally reached at which improvement stops. By comparing several subjects it has been found that great ease of fatigue, rapidity of improvement by practice and a quick loss from lack of practice are characteristics which go together.

Since fatigue is recovered from more quickly than practice is lost, there is a point reached after the stopping of work at which one is able to work with the greatest rapidity. The rest up to this point is called 'the most favorable pause' on account of its effect upon work which immediately follows. The increase of working power is not constant, however, from the beginning to the end of this pause, because of a momentum (Anregung) from the work, which lasts some ten minutes.

This arises from a sort of mental inertia, which prevents the rapidity of work from reaching its maximum at first, as well as continuing the working efficiency after the work has stopped. The duration of this momentum is found by varying the pause between two periods of work. The pause which is followed by the slowest rate marks the point at which the momentum is completely lost.

Another factor which influences the curve, especially at first, is adaptation (Gewöhnung) to the condition of the experiment and the various disturbances which figure in the surroundings. This tends to delay the rapidity of working, but after two or three days ceases to be a disturbing factor, as more complete mental concentration is attained. Yet another element of the curve of work arises from the effort (Antrieb) incident to starting the work of adding. This voluntary effort is difficult to maintain and shortly disappears. When most apparent it causes a rapid fall in the curve from the point of starting. If more than a couple of minutes at the beginning of the period are averaged for the first section of the curve, the effect is likely to be covered up because of its brevity. Effort is also often consciously present when a disturbance threatens the mental concentration, and also near the end of a period when anticipated release causes the subject to accelerate his speed.

A lithographic cut presents in distinct though somewhat imaginative proportions the curve elements which compose the curve of work.

These results are a good illustration of what persistence and well directed effort will accomplish in the way of solving a very knotty problem. Although this is not fully solved, there are the best of reasons for anticipating its complete solution in the near future. As a means to this end may be suggested more perfect experimental conditions to meet the delicacy of the problem. Complete control of auditory and visual impressions which in practically all laboratories make complete 'adaptation' impossible, could be accomplished by means of suitable apparatus in a dark room removed from auditory shocks. Adding, too, though the most satisfactory means for measuring work yet employed, has disadvantages. Five and one, *e. g.*, are more quickly added than seven and nine. Either carefully arranged columns, in which easy and difficult additions are systematically mixed, or the substitution of some other work, as perhaps counting, would greatly add to the uniformity of results.

John P. Hylan.

Cambridge, Mass.

NEW BOOKS.

Human Nature and the Social Order. C. H. Cooley. New York, Scribners. 1902. Pp. viii + 413.

The Psychology of Ethics. D. Irons. Edinburgh and London, Blackwoods. 1903. Pp. xviii + 176.

Heredity and Social Progress. S. N. Patten. New York and London, Macmillans. 1903. Pp. 215.

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What is Meaning? V. Welby. London and New York, Macmillans. 1903. Pp. xxxi + 321.

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Notice is reserved until the appearance of Vol. III.

L'Hypnotisme et la Suggestion. Dr. Grasset. Bibl. Int. de Psych. Expér. Paris, Doin. 1903. Pp. 534. 4 fr.

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Modern Spiritualism. F. PODMORE. 2 vols. London, Methuen; New York, Scribners. 1902. Pp. x + 307, xii + 374. $5.

Human Personality and its Survival of Bodily Death. F. W. H. MYERS. London and New York, Longmans, 1903. Pp. xlvi + 700, xx + 660. $12.

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Pure Sociology. A Treatise on the Origin and Spontaneous Development of Society. LESTER F. WARD. New York, Macmillans, 1903. Pp. xii + 606.

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Quarto-centennial Celebration, University of Colorado, November 13, 14 and 15, 1902, Boulder, Colo. (Univ. of Colorado Bulletin, Vol. II., No. 4, Dec., 1902). Pp. 112.

L'année biologique, Sixième année, 1901. YVES DELAGE. Paris, Schleicher. 1903. Pp. lxxxiv + 575.

More Letters of Charles Darwin. Edited by FRANCIS DARWIN. New York, Appletons. 1903. 2 vols. Pp. xxiv + 494, 508. $5 per vol.

Essai de classification naturelle des caractères. CH. RIBÉRY. Paris, Alcan. 1902. Pp. xxiv + 199.

The Study of Mental Science. J. BROUGH. London, Longmans, Green. 1903. Pp. 129.

Experimental Studies in Psychology and Pedagogy. Edited by LIGHTNER WITMER. *I. Spelling in the Elementary School: An Experimental and Statistical Investigation.* OLIVER P. CORNMAN. Pp. 98. *II. The Sensation of Pain and the Theory of the Specific Sense Energies.* ANNA J. MCKEARG. Pp. 87. Boston, Ginn & Co. 1902.

Life and Letters of the Rt. Hon. Friedrich Max Müller. Edited by his WIFE. Two vols. London and New York, Longmans. 1902. Pp. xiii + 534, ix + 521.

The preface starts out: "It may be thought that the publication of these volumes is superfluous after the two works, *Auld Lang Syne* and the *Autobiography*, written by Max Müller himself. But it seemed that something more was wanting to show the innermost character of the real man. * * * The object of this book is to show 'the elevation of soul and enlargement of mental outlook which was revealed more and more as his life's work opened before him.'" Truly a pious task and no doubt worth while — if we are to have three lives of Stevenson! The book is redolent of the Oxford setting, and that is 'to the good' for those who find Max Müller an overrated individual who never lost the personal and dramatic cue in his life's rôle. Why should 'the Rt. Hon.' be put in the title of a great man's biography? Think of Darwin or Spencer needing such an introduction! Intrinsically the volumes are of very great interest, and as specimens of book-making they rank high. J. M. B.

Carnegie Institution of Washington. Yearbook No. 1, 1902. Washington, The Carnegie Institution. 1903. Pp. xlvi + 305.

The report of the Advisory Committee on Psychology, included in this yearbook, is reprinted in the *Princeton Contributions to Psychology*, Vol. IV., No. 1, now in press (Princeton, N. J.). We doubt the wisdom of holding such a report for sale ($1); we understand that even press copies are not sent out. We have received an advertising circular. These reports would be most instructive to the public, and the general diffusion of such information would seem to fall within the scope of the Foundation. We hope the trustees will not adopt a too commercial policy with their publications. In the announcement of the *Index Medicus*, revived by the Carnegie Institution, it is said too of that publication, that it will not be sent, in exchange, to other journals, but sold — at a rate that seems likely to restrict its circulation greatly. J. M. B.

Outlines of Cosmic Philosophy. JOHN FISKE. Introduction by JOSIAH ROYCE. Boston & New York, Houghton, Mifflin & Co. 1903. Four vols. Pp. cxlix + 276, 411, 373, 390.

Professor Royce's 'Introduction' confines itself largely to the exposition of what Fiske actually said—to a sort of epitome of the 'Cosmic Philosophy' in its relation to the author's other books. This seems unfortunate because, as Professor Royce himself says, Fiske's opinions are now mainly of historical interest, and no one would read the 'Cosmic Philosophy' for current views. The reprint would have had greater justification if the 'Introduction' had attempted to bridge the gulf—to develop and modify Fiske in the directions of later scientific investigation. As it is, the present writer confesses that he does not see the utility of the reprint, except indeed in so far as there may be a demand for the complete writings of Fiske on the part of some who are not able to procure the old edition of this work. Professor Royce's 'Introduction' does have interest, but yet it seems somehow to lack sympathy or vitality—characters which arise, at least in part, from the straitness of its scope. J. M. B.

Les obsessions et la psychasthénie. II. F. RAYMOND and PIERRE JANET. Paris, Alcan. 1903. Pp. xxiv + 543. 14 fr.

A collection of clinical lectures following upon the Vol. I. by Janet, with the same title. It renews the colloboration begun by the same authors in the second volume of *Névroses et idées fixes*.

History of the Problems of Philosophy. JANET and SEÁILLES. Trans. by A. MONAHAN, with Introduction by H. JONES. Vol. I. *Psychology*. Vol. II. *Ethics*, *Metaphysics*, *Theodicy*. London and New York, Macmillans. 1902.

This book will undoubtedly be useful in English, especially the volume devoted to psychology. Yet we share the hesitation shown by the editor, Professor Jones, as to its meeting the full needs of English-speaking students. It has two distinct claims to recognition: it is — that is, the psychological volume is — the only historical work in English coming down beyond Kant; and it is full where the German treatises are empty. Furthermore, we have great sympathy with the author's protest against being criticised for not doing what he did not set out to do. The most serious defect of the psychological volume is, perhaps, its incompleteness — due to the arbitrary selection of topics. J. M. B.

NOTES.

It is with regret that we record the death of Professor D. G. Ritchie, of St. Andrews. Both chairs in philosophy at that university thus become vacant at once. The Scottish universities have had great losses recently, by death and removal, in this department — Fraser, Sorley, Stout, Knight, and earlier Caird resigned; Adamson and Ritchie removed by death.

We are also called to note the resignation of Professor Clark Murray, of McGill University, Montreal.

We regret to hear of the death of Professor Hiram M. Stanley, of Lake Forest University, which occurred on April 3.

Drs. C. A. Strong and Livingston Farrand, of Columbia University, have been appointed full Professors of Psychology and Anthropology, respectively, in that institution.

Miss M. F. Washburn, Professor in the University of Cincinnati, has been appointed associate Professor of Philosophy at Vassar College.

The *Revue de Philosophie* announces the publication of an annual *Index philosophique*, beginning with 1902 (to appear March, 1903). It will include 'Psychology (with the related subjects, Physiology, Medical Science, Biology),' as well as Logic, Metaphysics, Ethics, and History of Philosophy (169 rue de Rennes, Paris, VIe; 3 fr. 50, or to subscribers to the *Revue*, 2 fr. 50).

Dr. F. S. Wrinch, of Princeton University, has been appointed to a Research Assistantship by the Carnegie Institution, for research to be conducted in the Princeton laboratory, under the direction of Professor Baldwin.

Messrs. Charles Scribner's Sons announce that they have arranged for the publication of a 'Library of Historical Psychology,' under the editorial supervision of Professor Baldwin of Princeton University. The library is to comprise a series of volumes written by leading authorities at home and abroad on the history of the various leading topics of psychological thought from the earliest times, each volume being an independent work, but the whole constituting an encyclopedic 'History of Psychology.' The arrangements for the volumes of the library — of which there will be twelve or more — are now being perfected, and the publishers expect to make early announcement of certain of the titles, names of writers, etc.

Vol. X. No. 4 July, 1903

The Psychological Review.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY PROFESSOR JAMES ROWLAND ANGELL.

IV. Psycho-physical Tests of Normal and Abnormal Children. — A Comparative Study.

BY ROBERT LINCOLN KELLY,

President of Earlham College.

The tests reported below were made upon the pupils of the elementary school of the University of Chicago, and the cases in the Chicago Physiological School. The pupils in the first school are normal children, between the ages of four and fourteen, influenced by hereditary and environmental conditions probably superior to those of the average child. The cases in the Physiological School are backward and defective children who have failed to show the necessary ability to make satisfactory progress in the public schools. Some of these children rank quite low in intelligence but all are believed to be educable. They range in age from nine to nineteen years. The physician's examination frequently, in fact usually, brings to light quite unfavorable hereditary influences. There are also numerous and sometimes pronounced stigmata of degeneracy present. In some cases the defects are due to arrested development, either pre- or post-natal. The present study, however, is devoted to the existing conditions and possible progress of these cases under such favorable surroundings and pedagogical procedure as the Physiological School affords.

The Nature of the Tests.

The tests covered rather a wide range. This is a necessary incident of pioneer work. Very few tests were made, however, which did not yield readily to consistent interpretation. Despite their number the tests may be divided roughly into three classes. There were the ordinary tests of the senses of hearing, sight, taste, smell, touch and temperature together with sensitiveness to pain. There was a series of muscular tests, involving numerous forms of motor coördination with special reference to rapidity, accuracy and steadiness, of movement, and fatigue. And third, a number of tests were made with special reference to prevalent forms of imagery in peculiar types of children, certain emotional reactions, etc. Effort was made to conduct the experimentation under approximately identical conditions of time, place, environment, fatigue and so forth. At the Elementary School there was taken simultaneously a series of physical measurements, including weight, height, lung capacity, strength of grip, and the numerous girth, depth and breadth measurements.

These measurements, however, are not reported in this paper. At both schools the record of each child was carefully preserved for comparison and reference.

The Purpose of the Tests.

Certain definitely formulated purposes were held in view throughout this work which extended over part of one school year and all of the next.

(*a*) It was deemed desirable to get psychological data which would serve in determining the most fruitful pedagogical procedure for each child. This purpose proved immediately practical in numerous cases.

(*b*) It was believed by fashioning the tests to the possibilities of the two widely divergent classes and making them as nearly identical as possible, some ready and simple method might be determined of differentiating the abnormal from the normal child. Observation shows that about one child in fifty in our public schools is utterly incapacitated by physical defect or mental incapacity from reacting spontaneously to certain valuable parts of his environment.

(*c*) Furthermore, it was believed that some light might be obtained upon the problem of finding a substitute for the present formal, arbitrary and confessedly inadequate methods of determining children's real mental capacity.

(*d*) And finally and chiefly, it was hoped that some contribution might be made to the present meager knowledge of the psychical life of the child in each of the two disparate fields of investigation. The author of the present paper makes no claim to have accomplished much as yet in the realization of the last-named purpose. In fact, the work here reported has been done with the belief that such knowledge will not be secured by the promiscuous massing together of data obtained from different sources, by different individuals of varying ability in observation and insight, nor by one well-trained individual selecting his data from the same sources, but who, ignoring the necessity of following up the children in their slow development, fails to adopt the principle of repetition in his experiments. Most of the tests here reported have been repeated one or more times and it is with the expectation that the process of repetition will be continued that the work is now made public.

The Sensory Tests with the Normal Children.

An astonishing thing about the ordinary sense tests is the fact that although no great amount of skill or expenditure of means is required for their prosecution, teachers and parents remain ignorant of readily detectible and usually remediable defects. These defects, moreover, are continually interfering with the progress of the child if not indeed threatening the existence of much of the valuable content of his life.[1]

Hearing and Sight. — Of 53 pupils in the Elementary School whose hearing was tested, 3 were found seriously defective. But 1 pupil was found lacking in keenness of vision. 61 per cent. of the pupils tested were astigmatic, though of this number but 2 or 3 required immediate medical attention. Out of a total of 66 there were 27 cases of slight color blindness, 2 cases being so pronounced as completely to incapacitate the pupil for certain

[1] Rowe's 'The Physical Nature of the Child' is the briefest and most concise statement of practical technique in this series of tests.

kinds of school requirements. 11 of these matched the standard colors but were confused on shades, 13 were confused on greens and blues, though most of these could distinguish between the standard blue and green. 4 confused green and yellow shades, 2 pink and blue, and 1 green, blue, and yellow.

Taste and Smell.

The facts as to sensitivity in taste and smell are illustrated in Tables I. and II. Table I. shows the threshold of sensitivity

Table I.

Taste and Smell, Elementary School; Threshold.

		Taste.				Smell.	
		Quinine.	H_2SO_4	Saccharine.	Salt.	Violet.	Cloves.
a	Boys.	4	2	3	3	7	2
b		16	15	18	16	8	4
c		6	8	6	8	16	22
d		1	1	0	0	0	0
Total		27	26	27	27	31	28
a	Girls.	1	2	2	0	7	0
b		13	11	11	13	3	3
c		2	3	3	3	6	13
d		0	0	0	0	0	0
Total		16	16	16	16	16	16

Table II.

Taste and Smell, Elementary School; Discrimination.

		Taste.				Smell.	
		Quinine.	H_2SO_4	Saccharine.	Salt.	Violet.	Cloves.
Named.	Boys.	25	12	27	27	28	7
Not Named.		6	19	4	4	1	16
Total.		31	31	31	31	29	23
Named.	Girls.	15	15	15	16	19	4
Not Named.		0	0	0	0	0	10
Total.		15	15	15	16	19	14

as indicated by the naming of the sensation quality. While this ability to command the word corresponding to a sensation quality may not be a strictly reliable criterion as to the point where the actual discrimination occurs, it has in its favor the fact that it is sufficient for all practical purposes. Besides with children it is quite difficult for the observer to hit upon any more reliable criterion for the determination of the discrimination limen. That the actual discrimination sometimes occurs before the required linguistic capacity appears, is not to be questioned.

In Table I. the letters *a*, *b*, *c*, *d* refer to the different strengths of solution. For quinine they are respectively:

.00004 per cent.; .0004 per cent.; .001 per cent., and .0021 per cent.

For Sulphuric Acid — .001 per cent.; .008 per cent.; .016 per cent., and .024 per cent.

For Saccharine — .0005 per cent.; .0025 per cent.; .025 per cent., and .05 per cent.

For Salt — .01 per cent.; .13 per cent.; 2 per cent., and 2.8 per cent.

The smell solutions consisted of extract of violet: .0000001 per cent.; .00001 per cent.; .1 per cent., and .5 per cent.; and oil of cloves, .0000001 per cent.; .00001 per cent.; .001 per cent., and .004 per cent.

The first in each series is below and the fourth is above the ordinary threshold; the second and third are within the range of the normal threshold. The girls have a uniformly lower taste threshold than the boys.

The results in smell are interesting though the significance is sociological rather than psychological. Half of the boys failed to detect the violet until the third solution was reached, while almost half of the girls detected it in the first solution. There is a general deficiency in detecting cloves, but the difference between violet and cloves is much more marked in the case of the girls than the boys. While nearly half of the girls detected the weakest solution of violet, four fifths of them failed on cloves until the third solution was reached. Since cooking is a part of the course of study in the Elementary School, it seems a little startling that the æsthetic odor entirely out-distances the practical one in the race for recognition.

In the discrimination tests, as was said, the sensation qualities were named. It was not required, however, that the article producing the quality be named. For instance, 36 children said 'bitter' to 4 saying 'quinine,' and 21 said 'sour' to 2 'lemon' and 1 'puckering.' All said 'sweet' and all said 'salt.' In the first smell test 35 answered 'perfumery,' the other 12 answering 'violet,' 'cologne,' 'rosewater,' etc. Cloves afforded the greatest perplexity. Less than one third gave the correct answer and the other answers included cinnamon, peppermint, pepper, birch-bark, lead-pencil, spice and paste. Of the tastes sour is the least readily detected and named, particularly with the boys, three fifths of whom failed entirely in naming it. The transition of an agreeable into a disagreeable sensation was frequently illustrated in the case of the strong sweet solution. In 24 cases it was noticed that the fourth sweet solution was called 'bitter,' 'sour,' 'bitter and sweet,' etc., accompanied by expressions of the disagreeableness of sensation. In three cases this result was obtained from the third solution.

The Sensory Tests With the Abnormal Children.

A great deal of skill and patience are required for experimentation with the backward children. They must be sufficiently acquainted with the experimenter to have the 'at home' feeling. One will be suspicious of the apparatus and will even refuse to coöperate in the suggested 'play.' Others will be overwhelmed with curiosity and inquisitiveness. One who has evidently been maltreated by teachers will unconsciously place the experimenter in this 'enemy class' and will assume accordingly the attitude, if not the insignia, of a combatant. Another will consider that he is dealing with a 'doctor,' and the difficulty of getting a pure response will be chiefly due to the unconscious prejudice. John, one of the most alert cases, displayed his insight into the situation by asking the question, "You're going to see what kind of a man I will be, are you?" The different linguistic capacities of the children also introduce an element of ambiguity into the meaning of many of the responses, and the experimenter needs to understand not only each child's vocabulary but the peculiar meaning which each word conveys to his mind. In short he ought as much as possible *to live with the children.* The children reported upon in this paper ranged in age from ten to twenty-two.

Color.—The appreciation of color is certainly not so highly developed as is sometimes thought. Of twelve carefully tested six showed unmistakable evidences of color blindness, while of the total number only two (both girls) possessed anything like an accurate color vocabulary. There were four distinct cases of green-blue blindness. One, a very backward child, who was not deemed sufficiently educable to remain in the school, showed no evidences of any color appreciation, though very persistent efforts were made to elicit such. She sorted the yarns without a mistake on the basis of their form and this in the face of the fact that their form-difference was so slight that the work was almost completed before the experimenter himself detected what was going on. Another showed after repeated tests of different kinds covering the entire school year, that her ability at color discrimination was practically limited to yellow and red. In sorting colored beads great readiness was shown by her in discriminating yellow from red while there was utter confusion on blues and greens. Beside the above-mentioned color names, brown, purple, and pink figured prominently in the color vocabularies of these children.

Color Threshold.—An experiment was devised to determine the color threshold. Color disks were arranged on the wall and the child led so far away as to be unable to detect any color. He was then led up slowly until the colors appeared in succession. The impossibility of maintaining a uniform degree of illumination, together with numerous other drawbacks in the matter of technique makes the absolute distance here of little value, but the relative results are believed to give an accurate expression of the situation. The experiment was conducted satisfactorily with seven of the cases. With each of these, red appeared first both in the first series of tests and in the second, which was taken some months later. In the first series green came second in four cases, blue being second in the other three cases. Green was third in one case and yellow in six cases. Without further particularization, the order was *red*, *green*, *blue*, *yellow*. When the series of tests was repeated some months later, red still uniformly held first place but there was a general disposition to move yellow forward, the other colors, except in one instance, maintaining the same order as before. This change in yellow was due partly to the intervening color education and partly to the fact that the child had learned that when he saw no color the color was probably yellow. Experiments of this kind are absolutely valueless, unless the experimenter is intimately acquainted with the vocabulary of the sub-ject.

Color Preference.—An effect was made to get at the children's color preferences. Many of the results were conflicting, but five had a distinct preference for red. One each preferred indigo, pink, and green. There were a few marked cases of emotional accompaniment in this color preference. Ralph called red 'my color' and told with much glee of a red, white and blue flag which he had at home. J. liked indigo best and had an indigo ribbon in her hair. Frances liked pink best and her room at home was furnished in pink. John did not like red for he was once 'scared almost to death by a red cow.' The persistence of this prejudice was shown in the fact that the same circumstantial evidence against red was produced by him four months later than the first test. E. — a cretinoid — seems to partake of the ancient Greek conception of the beautiful. On both occasions — months apart — when the color preference question was put to him he carefully arranged the yarns in symmetrical rows, but did not seem to care to commit himself to any one color. The 'pretty' or 'like best' idea seemed to be better expressed in symmetry, form and order than in color. Other evidences of this peculiarity will be cited under another head.

Hearing. — But three cases in the Physiological School had perfect hearing in so far as this could be determined by the ordinary watch tests. In most of them both ears were affected.

Pitch.—The Galton whistle tests indicate a poor ability at pitch discrimination also, the upper limit never being above 28,000, which was rare, and running down as low as 14,000 vibrations.

Sight. — The eyesight of these abnormal children is poor; about half of those tested were below the standard in keenness of vision, and some degree of astigmatism was found in every child but one or two.

Taste and Smell. — It was usually impossible to distinguish between the threshold and the discrimination point. Three cases were entirely lacking in the sense of smell, no distinction being recognized between the strongest solution of violet and cloves. Four were more or less deficient in taste, while the average threshold is much higher on all than in the normal children. Nearly 80 per cent. of the children tested recognized nothing below the third strength of quinine solution.[1] Nearly half failed entirely with sour, 65 per cent. failed on sweet, and 63 per cent. on sour until the third solution. Their detection of violet perfume was almost as accurate as with the normal children, but one only detected the first solution of cloves and no other one detected the second solution. El. called all strengths of sour solution 'water.' R. called salt 'sweet' and sour 'bitter,' and while he called quinine 'bitter,' he applied the same name to each of the other tastes, and gave no indications of especial displeasure at the strongest quinine solution. When asked, he said he didn't like it. Em. uniformly called sugar 'bitter' and sour 'sugar.' Similar phenomena have been observed by Wylie in the laboratory of the Minnesota School for Feeble-Minded.[2]

Pain. — On the right temple the algometric readings range in kilos from 1.01 to 4, half of them being above 3.25.

Temperature. — *Discrimination.* — One case was able to discriminate differences as low as 5° C.; two as low as 1° C., while the rest ranged from 2° C. to 10° C. This power of discrimination tallied very closely with the general intelligence of the cases. The standard from which measurements were taken was about 30° C.

The threshold of the sensation 'warm' was located on the C. scale at a region ranging from 18° to 32° that of 'hot' from 30° to 55°, and that of pain from 49° to 65°.

Fatigue. — Dynamometric readings range with the right hand from 5 to 63, all but the lowest cases in intelligence being above 24, while nearly all were above 30. With the left hand the readings were almost uniformly a little lower (except in

[1] See Tables I. and II.

[2] *Journal of Psycho-Asthenics*, p. 109, March, 1900.

case of the left-handed subjects). The dynamometer readings were taken after the day's work and are considerably smaller than readings obtained on days when no gymnasium work was given. This emphasizes the fact brought to light on all sides, that fatigue with backward children, as would be expected from their low vitality, is very rapid and considerable.

A little experiment was made with four cases with the double purpose of testing this fatigue and also the effect of summation of stimuli under fatigue. Fifteen successive trials were given with dynamometer in each hand. The average of each five tests are reported. During the last five the experimenter, simultaneously with the subject, went through the customary movements accompanying the gripping process. With some cases lower in intelligence than here cited, the additional energy was expended but it took the form of divers facial grimaces and other contortions not measurable with the dynamometer. The following results are of interest: Ralph: R, 29, 24, 28; L, 23.5, 17, 18. John: R, 61, 56, 58; L, 60, 58, 53. Tom: R, 30, 25.8, 26.4; L, 25, 24, 23. Joe: R, 37.6, 36.6, 34.4; L, 20.4, 19.4, 20.8. (Joe is left-handed.[1])

Touch. — An effort was made, but without very satisfactory results, to obtain information as to the sensitivity of the tactual, pressure and muscular senses. Two serious difficulties lie in the way here. The process is tedious under the most favorable conditions and the abnormal child does not possess the delicacy of attention necessary to insure reliability in results. The fatigue element is also a serious interference. Beside this the child soon lapses into a series of rhythmic responses the stress of which prevents successful procedure. By means of the method of right and wrong cases, however, some work was done with E. which is considered approximately correct.

The æsthesiometric compass reading on the forefinger was 10 mm., while there seemed to be but one touch area on the little finger. The reading on the palm was 41 mm., wrist 10.5 to 11 mm. and forearm 55 to 60 mm.

[1] The ordinary dynamometer is wholly unfit for tests with children. Mr. F. W. Smedley, of the Chicago public schools, is using an excellent dynamometer constructed for use with children.

The average error in locating spots touched on the wrist was 15 mm. with the prevailing tendency 'in' and 'up.' Spots touched on the fingers were located quite accurately. No finer discrimination was made in active weight than that between 100 g. and 60 g.

Imagery. — A great deal of time and patience were expended in the effort to determine the prevailing form of imagery in each of the abnormal children. Table III. contains some of the results obtained. The first column shows the highest number of nonsense numerals the child could repeat immediately upon their being given him *orally*. It was found that the number varies somewhat according to the character of the original impression. It was also found that the length of time such a series can be remembered depends upon the same condition. Some can do better, both as regards number and time, with a spoken series, others with a written series and so forth.[1] This suggests an important shortcoming in the ordinary method of procedure here; it at least puts an important limitation on the meaning of the responses obtained by the ordinary procedure, and this limitation has, of course, greater applicability to abnormal than normal individuals.

TABLE III.

IMAGERY OF ABNORMAL CHILDREN.

Case.	Age.		Ear.	Eye.	Ear and Voice.	Eye and Hand.	All.
E.	19	4	$\frac{57}{80}$, $\frac{4}{20}$	$\frac{49}{80}$, $\frac{2}{20}$	$\frac{56}{80}$, $\frac{1}{20}$	$\frac{37}{60}$, $\frac{0}{15}$	$\frac{74}{80}$, $\frac{13}{20}$
J.	13	4	$\frac{80}{80}$, $\frac{5}{20}$	$\frac{62}{80}$, $\frac{5}{20}$	$\frac{74}{80}$, $\frac{9}{20}$		
F.	14	5	$\frac{80}{80}$, $\frac{20}{20}$	$\frac{80}{80}$, $\frac{17}{20}$	$\frac{78}{80}$, $\frac{18}{20}$	$\frac{80}{80}$, $\frac{14}{20}$	
El.	12	5	$\frac{55}{80}$, $\frac{3}{20}$	$\frac{46}{80}$, $\frac{2}{20}$	$\frac{69}{80}$, $\frac{10}{20}$	$\frac{39}{60}$, $\frac{4}{15}$	
Em.	14	5	$\frac{80}{80}$, $\frac{20}{20}$	$\frac{57}{80}$, $\frac{4}{20}$	$\frac{52}{80}$, $\frac{5}{20}$	$\frac{73}{80}$, $\frac{7}{20}$	$\frac{57}{64}$, $\frac{3}{16}$; $\frac{44}{44}$, $\frac{11}{11}$
R.	9	5	$\frac{67}{80}$, $\frac{10}{20}$	$\frac{53}{80}$, $\frac{6}{20}$	$\frac{79}{80}$, $\frac{19}{20}$	$\frac{24}{80}$, $\frac{3}{20}$	
M.	13		$\frac{80}{80}$, $\frac{10}{20}$	$\frac{78}{80}$, $\frac{12}{20}$			
Jn.	12	6	$\frac{80}{80}$, $\frac{20}{20}$	$\frac{78}{80}$, $\frac{0}{20}$		(?)	
T.	9	4	$\frac{36}{60}$, $\frac{0}{15}$	$\frac{57}{60}$, $\frac{14}{15}$	$\frac{61}{80}$, $\frac{11}{20}$	$\frac{24}{24}$, $\frac{6}{6}$	

As it was found when numerals were memorized that no case 'broke down' under four numerals, that number was chosen and uniformly used in subsequent experimentation. Five series of tests were made. In the first, four numerals were repeated orally three times. After ten seconds the subject repeated the

[1] Cf. G. Whitehead, PSYCHOL. REV., 1896, p. 258.

series. In another ten seconds he was asked to do the same and so on, until he had had five trials at this series. This made a total of twenty possible numerals and five series. This process was repeated with a new set of numerals over and over, until the subject had a chance at a possible eighty numerals and twenty series.

The numerator of the first fraction indicates the number of numerals correctly given out of a possible eighty. The numerator of the second fraction is the number of correct series out of a possible twenty. In the second test the same *modus operandi* exactly was followed except that the numerals were *written* and the subject silently looked at them ten seconds. In the third test the subject *heard* and himself *pronounced* the numerals and then waited the customary ten seconds. In the fourth, the subject himself *copied* the numerals after which the paper was removed. In the last test all methods of stimulation were combined. The prevailing types of imagery in each subject are apparent from the table. Some of the results are quite pronounced and are verified by other forms of tests. T.'s results are not complete, but there is cumulative evidence that he is a visualizer. Three cases are perfect in the auditory form. R. does best in the vocal-motor form. It was found that the results vary considerably according as the responses were given *orally* or in *written* form. This phenomenon was so pronounced in Em.'s case — a child with a marked speech defect — that some special results are given in her 'all' column. The first fractions are her oral answers and the second fractions are her written answers on identically the same series. That is to say, she would be confused in her oral answer but would immediately proceed to write not only the numbers but the series with absolute correctness. Moreover, she was utterly unconscious of the disparity in the results thus successively given in the two distinct ways. This test was repeated some months later and the same phenomenon occurred. This case seems to be similar to one reported by Charcot, and will doubtless yield to neurological interpretation. The work showed the familiar result that in the process of breaking down of memory there occur in order: (1) a change of *order* of the numerals, the same numerals being present in the response, (2) *the dropping out* of certain ones and the *inserting* of others, (3) the *dropping out*, decreasing the number and (4) the *adding* of extra numerals. The evidences of the operation of rhythm appear not only in this statement, but are constantly cropping out in the daily work of the children.

Motor Coördination.

Table IV. contains some results in motor coördination, accuracy and rapidity. The coördination tests consisted in sorting 100 beads into four boxes on the basis of color, there being 25 each of red, yellow, blue and green beads. The beads were put into a bag and one was withdrawn at a time, the time to complete the entire work being accurately recorded. The bag was held by one hand while the sorting was done with the other. This color test was followed by a sorting test of 100 forms of the same color under like conditions. The forms consisted of cubes, cylinders, spheres and parallelepipeds. In both cases the number of mistakes made was counted, although as this was not done with all the children the results are suggestive rather than scientifically valuable. This feature should be more carefully attended to in future tests.

Accuracy. — The tests in accuracy were made upon the first joint of the forefinger and upon the axial joint. A straw twelve inches long was attached to the finger, all the other joints of the finger being stiffened except the first joint, and with eyes closed the child made the 'least possible movement.' A light fishing pole 79 inches in length was under similar conditions attached to the arm and the least perceptible movement was made from the shoulder. The subject was not allowed to grasp the pole with the hand. Three trials were given in the case of each test and the average computed. In the table these results are expressed in terms of angular measurement. It would be well in subsequent tests carefully to keep the results of the three trials *in their order*, as it is believed some significant truth would come to light here. It is by no means true that the first trial shows the lowest degree of accuracy as might be expected.

Rapidity. — In the rapidity tests a fatigue counter was used. The subject being in a comfortable position, his arm was fastened

Table IV.

Motor Coördination.

A.	Group.	Color. No.	Color. *R.*	Color. No.	Color. *L.*	Form. No.	Form. *R.*	Form. No.	Form. *L.*	Accuracy. No.	Accuracy. Finger.	Accuracy. No.	Accuracy. Arm.	Rapidity. No.	Rapidity. Finger.	Rapidity. No.	Rapidity. Arm.
	Kindergarten.	5	443	4	393	4	346.25	2	349	4	2°13′	3	1° 9′33″	4	13.6	4	23.7
	III.	12	320	8	333	7	300.3	7	285	7	2°55′47″	7	1° 9′02″	7	21.5	6	21.8
	IV.	13	293	10	289.6	10	255.6	10	262.8	11	1°47′35″	10	38′52″	13	22.9	13	29.3
	V.	5	295	1	283	1	232	1	233	1	2°37′47″	2	1° 9′02″	2	25	2	21.
	VI.	5	294							2	2°26′30″	2	1°26′20″	9	24.5	8	27.
	VII.	14	224	9	229	9	188.5	8	195	15	1°47′04″	15	22′45″	12	27.9	12	32.5
	VIII.	7	219	3	215	3	204	3	178.3	5	1°13′16″	5	22′47″	8	29.6	8	28.6
	IX.	7	190	8	211.3	8	180.6	8	178	16	0°54′	16	19′31″	11	30.5	11	35.9
	X.	9	184	9	181	6	158.8	6	126	8	0°44′	8	16′26″	7	32.4	8	36.6
B	Total.	83	263.5	52	265.7	48	247.5	45	224.6	73	1°31′	68	36′37″	72	26	73	30
C		44	254.4	44	259	44	225.3	44	224.1								

To determine the average age of the children in each group add three to the number designating the group.

to the table in such a way as to allow free movement of the forefinger only. He then manipulated the counter with the utmost rapidity possible. The test continued for 60 seconds, the readings being taken at the end of each ten seconds. The number in each ten-second interval was then computed and the average taken for the six intervals. It is this result which is found in the table, column 'Finger.' The other 'rapidity test' was similar to this except that the shoulder joint moved, the arm and wrist being stiffened. From these same results the subject's fatigue curve was computed and the results preserved on his card for future reference. By comparing the number marked by the counter during the first ten-second interval with the number marked during the sixth interval the rapidity of fatigue comes to light and is subject to mathematical statement. Some of these results are made use of later on, and this should be made a prominent feature of subsequent work.

Table V. gives corresponding results for the cases of the Physiological School. In seven instances the tests were repeated after an intermission of about three months. The results of the second trial follow in the table, immediately after the first. The columns marked 'm' contain the number of mistakes made. In the color test for the first trial 100 colors were used, 25 of each of those mentioned in connection with Table IV. Unfortunately, however, but 75 forms were used at first, 25 of each kind. Later, when the value of this work had become manifest, the experiment was reduced to a uniform system and 80 colors and forms respectively were used. This number was chosen, as it was found that 100 makes too great a tax upon the attention of backward children. Each series of results is, therefore, inherently valuable, but the series are not subject to reliable comparison among themselves. The 'accuracy' and 'rapidity' sections in Table V. are constructed in the same way as the corresponding sections in Table IV. Under accuracy, however, it will be observed that two columns are given to the finger and two to the arm. It was found that these children could only understand what was required of them in this test by first allowing them to perform the work with the *eyes open*. The second column in each case gives the results taken immediately after-

TABLE V.

MOTOR COÖRDINATION. PHYSIOLOGICAL SCHOOL.

Case.	Color.				Form.				Cards.			Accuracy.				Rapidity.		Fatigue.	
	R.	*M.*	*L.*	*M.*	*R.*	*M.*	*L.*	*M.*	Colors.	Forms.	Pictures.	*F—o.*	*F—cl.*	*A—o.*	*A—cl.*	*F.*	*A.*	*F.*	*A.*
E.	360	1			230	5			127	130	115	2° 5′ 14″	3° 34′ 35″	0° 30′	2° 37′ 37″	35	29	75%	Inc.
R. S.	358	2	366	0	372	1	290	2	152	187	134	1° 47′ 22″	5° 3′ 35″	1° 13′ 24″		21	19	66	75
T.	360	14			225	11			(?)	160	115	2° 5′ 14″	5° 3′ 35″	1° 16′ 8″	3° 48′	21	23	36	84
T.	244	1	340	11	270	11	237	5								26	20	31	40
Jn.	250	1	310	1	221	3	207	1	105	125	112	1° 29′ 30″	1° 47′ 22″	1° 21′ 33″	2° 43′	28	26	66	77
Jn.	270	1	275	1	237	2	207	0	82	101	114	3° 16′ 43″	5° 21′ 20″	1° 2′ 31″	4° 4′ 20″	30	20	87	76
J.	320		430		250		230		178	180	155	2° 41′	17° 53′ 45″	1° 59′ 35″	4° 45′	28	22	51	54
J.	352	9	325	1	282	1	274	1	145	152	200	1° 29′ 30″	5° 56′ 47″	1° 27″	1° 51′	22	25	Inc.	90
F.	345	11	488	11	223	1	240	12	85	100	93	2° 41′	7° 42′ 41″	2° 15′ 53″	4° 9′ 45″	23	22	81	91
F.	332	4	325	4	230	0	260	0	102	97	88	1° 29′ 30″	2° 58′ 50″	1° 51′ 27″	2° 37′ 37″	25	22	34	71
R. N.	380	2	410		200		228		73	60	62	2° 23′ 8″	9° 27′ 43″	1° 29′ 42″	1° 29′ 42″	21	24	72	81
R. N.	266	1	252	2	216	2	230	2	55	102	84	1° 47′ 22″	2° 23′ 8″	0° 49′	2° 30′ 30″	20	23	70	Inc.
El.	600	16	480	28	300	2	210	4	(?)	160	170	7° 7′ 30″	14° 3′ 10″	2° 29′ 26″	5° 47′	14	9	Inc.	Inc.
El.	455	many	450	25	275		263	0	120	115	130	4° 45′ 44″	8° 0′ 14″	2° 15′ 53″	2° 54′	19	14	46	83
Em.	400	5							180	215	245	2° 23′ 8″	3° 34′ 35″	1° 24′ 16″	1° 29′ 42″	10	22	Inc.	Inc.
Em.	450	6	430	2	383	3	380	3	160			2° 23′ 8″	4° 28′	1° 24′ 16″	1° 51′	10	19	(?)	(?)
1st av.	379		425		236		223		112	125	118					22	21		
2d av.[1]	335		345		268		263		109	113	87					$21\frac{5}{7}$	$20\frac{3}{7}$		

[1] J being left-handed is omitted from the average.

1st trial = 100 colors, 25 each. 1st trial = 75 forms, 3 kinds. 40 of each kind of cards.
2d trial = 80 colors, 20 each. 2d trial = 80 forms, 4 kinds.

ward with the eyes closed. The 'Fatigue' column in this table gives in per cents. the value of the numbers recorded by the counter during the last ten seconds, in terms of the number recorded by the counter during the first ten seconds. The letters 'inc.' mean that in the instance cited the last number was larger than the first number. This does not mean so much an absence of fatigue, as an increase in skill in the required manipulation.

Tests were also given with three sets of cards. There were forty cards in each set. In one set, ten each of blue, red, green and yellow disks had been pasted on the cards. In another set there were printed ten each of four familiar pictures, *i. e.*, a boy, a hand, an ox-head and an eye. In the third set there were printed on ten cards a triangle, on ten a square, on ten a parallelogram and on ten a circle. The columns of the table contain the number of seconds required to sort the different sets. The card tests, though they have been much used, are essentially misleading. They have their value, but it is not that usually attributed to them. They afford a crude means of determining reaction time when the afferent current is visual. They also indicate a child's relative appreciation at a given moment for pictures, colors and forms, which is of pedagogical interest. But it is unpsychological to suppose that they offer any reliable comparison between color and form perception, since they entirely ignore all elements of form appreciation except that by means of vision!

We call attention first to Table IV., *B*. Here the results are given in totals. The actual results here are not considered of importance, for all the experiments of a given kind are dumped together regardless of the age or advancement of the children. But the results in the larger view, as gathered from Table IV. and Table V., have this importance, that they show in every case the abnormal children take more time for coördination, are less rapid and accurate in movement, and more rapid in fatigue. Both normal and abnormal children agree in taking *more time for color coördination than form coördination* — that is when the beads are used, and as for the cards they offer no test of the tactual sense so invaluable in form appreciation.

There is a vast difference between feeling and seeing forms. The chief value of the card results, so far as they throw light upon color and form appreciation, is this negative consideration. Both classes of children agree also in *more rapid form discrimination with left hand* and *more rapid color discrimination when the right hand is used.* This will be referred to again. It will also be seen that both classes of children are *more accurate with the large than with the small joint.* With the abnormal children the finger joint has a fractional advantage in the rapidity test, but as the apparatus for testing the axial joint was somewhat complex, these children had difficulty in keeping it going. The lower average is probably due to this fact.

In Table IV. the normal children are arranged on the basis of their standing expressed in terms of groups in the Elementary School. From each of the four columns showing coördination times it will be seen that there is a steady and uninterrupted decrease in the time of coördination from the kindergarten to the tenth group. The only exceptions to this statement are found in the fifth and sixth groups, color, right hand, but it will be observed here (*a*) that the number tested in these two groups is quite small and (*b*) even in spite of these facts, if the results of one pupil were taken out in each case, the totals then obtained would swing readily into line. The well-demonstrated fact must not be overlooked also, that children of this age are undergoing certain very definite and rapid physiological changes. The general principle stated above applies, though not with such absolute uniformity, in the cases of accuracy and rapidity. The fact much emphasized in recent writings is also worthy of notice — and this is particularly important educationally speaking — that children are *more accurate in their movements and move with greater rapidity when using the shoulder joint than the finger joint.* This suggests the necessity of what is already becoming recognized in some places, the necessity of a complete readjustment of kindergarten methods to the freer and larger movements for which the child is physiologically prepared. The setting of children to tasks requiring fine finger movements and delicate discrimination is certainly a species of malpractice. The figures also show that as children increase in wisdom and stature they become comparatively more dexterous with the finger, although even in the tenth group the arm has the absolute advantage.

In the course of such experiments as the above, the experimenter has over and over again many opportunities for getting a practical picture of the child's mental alertness and mental habits as well as his ethical motives. A fruitful field in which the latter crops out is found when the child picks up two beads, after having been strictly enjoined to take but one at a time. The numerous alternatives taken in this particular crisis are very significant indeed. A comparison of the coördination time with the right and that of the left hand disclosed some interesting results. A hint at the question is obtained from Table IV., *B*, and again from Table V., '2d *av*.' In both cases it appears that the longest time for sorting is with colors and the left hand. The colors right hand comes next, and quite below *these* figures come in order the forms right hand and finally for the shortest time the forms left hand. When it is remembered that Table IV., includes all ages and that only about one half of the normal children were given each of the four tests it may be concluded that the results have no significance. The average, made up from data representing such children as those reported in Table V., if taken alone, would certainly be no trustworthy guide to scientific truth. To be sure of the situation the results for 44 right-handed children are given in Table IV., *C*. Each of these 44 children took the four tests. The *relative numbers* are the same as shown in the tables just cited. Of the 44 cases 20 actually sorted the forms sooner with the left hand and the margin was close in some of the other cases. Of the same 44 cases 28 sorted the colors more quickly with the right hand and 8 of the others took the experiment with the left hand from 2 to 4 months after that with the right hand. This gave the left hand a decided advantage. In several cases where children were using forms with the left hand it was clearly apparent that the motor operation could not keep up with the tactual discrimination and numerous mistakes were made on this account. More mistakes were made with the forms than with the colors and this result is believed to be largely due to this fact.

Table VI. shows four left-handed children, '*II.*' and '*III.*' of whom, however, have been taught to use their right hands. These children were more proficient both in colors and forms

TABLE VI.

COÖRDINATION. LEFT-HANDED CHILDREN.

Subject.	Color.		3 Forms.		4 Forms.	
	R.	*L.*	*R.*	*L.*	*R.*	*L.*
I.	261	241	153	155	225	204
II.	198	220	112	110	160	177
III.	200	210	115	125		
IV.	237	270	168	137	208	191

with the right hand, as will be seen. '*I*,' however, who has remained left-handed, illustrates our hypothesis completely, the times being in order of decrease; colors right, colors left, forms left, forms right! This is when three forms were used. When the experiment was tried with four forms the results are more ambiguous. This matter is worthy of attention. Several other children reversed their order when four forms were used from that obtained when three forms were used. As the number of forms increases from three to four the motor phase of the coördination problem rises in complexity. There are 24 possible chances here while there are but six in the other case. This is illustrated in the possible ways of arranging the boxes. This side of the problem then is increased in complexity 400 per cent. Now the tactual phase of the problem is not so much affected — only to the extent of 25 per cent. This consideration seems to throw weight in favor of the proposition that in those children who sorted forms more readily with the right hand, the *motor* facility turns the balance — the results might be accounted for even if it were proved that the children had finer sensitivity to touch with the unused hand. Subject '*IV*' in the last table is a boy who is not only right-handed, but who insists he has particular difficulty in using his left hand. He is slower in colors with his left hand and swifter on forms with his left hand, both when three forms are used and when four forms are used.

With considerable unanimity the evidence seems to point one way. One point at least is beyond question, *i. e.*, in the given tests *children discriminate form more readily than color.* In about 100 tests there have been no exceptions to this statement. It is true that in the tests given the form experiment had one advantage in its favor because the child perceives the form immediately upon contact *in the bag* and may at once begin the motor phase of his task. It is not believed that this is the whole story, although this suggests the necessity of a different method of counting time in the experiment. There are strong indications, especially with abnormal children, that form appreciation chronologically precedes color appreciation. Witness, for instance, the cases cited on page 351. Bearing upon this point of the comparative priority and sensitivity of touch and other forms of sensation Dr. Martin W. Barr, chief physician of the Pennsylvania Training School for Feeble-Minded says[1]: "The basis of this scheme of development is the recognition of *touch* as the most sensitive as well as the most reactive of all senses; therefore we utilize it as the master key which shall set free the powers of the head — the hand — the heart." The other point which appears to be true is that the sense of touch is more delicate — at least relatively more delicate as compared with the motor power in the left hand of the right-handed people and the right hand of left-handed people. The interpretation of results of this kind is no simple matter. It is difficult to draw the line between the tactual discrimination and the motor control. It seems impossible to get either in perfect isolation. It would appear that with a right-handed person the right hand would have the same advantage with forms as with color. It may be that the left hand had a slight advantage on the score of practice, for it was the rule to take the right hand first. This statement could certainly be questioned, however, in the light of our knowledge in other psychological fields. This advantage, if any, was greatly counteracted, however, by changing the position of the boxes between tests. Furthermore, it would appear that practice should help in color as well as in form. Another factor, however, worked in the opposite direction to practice. That was fatigue.

[1] 'The How, the Why, and the Wherefore of the Training of Feeble-Minded Children.' *Journal of Psycho-Asthenics*, September, 1899.

In the younger children fatigue would tend to outweigh the value of habit. The novelty of the work soon wore off for them. On the contrary, with the older children the interest was maintained and habit may have helped in the solution of the problem. Further facts will throw light upon the question. The hypothesis is perfectly plausible, theoretically, when there are taken into consideration the comparative passivity of the unused hand and the persistent interpretative mood of the used hand. The theory, moreover, does not require that the unused hand shall be actually more sensitive to touch, but that when the algebraic sum of the sensitive qualities is obtained the tactual has a larger balance in its favor.

At this juncture there is submitted another table, VII., which gives the record of eleven pupils in the Elementary School who were subjects in these experiments in two successive years. The results are shown side by side. Each of these children has been promoted a group during the year and we find a *corresponding increase of ability in the lines of our inquiry.* The table plays very readily into the hands of the general relation already indicated of the power of attention to motor coördination. Table VII., also indicates the value of a method already suggested of studying children which the 'Science of Child-study' has thus far practically ignored. The effort heretofore has been to give a cross-section view of a larger number of children. This method suggests a longitudinal view of a few children as likely to be of more value. If the spider were tested by the cross-section method in motor coördination, some results quite derogatory to the child would be obtained. If the longitudinal method were applied to the spider and the child, the outcome could be easily prognosticated. There is about as much reason in measuring one child by another child as in measuring one child by a spider. The cross-section method is static and has little real meaning. This little meaning will be buried effectually, if the result is dumped in with a mixed multitude of others. The longitudinal method is dynamic; it is a measure of *progress.* The differences, not the resemblances, are the chief value, pedagogically at least. For such a purpose the 'average' is worthless; it is impersonal. When it is

TABLE VII.

Name.	Coordination.		Accuracy.				Rapidity.				Fatigue.			
	1899.	1900.	1899.		1900.		1899.		1900.		1899.		1900.	
	Colors.	Colors.	Finger.	Arm.	Finger.	Arm.	*F.*	*A.*	*F.*	*A.*	*F.*	*A.*	*F.*	*A.*
Clifford	249	189	1° 2′ 8″	34′ 16″	26′ 13″	32′ 33″	33	34	35	35	I	97	82	87
Edwin	209	208	0° 53′	20′ 33″	23′ 29″	11′ 19″	27	33	29	35	76	66	85	82
Fletcher	245	158	1° 46′	18′ 49″	33′ 49″	2′ 51″	23	16	28	35	36	I	80	90
William	206	162	26′ 31″	0° 12′	41′ 18″	13′ 8″	43	44	38	46	70	97	86	73
Barrett	290	222	4° 8′	23′ 55″	1° 7′ 38″	17′ 6″	31	29	30	34	46	51	86	76
Howard	304	205	1° 2′ 8″	18′ 49″	3° 11′ 30″	25′ 31″	27	27.5	28	30	66	80	72	70
Mary		191	44′ 20″	0° 12′	0° 30′	14′ 16″	31	32.5	30	37	82	50	50	86
Donald	252	213	0° 53′	30′ 49″	29′ 36″	10′ 49″	26		32	43	60		48	70
Henry	399	244			35′ 20″	15′ 24″	30	30	22	27			82	87
Stephen	460	285			2° 37′ 47″	59′ 6″	23	15	21	24	72	68	87	88
Lander	260	225	1° 18′ 33″	37′ 41″	1° 18′ 33″	17′ 6″	12	21	22	29	75	80	72	E

NOTE.—'I' = increase, 'E' = even.

considered that each individual is unique in temperament, rapidity and order of his nascent periods, heredity, environment and personal equation, the inadequacy of any 'average' standard is made manifest. The individual's progress should be measured by the possibilities of his own self-realization.

In the discussion thus far we have had under consideration as a rule no smaller unit than the group. Of course it is desirable, if the pedagogical side of the purpose is to be made practicable, to devise a system which will have individual applicability. In this kind of a device two things must be kept in mind. One is this longitudinal method and the other is that the tests, if they are to be of value without repetition, must cover a sufficiently long period of time, or if short in duration, must be of a sufficient degree of complexity. Two of the tests of Mr. Kirkpatrick[1] appear to me to be of questionable value for purposes of individual classification. In one he requires the children to count orally as rapidly as possible for ten seconds; in the other, to make vertical marks as rapidly as possible for ten seconds. The correspondence between the results obtained in this way and those given by the teachers is certainly so remarkable as to throw the burden of proof on any one who might attempt to criticise the tests. But I have not been equally fortunate. My results obtained by the use of the fatigue counter, the readings being taken every ten seconds for a minute, indicate no trustworthy correspondence between the work of the first ten seconds and the child's real mental power, so far as it may be applied to any practical problem of life. It is not only easily conceivable, but results in this study show it to be the case, that the 'C' grade pupils make excellent records for the first ten seconds. Under the novelty of the experiment they pull themselves together and work on borrowed energy as their fatigue curves show. They are among those who have no root in themselves and so endure but for a time. Of 16 pupils marked 'A' by the teachers of the Elementary School ten were graded 'A' by my scale of 'coördination,' 'accuracy' and 'rapidity' results. This is as close a correspondence as any two teachers would be likely to reach in marking the same children, especially when

[1] Psychological Review, June, 1900.

it is remembered that the Elementary School pupils are already grouped on the basis of ability alone.[1] But when I came to the pupils marked 'C' by their teachers, of whom there were 11, only two are put 'C' by my system. The 'C' pupils work better at my tasks than they do at the tasks the teachers put them at. This suggests a serious weakness found in every device thus far suggested, so far as the writer knows. The fact of there being in the schools two distinct types of children, *i. e.*, the sensory child and the motor child is ignored. If a motor child does not do the ordinary sensory tasks of the school well is it the fault of the child or the requirement? Leaving for the moment the question of requirements untouched, it seems clear enough either that there must be a different series of tests for the two different classes of children, or there must be found some common denominator in terms of which each class may be evaluated. The standard must not be chosen arbitrarily and it may be arbitrary and still have the custom of centuries behind it. This paper does not claim to have found that standard. If, however, these same 'A' and 'C' pupils are compared on the basis of the fatigue tests, the correspondence sought is marked. These results are computed from the 'rapidity' test, the record of the first and sixth ten second interval being compared. The figures given in Table VIII. show the per cent. which the sixth interval bears to the first interval.

It will be observed first that the 'C's' fatigue more readily than the 'A's,' there being a difference of more than 10 per cent. in both finger and arm in favor of the 'A's.' Both classes fatigue more readily with the finger than with the arm. Of the 10 'C's,' 6 fatigued more readily with the finger. One maintained his rapidity of arm movement and one increased it during the minute. Of the 13 'A's,' 7 fatigued more with the finger, one maintained his rate with the arm and one increased it, while two increased the rate with the finger. The general lesson of the table is clear and unmistakable. While it may be necessary to vary the tests to meet the peculiarities of the motor

[1] Two of the leading teachers of the Elementary School were asked to grade a given list of pupils and there was a considerable degree of variation in the results.

TABLE VIII.

FATIGUE.

No.	'C' Pupils.		No.	'A' Pupils.	
	Finger.	Arm.		Finger.	Arm.
1	62	55.5	1	88	67
2	66	76	2	73	96
3	48	70	3	93	96
4	94	70	4	67	59
5	82	100	5	66	90
6	76	79	6	85	82
7	89	79	7	86	73
8	53	74	8	142	112
9	89	55.5	9	70	90
10	91	105	10	81	91
			11	91	100
Av.	77	76.4	12	86	87
			13	107	97
			Av.	87.2	88

and sensory child, it would appear that the element of fatigue, if once understood and properly applied, would serve as a practicable common denominator.

The Emotions. — A study of the emotional life of abnormal children is of peculiar interest. While the cases in the Physiological School are emotionally erratic as all such children are, they, nevertheless, on the whole possess emotional tendencies. They are much less plastic in their adaptations to their surroundings than are normal children. But for detailed knowledge of the facts of their emotional life we must wait on empirical observation. Experimentation is of limited value.

The abnormal children though lacking intensity in their psychical powers, seem to possess the extent of the normal individual, so that Ireland seems to be justified in saying: 'I do not know of any power which existed in the mind of Shakespeare or Napoleon of which they are totally destitute.' It is also significant that throughout the whole range of the psychic processes here studied the laws of procedure harmonize in an unexpected degree with those already determined for normal adults.

SUMMARY.

The following considerations seem to come prominently to light from the work thus far done:

1. There is need of frequent psycho-physical examinations of children. This need applies not only to the neglected classes but to all classes regardless of social condition. Children of the most cultured, the most wealthy and the most alert parents frequently suffer not only physically but mentally because of the unknown physical defects which such an examination would readily bring to light. The amount of mental anguish which children suffer entirely on account of ignorance of parents and teachers is by no means duly appreciated.

2. The prosecution of this kind of work will soon result in the establishment of norms in terms of which a child can readily be scientifically classed for pedagogical purposes.

3. The work of the psychological laboratory is demonstrating that pedagogy will never become a science in truth, until the principle of individualization becomes its watchword.

4. Approximate uniformity of results in psychical reactions is a characteristic of the healthy consciousness. Inability to secure this uniformity is at once a sign of a neurotic condition, which if neglected may become permanent.

5. The study of 'what is in a child's mind' at a given moment is of very questionable scientific value; it is scarcely a psychological study at all. The most of the content of a child's mind at any stated time is determined by his environment. A child of arrested development has a well-developed *automobile consciousness*. He has power of imagery with reference to this machine, visual, and auditory, and motor, which measured in terms of race development alone would indicate a degree of intelligence far advanced. The child's 'religious ideas,' 'ethical ideas,' and so forth are largely the ideas of his elders unconsciously appropriated.

6. The scientific value of tests made on children by those unfamiliar with their habits, vocabularies, environments, etc., is very slight and this value reaches its minimum when the reports are dumped in with others of like character.

7. These tests with both classes of children agree in indicating that touch is a more primitive sense than color. It develops first and maintains its precedence for some years. (How long is as yet undetermined.)

8. Bright colors are generally preferred by these abnormal children.

(The experiments were not extended to the normal children.)

9. The grosser movements of the body develop before the finer ones. There is greater accuracy and rapidity of movement with the shoulder than with the finger and this rule is followed by children up to the highest group (tenth) in the Elementary School.

10. There is a uniform increase of ability at motor coördination as the intelligence rises. This runs through the groups and applies to individual cases.

11. The stress of motor imitation is so strong as frequently to overcome the deteriorating effects of fatigue. See dynamometric readings, page 354.

12. The lower the intelligence the more prominent the element of fatigue appears.

13. The effect of tendencies to rhythm in conscious activity is a very considerable though almost neglected factor in the attempt to teach children.

14. The chapter in children's imagery is yet unwritten.

15. It is quite possible for the simple motor test which discloses the degree of intelligence to be so conducted as to give valuable ethical data as well.

16. The abnormal child is deficient in intensity and not in extent of psychic function.

17. An interesting question is raised as to whether the sense of touch is not relatively more delicate than motor ability in the left hand of right-handed individuals and *vice versa*.

THE DISTRIBUTION OF ATTENTION. I.

BY DR. J. P. HYLAN.

1. Recent Studies.[1]

The question as to whether the attention can be distributed or divided has recently found interesting experimental treatment by Jastrow and De Sanctis. Jastrow[2] selected two types of processes to investigate their amount of mutual interference when both were carried on at the same time. The first type consisted of finger movements involving rhythm and counting. The beating of the finger was recorded upon a smoked drum; while the second type, consisting of adding or reading under various conditions, accompanied this. It was found that when the subject chose a convenient rate for the finger movements, the adding or reading did not interfere with them; but when the subject was required to tap at his maximum rate, the mental task always caused an interference. Also simple movements were less affected than those complicated with a rhythm, and reading aloud was more disturbing than reading to one's self. The rate of performing the mental work was measured by taking the time necessary to perform a definite task. Simple regular beats did not increase the time of the mental process, but seemed to hurry it up. But when a rhythm was used in the movements, the time was increased, depending upon the complexity of the movement. The greater the number of beats in a rhythmical group, the greater was the interference. The reading of disconnected words was interfered with more than that of words which made sense. These results show that the more simple and automatic the processes, the less is the mutual disturbance; while the more complicated ones, whether pri-

[1] A concise historical review of this subject may be found in James' 'Psychology' in the chapter on attention. A recent and more extended discussion is by W. Wirth, *Philos. Studien*, XX. (Wundt's 'Festschrift,' II.), pp. 487–669.

[2] Jastrow, Joseph, 'The Interference of Mental Processes,' *Am. Jour. Psych.*, Vol. IV., 1891–92, p. 219.

marily mental or physiological, cause a very pronounced interference.

De Sanctis,[1] whose interest has been especially in the pathological aspects of the subject, has conducted several experiments with regard to the power of fixating and dividing the attention. One method of testing the fixation of attention was to keep the subject busy making movements with the finger, and keep time with the strokes of a metronome, upon which the whole of the attention was turned. These movements were registered upon a smoked drum. While the subject was thus engaged, continually stronger and more numerous distractions were given, and he was told to close an electric circuit whenever these distracting conditions caused an interference. The distribution of the attention was tested in a similar way by employing the subject with two, three or more operations, and directing him to give an equal degree of attention to all at the same time. The movements were registered, and the time employed for the operations was exactly measured.

The objection to this experiment is that one of the processes required no constant attention. Work like Jastrow's indicates how automatic and unconscious the tapping in time with a metronome may be. Movements of this kind are so easily fallen in with that they soon come to require hardly more attention than walking or breathing. Hence the supposition that these simple movements constantly retain a part of the attention is groundless.

Another way of testing the fixation and distribution of the attention, and one which the author regards as clinching the distinction between them, was to use a perimeter, having the subject fixate upon the fixation mark of the apparatus. To test

[1] De Sanctis, Sante, 'Studien über die Aufmerksamkeit,' *Zeitsch. für Psychol.*, Band 17, S. 204.

'L'attenzione e i suoi disturbi. Saggio di psicopatologia clinca,' *Atti della Soc. Rom. di Anthropol.*, IV., 1, 468, 1896. Reviewed by Külpe in *Zeitsch. für Psychol.*, Band 15, S. 144.

'Ricerche psicofisiologiche sull'attenzione dei normali e dei psicopatici,' *Bul. Soc. Lancisiana*, XVII., 2. Also reviewed by Külpe in *Zeitsch. für Psychol.*, Band 19, S. 234.

'Lo studio dell'attenzione conativa.' *Atti della Soc. Rom. di Anthropologia*, IV., 2. Reviewed by Binet, *L'Année Pyschologique*, IV., 1897, p. 581.

the fixation of attention, the extent of the field of vision was ascertained when the subject was having read to him an interesting story, or being painfully pricked. To do this, supposing the right eye to be used, the slide on the perimeter would be moved from the right temporal to the nasal side until seen. Successive trials of this kind were used to determine to what extent the visual field was contracted by the distraction. As the meridians are numbered from o at the point of fixation towards the temporal side, the greater the reading the larger was the field of vision. In testing the division of the attention, the subject was required to count points, marks or circles exhibited at the point of fixation, and at the same time to attend to the field of vision and note when the object in the lateral field appeared. First, the subject's field was tested under normal conditions with the attention neither distracted nor divided, and the result compared with that when distraction or division was present. An examination of the results of this experiment shows that the extent of the field was somewhat less for distracted than for normal attention; while the average extent for divided attention was less than half that for the normal. The mean variation here also rises to about half the average. According to our author, the contraction of the field is evidence that in divided attention the disturbances are greater than in distraction, and shows conclusively the difference between the concentration and division of attention. Also the division of attention is attended with greater difficulty than its concentration, and in mental disintegration, as with paralytics, the insane, hysterical and aged persons, the power becomes vitiated or lost. The power of distribution thus becomes a prominent feature for psychogenesis, for, developing out of the simpler process of concentration, its accession marks the growth of a greater will power, and the ability to become acquainted with more objects in a shorter time.

I have gone into some of the details of this work because it is on an important subject, and has attracted a good deal of attention, especially in Europe. There is a serious criticism to be made regarding it. It is that the experiments are altogether too crude to deal with the subtle conditions of the problem.

The evident objection to the perimeter experiment is that it is impossible to tie the attention down when there are no distractions, while the difficulty is more obvious with them. It is continually flitting back and forth with reaches of varying breadth and minuteness, and with changing frequency and rapidity. What the experiment with distraction tested was the time and frequency of the fluctuations of the attention from the temporal field with the distracting stimulus as compared with these when the distraction was absent. With an effort to keep in mind the temporal field, the attention was intermittently diverted from it by the distraction, and the object in the temporal field allowed to move in towards the nasal side farther without being noticed than was the case when the distraction was not present. When an effort was made to divide the attention between the lateral field and the fixation point, the fluctuation was naturally more frequent than before, and at the expense of the lateral field, which now retained it for shorter periods and at longer intervals, thus allowing the object to move still farther without being noticed. Another condition which contributed to this result was the fact that counting involved a much more continuous mental effort than the listening to a story or feeling one's self pinched, the agencies used in the distraction experiment. Hence, on this account also, the lateral field was, in the case of division, more neglected. It is but to be expected, therefore, that under these conditions the object in the temporal field could often be moved very far towards the nasal side without being noticed, or that it sometimes reached, as the figures show, clear to the o meridian.

This interpretation is suggested by an experience of several years which have been devoted to experimental study of problems of attention. It is supported by the extraordinary variation shown in the division experiment. If the attention were really divided, why should the object in the temporal field be seen sometimes as quickly as under normal conditions, while at other times it was not seen until it had reached the median plane? Evidently because sometimes the attention was on the temporal field, while at other times it was not.

What becomes, then, of De Sanctis' theory and its significance for psychogenesis? Whatever may be its foundation upon other data, it certainly is not justified by these experiments. Any one can readily discover the great effort necessary to balance one's attention so that its fluctuations shall be rapid and regular between two or more objects. The difficulty of this feat is a sufficient reason why the demented are unable to perform it.

Other authors who should be mentioned are Münsterberg, Binet, Loeb and Krohn. If we discover the time (by the reaction method) needed for naming a member of a single class of objects, as giving the name of an American novelist, and also the time needed for making a comparison, as saying which is liked better, Irving or Cooper, we shall have the time for two different mental acts, each determined separately. The subject may now be required to do both these acts in one, as by telling which of the American novelists he likes best, and the time taken for the combined process. Münsterberg combined acts in this way, and found that while it took 103 σ to name a particular member of a class, and 922 σ to make a comparison, it required but 1,049 σ to do both together, a saving of 46 σ over the sum of the times when done separately.[1]

Binet required his subjects to press regularly a closed rubber tube which caused a tracing on a smoked drum. While this was being done the subject was required to execute some mental work, like reciting by heart, or performing a mathematical calculation. It was found that these operations greatly disturbed the regularity of the compressions. When different tasks were given to the two hands to do at the same time, as that of making two different outline drawings, one drawing would be deformed by similarities from the other. A voluntary process, however, could well be accompanied by one that was purely automatic.[2] Loeb describes experiments quite in accord with these results, and also with those of Jastrow. The rhythm in turning a wheel and that of repeating at the same time verses by heart were found to coincide, or one to be a multiple of the other, and without mutual interference; while raising the pressure on a dynamometer to its maximum was

[1] 'Beiträge,' I., S., 64–188.

[2] 'La Concurrence des états psychologiques,' *Revue Philosophique*, Vol. XXIX. (1890), p. 138.

found to interfere with the performance of mental number work. Thus two simultaneous, maximal, aperiodic processes of innervation which require effort were found to disturb each other.[1] Krohn gave ten simultaneous touch sensations to different parts of the body. Tambours carrying corks as instruments of touch were used, which immediately withdrew when the stimuli had been given. It was found that if the points of contact were somewhat scattered, and the subject attended closely, six out of seven simultaneous touches could be clearly grasped and correctly localized. It should be said, however, that after-images of touch were very persistent and were used to a considerable extent in locating the sensations.[2]

These experiments suggest the need of more critical and exhaustive methods of studying the problem in order that the purely mental factors involved may be given more accurate measurement. Other experimenters will be discussed as occasion suggests. There is also a class of pathological cases which has been interpreted as illustrating a doubling of the mental process. Lack of space prevents their discussion here, although I believe they differ from the normal in degree rather than in kind. At the present time there is an almost universal tendency to believe that the power of division is possessed by the normal mind. We have now to consider some new evidence bearing upon this question.

2. Counting Simultaneous Series of Similar Impressions.

From the foregoing discussion it is evident that two conditions must be fulfilled in devising experiments to test the division of attention. First, no dependence should be placed upon the subject's ability voluntarily to divide his attention; and second, if the mental work employed is continuous, the purely mental processes involved, the simultaneity of which is to be tested, must be accurately measured. In conformity with these principles, the following experiments were planned and executed:

[1] 'Comparative Physiology of the Brain and Comparative Psychology,' p. 289, *et seq.*

[2] Krohn, W. O., 'An Experimental Study of Simultaneous Stimulations of the Sense of Touch,' *Jour. of Nerv. and Mental Diseases*, 1893.

A revolving kymograph drum held horizontally had placed upon it a paper having two series of short horizontal lines extending partly around the drum. A screen, placed closely in front of this, had in it two small openings a centimeter apart, and so placed that when the drum revolved, the lines upon the same could be seen one at a time through the openings; each series of lines being adjusted to its own opening. A fixation mark was placed half way between the openings, and the subjects arranged at a convenient distance for counting the lines. With the eyes of the subject fixed upon the fixation mark and the attention upon one opening, the other being closed, the drum was started at too rapid a rate for correct counting of the lines. A signal was given before the first line appeared, and the rate gradually adjusted for the successive repetitions of the series, so that the maximum rate of counting at which the subject could count correctly and feel a fair amount of certainty in the correctness of his work was ascertained. The exact number of lines was known only to the experimenter. This process was repeated five times for each subject. The time taken for the passage of the whole series past the screen opening was taken with a stop-watch, and this divided by the number of lines in the series, giving the time for each line. The average and mean variation of this for the different trials were then reckoned.

In a similar way, with the eyes fixed as before, and both screen openings in use, the time was taken for counting the lines which appeared in both openings, both series being exposed simultaneously. In this part of the experiment the subjects were directed to divide their attention, if this were possible, between the two openings. Four subjects took part in this experiment, two men and two women, all young, vigorous, and having had some training in experimental work. The mental part of the process was never so rapid as to be delayed by the natural motor accompaniment in the vocal organs, and the number counted was always approximately the same, aggregating from thirty to forty, either when one or more than one series was used, in order to avoid inequalities of fatigue. When more than one series of lines was used this number was divided about equally between the different series. As it was found impossible to keep a separate count for each series when more than one was used at once, a single count was kept for all.

In addition to this double series, a triple and also a quadruple one were used, having three and four openings respectively, for the purpose of ascertaining the effect of an effort for the greater distribution of the attention, and comparing this with the single series. Similar methods and precautions were used here as those already mentioned, although only three instead of four subjects took part.

At first, when more than one opening was used, there was a strong tendency for the eyes to turn directly to the openings in response to the lines as they appeared. After practice had served to correct this, there still remained a responsive shifting of the attention which could not be prevented. A rhythm appeared in the counting which materially aided its rapidity. The nature of this rhythm depended largely upon the order in which the lines appeared, and was hence facilitated by an acquaintance with the series. A feeling of certainty in the correctness of the counting arose in connection with this rhythm, and depended upon the coincidence of the rhythmic beats with the appearance of the lines.

Since the time required for the passage of the lines as a whole was taken for each series, and the average time for the counting of each line in each series was reckoned from this, we have a basis of comparison for the different series. When the time of the different subjects was averaged, the time required to count a single line was found to be 437 σ for the single series, 307 σ for the double series, 278 σ for the triple series, and 1021 σ for the quadruple series. The mean variations were 9 σ, 14 σ, 11 σ and 8 σ respectively. No marked tendency to improve the rate by practice showed. In the single and double series, the order in which the lines were shown presented no difficulty in being formed into a rhythm that could be remembered. This was also true of the triple series if the order were not too complicated. A triple series, made too irregular to allow being remembered, was used, which required 483 σ, or nearly twice the time required for the simpler triple series already reported. The mean variation for this was but 6 σ. The quadruple series was also too complicated to be remembered.

In order to control any details of this result which might come from the peculiarities of the visual sense, the problem was also approached by means of auditory sensations. A toothed wheel was made to revolve by an electric motor, the speed being reduced by an extensive gearing. This, with the additional assistance of a resistance bridge, made it possible to vary the rate of rotation as desired. A light steel spring was clamped in such a position that as each tooth of the wheel passed, a musical tone, having a distinct pitch, was sounded. One side of the wheel was partly covered by a non-conductor, and a metallic arm made to press against the side of this as it revolved. The arm and wheel were put into an electric circuit with a sounder, which was thus made to give a signal for the subject to start and to stop counting the clicks. The breaking of the current, causing the signal for starting, was made by the passing of the non-conducting section of the wheel by the arm; and the making of the current, causing the signal for stopping, was caused by the reinstated contact through the wheel.

The method of procedure was the same as that for the last experiment. The motor was first started briskly and then slowed down until the subject could give correctly the number of clicks sounded. This was repeated five times and very great care taken to make each determination accurate. A second, third and fourth series was formed by having two, three and four springs respectively used at once, each giving a distinctly different pitch, none sounding at the same time, but in succession. The tendency to form a rhythm was especially pronounced in this experiment. Since a distinct rhythm tended to make the clicks fall into groups, and these groups rather than the individual clicks to become the basis of counting, great care was needed in the arrangement of the apparatus to avoid an objective rhythm. In spite of this a subjective rhythm continued to be more or less in evidence, though not of an extreme form. Two subjects took part in this experiment. As practice appeared as a significant factor in the problem, its effect was not eliminated by means of preliminary series of experiments, but distributed between the different series by the method of rotation, *i. e.*, each subject taking one determination in series one, two, three and four consecutively, and then beginning with one again, and thus continuing.

According to the most probable theory of the sensations of pitch, each minor center composing the auditory area in the brain gives a slightly different pitch-sensation in response to the variations in the vibration rate of the stimulus. Hence we would have, *e. g.*, in the quadruple series of our experiment, four brain centers acting in response to the four rates of vibration set up, and producing or correlated with the consciousness of the four pitches. If the attention can be divided, it must act as a multiple consciousness based upon the simultaneous activity of these different centers: and the possible sum of counted sensations in this multiple consciousness should be four times as great in the quadruple series as in the single series for the same lengths of time. We can see how this proportion might readily correspond to the amounts of brain disturbance under these respective conditions. The essential question is as to whether this four-fold disturbance can articulate itself into a four-fold correlated consciousness. Evidently the same argument holds for visual as for auditory sensations.

What came from this experiment with clicks was a gradual increase in the time necessary for counting each click in passing from the single to the quadruple series, the opposite of what would be expected if the attention had been divided. As in the last experiment, the time necessary for counting the sensations in each series was taken, this divided by the number in each, and the time thus found for counting each sensation averaged between the different subjects. We thus get for the single series, 165σ; double series, 180σ; triple series, 182σ; quadruple series, 200σ. The mean variations were 16σ, 30σ, 23σ,16σ, respectively. The effect of practice was general, although most pronounced in the double and quadruple series.

Since in the visual experiment less time was required for the double than for the single series, and less for the triple series, when this could be remembered, than for the double, there is an obvious disagreement between the results of these corresponding series in the two experiments.

In order to test this point as fully as possible, a single and double series of touch sensations were also tried. The toothed wheel used in the last experiment was also used here, but without the steel springs used to give the auditory stimulations. In their place were substituted cardboard rests with small openings in them, and so arranged relative to the rim of the wheel that when the fingers were placed against the openings of the rests, each tooth of the wheel in passing caused a touch sensation. The finger nails of the forefingers were used for this instead of the fleshy parts of the finger tips, since it was found that the latter fatigued, and confused the sensations much more readily. In general, the methods formerly used were also employed here. There were three subjects in the experiment.

With the single series of touch sensations no rhythm was present, only a simple series of unrhythmic touches being felt. For the double series a subjective rhythm appeared for all the subjects, and this seemed to assist somewhat in the counting. The average time for counting a single sensation was 185 σ for the single series, and 189 σ for the double, numbers which confirm the results of the auditory experiment. The mean variations were 6 σ and 18 σ, respectively.

3. Counting Simultaneous Series of Disparate Impressions.

In his experiments with simultaneous mental processes Paulhan found that dissimilar operations conflicted less than similar ones.[1] In order to give the method thus far described as exhaustive a trial as possible, a disparate series was arranged, composed equally of visual, auditory and touch sensations. The toothed wheel before mentioned was used. One steel spring and one finger-rest were arranged for giving a single series of auditory and touch sensations respectively, while a single series of visual impressions was furnished by an arrangement of lines on the side of the wheel, before which was placed a screen with a small opening. The electric sounder gave a signal for starting and stopping. As in the other cases the subjects, of whom there were three, were directed to distribute the attention

[1] *Revue Scientifique*, Vol. 39, p. 686 (May 28, 1887).

equally, so far as this was possible, upon all the three kinds of stimuli. Upon first trying this experiment, the subjects experienced extreme confusion, unlike that caused by the other experiments, and so great as to make it impossible to count at all, except after some practice. After this had become possible, the rate was much slower than for any other series, although it rapidly improved. The average rate for the three subjects per stimulation was 333 σ, while the mean variation mounted to 77 σ, an amount largely accounted for by the rapid improvement in the rate of counting. The amount of this increase of rate is shown in Table I., where the time in seconds for counting the whole series for each successive time is given for each subject. It will be noted that a point is soon reached at which improvement

TABLE I.

Subject.	*H.*	*F.*	*D.*
1st time.	14.25	16.25	8.00
2d "	14.00	10.50	8.25
3d "	7.50	7.00	8.25
4th "	6.50	7.00	8.00
5th "	6.50	7.25	8.00

stops. With subject *D* this point had apparently been reached in the preliminary practice before records were begun to be taken.

This comparatively slow rate, and especially the confusion, indicate the relative difficulty of combining dissimilar processes, and hence is opposed to Paulhan's results. There is, however, this essential difference between his method and my own. In the experiments described above there was one motor expression for both processes, while with him, as in the case of writing one verse and repeating another, the processes had unlike motor expressions. In attempting, he says, to write one verse of poetry and recite another, sometimes strange mixtures of the two would appear in the writing, but this was not often, at least when the same elements did not enter into the two. He continues: "The words which form a line and the lines which compose a piece each hold well together; in general, always in reciting, I recall one or two features of the lines which I wish to write; after that I think no more, the writing follows mechanically." He describes multiplying figures with one hand and reciting poetry at the same time. He multiplied 421,312,-212 by 2, which took six seconds. The recitation of four lines of poetry also took six seconds, but both were done together in six seconds. In trying to make two multiplications at the same time, one with the right and one with the left hand, time was lost. The two operations done at once took thirty-eight seconds; while when done separately, one took fifteen seconds and the other eight.

His experiments seem to have been many and carefully performed. He concludes that 'the most favorable conditions for doubling the mental process appears to be the simultaneous application of the mind to two easy operations and of a different kind.'

When the same elements entered into the two processes the motor expressions evidently became mixed, while when the processes were widely different this was not the case. Thus with divergent motor paths Paulhan found dissimilar processes to be more successful, while with convergent paths we have found similar processes to be more successful. From this we may readily infer that converging paths are more suitable for similar processes, and divergent for dissimilar ones. We have now the question as to whether distribution of attention really took place in these most favorable cases. The following table gives a more detailed view of the results of these experiments. The numbers denote thousandths of seconds required for counting a single sensation. Each number is an average from five determinations.

4. Was Distribution Present?

The table shows a striking uniformity between the different subjects employed. Thus all show a decrease of time in the double and first triple series in the visual experiment; all show gradual increase as the complication increases in the auditory experiment; the time remains nearly constant or the same for the two series in the touch experiment; and all show a marked increase in the combination experiment. The auditory experi-

Table II.

Subject.	Hy.		Ka.		Ko.		B.		F.		D.		All	
	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
One opening.	301	9	385	10	693	3	361	16					437	9
Two openings.	242	9	309	10	376	31	299	4					307	14
Three openings.	292	23	261	16			280	4					278	11
Three openings with irregular arrangement.	435	4	458	5			555	9					483	6
Four openings.	901	18	899	1			1263	5					1021	8
One click series.	171	13							150	18			165	16
Two click series.	197	34							162	26			180	30
Three click series.	200	34							164	12			182	23
Four click series.	220	24							179	7			200	16
One touch series.	195	5							180	7	181	5	185	6
Two touch series.	195	5							180	7	192	24	189	18
Combination.	388	129							328	97	279	5	333	77

ment shows the least time and a generally large mean variation. Here the rhythm was most difficult to suppress, a fact which partly accounts for both of these features ; for when the rhythm controlled the counting, the tendency to combine single clicks into groups caused a marked increase in the rate, while the successful attempts to suppress the rhythm caused a decrease of rate, and hence made a pronounced variation. It was very difficult to tell when a rhythmic grouping took place. Obvious rhythms were avoided, but it was so difficult to draw a line between the rhythmic and non-rhythmic that it was often difficult in this experiment to know when the counting was properly performed.

It is the difference between the rhythmic and the non-rhythmic counting which probably explains the decreased time of the double and first triple series of the visual experiment and the resulting conflict between these and the corresponding series of the other experiments. With the two and three screen openings of the visual experiment the rhythmic possibilities were greatly increased over the single opening, while the complexity of the series was not necessarily so great as to preclude remembering it, as was the case with the quadruple series. Hence the introduction of a rhythm, with its natural accompaniment of an increased mean variation, caused the greatly increased average rate. In sharp contrast with this double and first triple series is the irregular triple series, which could not be remembered, and where the time was greatly lengthened and the mean variation correspondingly decreased. It would seem to be this, rather than an economic distribution of the attention, which accounts for these decreased numbers and increased variation in the double series, the only ones upon which evidence of a simultaneous distribution could be placed. Indeed, in all the series in which we might have supposed distribution to be possible, the subjects experienced a rapid fluctuation, or oscillation of attention, a fact that of itself argues strongly against distribution.

But did not distribution take place in Paulhan's experiment with dissimilar processes and with motor paths diverging to different organs of expression? It should be noted that Paulhan speaks of simultaneous psychic or conscious acts rather than of simultaneous acts of attention. If we restrict all psychic acts to acts of consciousness, and all acts of consciousness again to acts of attention with their varied aspects as voluntary and involuntary, active and passive, then Paulhan's experiments test the division of attention. I have assumed this much and have regarded attention not as something added to consciousness, but as the character which it from time to time takes on.[1] Our author, however, seems to restrict attention to its more active and voluntary aspects, and to regard it as a phase of mental activity added to consciousness or much narrower than it. Thus, while he notes that the attention often oscillates in his experiments, he does not regard this as a reason for denying the simultaneity of two acts of consciousness. The fact, also, that at least one of the simultaneous processes must be learned by heart as a preparation for the experiment, raises the question as to whether consciousness is necessarily involved in both at the same time, and also places great hindrance in the way of any economic value which might rise from the practice.

One explanation may be ventured as to the cause of the decreased rate of counting the disparate impressions in the combination experiment, and of the same effect in passing from the simple to the complicated series with similar impressions. We know that there is an inertia which affects the functioning of the nervous system. This is illustrated by the time it takes for sensations to become fully felt, and for the transmission of pain. Cattell has estimated that it takes from 47σ to 58σ simply to become conscious of a small object, as a letter upon a white surface.[2] In reaction experiments a preliminary signal needs to precede the signal for reaction by about 1½ seconds, in order to give the attention time to reach the right intensity for the quickest response. When no preliminary signal is given, sensorial reaction is lengthened 26σ.[3] It thus takes an appreciable length of time for a center to respond fully to a stimulation.

[1] I have briefly developed this idea in 'The Fluctuation of Attention.' PSYCH. REV., *Mon. Sup.*, No. 6, p. 62.

[2] *Mind*, Vol. XI. (1886), p. 383.

[3] Wundt, 'Physiologische Psychologie,' II., S. 348.

When, in the above experiments, the intermittent stimulations were most frequent, as in the cases of the double series of auditory impressions where the fluctuation was between two tones only, the centers were kept in nearly as constant a state of excitation as when the series was single; but when, instead of two, there were three and four centers to share these, the frequency for each center must have correspondingly diminished, and hence the impressions occurred when these centers were in a less stimulated, and hence less responsive, condition. Therefore it took a longer time for them to act, and the rate of counting was correspondingly decreased. The presence of rhythm probably explains the absence of this effect in the visual series, where rhythm was not so carefully guarded against, and where the rate of counting increased with the complication of the experiment.

This perhaps explains the gradual increase of time needed for counting the series as they became more complicated, but another cause also made the combination series slow and distracting. This was the difficulty of combining unlike sensations. If one tries to attend, *e. g.*, to a visual and an auditory stimulus at the same time, he will notice a greater difficulty to attend the effort than when the sensations are similar. The mind apparently oscillates between the two rather than distributes itself between them in the case of both similar and disparate sensations, but with the disparate sensations the amplitude of the oscillation seems greater and requires a longer time. Practice, however, rapidly increased the rate of the combination series, apparently by decreasing the time of this transition, a result common to all associative processes.

5. Reactions with Concentration and Distribution.

One experiment or one kind of experiments is not sufficient, however conclusive it may be in itself, to test so general a proposition as that of the distribution of attention, because of the great variety of conditions involved, and of processes which may be combined. An experiment was accordingly devised to test by numerical results whether or not the attention really fluctuates from one to another of two or more objects upon which an effort is made to distribute it.

A cardboard screen was fixed in a vertical position upon a table, with six small openings arranged in a horizontal line three cm. apart. In the middle was a fixation point. Behind these was an electric sounder, muffled to be noiseless, and with a white bob attached. This was so arranged that the bob appeared behind one of the screen openings when the current was broken. The breaking of the current started a chronometer (Verdin), from which thousandths of seconds could be estimated. The subject was placed three feet from the screen with his eyes upon the fixation mark. Upon seeing the object through the screen opening, he reacted and closed the current.

With the subject's eyes always directed towards the fixation mark, a series was taken in which the subject knew in which opening the object would appear and had his attention on it; and also a series in which the opening in which the object would appear was not known, and when the attention was distributed, so far as possible, upon all the openings at once. A preliminary 'ready' was given before each signal, and the reactions were sensorial rather than motor. In order to avoid any influence from the fact that some openings were seen more indirectly than others, the signals in both series were divided equally between the different openings. Three subjects took part in the experiment, and one hundred reaction times were taken from each in each series.

An examination of the conditions shows that the only difference in these two series was that in one the attention was concentrated, while in the other it was distributed over the six openings, so far as this could be accomplished. We may, therefore, assume that the differences in the results of the two series would come from the effort at distribution. If the introspective record and the interpretation of the numerical results in the counting experiments were correct, we should expect an increased reaction time in the unknown as compared with the known series. This would be due to the unavoidable fluctuation rather than distribution of the attention, in the unknown series, between all of the six openings. The reasons why this fluctuation should cause a lengthened reaction time would evidently be similar to those for the increased time of counting as the complication increased in the counting experiments. An examination of Table III. shows this expectation to be realized. The average reaction time for the known series was 161 σ, while for the unknown it was 178 σ, or 17 σ greater.

TABLE III.

Subject.	*Hy.*		*D.*		*F.*		All.	
	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
Opening known.	160	20	147	17	177	17	161	18
Opening unknown.	164	26	157	23	214	17	178	22

The objection, however, may be advance that the distribution of attention does not necessitate the giving of the same amount to each of the several objects between which it is distributed as would be given to a single object when that received it all. A decreased intensity, instead of a fluctuation, would thus account for the increased reaction time in the unknown series. Hamilton says attention is subordinated to a certain law of intelligence. "This law is, that the greater the number of objects to which our consciousness is simultaneously extended, the smaller is the intensity with which it is able to consider each."[1]

There is a way of testing the validity of this objection. If, when the effort is made to distribute the attention among several objects, it really fluctuates from one to the other, naturally, it would sometimes, in the unknown series, be upon the right opening when the signal appeared, but more often upon the wrong one. In the former case the reactions would be short, while in the latter case they would be unusually long. In other words, the mean variation would be somewhat greater in the unknown than in the known series. The table shows this to be the case ; there being a greater variation by 4 σ for the unknown series. Here the exact amount of increase in the mean variation is evidently a matter of chance, since the attention could be expected to be upon the right opening but one time in six, and might be so many less than that as to make no marked increase in the variation. In this case, however, when the right opening would be chanced upon but seldom, the average reaction time would be distinctly increased. A glance at the table

[1] 'Lectures on Metaphysics and Logic,' Vol. I., p. 164.

will make it apparent that this was the case with subject *F*, whose mean variation is the same for both series, but whose average reaction time is increased in the unknown as compared with the known series 27 σ more than for any other subject.

I believe it would be difficult to devise a more convincing test than this experiment affords. Yet, as an effect of practice, there is a tendency for the results to change. In the combination series of the counting experiments, it will be remembered that practice had a marked influence in increasing the rate of counting, an effect presumably resulting from the use of the association paths relating the centers involved. A tendency appeared in the reaction experiment just described, which calls for a similar explanation. At first, when trying to distribute the attention in the distribution series, the fluctuation between the different openings was prominent, but as the experiment continued it became less so, and finally became almost unnoticed. Even the giving of the signal in an unexpected opening in the known series secured a reaction not appreciably lengthened. This raises the question, to which we shall return, as to whether practice may not be the means of making distribution possible, or if it serves to combine into a psychic unity things at first separate and unrelated.[1]

Wundt describes an experiment similar to the above, in which loud and weak sounds were irregularly interchanged as the signal for reaction. As the subject did not know which signal would be given, he was unable to prepare exclusively for the right one. There was an increase of 122 σ in the reaction time over the average of 121.5 σ when the right signal was

[1] The present study was continued for a period of four years, although during one of these years experimental work was suspended as it was impossible to take up the work at laboratories visited in Germany. The experiments thus far described were performed at the University of Illinois in 1898. While the present author directed and took part in them, they were personally conducted by Mr. J. M. Fisher, to whose ingenuity and enthusiasm much credit is due. The experiments which follow were performed later at the Harvard Psychological Laboratory. I am indebted for many suggestions to Professor Münsterberg, through whose courtesy the resources of the laboratory were placed at my disposal, causing the successful continuation of the work, and for the able and patient assistance received from the many subjects who took part in the work.

known, while the mean variation showed an increase of 46σ.[1] A valuable experiment similar to this would be to have the subject's attention directed to several disparate sensations instead of all being of the same kind. The same author mentions one of this kind in which sensations of light, sound, and touch were employed. No numerical results are given, although a very noticeable lengthening of the reaction time is mentioned, and also a continual fluctuating of the attention between the different senses.[2] He does not mention these experiments in reference to the question before us, although their results are obviously in accord with those here described.

6. Tachistoscopic Experiments.

On the other hand, Wundt has contributed the most formidable experiment we have in support of the theory of division. Its general features resemble Hamilton's well-known experiment, in which the attention was turned to a handful of marbles thrown upon the floor and the attempt made to observe them all, although it has fewer technical objections. The apparatus used was named the *Tachistoskop* by Volkmann. Cattell made an improved pattern,[3] while a still more improved form is described by Zeitler.[4] The essential features of the apparatus consist of a shutter having a rectangular opening in the middle, and sliding up and down in the grooves of two parallel uprights. There is a card holder arranged behind the shutter, so that when it is raised the card, upon which are placed letters or figures, is hidden by the lower part of the shutter. When it is in this position, the subject's eyes are directed to a fixation mark on the shutter, and over the center of the card. After a given signal the shutter falls, and in doing so exposes the card through the middle, and then immediately covers it over with its upper part. The apparatus thus serves to expose the letters on the card from 76.2σ to 93.7σ, those at the bottom being exposed the shortest time on account of the acceleration of the falling

[1] From figures on page 351 of the 'Physiologische Psychologie,' Bd. II., 4th edition.

[2] *Ibid.*, S. 352.

[3] *Phil. Stud.*, III. (1886), S. 94.

[4] *Phil. Stud.*, XVI. (1900), S. 380.

shutter. The theory of the experiment is that the number of letters which the subject can give from the exposure indicates the number of ideas that can be apperceived or attended to simultaneously.[1]

Consciousness is believed by Wundt to have wider limits than apperception. To test this limit, a specially arranged metronome gives a series of single beats, and it is found how many of these can be correctly judged as equal to another series which immediately follows it. As these series increase in length, a point is reached at which the subject can no longer judge correctly of their relative lengths. Sixteen single, or eight double strokes form this limit. Hence, sixteen is the number of separate impressions that can be held in consciousness at once.[2]

The number of separate objects that can be apperceived at once with the tachistoscope is given as varying from four to five. Six is usually considered the extreme limit, although this may be greatly increased if letters are used and they are arranged in intelligible syllables. Unfortunately we are without an explanation of the limit of this multiple activity of the attention, so that little satisfaction could be given one who should ask why ten or a dozen objects should not be simultaneously attended to as well as four or five.

We have the question as to whether the attention is really divided under the conditions of Wundt's experiment, or if the results from it are susceptible of a different interpretation. It came to be my purpose, therefore, to produce variations of the experiment in order to ascertain the real nature of the processes involved.

When one performs the experiment described by Wundt, a slight hesitation is often noticeable before the number of letters or simple objects seen can be named. This suggests that the objects are not clearly perceived during the time of exposure as Wundt claims to be the case. To test this, a tachistoscope was made embodying the essential features of the different forms of Wundt's apparatus, but avoiding the acceleration of the shutter

[1] 'Physiologische Psychologie,' B. II., 286 ff.

[2] *Ibid.*, S. 286 ff.

which would cause some letters to be exposed longer than others. Its front consisted of a black hard-wood screen, 20 by 40 cm. and 0.6 cm. in thickness, so supported upon a base that when placed upon a table it was perpendicular to the sagittal axis of the subject seated before it. In its center was an opening 6.3 cm. square, with bevelled edges to avoid shadows. Behind this an oblong shutter of similar material, with a bevelled square opening in the middle matching the first, was made to slide horizontally instead of vertically, as with Wundt's apparatus. A heavy rubber band attached between the forward end of the shutter and a small windlass at the outer edge of the screen furnished a means of propelling the slide past the screen opening, and of varying its rate in proportion to the tension of the band. A card-holder was arranged behind the shutter so that objects on the card were exposed as the shutter passed. A lever released the shutter, and a spring and rubber cushion stopped it. The rate of movement was ascertained for all parts of the passage by means of a smoked paper attached to the shutter and which passed by a vibrating tuning-fork. This showed that for about the first and last inch of the movement there was an acceleration and retardation respectively; but that for the middle part of the movement, which was used for the exposure, the rate was constant. Fig. 1 represents the back of this apparatus.[1] Wilson's black gummed letters, 7 mm. in height, were used upon white cards as objects to be exposed.

7. Letters Exposed in Succession.

If the letters are exposed in succession, one at a time, but appear to be exposed simultaneously, evidently they are not perceived until all have been exposed, *i. e.*, the act of perception would not take place during the exposure, but after. To determine this, the opening in the shutter was, by means of the insertion of a blackened metal plate, closed to a narrow slit 7 mm. wide and extending vertically the width of the opening. Twenty cards were used and six letters were arranged irregularly upon each card, but so that as the opening in the shutter passed in front, only one letter was shown at a time. Thus while the let-

[1] The figure will appear in Part II. of this article.

ters were placed in all parts of the square surface of the card exposed, they were exposed in a definite order of succession. The time of the whole exposure was 20 σ, or approximately 3.6 σ for each letter. It should be noted that, owing to the method of exposure, first the first part, then the whole, and finally the last part of each letter was uncovered. The time when the letters as a whole appeared was extremely short, as the width of the opening was but a little greater than that of the letters.

A practice series of considerable duration preceded the regular experiment, although the effect of practice was not very marked. The method of procedure was as follows: The subject was placed at a constant distance from the apparatus, and kept in position by a head-rest. This distance was 1 m. for all but one subject (*R*), who required to be but 60 cm. in order to get a distinct impression. The eyes were fixed on a fixation mark placed on the shutter and in front of the center of the area to be exposed, and the attention was distributed as far as possible over this area. The experimenter gave a double signal, one two, and one one second before releasing the shutter. The subject gave the letters which he saw in the order in which he saw them, and also their locations in the field in order to keep account of those wrongly seen. In order to guard against fatigue, a short recess was given after each ten exposures, and only thirty exposures were given in an hour, as more than that was found to be fatiguing. Each of the twenty cards used was exposed five times during the series, thus making a hundred exposures for each subject. A record was kept of the letters seen correctly, their order, those seen wrongly, and those misplaced. When this series had been completed, another was taken, in all points similar, except that the shutter was made to move from left to right during the exposure instead of from right to left as in the first series. This served to reverse the order in which the letters were exposed. No succession in the exposure of the letters was perceptible by the subjects. It might be, however, that the order of exposure unconsciously influenced the order in which the letters were perceived, and this would show that perception took place, partly at least, during the exposure. It was to test this that the changed order of exposure was tried.

Table IV. gives the numerical results of the two series, showing the number of letters seen correctly, those seen wrongly, and those misplaced in the hundred exposures for each subject in each series. Of those misplaced in the consecutive exposure,

TABLE IV.

Subject.	*A.*	*H.*	*R.*	*S.*	*Y.*	Av.
Consecutive Exposure.						
Number seen correctly.	212	222	158	160	224	195
Number seen wrongly.	40	30	58	50	58	47
Number misplaced.	17	16	5	18	23	16
Reversed consecutive.						
Number seen correctly.	208	225	167	151	229	196
Number seen wrongly.	32	22	64	53	52	45
Number misplaced.	6	13	7	10	9	9

75 per cent. were otherwise correctly seen, while in the reversed consecutive this number amounted to 69 per cent. The average number wrongly seen is about equal in both series, and also the number seen correctly. These features of close correspondence show that the conditions in both series were practically equally favorable for seeing the letters. If we divide these numbers by 100, we shall get the figures for a single average exposure. This would make the average number seen correctly slightly less than two, the smallness of which we shall see later is readily accounted for by the short exposure. Ten subjects took part in the first series, but since only five were employed in the second, and the results were uniform, only five are given in the table. All of the subjects were men, the most of whom had had considerable laboratory training.

It is, however, in the order in which the letters appeared and the introspective records that we find the most significant features of the experiment. Some features of the mental process would be more distinct with one subject than with another, and the experience would vary somewhat for different exposures, but a general harmony prevailed throughout. Uniformly at the time of exposure the card would seem to flash out without one side appearing before the other. Hence perception evidently did not take place until after the exposure was over. The first effect of the letters was that of a single complex impression, some characters appearing distinctly outlined, some confused, and some entirely unseen. This conscious impression followed the exposure in much the same way that a positive after-image follows a stimulation of light. It was sometimes possible to hold this impression with all its details an appreciable length of time without recognizing a single letter, until each character was recognized one at a time. But it was more frequent that one or sometimes two letters were recognized without being preceded by an appreciable interval, and these followed by one or two more, one at a time and in distinct succession.

The letters that came up last were nearly always less distinct, although it was sometimes the case that the order was not the order of distinctness. A special effort to recognize an indistinct character would frequently cause it to mature into complete recognition before others which were at first more distinct. Sometimes a delay in this maturing process would cause more distinct characters to be forgotten before they could be named, or else cause the indistinct one to come floating into the mind as an after-thought when all had been given that could at first be remembered. This indistinctness of the letters recalled last is a comment upon the common experience that the impression as it is first received rapidly fades. One grasps at the most distinct characters in order to secure them before they fade, but with the feeling that in doing so he excludes the possibility of catching others which he might have taken in their place. The naming of the letters makes them seem more sure, and this is hurried up in order to get as many as possible. One subject noted that the delay caused by locating each letter in the field as it was recognized caused fewer to be got than when they were all named at first and then located.

The order in which the letters matured seem to be in no way dependent upon the order of exposure. In general, those in the middle of the field of vision, and hence seen most distinctly, were given first. It was frequently noted that some factor, other than the distance from the center of the field and the occasional voluntary effort, influenced the order in which the letters were perceived. By referring the letters as given to the cards, it was found that the prevailing order was from left to right, as in reading; and this was as true of the reversed order of exposure as of the other. The habit derived from reading thus seems to have influenced the order to some extent. But no distinct influence seems to have been exerted by the reversing of the shutter.

Frequently the same letters would be given from a card for several exposures, and these would have their order varied independently of the direction in which the shutter moved. The letters in the center had the preference, while those sometimes upon one side and sometimes upon another would be given with these, a fact which would seem to indicate that the attention before the exposure wandered about the fixation point rather than distributed itself equally over the field, as was also the case in the reaction experiment with concentrated and attempted distribution of attention. This probably explains another experience which might be interpreted in favor of a distributed attention and which was common to the subjects. When several letters were seen they were less distinct than when only one was got. This is in line with the law above quoted from Hamilton and illustrated by Wundt,[1] that 'the greater the number of objects to which our consciousness is simultaneously extended, the smaller is the intensity with which it is able to consider each.' A comparatively large number of letters was commonly got as the result of a special effort of attention. Supposing this effort to cause the attention to fluctuate more rapidly about the field, a noticeable result of a special effort at distribution, and this to be correlated physiologically with a correspondingly intermittent rapid central stimulation of the visual centers, we can see that these centers would, as a whole, be kept in a more responsive state than when less effort was made, and the fluctuations were slower. This would cause the getting of a larger number with the greater effort. We can also see that with the less effort and slower fluctuation the center stimulated at the instant of exposure would reach a more highly excited state because the fluctuation was slower and the

[1] 'Physiologische Psychologie,' II., S. 268.

time of stimulation of the center longer. Hence the greater vividness of the single letter and the less vividness of the several. Thus this experience may be explained without the aid of distribution.

The fact that the letters in this experiment were not perceived or made conscious until after the exposure was over, separates the time after the instant of exposure into the two natural divisions, one a period of inertia, or subconscious period, and the other the conscious period.

8. The Subconscious Period.

When the eye is stimulated we have, first, a so-called latent period of variable duration during which no effect of stimulation is shown. This is founded primarily upon analogy with the general functioning of the nervous system, since electric stimulations applied to a nerve do not cause an immediate muscular contraction. This period is very short. Second, there is a very brief but relatively longer period during which the effects of stimulation reach a maximum. This is illustrated by 'recurrent images,' or the 'oscillatory' activity of the retina. It is shown when a black disk, illuminated brightly by sunlight, and containing a white sector, is rotated at the rate of about one revolution in two seconds. With the eyes fixated upon the center of rotation, the sector seems to have a shadow upon it a short distance behind the advancing border, and this may be followed by a second fainter one, and even by a third still fainter. The distance between these, and between the first and the forward edge corresponds to a time period of about .015 of a sec. "It thus appears that when light is suddenly thrown upon the retina, the sensation does not at once rise to its maximum, but reaches this point by a sort of vibratory movement."[1]

In addition to this inertia of the retina there is also inertia of a more central origin. By means of experiments in reaction time, Cattell found 119σ and 116σ as the time necessary for two subjects to distinguish one capital letter from all the others when the letters were the size of the capital of an ordinarily printed

[1] Bowditch, in 'American Text-book of Physiology,' p. 790.

page. With the same subjects he found the whole reaction time which included this process to be 308σ and 324σ respectively. It took longer for some letters than for others to the extent of some 20σ.[1]

This period of inertia preceded the completed act of perception in our experiment, and hence errors in the process are traceable to it. Thus it was a common occurrence for one letter to be taken for another which largely resembled it. *C* and *G* were often confused, also *V* and *Y*, *O* and *U*; here, no doubt, the part of one letter only was seen which resembled a part of the other, and the rest was filled out wrongly as in an illusion. More striking were the cases in which letters placed far apart on the card would be given a place half way between with the position seemingly sure, but with uncertainty as to which of the two letters it was. An *F* might be placed far from its real position and beside a *C* without the latter letter having been seen. A *Z* standing by itself was given as *Z* and *T* close beside each other. *G* was interpreted as *G* and *C* beside each other and in the place of an *I* which was not seen. Letters are made up of different parts or elements of form in the same way that words are made up of letters. Such cases as the above indicate that elements of form and position were received as disconnected impressions at the instant of exposure, and that while these in the majority of cases were correlated subconsciously into the right letters in their right places, they were yet often combined into wrong letters and in wrong places. It was not, however, until this correlation was completed at the end of the exposure that the letters reached their first conscious stage, and it was possible for the attention to be divided among them. So that if distribution can take place in tachistoscopic experiments, it is only when the exposure is over, or when it is longer at least than the 20σ exposure of this experiment.

In this connection it is important to know how long a time can be taken up by the consecutive exposure of letters and still have them seem to appear simultaneously. It was found that

[1] 'The Time Taken up by Mental Operations,' *Mind*, XI. (1886), p. 220.

For discussions of inertia see Wundt, 'Phys. Psych.,' I., 321 ff.; Fechner, 'Psychophys.,' II., 431 ff.; Exner, 'Herman's Phys.,' II., 215; *Pflüger's Archiv*, XXVIII., 487; Hofbauer, *ibid.*, LXVIII., 546.

the rate of the shutter could not be varied sufficiently for this purpose with the rubber band, so that a weight attached to a cord running over a pulley and attaching to the shutter was substituted for it. The slowest rate which could be attained by this means was produced by allowing the shutter to be drawn by the falling of the weight when it started from a position of rest. More rapid rates were produced by raising the weight to various heights before releasing it, so that a degree of velocity was attained before it reached the point in its fall at which the shutter was moved. The extremes of slow and rapid rate procured in this way were much greater than were needed by the experiment. The height from which the weight fell was recorded, and the exposure varied several times from both the too-rapid and the too-slow rates to the point at which succession was just indiscernible. When this point had been determined for each subject the time of the exposure was ascertained by means of the tuning-fork and smoked paper method previously described. The following table gives the time of exposure in thousandths

TABLE V.

Subject.	*Am.*	*An.*	*Ho.*	*Hu.*	*M.*	*R.*	*S.*	*Y.*	Av.
Time.	26	24	86	34	75	75	27	28	47

of seconds in which the sequence was just indiscernible for each of eight subjects. The average rate thus obtained for the whole exposure was 47 σ. This average is of little value because of the wide differences of rate found for the different subjects. In this test the shutter moved from right to left. When the exposure was too short for succession to be noticed, the order in which the letters came to consciousness was from left to right, as in reading. With all of the subjects except *Am* and *Hu* it was noticed that the first sign that the objective order had become apparent was the tendency to give the letters from right to left in the order of their exposure. With *Am* and *Hu* the movement of the shutter was first perceived as such.

What causes this lack in the perception of succession? At first we might think of it in connection with the after-effect of retinal stimulation which makes color mixing possible with the color wheel. The duration of this varies from .100 to .033 sec. according to the intensity of illumination, the length of the stimulation and the color. The difficulty with this solution consists in the fact that while in color mixing the same part of the retina is affected by the different colors, in the above test, different parts were affected in succession as the exposure took place. The divergence of these parts was about 0.97 mm. when one of 0.004 to 0.006 mm. is sufficient to distinguish two adjacent objects. In this experiment the breadth of the exposed field was 6.3 cm. and the distance of the eyes of the subject from the apparatus 1 meter, with the exception of *M* and *R*, for whom it was 60 cm. Even though the after-effect of the part of the retina affected first should continue until after the stimulation of the part affected last took place, this, it would seem, need not prevent the initial impulses from being felt separately. This suggests that somewhere on the route of transmission the visual impulses from different parts of the retina traverse paths held sufficiently common to cause the impulses received first to overcome inertia, thus allowing the later impulses to overtake the first and so reach the center of consciousness nearly or quite simultaneously with them. On the other hand, it may be that the stimulations which come in succession from different parts of the visual field are transmitted immediately to the cortex. A certain time interval between the central nervous impulses thus aroused might be necessary in order for them to remain sufficiently distinct to mediate discrete sensations.

(*To be concluded.*)

DISCUSSION.

IMITATION AND SELECTIVE THINKING.[1]

My original difference from Professor Baldwin was merely about the adequacy of 'imitation' as compared with 'identity and difference' in explanation of social processes. But, through the discussion of selective thinking as the instrument of the genesis of organization out of imitation (I am recapitulating roughly and without controversial intention), we have now got on our hands the value of selective thinking. And I am not sure that this does not open up the whole subject of Pragmatism. I say I am not sure, because this depends on the limit which Professor Baldwin puts to the meaning of his term 'genetic.' I will, if I may this once more trespass on the courtesy of the PSYCHOLOGICAL REVIEW, try to distinguish my attitude in respect of the different questions thus raised, mentioning Pragmatism so far as to show what I conceive would be the problem evoked by it, in contradistinction to those to which a genetic theory as such ought, in my judgment, to confine itself.

1. As to Imitation *vs.* 'Identity and Difference.' *a.* I can not agree with the view that this difference is verbal, *e. g.*, in its application to biology. It is easy to show that a working whole cannot be represented in terms of similarities between its members. And for the same reason the nature of the members themselves must in a great measure be omitted from such a representation. The Linnean classification, or the current 'natural' classification in botany, may be taken as a representation according to resemblances, though I should not admit that any scientific classification is so intended. But a region of the world, as a whole of competing and coöperating members, according to the light thrown by the principle of evolution, can never be represented in such a form as this. It can never bring together the things which have most to do with each other: competing species of plants, coöperative plants and animals, the soil, the climate, and their effect on the living things. Of course all this can be added in footnotes, as it were, to classification by resemblances; but it cannot be represented in the structure of the classification itself. It would be like trying to explain

[1] Continuation of the discussion in earlier numbers, with especial reference to Professor Baldwin's paper in the issue of January, 1903, p. 51.

a locomotive by arranging its parts in classes according as they resemble each other. The reason of the impossibility is that the parts or members have their connection through their differences; and in a classification by resemblances, these, though they have their weight as differences, have no weight as instruments of identity. This whole subject is treated by Green, 'Works,' II., 285, 'On Mill's Induction,' and I think is too little understood. I should strongly suspect that the reform of logic in this sense in the great Idealist days promoted, or at least was akin to, the transition from Linnæus to Darwin.

β. Imitation (I summarize in my own language) is alleged to be a *vera causa*, it *shows*, is psychical, genetic, you can see it at work; the operation of a universal is an assumption, shadowy, almost, I think, *a priori*, mystic, antiquated, invisible. I assume publicity, which ought to be explained.

Now I am very likely quite wrong, but I cannot see any ground for all this in the facts. Imitation no doubt is a fact, and plays an important part in furnishing the self with material. I quite recognize the value of Professor Baldwin's actual work at this point. But surely response and reaction, indices of communication through a common nature, are much wider and more primary facts, extending over the whole world, physical and psychical. The adapted response is earlier — is it not? — than consciousness; and the process of its passing under the control of intelligence and being emancipated from trial and error, is fairly well understood, though still doubtful in some details. But the adapted response, as controlled by intelligence, just means a consciousness of the situation based on an inference which *pro tanto* dispenses with the test of material action; an inference based on perception is substituted for a certain number of errors, as when a man sees at a glance how to open a gate, which a dog will paw at till it comes open. There seems to me no assumption in this; it is a plain statement of fact, and of fact more general and fundamental than imitation, and requiring no more assumption.

With responses adapted by intelligence on the part of two or more agents you have publicity or the situation. What you want, to account for this, is not imitation but the power of consciousness to combine perceptions and see their results — in short, the unity of consciousness. As I understand, it is urged that this must not be assumed but can be and ought to be genetically accounted for. This I will speak of when I come to comment on the meaning of the term 'genetic.' My present point is merely that imitation is the secondary, less general, and less completely stated fact, and that the assumption of it, while involving, as much as a response does, the assumption of the unity of consciousness, is in no special way a help towards explaining the apprehension of a situation as a whole.

γ. The treatment of facts introduced by this theory, in the extension claimed for it by Professor Baldwin and others, seems to me precarious all round. Particularly is this the case with the separation of the imitator and the inventor. I am convinced that a really critical study of any branch of history would demonstrate the crudeness of this antithesis when offered as a matter of principle. The advance of the human mind, independently, so far as can be judged, of individual original genius, is one of the most striking phenomena of history, and one is inclined to add that the deepest transformations are those which have taken place in this way. It is an old and true saying that man must advance or recede; to stand still is impossible for him. That is to say, the application of tradition to life is in itself a generator of inventions; it is impossible even to borrow ideas without drawing conclusions which the lender never drew. And it is well known how rarely, if ever at all, an invention can be assigned to a single mind. The history of art is very instructive on this point, *e. g.*, the education of a Turner.

2. The second question, as it has now opened itself up, seems to amount to this: Does a genetic account of thinking explain by what character judgments are true, or only under what influence we have come in fact to hold (often wrongly) certain judgments to be true? And what bearing has either alternative on the theory of selective thinking?

a. I will say at once that I see no meaning in a genetic account of knowledge, except as a history of opinion; but I admit that this involves a history of mental organization. A simple illustration will do as well as an ambitious one. We constantly make such judgments as this: "A. B. is a moderate Evangelical; he was brought up as an extreme one, in a family and circle whose views were extreme, but his work and intercourse with varieties of people have made him much more temperate." Here we have the true place of a selective theory of thinking, so far as I understand it, in a nutshell. A. B. inherited a platform, an organized mental constitution and logical or quasi-logical system; *i. e.*, he acquired it by adaptation to his parents' and teachers' views, or imitated them. Starting from this, he developed his later position through varied forms of social selection acting on his ideas, involving accommodation to practical needs; and he now has a mental content and organization at once fairly harmonious with the circle in which at present he moves, and determined as a whole by the platform which he inherited. I do not doubt for a moment that a history of all of us and of the human race could be written in terms analogous to these with a great deal of truth. And it would not omit the facts of mental organization. The metaphysician, the psychologist, the biologist, mathematician, and also the Englishman, Frenchman and German, would all prove to possess, yes, and to have acquired and developed, certain favorite categories, certain forms of logical or quasi-logical bias, and predispositions to accept explanations of certain appropriate types.

In such a historical enquiry some theory of selective thinking might have, so far as I see, very interesting applications. It would show by what needs and under what direction of attention the minds of nations and individuals had grown into certain structures, and had acquired certain logical predispositions.

But even here it would be necessary either to expand very largely the sense in which, or to limit very strictly the extent to which, we affirmed action to be the instrument of selection. If action meant all psychical change directed to an end, then, in referring the course of cognition and mental organization to the needs of action, we should be making cognition itself the standard of cognition,[1] and saying that it learns to act as it does act primarily by seeking its own ends and secondarily by taking account of a certain contact with material action. Then we might fearlessly say that 'action' is the sole test and instrument of selective thinking. How 'action' operates, would be the further question, to which Logic would be the answer.

[1] Mr. Stout in his 'Manual of Psychology' seems to me to agree much more with me than with Professor Baldwin, never blinking the relative importance of the cognitive system as compared with external action, nor the liability of social endorsement to be erroneous. But in one place he seems for a moment, as I venture to think, to slur the distinction on which I am here insisting. On p. 547 he insists that because belief is a condition of activity, therefore activity must be a condition of belief. And this remark he extends to theoretical activity, though, indeed, as referring it to the provisional acceptance of working hypotheses, he gives it a very restricted and innocent application. But the point I wish to urge is this. In a 'practical' activity the end is assumed to be given, and it is not a cognitive end; therefore in this case there is some tendency to adopt beliefs which purely cognitive processes might not confirm; *i. e.* there is a possibility of a real non-cognitive influence on cognition. But in a theoretical activity, unless a preconceived opinion is to be supported (which is an *aberration from* the theoretical consciousness), the end to be obtained is not given, but is itself a conclusion to be constructed. It therefore involves *ipso facto* a modification of the beliefs ancillary to it, and the dangerous primacy of action over reason is not confirmed by this instance.

If, on the other hand, action were taken in the sense of the production of change in the external world, we should return quite a different answer. We should say that the influence of practical needs was a diminishing factor as the content of systematic knowledge increased.[1] We should point out that when thought has become complex, action on the external world is to it as sensation is to science, a condition which is little more than negative; something, disagreement with which demands more or less modification of the discrepant thought, but any given agreement with which carries us but a very little way towards truth. We should further urge that the much talked of 'social endorsement,' as applied to systematic ideas, has no existence. This is a very important point in its practical bearing. Social endorsement does apply roughly to habits of action. But to cognitive ideas, to the actual content of inventions, and to theories, as such, it has no application, only touching them in one or two points out of thousands; and to suppose otherwise is a very mischievous superstition.[2] It is a transference of the ideal postulate of reason, that all valid judgment is valid for all intelligence, to the *de facto* social consciousness, to which it applies only in grades so contingent and varying as to be of no selective value whatever. The leading ideas of society, so far as they can be conjectured from their expression, are always in arrear of the truth known to experts, and more especially are discrepant with its own habits of action, which do represent in a rough and unorganized form the external needs of life.[3]

The exclusive importance attached to action on the external world, and to social endorsement, even as influences on the history of opinion, is, I hold, a mere paradox, unsupported by facts. The subordination of the vast cognitive systems and interests of mankind (which have, it must be remembered, their own relations, dictated by cognitive needs, with the 'external world' or sense-perception) to the test of action in the narrower sense of material external change, I believe to

[1] Mr. Stout in his Manual seems to me perfectly clear on this point; and to be wholly free from the ambiguity whether thought is made true by being socially and practically selected, which I find in Professor Baldwin.

[2] I hope I shall not annoy a friend who conversed with me in the U. S. A., in 1892, if I make use of his observation to me: 'Sir, the people of these States have endorsed the philosophy of Mr. Herbert Spencer.' The example seemed too apposite to be neglected, as showing the laxity with which a rough coincidence in one or two points is construed as an 'endorsement.'

[3] *E. g.*, T. H. Green usually agreed with J. S. Mill on questions of public policy, though on all theoretical matters their minds were diametrically, opposed. This is possible, just because theoretical ideas, even of social matters have so very little of their content in contact with practice.

be simply an elementary blunder. If, on the other hand, we are only asked to call these interests and systems 'practical,' as Aristotle carefully pointed out that they are, in virtue of their inherent conativeness, we are asserting, I take it, the contradictory of Pragmatism,[4] but are returning to obvious truths. (See Stewart, 'Notes on Aristotle's Ethics,' 1098, a 3, and citations from the 'Politics.')

β. And when we raise the whole question of Pragmatism, *i. e.*, as I understand, not '*How do we come* to think something?' but '*What is the test* of its truth?' the idea of selection by social endorsement, or by success in producing change in the external world, loses all claim to consideration, except as involving agreement with sense perception, which is provided by cognitive activities in a much more adequate form. As we have seen, nearly the whole of cognition is simply untouched by action on the external world. In such action itself the outward change effected is but a minor part, from which, as we know, *e. g.*, in all ethical considerations, it is impossible with certainty to understand a man's mind; and when we come to the great cognitive systems the prerogative of such action vanishes altogether. Indeed, there is but one criterion of truth, and that is, a fuller systematic cognition of the content whose truth is in question. No history of opinion, no formation of a platform, no idiosyncrasies of mental organization, can come into court when the question of truth is raised. Then we have to do with nothing but the systematic necessity of knowledge and the fact that fuller cognition can compel every false judgment to expose itself as flat self-contradiction.

I do not feel sure whether these remarks are relevant to Professor Baldwin's views. But he seems to me to mean that selection by social and practical needs not merely accounts for our holding opinions, but also constitutes their truth or falsehood. If so, then, as I said at starting, we have the whole of Pragmatism on our hands, and are, as I hold, beyond the limits of legitimate genetic explanation. Grant, *e. g.*, for the sake of argument, that the unity of consciousness first appeared in practical action in the narrower sense given above (as it must have done if there was a time when consciousness was entirely 'practical' in its aim), or that it is motor in its nature, or that it appears in some sort of general sensory process. All that is interesting in the history of opinion, but has no bearing on the logical value of

[4] Because Pragmatism says, as I understand, that the only ends of action are those which consist in change wrought upon the external world, and that, to these, cognition is a means. For me, cognition, as a harmony in our experience, has the character of an end of action, though not the whole end. But external change is *never* an end.

such unity. This is only to be discovered by an analysis of the part played by it in the organization of experience so as to avoid self-annihilation by self-contradiction. It is an old story; granting (what is not true) that we need not play the game, yet if we sit down to it we must observe the rules. If we are asked, Why must we? there is no answer but to show by analysis in any given case that in trying to evade them we are disguisedly throwing up our hand. I can imagine its being replied, 'But you say that A.B.'s rules and platform are got by his history and education; then surely his truth is so too.' The answer is that his rules and platform are an imperfect appreciation of *the* rules and platform, and cannot stand against another, in him or outside him, which more nearly approaches them, and therefore is able to exhibit his as self-contradictory. His knowledge, or rather opinion, *qua* his, may be compared to his body, a *de facto* structure, accounted for by accident and selection as well as nutrition and correlation. But his knowledge *qua* knowledge may be compared with the work his body is now capable of as a machine — a test to which his genesis has nothing whatever to say. Truth is the most organized organization of reality in the medium of judgment; our history may excuse our failures in it, but cannot make them successes.

It does seem odd to me that views like this should suggest to any one the idea of 'the mind, for no reason, and by no regular processes, making its truth what it will'; or of 'the essential mysticism of *a priori* formalism which prevailed before the rise of the genetic point of view'; or the assertion of determinate variation[1] in the narrow sense that no natural reason can be given for whatever balance of variation may be found in a given direction.

This again is an old story. The very error with which I am charged appears to me to be merely in the mind of my antagonist. The whole antagonism of principle between classical and modern

[1] Perhaps my terminology in speaking of determinate variations was incorrect according to biological usage. I meant by determinate variations those which by a definite cause are produced in a definite direction, which is such as to be appropriate to the environment, so that subsequent selection becomes needless or inoperative. This, it will be seen, is exactly the opposite of the meaning which my statement conveyed to Professor Baldwin. If I am understood I do not care to argue about the true biological conception; but I am disposed to think that this will soon prove less formal and more real than Professor Baldwin supposes. What is meant by speaking of a principle of determination that does not show itself in any recognizable or conscious process I cannot form any conjecture. Is it not a recognizable and conscious process by which we determine that the three angles of a triangle are together equal to two right angles?

logic; the whole conception of a modern development of the genetic point of view, considered as anything which affects the nature and criterion of truth; the whole idea of thought in itself as opposed to the nature of the real in cognition, all this appears to me to be the merest mare's nest. The truth of anything is for me simply its fullest nature so far as expressible in judgment, organized, as the fullest nature must be, so as to avoid diminution by the contradiction of its parts. What I deny is, not that thought is the expression of organized reality, but that the organization of reality is confined to the production of material change in things. The nature of things is both general and special, and besides its more general and formal characteristics, there are all sorts of grades and variations as we push deeper and deeper into the heart of complex individuality. These, as found by analysis, form respectively the more abstract and more concrete elements of Logic. But obviously all of them contain and confirm the general nature of truth.

Why should not the universal 'be a mental experience which has for its physical counterpart the synergy of adapted action' (p. 63)? To me the answer seems simple — because there is very little thought, proportionately speaking, to which there is any adapted action, *in the sense of external material change*, to correspond. I have said that I think that unity very likely first showed itself in adapted action. But no thought, probably, ever had its content exhausted in the adaptation of external action; no thought of a cultured mind can ever be so exhausted to-day, even in the most practical of activities; and a very great part of life, a part which even economically and industrially is an immense and commanding interest in the world, has no end in external adapted action at all, but on the contrary uses and transforms such action by making it its means. A great scientific laboratory, for example, has not its unity in a material operation to be produced; its actions have their unity in a cognition to be attained. The same point is very strikingly shown in the enormous material activities of a Wagner or Handel festival; whose whole practical business has for its determining purpose the production of a harmony in minds, of the same *general* (not specific) nature as a cognitive state. The harmony is the end; the 'action' is the means.

The formation of new reality seems to me a contradiction in terms; but the discovery of reality new to us, and the adaptation of intelligence to it, is surely a fact which no one has ever denied, and which, in general, is hardly worth affirming. It is presupposed, for example, in all education; and the ancient educationists at least never imagined that education was only for the young. Now if this distinction would satisfy the genetic point of view, I think we might come to terms. But if the genetic point of view means (*a*) that new reality is not merely discovered but created, (*b*) that action on the external world, and social selection, are the determinants and criteria of truth, then I fear there can be no truce between us.

B. Bosanquet.

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I venture to append here a word of reply to Professor Bosanquet, since as editor of the Review I am afraid I shall not be able to take further liberties with the space available for discussion in later numbers. The remarks which follow refer by number to his divisions above. The discussion has served a good purpose, let us hope; and readers of the Review will always be ready to follow Dr. Bosanquet in the further development of his views in these pages.

Introductory remarks, and 2β. — I do not intend to raise the question of pragmatism as a philosophy, in advocating a theory of selective thinking by active adjustment processes. Personally I am not a pragmatist for much the same reason, as I conceive, that Dr. Peirce (the originator of a certain form of that view) is no longer one (cf. Peirce, art. 'Pragmatism' in the writer's *Dict. of Philos.*, vol. ii.). I think pragmatism is not able as such to explain the general or 'universal' aspects of reality. I agree, therefore, with Dr. Bosanquet in confining genetic theory strictly to questions of genesis. But — and here possibly the final issue between his views and my own may stand for others also to divide upon — *but*, there can be no truth nor cognized reality which is not what the human mind has reason for accepting and believing; and the reason always is and must be that this aspect of reality, now called true, stands or has stood the tests of certain selective processes. Genetic theory, therefore, explains *both* 'under what influence we have come to hold (often wrongly) certain judgments to be true' and also 'by what character judgments are true' (2). It explains just the character which means to us truth — reality as postulated by thought. All such antitheses, accordingly, as that quoted above — that between what is true and what seems true — are radically false antitheses. It is the business of theory of knowledge to establish the 'is' in and upon the 'seems.' Now it is the claim of the theory of social selection that by it certain of these antitheses are banished. What 'seems' to the individual is true only relatively to the larger 'seems' of the social group; what seems to the evangelical is true, but relatively to an enlarged socially established creed, etc.

Otherwise we must go back to the traditional correspondence theory of truth — a dualism involving separate and static realities set over against a series of mental images which only partly correspond with them. Dr. Bosanquet seems not to hold such a dualism; indeed he defines truth as 'the most organized organization of reality' (2*a*). This definition I adopt provided 'reality' be defined (so far as cognized) as nothing more than the human system of truths. But I fancy this will not be acceptable to Dr. Bosanquet, for it then makes a tautology which can be resolved only by a theory of progressive organization of richer truths (and realities) on the basis of less rich — the whole by a process of selection.[1]

I believe, therefore (to answer the enquiry raised by Dr. Bosanquet), that reality is dynamic and progressive (just because truth of which it is a function — from which it is generalized — is such); that new realities are made on the process of human experience and discovery (for just by getting into the system of intelligent or other constructive experience, they are constituted as realities); that the phenomenal manifestations of change, of which time is the generalized mode, are the data of all real construction both cognitive and other.

With so much general statement I may take up very concisely certain of Dr. Bosanquet's more detailed positions.

'Imitation *vs.* Identity and Difference' (1, *a*). I subscribe to most of Dr. Bosanquet's remarks under this head; indeed I think 'imitation plus invention' is very fairly described as 'identity and difference.' The barrenness of the formula for the progress of science is what I should still claim. Certainly identity (the whole) realized in differences (the parts) is the ideal of knowledge, but empirical science proceeds by generalization, classification, etc., in constructing its experimental wholes.

1, *β*. The fallacy, as I take it, of Dr. Bosanquet's criticism of the imitation theory here resides in the assumptions: (1) that *adapted* action is the same as *adaptive* action. Certainly adapted action always exists, imitation is itself a case of it (I think, broadly defined as 'circular response,' the original case of it); but the question is *whence and how is action adaptive? How are new adjustments effected?* It is no answer to say it 'passes under the control of intelligence,' and is thereby 'emancipated from trial and error.' For such control must be either by the using of old adaptations simply or by

[1] The criticism of pragmatism as a philosophy would raise the question as to what place such a system of cognized realities has in the total system of our whole experience of reality.

the establishing of new ones. If the former, then there is no progress;[1] if the latter, then again — *how is it done?* No one would (or should!) claim that the intelligence works by saying to the contents of the mind, Do this — and it doeth it! It does it *by the process of trial and error*, instead of being emancipated from that process! And it is, I think, a fairer inference to conclude that, each new adjustment being acquired in this way, then the presumption is that all the old ones were also so acquired. Applying this to social life — the trial and error process *is imitation.*

2. A second assumption is that if two minds work similarly they will be *ipso facto* in social relation with each other in a situation recognized by each (1, β). The need of 'social endorsement,' imitatively secured, as applied to 'cognitive ideas, the actual content of inventions, and to theories, as such' is declared to have 'no existence' and to be a 'mischievous superstition.' This is the reply to the 'sheer mysticism' by which I characterized the opposite view — the view which flatly assumes 'logical' process. But on this I can not retract. Dr. Bosanquet says: 'It is the transference of the ideal postulate of reason, that all valid judgment is valid for all intelligence to the *de facto* social consciousness.'

It is! — not indeed to the *de facto* social consciousness, but to the developed social consciousness *as reflected*[2] *in the judgment of the thinking individual.* Otherwise must we assume an individual essentially unindebted to his kind so far as his logical competency is concerned, a society working by a sort of 'preëstablished harmony' among its members, and something like Rousseau's 'general will' to formulate the most striking, facts of social life. Instead of an 'ideal postulate' this, to me, is a 'postulate of the idealists' (of a certain type). It is logical formalism — and that smacks of mysticism.[3] Thinking has this postulate indeed; but how it gets it — that is the

[1] This is the case with calculation, deduction, etc., which show simply the mind running along its old established mental habits (cf. my earlier reply to Dr. Bosanquet, in the issue of January, 1903). You can't make this sort of logical process ultimate by simply saying that it exists, or that, 'we have to do with nothing but the systematic necessity of knowledge'; indeed Dr. Bosanquet's criterion of truth, the 'fuller systematic cognition of the content,' itself raises the question as to how the cognition can become fuller.

[2] Both by his heredity from ancestors all the while socially selected and also by his extended social education. This *account of*, rather than *denial of*, the individuals' essential initiative and self-determination seems to escape Dr. Bosanquet's notice. (Cf. *Soc. and Eth. Int.*, 3 ed., Chap. VII., VIII.)

[3] Something of what I mean by mysticism is seen in Dr. Bosanquet's chapter on the 'General Will' in the '*Philosophical Theory of the State.*'

question. I think it is one of the characters it owes to selective processes. Only by imitative social experiences can there arise the *identity of content* requisite both for social evolution and also for the individuals' integration in a 'public' situation. Mere identity of logical function — even though it be assumed — would not be sufficient.

1, γ. The 'separation of the imitator and the inventor.' This criticism seems beside the mark. So far from separating the two, I make all invention variation in the imitative processes. There is no mere imitator save the parrot; and there is no pure inventor, save the crank — and neither of these really is!

2, *a*. Here Dr. Bosanquet gives a good account of the more evident features of the theory of 'selective thinking' and then vitiates it by his definitions of 'action' as the instrument of selection. I do not accept either of the definitions he proposes; least of all is 'action' the 'production of change in the external world'; yet it is against this crude conception that most of his definite criticism is directed[1] (and I accordingly leave it unanswered). The other definition on which he would himself accept selective thinking is that action means 'psychical change directed to an end' — in other words, his 'logical process'![2] No, I cannot accept that — although such is always present. But the new — the acquisition — is *ipso facto* in so far the unforeseen. It is what survives, from variation, after an intended organization of contents. It survives (is selected) under two tests: first, its possible organization with the individual's earlier systematized stuff of thought (a psychophysical matter of synergy of motor, largely attention, processes); and second, its relative adjustment in the environment of any sort — intellectual, social, physical, moral — in which the particular item makes a claim. This latter is enforced also largely through motor processes — by the inhibition of some conduct and the encouragement of other, as reflecting thought which suit or inflict those (in the social case) who are appealed to. No truth is established *for the first time* apart from adjustment to an external (material), social, *or some other non-private world.*[3]

In conclusion, it appears to me that Dr. Bosanquet's criticisms, made from the point of view of 'logical process,' lose point for the following reasons:

[1] Unless by 'external' he means unindividual, not private; in that case my reply follows.

[2] "How 'action' operates would be the further question, to which 'logic' would be the answer" (2, *a*).

[3] These selective tests are worked out in detail in the chapter 'Selective Thinking' in the volume *Development and Evolution.*

1. He does not allow that logical process in the individual may not only not forbid, but may require, selective processes operative in the imitative and social functions.

2. He does not admit that social progress in the race may be determined in the individual (by whatever evolutionary method) as ideal or other 'postulates of thought.'

3. He does not admit that systematic thought, under whatever adequate genetic theory, may not only discover but actually *constitute* new phases of cognized reality (or any tenable definition of that term).

4. He does not distinguish between a pragmatic (selective) theory of knowledge, and a philosophy which makes pragmatic criteria the complete final tests of metaphysical reality.

I may finally thank Dr. Bosanquet for the instruction I have derived from this discussion. Whatever 'inventions' have come out of it — they would seem not to have been reached by 'imitative' processes!

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NOTES ON DURATION AS AN ATTRIBUTE OF SENSATIONS.

The relation of any mental process to duration may be conceived in four different ways. In the first place, such a process may occupy a certain period of what we may call objective time, quite apart from any corresponding consciousness of its duration. To the monk in the much-quoted fable, his absorption in the bird's song seemed a matter of moments; its duration in the sense of objective time was a thousand years. Secondly, a mental process may possess duration for the consciousness in which it occurs. Thirdly, it may represent, or be an idea of, a certain duration; as when I run over in a few minutes of objective time the events of yesterday. Or, fourthly, it may be an estimate, a measure of a certain present duration made by reference to accompanying mental processes; as, for instance, the judgments made in time-sense experiments.

In other words, the problem of duration as connected with conscious processes may be four-fold. It may concern the methods of measuring the objective duration of a conscious process; such is the task of reaction experiments. It may concern our simple consciousness of present duration; the difficulty of isolating this aspect of the matter we shall presently consider. It may be to study the conditions which enable an idea to represent a certain duration in past or future time; finding, for instance, that such a duration is overestimated if its contents are various, underestimated if they are monotonous. When Professor Münsterberg says, 'Wir können die Vorstellung eines flüchtigen Momentes lange festhalten, der lange Dauer schnell gedanken,'[1] he is expressing the fact of the independence of the first and third problems; the objective duration, measurable by chronoscope, of the idea of a long duration may be short. Finally, our task may be to find the subjective factors upon which our estimates of present duration depend; this is undertaken by the more recent 'time-sense' experiments.

Discussions of the temporal aspect of consciousness, as they are to be found in psychological treatises or even in special researches, seem not always to observe these distinctions. The third case is easiest to discriminate from the rest; the conditions of practical life make the difference between present duration experienced and past or future duration ideated, sufficiently emphatic. In regard to the others, however, we find two opposite tendencies. On the one hand, the simple consciousness of a present duration is identified with the objective duration of a mental process, measurable by physical duration. And on the other hand, this simple consciousness is identified with subjective estimates or measurements of it. Both of these tendencies, though differing in direction, arise from the same source: the difficulty of thinking of duration except as measured in some way. It may be worth our while to seek for some hint of the reason for this. We do not find the same difficulty in the case of the quality of conscious states. We can consider the quality of a color sensation by itself without being immediately led into the 'How much?' attitude. Now the process of introspecting any mental state is almost inseparably connected with the process of discriminative judgment. But the qualities of conscious states are many and varied; the process of discrimination finds material in the purely qualitative aspect of the phenomenon. The temporal aspect, however, must be quantitatively judged if judged at all; hence introspection is identified with estimate. Precisely the same difficulty arises in the case of intensity and extensity; here, too, the only possible judgments are quantitative ones. Wundt's interpretation of Weber's Law is of course an insistence on the distinction between consciousness of intensity and subjective estimates of intensity. And confusion between consciousness and estimate was responsible for the fact that the Stumpf-James doctrine of 'crude [*i. e.*, unmeasured] extensity' won its way but slowly.

Along with this natural tendency to identify fact with estimate of fact, there coöperates in the case of duration another condition, which

[1] 'Psychologie,' I., S. 247.

leads in the direction of making that estimate a physical rather than a subjective one. As regards intensity and extensity such a confusion could not occur. It is easy to convince the plain man that physical measurements of stimulus, a mode of motion, cannot be identified with measurements of the intensity of sensation; while in the case of extensity he finds it clear that since the spatial relations of the stimulus are lost in the nervous process, and the area of brain surface does not represent that of the body, the spatial character of the mental process and that of stimulus, or excitation, must be disparate. But the case is different with duration: the duration of a physical process is supposed to be absolutely homogeneous with that of a mental process. They can be measured in precisely the same terms; that is, the objective duration of a mental process is measurable in the same terms as the duration of a physical process. Now, the objective duration of a mental process is not the same thing as the consciousness of its duration: their identification is much less justifiable than that of the consciousness with the subjective estimate of duration. But given the tendency to make simple consciousness equivalent to quantitative measure, we can see why in many cases the measure assumed has been one of objective duration of conscious processes, which bear so attractively simple a relation to the duration of the accompanying physical process.

Turning at length from these general considerations to a more special question, there is a difference of contemporary opinion as to whether the simple sensation shall be allowed a temporal attribute. For example, Wundt says: 'A sensation thought of by itself can no more have temporal than it can have spatial attributes.'[1] Münsterberg would not credit the psychic element as such with temporal 'form qualities.'[2] On the other hand, Külpe and Titchener assign duration as a property of all sensations, and Ziehen says: 'Each sensation has a definite duration, which in general corresponds to that of the stimulus,' and absolutely to the duration of the central excitation process.[3] One is naturally led to try and disentangle the real significance of these opposed views, and for this end the question first suggests itself as to what kind of relation between the mental element and duration, and what conception of the mental element itself, are sasumed by the people who deny that sensations have duration. We mentioned four such possible relations at the outset; of these the third is naturally out of the question here. Nobody supposes that a simple

[1] 'Grundriss,' 4th ed., 183.
[2] Cf. 'Psychologie,' S. 289.
[3] P. 130.

sensation, though centrally excited, can be an idea of past or future duration. It is evident, from Wundt's statement, that the fourth kind of relation is the one he has in mind, and that he identifies consciousness of duration with its subjective estimate or measure. Immediately after the sentence just quoted from the 'Grundriss,' and by way of further expository comment on it, he says: 'Position in time can be possible only when single psychical elements enter into certain characteristic relations with other such elements.' Position in time is a quantitative determination, which seems here to be made equivalent to the temporal attribute of a sensation. The same conclusion is suggested in another passage, where he says: 'One time-idea may be more lasting than another, but no time-idea can have absolute duration, for without the double relation of different sensations to one another and to the ideating subject, no such ideas at all could arise.' 'Time-ideas' here must be estimates of time; and a time-idea that lasts longer than another must be a duration estimated as longer than another duration. It is evident that if duration is thus merged in estimate of duration, it cannot appertain to a mental element as such, for estimates can be made only by referring the estimated thing to something else. But why does not the same argument militate against making intensity a property of the mental element? Why should not intensity be identified with estimates of intensity? If it is, evidently the law of relativity demands that it appear only in connection with combinations of elements.

On the other hand, it is possible to deny not merely temporal modes and forms, quantitative determinations of the temporal attribute, to the mental element, but to refuse to admit even the first relation between sensation and time-attribute; one may deny that sensations possess objective duration. I do not refer here to Professor Münsterberg's much-discussed doctrine of the timelessness of the psychical, which seems to me to have been not always clearly apprehended by his critics. So far as I can at present understand that doctrine, he does not assert that mental processes have no objective duration, but only that they have none when considered apart from their accompanying physical processes. Objective duration is not confined to physical processes; it belongs to psycho-physical processes, although not to the purely psychic — which is a matter that need not concern the psychologist if his parallelism is thoroughgoing. But quite aside from this, and granting that mental processes in general possess objective duration, one may still deny that sensations possess it, if one regards sensations not as concrete processes but as purely abstract products of analysis, 'an ideal of analysis rather than an actually existing mental content[1],' to quote Professor Witmer's words. If they are concrete processes, they must be assigned a certain objective duration in which to run their course.

If we now turn our attention to the people who allow the temporal attribute to sensations, and ask in what sense they understand the term duration, the answer is not far to seek: it is objective duration they mean. Külpe plainly says: "To determine the quality, intensity and spatial characteristics of sensation, we have to rely on the subjective methods of sensitivity and sensible discrimination. Duration, on the other hand, may be measured by objective procedure,"[2] and he proceeds to discuss the reaction and frequency methods, which measure no subjective characteristic of the mental process whatever. They determine, not how long it is to the experiencing subject, but how long it lasts as a psycho-physical phenomenon measured by objective standards. The strength of this position lies in the fact that, as we have just remarked, every concrete process must possess objective duration. But can objective duration be regarded as an attribute of a conscious state? Should not the attributes of a conscious state be the aspects which it reveals to the consciousness in which it occurs? Objective duration belongs to a mental process not as that process is an object to the mind immediately conscious of it, but as it is an object to some other mind; as it is ejectively regarded. I may consider my friend's mental state to have had a duration of five minutes measured by a watch; for him, as he is subjectively experiencing it, its objective duration is not represented in any such terms, is not directly represented at all.

We may ask another question. A sensation is a simple mental process. If duration in the sense of filling objective time is one of its attributes, are we not to set any limit to the time it may endure and still be a single mental element? Would the tone sensation produced by a body vibrating continuously through five minutes be a mental element? Such a sensation might outlast the 'psychic present' and be apprehended not as a single, enduring moment, but as a succession of moments having similar content. Could we call such a process simple? And if the objective duration of a sensation may exceed the limits within which a sensation remains single and elementary, what are those limits? Should we say that the maximum objective duration of a sensation is that of a single apperceptive 'moment,' since a

[1] 'Analytic Psychology,' p. 226.

[2] 'Outlines,' pp. 379, 380.

sensation lasting through more than one is for consciousness rather a succession of similar contents than a single process? It seems to me that there are only two ways of avoiding such a conclusion. One is to declare that the sensation is not a concrete process at all, and has no objective duration, being merely a postulate of analysis; the other is to say that the duration attribute of sensation is a purely subjective duration consciousness, and not in any sense identical with that which is measurable by chronoscope and the 'frequency method.' After all, surely the attribute of a conscious process cannot be measured directly by objective means; it is quite as meaningless to say that a sensation attribute can be measured in thousandths of a second, as to say that another one can be measured in grams. The sensation itself as experienced knows no more of thousandths of a second than it does of chronoscope wheels.

But suppose, then, that we allow sensations a psychic duration, as distinguished from objective duration. Whatever the character of this consciousness, it must be contained in the 'psychic present,' else surely we are no longer dealing with a single element. Within this limit, then, sensations must be capable of differing in subjective duration. But can the 'psychic present' have subjective duration? It may have a varying objective duration, naturally, and this objective duration may be estimated in time sense experiments by being inferred from certain accompanying phenomena. But is not the subjective present, as such, without subjective duration? This is the thought expressed by Volkmann, as follows: "In der Vorstellung der Gegenwart als solche ist die Dauer nicht eingeschlossen, denn mag auch die Gegenwart andauern, wir werden uns dieser Dauer darum doch nicht schon unmittelbar bewusst, denn auf die Zeitfolge bezogen ist die Gegenwart nur der absolute Mangel jeder Zeitbestimmung." He adds a sentence which sharply distinguishes between subjective and objective time: "So wenig wir die Vorstellung der Folge haben, weil wir aufeinander folgende Vorstellungen haben, so wenig werden wir uns die Zeitdauer schon dadurch bewusst, dass eine Vorstellung andauert."[1] It is a corollary from this view that the psychologically primitive time judgment is not one of duration but of succession. The psychic moments follow each other; they have no duration. The perception of duration is the perception of two or more such successive moments having similar contents.

It is possible, then, to deny on three different grounds that sensations possess duration: First, because their duration cannot be sub-

[1] 'Psychologie,' II., S. 20.

jectively estimated or measured without comparing them with other mental processes. This view identifies fact with quantitative estimate of fact; and it would surely be equally possible to deny sensations intensity, since one intensity can be estimated only in relation to others. Second, because sensations are held to be mere abstractions, not concrete processes, and therefore possess neither subjective nor objective duration. Third, because it is held that a sensation which lasts, objectively, longer than the psychic present is not a single element, while one that lies within the psychic present has subjectively no duration. While it seems absurd to treat their objective duration as one of the attributes of sensations, yet the only other way to maintain that they have a temporal attribute is to hold that the subjective present does possess subjective duration, a view which I think introspection does not confirm.

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IMAGERY.

While it is generally recognized that in psychological experiments great importance attaches to the determination of the general sense imagery type of the subject or observer, it is not yet proved that a subject is always of the same type. That is, he might possibly be of one type in one set of experiments and of another type in another set. Or if it be objected that a type in this sense is too stable to suppose such changes to be possible, it is yet conceivable that an individual may employ at one time say in one set of experiments one kind of supraliminal[1] images, and in a set of experiments with conditions changed another kind of images. Or it may be that, for certain researches, one type of person is preferable to another. Thus, for example, in the research of Miss M. L. Nelson (Psych. Rev., Sept., 1902) on the 'Visual Estimate of Time,' the irregularity of the results may be explained by the fact that her subjects were not of the right type or that their type was not fully determined or kept constant. The subjects in this research were told to compare empty time end-marked by

[1] In order to make myself as clear as possible I shall use the term images or *supraliminal images* referring to those of which the subject is conscious and *subliminal images* those necessarily presupposed images that do not at the time enter into consciousness, but whose effect cannot be denied. Cf. Ebbinghaus, 'Grundzüge der Psychologie,' I., p. 53: "Wir gelangen also zu dem Resultat: unbewusste Vorstellungen sind zwar nichts den bewussten und uns bekannten Vorstellungen direkt Ähnliches, aber sie sind trotzdem als etwas Psychisches irgendwelcher Art anzuerkennen. * * * "

flashes of light with other empty time or with time filled with the same kind of light flashes. Miss Nelson says that this is what was done with short times by Meumann. But between the researches of Meumann and Miss Nelson there is a great difference, as will presently appear. In the research now under consideration the results were conflicting. It seems inevitable that if the present writer tried that experiment, the empty times, if he 'sat in a darkened room' in absolute quietness, would be subjectively filled with auditory imagery. This auditory imagery might conceivably take the form of a conversation in words or of a piece of music. In either case the original purpose of the experiment is frustrated. It was to compare the estimate of times objectively filled with visual stimuli, with times objectively empty (in some of the experiments), the result being the demonstration of an illusion (which for one of the subjects grew less as the standard time was increased from one half a minute to ten minutes). Now with the present writer, who has made a number of tests on the ability to estimate time intervals by means of auditory imagery, the illusion would vanish entirely after he had trained himself to use a certain piece of music as a measure of the time. Thus, *e. g.*, mentally start the theme of the *allegretto* movement of Beethoven's Seventh Symphony when that light was seen which marked the beginning of the standard time length, and note the bar of music when that light appeared which marked the end of the standard time length; and repeat the process, beginning with the flash marking the beginning of the portion of time which was to be subjectively measured. Whether the subjectively measured time were filled with flashes or not would make little difference if the attention were concentrated upon the internal music. But, it may be answered, the subjects in this experiment were asked not to use any such method. Miss Nelson has not stated whether this is the case or not. If they were, it would still amount to the same thing, for it would be impossible to prove (from what is told us of the conduct of the research) that *subliminal* images had not had some effect upon the time estimate. But Miss Nelson reports that the filling of an interval 'does not affect all three subjects alike.' Now this fact may be explained in the manner which I have just indicated, *i. e.*, a train of auditory imagery which has the objective quality of temporal protensity, may have helped some of her subjects. It may not have. But Miss Nelson has not told us positively that her subjects had no auditory imagery, and until she does, we cannot think that the experiment was performed with due regard to the mental content of the subjects.

The only remarks Miss Nelson makes about the introspection of her subjects is that they had difficulty in keeping their attention on the length of the standard (p. 449); consequently in some of the experiments she told them whether the standard was to be filled or unfilled, long or short; and that they 'expressed a dissatisfaction with their estimates and felt that they made little, if any, difference between the longer intervals' (p. 454); and on page 455, "the difference due to the filling was, I think, merely a difference in the direction of attention, the monotonous regularity of the lights being, in general, a means of holding the attention and preventing the mind from wandering. From this point of view the filled time was psychologically the more empty or barren of the two * * * the time being filled with monotonous sensations of light, but empty of vivid or interesting trains of thought." There is another remark that indirectly throws some light (p. 454). "During the longer periods it was impossible to keep the attention so closely fixed as during the intervals of one half and one, or at most two minutes. It is at about this point that the change of sign occurs in the estimates. The general feeling of weariness seemed to be the chief criterion in the longer intervals." The present writer thinks it extremely unfortunate that Miss Nelson did not either give her subjects something wherewith to keep their attention directed upon the standard or ask them what their mental content was during the time that they were paying attention to the standard. It seems necessary that the subject have some train of thought during the 30 to 600 seconds that they sat waiting for the flash to come which marked the end of the standard time. It is conceivable that after the first filled time they may have had (during an unfilled time) visual images of the flashes of light. It would be interesting, not to say important, to know this because we might voluntarily image flashes in rhythm. It is also conceivable that the subjects or any given subject may have had auditory imagery. As I have above shown, auditory imagery has a very real temporal quality; so that if the subjects of the present research had auditory imagery during the time of the experiment, it would be at least interesting to know this. It is possible, too, that any other of the half score or so of available kinds of mental imagery may have been in the consciousness of the subject, but we are not informed as to the facts. Now the really important question is the relation these different kinds of images bear to the time sense. Auditory images seem to the present writer to be the best as a means for judging time; but he realizes that motor or tactual might do the same service in other observers. The question as to the estimation of filled or unfilled time really, therefore, involves the question as to the elements of the stream of consciousness during the unfilled time. Here, however, we have a psychological experiment in which the conditions are all arranged for introspection, and, in spite of the fact so well stated by Professor Titchener, that 'A psychological experiment consists of an introspection or a series of introspections made under standard conditions' ('Experimental Psychology, Students' Manual, Qualitative,' p. xiii), the introspections are not recorded, or if recorded, not published. And it is apparent that the figures given are of little or no value, as that which they ought to enumerate is not even mentioned.

It is to be noted in defense of the position maintained above, that Meumann in his article referred to by Miss Nelson ('Beiträge zur Psychologie des Zeitbewusstseins,' *Phil. Studien*, XII., 127) meets the objection that images may creep into the mind during the times he is studying and so perturb the numerical results, by saying that of course there are really no absolutely empty times; but that there is no use in multiplying words over this fact (which he terms a *Binsenwahrheit*); and claims that in the short times that he used, viz., up to four or five seconds, one could keep images in a state of complete inhibition (in einem Zustand totaler Hemmung). But even Meumann does not claim immunity from images above five seconds, and Miss Nelson begins at thirty and goes up to six hundred. The present writer thinks, therefore, that not only are these experiments unsatisfactory, but they are not related to those of Meumann; as, if Meumann's statement be accepted, there is really an entirely different factor in the present experiments which has not been taken into account.

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PSYCHOLOGICAL LITERATURE.

Einleitung in die Philosophie. HANS CORNELIUS. Leipzig, B. G. Teubner. 1903.

Professor Cornelius defines philosophy in terms of endeavor, not of content, as 'Streben nach letzter Klarheit.' He examines the dogmatic methods, 'material-monistic' and 'idealistic,' and finds that both have failed of reaching this 'ultimate clearness.' The only adequate method is, he declares (S. 164), the 'empirical,' which starts from facts, not from assumptions. But all facts are, in the last analysis, facts of consciousness, and a study of consciousness is, therefore, a preliminary procedure of philosophy, which is in so far identical with psychology (S. 168).

A large part of this 'Introduction to Philosophy' consists, thus, of psychological analysis and classification. The most significant feature of the analysis is its recognition of the relational elements of consciousness, the *Gestaltqualitäten*: plurality, similarity, identity and the others (S. 208 *seq.*). The author lays his greatest stress, however, on the unity of experience, pointing out (S. 206 *et al.*) that the elements of consciousness are distinguished only by abstraction and that every experience comes to us as term of a series or member of a system (S. 248). "The fundamental fact, * * * " he says, "is no other than the unity of our psychic life, by virtue of which every event must stand in definite relations to others" (S. 250).

From this direction, the writer reaches the conception — closely akin to Kant's — of the objective world as 'the orderliness of our perceptions as known through experience (*die erfahrungsmässig erkannte Gesetzmässigkeit* unserer Wahrnehmungen' (S. 264). The permanence which characterizes the world of experience, is due simply to the constancy of our meanings: "We have found, for example, that there are objects characterized by the attributes which we learn to know under the name of salt. In future, therefore, we can classify an object as salt only if we discover in it all the attributes which characterize this concept in its previous meaning" (S. 291). And the "universal law of causality is nothing other than the demand — indispensable to the unity of our experience — for the ordering of all phenomena under constant empirical connections" (S. 294).

To the writer of this review, the great defect of the book is the one-sidedness of its idealism. The thoroughly idealistic character of the world of experience and the impossibility of a world beyond consciousness are brilliantly demonstrated; but there is nowhere any consideration of the selves who experience and are conscious. The 'I' is treated simply as one phenomenon among others (§ 31); and the existence of other selves is regarded (S. 323) as a mere probability. The result of this neglect of the self beneath phenomena is, first, a philosophic system, partially the Kantian, which lays stress upon the categories but leaves out of account the 'transcendental I,' and second, a phenomenalistic psychology which inadequately describes our consciousness in its fundamentally social aspects.

The book is clearly and well written and abounds in telling illustration. The criticisms of philosophical systems are skilfully introduced and are almost uniformly valuable.

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The Psychology of Ethics. DAVID IRONS. William Blackwood and Sons, 1903. Pp. xviii + 172.

Dr. Irons' little book naturally falls into two parts, as he tells us in the preface, the first covering the same ground as, and differing little in statement and not at all in theory from, his articles on the emotions that appeared in the *Philosophical Review* about 1897; the second dealing with man's active nature, and seeking to establish the ideal of worth as its supreme regulative principle, this last position having been in part anticipated in the author's 'Natural Selection in Ethics' that appeared in the same periodical.

The author's main points may be briefly suggested. As an essentially active being, man is characterized by many primary tendencies to reaction; pleasure is the result of their success, pain of their failure; and the emotions are various 'feeling-attitudes' excited by situations made, and felt to be, significant because of their relations to the above reactive tendencies, and to the interests incident to them. And pleasure, pain, and emotions, of course, add secondary tendencies to reaction to the primary tendencies mentioned.

Were this the whole of man's active nature, chaos would result. But he is also characterized by a tendency to realize his ideal of worth, his notion of what he owes to himself, and 'all his particular impulses must be brought into the service of this end.' Moreover, this end is eminently concrete, what each man owes to himself being relative at once to his nature and capacities, and to his place in the social organism.

One lays down the book with some reservations incident to Professor Irons' deliberate dependence on unaided introspection. Are not the theories advanced colored by the exclusive observation of a nature reflective and philosophic out of the common, and do they not, in resting on the observation of an individual, unduly neglect the social bearings of man's moral nature?

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De la Réalité du Monde Sensible. JEAN JAURÈS. Deuxième Édition. Paris, Alcan. 1902.

In this volume of 428 pages M. Jaurès presents an elaborate system of philosophy in which the chief problems of metaphysics, cosmology and psychology are about equally considered. It is manifestly impossible in the space at our disposal to do even rough justice to a work of such scope, and we shall content ourselves with a brief summary of the author's main conclusions, and a few comments upon the points that would seem of most interest to the readers of this journal.

M. Jaurès is at once too eclectic and too original to allow of our classing him with any one school. The cardinal principles that appear to govern his philosophy are: First, the Rosminian acceptance of *Being*, in its two aspects of activity and potentiality, as the highest and most significant category; second, the Cartesian identification of all energy with motion, and the consequent correlation of all sense-forms with modes of motion; third, the Spinozistic belief in extension as a true and essential attribute of the one Reality; fourth, the Hegelian and Schellingian belief in the purposeful, meaningful, and living character of physical nature.

In his first chapter the author considers the several criteria that are actually used to distinguish the real from the unreal. Persistence in time is the first mark of reality. That which is felt as the same at different moments is regarded as real. Touch gives a more vivid notion of persistence than sight, hence the tactual and with it the resistant are accepted as marks of reality. The coincident testimony of sight and touch is, however, a truer criterion than either alone; indeed it is not until we get the various attributes simultaneously presented through different senses that we clearly perceive the real. But many attributes cannot be thought of as existing together without some inward bond. The true reality for perception is thus a *substance* with its attributes. Now the unity of a substance could not be apprehended in perception unless the intellect possessed *a priori* the concept of unity. It is then the intellect which enables us to perceive reality; and the *de jure* criterion to which the examination of the *de facto* criteria has led us is *intelligibility*. The real is simply the intelligible. Dreams are rejected as unreal because though containing a measure of connectedness they lack the fuller unity and intelligibility of waking life.

Having thus attained a criterion for distinguishing the real from the unreal, our author is ready for the task which gives the title to his book. He will prove 'the reality of the sensible world' by showing that world to be intelligible, first in its general constitution, second in its specific manifestations.

It is a notable conception, this of M. Jaurès. As a natural realist and in the interest of natural realism he proposes to take as supreme criterion a principle which has for the most part been regarded both by realists and non-realists as the exclusive property of idealism. The world is real, but it owes its reality to its rationality. While we sympathize profoundly with our author's contention that a rationalistic realism is the only realism that is capable of withstanding the onslaughts of idealism, we must dissent from his interpretation of intelligibility as being far too narrow. Dreams and illusions usually possess a measure of intrinsic disconnectedness and irrationality, but M. Jaurès appears to us quite wrong when he asserts this irrationality as the ground for their unreality. Such disconnectedness is in fact often lacking in the dream, while the quasi-dreams of art are entirely without it. The world in which Hamlet moves has surely a finer intelligibility than the humdrum world of our waking experience. We reluctantly reject it as unreal, not from any want of intrinsic intelligibility or possibility, but because it lacks extrinsic intelligibility or 'compossibility'; it cannot be harmonized or made continuous with the totality of our waking experience. It is thus extrinsic intelligibility or compossibility which together with intrinsic reasonableness constitutes our criterion for testing reality. And it is only by considering this second aspect of intelligibility that we can appreciate the justness of the empiricist's contention that that which is *given* through the senses and independently of the will is the primary reality with which all other experience must accord on pain of being classed as 'unreal.'

The first use which the author makes of his criterion is to deduce from it *a priori* the general constitution of reality. The intelligible must be first of all *one*, but it could not be merely one without, so to speak, collapsing into nothing. It must therefore manifest itself in a system of many intelligible unities. These must be united by the external bond of spatio-temporal causality and also by a common striving toward the ideal. The ideal, however, could only be attained by beings that existed continuously in space and in time, and possessed a nature partly actual and partly potential. Indeed Being, which lies at the heart of everything, may be seen to have two aspects, actuality form or quality, and potentiality extension or quantity. Being *in actu* is God, Being *in potentia* is the World. These two aspects of Being are co-eternal though potentiality is logically second to actuality and ontologically dependent on it. The manifestations, or, as Spinoza would say, the 'modes' of Being result from the eternal permeation of the 'attribute' of potentiality by the 'attribute' of actuality. They thus reproduce in their own nature the duality of Being. Looked at from the side of quantity or potentiality they are motions, while from the side of form they are sensations.

So much of the general character of the real world can be seen to follow *a priori* from the demand for its intelligibility. But these high and general deductions need to be inductively verified and extended by a study of the manifestations of Being in their detail, and the six remaining chapters treat in turn 'Motion,' 'Sensation and Quantity,' 'Sensation and Form,' 'Space,' 'The Infinite,' and 'Consciousness and Reality.'

In the chapter on motion, it is shown, first, that motion itself reproduces the eternal duality of being *in actu* and being *in potentia*. Motion even as such is not purely indeterminate or quantitative. It must have a definite form or character. Modern science regards 'gross matter' or that which moves as being itself a mode of motion. The real substrate of motion is ether, or simply, extended homogeneous being. The unity of this being is manifested in the indestructibility of matter and energy.

The question as to whether matter originates *in time* from the ether is considered and answered in the negative (p. 127). Each distinct form of motion is correlated with a form of sensation, but each form of sensation has a unique and irreducible meaning, and hence cannot have had an origin. In pure ether only undifferentiated motion would be possible, therefore differentiated material bodies share the eternity of the sensory forms with which they are correlated.

The most important point in the chapter on motion is the defence of the Cartesian view that all energy is energy of motion. Force or potential energy is simply 'motion that completes itself in infinitesimal time' (p. 75). Instead of developing and elucidating this promising though somewhat enigmatic definition, the author, as it seems to us, weakens his case by attempting to show that the truly kinetic character of 'potential' energy is evidenced in the molecular changes that result from the *fatigue* of bodies subjected to strain.

In the chapter on 'Sensation and Quantity' many considerations are adduced to show that sensations are not only related by quantitative laws, but that they actually contain quantity. Selecting M. Bergson as representing his opponents, our author argues (pp. 164–168) that the contention that sensations differ only in quality is false for two reasons. First, the objective quantities of the physical world could only be presented in perceptions which participated in their nature. The soul could not perceive motion and extent unless it moved and was extended. Apparently M. Jaurès would not shrink at all from the paradox that the perception of a square is a square perception. This revival of the quaint psychology of Democritus will be less convincing to the average reader than the second reason which is alleged in support of quantified mental states. We estimate the quantity of an object by the quantity of sensation that it produces, and not *vice versa*. If two balls, one large and hollow, the other small and solid, be dropped to the ground, the greater intensity of the sound coming from the smaller is regarded as an immediate and sufficient justification for attributing to it a greater mass. The fact that each change in the intensity of a mental state involves a change in its quality is the only objection which M. Jaurès considers as at all serious. He explains it, however, as the result of the limited capacity of finite consciousness. On account of this limitation every quantitative change alters the structure or quality of the psychosis in which it occurs.

The theme of the chapter on 'Sensation and Form,' is the extramental reality of the objects of the several senses. They are shown (1) to require an extended world for their manifestation, and (2) to be expressions of eternal meanings. These 'secondary' qualities constitute a bond between the 'primary' qualities of being *in potentia* and the ideas of pure intellect — being *in actu*. Thus light expresses the unity of being with itself, and is consequently correlated with motions in the one homogeneous ether. Touch, on the other hand, expresses the irreducible disparateness and individuality of the many material manifestations of being. Sound expresses the communication of beings with one another.

The chapter on 'Space' is chiefly interesting for the criticism of Kant (pp. 335–340). Space is admitted by Kant to be the 'image' of quantity. But quantity is a category, and as such possesses, if not an objective, at least a transhuman validity, hence space as its 'image' should possess an equal validity, and not merely the arbitrary and human scope which Kant ascribes to it. Moreover, unless the forms of sensibility were intrinsically connected with the categories, through the idea of being (which Kant overlooks), it would be impossible for the 'imagination' to effect their union.

In the chapter on the 'Infinite,' the author devotes himself mainly to vindicating space from the charges of self-contradictoriness that have been made against it by Zeno and Kant. Those philosophers err in regarding space as a composite or aggregate, when, as a fact, it is a continuum and therefore a unity (pp. 350–360). The parts which we think of as composing space are nothing but arbitrary and subjective divisions. Space as a whole is logically prior to all such 'parts.'

The last chapter discusses the relation of consciousness to reality. M. Jaurès admits to the idealist that consciousness or egoity is as truly an aspect of being as is extent. Every finite ego presupposes, and, in a sense, contains the absolute ego. We must not, however, accept solipsism. We can only reduce the universe to our ego by raising our ego to the infinity of the universe. The Absolute or God is a conscious ego, but as he is possessed of no organism he cannot be regarded as an individual or as a person (p. 427). Even in the sphere of finitude we can feel that individuality is not essential to consciousness. The alleged facts of telepathy show the possibility of extending our consciousness far beyond the limits of our organism.

M. Jaurès' work seems to us, on the whole, an important contribution to philosophy. As literature it is wholly charming. The book is pervaded by an atmosphere of poetry and mysticism that harmonizes curiously well with the author's Cartesian lucidity of style, and his love for quantitative conceptions. And here, as in the work of Guyau, the illustrations which are used, and the piquancy with which they are presented, go far to reconcile us with details that from a coldly philosophic standpoint might appear as fantastic and inconsequent.

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Psychologie du Rire. L. DUGAS. Paris, Alcan, 1903.

Another of the French monographs, written under the fruitful influence of Ribot, this study of laughter takes as its text one of the theses of the master in his 'Psychologie des sentiments.' "Laughter," he had said, "manifests itself under so many and such different conditions — physical sensations, joy, contrast, surprise, the bizarre, the strange, the base — that the reduction of all these causes to a single principle remains quite problematical." Following out this thesis, M. Dugas comes to a wholly negative conclusion as to the ability of scientific method to give anything like a unitary explanation of the many phenomena of laughter. The work is thus wholly descriptive and critical, contains no new principle or hypothesis, unless indeed this negative attitude be looked upon as a novelty. His first point of critical attack is the conception that all forms of laughter can be reduced to a unitary physiological explanation. The physiological theories of Spencer and Bain which will explain elementary and simple forms of laughter, are, however, too abstract and simple to comprehend the species of laughter described as ideo-emotional. For these phenomena other principles, intellectual and moral, have been introduced by thinkers of varying tendencies. The intellectualistic theory which would reduce all forms of ideo-emotional laughter to reaction upon contrasts and contradictions of ideas; the pessimistic theory which would find the source of such laughter in the ethical moment, the sense of superiority of self over others, are in turn subjected to a criticism, the outcome of which is the recognition of these as subsidiary principles of explanation, as applicable to limited groups of facts, but which have, unfortunately, received an undue extension at the hands of men of genius with whom they had become fixed ideas.

His own view leans more toward what he calls, in distinction from the preceding theories, the æsthetic. All humor, ideo-emotional laughter, begins as play. "But from the fact that we recognize play as the essential and characteristic element in all laughter it does not follow that all forms of laughter are thereby reduced to unity. On the contrary, by its variations, playful laughter engenders the different species of humor." Laughter, which began as a playful impulse, differentiates itself into the intellectualistic and quasi-moral types. The latter are, in a sense, degenerations of primitive laughter.

M. Dugas has essayed a qualitative and quantitative classification of ideo-emotional laughter, according to the character of the emotions expressed and the emotional intensity of the expression. The quantitative classification is based upon linguistic symbols for different degrees of laughter but, since only the French language is taken into account and no attempt at comparative study is made, the result is suggestive rather than convincing.

In addition to this negative scientific view of laughter, he has also developed negative conclusions as to the moral and social functions of laughter. Laughter is in itself not a selective principle in ethical judgment, stigmatizing that which is opposed to the ideal of human perfection or that which varies from recognized social constants of judgment, sentiment and character. It is, in itself, non-moral and non-social and only accidentally produces effects of moral and social value. Ridicule may equally produce immoral and anti-social effects. His conclusion then is that laughter, from every point of view, is an accident, an expression of the individuality; it discloses as many forms as there are different characters, minds and states of the mind. It seems then to the author that it cannot in any true sense become an object of science. Practically, it may be an object of desire or aversion but not of volition — it cannot be taken for an end of action.

WILBUR MARSHALL URBAN.

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La Mimique. ÉDOUARD CUYER. Bibliothèque internationale de psychologie expérimentale. Paris, Doin, 1903.

This monograph by a painter and professor of anatomy has only an indirect value for the psychologist, for while it undertakes a very minute study of the variations of emotional expression in the different parts of the body, it is the expression for the artist which is constantly kept in mind and there is very little, if any, analysis and classification of emotional states on distinctively psychological principles. If, however, the student of emotional expression should ever develop a method for the experimental isolation of qualitative differences in emotional states, he would have a wealth of fact at his disposal as a result of M. Cuyer's painstaking work.

The author's method of determining the facts of mimetic expression is both analytical and synthetic. On the one hand the various muscles and groups of muscles of the face, head, arms, shoulders and lower limbs, etc., are all examined separately to determine, in connection with the separate emotions, the slightest variations of movement which will suggest distinct emotions. This analysis of expressive movements is controlled by the principle that the breaking up of emotional expression shall be carried only so far as a change in a single feature will unmistakably signify a distinct emotion, as when changes in the angles of inclination of eyebrows or lips have these suggestions. The minuteness with which these variations of eyebrows, nose, mouth, lips, wrinkles on the forehead, movements of the ears, accompanying variations of emotions, have been quantitatively studied with the help of photographs and works of art (there are 75 figures in the text) is extraordinary.

The synthetic study, on the other hand, seeks to reconstruct the various emotions in their fulness by combining the various movements thus analyzed out into total expressions. The author discloses no laws governing these combinations. In fact, his method is wholly empirical. He gives us some 115 varieties of emotion in alphabetical order with their corresponding synthetic expression. It is certain, he tells us, that the number of combinations is practically infinite as the variety of nuances of emotion is indeterminable. The only general principle which he notes as governing these syntheses of elementary movements and attitudes is the distinction between normal and abnormal combinations. Following Professor Pierret, he finds the synthesis of expressive movements in the normal individual rapid, concordant, adequate, homogeneous and persistent. In the abnormal individual they are slow, discordant, excessive or insufficient, disassociated and fugitive.

Some of the psychological assumptions upon which M. Cuyer has based these analyses and syntheses are open to criticsm. This is notably the case when he supposes that involuntary and voluntary, or mimetic, expression of emotion may be taken without distinction as the basis for his studies. In mimetic expression, however, there are certain processes of abstraction and conventionalizing of movements the psychology of which the writer has ignored entirely. With all these psychological defects, and they are in some ways serious for the psychologist, although perhaps unimportant for the scientifically minded artist for whom the work is primarily intended, it remains true that this study contains material of value and is, in a way, an extension of the work of Darwin, Bell and Duchènne. A historical résumé of the contributions of these writers to the subject adds to the worth of the monograph.

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On Active Attention. F. H. BRADLEY. Mind, XI., No. 41, Jan., 1902, pp. 1–30.

The purpose of this article is 'to fix the meaning of active attention in accordance with the ordinary usage of language, and next to deal with a certain number of questions concerning it.' The word attention is to be used in the sense of active attending, and the reader is asked not to forget that volition is assumed to consist in 'the self-realization of an idea.'

"The mere having of an object or objects is by itself not attention." "To attend in the proper sense I must by my action support and maintain an object in myself, but we have attention only so far as I maintain it theoretically or at least perceptively." Moving one's hand or eye to gain knowledge about an object need not involve attention. "My end in attention is to maintain an object before me with a view to gain knowledge about it." Attention is thus negative of any mere psychical interference with the object and its knowledge.

But attention implies also a volition on my part. When listless or absorbed, I may notice a bird fly across the field of vision without attention. An idea may develop itself theoretically before me without attention. Not that attention is the same thing as will, and not that all attention is directly willed. It may be directly willed but need not be so. Wherever an end of any kind involves in and for its realization the maintenance and support of an ideal object before me and in me — that is attention (p. 8). "The ideal development of the object in me is thus, directly or indirectly, the realization of my will" (p. 10).

So-called passive attention 'may be called the mere occupancy of myself,' and this is not essential to attention. Immediate action upon a sensation or a perception need not involve attention; and apperception, the modification of a sensation by a disposition, is not an attending. And yet, this activity of apperception 'may be said, if you please, to cause in a certain sense attention to the object' (p. 11), but we have first been impressed and laid hold of by an idea (= any suggestion even when coming straight from a perception) (p. 29). "Our will to realize this idea in external action and in inward knowledge is but the self-realization of the idea which so has possessed us. And you cannot, if you keep to facts, maintain even that the suggestion holds us in all cases because it arouses desire or even pleasure." We cannot get rid of ideo-motor action, and it is idle to deny that at least some idea-motor actions are volitions. Sometimes an idea whose psychical origin is apparently casual or undiscoverable is simply 'there' and remains 'there'; it 'goes on to realize itself and in this way unfeelingly forces, we may say, our will and our active attention' (p. 30).

On Mental Conflict and Imputation. F. H. BRADLEY, Mind, XI., No. 43, July, 1902, pp. 289–315.

Divided will, conflict of ideas in desire and impulse, alleged action contrary to will, and the principles on which we impute actions to ourselves, or again disown them, are the topics of this paper. "Volition I take to be the realization of itself by an idea, an idea (it is better to add) with which the self here and now is identified" (p. 290).

The paper first discusses the alleged case of action realizing an idea contrary to will — such as yielding to morbid desire for drink. The author holds that two ideas cannot be present at once, and that where they immediately succeed the one the other the self is not identified with both of them in the same degree. All my acts are mine, but they are not equally or in the same sense mine, as is shown by the following: (1) Viewing the self in its material aspect, we all distinguish between our true self, our self taken as a whole, and the lower or chance self of any moment. (2) Formally, we regard the more universal as the higher and the more mine; but the universal is often abstract, and hence may be higher in one respect while in other respects it is lower and worse. (*a*) A course will be formally higher when it explicitly and consciously asserts a principle, instead of embodying it unconsciously. (*b*) To adopt a course reflectively is higher than to adopt it at once and unreflectively. (*c*) *A* and *B* may be incompatible and known to be so, each involving the negation of the other, or they may be so related that *A*, for example, includes *B*, and in the latter case *A* will be the higher and the more mine. (3) An idea which is pleasant or more pleasant is so far higher and more mine, and one which is painful or more painful is lower and less mine.

The author next proceeds to give reasons for not accepting the alleged fact of action contrary to will. In the end he holds that if the alternatives are really incompatible and are known to be so, they cannot, 'while really taken thus as alternatives, be present together, and we are able to think this possible only because we really do not take them as opposites.' A man cannot knowingly and willingly do what is bad. And yet, the author admits that an idea which is contrary to will may get itself carried out (where the ideas are abnormal), denying merely that any act of this sort is volition. The reader feels at first that Mr. Bradley has yielded his case, at this point, to Mr. Shand, but he goes on to say that the idea which is carried out in such a case is really not an idea at all, because held subordinate to the alternative idea and negated by it (p. 309). Mr. Shand contends that ideas may realize themselves, in Mr. Bradley's sense of the word, without volition taking place.

If I hold the idea of another person's doing a thing and this thing follows in me, this is not volition; and if I imagine myself in a certain state, and my imagination is thereupon realized, this is not volition; for in neither case can the result be shown to follow as a genuine consequence of the idea, and in neither case is the idea had in mind the one realized in action.

The paper closes (pp. 314–315) with a restatement of the principles upon which the difference and the degrees of mine and not-mine rest. (1) If I can bring and retain A not-b before my mind and cannot do this with B not-a, B is so far higher and so far mine more truly than A. (2) The same is true, if, taken on the whole, B is more pleasant or less painful than A. (3) If A is the outcome of and represents deliberate choice, while this is wanting in the case of B, B is so far the lower and less mine. (4) If A appears as falling under a principle, while B falls under a principle lower and less general or under no principle whatever, A will to that extent be higher. (5) Lastly, the most important criterion of all consists in the material difference of content. If A represents some minor interest of my being, and if this feature is not contained or is to a less extent contained in B, B is so far lower and is not mine.

The Definition of Will. F. H. BRADLEY. Mind, XI., No. 44, Oct., 1902, pp. 437–469.

This paper proposes an explanation and defense of the author's definition of will as 'the self-realization of an idea with which the self is identified.' Violition involves the following aspects: (1) Existence, (2) the idea of a change, (3) the actual change of the existence by the idea to (4) the idea's content and in such a way that (5) the self feels itself realized. Assuming 'provisionally the existence of what is called ideo-motor action,' defined as 'the tendency of an idea to realize itself,' the paper proceeds to discuss the above five points.

(1) Existence is either the actual series of events that is now and here, or continuous with my now and here. (2) This existence must be altered in volition beginning 'now' with the 'present.' Even the will to continue the present in a certain character is a will for alteration. The alteration must not be merely ideal, but it must be an alteration to the character possessed by the idea, and it must be produced by the idea. Here follow discussions of several phenomena which in the author's view are not volitions, viz., resolve and intention, will in paralysis, and disapprobation or approval. In resolve, the existence to be altered by the idea is severed by a gap from the actual present; and after abstracting from the result, that is, from the actual realization of the idea, in volition, what remains is so far only an incomplete will (p. 446). We may roughly distinguish between two stages in volition, (1) the mere prevalence of the idea, and (2) the advance of the idea beyond its own existence toward its physical or psychical end. If taken strictly, the first stage 'is not a complete or

really an incomplete act of will,' and yet, 'viewed otherwise and under some conditions, the prevalence of the idea does amount to an incomplete but actual volition' (p. 446). The prevalence of an idea is a process whose stages the author goes on to develop, and the last step in the process in most cases involves an actual change of fact to correspond to the idea. "There will not be any case even an incomplete volition unless to some extent the idea carries itself out beyond itself. Where this aspect fails, there will at most be a doubtful experience, due to a confusion between imagination and fact." Even in the paralytic's will, this progress of the idea beyond itself is to some extent present, and the will to recollect also illustrates the point. As to approval and disapproval, "To approve of things as they really are or as they are imagined to exist, is to take an attitude in itself contrary to actual will. * * * Disapproval in itself is not will and, so far as it becomes will, it falls under negative volition" (p. 454).

The author next discusses the doctrine that all will involves a judgment or belief about the future or at least about the possibility of the end, coming to the conclusion at last that such judgment is not essential to all volition because it cannot be discovered in all volition. Two circumstances combine to make this doctrine seem plausible: (i) we often express the fact of a volition or a resolve in a judgment, and (ii) one cannot will to realize the impossible, and hence it would seem that in willing we must judge the end to be possible. Neither is desire essential to will, nor is choice, nor is active attending.

The discussion proceeds to the objection that an idea is not essential to will. This objection is based on what the author regards as a mistaken view of the nature of ideas, and an account of ideas is here introduced. (i) Our apparent idea and our real idea may be fundamentally different — the former may be but a part of the latter. (ii) An idea may exist in volition and may yet be unspecified and general. (iii) An idea is itself not an image, nor is it always even based on an image as distinct from a perception. An idea may be the 'meaning' of an object, a meaning which does not involve an image and which may be detached from the object. And yet, in all this, the reader is apt to feel that ideas are not defined, and that much vagueness still hangs about them. Actions from imitation, from word of command, and from impulse illustrate volitions which are said to be without ideas, the action being suggested by some perception. Here the author holds that 'If in the act an idea is suggested and realizes itself, that act is volition, unless the idea has in some way lost its own character and has in effect carried out something which is not itself.' For example, "The idea of another man striking, if as such it causes me to strike, is so far not a volition. And the same conclusion holds if the idea was of another desiring or ordering me to strike" (p. 466). Finally, the author briefly discusses the mistaken view that the end in volition must be realized for us, and that it is so realized when our idea passes into perception. One may desire objects which he knows, or might know, will never satisfy him. And yet, 'Desire is an inconsistent state, I agree, and its inherent contradiction, I agree, should be removed by satisfaction' (p. 468).

Of these three weighty articles, one fifth of the lines are in footnotes, and the author's method is that of defending and proving propositions which have previously been laid down in other writings. Doubtless many readers have found the task of mastering them somewhat difficult, and at the risk of exposing his lack of penetration the present reviewer is disposed to mention some of the sources of his own difficulties. 1. In the paper on 'Active Attention' (*Mind*, N. S., No. 41), we are told that active attention is not necessarily directly willed (p. 8), and is then referred to as 'the willed procurement' of psychical occupancy and dominance. Were this merely an accident in the use of words, it would not need mention here. Understanding that active attention is not always directly willed, what is the difference between active and 'passive' attention? Passive attention, attention 'in the low and perhaps improper sense of psychical dominance and occupancy,' is referred to on page 29 as a use of the word in an 'improper sense.' One of the subtle themes of the paper seems to be that all attention, properly speaking, is active. The reader is repeatedly warned in footnotes and elsewhere that the word attention is always used in the sense of active attention except where other words are explicitly used, and yet one feels that the word attention (unqualified) is not consistently used in the sense of what is *usually* called active attention. "We may will and may attend actively because we have first been compelled to 'attend' passively. * * * We may end in such cases, and we probably do end, by attending actively to the idea, but we may do this because and only because the idea has laid hold of us passively. Our will to realize this idea in external action and in inward knowledge is but the self-realization of the idea which so has possessed us." An idea sometimes 'unfeelingly forces, we may say, our will and our active attention' (p. 30). Just where the compulsion to 'attend' ceases, and 'our will to realize this idea' begins, is not clear from the author's language.

2. In speaking of conation, attention involves an opposition between existence and idea, but this opposition does not entail effort of any kind; it may cost little more than to anticipate its removal ideally (p. 28). But such opposition is, as such, unpleasant, and the tendency of all such oppositions to pass beyond themselves and become something different is usually consciously present. Whether this constitutes conation or not, surely the relation of pleasure-pain to attention is involved in this discussion.

3. The uses to which the term idea is put raise questions which will be referred to below.

4. The second paper is a discussion of action realizing an idea contrary to will, suggested by articles of Mr. Shand. The latter holds that an idea may realize itself in action and still not be voluntary, as it perhaps should be according to Mr. Bradley. The case of yielding, contrary to will, to a morbid desire for drink, is mentioned. Mr. Bradley says that two ideas cannot be present at once, and that when they alternate, the self is not identified to the same degree with both of them: they are not genuine alternative ideas, in other words. The man drinks contrary to his will, but the drinking is not willed. Very true, but an idea is apparently here realized. No, replies Mr. Bradley, it is not an idea, properly speaking, which is here realized. What then, we asked ourselves, is an idea? To this question we find no complete answer in these papers. On the other hand the case of the drunkard seems to answer to the description on pages 29 and 30 of *Mind*, N. S., No. 41, as a case where an idea is simply 'there,' remains 'there,' and goes on to realize itself by 'unfeelingly' forcing our will and our active attention: it seems like the 'self-realization of the idea which has so possessed us.'

5. The account of the criteria of 'mine' and 'not-mine' (No. 43, pp. 314, 315) represents the evaluating consciousness as less organized than it seems to be in experience. If volition were the self-realization of an idea, simply that and nothing more, the criteria of value, or of 'mine,' ought to be purely logical, as in 1, 3 and 4 above. The author has however added (recently, I suppose) to this definition of will the phrase, 'with which the self is here and now identified'; and this gives the fifth criterion above. Finally the pleasure-pain consciousness is, without being correlated with either the self or the ideas which realize themselves, represented in the second criterion. Possibly three theories of the will, namely, as the self-realization of ideas, as the self-realization of the ego, and as the self-realization of pleasure, are here demanding correlation or suprem-

acy. The author's discussion seems to the present reviewer to prove that the definition of will simply as the self-realization of an idea will not hold unless by idea we understand much more than is usually covered by the term.

I briefly note the following questions which were not answered for me, after being suggested, by the reading of the paper on 'The Definition of Will.'

6. What is here meant by ideo-motor action? The paper seemed to use the term to cover all tendency of ideas to produce motor changes, and yet only the tendency of 'an idea of movement's sensible effects' to do so is usually meant. The difference between ideo-motor action in general and volition in particular is not clearly made out in these papers.

7. The author discusses ideas on page 461 of the paper on 'Will,' but his predications are either very formal and general or negative in quality. Whether the idea involved in volition is an idea of a remote end, or of a movement's sensible effects, or of a possibility, or of present existence as distinct from the future, or of self, or of reality in general, is not stated. We are told that an idea that is subjected to another idea is not, properly speaking, an idea at all (No. 43, p. 309). When the idea of another person as performing a certain action or being in a certain condition is followed by the same action or condition in myself, and when the idea of myself as being in a certain condition is followed by that condition, 'that which has been carried out in act is no more than a partial aspect of my idea, and it, therefore, in the proper sense, is no idea at all' (No. 43, p. 311). An idea, again, is not necessarily an image. The 'meaning' of a perceived object may be 'detached or loosened' from the object, 'but this loosening does not imply always the existence of an image or images, separated from the object and maintaining themselves' (No. 44, p. 461). In a footnote to page 29, No. 41, 'Idea includes any suggestion, even when coming straight from a perception.' In the paper on 'Mental Conflict,' page 311, Mr. Bradley denies that imitation and suggestion are cases of volition, because what is carried out in action is only a partial aspect of an idea, and, therefore, not an idea at all; but in the article on 'Active Attention,' page 29, where he is deliberately characterizing volition as ideo-motor action, imitative and suggested actions seem to me to come within the scope of his characterization. Only, in the latter article, these processes illustrate the case of an idea laying hold of us and getting itself realized willy-nilly, illustrate, that is, the self-realization of ideas and of volition;

while in the former article on 'The Definition of Will,' the idea realized in these processes of imitation and suggestion are not ideas, properly speaking, and cannot, therefore, be regarded as volitions.

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Choice and Nature. EDGAR A. SINGER, Jun. Mind, XI., No. 41, Jan., 1902, pp. 72–91.

My micrometer readings and my liking for port wine and Beethoven sonatas are alike in inaccessibility to you. Are there any judgments in which the subject is the sole arbiter of the truth of his own statements, so that he can say what he will without risk of error? The sophists said that judgments of 'immediate certainty' are of this nature, but the immediate, if certain, is also idiosyncratic. It can neither be contradicted nor confirmed. Moreover, the assumption that the case can never occur again makes it quite indifferent what judgment is passed upon it. $+a = -a$ only when $a = 0$; 'the only absolutely free judgment is the meaningless one.' Truth means a mental grasp of reality capable of confirmation or refutation from an indefinite series of other points of view; and the average of these observations is the only 'fact' of either æsthetics or science, although the variable error is larger in the former than in the latter. Consequently, if empiricism "urges that the answer to every meaningful question must be wrung from experience, and hence must involve a question of fact, I think history forces us to accept the dictum. So that if any class of judgments involves the exercise of a choice, it is because the statement of fact itself depends upon choice." But we are not justified in saying that scientific method excludes all choice on the part of the describer of nature.

"Is there only one, or are there more than one, way in which the scientist may present nature as a uniquely determinate process?" If more than one, is the scientific describer's selection capricious or can we discover a principle by which it must be guided if his description is to be true, the nature it portrays real? A zero probable error is, in finite experience, impossible; hence a probable error and the infinite series of points of view which it summarizes are included in the scientist's meaning when he speaks of a fact. All the disjunctions of ignorance and the 'neglects' practiced by science could be included in the form of this 'probable error.' Some philosophers (James) hold that the psychological factors determining the choice between the disjunctions of science have something to do with 'the result';

while others maintain that science always abstracts from nature (Ward) and gives us only an 'ideal construction' on which it would be unsafe to base our view of the world. The disjunction of ignorance is, however, no ground for the play of choice, but only for the wavering of doubt. From this it follows that the very simplest statement that science can make about nature must take on a hypothetical form. And yet, the account of nature which interests us must be expressed in categorical judgments, and science makes categorical predictions. What has become of the conditional clauses? Science absorbs the conditions in the categorical statement by the simple device of 'standard conditions.'

These conventional 'standard conditions' are arbitrary, are social, aim at representing nature as a thoroughly determinate process, and are of such a nature that no categorical account of nature can be given which does not involve a series of such choices. Nature is indifferent to any particular classification, but not to all classification; for choice is involved in anything we do, or can, mean by nature.

The author illustrates and confirms his thesis by referring to the history of science, to the choices which have been made there, and to the way in which they have made 'nature.' These choices have not been capricious, but according to a principle, and this ground of preference is also the ground of truth. These choices are the only *a priori* factors in experience, and this completes the motives for the doctrine of synthetic judgments *a priori*. Choice is illustrated in the distinction between 'artificial' and 'natural' classifications, in the older conception of 'true orders,' and in the language of such scientists as Linnæus who imply that there are classes *in re*. The genetic classification now reigning in the realms of biology is a choice. And classifications always determine the next questions for science. The next question for biology, moreover, concerns other sciences than biology, involving as it does 'mechanical factors' of evolution which are fundamental in all natural science. It is just such insights into the factors involved in any new classification which tell us what nature *is*. Only in this sense can a classification be 'true to nature,' and only in this sense can there be classes *in* nature. Copernicus' system suggested the question of Kepler, Newton, Huygens, Kant and Laplace, whether motion can be subsumed under growth; and at last nature has come to cover an evolution of mechanical processes.

But facts might lead us to reject any of these laws — the *a posteriori* element in knowledge! In other words, we choose that our descriptions of nature shall be simple. We assume that universal judgments are possible. But why and how are universal judgments possible? Because we have the remaking of facts within our power. In the resistance of facts to our formulæ we simply experience the discrepancy between old choices and present needs; and hence, the search for universal formulæ is bound to succeed. Every one says that the law of gravitation does not express the facts: we have other 'laws' dealing with the exceptions. In other words, we have introduced new classifications which have the conditional flavor of all classifications. Exceptions only invite, they do not force, a rejection of the old classification.

When we wonder at Nature's order and simplicity, it is at our own handiwork. When we find the scheme of things 'sorry' we 'shatter it to bits and remold it nearer to the heart's desire' — only the heart's desire must not be unprincipled. It must abide by the principle of maximum simplicity, economy or unity. Why is this regarded as true? It is in fact a strong intellectual need. Second, it is the principle expressing a universal will. But why listen to intellectual rather than to æsthetic or spiritual needs? The social will is no mere *consensus gentium:* the will we seek must be found *sub specie æternitatis.* We do not mean to stop with any empirical generalization, for our problem is the same as Kant's in the deduction of the 'categories.' Post-Kantian thought has passed from Kant's static to a dynamic attitude toward experience. Not the unity of thought but our thought's struggle after maximum unity constitutes experience what it is. Observations stimulate new interpretations determined by the principle of choice — not the individual's choice, but society's — not that of any particular society, but of all society, to contradict whose will is to destroy the meaning of experience. The question, are not the demands for the goodness and beauty of our world involved in this struggle, is put but not discussed at length.

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Ueber die allgemeinen Beziehungen zwischen Gehirn und Seelenleben. TH. ZIEHEN. Leipzig, Barth, 1902. Pp. 66. Mk. 1.80.

This is the German edition of an address which first appeared in a Holland magazine (Gids). The writer's reason for reproducing it is his conviction 'that in the many addresses by great physicians and naturalists on the same subject, the historical and epistemological standpoint has everywhere been kept too much in the background.'

A brief history of this problem in ancient and medieval thought, interesting for its mention of many unfamiliar names, leads up to Cartesianism and the Associationists. Many attempts were made by the anatomists and physiologists of the seventeenth and eighteenth centuries to find the organ of the soul in the brain. At the beginning of the nineteenth century, Gall and Spurzheim emphasized the view that the soul life is directly related to the brain, and Flourens, by experiments on the brains of living animals, showed that all that is characteristic of spirit disappears with the disappearance of the cerebrum. Foville and Delaye studied pathological cases and began dissection of the brain. Then came the localization theory, finally established by Broca in 1861. The author gives a brief review of the pathological and other evidence for the localization of brain-functions, leading up to the doctrine of psychophysical parallelism. After explaining this principle, the question is asked, 'What relation obtains between material processes and our sensations?'

First, a group of insincere answers to this question: the answer of those who deny the facts of localization and parallelism, that of those who ignore science entirely, and that of those philosophers who, beginning with the absolute, still hope by a play of logic to deduce a world and more. The sincere theories are either dualistic or monistic. Dualism breaks over the question, 'How comes it that out of all the numberless material processes, only those of the cortex are accompanied by psychic processes?' The psychic is known only as the conscious, and those who look upon it as an epiphenomenon, see it as forever mysterious and unintelligible. Another form of dualism goes by the name of psychophysical causation, holding that through the body, material things act on the soul and the soul on material things. This view is opposed to the scientific principles of the conservation and persistence of motion and the conservation of energy. Some have maintained that when the material world acts on the soul, kinetic energy is changed into potential, and vice versa when the soul acts on the material world. But in this case we have monism of the materialistic type, and not dualism.

In his discussion of monistic theories, the author first mentions what he calls pseudo-monism, the doctrine that the mental and the physical are two attributes, or manifestations, or aspects of one reality. Here belong Spinoza, Fichte, Schelling, Spencer, Münsterberg, and others. Similarly, Fechner, the physiological psychology of Ebbinghaus, and others, hold that the two are the inside and the outside of the same thing. But a dualism of attributes or aspects is unintelligible until we ground them in the 'one' reality; and then the one reality ceases to be one, and we have dualism in a worse form than before. Of genuine monisms the author mentions three: (1) Materialism. But thought cannot be scientifically classed as a secretion of the brain. Materialism, moreover, contradicts the epistemologically fundamental fact that we are given in experience only sensations and ideas derived from sensations. (2) Spiritualism regards material processes as functions of the psychic; and 'on the vanity of such a view we need waste no words.' (3) Idealism accepts the opposition of the material and the psychic, but goes on to ask the critical question whether both are given to us in experience as primary. To this idealism answers, no; and with reason; there stands the fundamental fact of epistemology. Kant tried to overthrow the idealism of Berkeley, but failed. He had rightly limited the knowledge of causal relations to phenomena, but himself fell foul of this limit when he said that we know of a cause for phenomena — a thing-in-itself. We are forced to remain in the psychic, and for this reason the idealistic theory has been called the 'immanental philosophy.' Above all, it is necessary to rid ourselves of the prejudice that our sensations are *in* our brains — what Avenarius called the introjection hypothesis. Sensations by no means have a spatial locus in the brain. "The only place of our ideas is yonder in the world." The world of sensation is to-day reduced by science to movements of mass and the ether, and there are indications that the ether may, *in futuro*, be regarded as a sort of mass. But this 'mass' is certainly not a 'matter' which is different from our sensations. It is impossible to form an idea of anything that is not psychic. Impenetrability and extension, the two most general characteristics of matter, are simply very general marks of our sensations, very general notions of sensations themselves.

But, we add, the place of our sensations is no more 'draussen in der Welt' than it is here in the brain-cells. The difference between yonder and here is an experience which the immanental philosophy must account for — and that, perhaps, by turning to social intercourse and to the social consciousness.

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SOCIOLOGICAL.

Games, Sports and Amusements. Walter E. Roth. North Queensland Ethnography Bulletin, No. 4. Brisbane, 1902.

Mr. Roth gives an exceedingly interesting account of the play activities of the aborigines of northern Australia. The different games are classified by him as follows: imaginative, realistic, imitative, discriminative, disputative, propulsive and exultative. The classification is, however, as the author states, merely tentative; the importance of the article lies rather in its first-hand description of these aboriginal plays and games. Many of the activities described as play are only such from a conventional civilized standpoint; from the standpoint of the aborigines they are adult serious occupations, ceremonial observances, etc. That which is a matter of religious creed to them may appear to us as legends and stories. The mimicking of animals in a dance is to them a serious part of the hunt; we, on the other hand, consider it as a part of the general propædeutical function of play.

The social values of their jousts or tournaments, such as the opportunity afforded for wiping off old scores and thus settling old disputes, for the exhibition and development of prowess and courage, for co-operation, etc., are excellently portrayed. The *Rausch* cultivated at their ceremonials, by the chewing of leaves of the 'stinging-tree' (*Laportea* sp.), the excessive obscenity of many of their plays, the naïve animism of their legends, the social functions of the 'corrobborees,' these and many other good points make the perusal of this bulletin very interesting to a psychologist.

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Die Wette. RICHARD M. MEYER. Archiv für Kulturgeschichte, Bd. I., Heft I., 1903, pp. 1–17.

The bet or wager is usually regarded as a sort of struggle, fight or war, and it may without doubt be correctly called such, since it signifies the measuring of strength in a contest for a prize. The wager is, however, distinguished from the other forms of the struggle by five fundamental differentiæ:

I. The wager as a general rule is distinguished from other forms of fight by the fact that no effort or exertion to influence the outcome or result of the bet on the part of the participants is presupposed. This is obviously true of what the author calls determination wagers (*Feststellungswetten*), where the wager concerns a date or an historical name. This does not hold, however, of futurity wagers (*Erwartungswetten*), as *e. g.*, a wager as to whether an engagement will take place between two young people or as to whether such and such a candidate for office will be elected. It is obvious that a special code of honor thus surrounds the wager, one which has grown up since early primitive times. It is almost wholly a matter of sport in modern times, whatever its possible origin in grimmer days.

II. The wager is a struggle with equal stakes, deposits or pledges. The size or amount of the stakes as deposited by each participant may bear a varying ratio to the stakes of the other participant in accordance with the degree of assurance and certainty in his own mind. In chess or checkers a skilled player may assume a handicap voluntarily. The betting at horse racing involves very complicated rules of observances, judges, holders of the stakes, securities, etc. The deposits or stakes may possibly take the place of the war hostages of former times.

III. The first differential characteristic, that of passivity, led clearly to the second, that of equal stakes. The third follows as a natural consequence that the contest is to be decided by a third higher instance. A weather wager, for example, depends on factors other than the contestants. The oath, a vow, is so to speak a one-sided wager.

IV. The wager is in nearly every case a measuring or pitting against each other of *mental abilities*. It may be of memory, of anticipation and prophecy (in the primitive sense of the term, especially), of comparison. Even in its most material form —*jene greulichen Fress- und Saufwetten* — the question is really as to which of the contestants has more correctly estimated his own barrel-like capacity.

V. The wager is a struggle ensuing upon mutual agreement or stipulation. Arbitration takes the place of a free-for-all or catch-as-catch-can fight.

The close connection of the wager with play is discussed, as also some of its early primitive forms.

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EXPERIMENTAL.

Discrimination of Shades of Gray for Different Intervals of Time. FRANK ANGELL. Philosophische Studien, XIX., pp. 1–22.

The immediate purpose of the experimental investigation reported in this article was to discover the effect of a lapse of time between the appearance of the norm and the comparative stimuli on the discrimination of different shades of gray.

The experiments were made with intervals of 5, 15, 30 and 60 seconds elapsing between the appearance of the normal and the comparison, and under three different conditions: (1) "Eyes closed during the interval, whilst an effort was made to hold fast a visual image of the disc. (2) Eyes open with relaxed attention during the time interval." (3) With some distraction introduced during the interval.

Two series of experiments were made, one at Würzburg, the other at Leland Stanford University, the personnel of the observers being changed in the two cases. The results in both series show that correctness in the discrimination of differences is practically independent of the lapse of time between the two stimuli. The percentage of right judgments made with the attention held on the visual image, varies very slightly from that made when the attention was relaxed, as is also the case with those judgments before which some distracting factor was introduced.

Remarks based on the introspection of the observers, from which some explanation of the results is sought, indicate that the visual image of the norm is a comparatively unimportant factor in the judgment, but verbal associations play an important part. From this the writer infers that 'most of the judgments are based on contiguous association, specially on verbal reproduction.' This basis of the formation of the judgment would naturally account for the fact that no marked effect on the correctness of the judgment is caused either by the length of the interval, or the distraction introduced between the appearance of the norm and the comparison. There were also many 'free' judgments, in which no conscious comparison with the norm occurred, but in which the comparative stimulus appeared immediately darker or lighter than any that had yet been seen.

An attempt to secure further data for the explanation of the results was made in the last series by recording the time occupied by the judgment process. This shows, in general, that 'sure' judgments are formed most rapidly, 'like' most slowly, and 'fairly sure' judgments hold an intermediate position.

Concluding, the writer offers three grounds for the explanation of the above and similar results:

1. "From the presence of contiguous reproduction, usually verbal, coming from the formation of a scale of values.

2. "From the presence of free judgments, resulting also from the formation of a scale of values.

3. "From the relatively large number of judgments, 'like' for the shorter intervals, resulting from the maintenance of common conditions during the periods of exposure of norm and comparison."

The work has the marks of resourcefulness and scientific accuracy; and the careful gathering and estimating of remarks based on the introspection of such 'careful and well-trained' observers as Professor

Külpe and the writer himself, make the contribution both reliable and valuable.

The Time of Perception as a Measure of Differences in Intensity. J. McKEEN CATTELL. Philosophische Studien, XIX., pp. 63–69.

Professor Cattell in a very few pages opens up some interesting problems and presents a number of exceedingly suggestive and interesting considerations.

After a brief résumé of criticisms of the generally employed psychophysical measurement methods, advanced in an earlier article (cf. *American Journal of Psychology*, Vol. V., pp. 285–294), he proposes a new method of approaching the problem of measurement of the intensity of sensation, which is based on the measurement of the time occupied in perceiving a difference between two sensation intensities or qualities. The shade of gray which takes as long to discriminate from white as from black should, according to this scheme, be the gray which for consciousness is midway between white and black. The method may also be used for testing differences of sensibility.

The experimental sections of the paper include first, a series of investigations in the discrimination of light intensities, carried out by means of a series of 211 shades of gray papers, using the method of average errors. The results show a decrease in the sensibility of discrimination, with the increase of the intensity of the stimulus, not however such a decrease as would satisfy Weber's Law, but rather a decrease approximately in direct proportion to the increase of the square root of the stimulus. This is in accord with the hypothesis proposed in the above-mentioned article, viz., "The error of observation tends to increase with the square root of the magnitude, the increase being subject to variation whose amount and cause must be determined for each special case."

The second section of the experiments investigates the differences in intensity as measured by the time of perception. The same gray cards were used. The results indicate that with the decrease of the difference between the intensities compared, a longer time is required to make the discrimination. This tendency is approximately uniform with the two observers, but as the writer himself remarks, the number of the experiments is quite inadequate to enable one to draw any very definite conclusions.

F. S. WRINCH.

PRINCETON UNIVERSITY.

Ueber die Beziehungen zwischen Ermüdung, Raumsinn der Haut, und Muskelleistung. THADDEUS L. BOLTON. Psychologische Arbeiten, Bd. IV. (1902), Heft 2, pp. 175–234.

Professor Bolton has undertaken three series of experiments on the relation of mental fatigue to bodily condition; in all three the æsthesiometer was used to test the validity of Griesbach's results; and in the last two the ergograph was also used, following out the work of Mosso and Kemsies.

In the first series of experiments, fatigue was produced by the addition of numbers from 1 to 100 for different periods: ½ hour, 1 hour and 2 hours. The tests with the æsthesiometer were made before and after each period and were made on the skin of the forehead. The results showed many variations in the threshold, but on the whole no definite increase of space threshold could be proved as the fatigue advanced.

The second series of experiments with the æsthesiometer led to similar negative conclusions. The ergograph used by Professor Bolton was constructed according to Kraepelin's plan and differed somewhat from Mosso's. The essential modification consists in a contrivance for holding the weight during the period of relaxation of the finger. This holding of the weight prevents the strain which Mosso's ergograph always imposes on the finger and thus reduces the work which the finger ordinarily does during the period of relaxation. The ergograph tests in the second series showed that in addition to mental fatigue, there were many other factors which deserve great emphasis in working out the results. Such factors are: the influence of practice, familiarity with the instrument, and the temperamental disposition of the subject.

Professor Bolton then investigates more fully the relative influences of fatigue and practice, by comparing the results of the adding for successive quarter-hour periods. He finds that the effects of practice overbalance those of fatigue at first, but that as the experiment is continued the fatigue becomes relatively more important, and the effects of practice diminish in strength. Other minor influences enter in, such as the stimulus which the subject feels when he is nearing the end of the experiment (Schlussantrieb).

The third series of experiments reported in the paper verifies the conclusions reached in the first two series.

Professor Bolton's conclusions are essentially negative, both in regard to the æsthesiometric and ergographic tests, and in this they agree with those of other recent investigators. The experiments were obviously conducted with great accuracy in detail, and conclusions are drawn with due caution. The general problem of the nature of fatigue has certainly not been solved by these tests, but the complexity of fatigue states has been fully demonstrated.

G. B. Lovell.

Yale University.

Ein neuer Fallapparat zur Kontrolle des Chronoscops. Herm. Ebbinghaus. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. 30, Heft 4 (1902), pp. 292–305.

This apparatus for the control of the Hipp chronoscope is a substitute for the fall hammer and utilizes a freely falling ball of 27 mm. diameter and 90 g. weight.

On a solid base of wood are erected two nickel bronze uprights 3 cm. in diameter and 86 cm. in height. These uprights are joined by an adjustable bridge which slides up and down and is held on each side at the same height by set-screws. On each upright is a millimeter scale so that the height of the bridge above the base can be read at a glance. In the center of the bridge is a circular opening. On the left end of the bridge is the contrivance for holding and releasing the ball. This releaser consists of a pair of straight brass strips one of which is on either side of the ball. When in position, the strips are exactly parallel and hold the ball securely just over the circular opening in the bridge. The separating of these arms is accomplished by a hand-lever arranged just behind them near the upright. The ball immediately begins its descent when the lever separates these arms. The two parts of this holder are insulated from one another and from all other parts of the machine. An electric current, accordingly, can pass from one to the other only as long as they hold between them the ball. As soon as they spring apart the circuit is broken. The break comes, therefore, exactly at the instant when the weight is released and begins to fall.

In order to place the ball squarely in the releaser, a small plate is attached to the right end of the bridge. This can be lowered or raised on a spring holder and can be turned into the circular opening just under the releaser. The ball is placed on this plate and is kept in position by a small ring. The arms of the holder are closed without moving the ball. The plate is lowered and withdrawn. The ball is now in position over the circular opening.

Being released, the ball drops and strikes below upon one end of a board which is nicely balanced at its center. The weight of the ball lowers the end on which it strikes and raises the other end. The instant the end is raised a second electric circuit is broken. Thus, without any intermediate interruption, the ball controls the two breaks of the electric current, one at the beginning and one at the end of the fall.

The apparatus was tested for various heights to ascertain whether the times marked off by the two broken contacts agreed with the time theoretically demanded. Plates are given showing comparisons of the apparatus with a standardized fork. The results show that the error is well within a single sigma.

Details are given showing various series of connections which make it possible to utilize two breaks in making records or in testing the Hipp chronoscope.

W. M. STEELE.

YALE UNIVERSITY.

Minor Investigations in Sense Perception. R. MACDOUGALL. Am. Journ. Psychol., Vol. XIII., No. 4.

These are certain minor investigations undertaken in connection with the problem 'concerning the subjective determination of the primary point of regard,' reported in the 'Harvard Psychological Studies' (Monograph Suppl. PSY. REV., No. 17).

The first (I.), 'On Determinations of the Subjective Horizon by Motor Coördination,' is an attempt 'to ascertain the relation of the subjective horizon of the eye as determined by raising the index finger, to its position when determined visually, and the influence upon such location of changes in the orientation of the head and eyes.' The displacement is much less in the case of the downward movement of the eyes and head than with the upward movements. "The upward rotation of the eyes in their sockets develops a relatively intense strain experience, while in rotations of equal magnitude downward from the primary position of the eyes these muscular tensions are practically lacking. This difference arises from the biological relations of the organism to its environment, which call forth constant exploring movements of the eyes within the lower half of the field of vision, while very few are made above the horizon in the expanse of the sky." That is, 'these forms of spatial orientation are related to oculo-motor conditions, and the direction of the characteristic errors which they present are dependent upon the coördination of eye and hand in the perception reactions of ordinary practical life.'

The second (II.) is on 'The Relation of Saturation in Homogeneous Colors to the Area over which the Color is Spread.' "When the whole hand is plunged into warm water, for instance, it feels hotter than when only the tip of the finger is immersed." The same is true of taste solutions. "It is a natural inference from the connection which is found in these instances that the number of elements of the sensitive surface stimulated and the intensity of the resulting sensation stand always in such a relation of functional dependence that the subjective estimation of the intensity of a sensorial stimulus cannot be considered apart from the magnitude of the area excited." "In the case of certain senses it has been noted further that this summation effect is independent of continuity in the surface to which the stimulus is applied; intensive reinforcement takes place when the sensitive elements affected are not contiguous but form a discrete series." Thus in color vision 'there is least difference in saturation between small and large areas of red, of all the colors observed, and most difference in the case of green,' with blue, yellow, violet, orange in order between. "Therefore, the influence of the number of elements stimulated upon the intensity of the color sensation is greatest in the case of green, least in that of red."

The third investigation (III.) is a brief examination of the 'Quantitative Relations of Stimulation Area and Color Threshold in Discrete as Compared with Continuous Extents.'

H. HEATH BAWDEN.

VASSAR COLLEGE.

Ueber Hemmung gleichzeitiger Reizwirkungen. PAUL RANSCHBURG. Zeit. f. Physiologie u. Psychologie d. Sinnesorgane, Band XXX.; Heft 1 u. 2.

This is an experimental contribution to the theory of the conditions of attention. The work was devoted to the determination of the nature and explanations of certain errors that had appeared in some previous investigations upon the correctness of the capacity for comprehension (Sicherheit der Auffassungsfähigkeit). The test was made of the capacity to reproduce correctly all the digits in a six-place number when they were passed before a slit in a card on a revolving drum. An exposure of one third of a second was allowed. The analysis of the results showed that the false reading of a number appeared proportionately more frequent in these numbers where, in the four places at the right hand, particularly in the third to the fifth place, two identical or two similar figures next to one another or separated by one or two figures were found, whereby the identity as well as the similarity of the several elements appeared considerably more important. The remainder of the research turned upon the value of identical and similar elements among a number of successively presented figures. Numbers that contained repeated figures proved more difficult to grasp than those that contained no repetitions. The proposition with which the experimenter sets out is this: "The threshold for comprehension of simultaneous or quickly successive heterogeneous stimuli lies deeper than that for homogeneous stimuli." Series of six-place numbers were constructed upon the basis of repeated and entirely different digits and called respectively homogeneous and heterogeneous series.

The number of errors for the heterogeneous series is about one third that for the homogeneous. For the latter there were 108 errors in a possible 180. The character of the errors is very different for the two classes of stimuli. Inversions of the order of two figures are not only greater proportionately, but really, for the heterogeneous stimuli. The larger proportion of the errors for the homogenous series are either substitutions of one digit for another or blanks from which a digit has been dropped. The errors of whatever character in the homogeneous series usually occupy one place only while they are two-place in the heterogeneous series. Similar digits seem to act in this inhibitory manner much the same as identical or repeated digits. Similarity of digits was allowed to exist when any two digits were frequently interchanged for one another. The inhibitory effect of various similarities was of different degrees, depending upon the degree of similarity. Zero was found to have a higher heterogeneous value than any of the other nine digits. The subjective declarations of the reagents, although they knew nothing of the homogeneous and heterogeneous character of the series, show that they found themselves more often in uncertainty with respect to the series containing identical elements, although uncertainty did not always mean false comprehension. The author holds that all the phenomena become intelligible upon the supposition of the retarding influence of inhibition—'in a short space of time that is just sufficient for the sharp up-building of two psychological processes of a heterogeneous nature, two processes of a similar nature cannot be conceived as autonomous processes separate from one another, as a result of which the analyzing consciousness receives the impression of one process only, the more identical the two processes were.' This supposition 'that simultaneous homogeneous stimuli-effects inhibit one another in their development and lead in the psychological field to the apparent blending of simultaneous homogeneous sensations' is shown to be applicable to every form of sensation process. The paper presents an exceptionally interesting and valuable piece of work along lines that have not received the attention they deserve.

THADDEUS L. BOLTON.

UNIVERSITY OF NEBRASKA.

An Experimental Study of Writing Movements. CHARLES H. JUDD. Philos. Studien, XIX. (Wundt's Festschrift, I.,) pp. 243–259.

This study aims to discover the relation of consciousness to the acquirement of the writing movements, and how this relation changes as the movements become automatic. The apparatus used was a brass spring clasped tightly around the fifth metacarpal bone just behind the little finger, and to which was attached a short aluminum rod carrying at its outer extremity a glass tube held in a vertical position. A writing stylus passed through the tube was allowed to write through the pressure of its own weight. The same hand of the subject wrote with a pen held in the usual way. As the stylus wrote with only the movements of the hand, and the pen with the combined movements of the hand and fingers, a comparison between the two writings shows how much is done with the hand and how much with the fingers.

It was found that the fingers do the work of constructing the letters, while the hand participates only in the forward movements, and the arm acts in the intervals of the words to carry the hand forward. In learning to write one is unconscious of the kind of movements he makes, but gropes about until an easy and effective set of movements is acquired which makes the result conform to the visual pattern. If this pattern is tenaciously held to, the writer may come to produce a perfect copy, but in so doing he sacrifices mental content to form, since attention to form crowds out attention to content. Herein lies the key to individuality in handwriting. If the visual pattern is relinquished early, individual variation is more pronounced, because attention is relieved from its control.

While the study makes an interesting beginning, one regrets that it could not have been carried farther and embodied more results. A real science of chirography is perhaps attainable in this direction.

JOHN P. HYLAN.

CAMBRIDGE, MASS.

ILLUSION.

Zur Lehre von den Urtheilstäuschungen. O. ROSENBACH. Zeitschrift f. Psychol. u. Physiol. der Sinnesorgane, XXIX., 443–448.

The observations upon which the theoretical parts of this paper are based are as follows: Cut out small geometrical patterns from colored paper; and place across these patterns, in such a way as to cover up their middle parts, a strip of wholly untransparent paper. The covering strip should be about 1 cm. in width, and the figures should be long enough to extend beyond both sides of this strip. When the figures covered by the strip are viewed in a not too strong light, the outlines of the covered parts will seem to appear through the covering strip as if it were transparent. This is explained as due to an unconscious judgment. A variety of familiar cases in which percepts are filled in, such as the case of the blind spot, the case of a misprint, etc., are cited as analogous unconscious judgments.

The paper may be discussed with reference to two distinct questions. First, is the apparent transparency of the covering strip explicable on any hypothesis other than that of unconscious judgment? In dealing with this question the author dismisses in a very summary fashion the possible explanations by irradiation and after images. When we recognize, however, that a weak light is one of the chief conditions for the observation, these explanations based upon retinal conditions seem to call for much more careful consideration.

The second question open to discussion is the much-debated question of whether one is justified in using the concept judgment in such a connection. The paper does not help to advance this discussion, for it simply assumes the unconscious judgment and does very little, if anything, to define or defend the assumption.

CHARLES H. JUDD.

YALE UNIVERSITY.

FEELING.

Sur le senil de la vie affective. GASTON RAGEOT. Revue Philosophique, February, 1903, Vol. LV., pp. 153–175.

This article is an attempt to solve the question of the nature of the affective life, on the basis of the author's observation of the development of the emotional life of several children. In the course of the article recent theories such as those of Lehmann and James are subjected to a critical examination.

In opposition to the theory of Lehmann, the author holds that the earliest conscious life of the child is one of pure feeling. This feeling is at first pain. The relief of pain results in feelings of pleasure, which soon become the positive accompaniment of unobstructed motor activity. On the other hand it is held that Lange and James go too far in attempting to connect emotions wholly with the exercise of activities, rather than with the organization of activities.

Throughout the article the author lays emphasis upon the organization of activities both mental and physical as the essential condition of emotional life. It is shown that the same physical expression may accompany widely different forms of affective mental life. The movements accompanying colic, for instance, are exactly similar to those which accompany anger. The manifestations usually expressive of a certain emotion may also become transferred to another through association. These facts show that the emotion is dependent upon the way in which the organization has taken place, rather than upon the purely physical activity accompanying it.

Three stages of emotional life and corresponding development of organization are recognized. The simplest emotions which are the only kind existing for the very young child occur when organic movements, originally reflex, become the expression of a process of organization and disorganization, called forth by one or more motor images.

A second kind of emotion begins to appear in the child after the sixth month. This is of a less violent type than the first. Here the sensory-motor associations of custom and experience begin to realize themselves under the control of familiar images. This is the type of the ordinary emotion.

Finally a stage is reached in which, though the motor images are present, the movements are not actually executed, but remain as mere tendencies to action. Thus, as we ascend the scale of emotional development, it appears more clearly that the actual execution of the movement is not an essential condition of the emotion. On the other hand it is obvious that in all cases the condition of emotions is to be found in the organization which either reinforces or interferes with tendencies towards activities.

YALE UNIVERSITY. E. H. CAMERON.

ATTENTION AND CONSCIOUSNESS.

Fluctuations of Attention and After-images. EDWARD A. PACE. Philos. Studien, XX. (Wundt's Festschrift, II.), pp. 232–245.

We may assume that fluctuations are produced centrally, in which case the conditions in the sense organ do not change, or that changes take place simultaneously in the sense organ and the brain. The present study aims to throw some light on the part the retina of the eye plays in producing the phenomenon.

A semi-transparent porcelain plaque was fixed in the side of a box, between which and a light placed on the inside was a plate of ground glass lined with paper and a sheet of cardboard having a horizontal slit for letting the light through. In front on the outside was another light made adjustable for varying the illumination of the visual field. A movable screen held in position by an electro-magnet was used for cutting off the light from within the box when desired. The subject fixated the band of light, and as it disappeared allowed the screen to fall and cut off the stimulus. In this case the after-image appears, but if the fluctuation takes its course until the moment of reappearance before the screen is dropped, no after-image, or only a barely perceptible one, is seen. There thus appears to be considerable fatigue present in the retina at the time of disappearance but practically none at the reappearance. From this it appears that from the point of appearance to that of disappearance there is a phase of increasing fatigue, and from the point of disappearance to that of appearance is one of decreasing fatigue.

In spite of the several studies which have been published upon this subject, the development is slow and much the same ground has continually to be plowed over for a preliminary discussion. A fundamental advantage would be gained by having a basis of distinction between the fluctuations peripherally and those centrally originated. Such a one may be found in the fact that when the peripherally conditioned disappearance has taken place, the mental image may yet be retained. Although the direction of attention may be slightly different when fixed on an external object than when on the remembered image of it, there is yet a sufficient similarity between the two states to prevent there being a fluctuation of the attention properly speaking. What, therefore, has most commonly been called a fluctuation of attention is essentially a fluctuation in the functioning of the sense organ, or possibly in the sensory tract before the ideational centers are reached.

J. P. Hylan.

Cambridge, Mass.

Zur Theorie des Bewusstseinumfanges und seiner Messung. Wilhelm Wirth. Philos. Studien, XX. (Wundt's Festschrift, II.), pp. 487–669.

Of this contribution one hundred and forty-eight pages are devoted to theoretical discussion, nineteen to the description of new apparatus, and eleven to experimental results. There have been several studies of late either directly or bordering upon the question of the boundaries of consciousness, and the present discussion aims to clear the ground for more intelligent work.

The starting point is the well-known experiments of Wundt with the tachistoscope, and that in which two consecutive series of metronome strokes are compared to find how long these series can be and yet have their relative lengths rightly judged. The question arises as to why in the former experiment only from four to six objects can be perceived from an instantaneous exposure, while in the latter from sixteen to forty may be correctly compared. A cue is found in Cattell's work, where it was observed that in addition to the four or five simple objects that could be clearly seen, there was an estimation of the whole number, but characterized by a large average error and a tendency to underestimate. The greater distinctness and smaller number of objects as compared with the auditory experiment is the result of greater practice in seeing these particular forms. Also with the series of metronome strokes there is a tendency towards a rhythmical grouping which corresponds to the grouping of letters into words with the visual impressions and which make it possible to see a much larger number of letters. In both experiments no judgment is possible until the sensory impressions are passed, and in both two images are compared; in the one case the two series of strokes, and in the other the memory of the visual impression with the expressed judgment. If an arrangement of the tachistoscopic experiment could be made to conform more closely with the conditions of the auditory experiment, more similar results could be expected.

To this end a new form of tachistoscope was constructed, with which it was possible to present to the subject two groups of simple figures for the purpose of comparison, one quickly following the other. In the main the two groups were alike, but with one or more changes of the figures in the second. It was found that changes could be indicated correctly within a limited region of the point of fixation. When twenty-five figures made up the group, not more than one change in the outer edge would be noted if the attention were not directed there. Whenever there were several changes, only one would be noted, except when they were close together.

The author is well informed and the discussion well arranged. A condensation of the theoretical part would greatly increase its value, although an 'exhaustive' discussion, as with Kant, may derive much of its value by exhausting the reader to the point of consistently avoiding the past of a problem in favor of its future. The apparatus described is ingenious and somewhat elaborate. We trust that a more extensive contribution to experimental results will come from it in the near future. J. P. HYLAN.

CAMBRIDGE, MASS.

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Le Mensonge. G. L. DUPRAT. Paris, Alcan. 1903. Pp. 190. 2.50 fr.

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Zur Grundlegung der Psychologie des Urteils. E. SCHRADER. Leipzig, Barth. 1903. Pp. 98. Mk. 3.

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Spelling in the Elementary School. O. P. CORNMAN. Exper. Studies, ed. by L. Witmer, No. 1. Boston, Ginn. 1902. Pp. 98.

The Sensation of Pain and the Theory of the Specific Sense Energies. ANNA J. MACKEAG. Same Series, No. 2. Boston, Ginn. 1902.

Psychological Norms in Men and Women. HELEN B. THOMPSON. Univ. of Chicago Cont. to Philos., iv, i. Chicago, Univ. Press. 1903. Pp. 188.

Certain Aspects of Educational Progress. Various Authors. Invest. Dept. of Psych. and Ed., Univ. of Colorado. 1903. Pp. 84.

Outlines of Psychology. JOSIAH ROYCE. New York and London, Macmillans. 1903. Pp. xxvii + 392.

L'Ennui. E. TARDIEU. Paris, Alcan. 1903. Pp. 297. 5 fr.

Introduction to Philosophy. W. T. MARVIN. New York and London, Columbia Univ. Press, Macmillans. 1903. Pp. xiv + 572.

NOTES.

We note the appointments: Dr. B. Bosanquet and Dr. G. F. Stout to the two philosophical chairs vacant in the University of St. Andrew's; Dr. Montague of the University of California, to an Instructorship in Philosophy in Columbia University; Dr. Wrinch of Princeton to an Instructorship in Psychology in the University of California. Professor William Caldwell, of Northwestern University, and Mr. A. E. Taylor, of Owens College, Manchester, respectively to the moral and mental philosophy chairs in McGill University, Montreal; and Mr. Carveth Reid to the Grote Professorship of Philosophy of Mind and Logic in University College, London.

Professor James R. Angell is lecturing on psychology in the summer session of the University of California.

The prospectus has been issued for a new Journal of Psychology to be issued by a board of editors include Professor James Ward and Dr. Rivers of Cambridge University. The journal is to publish researches and original papers and will appear at irregular intervals. Further announcement will be made later on.

Another new serial publication, also to appear irregularly, is *Beiträge zur Psychologie der Aussage*, of which the first number has reached us. It is issued from the press of Barth of Leipzig and is edited by Dr. L. William Stern with the coöperation of a weighty committee. (Price 4 Marks per number.)

VOL. X. No. 5 September, 1903

THE PSYCHOLOGICAL REVIEW.

THE CASE OF JOHN KINSEL. I.

BY GEORGE B. CUTTEN, M.A., PH.D.

This case is here presented in the hope that it will be an addition of some value to the small number of cases of like nature which have been already published. For obvious reasons, the principal in the case wishes to have his identity unknown, and hence all possible precautions are taken to that end. To assist in this purpose, the name used here is fictitious, and even those to whom the writer is indebted for help in the preparation of this paper are unnamed, in order that the object may not be defeated. The appropriate portion of this article is to be used in the writer's 'Psychology of Alcoholism,' shortly to be published, and there due acknowledgment will be made in connection with the names of many others to whom he is indebted. The writer wishes, however, to acknowledge here his obligations to the subject, Mr. Kinsel, who recognizing the scientific value of the case, has given his consent to the publication of this article, has furnished all available data, and made some valuable suggestions, assisting in every way possible.

PART I.

The writer wishes to insert another foreword. The presentation of this part is purely descriptive, and carries with it no theory whatever. It is necessary to use certain terms in order to be understood, but these terms are used simply to aid in the description, and do not carry with them any theoretical implications. For example the term 'double personality' does not imply any theory, not even the theory of a double personality, but is simply descriptive of a state or states, concerning which this is the common term.

John Kinsel was born in one of the most beautiful and healthful country districts in New England. His parents are kind, hospitable and intelligent people, highly respected in the community in which they dwell, and living as would be expected of the better class, well-to-do farmer, residing some distance from the railway or any town. On both sides of the family the diathesis is unfavorable for a sound nervous and mental life, showing insanity, alcoholism and other tendencies to nervous degeneracy. The father is a large land owner, possessing over five hundred acres, not all of which is under cultivation. This has and still does entail considerable responsibility and labor on his part, and yet to-day at sixty-six years of age, he is in good health, active, hardworking, capable of doing his full proportion of work. He was able to give his son a common school education, a high school training, and to assist him in his college course. Like the New England farmer of years ago, he makes every fall from twelve to fifteen barrels of cider, which before spring gets strong and intoxicating. Of this he drinks eight or ten glasses every day, but was probably never intoxicated in his life, and would consider a man weak indeed who would become intoxicated on cider however strong it might be. Beside the regular beverage of cider, the old custom of a jug of brandy for haying time was rigidly adhered to, and frequent indulgence was the rule. While all the male members of the Kinsel family drink, only two carry it to excess, viz., John's uncle and cousin.

His father's sister was insane and died in a retreat. In her youth she was morbidly sensitive, but not until the age of thirty-three did she have the first outbreak of insanity. She at first refused to see anyone, and would do nothing else but read her Bible. From this she recovered without being sent to a retreat, but five years later when she suffered from a second attack, she was admitted to an insane hospital for treatment. The diagnosis was mania, and she was discharged as recovered after six months' residence there. The third outbreak occurred six years after the second, when she was again sent to the hospital for the insane; but between these two attacks there is a history of irritability extending over several years. Upon her admission at this time she was noisy, violent and excited, the form of her trouble again being diagnosed as mania. She remained in this condition about two years, difficult to manage and taking offense at trifling things. She gradually became demented, more quiet and less frequently violent, but retained her irritability which took the form of scolding. She died at the hospital, of typhoid fever, at the age of fifty-nine, without recovering her reason.[1]

John's paternal grandfather drank moderately as do all the Kinsels, but otherwise, as far as can be ascertained, was normal. His wife (John's grandmother) died in a 'fit,' her mind was slightly affected, having had a 'shock' when seventy years of age. One of John's paternal great-grandmothers, his father's mother's mother, died insane; but no particulars concerning her case could be ascertained. In tracing the Kinsel side of the family, we find insanity in two different generations, alcoholism in all the male members, and paralysis.

Turning now to the maternal side af the family we find the record quite as unfavorable. John's mother is neurotic and far from strong. When warm there are noticed urticarious blotches on her throat, probably of nervous origin. Apart from her general nervousness there appears to be no specific trouble. One of John's maternal cousins, his mother's sister's daughter, a young lady of about his age, became quite unsettled mentally when twenty-five years old. She became nervous, ugly, hypochondriacal and pessimistic. She had a special antipathy to her mother, and scolded considerably. She finally refused to work, and resigned a good position as teacher. After three years she completely recovered, and accepted another position as teacher. Mrs. Kinsel's father, John's grandfather, drank heavily all his life, and died of paralysis at the age of seventy-two; but none of his children (John's uncles, aunts or mother) drank at all. John's maternal grandmother was operated on for cataracts of the eyes, after her eightieth year. One great-grandparent, his

[1] The writer is indebted to the superintendent of the hospital for the account of this case. The early records of the hospital being incomplete, they have no history of heredity in her case. The fragmentary history of heredity here presented has been obtained from other sources.

mother's father's father, drank heavily as did all of his children; and one other, his mother's mother's mother, died insane, but there have been no particulars of her case gathered. So, here on the mother's side we find insanity in two generations, alcoholism direct for several generations, excepting the mother, and one death by paralysis. The case of cataract is also interesting, as we find John suffering from the same trouble. We have here on both sides a characteristic epileptic family history. We give it below in outline.

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                                   —Grandmother—Great-grandmother
Cousin ————                          cataracts.      insane.
melancholic.  —Mother ————
                 nervous.
                                   —Grandfather—Great-grandfather
                                     alcoholic,      alcoholic.
                                     paralysis.
John Kinsel.—

                                   —Grandmother—Great-grandmother
                                     weak mind,      insane.
                                     paralysis.
              —Father ————
                alcoholic.
                    |              —Grandfather
                  Aunt               alcoholic.
                 insane.
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Mr. and Mrs. Kinsel were married at the ages of twenty-eight and twenty-four respectively. A girl was first born to them, but she lived only about twenty-four hours. There have been no other children except John, who was born in 1873, nine years after their marriage. His birth was normal, no instruments being used. He was a nervous child, but healthy and happy, seldom crying. When four years of age, he had a very severe attack of dysentery. He went from one convulsion into another for over twelve hours, and for three weeks afterwards was dangerously ill. The same year while out riding with an old lady, the horse became unmanageable, and running up the side of a steep bank, threw out both occupants of the carriage. The lady fell upon John, the latter striking his head against the edge of a small wooden box. He was unconscious when first picked up, but recovered before long, and immediately inquired for the old lady whom he feared had been killed; he said that he was not hurt except a little on his forehead. Upon examination it was found that the box had come in contact with his forehead, about a half inch above his eye; the physician who was called said it had made a 'dent' in his forehead. The skin was not broken, but his forehead became much swollen, and all that side of his head, black. He speedily recovered, and apparently with no serious consequences. We might find a traumatic origin for his epileptiform condition here if it were necessary, but his family history would make this superfluous; for not only are we able to charge it to heredity, but given such a heredity we would look for it in his life.

John's life on the farm cannot well be differentiated from that of other children in like circumstances; he assisted about the work and attended the district school. From his earliest recollection he has been nervous. He stuttered badly from the age of four until he was twelve, but gradually outgrew it, so that it was scarcely noticeable when he was in high school. But even yet, if in the company of those who stutter or stammer, it is impossible for him to talk normally. While in college, during a recitation, the professor in charge called upon three students in succession to recite, all of whom stuttered; he then called upon John, who upon trying to recite stuttered so as to be unable to respond, and not until after class when the professor received an explanation of his former habit and present nervousness, was John clear of the censure of both professor and class on the ground of mocking the others. Even now when he talks about his childhood stuttering, he is unable to proceed without perceptible trouble.

During childhood and youth he had very vivid dreams, which continue to the present time. Very early he became somnambulistic; on one occasion when about nine years of age, after having gone to sleep in his bed at home, he awoke to find himself out in the fields. His little dog had followed him, biting at his heels. When he was able to orientate, he discovered that he was a half mile from his home, it being about two o'clock in the morning; he having traversed this distance without accident, and on the journey crossing some very difficult places. He returned to his home and bed. He has always required an excessive amount of water to drink, quite frequently exceeding one gallon in twenty-four hours; he is correspondingly troubled with polyuria. As a child his father tried to limit him in his drink, and later he himself endeavored to lessen the quantity, but without success. He has since required less, but at the present time drinks more than normal.

All his life, up to the time of the operation upon his eyes, he has been subject to violent headaches, continuing sometimes for days. For a day the headache may not be very severe, then becomes almost distracting. It started over his eyes, then worked back until it apparently crowded the whole head; then sometimes he would become 'light-headed,' and would be finally relieved by an attack of vomiting. These headaches were more frequent and more violent during his high school course than while he was in college; and since the operations for cataracts have taken place, the headaches are much less frequent and severe.

It was decided that John should study for the ministry, and to this end he entered high school to prepare for college. This decision was probably reached, not because of any special religious fitness on John's part, or because of what the older theologians would designate as a call, but for the same reason that so many choose one occupation rather than another, viz., the way was opened more towards the ministry than any other profession, and both the parents and John were ambitious concerning his future, desiring him to enter some profession. Further, the farm life was very distasteful to him, and notwithstanding that he is the only child and must come into possession of this large farm, he still dislikes the occupation of farming.

He entered the high school, and considering the disadvantage of poor eyesight did good work. On account of cataracts, he had not more than two sevenths normal vision in his best eye. The irritation must have been great, nevertheless his health seems to have been good, except for the headaches, and at this time he overcame his stuttering habit. At the age of twenty he entered college, having as a room-mate one of his classmates, who faithfully served him and loyally remained with and assisted him all through his course, even at the expense of a high stand, for which he was ambitious and capable of attainment. It was largely through the help and self-sacrifice of this room-mate that John was able to finish his college course; not that John did not work and maintain a fair stand, but his sickness and poor sight were great handicaps to him.

During the freshman year John continued his work like all the other members of the class. He indulged in athletics to some extent, trying for his freshman crew, but did not succeed in making it. He was ill twice during this year; first, from an attack of mumps which was sufficiently severe to affect one testicle seriously for a time, but from this he recovered. After this he had quite a severe attack of la grippe, but after a short illness his recovery was rapid, with no serious consequences. There appears to have been no abnormal mental or nervous trouble during this year, with possibly two exceptions. There were strange swellings of the hands at night, accompanied by neither pain nor inflammation nor disability of any kind. No remedy was used but the swelling went down spontaneously and quickly. This may have been of nervous origin. The other possible exception was the great tendency to sleep, often much of the day being spent in this way. He could sleep any time and anywhere, but he attributed this state to his tired eyes. The summer vacation following the freshman year was spent at his home on the farm, assisting in the haying, and in the autumn he returned to college to take up the work of the sophomore year.

It was in the sophomore year that the somnambulistic states began to show positively. There were four stages in his abnormality, viz., first, asleep, with eyes closed, lying down; second, asleep, with eyes closed, sitting up; third, asleep, with eyes closed, walking around; fourth, asleep, with eyes open, walking around and carrying on the ordinary duties of life. The first of these periods corresponds in time to the sophomore year; the second to the greater part of the junior year; the third to the last part of the junior year and the first half of the senior year, and the fourth to the latter half of the senior year, and for nearly a year following graduation. The term 'sleep' will be used to designate the abnormal state in all its forms, for it was thus named by John and his friends on account of the way in which the state originated, but it is hardly appropriate for the last stage of the abnormality.

At the beginning of these states in the sophomore year, John was sleeping considerably in the daytime as he had done during the freshman year. His class-mates who came into his room noticed that he was suggestible at these times but this was discovered quite by accident. Frequently he would talk when he seemed asleep and when a bunch of keys was shaken near him or a similar noise was made he would start to sing quite lustily, 'Jingle bells, jingle bells, jingle all the day,' etc. The fellows began talking to him and he would answer quite brightly, showing a keener display of wit than when he was normal. Of course his friends did not realize the seriousness of his trouble and it was considered a great joke, furnishing amusement for all his associates. Later another accidental discovery of suggestiblity occurred; some of his friends came into the room and found him asleep; they began singing, in a way common among students, a college song concerning the initiation of freshmen into secret societies. The chorus is rendered loudly and with great spirit, and when this was sung John would arise and beat about the room in a very amusing pretence of initiating the freshmen. It became quite common for those who knew about it to sing this when John was asleep in order to enjoy the sight of the initiation which frequently changed according to the circumstances. It was also noticed at this time that in some ways he was very bright if he talked while 'asleep,' but that when he awoke he failed to remember anything that was said or done while he lay on the couch with his eyes closed, but from his actions and words apparently awake. Most of his companions refused to believe that he was not awake, nor would they accept the statement that he could not remember what had happened, but thought that this was carrying the joke further, such as frequently happens among college boys.

In April of the sophomore year five students including John went out for a sail one Saturday afternoon, intending to return within a few hours. They got becalmed outside the harbor, and when a breeze did spring up they were driven to the opposite shore. It was so dark that they could not see to land, so they anchored as soon as they heard the breakers. Cold and hungry, they remained in this position all night, and the following morning after landing to obtain something to eat, returned home, reaching the college town Sunday afternoon. Their friends, being much frightened, had given them up for lost, and were about to charter a tug-boat to search for the remains. On the sail home, just before entering the harbor, John lay in the bow and went to 'sleep.' He then began to compose and recite doggerel rhyme as fast as he could talk, greatly to the amusement of the others. He described the different incidents of the trip in his rhyme, and soon afterwards awakened. When next he went to 'sleep' they suggested the trip, to which he responded by reciting this rhyme and adding the incidents which occurred after his awaking on the boat. This became very popular among his friends, and seldom was he found 'asleep' for some time after this without being asked to recite the doggerel. It was found impossible to obtain a copy of the poem as it was first spoken, but through the memory of one of the party, assisted by a process to be explained later, nearly the whole poem was reproduced. It exhibited a quick and spontaneous power of rhyming, together with a change of character quite characteristic of the second state. We see here a young man studying for the ministry, of generally good conversation in his normal state, when abnormal producing low, vulgar rhymes. We present the first four lines to show the general style:

"H—b— A—g— had a scheme, a wild, fantastic, fevered dream;
He thought if westward he should sail, before a strong, propitious gale,
That he would find a wondrous land, where gold lay sparkling in the sand;
Green bank-notes grew on all the trees, and rustled there in every breeze."

During the sophomore year his friends discovered that he could be awakened by running their fingers over his face, but this did not always suffice to keep him awake. At one time his room-mate kept account of the number of times he would awake and go to 'sleep' again during a certain time. The exact results have been forgotten, but it was oftener than once every minute for several minutes in succession. In this year not only were his dreams very vivid, but he began to have serial dreams in his normal sleep. He would dream of some person or thing, and the next night, or for several nights following, begin and continue the dream where he had concluded it on the preceding night. The most important serial was that of a young lady whom he met in his dreams for several years. She was usually playing the piano and her name was Edith. John told of his dreams to his friends, and thus the name Edith became a common one in conversation and joke. During the latter part of this year, while lying on a couch with his eyes closed, he would talk, joke, smoke and move around on the couch as a waking person, but remember nothing of it when he awoke.

The summer following sophomore year was spent home on the farm assisting in the various duties about the farm, especially in the gathering of the hay. He returned to college in the fall encouraged concerning the prospective year's work, on account of the almost total absence, during the summer, of somnambulism so characteristic of the latter part of the sophomore year. The expectations were not realized, for no sooner did the study and the regular term's work begin than the somnambulism again became prominent. He did not sleep so well at night, and began to walk around the college grounds in his sleep. His room-mate would sometimes miss him and go in search of him. He was found at one of the other dormitories at times, where some of his friends roomed. There he rapped on the windows where he could reach, or threw small stones to the windows of those who roomed in the upper stories. They were awakened and took it good-naturedly at first, thinking it was done for a joke, but finally began to resent the frequent repetition of it. He would generally be reported to his roommate, who would go after him and bring him home, noticing that when he was walking along or going up stairs he never stumbled. For fear that some mischief would befall him, not from his inability to take care of himself in these sleep-walking experiences, but from some pranks of his friends who did not comprehend his real condition, the plan was conceived of his room-mate's locking the door at night and retaining possession of the key. This was done, and, as far as the room-mate knew, never once did he try to get out; but after two months' success they got lax, the key was left in the door and the sleep-walking again started.

In the sophomore year the abnormal states began when he was lying on a couch with his eyes closed, but at times this was succeeded by his sitting up with his eyes closed and taking an active part in some of the affairs about the room. This began about the time of his returning to college in the fall, the beginning of his junior year. At this same time it was found that rubbing the fingers over his face did not suffice to awaken him, and a new expedient was resorted to, viz., spanking him on the buttocks with a book or some flat and heavy article. This was very successful for a time, and not infrequently John would request his friends to awaken him thus. Sometimes the very threat or posture of spanking him was sufficient to awaken him, and the suggestion that his friends were spanking him was frequently taken and resulted in his awakening. This method was finally ineffectual, and at one time a class-mate spanked him at intervals all the afternoon with a large Latin lexicon, but notwithstanding the physical pain and his shouting and crying, he remained asleep the whole day.

He continued to be very suggestible and dreamed a great deal. During the year while sleeping near a radiator, his friends sprinkled water on his face to try to awaken him. When he awoke he told them of a dream he had had. He dreamed that he was at a fire and was very warm (suggestion from the heat of the radiator) and that the firemen had turned the hose on him (suggestion from the water sprinkled on his face).

In the 'sleeping' state he seemed at times to be much brighter, wittier, and in many respects more intellectual. His friends delighted to find him 'asleep' when they went to his room, on account of the fun in his retorts and conversation. Sometimes he would start up with an exclamation and his friends would carry on a conversation with him from this beginning. One day while sleeping on the couch, with his friends conversing in the room, he suddenly started up saying, "H—l—'s dead and gone to hell." Someone replied, "Tell us about it." He then began and gave a description: "Prexy D——— preached the funeral sermon. He took his text from the second chapter of Colliseums—He hath grinned what he could." This was very appropriate, for H—l—'s smile was a standing joke among the boys. John then composed and repeated some doggerel as fast as he could talk, commencing: 'H—l—'s dead and gone to hell.'

During this year he showed a wonderful exaltation of memory. The best example is that of remembering several lines of Greek prose while studying with some class-mates. John was reading while the others were finding the unfamiliar words in the lexicon. In the midst of the preparation John went to 'sleep.' When the time came for him to read again, with but a glance at the book, he turned away so that he could no longer see the book, and then repeated six lines of the Greek as though he were reading it, a feat entirely beyond his ability in his normal state; in fact, to but glance at a book and then repeat six lines of Greek prose, would be an accomplishment out of the range of almost any one, especially a student none too familiar with Greek. Some of the classmates attributed this to some telepathic or clairvoyant power, but it was noticed that when he went beyond the sixth line, he still continued with Greek, not according to the text, but he repeated a combination of Greek words that he remembered, the words being put together regardless of the sense. This shows that memory accounted for all, not only that he had no clairvoyant power, but because he did not go beyond the sixth line, that he was not reading. This trait of substituting other words and composition for the original, when memory failed, will be spoken of again when we come to deal with his experience in the hypnotic condition.

During the last part of the junior year the third stage of the somnambulism developed. Before this he had gone to 'sleep' on the couch, with eyes shut, responded to suggestions and talked considerably; next he had sat up with eyes open and participated in certain actions about the room; but now he commenced to wander about the college grounds, with eyes shut, yet without receiving any harm. Probably, as frequently happens in the hypnotic and other somnambulistic states, the eyes were not entirely closed and admitted of some vision. Near the latter part of this year (May, 1896) he had a slight attack of jaundice. He went home for a few days and upon recovery returned to college. During this time there appeared a bloody sweat upon his forehead, caused evidently by the strain incident to a violent attack of vomiting.

In the spring of the junior year there developed some epileptic attacks. The seizures were only slight, but some were sufficient to throw him to the ground, and during some of them he was unconscious. The first one known of happened after he had come down stairs from his room. The last that he remembered was arriving at the foot of the stairs. When he came to himself, he was in the closet, the place for which he started. He did not know how he got there or what had transpired since he left the foot of the stairs, but from the fact that his lip was cut, his face bruised, and his clothes dirty, he concluded that he had fallen down. When he came to himself he was still dazed and felt peculiar. Shortly after this at the advice of the college physician he went to his home for a few days. Here while harrowing he fell to the ground in an unconscious condition. The third was seen by his room-mate. They were in the room together, John being in his normal condition. He felt the attack coming on and moved over to the couch on which he fell. Immediately there appeared tonic contractions of the extensor muscles, his head was thrown back, his eyes rolled up, his legs, arms and fingers rigidly extended so that it was impossible to bend them. There was no foaming, no blood, no clonic contractions, and he did not hurt himself in the least. His room-mate put water on his forehead and he returned to his normal condition, not having been totally unconscious. There were some other attacks of which we have no definite account, but probably all less severe than these three, and not numbering more than ten or twelve all told. When alone at one time he thought that he felt one coming on, and by resisting it, he considered that he had prevented it. It will be noticed that these came invariably when he was in his normal condition. Besides this 'grand mal,' there were numerous attacks of 'petit mal.'

The summer vacation following the junior year was spent on the farm, as previous summers had been. During the three months, he was 'asleep' not more than two or three times, and then not for very long. In the fall he returned to college to complete the last year's work. It was during this year that the somnambulism became most aggravated, and showed itself in the most vivid and interesting form. Immediately on his return, the trouble came on again very much as it had been at the end of the junior year. He walked about with his eyes closed, presenting a rather ghostly appearance, yet making his way about without any harm to himself or others. At this time there were some feats performed which some of his friends thought almost superhuman. On one occasion he was lying on the couch in the corner of the room, with his eyes closed while two of his class-mates were playing checkers. The table on which the checker-board was placed was some distance from him, and at least two feet higher than his head, so that normally it would be impossible for him to see the board. He did not appear to be paying any attention to the play, when suddenly he cried, "You can jump two there!" Both those playing and those looking on laughed, thinking that he intended it for a joke; but he got up, went over and showed them where the two men could be jumped, a move which none of those around the board had noticed. Only in one way can this be explained normally. Of course we know by experiment with hypnotic subjects, that when the eyes are apparently closed, there is an opening sufficiently large through which to see; and the fact that John's eyes were apparently closed does not mean that with the hyperæsthesia so common in cases of this kind, he could not see. But according to the normal laws of vision, it was impossible for him to see when the board was at least two feet above his head. John was an excellent checker player and usually took a lively interest in the contest; it is barely possible that being very familiar with the game, he had watched the hands and arms of the players without being able to see the board itself, and thus kept the game in mind before him. No other explanation occurs to the writer without resorting to clairvoyance, for telepathy it was not, as no one knew of the move except John.

On another occasion John played a game of chess with a classmate when his eyes were closed, and in addition to this he was blind-folded. He played through correctly and won the game. Any attempt on the part of his opponent to move out of turn, or to remove a man from the board was immediately detected. He did not feel all over the board, but put his hand on the man which he wished to move and put it in its appropriate place. One thing that would assist him in detecting moves out of turn was his excellent hearing. This was more acute on account of his poor sight, a feature that we see illustrated in the case of the totally blind. At another time he was tested by his friends after the game. His eyes were closed and different chess-men were placed in various places on the board, some of them being behind a tobacco box. He made mistakes in naming the men until he touched one of them, when he appeared to see the whole board, and rapidly told where the different pieces stood. Another feat frequently performed was the recognition of persons introduced by the wrong name, when his eyes were closed. When they spoke, his acute hearing would account for this, but the hyperæsthesia and slight opening of the eyes would be sufficient explanation of the mystery.

In November of this year he went to the room of a class-mate to be tutored in German. He would not study at first, but finally sat down. The class-mate began to read and continued until he noticed John's book drop from his hand and on looking up discovered his eyes closed. When the reading stopped, immediately John said, "Go on, damn it." The reading continued and as a test, words were omitted or put into improper connections. Whenever this was done John objected and demanded a proper reading. Not long after this his friends asked him to recite, as he frequently did for them when 'asleep.' He chose a reading quite popular among the boys, entitled 'How Ruby played.' This purported to be an account of a rustic who went to hear Rubenstein, the great pianist, play, and is written in an appropriate style. It is an endeavor to show the effect of music upon the emotions. It ends very boisterously as follows:

> **"—P-r-r-r-rlank! Bang! ! ! lang!**
> **perlang! p-r-r-r-r-r! ! Bang! ! !"**

At the last word he jumped off the floor as high as he could, and when he alighted he awoke, looked around at the laughing fellows, and was utterly at a loss to know what had taken place. The last thing that he remembered was being down stairs about an hour previously. Instead of reciting he would sometimes entertain his friends by imitating the professors in lecturing, bringing in their idiosyncracies with considerable skill.

Before January of his senior year there developed the fourth and last stage of his somnambulism, that of going about in a secondary state with his eyes open, what Binet[1] calls 'vigilambulism.' At this time he was very boisterous, and his first appearance to his room-mate with open eyes was quite exciting. John entered his room to find his room-mate there alone. He was 'asleep,' and had evidently been drinking; but told his room-mate that he was going to call on his cousin who was in town for the day. His friend being afraid that he would disgrace himself in his partially intoxicated condition, tried to dissuade him. This he was unable to do, so stood against the door and prevented his exit. John was apparently very angry, threatening all manner of injury, but finding this of no avail, he grasped a large bottle used for holding spring water, and lifting it in the air he started for the door, threatening his room-mate with assault. As he came toward the door with upraised arm and bottle, his room-mate saw his eyes open for the first time when he was 'asleep,' and he looked very wild. The room-mate did not stir, and John gave up the proposed visit.

Another class-mate with whom he was quite intimate, describes the first time that he saw him with open eyes, which was a few days later. While 'asleep,' John had gotten into an altercation with a class-mate across the hall, who had little patience with him. John's room-mate succeeded in getting him into his own room, but John wanted to get out and continue the quarrel, threatening great bodily harm to his opponent. The room-mate, as before, placed his back against the door to prevent his going. John took down from the wall an old revolutionary sword, which was one of the decorations of the room, and ran at his room-mate with the sword, jabbing first on one side, then on the other, the class-mate being almost paralyzed with fear for the safety of the room-mate. John seemed to know that he

[1] 'Alterations of Personality,' p. 3.

was acting a part, so was careful not to strike anyone. Finally the room-mate grasped hold of John, they clinched and fell with the room-mate on top. This seemed to take all the spirit out of John, and he arose whimpering like a child, and saying, "Where is my cigar?" he had lost this in the struggle. He had his eyes open at the time and looked vicious. The excitement seemed to cause his eyes to open in both of these cases, and thereafter they remained open when he was 'asleep.'

This last stage was the fully developed 'double personality,' and now let us give as full a description as possible of this secondary state. In the secondary state John remembered all of his past life, but when he returned to the primary state he could not remember anything that had taken place during the secondary state. His memory of the details of his primary state was frequently more acute in the secondary state than in the primary state. There is only one incident of which we know which shows any memory of the secondary in the primary state; it is as follows: when in the secondary state at one time, John was boisterous, partly on account of his natural temperament when in this condition, and partly through the influence of some alcoholic beverage. He became so violent that he was arrested and taken to the station-house. As soon as his friends heard of his predicament they applied to the authorities, who upon hearing of his infirmity, very kindly and courteously ordered his immediate release. His room-mate took him home and he went to bed and to sleep. He awakened in his normal state, and began to relate a dream which he had — the dream was an exact account of his arrest and the subsequent events. He asked his room-mate if it were so and he was quickly assured that it was not. This is the only trace of any memory of the secondary state by the primary.

Apart from the memory, there is frequently a great difference between the two states, which is shown not a little in the character. Naturally, John is good-natured, kind-hearted, generous, sympathetic, — in short a good fellow and a kind friend. When 'asleep' he became very surly. To some persons he appeared to take a special antipathy in this secondary state. With a few of these it was evidently nothing more than a letting loose and displaying normal feelings toward them. He would not like them when 'awake,' but of course would not show it; when 'asleep' there was no attempt at control. There were two class-mates in particular with whom he was never pleasant, showing his dislike to them by every means in his power. He would call them by mean and ungentlemanly names, and in his doggerel rhymes would go out of his way to say some unkind thing.

But these were never confined to any particular persons; it was characteristic of him to be surly and disagreeable. Even to his room-mate, to whom he was under such great obligations and whom he normally liked very much, he was usually surly. It was a common thing for him to say that he was going to move out of his room because his room-mate was so 'grouchy'; and he would frequently blame others for being in this condition. John and the writer were always on the best of terms when he was normal, but a few times when 'asleep' he tried to engage in a quarrel. While in his room on one occasion near the end of the senior year, he ordered the writer to leave the room, stalked energetically to the door, opened it and said, "Do you see the door? Well, go out and do it damn quick." His command not being obeyed, he seized a chair and raised it over his head in a threatening attitude, but no attention was paid to him, and before long he lowered it. Finally both went out together, and he continued his abuse and after walking along a short distance, he seized the writer by the neck with one hand and hit him on the mouth with the other, but not hard enough to do any injury. Only on one other occasion did he try to engage the writer in any altercation.

Not only would he endeavor to quarrel with various persons, but he would frequently show his surliness by mean acts. The writer has seen him enter the room of a class-mate where there were three or four persons sitting, walk up to the desk, and with one sweep of his arm knock from the desk on to the floor a whole pile of twelve or fifteen books, then turn around and walk out of the room. The least word frequently sufficed to anger or offend him. With this he was often boisterous, shouting so as to be heard some distance, or again he might be puerile or clownish.

He appeared to have little judgment of the fitness of things; his speech might be nonsensical or improper, and his acts rash and precipitous. Along with his judgment of ordinary things went his ethical judgment. He seemed to be almost entirely different from the normal in this respect, which fact was shown by his language, temper, and his partaking of intoxicating liquors when 'asleep.' The latter is just mentioned here and will be taken up later in detail. He smoked when normal, but smoked most when abnormal. He was careless about money matters when 'asleep,' borrowing indiscriminately and not caring to pay back when 'asleep,' and knowing nothing of the debt he had incurred when he awoke. Some of his classmates thought ill of him on this account, for which of course he was not responsible. Even when he had money of his own he would wake up and learn that he had been intoxicated, yet upon examination find that he had as much or more money than when he went to sleep, and of course he would be unable to tell from whom he had obtained it.

He did not consider the cost, his judgment seemed weak here, and frequently he contracted debts which he would never think of doing if normal. On one occasion he purchased an expensive pipe for which he did not pay. On awaking he enquired concerning it, and when he ascertained the circumstances from his room-mate, returned it to the store. He at another time subscribed for a paper which he had refused to do when awake, and again he contracted with a firm to canvass, but on awaking he was enabled to have the contract annulled. While in his room one day he complained before the writer of being sore and stiff, especially in the arms. His room-mate questioned him when next he went to 'sleep' and found that while 'asleep' he had purchased a snow-shovel, gone a few blocks above the college and shovelled off a sidewalk for which he had received sixty cents. He engaged to come next morning and do further shoveling for one dollar, and left his shovel there during the night. In the morning he was normal, but before noon he went to 'sleep' and asked what time it was; he was told that it was half past ten, and then he related his yesterday's doings, saying that he had lost his job for he had promised to be there early in the morning to shovel. Although told where the shovel was he would never go and get it.

His will appeared to be weaker, and physically he was weaker when 'asleep.' His room-mate is a slight fellow, and when awake John, who is strong and muscular, could handle him, but when 'asleep' the conditions were reversed. His room-mate also thinks that when John was 'asleep' a slight difference could be detected in the quality of his voice. While his eyes were closed any one could tell when he was in the abnormal condition, and at first when he opened his eyes it was comparatively easy to distinguish the two states. His whole appearance was different and the change in his character showed very plainly. As he came to be more and more in this abnormal state it became correspondingly difficult to determine, and finally no one except his room-mate could tell, and even he at times was unable to detect the difference. It was not only so with others, but with John himself. Frequently the writer has accosted him in the morning with, "Well John, how are you this morning?" to which he would reply, "I'm asleep, been asleep since eight o'clock," or "Woke up asleep," but later on it was difficult at times for him to tell which state he was in. One evening he and his room-mate were sitting before the fire, when the latter said to him, "John, are you awake?" John thought for a moment and then was unable to tell. He was asked if he remembered certain circumstances, and in this way it was found that he was 'asleep,' for memory was the final test. There were certain stock questions, which his room-mate asked him; if he remembered, so that he could answer them, he was 'asleep'; if not, he was awake, for the circumstances occurred when he was 'asleep.' When it was difficult for John to tell his true state he would test himself by these questions, but it is obvious that this would be when he was 'asleep,' for he could not remember the questions when he was awake. He could sometimes tell by the way he felt, and his friends could as often tell by his mood. The latter was not always an index, for the writer has seen him 'asleep' when he was pleasant and apparently perfectly normal as far as mood was concerned, but this was very exceptional. If when 'asleep' he had been very angry, or in a serious altercation with any one, he would know it when he awoke by the way he felt. He did not know any of the details, however, the cause, the place, or even the person with whom he had quarrelled.

John was liable to go to sleep at any time; there seemed to be no rule about it with this possible exception — he was more disposed to be abnormal after studying hard, or exciting himself; therefore he was more liable to be 'asleep' in the afternoon than in the morning. He would go to 'sleep' between two sentences and continue the conversation so that no one would notice the change. His friends naturally attributed his abnormal states to his eyes, and considered that the strain caused by the eyes upon the nerves was not only the occasion of the individual attack but the total cause of his trouble. They noticed that he was almost always 'asleep' for Hebrew class, which came in the afternoons, and attributed this to the strain on the eyes, caused by deciphering the Hebrew points which he would study in the morning. It was also noticed that if he would take a vacation for a few days or weeks he was almost if not wholly free from attacks, and in the summer vacation only two or three attacks would occur during the four months, and these for not very long at a time; while in college they came to occupy quite half of his time. This supposition was reasonable from the standpoint of a layman, but expert testimony contradicts it. The opinions of two oculists and two eminent neurologists were obtained on this point, and they agree in affirming that the eyes were neither the predisposing nor the exciting cause of the trouble. To explain the 'post hoc' they say that while using his eyes he was also using his brain, and that this was the cause rather than the strain on the eyes; and that when he was home on the farm he not only did not use his brain so much, but that he was also in better general health. The strain of the eyes would have no more effect than the tiring of the arm, or any of the muscles of the body. The cause of the trouble as given by one of the neurologists was the epilepsy, in fact, that the abnormal state was epilepsy, the abnormal state being the equivalent of an epileptic seizure. This accords well with the diathesis and the history of the case. One physician mentions the possibility of a self-hypnotization, on account of the peculiar nature of the cataracts. When he looked at anything, he tilted his head forward and looked up, as it was found out later, looking over the top of the cataracts. This is a condition favorable to hypnosis, in fact is the Braid method of hypnotizing and a method in common use to-day. This would be valuable if we were at a loss for a cause; the epilepsy is so evident that this explanation is superfluous, as a *predisposing* cause, but it will be referred to later as the exciting cause.

Besides the liability of his going to 'sleep' at any time, he was as uncertain about waking up. The length of time in these states ranged all the way from less than a minute to several days. Above, we spoke of his waking and 'sleeping' several times in less than as many minutes. This was the shortest time. The longest time was during the latter part of his senior year. He went to 'sleep' on Wednesday morning at half-past nine, and that afternoon he, the writer and several other students went to a symphony concert. For the rest of the week he continued to go about his work as usual, attending classes and performing his regular duties, and in the middle of a sermon Sunday morning he awoke at a quarter of twelve. This makes his longest known 'sleep' four days and two hours. When he would awake he was unable to tell how long he had been 'asleep,' and had to look at his watch to find out the time of day, and find out in some way, usually by asking his friends, what day it was, in order to carry on his work.

In hiding his ignorance of time and other circumstances he has become very skillful. This he has tried to cultivate on account of his desire to keep the knowledge of his condition from the public. By adroit questioning he is able to discover the time and circumstances without betraying his ignorance of them. He has told the writer of his greatest predicament of this kind. John acted as a waiter in a students' club, a custom quite common among students trying to contribute to their expenses. He awoke at one time when coming from the kitchen with his hands and arms piled with filled dishes. He had not the least idea where any of them belonged, but very skillfully found the right places without betraying his confusion. Many incidents of this kind taught him to hide successfully the embarrassment caused thereby.

There were different depths to his "sleep," as shown by his different moods, but concerning this there was no rule either. During the latter part of his senior year all efforts to awaken him on the part of his friends were futile; when he awoke it was spontaneously. When 'asleep' one day John threatened someone, and two fellows seized him, threw him down, and pounded him some time in an endeavor to awaken him. He shouted and screamed, but with all their efforts they were unable to succeed, and had to allow him to go. On one occasion, though, he was awakened by a blow. He went to the door of a class-mate's room and demanded admittance; this was denied him. He then broke open the door, grasped an alarm-clock near by and threw it at the class-mate. Then he came over, sat on the knee of the class-mate and demanded twenty-five cents. On not receiving it he tried to obtain it by force. The class-mate could not stand any more, and being considerably the smaller, he struck John as hard as he could on the solar plexus. John immediately awoke, saw where he was, and on turning around perceived a number of fellows in the doorway, who were attracted there by the noise of the encounter. When he realized his position he was so chagrined that he went down to his room crying like a child. This episode occurred with one of his class-mates against whom he entertained a great dislike when 'asleep,' but, as with many others, if he awoke two minutes later he would be on the best terms and very agreeable.

He suffered considerable inconvenience from his trouble as one could imagine, the chief cause being the lack of memory of the events which occurred in the abnormal state. When we consider that during the last part of the senior year, fully one half of his time was spent in the abnormal state, we can see what a handicap it was. For instance he might prepare a lesson while 'asleep' (as he frequently did), and when the time came for recitation he would be awake and unable to remember a word of it. He took notes which were entirely foreign to him in his normal state, borrowed money and made purchases of which he knew nothing, and made engagements which he did not keep. One day he said to the writer, "Well, I must go over and make arrangements with old D. (Prof. D.) for an exam." He had already made an appointment for the examination of which the writer knew. John continued talking about it for some time until the writer felt sure that he knew nothing of the arrangements, when he was told concerning them. He accepted the matter as settled on account of his knowledge of like situations, and said, "Well then, I guess I'll get to work at it; I haven't much time."

His examinations were difficult for two reasons, first, because probably half of the class work would be prepared and recited when 'asleep,' and he always had to reckon on being awake when the examinations were taken; and second, he could not well prepare for them when he was 'asleep'; for if awake during the examination the preparation would be of no use to him. If he could plan on being 'asleep' during examinations, well and good, he could remember all, and probably remember it better when 'asleep,' for in this abnormal condition he was the better man, not only because he remembered more of his life, but because in many ways he was quicker, keener and more intelligent, and his memory for the events of normal life was better than when awake. The writer remembers going into his room one day when there was to be an examination in sight translation in Hebrew, in which both were interested. John, as usual, on Hebrew days, was 'asleep' and recited from memory the first two chapters of Genesis without a mistake. He read Hebrew much better at sight when 'asleep,' but this was no doubt because he was 'asleep,' so much on Hebrew days that few words were familiar to the normal self. He passed one examination in Biblical literature when 'asleep,' and got along very well with it. In talking with him recently, he said that because he had most of his Hebrew when he was 'asleep,' he had requested the writer to hypnotize him in order that he might the better pass the examination. This was done, and he passed it quite successfully. Of this the writer remembers nothing, but would rather trust John's memory than his own in regard to it.

Notwithstanding the obvious advantages of being 'asleep,' he never liked to be in this condition. Whenever he would tell the writer that he was 'asleep,' it was always in a voice showing disgust and disapprobation. He did not feel the responsibility for his work, and complained of making many mistakes, and got into more awkward positions. Where he thought that there was any possibility of success he made requests to have his friends try to awaken him. The only time that he ever courted 'sleep' was when he wanted to pass an examination.

The young lady John so frequently met in his dreams was named Edith, and when he had a fiancee his friends transferred the name Edith to her, this of course not being her real name. She was entirely ignorant of John's trouble and was purposely kept so, and quite frequently it was difficult to make explanations. When 'asleep' he would write her certain things, and on receiving an answer when awake, he would not know to what things she referred. When he first had this trouble he would, when awake, find a letter addressed to him which had been opened, and would accuse others of opening his letters and reading them, but he soon became accustomed to this situation and jokingly said: "I have double pleasure out of my letters; I get them when asleep, read them and enjoy them, and then when I wake up I read them and enjoy them again."

But more serious trouble was in store. One day when awake he started to see his fiancee, who lived about seventy miles from the college town. He went to 'sleep' on the train and awoke in a depot. He had no idea where he was, although he remembered starting in the morning. He went out and read the name on the depot and found out that he was still en route, being in a depot where he had to change trains, and was awaiting the second train. He arrived at his destination all right, remained awake until the second day, when he went out in the woods with the small brother of his fiancee, to teach him to snare rabbits. There he went to 'sleep,' and returning to the house endeavored to embrace his sweetheart in the presence of other members of the family. This she resented, and on returning to college the regular letter from her did not appear. He was awake and did not know the cause, but suspecting that something was wrong he requested his room-mate to question him when next he went to 'sleep.' This was done, the trouble ascertained, apologies sent, and peace restored. One incident which caused some surprise at the time was apparently of telepathic nature. One evening while 'asleep' he told his roommate that he knew what his fiancee was doing, and thereon related an account of her going to some social affair and the incidents which occurred there. The next day he received a letter confirming all that he had said. Although at the time some known connection, such as reference to the event in some previous letters was looked for, none such could be discovered.

In the latter part of the senior year John met a graduate student, Mr. X,[1] who endeavored to help him through the agency of hypnotism. John was not very well pleased to have his case examined, for this was what he feared someone would try to do, and naturally he did not wish his infirmity to become a public affair. However, his desire to be awakened overcame his fear of publicity, and he submitted. The writer quotes from Mr. X's thesis:

"On account of the grave nature of the case, I have refrained from disturbing (John) by experiments or from using hypnotism except as a therapeutic agent. Up to date (John) has been placed in slight hypnosis by me three times. The first occasion was an attempt to teach him to wake himself from the secondary state so as to escape the rough handling which was being resorted to. I will describe his visit to my room and the method employed. (John) had never formerly been hypnotized and knew practically nothing about the subject. In actuality autohypnosis and somnambulism had often taken place. The boys had been having him perform all sorts of feats and he was really a very suggestible person. I base what I now write on notes taken at the time.

"March 30, 1897. — About 9 P. M. (John) and his room-

[1] Mr. X wrote a fragmentary account of the case and presented it as an appendix to a doctor's thesis to one of the large universities. He confined himself, however, almost entirely to the events of the latter part of the senior year. Some of the incidents related here were also in his account, but the writer was in no way indebted to Mr. X for them. In fact some of these incidents were obtained from the writer by Mr. X, and the remainder were known by the writer or obtained from independent sources. The work of Mr. X should not be minimized, as it was extremely important, and the account of his hypnotic treatment is copied verbatim from his thesis.

mate called on me. (John) was 'asleep' and his room-mate said they had come over to get me to try my method of waking him. This morning in conversation with (John) I told him I was going to teach him to wake himself. I did not say I was going to hypnotize him; he did not know what to expect; but the sequel showed that he was very susceptible to suggestion.

"We sat for a few minutes talking and eating bananas. * * * After a few minutes I asked (John) if he was ready for me to try my method of waking him. He replied that he was. Up to this time he had sat rather quietly in my arm-chair but speaking heartily when he took part in the conversation. I noticed nothing in his demeanor specially characteristic of a secondary state. * * *

"Without telling him or his room-mate what I was doing, I had him settle comfortably in his chair and asked him to look steadily at a small square of white paper (about 1 in. sq.) which I pinned on my breast. I was standing directly before him and the spot was about a foot and one half from his eyes in an easy position. No attempt was made to fatigue his eyes but only to fix his attention a little. My orders were about as follows:

"'Look steadily at this white spot until your eyes feel drowsy, then close them. Think of nothing else but become drowsy and sleepy. You will have no headache or pain but go right to sleep.' After about a minute his eyes closed. At the same time I held my watch to his ear and continued: 'Listen only to this watch and my voice. They will help you to go to sleep. Go quietly and soundly to sleep now.' At about this stage I asked him if he were asleep. He seemed partly confused by the question, slightly opened his eyes and said, 'I guess so.' Questioning him some days later, he told me it seemed as if the world got dim and my voice sounded far away when I was having him go to sleep. To make sure that he had reached a suggestible state, I held the watch to his ear a little longer and continued, 'Listen to the watch and go to sleep. Go soundly and easily to sleep now, and then do what I tell you.'

"All this had not taken more than three or four minutes. (John's) head was slightly drooping forward and, with closed eyes, he seemed quite somnolent. Judging him ready for suggestions, I spoke to him in an easy, but firm, voice about as follows: 'I am now going to have you wake yourself up. I am going to tell you to count "One, Two, Three," and then clap your hands together. You understand now it is to count out loud to yourself, then clap your hands sharply together, and at that you will wake. You see I am teaching you to wake yourself. All ready now; count "One, Two, Three," clap your hands and wake up.'

"(John) had remained quiet, though apparently attentive, until I gave the last command and paused. Then he at once counted in an energetic voice 'One, Two, Three,' clapped his hands sharply together, then opened his eyes and looked about at his room-mate and myself in a surprised way. He probably would have betrayed more confusion had he not already become used to waking in unexpected places. As it was he only murmured something like, 'It has worked, has it?'—probably echoing his room-mate's exclamation, 'Well that worked fine!' To break a somewhat awkward silence, I reached to (John) a plate having on it a banana and the skins of those we had been eating previously, saying, 'Won't you have another banana?' This was an unfortunate remark in one way, but brought to light how complete was his lapse of memory. He looked puzzled and hesitatingly said, 'One of those (skins) is mine?'—and stopped with this conjecture, rising inflection, waiting for me to corroborate it. I told him he had eaten one banana while 'asleep' a few minutes before. He said he now remembered nothing since before 6:30 P. M. (Earlier in the day he had been asleep from one to six P. M., at the end of which time he had been awakened in the room of one of his acquaintances; then had stayed awake perhaps half an hour; then had been 'asleep' until this visit to me at about nine P. M.) I explained to him that I had taught him to wake himself; that he did not now remember how it was done, but that he would remember all the next time he was 'asleep;' and that he must then wake himself; I told his room-mate in his presence not to tell him how he was waked. After a few minutes both went home apparently highly pleased.

"After the above I did not see (John) for two days; then I called at his room. He told me that he had found out how I waked him. This came about in a peculiar and amusing way. Going to 'sleep' in his usual off-and-on manner, he had tried my recipe several times with great success and much to his pleasure in thus having some control over his states. He found it seemingly harder to work, however, and resorted to trying to go sounder to sleep before using the formula. While over at his eating club on the morning of April 1, he waked himself up and was surprised to find himself repeating 'One, Two, Three,' and feeling as if he had clapped his hands. After his breakfast he came to his room and stayed awake until about 10 A. M. Was 'asleep' five or ten minutes when he went down stairs. While there he woke himself up by the recipe, but repeated '1, 2, 3; 1, 2, 3; 1, 2, 3;' and clapped his hands several times after he was awake and could not stop it. (John) soon went to 'sleep' again and had a slight quarrel with (a friend). He remained 'asleep' only a few minutes when he woke himself again by seating himself in a rocker, swaying to and fro until he went sounder to sleep, then saying '1, 2, 3,' and clapping. He again had to repeat and finally broke off with the interjection, 'O damn, can't I stop this!' Telling me of his experience later, he said it seemed as if his whole body was paralyzed for a few minutes, except his tongue and hands, which kept repeating '1, 2, 3,' and clapping.

"After my visit of April 1st (John) did not go to 'sleep' until April 3d. This was staying awake more than usual and it is possible he took as a 'suggestion' my merely telling him that I not only was going to stop his repeating, but should keep him from going to 'sleep' at all. I had laughed at what he had told me and assured him that it could soon be stopped. I felt certain that it was a special case of the tendency to continue induced actions which is found so peculiarly in some subjects. * * *

"Monday, April 5, 1897. About 10 A. M. (John) and his room-mate called at my room. (John) was 'asleep' and wished to wake up. He had gone to 'sleep' Saturday afternoon and had been 'asleep' and awake several times since. He had waked himself by my recipe, but had to repeat '1, 2, 3, clap' too many times. He thought he had repeated it fifteen times; his room-mate had seen him repeat about eight times. * * * When I got ready to hypnotize him he kept on talking and would not fix his eyes as I directed. I left him alone a few moments till he became quiet, then I placed him in a slight hypnosis as on the first day, only cutting the time shorter. I gave him suggestions about as follows:

"I am going to wake you myself this time. Then you must stay awake for the rest of the week. Do not go to 'sleep' when you see A, or anyone. If you should accidentally go to 'sleep' come to me to be waked up. Now when I say '1, 2, 3,' and clap my hands, you will become wide awake and stay awake for a whole week. Have you understood everything? Nod your head if you have. (He immediately nodded.) All ready now: 'One, Two (I think he woke at the word 'Two,' at least he then opened his eyes), Three, Clap'—clapped my hands. He aroused more, looked around puzzled and said, 'Well, I came up to your room again.' I watched for any tendency on his part to 'repeat' as if he had waked himself, but there seemed none. * * * His pipe was lying on the table where he had put it a few moments before. He looked at it and said, 'Have you a pipe just like this?' He thought it was his pipe but did not remember putting it there. As he arose from the chair his back twitched and he started to tell me about having hurt it in the gymnasium. He seemed to have no idea that he had explained all this fully a few minutes before.

"After (John) awoke, I threw the little square of paper at which he had been looking in my open fire. He saw the action and I remarked, 'There goes what we did it with.' He immediately asked, 'Did you have me sign a contract?'—thus unconsciously illustrating how complete was his lapse of memory, and how he was trying to piece it out with conjecture.

"Friday, April 9, 1897.—(His room-mate) saw me in the Library at 11 A. M. and told me (John) had just gone to 'sleep.' He seemed to linger in a sort of half-sleep a few minutes before going fully over. Soon (John) said he had gone fully to 'sleep.' I immediately went over to see him and found him in the room above. He was trying to hypnotize Z. in the manner in which I treated him. He was not having good success with it, however, and there was considerable fun over it. (John) had remained awake the prescribed length of time lacking one day to finish out the week. It seemed, however, that he had irritable spells which ordinarily went with his 'sleep,' but at the time he would declare that he was not 'asleep.'

"After a little conversation I took him over to (the college physician's) office. In the presence of the doctor I placed him in a slight hypnosis and gave him the following suggestions: 'I am going to have you do three things. 1. You will wake up and stay awake. 2. You will not fight or quarrel with any of the boys. 3. You will wake yourself by counting "1, 2, 3, clap," but not repeat it at all.' I repeated these suggestions to him and had him nod that he understood them. At my saying that he would not fight with the boys he smiled. When all was ready I had him wake himself. He did so in a moderate manner and did not in the least repeat * * *.

"After getting waked up in (the doctor's) office April 9, (John) stayed awake thirteen days. On the morning of April 22 he went to 'sleep' on the train on his way back to college. (He having gone home on his Easter vacation on April 14.) * * * During the week after his return to college (John) went to 'sleep' twice."

Besides the above records taken from the thesis of Mr. X, John was awakened hypnotically three other times; once again by Mr. X in the presence of the writer, once by the writer, and once by the college physician. On May 3, the writer met him on the college grounds, and was told that he was 'asleep.' The suggestion was immediately made that he go to Mr. X to be wakened. He at first refused and began to speak of Mr. X in a very disrespectful and antagonistic manner. After considerable persuasion and argument, and the writer's agreeing to accompany him, he consented to go. But after getting started he did not seem to be real anxious to continue his journey, for he stopped as frequently as he could find any pretext for so doing, and in one instance making a pretext by going into a store and purchasing some maple-sugar. This was partially eaten on the remainder of our way.

At length we arrived at the house and found Mr. X at home. After being seated Mr. X said, "Well how is everybody?" John jokingly replied, "Cutten has gone to sleep." Mr. X then asked how long 'Cutten' had been asleep, to which he replied, "I can't tell very well, but I think since sometime this morning." John offered Mr. X some maple-sugar and they talked for some time, when finally the latter said, "I suppose the best thing for Cutten is to get waked up, isn't it?" to which John replied, "That is what I came up for, but I do not believe your method will work. I tried to wake myself by saying your '1, 2, 3,' twenty times but it would not work." An appropriate answer was given and John was told to lie back in his chair. This he did and after some difficulty, for John would persist in talking, he went off to sleep. The following suggestions were then given: "Now I am going to wake you up in a new way. It is a simple, easy way, but just as good as any. You will snap the fingers of your right hand and then you will wake up. All right, snap your fingers and wake up." At once he obeyed the commands, and looking up to Mr. X he said, "I came up to your room, did I?" then looking around and seeing the writer he said, "Hello Cutten, are you here? Did you bring me up?" He then started to help himself to the maple-sugar, but quickly excused himself for what he considered a breach of manners due to his confusion. The writer then explained that he had bought it on the way up and therefore it was his to do what he liked with, and then he passed it around to Mr. X and the writer. The writer and John then left Mr. X and went to John's room, he asking on the way the circumstances attending his going to Mr. X.

Three days later again the writer met John on the college grounds, and as before, on enquiry was told that he was 'asleep.' John was again advised to go to Mr. X to be awakened. The resistance was stronger than at the other time, very uncomplimentary remarks were made concerning Mr. X, and in addition to this he said that he thought Mr. X had power over him. He resisted every effort to this end, and finally said, "You come and wake me up Cut., you can do it as well as that little pimp." His request was finally complied with and we went to his room. He was told to look at a thimble held before his eyes and to listen to the ticking of a watch held to his ear. He soon went to sleep and he was told to say 'Presto,' clap his hands together and wake up. This he did with great vigor and awoke. He looked up and said in a pleased tone, "Did you wake me up, Cut.? Bully work." Later in the day he was 'asleep' and ordered the writer out of the room as recorded above.

One evening after the class in physiology John followed the college physician into his office and told him he was 'asleep' and then requested that he be awakened. The doctor put him in a slight hypnosis and told him to slap his knee when he heard the doctor count up to three and awake. This he did and said that he knew how he had been awakened, his knee tingled so that he thought he must have slapped it pretty hard. Besides being awakened these six times his room-mate awakened him once or twice.

On the seventh day of May John went to his home and remained for the rest of the month. During this time he had very few attacks and returned to college on the first of June to take his examinations, feeling comparatively well. From then until commencement he was 'asleep' a number of times, but not so frequently as before his visit home. One thing which has not been recorded and which was quite severe at the latter part of the senior year, as well as all through that year, was the frequent bleeding of the nose. This he has been more or less troubled with all his life.

(*To be concluded.*)

THE DISTRIBUTION OF ATTENTION. II.

BY DOCTOR J. P. HYLAN.

9. The Conscious Period.

From the conditions pertaining to the processes of the subconscious period, we have no reason to think that the number of the elements of an impression are here interfered with by any of the difficulties that might attach to simultaneity. Thus, if we take a passing glimpse of the page of a newspaper the words and letters do not appear to interfere with each other's distinctness, although many of them serve as stimuli at the same time. But we do not perceive these as so many objects with distinct meanings. Thus we have two distinct functions involved. One has to do with the mediating of stimuli in their right number and proportions so far as they are brought within range of the sense organ. The other has to do with the meaning and interpretation of these. Plainly, if some objects are perceived in smaller numbers or less readily than others when all are of equal distinctness, the difference is due to the function of perceiving rather than to that of simple transmission; and such a result would indicate an interference in perception, or, in other words, a lack in an adequately divided attention.

Wundt distinguishes no difference in the number of letters, figures or lines which can be seen from a tachistoscopic exposure, and refers to Cattell's work for details.[1] Cattell used an exposure of 0.01 sec. Twelve cards with vertical lines 2 mm. apart were shown five times each. There were from four to fifteen lines on each, and the subject was told to estimate the number seen. In a similar way letters and figures were exposed, and also one-syllabled words, and sentences. The observer was required to give the arrangement of the single elements of an impression, and different characters were sharply

[1] Wundt, *Phys. Psych.*, II., 287, 288. Cattell, *Phil. Stud.*, III., 121 ff. See especially page 126.

distinguished. The results were arranged according to the numbers of right and wrong cases. If all the objects on the card were rightly judged, it was a right case; if not all rightly judged, then a wrong case. Two was the smallest number of words shown at once, three the smallest number of figures and letters, and four the smallest number of lines. By combining the essential results from the seven subjects, all of whom served in the four kinds of Cattell's experiments with lines, figures, letters and words, and retabulating for purposes of comparison, we have the following table based upon two of his.[1] I have arranged it in four parts corresponding to the four experiments. The column to the left gives the number of objects on the card exposed. Under each of the headings, 'Lines,' 'Figures,'

TABLE VI.

Objects Ex-posed.	Lines.			Figures.			Letters.			Words.		
	Right.	Wrong	%	Right.	Wrong	%	Right.	Wrong	%	Right.	Wrong	%
2										48	32	67
3				65	5	8	61	9	14	14	46	329
4	50	5	10	52	18	35	50	30	60	19	21	111
5	41	14	34	68	42	62	36	64	178	1	19	1900
6	41	14	34	31	59	190	14	46	329			

'Letters,' 'Words,' are three columns. The first column gives the sum of cases from all the subjects in which the objects were seen correctly, — the right cases. The second column gives the sum of cases from all the subjects in which a mistake was made in seeing the objects, — the wrong cases. The third column gives the per cent. that the second sums are of the first. With some experiments, especially in the cases of the words and letters, some of the subjects dropped out who could not see the larger number of objects correctly, so only those who could see most continued to the higher numbers. This accounts for a comparatively small number of wrong cases and low per cents., *e. g.*, 111 under 'Words.'

This table shows a very marked and constant increase in the number of wrong cases as we pass from the lines to the words. Thus for four objects the per cent. amounts to 10 per cent. for

[1] *Loc. cit.*, pp. 124, 126.

lines, 35 per cent. for figures, 60 per cent. for letters and 111 per cent. for words; while the wrong cases were 67 per cent. of the right cases where but two words were given. The figures, it was noted, tended to form numbers and this helped to increase the number seen over the letters. One could see three times as many letters when they formed words as when this was not the case. If the words formed a sentence, twice as many could be got as when they stood together without this relation. Sentences were also exposed and the length of the sentence which could be read was from three to seven words, depending upon the subject. It was found that the sentence was perceived as a whole. If the sentence were not perceived, neither were the words that composed it; but if it were, the single words appeared very distinct.

These results show two things very clearly. First, that the greater the function of perception that is involved, the greater is the mutual interference and the smaller the number of objects seen; and second, that the close perceptive relations of the objects causes a mutual reënforcement and an increase of the number seen. I believe also that the number of objects which Cattell's table gives as seen is too large, since the cards with letters and figures seem to have been used a great many times, and this could not but have promoted a familiarity with them which would cause them to be partially remembered from time to time.

This close study of Cattell's tables was suggested by what at first appeared as an incongruity between his results and those of the following experiments of my own. As parts of two different series of experiments to be described later, one consisted of exposing simple objects and the other of letters upon cards with the tachistoscope above described. The objects consisted of circles, squares, diamonds, oblongs, etc., all about the same size, of black paper upon white cardboard, with ten objects on each card, and irregularly arranged over the surface exposed. The full width of the opening in the shutter, 6.3 cm., was used, thus giving an exposure which, from beginning to end, lasted 42σ. This gave a uniform exposure to all parts of the field. First the right, then the whole, then the left of the field were shown successively, but the rate of movement was too rapid to make succession perceptible. The heads of the subjects were placed in a rest one meter from the apparatus; the eyes were fixated upon the fixation mark in the center of the field to be exposed, and the attention distributed over the field so far as possible. After the exposure, the subject gave the number of objects distinctly seen and was then immediately asked to identify them by referring to the card which was given for inspection. There were twenty of these cards, each shown five times, at the rate of twice a week, on Mondays and Wednesdays. The experiment thus consisted of a hundred exposures for each subject, of whom there were six. Table VII. gives the sum of the objects thus

TABLE VII.

Subject.	D.	H.	Mea.	Mer.	R.	Rog.	Average.
Number of objects.	955	791	699	695	619	454	702
Subject.			A.	S.	R.	Y.	Average.
Number of letters.			299	228	238	287	263

seen in the hundred exposures for each subject.

Under the same conditions the black letters previously described, of about the same distinctness as the objects in the last experiment, also irregularly arranged upon white cardboard, were exposed for the same length of time. Here there were but four subjects, and all different from those who saw the simple objects except one, *R*. This fact deprives the table of much of its value, except so far as we may regard the results of these two groups of subjects as generally characteristic. All the subjects were students of more or less training in laboratory work.

Here more than twice as many objects were seen as letters; a result in general accord with that of Cattell, although differences of method make a close comparison impossible. The objects which could not be perceived formed an indistinct blur at the instant of exposure; while those which were, appeared with varying degrees of distinctness and were counted, in most cases, one at a time, from the fading mental after-image which followed the exposure. All remarked a tendency to see the objects in rows and groups, a device which seemed to assist the getting of a large number, and sometimes the positions were slightly changed, apparently to assist this grouping. This is evidently of a kind with the seeing of more figures and letters when they formed numbers and words, and illustrates the mutual reënforcement arising from close perceptive relations. The experiences with seeing the letters were not essentially different, and are not to be distinguished from those already described with the successive exposure of letters. We have now to ask what bearing does the seeing of a small number of complex objects as compared with that of simple ones, and this reënforcing tendency in perception, have upon the question of the simultaneous distribution of attention.

It has been pointed out that a perceived object may be regarded as composed of more or less discrete elements, as of form, position, color, etc. The elements of form in a circle or square, *e. g.*, are less in number than those composing a letter; and those of a letter less than those composing a word. It has also been noted that what I have called the mental after-image fades very quickly. Immediately after the exposure the objects are distinctly in mind, and may be more vivid even than at the instant of exposure. This, however, is not an ocular after-image but a mental one, and fades out in from one to five seconds, depending upon the subject and conditions; after which, if an object has not been perceived, it cannot be recalled. Now, if we should find that it takes a longer time to perceive an object in proportion to its complexity, we should have an explanation of the fewer complex objects that can be got from an exposure; for since the mind apparently passes from one object to another in succession, and the mental after-image is of limited duration, the number of objects got would depend upon the rapidity with which one's mind could pass from one to another.

In finding the time required to recognize one letter out of twenty-six possible ones, Cattell, whose simple reaction time was 146 σ, reacted in 326 σ, while one short English word from twenty-six took 360 σ, a short German word 367 σ, and a long English word 375 σ. This indicates a distinct difference of time for recognizing letters and words, although much less than the difference of complexity between them, and shows the effect of practice in recognizing words as a whole. Another series of experiments by Friedrich with three subjects, in which from one to six place numbers were recognized, showed a similar increase. Here the simple reaction time was 186 σ. In passing from the recognition of one-to-two or of two- to three-place numbers, the increase in time was slight. Thus for one-place numbers it was 318 σ, and for three-place, 397 σ. But from here on the increase grew with the increase of the number of numerals in the following steps: 53 σ, 147 σ, 322 σ.[1]

This fact of the increased time of perception in proportion to the complexity of the object seen is an explanation of the fewer complex objects than simple ones that can be seen from a tachistoscopic exposure. But this explanation rests upon the supposition that the mind passes, as appeared often to be the case as examined by introspection, from one object to another as they remained in the after-image. If this transition from one object to another takes place, no simultaneous distribution of the attention necessarily takes place, and tachistoscopic experiments are satisfactorily explained without its use. It therefore came to be my purpose to test this question of mental succession by other than simply introspective means.

We may naturally ask why it should take longer to see a complex object than a simple one. We can see how this would be if there yet remains in the perception of the complex object a degree of succession in its parts. The slow process in learning to read suggests that this is the case with words and numbers. This is supported by the introspective results of recent research in the Leipzig Laboratory.[2] In a study of reading by means of an improved form of tachistoscope, Zeitler concludes that the reading of a word is a successive process. This was shown the most clearly by putting a false letter in the middle of a word. When this was done, the attention would hang there and not get farther. The subject also distinctly noticed intervals in the appearance of the signs of the word in con-

[1] See Jastrow, 'The Time-Relations of Mental Phenomena,' p. 38, and table pp. 32–33.

[2] 'Tachistoskopische Untersuchungen über das Lesen,' by Julius Zeitler, *Phil. Stud.*, B. 16 (1900), S. 380.

sciousness. These signs were formed by (*a*) the arrangement of the controlling groups of letters from left to right, and (*b*) the different vertical height of letters in the structure of the word. Succession was more noticeable with foreigners than with Germans, since with the latter the more rapid assimilation made the process more frequently appear simultaneous.

Numerical results bearing upon this point would be much more valuable. If we can arrange simple objects together in such a way that they do not form wholes with such close perceptive relations as letters and figures possess when they form words and numbers, we might, according to this hypothesis, expect it to take distinctly longer for perceiving a number of such objects than for one, and in proportion to the number presented together, even though the elements composing the groups be very simple. And this result would be weighty evidence against the simultaneous perception of the objects. The tachistoscope above described was used according to this plan. Four white cards were used, all having one small black circle, and in a different part of the field exposed for each. Four cards were used with two such circles, but arranged in different relative positions in each. In a similar way there were four with three circles, four with four circles, and four with five. All the circles on one card were of the same size, so that they might be seen with equal distinctness; but the size was different, varying from seven to eleven mm. in diameter, for each of the four cards composing a set. This was to keep the subjects from reacting immediately to the amount of color rather than to the number of objects; since if the circles were all of the same size, it had previously been ascertained, one would distinguish the number simply from the amount of black upon the card. The electrical connections of the tachistoscope were so arranged that the hands of a Hipp chronoscope were started at the instant of complete exposure of the card, and stopped by the subject's reaction. The shutter of the tachistoscope moved at the same rate as before, and the whole opening was used. The subjects used a five-finger key, pressing the key corresponding to the number of objects seen. The subjects were given a short period of practice twice a week for six weeks before the reaction-time was regularly recorded, in order to establish quick connections between the number of objects seen and the response of the appropriate finger. During this practice a different set of cards was used than that when the reactions were recorded, in order to prevent a familiarity which would cause the objects to be seen simply as groups rather than individually. The same hour of the day was always used for each subject. In conducting the experiment, the cards were used in an irregular order so that the subject did not know beforehand to which he was to react. The order of events at an exposure was as follows: the chronoscope was started, the experimenter counted 'one' two seconds before the exposure, and 'two' one second before it. The exposure was made, the subject reacted, and then gave the number of objects seen. It was found of advantage to begin each sitting with a few unrecorded reactions in order to overcome the subject's inertia. The dark room and electric light were used to secure constancy of illumination.

It may be seen that this experiment presupposes the subject's ability to see five simple objects at one exposure, a larger number than could be seen of the letters under the same conditions. During the preliminary training, one subject, *Rog.*, had difficulty in getting five objects. This occasioned the experiment with simple objects, the results of which are given in Table VII. This showed that the average number seen for this subject was about four and a half for each exposure. The record of the five days in which the set of twenty cards was shown to this subject in that experiment shows the sums of the objects seen each day as follows: 83, 77, 86, 97, 111. No doubt a part of this increase was due to a growing familiarity with the cards, but in the reaction experiment which followed, practice had had sufficient influence to prevent any difficulty in seeing five objects.

The choice-reaction to from one to five objects evidently involves the following factors: (1) the perception of the number of objects to which reaction was required; (2) the afferent and efferent processes involved also in simple reaction; (3) the choice by which the number of objects seen was associated with its appropriate motor expression, and (4) the specific time of each finger. While we are especially concerned with the first of these, all helped to determine the time of the reaction. In order to determine the value of each of these, two other series of reactions were taken with the one already described. One of these consisted of reactions with the appropriate finger when the subject knew the number of objects about to appear. Since the subject was required to perceive before the response only the group as a whole, not the number of objects exposed, the time was that of simple reaction, including the specific time of each finger separately. All reactions belonged to the sensorial type. This series consisted of ten reactions for each finger of each subject. Half of these immediately preceded, and half immediately succeeded the other series.

The other supplementary series immediately followed the main experiment, and consisted of choice-reactions to the five Arabic figures, 1 to 5, presented upon white cards. The figures were of jet black gummed paper and 13 mm. high. Ten reactions with each finger of each subject were also taken here. The difference between the time here and that of the simple reaction is obviously the amount needed for the clear discrimination of the figures and the choice of movement. This difference, regarding the time needed for the discrimination of the different figures as equal, would vary with the quickness with which each figure could be associated with the appropriate finger for reaction. The difference between this series and that of choice reactions to from one to five objects is the difference between perceiving figures and numbers of objects. We may assume that it takes about the same time to perceive the figures 1 to 5 with the possible exception of the figure 1 on account of its simplicity. But two subjects were able subjectively to distinguish a difference of time: to *Mea.* it seemed that the time was slightly shorter for 1, while for *R.* it seemed distinctly shorter, and also slightly shorter for 2 than for 3, 4 or 5. By subtracting the time required for reacting to the figures from that required for reacting to the objects, we get the time needed for perceiving the objects more than that needed for perceiving the figures.

Table VIII. is arranged to allow a comparison of the two supplementary series. The mean variations and averages in

TABLE VIII.

Subj.	Finger Used.	1st.		2d.		3d.		4th.		5th.	
		Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
	1–5 figures.	466	23	529	67	560	58	542	29	502	47
D.	Simple reactions.	145	22	149	17	129	27	162	37	212	16
	Difference.	321	1	380	50	431	31	380	—8	290	31
	1–5 figures.	459	67	484	48	443	45	470	35	531	44
H.	Simple reactions.	134	8	143	16	162	38	138	15	140	19
	Difference.	325	59	341	32	281	7	332	20	391	25
	1–5 figures.	485	41	516	21	590	36	604	101	508	31
Mea.	Simple reactions.	178	21	153	18	166	34	147	18	147	17
	Difference.	307	20	363	3	424	2	457	83	361	14
	1–5 figures.	416	36	482	29	494	59	543	48	454	45
Mer.	Simple reactions.	130	11	128	15	113	8	144	22	124	10
	Difference.	286	25	354	14	381	51	399	26	330	35
	1–5 figures.	358	25	345	27	364	39	394	29	398	56
Rog.	Simple reactions.	106	5	103	8	97	3	93	8	96	10
	Difference.	252	20	242	19	267	36	301	21	302	46
	1–5 figures.	537	30	669	42	687	57	775	53	682	71
R.	Simple reactions.	204	49	205	47	181	23	165	14	169	17
	Difference.	333	—19	464	—5	506	34	610	39	513	54
Sums of differences.		1824	106	2144	113	2290	161	2479	181	2187	205
Av. of differences.		304	18	357	19	382	27	413	30	365	34
Sums, simple reactions.		897	116	881	121	848	133	849	114	888	89
Av. of simple reactions.		150	19	147	20	141	22	142	19	145	15

thousandths of seconds of ten reaction times are given, while the difference between the series is also given for each finger in the case of each subject.[1] The averages of simple reactions at the bottom show that the first finger — the thumb — was slowest, and the third or middle finger was the quickest. Also the fifth or little finger was most constant, as shown by the mean variation. No strict uniformity, however, obtains between the different subjects in these respects. The averages of differences show either that the figure 1 was more quickly recognized than the other figures, or that it was more readily associated with the movement of the thumb than the other figures were with their respective fingers. Regarding the latter as partially causing this small number, we see that the association time gradually increases until it reaches its climax with the fourth finger; while the mean variation increases in a similarly gradual way, reaching its climax with the fifth finger.

Table IX. presents a comparison between the choice-reaction with figures 1 to 5, the results of which are also given in Table

[1] With one subject, Rog., the averages in the simple reaction series are of five instead of ten.

TABLE IX.

Subj.	Finger Used.	1st.		2d.		3d.		4th.		5th.	
		Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M.V.
	1–5 objects.	481	58	531	44	578	48	651	116	537	61
D.	1–5 figures.	466	23	529	67	560	58	542	29	502	47
	Difference.	15	35	2	—23	18	—10	109	87	35	14
	1–5 objects.	511	45	511	51	541	59	577	69	563	96
H.	1–5 figures.	459	67	484	48	443	45	470	35	531	44
	Difference.	52	—22	27	3	98	14	107	34	32	52
	1–5 objects.	483	52	550	43	630	63	609	62	516	56
Mea.	1–5 figures.	485	41	516	21	590	36	604	101	508	31
	Difference.	—2	11	34	22	40	27	5	—39	8	25
	1–5 objects.	481	47	542	65	574	75	606	87	495	45
Mer.	1–5 figures.	416	36	482	29	494	59	543	48	454	45
	Difference.	65	11	60	36	80	16	63	39	41	0
	1–5 objects.	415	35	424	39	410	47	454	39	467	63
Rog.	1–5 figures.	358	25	345	27	364	39	394	29	398	56
	Difference.	57	10	79	12	46	8	60	10	69	7
	1–5 objects.	545	82	698	96	719	112	850	178	664	104
R.	1–5 figures.	537	30	669	42	687	57	775	53	682	71
	Difference.	8	52	29	54	32	55	75	125	—18	33
Sums of difference.		195	97	231	104	314	110	419	256	167	131
Av. of difference.		33	16	39	17	52	18	70	43	28	22

VIII., and the choice reaction with the simple objects, one to five in number. With the latter, as there were four cards for each number of objects, and as these were shown five times, the numbers in the table are averages of twenty reaction times. The averages of differences for each number of objects in the last line of this table are what especially concern us, as they give the pure perception time of the objects over that of the figures. We see here that the time increases gradually from one object to four with a very decided dropping off for five. The smallest increase is 6σ, that of two objects over one. This probably should be larger, since the tendency for the figure 1 to be recognized more quickly than the other figures would make the difference time for recognizing one object (33σ) too large. It should be observed that the mean variation increases in degrees similar to those for the time of perception, which is additional evidence of the increasing number of successive steps of the mental process as the number of objects increases. This also falls off with the time of perception in the case of five objects. This decrease of time needed for perceiving the five-object cards appears out of harmony with the time for the other cards and indicates a difference of method for perceiving them. We are able, however, to explain the decrease of time for the five-object exposures. There can be no question that the mental process here involved was different in degree or in kind from that for the two-, three- or four-object exposures, and this difference, as shown by the low mean variation, was fairly constant. The introspective records throw light upon this point. In general, the cards did not seem to present discrete

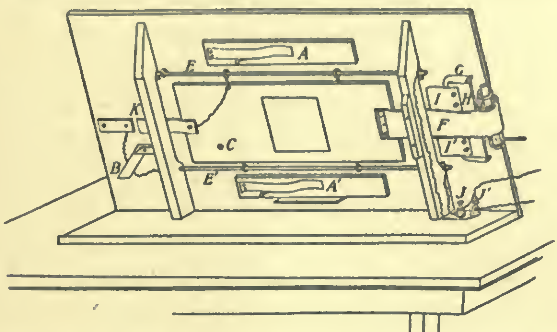

FIG. 1. Shows the back with the shutter in the position giving complete exposure. *A*, *A′* is the card-holder, and *B* the lever, the inner end of which fits against the screw-head *C* when the shutter is drawn back preparatory to an exposure. The shutter slides upon the brass rods *E*, *E′* and is propelled by the rubber-band *F*. *G* is a stuffed rubber tube which deadens the compact of stopping against the stop *H*; while *I*, *I′* are brass springs which fit closely upon the end of the shutter and prevent a rebound. Electrical connections are supplied via the binding-posts *J*, *J′* so that the current is broken at *K* when the shutter has reached the position of complete exposure.

objects, but a figure which, by its general appearance, was assigned to a certain finger and according to its degree of complexity. Counting was often used as a test of this complexity and usually before reacting. On account of the large numbers, the cards with five objects were more difficult to count than any of the others and recourse was had to devices for shortening the process. One of these consisted of telescoping the process in counting, so that one or two of the numbers dropped out and the others slipped over with varying stress, *e. g.*, 'one, three, four-five,' with emphasis upon the last. The grouping of the five objects into one group of one or two and another of three or four was more generally used; while two subjects were able to recognize the five-object cards immediately, because they were instantly seen as more complex than the four and it was known there were none with six.

Do these methods accomplish a distribution of attention for the five objects? Evidently the last does not, since the group was judged as a single complex object simply having one or more features different from those of other groups. The dividing of the objects into two groups, the constituents of which could be more immediately perceived, is apparently another form of the same thing where the groups were immediately perceived from their form and then combined. Introspectively, this seemed to be a slow method. Probably telescoping was only a device for hastening the process of counting. It was mentioned by but one subject. Table IX. shows two subjects with whom the difference-time was longer for five than for four objects. These were the only ones who did not resort largely to some of these devices for the five objects. *Mea.* observed none, while with *Rog.* the five objects required the most distinct counting.

The question arose as to whether the immediate perception of the five objects as experienced by some was really due to the fact that five was known to be the maximum number, or perhaps to a real distribution especially facilitated by the number five. Would the same result appear for the maximum if it were *e. g.* six? Also was not the perception time influenced by the association time for the different fingers since the main features of the curves for both would coincide? To test this the above experiment was repeated, but both objects and figures ranging from one to six were used. Two five-finger keys were employed, one of which was used by the thumb and first and second fingers of the right hand, and the other by the same members of the left hand. There were four subjects, only one of whom took part in the above experiment. The time for perceiving the objects above that for perceiving the figures was ascertained. Two of the subjects perceived the six objects immediately as more than five. For them the time was distinctly less than for four objects, and for one of these it was also less than for five objects. With the other two subjects the six objects were not perceived in this way but were counted and sometimes grouped. With these the time was greatly lengthened. The subjects averaged together as follows: for one object $0\,\sigma$, for two $-10\,\sigma$, for three $16\,\sigma$, for four $14\,\sigma$, for five $34\,\sigma$, for six $133\,\sigma$. This result is in accord with that of the earlier experiment although a different proportion of the two kinds of subjects with regard to their perception of the maximum groups would make the form of a resulting curve somewhat different. The evidence against distribution is thus strengthened since this additional experiment supports the interpretation placed upon the former. Whatever may have been the cause of the $-10\,\sigma$ for the group of two objects, this could not be regarded as a case of distribution since the time is less than that for the single objects.

The argument against distribution in these experiments may be summarized as follows: In Cattell's experiments, as well as with my own, it was found that the more complex the objects exposed by the tachistoscope, the smaller was the number that could be retained from the exposure. Since introspection shows that there is a definite limit to the time during which the objects may be retained before perception is completed, this difference in the number of simple as compared with complex objects that can be perceived may be explained if a longer time is required to perceive a complex than a simple object. Cattell has shown this to be the case with letters and words, and Friederich with numbers. The most natural explanation of this lengthened time for the complex object is that its parts become conscious successively rather than simultaneously, a solution supported by the introspective results of Zeitler in reading words from short exposures, and also by the process experienced in learning to read, where the letters of a word are first recognized one at a time, and then recognized apparently as wholes as a result of practice. If this is the true explanation, we might expect a distinct difference of time to be required for discriminating groups of objects in proportion to their complexity, even when their elements are much simpler than the letters and figures that compose words and numbers, and when no habit in succession, as in reading, has been formed. Our experiment has shown this to be the case when the elements are perceived as such. When they are not so perceived, evidently the question of distribution is to that extent not involved, since the group, not the element of the group, is the unit for the attention, in the same way that we recognize a word from its configuration rather than by perceiving the letters as such that compose it. We have thus obtained adequate numerical evidence to support the introspective finding that the mental process involved in the tachistoscopic exposure employs successive rather than simultaneous acts of attention.

We have noted the reënforcing effect in the number of letters and figures seen when they form words and numbers respectively. This is evidently caused by previous familiarity with the words and numbers thus formed. In the last experiment this familiarity tended to make the groups perceived as wholes from their configuration rather than to present discrete elements. The introduction of a new set of cards caused hesitation and a feeling of strangeness. The effect of this acquaintance was naturally to vitiate the experiment. It assisted counting. Also, if distinctly remembered, the configuration of the card suggested the reaction rather than the number of discrete objects, and hence caused a decreased reaction time due not to a distributed attention but to the perceiving of the group as a whole. The fact that in spite of this tendency there yet remains a distinct increase of time in passing from the one-object to the four-object cards makes the argument against distribution still stronger. On account of this tendency, the practice set was different from that used in the final series. When the series was over, each subject was asked to note such cards as were recognized after an interval equal to that between the different sittings. An average of 53 per cent. was remembered, although it was difficult to pronounce definitely in regard to many of these. Those remembered were distributed fairly evenly between the different numbers of objects, and were cards which gave both unusually long and unusually short reactions.

10. Reactions to Disparate Impressions.

If it takes a separate associative process requiring an appreciable amount of time to perceive each object in a group when these objects are of the same kind, we may expect this time element to be increased when instead of similar we deal with disparate impressions. This result is suggested by the experiment in which disparate impressions were counted and where the absence of distribution was shown more distinctly than with the counting of similar impressions. On the other hand, if distribution is possible, it should be in evidence as much in the perception of groups of disparate as of similar impressions.

To test this, the following apparatus was constructed. A wooden screen 58 cm. by 30 cm. was attached to a base so that when placed upon a table it rested in a vertical position with the long axis horizontal. Near the center of the bottom of this were two round openings arranged vertically 1 cm. in diameter and 2.5 cm. apart, with a fixation mark half way between. One of these was filled with red colored glass and the other with white ground glass. Behind these was an oblong shutter hung vertically by a pivot at the top and allowed to swing in pendulum fashion past the two openings in the screen. Two openings were made in the shutter to match those in the screen, and each could be closed by a light-proof valve so that when a light was placed behind the screen, either one, two, or no light impression could be given through the screen openings when the shutter was made to swing past them. Electrical attachments were arranged so that a Hipp-chronoscope was started when the light impressions were given, and stopped by the reaction of the subject. Also at the time at which the chronoscope started a spark from an induction coil could be given to the wrist of the hand to react, by means of dry electrodes. A fourth stimulation could be given by an electric sounder also controlled by the electrical attachments of the shutter, while a fifth, a tactual, was given mechanically. This consisted of drawing a knotted cord through the left hand of the subject, which held it lightly but firmly. The setting of a trigger attached at one end of the cord stretched a heavy rubber band attached to the other end. The movement of the shutter, if desired, released the trigger and allowed the contraction of the band to give the stimulus, by drawing the knotted cord smartly through the hand.

By means of this apparatus any one or any combination of impressions from one to five could be given simultaneously to the subject; these being (1) a stimulus of red light, (2) a stimulus of white light, (3) a click from the electric sounder, (4) an

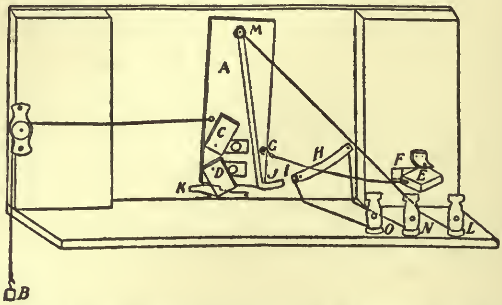

FIG. 2. This gives a diagrammatic view of the rear of the screen. An electric light, suitably shaded, was fixed in the middle. The shutter *A* is made to swing at a constant rate by means of the weight *B*. The valves *C* and *D*, which are carried by the shutter, are drawn back so that in the present position light is allowed to pass through both the oblong openings of the shutter and the round glass windows in the screen. *E* is the trigger for giving the tactual stimulus. When drawn back to the right the cord, which passes through the opening *F* and under tension from the rubber band to which it is attached, keeps *E* in a state of equilibrium, but which is destroyed when the shutter moves, provided the attachment is made at *G*. This allows the cord to pass through *F* about an inch and also through the subject's hand on the other side. *H* and *I* are brass plates over which the brass spring *J* passes to make electrical connections. When the shutter is drawn back and secured by the spring catch *K*, a current flows from the binding-post *L* by way of *M* to *N*. The chronoscope was placed in this circuit. Both the circuit for the electric shock and that for the sounder went through *L* to *O*. *J* pressed flatly against *H* and *I* over a space about 1 cm. long, so that the current was made through *I* before it was broken through *H*. This was to allow time for the overcoming of the inertia of the sounder before the chronoscope started. The light stimuli and also the tactual were given when the chronoscope started, but the electric shock was not given until contact was broken between *I* and *J*. This served to delay this stimulus slightly in order to make the reaction to it more clearly coincide with the others, which would otherwise be prevented on account of its well-known rapidity.

electric stimulus applied to the wrist of the reacting hand, and (5) a tactual stimulus applied to the other hand. The screen was placed 70 cm. from the subject. The dark room was used. As a test of the degree of simultaneity with which the impressions were given, a series of ten simple sensorial reactions to each of the stimuli was taken from each of the five subjects employed in the experiment. In the following table *S* stands for the sound stimulus; *T*, for the tactual; *R*, for that of the red light; *E*, for the electric stimulus; and *W*, for the white light. The numbers are averages of ten trials from each of the different subjects and are for thousandths of seconds. From the final averages we see that the longest time, that for sound, was but

TABLE X.

Subject.	*S*	*T*	*R*	*E*	*W*
D.	182	169	195	170	167
H.	190	167	165	156	153
Mea.	167	187	177	163	155
Rog.	159	157	143	163	155
R.	204	169	188	182	197
Average.	180	170	174	167	165

15σ more than for the shortest, that for the white light. So far as possible it was tried to overcome, by the structure of the apparatus, the difference of time incident to the use of the different senses, so that when several stimuli were given the time for reaction would be coincident for all.

As there were five different stimuli which could be given and combined as desired in the reactions to these disparate stimuli, we may see that there were five alternatives for the single impressions, ten possible combinations for the double impressions, ten for the triple impressions, five for the quadruple, and one for the quintuple. All the possible variations for each of the five series were used an equal number of times. The five-finger key was used as in the reactions to the groups of from one to five similar objects, and the same subjects were employed, with the exception of *Mea*. Each, therefore, had had a long experience in choice-reactions. The same number of hours per week and the same days were also used here. Since practice seemed an important element of the problem, two hundred reactions were taken from each subject, instead of one hundred as in the experiment with the similar objects. This made forty for each in each of the five series. All five series were kept along together to avoid different degrees of practice for each, and also to prevent the subject from knowing at any time to how many impressions he was to react. In general, the same methods and precautions were used here as in the experiment with similar objects. It will be remembered that one subject in that experiment had difficulty at first in combining all the objects for making correct reactions. In the present experiment with disparate impressions, all of the subjects had this difficulty, and this made necessary a period of practice extending over three weeks before all could react with precision to the groups of four or five impressions, although reactions to the smaller numbers were possible from the first. Even after records began to be taken, a large number of errors was constantly made. During the second week but about one third of the reactions could be recorded, but marked improvement had taken place for most of the subjects by the third week, although even much later than this *Rog.*, who had difficulty before, would have days when not more than a fifth of his reactions were made correctly. Frequently on stormy days, or when a storm threatened, nearly all of the subjects would relapse to an early stage of practice, as shown by the frequent errors. These difficulties made the work proceed so slowly that while it took but three weeks to get one hundred reactions from each subject in the last experiment, here it took eleven weeks to get two hundred.

In addition to errors of discrimination and of choosing the right finger for reaction, which are more or less common to all choice-reactions, those which were peculiar to this experiment were of two kinds. One was caused by the inability to retain the larger number of stimuli in the mental after-image at once. Two or three could be held with comparative ease until perception took place, but the fourth or fifth would sometimes crowd out one already grasped, and this would make an error in the number retained and reacted to. The other kind of error arose from the distinct succession experienced in the maturing of the stimuli in consciousness, which was much more marked than with stimuli of the same kind. The reaction would sometimes be given before sufficient time for this maturing had elapsed, and would hence be for the wrong number. The order in which the impressions matured was not fixed. If for any accidental reason one or more were unusually strong, they would tend to come in first; and if weak, would come last. The habit in the order when all were given was also likely to change from time to time, but the most common order was: first and second, the visual stimuli at nearly the same time; third, the electric; fourth, the tactual, and fifth the auditory stimulus. The averages in Table X. suggest that this order was influenced by the real order of precedence in receiving the stimuli, the two visual stimuli naturally holding together, when all were given, on account of their related degree of similarity. The subjects, however, did not tend to ascribe it to this.

In preparing for the reaction the subjects were not successful in holding the different stimuli by anticipation in mind at the same time, although it was found that rehearsing them serially distinctly assisted in apprehending them and reacting promptly after they were given. When first given there was a tendency to dwell on the qualities of the impressions, then the process consisted of giving a name and counting them as they matured, before reacting. At a very early stage as the experiment progressed conscious counting disappeared for five impressions, although it would return occasionally. In place of counting, the number appeared as a definite total and reaction took place to a feeling of 'all' or 'many.' No doubt this was assisted by the fact that this number always had the same character and could be easily remembered, whereas the other multiple, as also the single impressions, had five or ten forms in which they appeared. For the combination of four impressions a method of subtraction came after a time to be used, in which the total would be arrived at by subtracting the lacking impression from the total remembered when all five had been given. Here the remembering of the quintuple impression and the subtraction of one from it would be a shorter mental process than the counting of four impressions in succession. Both of these methods decreased the time, especially that used for the five. Subject R. also used subtraction to an extent for the triple impression. This is a comment upon a feature of his record which is not shared by the others, viz., that the average time of perceiving the triple impression for the second hundred reactions was less than that for perceiving the double one.

Table XI. gives the result of the experiment. The first line of numbers with each subject gives the averages and mean variations of the first twenty reactions, in thousandths of seconds, for each group, single, double, triple, etc., or finger which reacted to it. The second line gives the corresponding numbers for the second twenty for each finger. The third line gives the average time and mean variation for choice-reaction to the Arabic figures 1 to 5.

TABLE XI.

Subj.	Finger Used.	1st.		2d.		3d.		4th.		5th.	
		Av.	M. V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
	1–5 disp. imp.										
	1st 100	796	195	1006	197	1140	226	1364	321	1292	334
	2d 100	695	137	925	85	1097	182	1265	144	1201	164
D.	1–5 figures.	439	36	469	46	511	36	539	37	516	29
	Difference.										
	1st 100	357	159	537	151	629	190	825	284	776	305
	2d 100	256	101	456	39	586	146	726	107	685	135
	1–5 disp. imp.										
	1st 100	747	170	977	226	1047	259	1244	240	1021	233
	2d 100	698	97	1056	172	1226	227	1218	272	1148	180
H.	1–5 figures.	441	27	425	22	413	44	384	45	462	46
	Difference.										
	1st 100	306	143	552	204	634	215	860	195	559	187
	2d 100	257	70	631	150	813	183	834	227	686	134
	1–5 disp. imp.										
	1st 100	828	175	1239	179	1432	262	1512	228	1257	343
	2d 100	665	104	1111	255	1417	223	1141	257	721	153
Mea.	1–5 figures.	451	30	510	38	575	63	571	53	509	23
	Difference.										
	1st 100	377	145	729	141	857	199	941	175	748	320
	2d 100	214	74	601	217	842	160	570	204	212	130
	1–5 disp. imp.										
	1st 100	822	225	992	152	1149	210	1303	318	1461	399
	2d 100	636	103	868	149	944	184	1153	300	932	128
Rog.	1–5 objects.	412	31	436	28	443	46	443	29	440	28
	Difference.										
	1st 100	410	194	556	124	706	164	860	289	1021	371
	2d 100	224	72	432	121	501	138	710	271	492	100
	1–5 disp. imp.										
	1st 100	698	195	921	139	1029	206	1041	133	968	209
	2d 100	773	141	920	142	932	136	938	124	872	132
R.	1–5 figures.	537	30	669	42	687	57	775	53	682	71
	Difference.										
	1st 100	161	165	252	97	342	149	266	80	286	138
	2d 100	236	111	251	100	245	79	163	71	190	61
Average of difference											
	1st 100	322	161	525	143	634	183	750	205	678	264
	2d 100	237	86	474	125	597	141	601	176	453	112

This experiment of reacting to the Arabic figures was conducted in the same way as that recorded in Table IX., except that here the averages are of twenty instead of ten as there. This experiment immediately followed the one just described. The numbers for subject R. are taken from the similar previous experiment of this subject, the averages of which are given in Table IX., as it was impossible to have him for the longer and later experiment. The fourth line of numbers under each subject in Table XI. gives the difference between the time for reacting to the figures and to the first twenty of the disparate stimuli for the same finger. The fifth line gives this difference for the second twenty. At the bottom of the table is given the average difference for all the subjects for the first and also the second twenty reactions for each finger. These were kept separate throughout the table in order to show the effect of practice on the second as compared with the first hundred reactions.

From the average differences for the first hundred we see that it took 322 thousandths of a second longer to perceive one of the five disparate impressions than it did to perceive the figure 1. For the double impressions this time was 525 thousandths more than for the figure 2; for the triple, 634 more than for the figure 3; for the quadruple, 750 more than for the figure 4, and for the quintuple, 678 more than for the figure 5. We thus have a very marked increase in the time needed for perception as the number of disparate impressions increases, with the exception of the quintuple series, where the time is greater than that for the triple but less than that for the quadruple series. This decrease is sufficiently explained by the introspective experiences already given, which showed that this number was frequently reacted to as a total. The lack of a settled method in reacting to this explains why the mean variation is here the largest.

The average differences for the second hundred show that practice produced a decrease in the time required for perception. The mean variations are also in closer accord with the averages, which shows a greater regularity of method. This decrease is shared by all the series, but more pronounced with the quadruple and quintuple than with the others. A record kept of the reactions as to a total in the latter and of subtractions and reactions as to a total in the former shows these to have been much more frequent in the second hundred than in the first. The effect of practice upon the mental process in first receiving the stimuli was to decrease the tendency to dwell upon their qualities and to cause a diminution of their discreteness. This was accompanied by a decreased prominence of counting in determining the number. This first appeared with the quintuple impressions where a less likelihood of error and a greater familiarity was felt, but later became general. The number of

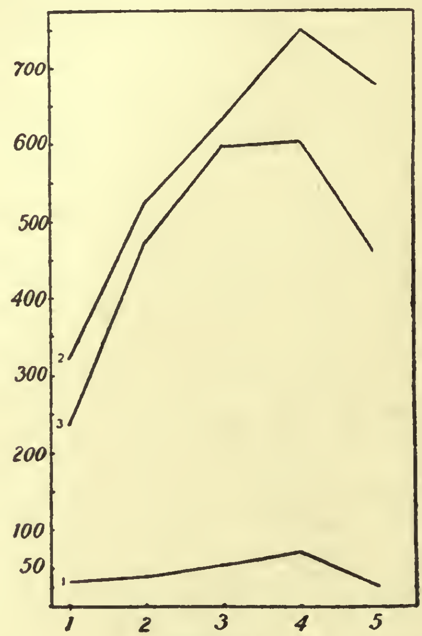

FIG. 3.

stimuli thus came to be associated directly with the right finger for reaction without the image of the name of the numeral coming in between. One subject, *H.*, became finally convinced that it was this rather than a reaction to a total which really occurred for the quintuple impression. Yet for this subject the counting came to be more distinct for this series during the second hundred, and it is of interest to note that this is the only subject for whom practice did not generally decrease the time of this series in the second hundred as compared with the first.

Even here, however, the time for the quintuple is less than that for the quadruple series.

In Fig. 3, the curve marked *1* gives the result of the experiment with similar (visual) impressions in which the tachistoscope was used. The curve marked *2* is made from the first hundred of the disparate impressions, and the one marked *3* from the second hundred. The figures on the left mark gradations in thousandths of seconds, and those at the bottom the number of stimuli given at once. Curve No. 1 (based on Table IX.) begins with 33 σ, rises gradually to 70 σ for the quadruple impression, and sinks to 28 σ for the quintuple. Curve No. 2 (Table XI.) begins at 322 σ, rises abruptly to 750 σ for the quadruple impression, and sinks with the 'total' reactions to 678 σ for

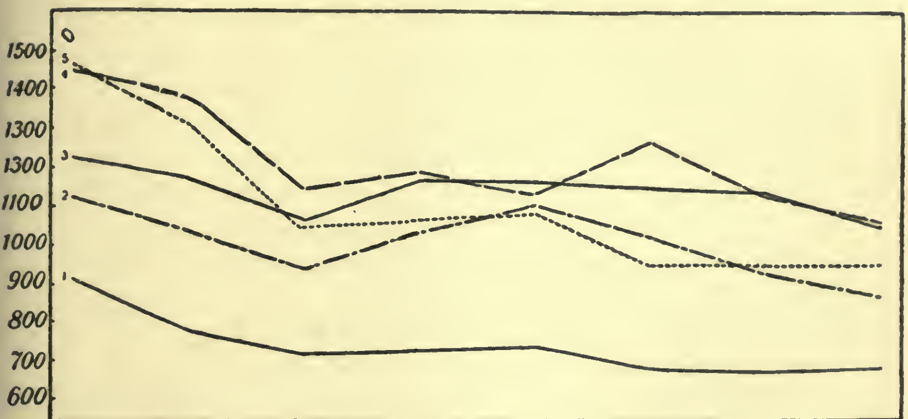

FIG. 4.

the quintuple. Curve No. 3 begins at 237 σ, rises abruptly to 597 σ for the triple impression, then very slowly with the increased method of subtraction for the quadruple to 601 σ, and sinks with the 'total' reactions to 453 σ. The difference of time for the curves at starting, and also the rate of increase of the similar as compared with the disparate stimuli as the number of impressions increased are here shown in a striking way.

Fig. 4 shows the effect of practice on the different series of the disparate impressions. The numbers at the left indicate a gradation in thousandths of seconds. Each curve indicates a series and is marked with the number of simultaneous stimuli which characterized it. Each point in the curves was determined by averaging groups of twenty-five reaction times, divided equally between the five subjects. The first point in each curve thus came from the first twenty-five, the second point from the second twenty-five in its series, and so on. A variety of ways is used in drawing the curves in the use of dotted and broken lines to make them the more easily distinguished. Until the third point, or the fifteenth reaction for each subject, the effect of practice is very marked in all the series. This is followed by an increase of time most evident in the double and triple series, and then by a general decrease to the end. The effect of practice is most marked with the quadruple and quintuple series. Both curves fall precipitously to the fifteenth reaction, the fifth curve crossing both the fourth and third, and later crossing and recrossing the second. The fourth follows the third in a similar way. If the fourth and fifth series could have been reacted to from the time practice first began, the curves which represent them would probably have begun in the neighborhood of two or three thousand sigma. By subtracting the average which determines the last point of each curve from that for the first point, we get the net decrease of time which practice caused in each series. For the first curve this amounts to 226 σ; for the second, 259 σ; for the third, 184 σ; for the fourth, 402 σ; and for the fifth, 521 σ. This result is in accord with the effect of practice observed with the counting of disparate impressions in an earlier experiment.

The argument against the distribution of attention, as supported by the reaction times to different numbers of similar objects, is evidently made stronger here, where with dissimilar stimuli the time of perception increased much more rapidly in proportion to the number used. This result is in accord with the fact that simultaneous disparate stimuli cause a displacement when it is attempted to perceive them together. They do not seem to coincide as they actually do in being given, but one is displaced and seems to be advanced or retarded. This result has been common to so many students of this phenomenon, Von Tchisch, Pflaum, Gonnessiat, Exner, Wundt, etc., that it is beyond question. Exner found this displacement to amount to 44 σ in the case of similar visual impressions. In order to make tactual-visual stimuli appear to come together they had to be separated, when given in this order, by an interval of 53 σ, but when the visual came first, by an interval of 71 σ. Auditory and visual stimuli were displaced 60 σ when given in this order, and 160 σ when given in the inverse order.[1] It was no doubt the presence of this factor as it occurs with similar impressions which prevented the succession from being discerned in the experiment with the successive exposure of letters; and if from the results of the experiments with disparate stimuli it were possible to subtract the time of delay caused by not knowing the stimuli to which reaction would be required, there would doubtless be a reasonable degree of conformity between Exner's figures and my own here also. This displacement would naturally cause an increase in the time needed for perceiving the number of the stimuli, and thus would be a factor in causing an immediate hindrance to simultaneous perception.

A possible argument for distribution, or at least for an overlapping of the processes, for perceiving both the similar and disparate impressions may be based upon the fact that it did not take twice as long to perceive the double impressions as it did the single ones, or three times as long for the triple as the single, and so on. It might be claimed that, providing distribution is not present, each step in the increase of the complexity of the impression should be accompanied by a constant increment of time equal to that for perceiving the single impression. It should be noted, however, that all of these averages are greatly influenced by practice which caused an abbreviation of the process. We should expect the above requirement to be more nearly fulfilled by the first reactions in these experiments, and this is really the case. But even here, it should be remembered that both the experiment with disparate impressions and also those that with similar ones were preceded by a long practice series, during which the time was greatly reduced. There is, then, no need of an explanation of the overlapping, aside from

[1] Exner, S. 'Experimentellen untersuchungen der einfachsten psychischen Processe.' *Pflüger's Archiv*, XI. (1873), S. 403.

the effect of practice. The influence of practice, on the other hand, is the most readily explained by the increased rapidity of action common to all nervous processes as a result of use. The mental acts employed in perceiving these impressions are unquestionably conditioned by the functioning of nervous processes.

11. The Mental After-Image.

The fact of succession in acts of attention in the reaction experiments just described explains why a smaller number of complex than simple objects can be retained from a tachistoscopic exposure, and why the longer time for associating unlike impressions makes it more difficult to perceive these together than for similar ones. The rapid fading out of the mental after-image prevents the retaining of those objects which cannot be immediately rooted by means of the establishing of perceptive relations through apparently exclusive acts of attention. The duration of this after-image would, then, be a condition which determines the number of objects retained. What determines this duration? Its length varies for different people. In the experiment previously described, in which ten simple objects were exposed to find how many could be retained, the duration of the mental after-image was approximately got for each of the subjects. An ordinary stop-watch was used for this from which the time was taken from the exposure until the giving of the number of objects seen by the subject. The gradual fading of the image made it difficult for the subject to indicate the point at which no more objects could be recovered; but the number was given when all distinct ones had been enumerated, and no other seemed obtainable. Also the time required for the experimenter to stop the watch made the time from one to two fifths of a second too long. Table XII. gives the time in seconds

Table XII.

Subject.	D.	H.	Mea.	Mer.	R.	Rog.	All.
Av.	3.1	2.0	2.7	4.7	2.3	1.9	2.8
M. V.	0.3	0.3	0.6	0.5	0.6	0.2	0.4

as read from the watch. The averages under each subject are each from twenty determinations. If we compare this table with Table VII., in which is given the number of objects each of these subjects saw in a hundred exposures, we see that Subject *Rog.* saw the smallest number and also had the shortest after-image. *Mea.* also ranks third in both tables. But aside from these two instances the orders are not the same. Thus *D.* saw the greatest number, but comes second in the length of his image; *H.* comes next in the number seen, but fifth in length of image. This indicates the presence of some factor, other than the length of the image, which determines the number of objects perceived, probably the individual rapidity of perception. Evidently it was the effect of practice upon this rapidity which brought the perception of the larger number of disparate impressions within the duration of the after-image in the last experiment.

This after-image is apparently the direct product of stimulation in the same way that the ocular after-image is. Either can be disregarded if the attention is otherwise employed, both remaining a certain period to represent the stimulus that has passed. The duration of the ocular after-image depends upon the length and intensity of the stimulation. May not the mental after-image similarly be a product of these? May not the number of objects of the same degree of simplicity seen in a tachistoscopic exposure depend, for a given individual, upon these conditions of nervous excitation?

To test this, a somewhat elaborate study was made by means of the tachistoscope, in which the distinctness and duration of the exposures were varied. The time of the exposure was changed by changing the width of the opening in the shutter. There were three openings, 6.3, 3.5 and 1.1 cm. wide, respectively, and making the corresponding times for the whole exposure 42σ, 32σ and 24σ, or for each letter 23.8σ, 15.0σ and 6.6σ. There were three sets of cards, twenty in each, with the same black letters before used, eight distributed irregularly upon each card. Set *A* was of white cardboard; set *B* was 233.5/360, or about two thirds white, as tested by a color wheel; and set *C* was 77/360, or about one fifth white. Each set was employed in each of the three periods of exposure, thus making nine series for each subject; and as each of the twenty cards was exposed five times in each series, this made one hundred exposures to a series. There were four subjects who took part in the experiment, thus employing in all thirty-six hundred exposures. The technique of the experiment was conducted in all respects like that with the consecutive exposure of letters. It was found advisable, however, to work equally upon all of the nine series within each hour in order to equalize any influence which might arise from practice or the varying brightness of the daylight from day to day, since electric illumination was not used.

Each card was shown five times with each period of exposure, or fifteen times in all to each subject; but as each subject served but two hours per week in the experiment, and but twenty-seven exposures were made in an hour, the cards could not be expected to be remembered to a great extent. Subject *A* remembered two cards at the end of the experiment as having been seen before, and subject *Y* three. Identifying them as having been shown before did not, however, seem to influence the recalling of the letters. Yet there no doubt was a subconscious influence exerted to some extent; but owing to the method of the experiment this was distributed equally among the different series.

The subjective experiences were in the main similar to those of the experiment with the consecutive exposure of letters, but with an increased tendency to see more letters that could not be recalled. Two subjects found the longest exposure of the white cards somewhat dazzling, and one of these, subject *A*., saw a few more letters on the medium gray for the same time of exposure in consequence. The size of the exposed surface seemed distinctly smaller with the darkest cards and the shortest exposure, and its outline was less distinctly defined. More letters seemed to be present in the brighter exposures, and also more that could not be given. Table XIII. gives the results of the experiment. As before, the numbers give the letters seen in a hundred exposures. The averages show a gradual and almost constant decrease in the number of letters correctly seen from the white cards with the full opening to the darkest cards with the smallest opening. In the case of this last, the per cent. of

TABLE XIII.

	Subject.	Full Opening.			Medium Opening.			Small Opening.		
		A.	B.	C.	A.	B.	C.	A.	B.	C.
Number seen correctly.	A.	299	305	279	287	274	236	256	195	79
	R.	238	239	234	230	217	211	199	174	85
	S.	228	203	206	209	195	202	190	151	92
	Y.	287	277	267	266	251	246	246	182	111
	Average.	263	256	246	248	234	224	223	176	92
Number seen wrongly.	A.	42	36	42	40	31	40	20	18	31
	R.	69	67	71	70	61	74	81	66	58
	S.	61	73	79	53	53	60	44	37	43
	Y.	71	69	85	83	69	73	65	56	46
	Average.	61	61	69	62	54	62	53	44	45
Per cent. of those wrongly to those rightly seen.	Per cent.	23	24	28	25	23	28	24	25	49
Number misplaced.	A.	3	4	7	8	8	7	1	1	11
	R.	14	12	16	11	11	14	12	11	6
	S.	14	22	18	11	20	12	16	10	9
	Y.	19	18	23	13	16	15	22	4	9
	Average.	13	14	16	11	14	12	13	7	9

those wrongly to those rightly seen has a very striking increase, while the number of those simply misplaced in proportion to the whole number seen has also increased. Evidently this shows that the conditions under which this series took place greatly facilitated the combining of the component parts of letters wrongly. Yet in this series the concentration of the attention was on the whole greater than for any other series, owing to the feeling that more effort was needed to see the letters.

The following curves (Fig. 5) show where and to what extent the number of letters seen corresponded to the time of exposure and the brightness of the cards. The curves are headed at the top in the same way as Table XIII. The dotted line indicates the time of exposure of a single letter for the different series and is graduated to thousandths of seconds on the left. The broken line represents the brightness of the cards and is graduated in corresponding degrees of white as tested upon the color wheel. The solid line represents the number of letters seen in each series and is graduated according to the number of letters seen.

The beginning of the last curve at the left under *A* shows the optimum conditions to have existed where the longest exposure occurred and the brightest cards were used. Under *B* the curve has sunk in response to the decreased brightness of the cards and the consequently diminished distinctness of the letters. It continues to sink under *C* because of the still darker cards used. Under the second *A* the curve rises slightly to the maximum brightness of the cards, although the time of exposure has been decreased. Under the second *B* the curve falls again on account of the darker cards and this fall is increased for the second *C* in response to the darkest cards, although the

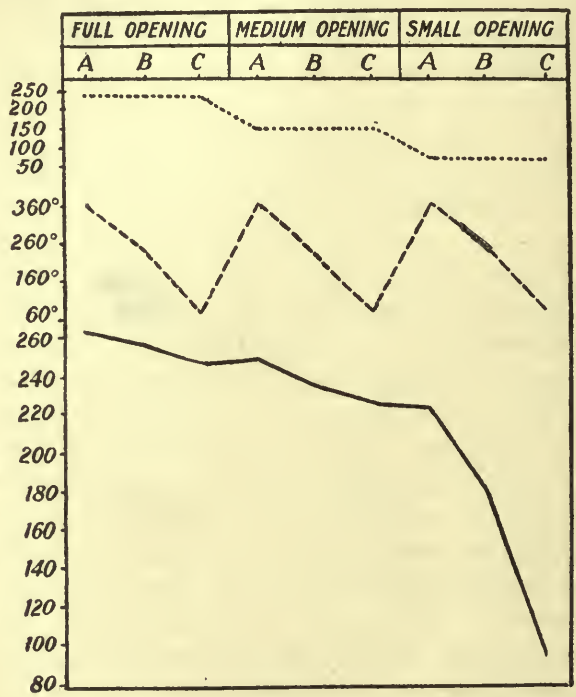

FIG. 5.

exposure has remained constant. The downward progress is again arrested under the third *A*, although not quite so markedly as for the second *A*, from the use again of the brightest cards. But from here on the fall is precipitous, due again to the decreased brightness of the cards.

This shows that while the curve responds to the time of exposure as evidenced by the general downward tendency, it yet responds even more distinctly to the differences of distinctness in the letters and this response is by far the greatest when the exposure is shortest. The curve hence appears as a function of these two conditions. It resembles in shape that illustrating Weber's Law, and like that law, shows that the effect, here the length of the image, has a large value in proportion to increments of slight stimuli and slight in proportion to those of great stimuli.

Thus judging the length of the mental after-image to be indicated by the number of exclusive acts of attention possible before it fades, we have a close analogy obtaining between the mental and ocular after-images in respect to their duration as resulting from the degree of stimulation. The experiment as a whole, and especially the small number of letters seen with the dark cards and shortest exposure, although there was an unusual effort of attention, plainly supports the conclusion already reached that the number of objects seen from a tachistoscopic exposure depends upon the duration of the after-image rather than upon any special effort to distribute the attention apart from the incidental rapidity of its consecutive acts.

It seems likely, also, that this after-image is an important factor in determining what Wundt regards as the broader limits of consciousness as tested by the power to compare two groups, one coming after the other, of consecutive metronome strokes. He gives the interval between the strokes which favors the getting of the largest number as 0.2 to 0.3 seconds; and since sixteen single strokes mark the limit this would require the retention of the first stroke of a series from 3.2 to 4.8 seconds — a period like that which we found to be equal to the duration of the after-image. Wundt also states that if the strokes are separated by more than four seconds, it is impossible to combine them in consciousness. This is no doubt explained by the fact that the after-image hardly survives this period. It is also possible that the different results obtained in this experiment by different observers are explained by the different lengths of after-image possessed by different people. Thus Bechterew gives 12, 14 and 18 for the number of single strokes that were got from one subject at three different trials in ascertaining the limits of consciousness, while Tschisch found 11 for one subject and 13 for another.[1] The differences for different subjects which we found in the lengths of after-images (1.9–4.7 sec.) would explain much greater differences of results.

12. Summary.

In the counting of simultaneous series of similar impressions the rate decreased as the number of series increased. The exceptions to this in the double and triple series of visual impressions are readily explained as the result of rhythm. In the counting of simultaneous series of disparate impressions the loss in rate was still more pronounced. These results show that in these simultaneous series with motor paths converging to the same organ of expression distribution is excluded. Experiments with divergent motor paths, which are most successful with disparate processes, require sufficient automatism to make an explanation of the results based upon distribution unnecessary.

Reactions in which the concentration and also attempted distribution of attention were compared showed, by the increase of time and mean variation in the reactions in which distribution was tried, that the attention was not distributed, but fluctuated between the possible stimuli expected.

These results led to the questioning of the interpretation of tachistoscopic experiments as given by Wundt and others. An experiment of this kind, in which the exposure was short, and the objects were presented in succession, showed that although the subjects' experiences were typical of such experiments, the impression was not made conscious until after the exposure was over: so that distribution, if it took place, was not, as has been claimed, coincident with the exposure. Since distribution is not possible during this subconscious period of inertia, the question came as to its presence during the conscious period which followed. To answer this, the experiment was conducted in which the subject reacted differently to different numbers of simple similar objects from one to five. By subtracting from the time thus obtained the time required for reacting in a similar way to figures 1 to 5, the time necessary for perceiving the ob-

[1] See review of article in *Neurologisches Centralblatt*, VIII. (1889), S. 272.

jects over that for perceiving the figures was obtained. This time increased in proportion to the number of objects except in the case of five, which is readily explained by special devices for its quick recognition. Consecutive acts of attention rather than distribution were thus shown to characterize the perceptive process. Since it takes longer to perceive complex objects than it does simple ones, this successive process explains why fewer complex than simple objects can be got from a tachistoscopic exposure. These results were confirmed and made more apparent by the reactions to simultaneous disparate impressions where the time of perceiving the number was greatly increased.

According to introspective experience the limit to the number of objects which could thus be got was fixed by the duration of the mental after-image. The experiment in which the duration and distinctness of the visual impression were varied showed that the length of the image was distinctly controlled by these features of the impression. So that the number of objects got from an exposure depends upon physiological conditions rather than upon a specialized form of mental activity.

In the same way that in learning to read, originally separate objects come to have close perceptive relations, so practice tends to unite into a closer perceptive unity impressions at first combined with difficulty. This was shown in the counting of disparate impressions, in reacting to different screen openings between which it was tried to distribute the attention, and in the reactions to different numbers of similar and also of disparate impressions. As devices for obtaining this unity may be mentioned the rhythm in the counting experiments, where each impression thus came to have a special relation to other impressions, and the grouping which accomplished the same end with simple objects in the tachistoscopic experiments. In the latter experiment some subjects noticed a decreased tendency to group as the experiment progressed, which suggests that an acquaintance with the cards was a substitute for this. This close perceptive or associative relation, which results from practice in combining discrete impressions, accounts for the reënforcement of closely related impressions, as shown by the large number of letters and figures that can be seen, from one exposure, when they enter into intelligible relations to form words and numbers as compared with arrangements in which this is not possible.

13. Conclusion.

These results seem to support the following conclusions:

Those things which we perceive as single objects are composed psychologically of a group of elements which in many cases were primarily discrete objects of attention, *e. g.*, the color, elements of form, size, and location involved in a letter of the alphabet may each be separate objects of attention, and were obviously so until united into an apparent unity by practice. Those elements which are habitually found together in objects of perception become so closely associated mentally that we are not conscious in recognizing them of the steps which bring them together, because of the decrease of association time. Evidence of the steps which still pertain to apperceptive groups of this kind can be had only by the most exact time measurements, as shown by experiments with words, numbers and groups of similar objects. The increased association time for unlike impressions makes the steps by which they are united more apparent. The conscious difference, therefore, between one object and more than one is a difference in the closeness of association, and is hence one of degree rather than one of kind. Distribution, then, can take place only when the association time has so decreased that succession has disappeared. But at this point the conscious plurality has become a conscious unity, the elements of which are not perceived as such. Hence distribution also does not occur here. Simultaneous distribution is thus seen to be a psychological impossibility.

The duration of the mental after-image easily explains the phenomena that have been ascribed to distribution in tachistoscopic experiments. This, with the subconscious and conscious relating of the elements of a single object, and also of separate objects into quickly perceived complexes, serves to do for perception what the coördination of many nervous discharges does for the production of effective muscular movements. Practice comes to be the great factor in the organic development of mind, since it brings a constantly increasing number of elements within the grasp of the after-image for immediate perception through the shortening and dropping out of the conscious links which are at first necessary to unite them. We know in general that when two portions of the cortex are stimulated quite or nearly simultaneously, they form mutually reënforcing relations so that afterwards when one is stimulated, the other is excited also. This is illustrated in associative memory where one idea calls up another when it has occurred immediately before or after it. The duration of the mental after-image seems to mark the period of cortical excitation, during which a new excitation may enter into functional relations with a former one, but after the subsidence of which, excitations no longer are able to produce this result. We may thus undoubtedly experience simultaneously excited ideational centers in the cortex, but we have seen that this does not bring about simultaneous consciousness of the corresponding ideas. We may, perhaps, picture consciousness as traveling along the association fibers from one center to another, the path adopted being determined by the degree of intensity of nervous excitation. The distance and degree of resistance between some of these, as in the case of unusual relations of different sensory areas, causes the transition to be sufficiently slow to be clearly noticed; but in other cases, where the centers are close together, or where frequent use has decreased the resistance, the transitive stage is of much less duration, and may become so short as to make the impressions appear to have completely fused, introspectively, and to have reduced the actual time to five or ten sigma.

SOME POINTS OF DIFFERENCE CONCERNING THE THEORY OF MUSIC.

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Mr. Dixon's excellent criticism in *Mind*[1] of my theory of music[2] suggested to me to write the following pages, which, I hope, will make some details of my theory clearer than I was able to make them in the publication which he reviewed. I shall further have to refer to an article of Lipps[3] and an earlier one of my own[4] and therefore mention them here at once.

Mr. Dixon states that according to my experience melodic relationship does not exist between two tones the ratio of whose vibrations involves primes higher than 7. The latter part of this statement does not quite agree with what I wished to say. One must not infer that there is relationship whenever the primes involved are lower than 11; I do not believe to experience, *e. g.*, relationship with 7–9, or 7–15. I am extremely glad, however, for his use of the word experience. My theory, indeed, is not intended to be a mere dialectic, but the systematized expression of my experience. I regret as much as the reviewer that I have not tried my experiments on a large number of unbiased observers. This is not caused by any belief in my own self-sufficiency, but by circumstances over which I have no control. To find unbiased observers for experiments of this kind is much more difficult than it seems to be. Further, the methodical difficulties of such experiments are extraordinarily great; and to make the experiments in such a way as they ought to be made requires an amount of time of which those

[1] *Mind*, New Series, 44, Oct., 1902, pp. 567–571.

[2] Max Meyer, 'Contributions to a Psychological Theory of Music,' *University of Missouri Studies*, I., 1, 1901, pp. 1–80.

[3] Th. Lipps, 'Zur Theorie der Melodie,' *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, Bd. 27, 1901, pp. 225–263.

[4] Max Meyer, 'Elements of a Psychological Theory of Melody,' PSYCHOLOGICAL REVIEW, VII. (3), 1900, pp. 241–273.

have no conception who never attempted it. The chief cause why I did not furnish my theory with a greater material of data is the well-known fact that a college professor has plenty of other duties besides making experiments. Therefore the experience referred to is mostly my own experience; but this does not mean that it is only a casual observation. I have become convinced of the value of my theory, and I regard this a sufficient reason for publishing it. Nevertheless I may be wrong in many respects, and I shall be thankful for any objections brought forward in so truly a scientific spirit as the reviewer's.

The melodic relationships which I believe to experience are expressed by the symbols: 2–2, 2–3, 2–5, 3–5, 3–7, 2–7, 2–9, 2–15, 5–9 and 5–7. No account is taken of 2 as a factor. This is of the utmost importance, as it simplifies the theory immensely. It is interesting to me to see that a mathematically trained mind like Mr. Dixon's does not object to this simplification, whereas another reviewer called this omission of all factors which are pure powers of 2 unwarranted, without being aware, obviously, of the fact that musicians for centuries have done what amounts practically to the same in naming all tones by the same letter which differ by multiples of an octave. This fact, with which he was familiar since his earliest youth, seemed to him quite natural; but my omission of the powers of 2 he could not grasp and therefore rejected it.

It seems to me that I have been greatly misunderstood with respect to my psychological definition of a 'tonic.' This tonic is not identical with what musicians call a 'key-note.' The word key-note, as I would like to use it, refers only to the system of musical notation which we desire to employ in order to write down a certain tune; to the sharps and flats which we use. It has no psychological significance — *i. e.*, in my writing. I expected to make this clear by not usually employing the common musical notation. If we use the common musical notation, if we distinguish a tone by calling it key-note, we should of course select as a key-note a tone which is psychologically emphasized in the melody above the others. But since this emphasis may be based on very different psychological facts I shall not here introduce 'key-note' as a scientific term. A scientific term must have a distinct meaning, and not more than one meaning. My definition of a 'tonic' is correctly quoted by Mr. Dixon: It is a pure power of 2 when combined with melodically related tones, *i. e.*, with 3, 5, 7, 9 or 15. Such a combination of successive tones we wish to have end on no other tone but the power of 2. I do not mean that, physically, one *cannot* close on one of the other tones; of course one may, but he produces then an æsthetic effect that is hardly ever desired. Further, I do not mean that, whenever we are satisfied by a certain tune ending on a certain note, this note must be a tonic as above defined. It may be a tonic only relative to some of the tones contained in the whole melody; not relative to others, not to the melody as a whole. Indeed, it may not be a tonic even relative to a single other tone, and yet we may be satisfied by the ending, perhaps for no other reason but that we know that this tune ends on this tone, and if it did not, it would not be the tune we expect to hear. This multiplicity of the causes of a certain æsthetic experience is the greatest of all the obstacles to experimental research in this field. This will do away with Mr. Dixon's objection to my theory that it necessitates the use in a complex piece of music of very large numbers to represent the tones. "If, for example, a melody, in ordinary language, modulates to the subdominant key, Professor Meyer would say: 21 becomes a partial tonic. The subdominant of the new key would then be represented by 441, which contains the square of 7, and it is not given in Professor Meyer's scale." Herein I cannot agree with the reviewer. I omitted the square of 7 in the complete scale because, after a good deal of experimental work, I became convinced, and am convinced, that the square of 7 is never used in actual music. I should *not* say that 21 becomes a partial tonic, since I should not represent this subdominant by 21, nor the key-note in this case by 2. On the contrary, I should say that this piece of music, as a whole, has no tonic. What this means I hope to make clearer by the following.

I define the complete musical scale as the infinite series of all products of the powers of 2, 3, 5 and 7; because within this scale is to be found any melody imaginable. Of course, we cannot write down a complete infinite series. Let us here write down the series up to 405. The reader will see that for the present purpose this is sufficient. Let us also omit all those numbers which are derived from smaller ones by multiplication with a power of 2. We do not need them here. We then have the following series:

2, 3, 5, 7, 9, 15, 21, 25, 27, 35, 45, 49, 63, 75, 81, 105, 125, 135, 175, 189, 225, 243, 315, 375, 405.

It seems to be a simple fact that the æsthetic effect of a melody depends on both these conditions: variety and closeness of relationship. (I do not mention here the tonic effect, since this may be absent.) But Lipps, Dixon and others add a further condition for a succession of tones to be a melody. Mr. Dixon says: "To apprehend a succession of tones as a melody is, psychologically, to apprehend relationships which imply the relation of each note to a tonic, just as on the physical side to represent the notes by numbers having simple numerical ratios is to imply a number which is the greatest common measure of all." I must say, on empirical grounds, that this seems to me an *a priori* assumption. I do not find it necessary that in a melody all tones must be related to a certain single tone, but merely that *each tone must be related to some other tone, and any group of tones to some other group, temporally near enough, so that the relationship can take effect.* Neither can I admit that the greatest common measure has anything to do with musical theory; else, why should there be relationship between the tones 15 and 16 and no relationship between the tones 10 and 11? To speak of the greatest common measure means to go beyond the facts actually observed. This is one of the reasons why I used as symbol for the power of 2 the figure 2 and not 1; I anticipated that the figure 1 would falsely suggest something like the greatest common measure.

The conditions of melodic effect, variety and closeness of relationship are to some extent opposed to each other. If we use for a melody six different tones in intervals of octaves, there is the greatest closeness of relationship, but no variety; there is only a single relationship in the melody, namely, 2–2.

If we play the tones successively as we find them in the complete scale, there is enough variety of relationship, but hardly any closeness to speak of. *Actual music, therefore, will be a compromise of these two conditions.* This consideration can help us to derive from the complete scale smaller scales, which are more easily applicable to special pieces of music.

Let us determine the greatest number of tones which are all mutually related. Here, however, we may either make use or not of the particular effect of a power of 2 when combined with a related tone, the tonic effect, as I have called it. Let us first use this effect in the strongest possible manner, and then try the other way of avoiding this effect as far as this is possible.

Which, then, is the greatest number of tones, including 2, which are all mutually related? I must suppose, of course, that the reader will always keep in mind those relationships which I experimentally determined: 2–2, 2–3, 2–5, 3–5; 2–7, 3–7, 2–9; 2–15, 5–7, 5–9. Now, if we look over the complete scale as we find it above, we have to accept 2, 3 and 5. But we cannot accept all of the next three tones, 7, 9 and 15, because 7 is not related to either 9 or 15. We therefore have to choose between 7 on the one side, 9 and 15 on the other. Since the addition of two tones makes the number of tones greater than the addition of one, the above condition compels us to accept 9 and 15 and to reject 7. We then have the five tones 2, 3, 5, 9, 15, which are all mutually related. No further tone of the complete scale is related to all of these five.

We agreed to make the tonic effect as strong in these scales as possible. The best method of introducing further tones is then, obviously, to use as a secondary tonic the tone which is most closely related to the tonic. Of the four tones 3, 5, 9 and 15 the tone 3 has by far the closest relationship to 2. If we make 3 a secondary tonic and use relationships of the first degree only, 9 and 15 offer themselves. But they are already in the scale and need not be added. If we use relationships of the second degree we have to add 21 and 27. Our scale is then the following:

2, 3, 5, 9, 15, 21, 27.

If we use also relationships of the third degree, we have to add 45. Our scale is then this:

2, 3, 5, 9, 15, 21, 27, 45.

He who desires may add further tones by making another of the original tones a secondary tonic. If he does so, he will become able to have within the whole melody a greater number and a greater variety of partial melodies, each containing a secondary tonic. According to my experience, however, music with a primary tonic is hardly ever very complicated, so that the addition of further tones to the scale is not of much practical significance. The table below permits us to compare the different scales. That 2 has been given the musical name F is quite arbitrary.

Scales with a Primary Tonic.

F	F#	G	G#	A	A#	B	C	C#	D	D#	E	F
2	—	9	—	5	—	—	3	—	—	—	15	2
2	—	9	—	5	21	—	3	—	27	—	15	2
2	—	9	—	5	21	45	3	—	27	—	15	2

and so on.

Our second intention was to determine the greatest number of tones mutually related, while neglecting as much as possible the peculiar effect of a tonic. We therefore omit 2 entirely and accept three and five. 7 has to be omitted again, because its acceptance would necessitate the omission of the *two* tones 9 and 15, with which 7 is not related. We have further to add 45, this being related to 3, 5, 9 and 15. No further tone of the complete scale is related to all of these five:

3, 5, 9, 15, 45.

If we desire to introduce further tones and yet have the closest relationship possible, we must find out to how many of the above five tones each further tone is related and then select the one or those which show the greatest number of relationships. Below we see the tones of the complete scale which are left, each one marked with as many stars as it shows relationships. *E. g.*, 7 is related to 3 and 5, but not to 9, 15, 45; therefore it has received two stars. And so on.

7	21	25	27	35	49	63	75	81	105	125	135	175	189	225	243	315	375	405
*	*	*	*	*	–	*	*	*	*	–	*	–	–	*	–	*	–	*
*	*	*	*	*		*	*	*			*							
	*	*	*				*				*							
			*															

Only 27 has four stars; all the rest have three or less. We therefore add 27 to our scale, which now is the following:

3, 5, 9, 15, 27, 45.

The remaining tones of the complete scale are again marked with as many stars as they show relationships with the tones of this last scale.

7	21	25	35	49	63	75	81	105	125	135	175	189	225	243	315	375	405
*	*	*	*	_	*	*	*	*	_	*	_	*	*	*	*	_	*
*	*	*	*		*	*	*			*							*
	*	*			*	*	*			*							
										*							

135 has in this case four relationships; no other tone an equal number. We therefore add 135 and have the scale:

3, 5, 9, 15, 27, 45, 135.

The remaining tones are the following:

7	21	25	35	49	63	75	81	105	125	175	189	225	243	315	375	405
*	*	*	*	_	*	*	*	*	_	_	*	*	*	*	_	*
*	*	*	*		*	*	*				*			*		*
	*	*			*	*	*									*
						*	*									

75 and 81 have in this case more relationships than the rest. We therefore add 75 and 81:

3, 5, 9, 15, 27, 45, 75, 81, 135.

The remaining tones are:

7	21	25	35	49	63	105	125	175	189	225	243	315	375	405
*	*	*	*	_	*	*	*	*	*	*	*	*	*	*
*	*	*	*		*	*			*	*	*	*		*
	*	*			*				*					*
		*												*

25 and 405 have more relationships than the rest and are added to the above scale:

3, 5, 9, 15, 25, 27, 45, 75, 81, 135, 405.

The remaining tones are:

7	21	35	49	63	105	125	175	189	225	243	315	375
*	*	*	_	*	*	*	*	*	*	*	*	*
*	*	*		*	*	*	*	*	*	*	*	*
	*	*		*				*	*	*		

21, 35, 63, 189, 225 and 243 have three relationships each and are added to the last scale:

3, 5, 9, 15, 21, 25, 27, 35, 45, 63, 75, 81, 135, 189, 225, 243, 405.

If one wants to continue this, he must take into consideration a greater part of the complete scale than we did. We stopped at 405. But a greater extension of these scales is of little practical significance. The table below permits us to compare these scales.

SCALES WITHOUT A PRIMARY TONIC.

G	G♯	A	A♯	B	C	C♯	D	D♯	E	F	F♯	G
9	—	5	—	45	3	—	—	—	15	—	—	9
9	—	5	—	45	3	—	27	—	15	—	—	9
9	—	5	—	45	3	—	27	—	15	—	135	9
9	75	$\binom{5}{81}$	—	45	3	—	27	—	15	—	135	9
9	75	$\binom{5}{81}$	—	45	3	$\binom{25}{405}$	27	—	15	—	135	9
9	75	$\binom{5}{81}$	21	45	$\binom{189}{3}$	$\binom{25}{405}$	27	225	$\binom{15}{243}$	63	135	$\binom{35}{9}$

and so on.

An important fact, to be learned theoretically from the above scales, is the relative insignificance of 7, although the complete omission of 7 would prevent a truly scientific theory of music. In the scales with a primary tonic the pure 7 is absent; so is it in the scales without a primary tonic. In the latter the products containing the factor 7, *i. e.*, 21, 35, 63, 189, enter into the scales according to closeness of relationship only comparatively late, after many other tones. The square of 7 never enters into any of these scales. This agreement of my theory with the generally recognized fact of the comparative insignificance of the number 7 for musical theory proves the correctness of the principles of my theory, the correctness of my observations concerning the laws of relationship of any two tones.

In the scales with a primary tonic there can be no question as to *which tone should be the key-note; of course the tonic.* But in the scales without a primary tonic this depends or may depend on many different conditions. The tones which are most commonly used as key-notes are 9 and 5. Why these tones are used thus, I shall now try to make clear.

Let us study more closely the relationships contained in the series 3, 5, 9, 15, 27, 45, 75, 81, 135. There is no primary tonic, but several of the tones are secondary tonics. We remember that the tonic effect is particularly strong in the relationships 2–3 and 2–5. Let us therefore see which of the tones of the above series can be combined with tones of these two relationships.

3	5	9	15	27	45	75	81	135
9	15	27	45	81	135			
15		45	75	135				

We see that only 3, 9, 15 and 27 can be combined with tones of both the relationships 2–3 and 2–5. Let us for the present discussion call these four groups of three tones each by the names group 3, group 9, group 15 and group 27. We should now determine how these groups are interrelated. Each combination of two groups makes nine relationships possible. The table below shows how many of these nine are lacking.

Group	3	combined with group	15 (–2),	with g. 27 (–3).		
"	9	" " "	15 (–2),			
"	15	" " "	3 (–2),	" "	9 (–2),	with g. 27 (–3).
"	27	" " "	3 (–3),	" "	15 (–3).	

The tones of group 3 and group 9 are all mutually related. Group 3 combined with group 15 lacks two relationships, 75 not being related to either 3 or 9. This is expressed by the parenthesis (–2). Group 3 combined with group 27 lacks three relationships, 3 not being related to either 81 or 135, 15 not to 81. This is expressed by the parenthesis (–3). Group 9 combined with group 15 lacks two relationships, 75 not being related to either 9 or 27. Group 15 combined with group 27 lacks three relationships, 81 not being related to either 15 or 75, 27 not to 75. The group that is most deficient in relationships to the other groups is therefore 15; the one that lacks only two of twenty-seven relationships is the group 9. If we have to emphasize a certain tone melodically, to make it the key-note, nothing is more natural than that we choose the tonic of this group 9–27–45 as the key-note. *Therefore I have made 9 the key-note.*

The above explanation is somewhat similar to that of Lipps. The most important difference is that he needs about fifteen pages in order to explain what I explain on a single page.

Lipps insists on calling the tones 3 and 27 in the above series 'dominants.' Any scientific term should be definable; but I have not been able to discover any definition of 'dominant' which could consistently be used. On the other hand, I do not see of what advantage it is to use the word dominant. Nothing becomes clearer or simpler by using the word. If 3 and 27 are to be distinguished by special names, I do not see why 15 should go without; I propose the name of duke or prince.

It is decidedly dangerous to call 3 and 27 in the above case dominants. Musicians invariably call F and G dominants in any music written in the key of C. But we should remember that F and G in other scales, *e. g.*, the scale 2, 9, 5, 21, 3, 27, 15, 2, have psychological characteristics quite different from those we found here.

Nearly all music that is highly complex, particularly vocal music for a choir of several voices, polyphonic music like organ fugues, and whole operas, seem to be represented by scales without a primary tonic with 9 as a key-note. The scale can of course be further extended than we used it above; remaining, however, within the complete musical scale. Within the complete scale there are many possibilities of partial melodies containing a partial tonic. The greater number of arias in operas are partial melodies of this kind.

Here may be mentioned a fact which has been much discussed by the theorists. It is possible to use in a piece of music written in the key of C (9) both these chords; D-G-B (81–27–135) and D-F-A (5–3–15). This does not cause us the slightest theoretical difficulty. We see in our scales that the tone D on the piano is either 5 or 81. There is no reason why in our music we should not use now the one, now the other. But those whose adhere to the 'diatonic scale' of Zarlino-Rameau-Helmholtz do not get off so easily, since their scale contains only 81, not 5. In order to explain so simple a fact, a single chord, they have to introduce such a complicated theory as that of 'modulation.' Indeed Helmholtz says, though somewhat reluctantly, that this chord D-F-A in the key of C is 'eine beginnende Modulation über die Grenzen der C-Durtonart hinaus.' If simplicity is a requirement for a theory to be called scientific, no further criticism is necessary. I might easily mention a hundred similar cases, if it were not for the waste of paper and printer's ink.

In music without a primary tonic, besides 9, 5 is frequently used as a key-note on which a melody ends. I shall now try to answer the question why this ending on 5 is 'satisfactory.' Dixon says in his review: "There are some respects in which Professor Meyer's paper is disappointing. In particular he throws no light on what to the psychologist is one of the most interesting problems presented by music, namely, the peculiar æsthetic effect of minor melodies." It is disappointing to me to read this, as I actually thought to have thrown some light on this problem. I shall try to do so more successfully now. Let us consider the æsthetic effect of a melody made up of the tones 3, 5 and 15. 3 is a tonic relative to 15. 5 also is a tonic relative to 15. When we hear alternately these three tones, each time when 15 sounds we experience the strong desire to hear now 3; but also the desire to hear now 5. Fortunately, it is possible to hear both at the same time in a chord. But when we hear both 3 and 5, our attention is attracted by two sensations almost equally; indeed, we may say equally. The relationship 15–5 is closer than the relationship 15–3; but not very much. And while this makes the tone 5 psychologically more active than 3, there is another important factor. The relationship between 5 and 3 is very close. I have repeatedly pointed out in other writings that closeness of relationship tends to spread our attention over different tones, prevents our attention from remaining concentrated upon a single tone. (This is very important in the process of analyzing.) If one of the two tones 5 and 3 were a tonic relative to the other, our desire to return to the tonic would render that tone predominant. But this is not the case. Such an experience is something unusual; our usual experiences end in definite reactions. This wavering between two sensations has a strong emotional effect, which may be fittingly described by the German proverb: Wer die Wahl hat, hat die Qual. Our experience is highly 'unsatisfactory.' And yet it is very satisfactory in another way; just as it is satisfactory to see the sufferings of the hero in a drama.

If we regard one of the three tones of the melody as the key-note, it is natural to take either 5 or 3, not 15. We select 5, because this has the closer relationship to 15. We may of course construct a melody of more than the three tones 3, 5, and 15. But there are no other three tones in the complete scale (excluding 2) which are united by so close relationships as 3, 5 and 15. The addition of further tones does not, therefore, alter our conclusions as to *the tone which should be called the key-note, namely 5.*

	C	C#	D	D#	E	F	F#	G	G#	A	A#	B	C	
Tonic	2		9		5	21	45	3		27		15	2	Major
Atonic	9	75	$\left(\frac{5}{81}\right)$		45	3		27		15	63	135	9	Major
Atonic	5		45	3	25	27		15	63		9	75	5	Minor

The above table shows how the three types of music which we have to distinguish upon psychological grounds are related to the musical notations of music as music in major and minor keys. The three types are represented by three arbitrarily selected series of tones of which a melody of each type may be constructed. Of course, fewer tones may be used or further tones be added. The table shows that tonic and atonic music are by no means identical with major and minor music. But minor music is never tonic.

In a similar manner as by Dixon, I have been misunderstood by Lipps in his recent article quoted in the beginning of this paper. I regret that Lipps in his discussion of my theory does not sufficiently distinguish between what I mean by 'tonic' and what he means by 'Tonica.' He calls Tonica that tone on which any melody satisfactorily ends. I call tonic that tone which is a pure power of 2 relative to some related tone. Lipps implies that, whenever a melody ends on a certain tone, I call and must call this tone tonic and represent it by a pure power of 2. This idea has never entered my mind. On the contrary, I believe that the greatest error which can be made in musical theory, is to assume that there is only one cause of a satisfactory ending of a melody. That the final tone is a 'tonic' is one cause for a satisfactory ending; but I have never denied that a melody can satisfactorily end from many other causes. In my first publication concerning this matter (PSYCHOLOGICAL REVIEW) I published the intonation of a melody which ends on 9. To be sure, the psychological effect of such a melody is different from that of a melody containing and ending on a tonic. But there is not a single word in any of my publications, so far as I am aware, stating that a melody which does not contain a primary tonic (2) and, therefore, does not end on it, for this reason has no satisfactory ending, is disagreeable, is ugly. This is one of the prejudices I am fighting, that a certain æsthetic effect must have a single cause, can have no others. Our psychical processes are not so simple. Lipps then proceeds to assert that according to my theory a succession of tones that does not contain a 'tonic' cannot be a melody at all, since 'the most elementary law of my theory' is that a melody must contain a tonic and since each melody naturally has a final tone, which Lipps calls 'Tonica.' The confusion is here caused by Lipps' unwillingness to grant me the right of calling something tonic which differs from what he calls Tonica. Of course, each melody must have a Tonica, *i. e.*, a final tone. But my 'most elementary law of melodic succession' says nothing of the Tonica, the final tone of any arbitrary melody, but merely states ('Contributions,' p. 24): 'that no hearer is satisfied, if *after having heard* once or more often the tonic 2 he does not find 2 finally at the end of the melody.' I did not suspect that any one could understand this 'after having heard' otherwise than as a conditional clause.

On page 253 of his paper Lipps proves: Meyer verkennt das Wesen der Melodie. One of the premises of this conclusion is the following: Immer, wenn C 'Tonica' ist, soll, nach Meyer, F zu C im Verhältnis von 21–2, A zu C im Verhältnis 27–2 stehen. This premise is imaginary. That I do *not* represent every key-note by 2, Lipps might have noticed. *E. g.*, in Chapter VI. (4) he might have found that among the numbers representing the whole of Schubert's Heidenröslein 2 does not appear at all, in spite of the fact that the piece has a key-note, a 'Tonica,' being written on page 55 in the key of C. If Lipps should think that my interpretation of *his* term 'Tonica' is incorrect, I challenge him herewith to give a clear-cut and universally applicable definition of what he means by Tonica.

A very brief, but equally unconvincing refutation of my theory is to be found on page 234 of Lipps' article in the paragraph ending with the words: Meyer's Theorie ist also falsch. Lipps speaks of a melody made up of the tones 2, 3, 5 and 7. In order to understand the argument, it is well to multiply all the numbers with 3, so that we have the tones 3, 9, 15 and 21. The relationships are not altered by this arithmetical procedure. Lipps then rightly states that according to my theory a melody made up of the tones 3, 9, 15, 21 cannot satisfactorily end on any other tone but 3. *But*, he adds, *the melody ends most satisfactorily only on 2*. Now, I do not see how, logically, a melody made up of the tones 3, 9, 15, 21 can end on a tone which is not among those tones of which the melody is made up. To my mind the final tone of a melody is as much a part of that melody as an animal's tail is a part of that animal's body. That a melody made up of the tones 2, 3, 9, 15, 21 can satisfactorily end only on 2, is exactly what I have been preaching all the time, and I am delighted to see this confirmed by Lipps. But I do not see how this proves: Meyer's Theorie ist also falsch. The succession of the tones G-B-d-f-G-c is one which we have heard innumerable times, so that it would be most wonderful if we did not expect, after having heard the separately quite unusual succession G-B-d-f-G, to hear a final c. We should never forget that one effect may have more causes than one.

On page 260 Lipps says, with the apparent intention of contradicting me: Why should not F : C in the key of C be represented by both ratios simultaneously, 21 : 16 *and* 2 : 3? That my answer to this question would not be unconditionally in the negative, Lipps could know if he would look at the complete scale (this scale was published in order to be used thus), where he can find that, for C = 9, F is represented by both 189 and 3; so that in this respect there is perfect agreement between Lipps and me. Only I do not believe that the tone F on the piano can *simultaneously* act as 189 and 3; this seems to me in contradiction to all our general psychological experience, as I have stated already in my Contributions. But it may act now as 3, later as 189, then again as 3, and so on.

Musically trained persons, however, frequently deny that F in the key of C can ever be 21 to 2. Their theory as well as their practice have usually made them so accustomed to the chord F-A-C in the key of C, that they cannot help imagining this chord when they hear F. Save this artificially acquired habit, no experiential facts necessitate the use of the chord F-A-C in each and every music written in the key of C. One can write a most beautiful accompaniment of a song made up of the tones 2, 3, 5, 9, 15, 21, 27, C = 2, without using at all the chord F-A-C. But musical theorists rarely care for experiential facts. The chord F-A-C is 'the chord of the subdominant,' and not to use it would be irreverence shown to the musical Idol Subdominant. If one insists upon using, in the key of C, the chords C-E-G, G-B-D *and also* F-A-C, — and there are of course no physical means to prevent any one from doing it — then indeed C cannot theoretically be 2 or F 21.

In case the chord F-A-C is used, one must select the tones from the series which I have characterized above as atonic and major, the key-note C being 9. Should we use in the music the tones 9, 81, 45, 3, 27, 15, 135, but omit 5, as we may of course use any smaller number of tones than we find in any of the above series, then my theory seems to be confronted with an apparent difficulty. But the difficulty is merely one of formal logic, not a scientific one. Namely all the symbols can be divided by 3, and the resulting series is: 3, 27, 15, 2, 9, 5, 45. I called this series *atonic*, but since it contains 2, it seems to be *tonic*. Logically, this is contradictory; but scientifically, there is no contradiction. The tone 2, which is numerically a tonic, does not act psychologically as a tonic in this case, because the tonic effect is overpowered by another effect. I have shown on an earlier page that of the three groups C-E-G, G-B-D and F-A-C, or in numerical symbols 9-45-27, 27-135-81 and 3-15-9, or after division by 3 the groups 3-15-9, 9-45-27 and 2-5-3, the group C-E-G is by far more closely related to both the other two groups, than G-B-D is to the remaining two, or F-A-C to the remaining two. This closeness of relationship gives to the group C-E-G such a great psychological effect, that the tone C becomes the chief tone, and the tonic effect of F becomes insignificant. Scientifically, therefore, there is no contradiction. That the tonic effect is actually suppressed is caused by the peculiar combination of relationships in this special case, as above described.

It is a very important fact which the experimenter should never forget, that such habits as the one just mentioned make most musically trained persons perfectly incapable of acting as observers in certain cases of experimental investigation concerning intonation. They are one-sidedly trained and therefore not unbiased.

Lipps says in conclusion: A melody is an oscillation between the key-note, its fourth and its fifth. If he had said, *certain* melodies are such oscillations, I should consent. But very many are decidedly not such oscillations, but structures of quite different types.

Let me close with some general remarks concerning the method of investigation. There has been, for so many centuries, too much deduction from *a priori* principles, and too little induction from specialized experience. The deductive, as well as the inductive method, has its advantages and its disadvantages. The inductive method does not lead at once to completeness, as the deductive does, but leaves a great many questions open. So I am blamed by Lipps and others for not answering this or that question. But if the principle of deduction is wrong, the complete theory is a complete failure, whereas the theory resulting from the inductive method is right so far as it goes. I believe that the only way of finding the æsthetic laws of music, and of melody in particular, is to determine experimentally the æsthetically most effective intonation of a melody and then to analyze the melody, in order to see how the different relationships are combined so that this effect results. But when I read publications concerning musical theory, I almost invariably get the impression that the author believes in only one, more or less narrow, principle of explanation and assumes, without having made any experiment at all, that a more effective intonation is simply impossible. I believe that the basis of all experimental work along this line must be the complete musical scale as developed in my publications; not any small scale of arbitrarily selected tones. I made a number of experiments — as many as time permitted; I found that some of the melodies contained 2, some did not. As I said in my publication ('Contributions,' p. 20): 'Pitches represented by the number 2 I shall call tonics,' so I expressed my experience by saying: there are some melodies which contain a tonic, some which do not. There is not a single sentence in my publication, in which I identify a 'tonic' with what Lipps calls a 'Tonica,' or what others call a key-note, or what not. If any one assumes this identification, I cannot prevent him from doing so; but I do not wish to be responsible for the consequences. My experimenting taught me that it would be advantageous for my structural analysis of music to distinguish '2' by a special name, a scientific term; and I believed and still believe that the term 'tonic' is most appropriate. That the word is often used in a different sense, is regrettable; but what word is not used in more than one sense? It certainly is not used in more than one sense in my publication. I openly confess that I have little reverence for a terminology which when seen under the light of experiment discloses no other claim for being respected than the sacredness of old age.

DISCUSSION.

AN ILL-CONSIDERED COLOR-THEORY.

That the making of color-theories goes on apace is a most healthy sign of intellectual activity — a sign that there is a wide-spread feeling of the utter inadequacy of the theories of Helmholtz and of Hering. These are both theories which served a useful purpose in their day, as a means of holding together the vastly complicated facts of color-vision, but they are both wholly inadequate to represent our present knowledge of the subject. The theory of Ebbinghaus met certain logical requirements which must be made of any theory in a very satisfactory fashion, but it was unfortunately wholly in discord with facts discovered immediately after it was brought out, and it has now been withdrawn by its author.[1] It is much to be regretted that Professor von Oppolzer has not been any more successful in meeting the conditions of a successful theory.

The theory of color here laid down[2] may be characterized as, in the first place, a 'return to Goethe.' The author considers that Newton did not sufficiently emphasize the subjective character of color — and this in spite of the fact that Newton states in the clearest terms that color as an experienced sensation is something entirely disparate from the cause of color — a given wave-length or combination of wave-lengths — in the external world. The purely metaphysical (or non-scientific) standpoint from which the problem of color is approached in the paper before us may be inferred at once from the disquisition which meets us already on the second page on the subject of total color blindness (or, as it ought always to be called, choosing one word rather than three, achromasy). What the sensations of the achromatic really are, we are told, it is possible to infer, after frequent conversations with them, by means of the fact that color sensations for normal individuals are accompanied by certain feelings, certain

[1] In a very interesting investigation of Fechner's colours which has lately appeared in the *Journal of Psychology*, the results obtained are regarded as confirming the theory of Ebbinghaus, exactly as if that theory were still in existence. But surely an author must be conceded the right to withdraw his own theory when the occasion demands it!

[2] 'Grundzüge einer Farbentheorie,' Prof. Dr. Egon Ritter von Oppolzer, *Ztsch. f. Psychol. u. Physiol. der Sinnesorgane*, XXIX. (3), 183–203.

æsthetic effects, which we discover to be wanting in these defectives. Even after we have made out that the spectrum looks alike to the totally color blind throughout its whole extent, how can we tell that it does not look to them all red or all green or all blue (as the Helmholtz theory would require us to believe)? "Durch die Art, wie er seine Empfindungen beschreibt, was nur durch Angabe von aesthetischen Wirkungen möglich ist, erhalten wir die Gewissheit, dass er alles so sieht, wie wir, wenn wir Kreide, Schnee, Tageslichte ansehen." This is very wonderful! Is our author really so acute that he can tell, when his patient sits in front of, say pink and white ice-cream, whether the æsthetic feelings which he describes are those appertaining to the sensation pink or to the sensation white? If so, his powers of psychological insight are something marvellous, and he ought to be able to revolutionize our whole science, to lay its foundations broad and deep as they have never been laid before, if he will but bend himself to the task. And would the same principle, may we ask, enable us to pick out also the sensations which remain intact in the case of the partially color-blind? We can easily make out from what they tell us that their sensations are of two kinds only, as the entire spectrum moves before their eyes. But what two? Are there four sorts of æsthetic feelings attached to the four colors, so accurately characterizable that we can infer from our conversation with the patient that red and yellow and green all give him the sensation of yellow alone? If the thing can be so easily made out as this, our ancestors were very foolish to believe as they did for several generations (misled by the theory of Young and Helmholtz) that when the cherries and the cherry-tree leaves looked alike to these defectives, they saw them sometimes both green and sometimes both red (and that yellow was a color-sensation of so little importance that it was not necessary to mention whether they saw it at all or not). A simple examination into these æsthetic feelings, according to our author, would have settled this question correctly many years ago — in fact, upon the first detection of color blindness. What a pity that this idea did not occur to Dalton, who concerned himself much about the character of his color sensations! Even William Pole, who showed wonderful acuteness in this subject, overlooked this method. The fact that the question has been wholly set at rest by the occurrence of individuals who are achromatic and dichromatic in one eye only seems not to have attracted the notice of Professor v. Oppolzer.

To return to the contributions of Goethe to color theory — he has shown himself to be, according to our author, a wonderfully fine observer in all questions which concern 'die innere Anschauung,' as appears at once from this passage of his among others: "Die Farbe sei ein elementares Naturphänomen für den Sinn des Auges, das sich, wie die übrigen alle, durch Trennung und Gegensatz, durch Mischung und Vereinigung, durch Erhöhung und Neutralization, durch Mittheilung und Vertheilung und so weiter manifestirt, und unter diesen allgemeinen Naturformeln am besten angeschant und begriffen werden kann." This passage our author himself characterizes as 'allerdings höchst dunkël,' but he takes it as proving 'zur Genüge that Goethe regarded color as a purely psychological phenomenon, 'das auf innerer Gegensätzlichkeit beruht.' No one can have glanced at Newton's writings without seeing that he too regarded color as a psychological phenomenon; to say that it rests upon an 'innere Gegensätzlichkeit' is a statement that would not be contradicted by saying that it rests upon an inner harmony, for both are statements that are totally devoid of meaning. But it is especially upon Goethe's view that color is accompanied by specific effects 'die sich unmittelbar an das Sittliche anschliessen,' and that while certain colors 'stimmen regsam, lebhaft, strebend, andere ruhig' (why not four distinguishable temperamental qualities, as there are four colors?), 'voll und ganz rein wirkt nur die Weisserregung.' 'Hence' (to give at last the details of the author's theory) the sensation conveyed by any single rod or cone is purely white, and a color-sensation is mediated by a combination of any two or of any three rods or cones. (Apparently it makes no difference whether a group of three is composed of rods and cones mixed up together or not.) But these retinal elements are not affected directly by light — that first reaches the cells of the pigment epithelium, passing through the thin plates of the retinal elements on its way. Here it effects a chemical change in the contents of these cells, and also in the pigment crystals which penetrate into the space between the rods and cones, and the products of this change act in turn upon the nerve ends within the visual elements. The idea that color can result from a fusion of colorless sensation-elements the author says has been suggested to him by the phenomenon which we call tone-*color* in sound — surely a very wild piece of analogizing!

It is singular how many ineptitudes a single theory of color is capable of containing; the one here reported on has them on every page. Space in this journal would be ill used in setting them all out, but it may be of some interest as a study in the working of a human mind, to indicate a few of them:

(*a*) When we get a tone-color by the fusing of a fundamental tone with its overtones, the things fused together are tones which already differ in quality (namely in pitch); but the three elementary sensations whose fusion is here to produce color are all alike — namely white. Three pieces of molasses candy fused together do not produce the taste of thoroughwort, nor of anything whatever other than molasses candy. No application of Fechner's law, nor of the formula

$$H = \sqrt{x^2 + y^2 + z^2},$$

will make anything but nonsense out of an idea like this.

(*b*) That it takes the working together of three visual elements (three cones, two cones and a rod, two rods and a cone or three rods) to produce the least extended sensation of color contradicts well-known facts — the firmness of vision in the fovea corresponds very closely with the fineness of structure of the retina, as has been well confirmed lately, among others, by Schoute. Minute points of light do not give different sensations as they fall upon one or another visual element in the rod-less region of the retina; hence there can be no difference in the functions of the several cones.

(*c*) More than this, it is fundamentally wrong to assume as essential to any visual sensation-quality the union of three contiguous visual elements. The spread-out-ness of the visual elements in the retina is the physiological correlative to the subjective feeling of extension; it cannot be at the same time the substratum for visual quality. In the ear, indeed, the successive efficient elements of structure are devoted to differences of quality, but that is not the case in the eye — there objective spatial extent gives us subjective spatial feeling.

(*d*) While there is no difference in the sensation produced by each of the three members of a group of visual elements, there is supposed to be a difference in their receptivity — the thickness of the plates in the end members is supposed to be so regulated as to make them pervious now to blue rays only, now to red and now to green. But this is just as it should not be — this is an arrangement for giving lying messages from the real world. Take the case of a small surface of a non-saturated purple; if it falls upon a happily chosen group of three visual elements, it will look as it should do — as if constituted of red and blue and a small portion of green. But if it changes its position a bit, it may hit a different group of three — say two red and one blue producing element; then it would look wholly saturated, and far more red than blue. And a wrong group of three it would fall upon much more frequently than a right one. No correct and unvarying representation of nature could be obtained upon this scheme. This is, of course, an objection which applies to any three-fiber theory of color, but a three-fiber theory was long since given up by Helmholtz — hard as it seems to be for the knowledge of this fact to become widely distributed.

(*e*) A more fundamental difficulty still remains. When three fibers of a proper group are all equally affected, the sensation produced is white (including gray). A saturated blue means that a certain one of the three is strongly affected and the other two vary slightly. But when we come to the case of that same one being affected to the total exclusion of the other two, then we have again absolutely colorless sensation. But is not this pure nonsense? Was there ever a case of a theory which had been more purely non-thought-out than this?

(*f*) The beautifully fine structure of the retina in the fovea permits an exceedingly fine discrimination of parts in the visual field. But if a ray of light must go through the layer of cones and enter the pigment cells of the epithelium in order to produce there a chemical effect which has then to be reflected back into the cones, this firmness of structure would be thrown away; it is impossible that such a chemical effect should be reflected back into the single cone from which it came, and hence we should get only an enlarged and blurred image instead of a sharp one of any small point of light.

(*g*) As a fitting wind-up, we may mention that the author is guilty of a very singular lapse in a matter of elementary geometry — he thinks that if one cylinder is n times as long as another, it has a surface which is n^2 times as great. This is so curious a phenomenon that it is worth while to chronicle our author's very words. Speaking of the rods he says: *Ihre im Vergleich zu den benachbarten Zapfen mindestens um das Dreifache grössere Länge des Aussengliedes vergrössert natürlich die Oberfläche auf das Neunfache.* This is not so '*natürlich*' as Professor v. Oppolzer supposes, and such lapses are not calculated to inspire confidence in his long mathematical lucubrations. It is sad to see that the present paper is only a first installment of what is apparently to be a work of considerable length. It is a pity to waste precious pages of the *Zeitschrift* on such worthless matter, but it seems that even the acute responsible editor of that journal must sometimes nod.

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PSYCHOLOGICAL LITERATURE.

Human Personality and its Survival of Bodily Death. FREDERIC W. H. MYERS. London and New York, Longmans, Green and Co. 1903. Vol. I., pp. xlvi + 700; Vol. II., pp. xx + 660.

This posthumous work by the president of the Society for Psychical Research is called by its author a 'most imperfect text-book to a branch of research whose novelty and strangeness call urgently for some provisional systematization.' The text proper, which takes up scarcely one third of the space, is prefaced by a glossary and syllabi and is supplemented by elaborate appendices containing 'the mass of evidence already gathered together in the sixteen volumes of *Proceedings* and the nine volumes of the *Journal* of the S. P. R., in *Phantasms of the Living* and other books hereafter referred to, and in MS. collections.'

The book is put forward as an exposition rather than a proof and its line of argument is briefly an advance from the analysis of normal to the evidence for supernormal faculty ending with a discussion of the nature of the proof acquired as to the persistence of human personality after bodily death. The inquiry begins by discussing the subliminal structure, in disease or health, of those two familiar phases of human personality, ordinary waking and ordinary sleep. The next consideration is in what way the disintegration of personality by disease is met by its reintegration and purposive modification by hypnotism and self-suggestion. Dealing separately with the various groups of subliminal phenomena, the author treats of their mode of automatic manifestation and first of the sensory automatism which is the basis of hallucination, including phenomena claiming an origin outside the automatist's own mind. Finding that that origin is often to be sought in the minds of other living men, various forms of telepathy are reviewed. This telepathy is not confined to spirits still incarnate and evidence is offered that intercourse of similarly direct type can take place between discarnate and incarnate spirits.

In the introductory chapter is given this characteristic outline and justification of the work. Although it proposes to treat of the evolution of human personality, of faculties newly dawning and of a destiny greater than we know, yet there must needs be a detailed discussion of certain modes of that personality's disintegration and decay. The extreme instances of such decay, actual imbecility or insanity, lie outside the author's province. But there are many cases where there is no actual insanity, probably no organic disease of the brain, but in which there are disturbances of personality which teach us of that complex structure or synergy which it is our object to upbuild or develop. Alterations of personality and hysterical phenomena are spontaneous experiments of the most instructive type. In hysteria, a vague range of phenomena called by a meaningless name, there is a contraction or effacement of the spectrum of consciousness, which leaves the hysteric occupying much the same position relatively to ourselves as our own supraliminal consciousness occupies relatively to our whole self. The essence of hysteria is an instability of the thresholds of consciousness and of voluntary movement, insomuch that many perceptions which should be fully conscious are for the time submerged, and many actions or motor syntheses which should be subject to waking will have sunk out of that will's control. Occasionally some faculties habitually submerged may rise into apprehension and there may thus be an analogy between hysteria and genius; genius consisting in an intensification of the conscious spectrum, hysteria in its dimming and interruption by dark belts of anæsthesia and aboulia, defect of perception and of will; genius consisting in the uprush of subliminal faculty, hysteria in the descent and disappearance of faculty which should be supraliminal into depths from which it cannot voluntarily be recalled.

An inspiration of genius is a subliminal uprush, an emergence into ordinary consciousness of ideas matured below the threshold. This view, given in Chapter III., differs from that of a current school of anthropologists who regard the man of genius as of an aberrant or even degenerate type. The alleged nervous disorder of men of genius illustrates the instability which in a rapidly changing species characterizes those very organs which are moving most decisively along the path of progress. If the word normal signifies such a combination of new with old powers as can at the present stage be affected without dangerous instability, so genius signifies a perturbation which masks evolution, the straining and disruption of the spiritual organism adapted to the earlier phase. The true analogue of the genius is not the criminal nor the lunatic, but the child. Chapter IV. deals with sleep as the alternating phase of personality adapted to maintain our existence in the spiritual environment, and to draw from thence the vitality of our physical organisms. Sleep is a relapse or reversion to an earlier animal condition; a condition where the conscious part of the spectrum lay nearer to the red end; it also represents a stage of wider potentiality, where a longer spectrum is more faintly seen. Here there are traces of ultra-violet luminosity, faculties like telepathy and telæsthesia which form man's link with the spiritual world.

In Chapter IV. hypnotism is said to include all those empirical methods successful in inducing in man what is a development and concentration of his sleeping phase. Suggestion is a mere name for an appeal to subliminal faculty, but here we are not able either to predict or to explain its success or its failure. Long opposed or ignored by orthodox science, the psycho-physiological problems of hypnotism are quite unsolved and its profounder influences on personality have hardly yet been approached. Mesmerism and suggestion are different aspects of an influence which no theory fully explains. Suggestive therapeutics reproduces certain cures held of old as miraculous. As to religion, the influence which has been exerted upon the convert is intermediate between hypnotic artifice, dependent on trance-states for access to subliminal plasticity, and ordinary moral suasion, addressed primarily to ordinary waking reason. In somnambulism the influence is exercised by suggestion and self-suggestion on higher types of faculty, supernormal as well as normal. These nascent experiments give a pregnant hint that it may be in man's power to hasten his own evolution in ways previously unknown.

Chapter VI. deals with sensory automatism, especially the messages which the subliminal self sends up to the supraliminal in sensory form; the visions fashioned internally, but manifested not to the inward eye alone, the voices which repeat as though in audible tones the utterance of the self within. These hallucinations not only are consistent with health and sanity but also surpass the inspirations of genius in their manifestations of important faculty. Here we come upon experiments which prove telepathy, the transference of ideas and sensations from one mind to another without the agency of the recognized organs of sense. There is still a vast separation, unbridgeable at present by any hypothesis of ethereal vibrations or the like, between the smallest act of telepathic transmission and previous knowledge concerning matter and motion. There is no logical halting place between the first admission of supersensory faculty and the conclusion that such faculty is not generated from material elements, nor confined by mechanical limitations, but may survive and operate uninjured in a spiritual world. Among telepathic experiments there is the occasional power of some agent to project himself phantasmally.

Of spontaneous telepathic phenomena are apparitions of a distant person at moments of crisis, of coma, and of death.

In Chapter VII. is presented the supreme problem of the existence or non-existence of a spiritual world. The old conception of the ghost has received a new meaning from observations of phenomena occurring between living men. Phantasmal figures may bear a true relation to some distant person whose semblance is thus shown; wraiths of this kind correspond with death too often to leave the correspondence attributable to chance alone. There is no real break in the appearance of veridical phantasms or in their causation at the moment of bodily death, but there is evidence that the self-same living spirit is still operating. Telepathy looks like a law prevailing in the spiritual as well as in the material world, this is proved by the fact that those who communicated with us telepathically in this world do so from the other. Here the need of actual experiment is felt. There is a possibility of inducing a spiritual bearing and a spiritual picture-seeing or reading, and also a spiritually-guided writing and speech.

Chapter VIII. considers in what way motor automatism, the unwilled activity of hand or voice, may be used to convey messages which come to the automatist as though from without himself. Of course their apparent externality does not prove that they have not originated in submerged strata of the subject's mind. The messages from automatic writing among sane and healthy persons do not generally rise above the level of an incoherent dream. Sometimes they become veridical, convey a knowledge of actual facts of which the automatist has no previous information. This indicates some subliminal activity of the writer's own or some telepathic access to an external mind. This is independent of the question whether both minds, or only one, be still clad in flesh.

Chapter IX. deals with trance, possession, and ecstasy. Side by side with the automatism of arm and hand we must place the automatism of throat and tongue. Automatic utterance begins with mere incoherence but assumes a veridical character, with knowledge delivered from some subliminal stratum or some external mind. In trance the ordinary consciousness of the automatist seems suspended; this seems but the preparation for an occupation by an invading intelligence—by the surviving spirit of some recognizable departed friend. His friend then disposes of voice and hand almost as freely as though he were their legitimate owner. These trance-utterances can in part only be explained by telæsthesia and telepathy operating among actual scenes and the minds of living men. Through the trance-phenomena of Mrs. Piper and Mr. Stainton Moses the evidence for communication with the spirits of identified deceased persons is established beyond serious attack. Eliminating conscious or unconscious fraud, self-suggestion, telepathy between the living and the like, we are forced to accept the messages as representing the continued identity of a former denizen of earth. Neither tradition or philosophy affords us any solid standpoint from which to criticise these messages. These evidences for the survival of human personality have never before approached so near to fulfilment. Especially in ecstasy do these messages from behind the veil help us to solve the relations of spiritual phenomena to space, time and the material world. The difficulties of communicating are such as might be inferred from the analogies between possession and alternating personalities, dreams and somnambulism, but the relations between mind and brain may be elucidated by the difficulties shown by the spirit in using the medium's brain. [1]

Chapter X. is an epilogue. It is here said that the evidence set forth in this book should prompt toward the ultimate achievement of scientific dominance in every department of human study, including, as never before, the realm of 'divine things.' Thus the conception of telepathy proves that the kinship between souls is more fundamental than their separation. Again whilst incarnate men have risen from savagery into intelligence, discarnate men have become more eager and able to communicate with earth. The response made in the past by human spirits of high type has been concordant in recognizing that a spiritual world underlies the material. This agreement is now supplemented by nascent discovery and revelation. Our evidence seems to indicate that the spiritual world is now just beginning to act systematically upon the material world.

These concluding words of Myers remind one of the manifesto of American spiritualism in 1848, the message to the Fox sisters that a reformation was going on in the spiritual world. It seems unfortunate that after all the efforts of the S. P. R. in studying borderland and residual phenomena their head should have brought forth such

[1] Telepathy indefinitely extends the range of an unembodied spirit's potential presence. Powers even more remote are retrocognition and precognition which drive us to postulate some coexistence of past and future in an eternal Now. As individual memory would serve to explain a large proportion of retrocognition, so individual forethought, a subliminal forethought, based often on profound organic facts not normally known to us, will explain a large proportion of precognition. Hence we are tempted to dream of a World-Soul whose future is as present to it as its past.

an historical paradox as this. The initial problem before the society was as to the nature and extent of any influence which may be exerted by one mind upon another apart from any generally recognized mode of perception. Assuming telepathy as proven and adding to it the more occult powers of telæsthesia and telekinesis Myers has based them all upon the subliminal self. But in sinking the foundations so deep the structure itself has suffered distortion. In point of style the outward embellishments are irreproachable, but from the standpoint of normal psychology it is a well-nigh hopeless task to attempt to straighten the building. Beginning with the contrasted views of personality, the old fashioned view of a single unitary personality and the modern view that the self is a coördination, the author's contention as to the abiding unity of the ego is that it consists of ultimate infinitesimal psychic elements which withstand the shock of detah. This proposition is subsumed under another equally mystical, namely that there is a more comprehensive consciousness, a profounder faculty which reasserts itself after death. To prove that this transcendental self survives the empirical self, the nerve-tract theory is brushed aside and there is offered in its stead a vague statement as to subliminal uprushes, the impulses or communications which reach our emergent from our submerged self. At this point a logical difficulty is anticipated; the theory of the subliminal self, it is said, need not be pushed so far as altogether to negative spirit-intervention, because the faculties of telepathy and telæsthesia suggest either incalculable extension of our mental powers, or else the influence upon us of minds freer and less trammelled than our own.

Discussing, in the next place, disintegrations of personality, the author employs the contradiction of an unconscious consciousness in speaking of the unreachable subliminal reminiscences which give the signals for hysterical attacks. The statement that the latter are caused as well as cured by the subliminal self is evidently not seen to be inconsistent with the later optimistic assumption that the subliminal activities are beneficial. Furthermore, in the failure to relegate terrifying dreams, epileptic mania and the deep self-absorption of melancholia to abnormalities of the nervous system, Myers' 'scheme of vital faculty' is indeed such as no physiologist would care to sanction. This neglect of cerebral conditions is matched by the failure to attempt to explain alterations of personality in terms of more ordinary psychic functioning. In multiplex personality the neurosis plays its part; so, for one thing, does the imagination in this dramatic sundering of the self.

The treatment of the inspiration of genius as due to subliminal ideation leaves the subject as much in the dark as ever. The achievements of arithmetical prodigies may be offered in support of the marvellousness of subconscious activities, but the rather dubious tales of mathematical solutions in dreams are given without reference to the preceding waking efforts to solve the problem. If the subnormal fails to explain normal ratiocination, equally little light is thrown on the phenomena of memory by emphasis on the unbroken continuity of the subliminal recollection. A physiological definition of sleep has in truth never yet been achieved, but the cases of hypnotic sleep here cited show that there is persistence of suggested ideas in a dim way into the conscious state. Why the awakened subject remembers these products of 'subliminal mentation' is, in essence, no more mysterious than that he remembers at all. A further overburdening of the subconscious and a further disregard of the physiological side is illustrated by the contention regarding the regenerative and vivifying power which the subliminal self habitually manifests in sleep. The meaning of this magical formula is further explained in the statement that we are living a life in two worlds. The working personality is adapted to the needs of earthly life; the personality of sleep maintains the fundamental connection between the organism and the spiritual world by supplying it with spiritual energy during sleep, and itself develops by the exercise of its own spiritual faculties.

The chapter on hypnotism is perhaps the most interesting in the book. Myers openly takes issue with the Nancy School and asserts that suggestion is a premature attempt to simplify supernormal communications. But in attributing suggestion to aura and emanations the author is guilty of a theory equally retrogressive. Recent attempts to place suggestion under the general laws of association are stultified and there is a return to the days of odyllic forces. Without giving an historical survey of hypnotism, which manifests a decided trend toward explanations in terms of normal psychology, Myers claims that suggestion is not comparable with supraliminal sensation and endeavor. Its real efficiency, he says, lies among subliminal processess, as an empirical facilitation of our absorption of spiritual energy or acquisition of directive force from a metethereal environment. The grounds for this mystical conclusion may be shown to be quite self-contradictory. To give them as they are presented: Hypnotism in animals is laid to catalepsy and paralysis from fright but in man to the subliminal self. Metallæsthesia is attributable to obscure chemical reactions and also the sensitiveness of the central sensorium. The perception of the lapse of time in birds is due to the panæsthesia of the primal germ, in man to the secondary personality. Stigmatization on the right side, while the stimulus is on the left, is attributed not to decussation but to the subnormal intelligence presiding over the organic suggestions. Crustacean recuperativeness is possessed by the lobster because of self-suggestion; it is lacking in the mammal because of the inhibition of hysterical self-suggestion. In vesication hypnotic suggestion awakens the dormant plasticity not blindly but with intelligent caprice. Post-hypnotic suggestions are obeyed blindly and also manifest the intelligence of subliminal mentation; they may lead to kleptomania and also develop the higher sense of propriety. In rapport the hypnotized subject knows supernormally the superficial sensations of his hypnotizer, but this community of sensation needs education and development. In subliminal states like trance there is greater responsiveness to spiritual appeal, yet it is the lower organic centers which are under more control. Finally, in the somnambulic state we are introduced to two subliminal powers apparently quite disparate, the sanative which modifies the body, the talæsthetic which quits the body. In the face of these inconsistencies and incoherencies Myers still claims to have placed suggestion in truer relation to other forms of external suasion and intellectual will than those who compare it with supraliminal suasion and endeavor.

In the chapters on sensory and motor automatisms there again appears the writer's curious aversion to the correlation of the alleged subconscious with ordinary psychoses, for example rapport as a form of concentrated attention, voluntarily limited to the operator. There is also exhibited his disinclination to recognize mere neural activities. Thus in citing experiments in which by 'silent willing' a finger becomes cold much is said about supernormal effluence, nothing of peculiarities of vaso-motor action. So in regard to glossolaly, automatic writing and inspirational speaking, there is much about messages from the subliminal, nothing about cerebral decentralization.

To convert the author's proposition — it is ordinary psychology which has become the excrescence on the subliminal life. When hypnagogic images are called vaguely typical this is laid to subliminal generalization not to suspension of voluntary attention. So many illusions of sense are counted veridical impressions or pictures and not mere subjective fancies. In crystal visions normal and supernormal knowledge and imaginings are considered to be strangely mingled. Memory, dream, telepathy, telæsthesia, retrocognition, precognition, all are there. In trying to account for these occult powers Myers is naturally averse to physical explanations and tests. Sir W. Crooke's vibration theory of telepathy is discarded because incompatible with the phenomenon that the percipient's mind modifies the picture despatched from the agent. There is of course no recourse to the alternative explanation of everyday psychology, that the so-called telepathic message, obtained from vague sensory hints such as pressure and unconscious whispering, undergoes an inevitable subjective generalization. Again it is asserted that telæsthetic visions may show great laxity of time relations, because if they are not synchronous they are either precognitions or retrocognitions. This convenient method of evading the natural by offering supernatural alternatives is further exemplified in clairvoyant visions which are symbolical, not because there is no such thing as clairvoyance, but because they are not located by the observer in ordinary three-dimensional space.

The convenience of having several lines to catch your fish is best illustrated in the chapter on phantasms of the dead. Discarding a previous view of the essential unity of the self through a subliminal substratum, the problem now starts from a root-conception of the dissociability of the self, that segments of personality can operate in apparent separation from the organism. At this point the author appears to recognize the hypothetical character of his whole argument. He puts it in this syllogistic form: If we have once got a man's *thought* operating apart from his body there is no obvious halting-place on *his* side till we come to 'possession' by a departed spirit, and there is no obvious halting-place on *my* side till we come to 'traveling clairvoyance,' with a corresponding visibility of my own phantasm to other persons in the scenes which I spiritually visit.

The rest of the work is confessedly a palæolithic psychology and Myers' reversion to the beliefs of the Stone Age in ghosts, haunted places, and heaven-sent dreams is supported merely by a negative: 'What definite reason do I know why this should *not* be true?' Against this constitutional will to believe the only thing to do is to separate the elements of fact and fiction in the psychology of the psychic researcher. The census of hallucinations, the citation of hundreds of cases of externalized apparitions is not a complete answer to the danger of illusions of memory, creating or magnifying the interesting coincidences, for these illusions are matters not of general tabulation but of individual introspection. The incoherence of planchette messages is scarcely to be taken as due to the jostling of the spirits, when we have the same incoherence in the automatograph due to uncontrolled muscular action. This persistent turning toward subterranean explanations is again illustrated in the citation of Professor Flournoy's study of a case of somnambulism with glossolaly. After a painstaking *Quellensucht* for the elements of Mlle. Smith's cryptic Martian language, Myers concludes that the knowledge of Sanscrit betrayed therein was not derived from a Sanscrit grammar known to be in the room in which the séances were held. On the contrary it was clairvoyantly acquired by the subliminal self.

In conclusion, by putting forward the Moses-Piper group of trance phenomena as evidence for spirit possession, the author's language, to use his own simile, suggests the medicine-man's wigwam rather than the study of the white philosopher. This unscientific attitude is unfortunately true. Here for once there is failure even to consider the alternative explanation. In the case of Stainton Moses, it does not seem to have occurred to Myers, as it has to his colleague Podmore, that there was deliberate fraud in the twilight materialization of a phosphorescent demon. In the case of Mrs. Piper there is no rebuttal of Professor Lodge's surmise that on the part of the Phinuit 'control' there was a system of ingenious fishing: the utilization of trivial indications, of every intimation, audible, tactile, muscular, and of little shades of manner too indefinable to name.

The epilogue to these volumes reads like the *Philosophy of Spiritual Intercourse* of Andrew Jackson Davis, the 'Poughkeepsie seer.' In the one psychic research is urged as a duty much as in the other spiritualism was urged as a religion. But this study of 'responses to stimuli spiritually controlled' can scarcely be considered a 'profound cosmic thesis for scientific proof' so long as the S.P.R. requests posthumous letters, containing test sentences, as proofs of spiritual survival, and so long as it depends on the utterances of a medium who mistakes Baby Marian for Baby Timmins.

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Agnosticism. ROBERT FLINT. New York, Scribners. 1903. Pp. 664.

The present volume forms a part of a general system of natural theology which embraces also the well-known writings of Professor Flint on *Theism* and *Anti-Theistic Theories*. The work on *Agnosticism* is a continuation of the latter, and is conceived and carried out in the same spirit of historical research and candid criticism which characterizes all of Professor Flint's philosophical discussions. As a part of a system of natural theology, religious agnosticism is the special phase of the subject which it is the purpose of this book to examine and combat. There is, however, a full recognition of the other phases of agnosticism, such as that concerning cognition in general or concerning the world, or self. While these phases are discussed from the point of view concerning the separate spheres to which they specifically relate, nevertheless Professor Flint has always in mind their ultimate bearing upon religious agnosticism. There is from the outset of the discussion a clear cut line of distinction drawn between agnosticism proper, and the many loose and misleading senses in which the term is used. Thus agnosticism is sharply differentiated from honesty in investigation, from nescience, from atheism, and from such a view as Leslie Stephen's that agnosticism is the opposite of gnosticism.

Agnosticism with the author, means 'the theory of the nature and limits of human intelligence which questions either the certainty of all knowledge and the veracity of every mental power, or the certainty of some particular kind of knowledge, and the veracity of some particular mental power or powers' (p. 22). Again in a similar vein he defines agnosticism as that form of skepticism which is doubt or disbelief 'resting on the supposition that what are really powers of the human mind are untrustworthy; that what are actually normal perceptions, natural or even necessary laws and legitimate processes are not to be depended upon' (p. 23). In this definition, the author has rendered to the philosophical world a double service. He has extricated the term agnosticism from a confused mass of conflicting connotation, and unwarrantable implication. And also, he has emphasized the necessity of grounding any satisfactory defense of theism upon a sound epistemological basis. He is, moreover, out of all sympathy with the so-called 'agnosticism of piety' which exalts pious feeling at the expense of reason. The main lines of Professor Flint's criticism of the agnostic position may be briefly indicated as follows:

Epistemology as the theory of knowledge should be the complete theory of knowledge. It is not entitled to lay down as limits of knowledge what are merely limits of a specific kind of knowledge. "Whatever claims to be knowledge should have its claims fairly examined and should not be set aside as pseudo-science in misplaced confidence on any superficial generalization or dogmatic assumption as to what is and what is not knowledge. Hence epistemological theory cannot of itself warrant us to pronounce physiology, for example, a real science, and psychology a pretended one, sense perception a faculty of knowledge, but apprehension of the divine an illu sion, phenomena within and noumena without the sphere of cognition, etc." (p. 339). He criticises the agnostic position that because there is not complete knowledge therefore the knowledge which the mind does possess is of no value, and maintains that all knowledge is progressive, that while the knowledge of God is incomplete, it is nevertheless a knowledge which is growing from age to age, and forms so integral a part of the knowledge of the world and of ourselves that the advancement of science, the progress of history, the more exact investigations of the human mind, all contribute to a deeper knowledge of the divine power of which they are the various manifestations.

Against the doctrines of Hamilton, Mansel and Spencer, the author contends that the idea of the absolute is not involved in absurd contradictions, is not the wholly indeterminate, not that which is out of relation to all things, not the unknowable, but the ground of all relationship and the essential content of all knowledge.

He contends, moreover, against the Kantian diremption of phenomena and noumena, of thought and of being that, so far as it is possible to know at all, we know through 'ideas which are absolute and noumenal in the only intelligible and in a very real and important sense' (p. 645). They condition experience and are not conditioned by experience. He maintains, moreover, that Kant's criticism of the theistic proofs has modified their statement but not their underlying and essential principles, and that Kant's moral argument for the being of God to which his practical reason so strongly assented, is in principle similar to the cosmological, and the physico-theological arguments, that, in short, no sharp line or distinction should be drawn between the pure reason and the practical reason. Throughout this discussion Professor Flint has treated his opponent's views with fairness, and with a sympathetic appreciation of their positions. His historical survey of the history of agnosticism is very complete, and provides an excellent source for convenient reference. The arrangement of his material in a different manner would have rendered his exposition clearer and more effective. For instance, in the earlier part of the work, he gives an historical sketch of agnosticism in general, and in the latter part of that particular form of agnosticism which is anti-religious. Again, in one chapter he treats of agnosticism as to God, and in a later chapter of agnosticism as to knowledge of God. The distinction is not clear and is not significant. Moreover he refers to Huxley's agnosticism as equivalent to 'a spirit of intellectual honesty in investigation' (p. 42), which definition of course can raise no dissent. Huxley's agnosticism, however, was more than a plea for absolute honesty in investigation, it affirmed also that an intellectual honesty in investigation could never carry one beyond the sphere of an experience resting solely upon data of the senses, and that the only proof which could appeal to an honest mind is that which swings clear of all metaphysical presuppositions and dogmatic assertions. Huxley's name is so identified with the term agnosticism that the precise connotation which he gave to it should be clearly recognized.

Professor Flint's plan for his system of natural theology calls for further volumes on the idea of God as disclosed by nature, mind and history, together with a tracing of the use and development of the idea of God, and the history of theistic speculation (p. 640). We sincerely hope that his strength and years may be sufficient for the completion of a task whose accomplishment so far has placed the philosophical world under so great indebtedness to him.

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Le Caractère. P. Malapert. (Bibliothèque Internationale de Psychologie Expérimentale.) Paris, Octave Doin. 1902. Pp. 305.

Noteworthy attempts have been made within the past few years to give form and substance to the idea of a science of character first proposed by J. S. Mill under the term ethology and variously designated by other writers as characterology and as individual or, again, differential psychology. The result has been the accumulation of a large amount of material bearing on the problems involved and the development of numerous conflicting, but suggestive, points of view. It may even be said that the science has been founded, at least a beginning had been made, which would seem to justify reasonable expectations. The work of construction has been undertaken mainly by the French. The work is relatively so new, while yet so much work has already been done, that it is both possible and profitable to survey and estimate the whole of it. It is this task which M. Malapert has set himself, and which he has executed with admirable skill, in the volume before us.

Mill's conception of a science of character has been appreciably modified in the course of the discussion. Mill conceived the science as a counterpart of the art of education, its object, or at any rate its principal object, being to deduce from the general laws of psychology, assumed as known, the sort of character, national, collective or individual, which would result from a given set of physical and moral conditions. Ethology, in brief, was to be a science of the laws governing the formation of character, its method, in Mill's view, being mainly deductive. In the newer conception, the first object of the science is to define and classify the various types of character. It is only in the second place that it investigates the causes and laws of their genesis and transformation. The emphasis put upon the former object by recent writers has accordingly tended to thrust into the background the deductive method approved of Mill and to the employment of clinical and experimental methods of observation. The main stress is still on individual ethology, though various attempts have been made to deal also with the characters of social and national groups.

It should be observed that the term 'character' in this connection is used in the large sense of the system of psychical dispositions by which an individual is characterized. What is meant by a 'type' of character is not so easily stated. It is held, however, in general, to be constituted by the relative preponderance, force, form, direction, vivacity and mutual relations of mental elements and functions. The science has mainly to do with the precise analysis and synthesis of these elements. But the terms employed are all more or less vague and the problem is extremely complex. The psychical individual seems to elude precise analysis, psychical individuality, constituted as it is by a unique synthesis, appears as a positive limit to science. Nevertheless psychical individuals do represent — who can doubt it? — more or less distinct types of character, corresponding somewhat to the genera, species and varieties of general biology. And the determination of these types presents itself as a possible problem. It is only when we come to examine the attempts that have been made to solve the problem that the vagueness of the conceptions and the arbitrariness of the principles of classification adopted by different writers make evident how great and difficult the problem really is. All this appears frankly confessed in the pages of M. Malapert. The successive chapters treating of the object and method of the science, of the factors of character, of the metaphysical theories, the theories of temperament and the psychological theories, then the important chapter on the classification of characters, finally a chapter treating of abnormal and morbid characters, all show a conflict of opinion that makes the task seem well-nigh hopeless. And yet it is not perhaps altogether hopeless. In a concluding section M. Malapert enumerates several circumstances which serve to tone down the apparent conflict in opinion, *e. g.*, the lack of precision in the psychological vocabulary and the tendency on the part of writers themselves to exaggerate their differences. He also remarks, as a sign of progress, on the general approach to at least a negative agreement in the elimination of certain problems and points of view, *e. g.*, metaphysical speculations concerning the nature and origin of the individual and, again, the theory of temperament as a point of departure for the study of character. He points further to the large amount of positive material bearing on the problem obtained in numerous researches into the variations of mental processes in individuals. And although little has been done which finds general acceptance in the way of determining the relations of the different mental processes in individuals, little to give definiteness to the vague conception of preponderance of elements and to fix the character and limits of their mutual influence, we have, he thinks, even in this regard the tentatives which can alone be expected in so new a science.

The student of human nature will justly estimate highly the value of a monograph such as this, which serves as a trustworthy guide to the history and problems of a branch of psychology so new, so attractive, so large in possibilities. M. Malapert is very clear and objective in his expositions and singularly fair, temperate and broad-minded in his criticisms. He is scarcely, if at all, less conscious of the limitations of his own views than he is of those of others. Thus, after giving his own classification of characters into apathetic, affective, intellectual, active, balanced (*tempérés*) and voluntary, he indicates quite plainly that he does not consider it one which can claim to be perfectly natural, based on a principle recognized by all (p. 269). In fact it rests on a four-fold division of the mental faculties into sensibility, intellect, activity and will (p. 209) which may very well be questioned. For while it is perfectly true, as the author, following Fouillée, contends, in criticising Ribot's division of faculty into feeling and action, that intellect is a human characteristic, and while voluntary activity, in the strict sense, is, as the author justly maintains, obviously distinct from mere motor activity, the sense in which the term 'activity' is used in the above division, neither intellect nor will can perhaps properly be regarded, from the psychological point of view, as fundamental, certainly not as elementary mental functions. Voluntary activity, in the strict sense, would seem rather to be but a highly developed complex organization of ideal tendencies or conations and the intellectual life itself but a form and expression of the fundamental sensibility in which it is found constantly embodied. But if this is so, if feeling and the conative aspect of feeling form the sole ultimate empirical basis of consciousness, and if a classification of types of character is to be based on an analysis of ultimate functions, the classification adopted by M. Malapert can scarcely be maintained. But while it would be interesting to see a classification of types of character derived from the above principle not open to the objections brought against Ribot's classification (p. 251) based on this principle in a somewhat different form of statement, it must be admitted that the principle itself, like every other, is still too little developed and too far from general acceptance to form the foundation of a science.

H. N. GARDINER.

SMITH COLLEGE.

Studies in the Cartesian Philosophy. By NORMAN SMITH. London and New York, Macmillan.

This is a somewhat notable contribution to the literature on Descartes and his influence on subsequent thinking. The aim of the work is critical rather than expository, but there is no lack of exposition of a very clear and illuminating kind. In fact the best informed student of Descartes will come from the perusal of this book with renewed insight; or at least with sharpened perceptions. The quality of Professor Smith's work cannot, in fact, be too highly commended. His faculty of analysis and his power of defining and stating issues clearly, is quite remarkable and his criticisms are always trenchant and mostly just. The discriminating way in which he traces the Cartesian influence through Descartes' successors down to Kant is quite masterly and generally convincing. At the basis of Professor Smith's criticism is a distinction between Descartes' contributions to natural science and his metaphysics. Of the former he says: 'In a more adequate manner than even Galileo or Bacon, Descartes formulated the methods and defined the ideals of modern science.' But in metaphysics he finds him still in bondage to scholastic abstractions and to what he calls Plato's mystical idealism. The author deals with the system of Descartes under two general heads, his *Method* and his *Metaphysics*. The former is found to be mistaken not so much *qua* mathematical, as because it involves a false conception of mathematics itself, one that having first created a dualism between perceptions and conceptions and elevated the latter into absolutes, identifies mathematical method with an analytic deduction of the content of these conceptual terms. It is easy for the author to show that such a method results in a pure rationalism that eliminates sense perception altogether. Moreover, it results in a dualism between algebra and geometry, inasmuch as the latter clearly involves space which the author claims is no conception but a concrete reality revealed in perception. The one merit of Descartes' method is its insistence on clearness and distinctness as essential to validity.

In his discussion of the Cartesian metaphysics the author points out how the conceptualism of Descartes leads logically to pure rationalism which would exclude all perceptual and volitional elements from knowledge. But the dualism of matter and mind breaks the rationalistic flight as it were in mid air. We reach the existence of self, not intuitively, but through the immediate implication of the *cogito* But we have no such resource in the case of the external world and the completely alien nature of matter. Here Descartes' representative theory and with it the unimpeded flight of his rationalism breaks down. In order to guarantee the truth of our ideas of the objective world or non-ego, Descartes is compelled to bring in his spiritualism, including the agency of God, and the sensational and volitional activities of the soul. The author shows very clearly how the spiritualism of the system drifts inevitably to *Occasionalism* although Descartes fights against that consummation.

The most interesting chapters in Professor Smith's book are those in which he follows the fortunes of the Cartesian rationalism through the thought of Descartes' successors of both the rationalistic and empirical schools. We, of course, expect to find Spinoza and Leibnitz, reputed rationalists, more or less under Cartesian influence. And we have become accustomed to regarding Locke as a sort of halfway thinker in whose work a certain amount of rationalistic lumber exists as a mere uncritical survival. Our author contends, however, and we think successfully, that Locke was fully as much a rationalist as an empiricist in his own method and doctrines. But that the thorough-going hater of rationalism, David Hume, should himself be to some extent its victim is a fate that we would not invoke for our worst enemy. Nevertheless, the great sceptic stands convicted and Professor Smith is able to distinguish the actual Hume from the Hume history would have presented to us, had he been wholly free from traditional Cartesian prepossessions.

It is only in Kant, the author says, that we reach a conception that has finally emancipated itself from rationalistic prejudices. The chapter on Kant is in some respects the most unsatisfactory in the book. It is long enough for the author to tell us that in Kant's Copernican revolution and critical idealism he has achieved for the first time the hypothetical method and pure phenomenalism of modern science. It is too short, however, to inform us whether the author thinks the significance of Kant is exhausted in this result. There is a point of view, which the author has stated clearly from which pure phenomenalism or positivism could be considered a logical outcome of Kantism. But if the claim were made exclusive so as to shut out other points of view as mistaken, then Hegel might put in a demurrer. Those who believe that Kant may be reconstructed from the point of view of things in themselves might also be impelled to file an objection. Nevertheless, we have reason to thank the author for the Cartesian clearness and distinctness which make his own work delightful reading, as well as for the masterly character of his discussion as a whole. A feature of the book is its beautifully clear typography, especially that of the footnotes which the eye can take in at a glance.

ALEXANDER T. ORMOND.

PRINCETON UNIVERSITY.

Hegel's Logic, An Essay in Interpretation. JOHN GRIER HIBBEN. New York, Scribners. 1902. Pp. x + 373.

We are glad of this addition to the books, already become a row on the library shelf, which are entitled Hegel's Logic. Although coming last it will serve for many as a key to its predecessors. To say the least it is intelligible and easy to read, two factors which will not lay Professor Hibben as open to the charge of infidelity toward Hegel as some might think.

In bringing Hegel up to date there is of course a tendency to substitute phrases which shall make us less prone to disagreement and make him more in line with the emphasis of to-day. For instance, on page 18 Professor Hibben asserts as Hegel's position that 'Reason has two sides — a thought side and a force side, a rational and a dynamic essence — and these two are one.' And again on page 4: 'The creative and sustaining source of the universe is thought force.' To us the phrase 'thought force' speaks of Fouillée's *Idées Forces*, of Ravaisson's realistic spiritualism, of modern panpsychism and of the whole American emphasis on will; an emphasis perhaps not inconsistent with, yet certainly not to be gathered from Hegel's own statement of his position. Professor Hibben has on his side the possibility that had Hegel foreseen the direction which criticism would take, his emphasis would have met it in about this way.

The book is and perhaps purports to be a summary with explanatory notes woven in, in readable form, of Hegel's shorter Logic as found in Part I. of the 'Encyclopedia of Sciences,' which was published in 1817. This part I. in turn is an abbreviated and annotated edition of the two-volume work entitled the 'Science of Logic,' 1812–1816. This larger Logic is the one which Professor W. T. Harris took as the basis for his Hegel's Logic in Grigg's Philosophical Classics.

Of Professor Wallace's two volumes entitled Hegel's Logic, the first is a translation with notes of the shorter Logic of the 'Encyclopedia of Sciences.' In the second volume are prolegomena to the whole of Hegel's philosophy as much as to the Logic.

Dr. Baillie's recent book on Hegel's Logic is a general introduction to Hegel's system not at all adapted however to give one a first insight into Hegel.

For this Professor Hibben's book is specially adapted and the glossary of philosophical terms in the appendix would indicate that he intended the book to be used as an accompaniment to the reading of Hegel in the original. Lucidity has apparently been his chief desire and therefore he is to be pardoned for his boldness in always trying to make Hegel say something intelligible. It is a greater injustice to Hegel to expound him so delicately that he remains unread than to run the risk of misinterpretation or underinterpretation while making clear his importance to us. Professor Hibben's book will do good service not only as an introduction but also as a stimulus to the reading of Hegel.

GEO. R. MONTGOMERY.

YALE UNIVERSITY.

Heredity and Social Progress. SIMON N. PATTEN. New York, Macmillans. 1903. Pp. vii + 214.

Readers of Professor Patten's former works will be prepared for audacious generalizations, but this book seems to offer a maximum of hypotheses with a minimum of supporting evidence. The fundamental thesis presented is that progress starts from a surplus rather than from a deficit, as is assumed by 'current biology and classical economics.' The problem as stated in the terms of economics is: How can the social surplus, wrung from nature by conscious effort in the face of diminishing natural returns, be transformed into mental traits that abide and become the basis of subsequent progress? Stated in biological terms, the problem is: How can acquired characters become natural?

The method is an attempted parallel between biological and psychical processes which will be likely to impress the psychologist as based on analogies of the most superficial kind. Thus: memory implies related parts; it corresponds, therefore, to growth in the structure. Visualization is the mental struggle for complementary ideas; it corresponds, therefore, to a physical process of regeneration in a disrupted structure. "Memory and visualization are thus at opposite poles of thought."

Perhaps the most startling bit of psychology is found in the chapter on 'The Inner Organs of Expression.' The general thesis of the chapter is to the effect that, as there has been a parallel development between the outer body on the one hand, and an inner neural body on the other, we may expect the inner as well as the outer to retain some traits characteristic of more primitive stages, and these may become the organs for acquired characters. The physical basis of reasoning is sought for along this line: Reasoning is a process of rejecting dissimilars and accepting a similar. Now recoil from the dissimilar and the acceptance of the similar is a tendency of unicellular organisms, like the amœba. Reasoning has no new element. Here consciousness shows its elementary and primitive character, whereas digestion demands a hundred independent reactions. The naïveté with which the process of reasoning is here conceived is scarcely to be matched this side of the earliest efforts of the Greeks, and while there are many problems proposed throughout the book which challenge attention, the author's psychology is certain to awaken distrust as to his solutions. If they are right it is not because of his processes.

J. H. Tufts.

University of Chicago.

HEARING.

Der Tonvariator. L. William Stern. Zeitschrift f. Psych. u. Physiol. d. Sinnesorgane, 30 (5–6), 1902, pp. 422–432.

The instrument is a blown bottle, made of metal, without a bottom, but with a movable piston in its stead. The piston is moved by means of a spiral in such a way that the increase or decrease of the vibration rate is proportional to the angular velocity of the spiral. More than one bottle may be combined on the same stand. The range of a single bottle is usually about an octave; less, if the pitch of the bottle is very low. The instrument is doubtless a very useful source of sound for many purposes.

Max Meyer.

University of Missouri.

Ueber binaurale Schwebungen. P. Rostosky. Philos. Studien, 19 (Festschrift, I.), 1902, pp. 557–598.

The author's problem is this: Are there binaural beats of central origin? The experiments made to solve this problem are the following: The tone of an electromagnetic tuning fork was conducted through two brass tubes of equal length to the two ears of the observer, sitting in another room. When the intensity of the tone acting upon one ear was made less than on the other, the tone perception was localized on the side of the stronger stimulus. When the intensity was the same, but the phase was altered, a very peculiar change in the subjective localization was observed. Now, since the phase as such is imperceptible, the author attempts to reduce theoretically the case of difference of phase to a case of difference of intensity by assuming that the vibrations acting upon the left ear do not send an exactly corresponding nervous process to the center, but that they suffer a certain interference from the vibrations acting upon the right ear. (And the same for the other ear.) He develops the mathematical theory of this function in all details, demonstrating that the result of the above assumption must be exactly those changes in the relative intensity of the sensation of either ear, which agree with the peculiar changes of the subjective localization observed in the experiments. It is, therefore, proved that the vibrations set up within one ear suffer interference from the vibrations set up within the other ear; and the amount of this interference is accurately expressed by a formula. The question to be answered now is this: Does this interference occur within the peripheral organs, or do the nervous processes coming from both ears, before arriving at the cortex, pass through two lower nerve-centers in such a way that the greater part of the nervous process coming from one ear passes through the lower center of the same side, a fraction, however, to the lower center of the other side, causing thus the interference which the author has mathematically analyzed? The latter explanation of the interference is possible only if we assume that the nervous impulses set up by the objective vibrations are of an oscillatory nature. The author rejects this assumption as unfounded and improbable, and adopts the assumption that the interference occurs in the peripheral organs. The vibrations are mechanically conducted from one ear to the other by way of the cranium. The physical constants in this case agree sufficiently well with the constants assumed in the mathematical analysis. The theory of central interference is, therefore, not only improbable, but entirely superfluous. The objections to this explanation by mechanical conduction raised by different writers are not valid. The method of observing the localization is much more sensitive and exact than any direct method of observing the relative intensity of the sensation in either ear, and its results are, therefore, of more weight. It can be applied with the same result to stimuli, which, on a single ear, are below the threshold.

MAX MEYER.

UNIVERSITY OF MISSOURI.

EXPERIMENTAL.

An Analytic Study of the Memory Image and the Process of Judgment in the Discrimination of Clangs and Tones. G. M. WHIPPLE. Amer. Journal of Psychology, 13 (2), April, 1902, pp. 219–268.

The author continues his previously published investigation into the discrimination of tones, by using tones of continuously changing pitch instead of discrete tones. The observer reacted when he thought that the second tone had reached a pitch equal to the standard pitch. Between the first and second tone was a time interval of either ten or forty seconds. Generally the observers reacted too early, and the more so the longer the time of change (proportional to the pitch difference) was. The author mentions a great number of interesting introspections of the observers with respect to the early reaction and other facts. Some observers have a distinct emotional preference in regard to the direction of the variable stimulus; they prefer to listen to a rising, or to a falling tone. One of the observers (who was less musical) used other characteristics more than the pitch itself: visual and temperature sensations in particular. In a series with a time interval of forty seconds the quantitative results were much more irregular than with a time interval of ten seconds. The loss of the auditory image did not make the judgment impossible, but the presence of the image afforded greater assurance in the reaction. A third series consisted of experiments with knowledge. The general effect of the knowledge of the coming position of the variable tone gave a feeling of security, did away with the momentary perplexity. A second effect is the presence of an anticipatory image of the variable stimulus. In a fourth series of experiments with a time interval of ten seconds the memory image was eliminated, so far as possible, by distraction set up by odors. Distraction, like a long time interval, lessens assurance. The expectation error is very materially broken up.

Some special tests were made on the unmusical observer, who used visualization. Her drawings of the movement of the variable tone agreed very well with the actual change.

In regard to the nature and course of the memory image the author draws these conclusions: The auditory memory image is but one part of a complex structure which represents the original experience. The memory image of a tone is not a tonal memory image; it is that and much more. The auditory image proper attains its maximal excellence about two seconds after the stimulus. It is in a very unsatisfactory condition at 40 seconds. The other constituents of the memory image do not necessarily follow the course of the auditory core; they may be serviceable for purposes of discrimination when the auditory image has disappeared entirely. Practice increases the serviceability of the image. The task of actively holding the image very soon develops a habit of imaging; the image, that is, of itself becomes so insistent that, when exclusion of the image is desired, very active attention to naturally powerful distractors is necessary. The presence of the auditory image is not necessary to the recognition of either difference or equality.

MAX MAYER.

UNIVERSITY OF MISSOURI.

Ueber Vertheilung und Empfindlichkeit der Tastpunkte. FRIEDRICH KIESOW. Philos. Stud., B. 19, 260–309.

This is a psycho-physiological study of 'touch spots' with special reference to the relation of tactile organs to hairs. It is a continuation of investigations begun at an earlier date in company with von Frey (*Zeitschr. f. Psych.*, 20, 126). Aside from a brief critical review of work done by Blix, Goldscheider and others the work is valuable chiefly for the large amount of carefully collected data. It contains twenty-seven tables, besides many columns of figures not included in the tables. For the purpose of close study the surface of the skin on different parts of the body was marked off into small areas, and a lens was used to locate the points, which were carefully marked when found. Pure tactile points not connected with hairs, were found on haired surfaces, but in comparatively small numbers. Individual differences are considerable, as might be expected from the great difference in the number and arrangement of hairs on different persons.

Die Ebbinghaus'sche Combinationsmethode. E. WIERSMA. Zeitschr. f. Psych. u. Phys. der Sinnesorg., B. 30, Heft 3, 196–222.

In this article Dr. Wiersma reports some investigations carried on under what seemed to be very favorable conditions for testing the so-called Combinationsmethode of Ebbinghaus. He used the same methods of investigation and calculated his results in the same manner as did Ebbinghaus (*Zeitschr. f. Psych.*, 13, 401). The experiments were made on the students of two schools, one for each sex in which admission was by competitive examination. This, he thinks, secures students of about the same ability when they enter school. It may be said, parenthetically, that no significant sex difference was noticed. The results from these students were compared with those from another school in which no unusual entrance conditions existed. By comparison of the results obtained from the lower and higher classes in the schools it seems possible, in some degree, to distinguish between the capability due to endowment (*Begabung*) and that due to development (*Entwickelung*). This is in part based on the assumption that the younger students have better natural endowments than the older ones of the same class, since it has taken the older ones longer to reach the grade. This may be a safe criterion in some schools but usually the conditions which determine the age at which a pupil reaches a certain grade are so complicated that it is doubtful if it could be put to practical use. The article is interesting and suggestive and the results seem to justify the writer's conclusion that the Ebbinghaus method is of use for both normal and pathological subjects.

J. F. MESSENGER.

COLUMBIA UNIVERSITY.

PSYCHOLOGICAL REVIEW MONOGRAPHS.

The Practice Curve — A Study in the Formation of Habits. J. H. BAIR. Psychol. Rev., Mon. Sup., Vol. V., No. 2, Nov., 1902.

This research was occupied for the most part with an investigation of the conditions under which habits are formed and broken, and the laws governing them.

The method employed was to practice a series of stimuli, respectively, with a series of responses until the process became automatic, or until at a given rate no more errors were made. The instrument used was a typewriter, on the carriage of which was placed a series of colors which moved with it and of which one after the other was exposed through a slot in a screen placed tight in front of the series. Colored caps corresponding to the series of colors were placed over several of the keys in a certain order. The experiment consisted in pressing down these keys as rapidly as possible as their corresponding colors appeared in the slot. The rate at which the series could be completed without making errors was recorded from time to time as the practices were continued until the maximum of speed was attained. The curve of progress for speed represents an asymptotic approach to a physiological limit. Another form of the experiment was when the rate of response was kept uniform by keeping time with the beat of the metronome, and rating the practice skill in terms of the number of errors made. Here again the same law is expressed in the progressive elimination of errors with succeeding practices. This experiment was made with different rates of the metronome and the results show that the more rapid the rate the greater the number of errors and the more practices required to eliminate them at that rate.

When the practice was long enough continued so that the series of responses had become thoroughly coördinated with the series of stimuli a change in the relation was made.

First, the order of the series of colors behind the screen was changed and the practice was continued and the increase in time required and the number of experiments to attain the old speed; or, the number of errors made and the number of practices to eliminate them were noted. *Second*, the order of responses was interrupted by reversing the order of keys and the time recorded as above. *Third*, both the series behind the screen and the order of the caps were interchanged and the results noted accordingly as above.

This experiment shows that the increase in time required and the number of practices to reduce it to the former speed is less when the series is changed than when the caps are changed, and most when both caps and series are changed, but in no case as much as in the first practice.

This experiment shows that the sensory associations, in the series, and the motor ones or less definite than those which are sensori-motor.

Further experiments were made to determine more definitely the relation between the number of practices made in one order and their practice effect, or interference effect, on the ability to run over a new series which is antagonistic to the one practiced. It was found that there is no such a thing as interference and that continued practice in one order increases proportionately the ability to make quickly and accurately a new and antagonistic order. This law of the decrease of interference as we pass from one order to the other is also expressed by a curve similar to that described above, only the approach to the limit where there is no more interference experienced in changing from one to the other, is much more gradual.

Other experiments were made with cards which corroborate the same fact of learning and interference.

THE AUTHOR.

COLUMBIA UNIVERSITY.

Motor, Visual and Applied Rhythms. JAMES BURT MINER. Psychol. Rev., Monog. Sup., Vol. V., No. 21, 1903.

The thesis brings together four lines of investigation related to rhythm. A revision of the explanation of rhythm is attempted on the basis of muscle curves obtained during involuntary movement. A beginning is made in the study of rhythms experienced from flashes of light. The reproduction of time intervals is tested under different conditions. Finally, from the more practical side, the effect of independent rhythms on mental work is approached by correlation methods.

The first part of the paper deals mainly with the explanation of the feeling of unity in the rhythmic group. Curves are given which demonstrate, at least for eight subjects, that the motor effect of listening to a series of like sounds was something more than a single muscular response to each stimulus. The involuntary activity of the muscle seemed to be a reaction which set off succeeding groups of stimuli. A kymograph record is printed which shows that electrical stimuli applied successively to the thumb may produce this same regular grouping activity of the muscles. It seems, therefore, to be a fundamental structural condition similar to that producing the grouped reactions shown by the experiments of Richet, Lombard and others. When actual movements are not recorded the writer concludes that they are replaced by strain conditions in the muscle. The absence of movement is accounted for by the fact that most people in the waking state always hold their muscles sufficiently tense to prevent any slight tendency to movement. This conclusion is supported particularly by movement curves obtained, without suggestion, from a subject in the hypnotic state, although none were found normally. The involuntary grouping activity in the muscles is interpreted as the physiological correlate which explains the 'unitary character' of the rhythmic group. The tendency of experimenters to relate rhythm more or less definitely to muscular activity is traced through the work of Bolton, Meyer, Wundt, Smith, Stetson, MacDougall and others. The author rejects, as inadequate or incomplete, those explanations which are based on regular bodily rhythms, on attention or on expectation and satisfaction.

If a kinæsthetic explanation holds, it would seem that visual rhythms, or even rhythms of odors and tastes, might be experienced. Contrary to general opinion, a subjective experience of grouping within a uniform series of light flashes was found to be quite easily developed by the twenty-six subjects tested. Two naïve subjects seemed to perceive involuntarily the rhythmic grouping, without having received any external suggestion. In the essential characteristics of rhythm, the effect of the lights was apparently the same as that of the sounds; but the light rhythm was more vague and easier disturbed. On account of visual rhythm being a novel experience, it offers suggestions toward the general problem that are obscured in the introspection of the familiar auditory rhythms. The connection of rhythmic changes with muscular activity comes out more plainly; illusions due to rhythmic causes are separable, by means of tabular classification, from those due to other conditions; finally, rhythms of sight are especially helpful in tracing the genesis from simpler forms — the investigation here corroborates Squire's conclusion that the unaccented group is the most primitive. A silent electric contact wheel was devised to be used with a relay and incandescent lamp, in throwing a flash against a wall. With this apparatus it was possible to compare the appearance of a uniform series with one in which the lights varied in interval, duration or intensity.

The third part of the monograph includes two series of experiments in the motor expression of time intervals. Under a chance arrangment, intervals of 1, 2, 3, 4 and 6 seconds were reproduced once after each occurrence of the standard. The reproduction was made by two taps on a telegraph key. Tables are given for the results of five subjects making 100 reproductions of each interval. A marked difference (amounting on an average to half a second) is demonstrated between the reproduction of the intervals when the standard is bounded by like stimuli (two lights or two sounds) as compared with the same intervals when the standard consists of a light followed by a sound or vice versa. This result is in conformity with the theory held by Münsterberg and others that the reproduction of a time interval consists of an attempt to repeat the strain sensations remembered. The muscular adjustment following stimuli directed to different sense organs requires more effort and makes the interval seem longer. The change necessary in merely perceiving first a sound and then a light would hardly account for the pronounced lengthening which occurs. A new 'Carbon-Ribbon Kymograph' is shown. It utilizes the common typewriter ribbon in making records with electric pens on a telegraph-ticker tape. Records were obtained at Columbia University for 140 subjects making reproductions of a one-second interval continuously for 40 seconds. These records of their memory of a time interval were then correlated with the reaction times of the same subjects, using the Karl Pearson formula. A correlation of .55 brings out the interesting result that there is a strong tendency for slow reaction time to be found among those who shorten the interval most in reproducing it. This agrees with Seashore's suggestion that a brief interval probably seems shorter to the slow person than it does to the quick. The results are further discussed in connection with the sense of time and the theory of indifference points criticised.

Does an independent rhythm, which is kept up while we are working, hinder or aid us? In the last part of the thesis this question was answered, somewhat curiously, for groups of a hundred subjects, in respect to the effect of a metronome on a continuous choice reaction (distributing playing cards according to suits) and as to the effect of beating a rhythm with the fingers while filling words in blanks left in a poem. The correlations were .75 in the poem experiment and .32 and .39 in the choice experiment. These indicate an important principle which is suggestive for improving mental ability. Two classes of people are demonstrated, on whom the independent rhythm tends to have opposite effects. Those who were normally slow in the activities tested, tend to profit by the independent rhythmical stimulus. Those who were normally quick were seriously disturbed. The suggestion is made that the condition of very keen attention, found among the quick, is a more sensitive equilibrium which the secondary stimulus upsets. The slow person, on the contrary, is urged on by the accompanying rhythm.

THE AUTHOR.

COLUMBIA UNIVERSITY.

Sociality and Sympathy. J. W. L. JONES. Psychol. Rev., Mon. Sup., Vol. V., No. 1, April, 1903.

Sympathy is defined as the feeling accompanying a representation or memory state, when referred by the subject to an object. Sympathy in this view presupposes self-consciousness. It is involved in the conscious reference of a state of the subject to some object.

After comparing this definition with representative definitions of sympathy, the author analyzes what is presumably an instance of sympathy in order to show how the elements of the sympathetic state are contained in the definition given.

Imagination is not considered necessary to sympathy. I do not begin to sympathize by identifying myself with another. The sympathetic 'reference' is made, because, in the first place, the other person is identified with me. Imagination may, however, enter in as a factor of systematic or habitual sympathy.

The discussion of the definition is followed by an attempt to fix the rise of sympathy in the race and the individual. This is made by tracing out in the history of conscious experience, the causes that contribute to make a sympathy possible. The most fundamental factor in the state of consciousness immediately underlying a sympathy is the consciousness of kind, the recognition of oneself in an object. Genetically speaking, 'kind' means being of the same species, and the consciousness of kind signifies the consciousness involved in the various manifestations of the social relationship.

The social relationship is foreshadowed in 'passive association' (Spencer). The earliest form of sociality is the feeling of inclination, which creatures of a kind come to have for each other from simply living together. The social relationship proper arises out of organic imitation of one creature by another principally along the lines of defense. The defense may be by attitudes of flight with corresponding feelings of fear, or by resistance with corresponding feelings of anger. In the habitual attitudes of coöperative resistance to a hostile force is found the ground work of all social structure. At the earliest period they may be called collectively the instinct of mutual aid.

The rise of the sense of self and the representative consciousness with its memories and recognitions are important factors in the rise of the consciousness of kind. They do not, however, yield of themselves the consciousness of kind. For this there is required a situation in which one creature may become the object of another creature's deliberation in his own stead and not as a mere means to the other's safety or salvation. This situation is adumbrated in the phenomenon of play, and particularly in the restraints put upon play in the game. Especially in the restraints of play is found the way to conscious imitation of one creature by another, which at one and the same time develops the sense of self and gives rise to the consciousness of kind. Among the other factors that underlie sympathy, attention is called to the feeling of tenderness, which seems to be the sheer love of one's kind, as such. Being like sympathy, a feeling experienced in repose, it is always a potent coadjutor in the expression of sympathy.

The instinct of mutual aid, having as its ulterior end the well-being of the individual is inimical to sympathy with its elements of vicarious self-sacrifice. For this reason sympathy springs up and reaches also its richest fruition in the family. Vicarious sympathy or altruism, entailing as it does reference to another, involves, just so far as it takes the individual apart from his own self-seeking, a consciously initiated variation upon the instinct of mutual aid. Hence altruism in the gregarious relationship is always an uncertain quantity. Some creature preëminently social and sympathetic may at times become a copy for imitations, which will tend to override the blind selfishness of the stereotyped social individual. At such times in the struggle for existence, the individual is conscious of acting in behalf of others as well as in his own behalf. Nevertheless such activity is not thoroughly purposive. Nor is altruism of value to society until moral personality furnishes standards that do justice to both the ego and alter elements in individual activity. It is in sympathy with such a moral ideal that the individual makes his altruism become a factor in social progress.

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BIOLOGICAL.

Variation in Animals and Plants. H. M. VERNON. New York, Holt. 1903. Pp. ix + 415.

This book is a thorough going-over of the question of variation in its many aspects by a competent hand. It shows the present state of the problem — notably its unsettled state, pending the further application of exact statistical methods. On the whole, Vernon finds that research to date upholds the Darwinian conception, both in the matter of the distribution of variations and in that of their cause. He is also Darwinian in his opinion as to use-inheritance, although impressed with the force of the point that in certain cases the effects of environmental changes seem to be cumulative from generation to generation. To account for this — which is, by the way, even if proved, not specific enough to comfort the Lamarkians — Vernon urges the possibility of such changes being due to modification of the internal secretions which are supposed to affect the germ cells of the organism more readily than would other modifications. As Vernon himself notes, this suggestion had already been made by Delage. In the matter of the apparent operation of the use-inheritance principle, Vernon comes upon the idea — apparently independently — that has been expounded recently by others under the term 'organic selection'; and in this, too, he recognizes that the point had been made before by Lloyd Morgan, in 'Habit and Instinct'. On this point Vernon says: "Supposing for instance a number of organisms are more or less suddenly exposed to a considerable change of environment, whereby the majority of them are killed off. The survivors will be those which had the greatest power of adaptation [accommodation] to new surroundings, and though the somatic variations [modifications] will not be as such inherited, yet the survivors will be, on the whole, those organisms which originally possessed the largest proportion of the particular characters which have appeared as adaptive somatic variations * * * and hence the average hereditary characters of the survivors are in the direction of adaptation" (p. 390). This general solution of the determinate character of evolution is gaining ground steadily among biologists and psychologists and this opinion of Vernon will give to it further authority.

On the whole, readers of this book will find in it altogether the best general résumé on the topic of variation. The author also expounds certain original experimental results of his own. Certain points in the recently developed biometrical methods are given elementary statement for less technically educated readers. One of our reservations, however, is in the matter of terminology; it would seem to be a pity that Dr. Vernon does not adopt certain distinctions recently made in terminology, *e. g.*, that between variation and modification, that between adaptation and accommodation, that between development and evolution, etc.

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More Letters of Charles Darwin. FRANCIS DARWIN (editor), New York, Appletons. 1903. Pp. xxiv + 494, and viii + 508.

These two volumes, published in America uniformly with the Huxley letters and by the same house, are a most interesting edition to the literature of Darwinism. They are handsomely printed and illustrated with good portraits of many of Darwin's contemporaries.

The letters shed fine light upon the personality and personal relations of the great master of modern biology. Of especial interest on this side the water are the letters to American biologists.[1] Asa Gray stands out in high relief (I., 69 f) in Darwin's estimate of the contemporary Americans. We have also in these letters frank allusions to the neo-vitalistic movement led by Cope and Hyatt, and a confession of failure to understand, made in the polite tone which gives point to the charge of essential mysticism (I., 341 ff).

Among the interesting side-lights thrown upon questions still under discussion the present writer notes the views of Darwin on 'saltation' or abrupt variation, and on inter-specific sterility. As to the former, it is well to remember that Darwin made extended inquiries as to the possible origin of strains breeding true from sport variation, and at the same times saw that such cases were rare and not available for a general theory of descent (see letters 95, 169, etc.). In the following words, written to Lyell in 1860 (letter 95), he anticipates a valid criticism against the extreme mutationists, *e. g.*, Scott. He says: "Harvey does not see that if only a few (as he supposes) of the seedlings being [are] monstrosities [abrupt variations], natural selection would be necessary to select and preserve them." These words would be subscribed to indeed by DeVries. Again he says, writing to Harvey (letter 110): "About sudden jumps: I have no objection to them — they would aid me in some cases. All I can say is that I went into the subject, and found no evidence to make me believe in jumps; and a good deal pointing in the other direction." And in a letter to Hooker (No. 135, 1862): "I am not shaken about *saltus*. I did not write without going pretty carefully into all the cases of normal structure in animals resembling monstrosities[2] which appear *per saltus*."

In the following remark, from letter No. 169 to Asa Gray, Darwin makes the point in regard to co-adaptations coming by chance in a single variation, which has been so strongly urged recently against natural selection in general (*e. g.*, by Romanes and others in the case of instinct; the further argument being that incomplete beginnings of such functions would be of no use); he says: "Do you not consider such cases as all the orchids next thing to a demonstration against Heer's view of species arising suddenly by monstrosities? — it is impossible to imagine so many co-adaptations being formed all by a chance blow. Of course, creationists would cut the enigma."

As to the sterility of species *inter se*, the fluctuations of opinion in

[1] In a recent issue of the *Pop. Sci. Monthly*, many of these American letters were given advance publication.

[2] Darwin used 'monstrosity' for all cases of extreme or 'sport' variation.

Darwin's mind clearly appear — notably the reiterated conviction that the subject was too obscure for a definite belief (see the most interesting letter 92, also 156, 157, all to Huxley). He often expresses the opinion that sterility may be eliminated by domestication, the exact adjustments of the two species to different environments (from which the sterility arose) no longer being maintained; and that it is for this reason that varieties produced by artificial selection — being practically domesticated — do not show the sterility *inter se* which natural species do. In one place however he makes a remark which, as the editors of the work point out (note to letter 153), anticipates Romanes' way (physiological selection) of accounting for such sterility by original variation; a suggestion also made by Darwin in a letter to Huxley printed in the 'Life and Letters,' II., p. 384.[1] He suggests experiments of the subject (letters 153, 154, etc.). In letter 157 he remarks (pursuing the idea about domestication mentioned above): "There must be something in domestication — perhaps the less stable conditions, the very cause which induces so much variability — which eliminates the natural sterility of species when crossed. If so, we can see how unlikely that sterility should arise between domestic races." The passage I cite, moreover, to suggest — what Darwin apparently does not mean — that just the increased variability which he notes in domesticated animals, and which would extend to the reproductive organs, may be the occasion of the absence of sterility — by giving more chances for the preservation of fertility under conditions of increasing divergence, under which in nature, with less variation, it would not be preserved. Possibly this suggestion has already been made by some one — it may be by Darwin himself elsewhere — but I have not met with it.[2]

Psychologists will be interested in Darwin's enthusiasm for child study, as shown in the following (letter 208, to Huxley): "Give

[1] The extended arguments of Darwin and Wallace given in letters 208–214 (especially Wallace's, No. 211) involve, indeed, a pretty complete statement of 'Physiological Selection.'

[2] The present writer has recently suggested that the progression in the teeth in fossils cited by paleontologists to prove determinate variation without natural selection (the early stages not being useful or not being in the direction of individual use), may have proceeded in correlation with muscular or other adaptive functions. Darwin makes about the same point (letter 143, to Falconer): "To explain how I imagine the teeth of your elephants change I should look at the change as indirectly resulting from changes in the form of the jaws, or from the development of tusks, or in the case of the *primigenius* even from correlation with the woolly coverings; in all cases natural selection checking the variation."

Mrs. Huxley the enclosed [queries on expression], and ask her to look out when one of her children is struggling and just going to burst out crying. A dear young lady near here plagued a very young child for my sake till it cried and I saw the eyebrows, for a second or two, beautifully oblique just before the torrent of tears began."

Other matters worth noting are (and many besides might be written down) the opinions of John Scott, Darwin, and Huxley (letter 151, with the editors' notes), on the idea embodied in Weismann's theory of amphimixia (variability due to union of different sexual elements), in connection with which the need of the conservation of the mean, later formulated in Galton's principle of regression, seems to have impressed Darwin. "I see in the *Cornhill Magazine*, a notice of a work by Cohn on the contractile tissues of plants. * * * I am much interested in the subject and experimented a little on it this summer, and came to the conclusion that plants must contain some substance most closely analogous to the supposed diffused nervous matter in the lower animals."

Darwin's relation to Wallace is also brought out further in certain of the letters. We read of another incident in which the large-heartedness and generosity of Darwin appears in the matter of their common researches on sexual selection (II., 59 ff). Darwin's conscience seemed to have given him no peace after he had quite naturally suggested that Wallace was anticipating him; and he insisted on making amends and asking forgiveness. On the other side, Wallace is noble too; he offered to give his material to Darwin — a nobility shown so handsomely in his giving to his own book, expounding his own statement of natural selection, the title *Darwinism*. The relation of the two men is in all respects perhaps the finest lesson in the ethics of scientific discovery which was ever acted out; and how mean it is to cite Charles Darwin as a case of the atrophy of the finer sentiments on the basis of the passage, so much quoted, in which by his very self-depreciation, he is but illustrating that personal humility which is one of the finest of these sentiments! He may not have cared to read poetry, but he had that spiritual vision of the higher values of life which 'poets' so often degrade in practise.

These volumes should be put reverently on the library shelf within easy reach, along with the Huxley *Life and Letters*, by every one who loves honesty allied with strength.

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NEW BOOKS.

Le Goût. L. MARCHAND. Bibl. de Psych. expér. Paris, Doin. 1903. Pp. 332. 4 fr.

Studies in the Evolution of Industrial Society. R. T. ELY. The Citizen's Library, New York and London, Macmillans. 1903. Pp. xviii + 497. (An extremely interesting series of discussions, characterized by broad philosophical points of view.)—J. M. B.

Essai sur la Psycho-Physiologie des Monstres humains: Un anencéphale — un xiphopage. N. VASCHIDE and CL. URPAS. Paris, Rudeval. 1903. Pp. 294.

Studien zur Methodenlehre und Erkenntnisskritik. FRIEDRICH DREYER. Leipzig, Engelmann. Bd. I. 1895. Pp. xiii + 222. Bd. II. 1903. Pp. xxi + 498.

Etude expérimentale de l'Intelligence. A. BINET. Paris, Schleicher. 1903. Pp. 309. 6 fr.

Un Médicin philosophe au XVI^e Siècle (Jean Fernel). L. FIGARD. Paris, Alcan. 1903. Pp. 365. 7 fr. 50.

Vers le positivisme absolu par l'idealisme. LOUIS WEBER. Paris, Alcan. 1903. Pp. 396. 7 fr. 50.

L'Année philosophique. 13^e Année (1902). F. PILLON. Paris, Alcan. 1903. Pp. 306. 5 fr.

Saggio di uno Studio sui Sentimenti Morali. G. SALVADORI. Florence, Lumachi. 1903. Pp. 138.

Ueber die Bedeutung des Darwin'schen Selectionsprincips und Probleme der Artbildung. L. PLATE. 2te Auf. Leipzig, Engelmann. 1903. Pp. viii + 247. 5 M.

Sexual Dimorphism in the Animal Kingdon. J. T. CUNNINGHAM. London, Black; New York, Macmillans. 1900. Pp. xi + 317.

Études de Psychologie physiologique et pathologique. E. GLEY. Paris, Alcan. 1903. Pp. viii + 335. 5 fr. (A collection of papers including M. Gley's well-known researches on the 'muscular sense' and 'unconscious movements.')

Swain School Lectures. A. INGRAHAM. Chicago, Open Court Publishing Company. 1903. Pp. 197. $1.

Mental Traits of Sex. HELEN B. THOMPSON. Chicago, University of Chicago Press. 1903. Pp. vii + 188.

Lehrbuch der Wädschenerziehung. MARIE MARTIN. Leipzig, Dürrschen Buchhandlung. 1903. Pp. viii + 188.

NOTES.

WE are glad to learn that Professor William James' 'Varieties of Religious Experience' is being translated into French.

DR. ALBERT LEFEVRE has been appointed to the chair in Philosophy in Tulane University.

La Revue de Philosophie announces the issue of an *Annuaire des Philosophes* — devoted to men and things philosophical — under the charge of M. N. Vaschide, the energetic *Chef de Traveaux* of the Hautes-Études psychological laboratory. The *Revue* has issued a 'questionnaire' to philosophers, asking for various personal and professional details. As the success of such a publication depends upon its completeness we bespeak a courteous response to this circular; if revised at intervals it would constitute an interesting *Vade mecum.* (Address M. N. Vaschide, 56, rue Notre-Dame-des-Champs, Paris.)

IT is announced that the Fourth International Congress of Psychology is to meet at Rome in the spring of 1905 instead of the autumn of 1904, since the latter date would cause conflict with the Congress of Physiologists at Brussels.

WE also have to announce that the second International Congress of Philosophy is to be held at Geneva, in the first week of September, 1904. It is possibly not generally known that the scientific congresses of the St. Louis Exposition which are also to meet in the early autumn of 1904, will include all the mental, moral, and philosophical disciplines. It is to be hoped that the committees will arrange some sort of coöperation between the St. Louis and the Geneva Congresses.

WE regret to announce the death of M. Ernest Murisier, Professor of Philosophy at Neuchâtel.

WE have received the announcement of a projected *Journal de Psychologie pour la France et pour l'E'tranger*, to be edited by MM.

Pierre Janet and G. Dumas, and published by M. Felix Alcan, Paris (first No. Dec., 1903). It is to be issued bimonthly (about 144 pp.), and is to cover the whole field of psychology, giving full accounts of the literature.

THE Scribners announced that the following volumes have already been arranged for in Professor Baldwin's 'Library of Historical Psychology': 'Memory and Imagination,' by Professor G. F. Stout, (St. Andrews); 'Logical Processes,' by Professor John Dewey, (Chicago); 'Self-Consciousness and Personality,' by Professor Josiah Royce (Harvard); 'Sensation,' by C. Ladd Franklin (Baltimore); 'Æsthetics,' by Professor J. H. Tufts (Chicago); 'Epochs and Problems on the History of Psychology,' by the Editor. Other volumes are to be announced shortly.

IT is also proposed to issue in the same historical series a volume of important psychological texts — reprints of papers which are important to preserve and which have become rare or inaccessible. The original languages will be preserved except, possibly, in certain cases. The editor of the series requests suggestions, from any one interested, as to texts which it would be well to include (address J. Mark Baldwin, Princeton, N. J.).

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The Psychological Review.

THE PERSONAL SOURCES OF CHRISTIAN SCIENCE.[1]

BY PROFESSOR I. WOODBRIDGE RILEY,

University of New Brunswick, Fredericton, Canada.

"I am a Christian Scientist, the founder of this System of Religion, — widely known, one readily sees that this science has distanced all other religions and pathological systems for physical and moral reformation." These words of Mrs. Eddy furnish a proper point of view for an examination of her cult. A study of the sources of Christian Science is the study of a certain sort of personality. Were the psychology of religion to be put in pathological terms, here indeed is a 'variety of religious experience' — a discordant inner life, a liability to obsessions and fixed ideas, a falling into trances, a series of mental overtensions resulting in self-importance exaggerated to a disease.

Mary Baker was born about 1820, at Bow, New Hampshire, within sight of her present home at Pleasant View, Concord. According to her autobiographical reminiscences entitled 'Introspection and Retrospection,' she counts among her ancestors Scotch Covenanters and English Dissenters, the hero Wallace

[1]References: 'Science and Health,' by Mary Baker Glover, first edition, Boston, 1875; third edition, Boston, 1881, especially the chapter on 'Demonology.' Later editions of 'Science and Health with Key to the Scriptures,' by Mary Baker G. Eddy, are not here used because of their various transpositions, additions and suppressions of compromising contents. 'Miscellaneous Writings,' 1883–1896, Boston, 1899, contain Mrs. Eddy's various articles from the *Christian Science Journal*. 'Retrospection and Introspection,' Boston, 1900.

'The Philosophy of P. P. Quimby,' by A. G. Dresser, Boston, 1899. 'The True History of Mental Science,' by J. A. Dresser, Boston, 1899. Local histories of Concord, N. H., Lynn, Mass., Portland, Me.

and the poetess Hannah More. But the local histories of her native state do not bear out this Pumpernickel pretence. Unfortunately for the pathologist there is little to be gleaned here concerning the family history. However, the young religionist herself described her father as of a strong intellect and an iron will; her mother as a great bible reader and believer in miracles. Thus it is evident that the auditory hallucinations of the daughter were considered veritable calls from heaven. In the section designated as 'Voices not Our Own' there occurs this passage: "For some months, when I was about eight years old, I repeatedly heard a voice, calling me distinctly by name, three times in an ascending scale. I thought this was my mother's voice, and sometimes went to her, beseeching her to tell me what she wanted. * * * This continued until I grew discouraged, and my mother was perplexed and anxious. * * * That night, before going to rest, my mother read to me the scriptural narrative of little Samuel, and bade me, when the voice called again, to reply as he did."

During Mary Baker's childhood her parents removed to the town of Tilton, eighteen miles from Concord and adjacent to the Shaker community of Canterbury. The pervasive influence of these mystic neighbors and their works upon the impressionable mind of the youthful seeress is to be considered later. But Mary Baker's educational opportunities were by no means as scanty as those of the contemporary visionaries and religious founders like Joseph Smith, the Mormon prophet, and Andrew Jackson Davis, the Poughkeepsie Seer, who both wrote not wisely but at length. The incipient Christian Scientist has herself said: "At ten years of age I was as familiar with Lindley Murray's Grammar as with the Westminister Catechism; and the latter I had to repeat every Sunday. My favorite studies were Natural Philosopy, Logic and Moral Science. From my brother Albert I received lessons in the ancient tongues, Hebrew, Greek and Latin. My brother studied Hebrew during his college vacations. After my discovery of Christian Science, most of the knowledge I had gleaned from school books vanished like a dream." The last is the saving clause, the explanation of a paradox. In parts of her writings Mrs. Eddy borrowed her learning and forgot the sources; in other parts she remembered the sources and forgot her learning. In the former case the amnesia appears real, in the latter fictitious: blank ignorance alone would account for such curious etymologies as are found in the first edition of 'Science and Health,' where Pantheism is derived 'from the Greek god Pan' and Adam 'from the Latin demens.' Without referring to what Voltaire said of comparative philology, it is clear that Mary Baker was nothing if not imaginative. It is therefore hard to say how far the constitutional romancer appears in what she called 'theological reminiscence.' Here she tells how, at the age of twelve, she was admitted to the Congregational (Unitarian) Church, but only after suffering from a fever superinduced by the doctrine of Unconditional Election. Her father's relentless theology, she relates, 'emphasized belief in a final Judgment Day, in the danger of endless punishment, and in a Jehovah merciless towards unbelievers; and of these things he now spoke, hoping to win me from dreaded heresy.' On the other hand it was through the mother's tenderness that the 'horrible degree' of Predestination was exorcised: "She bathed my burning temples * * * I prayed; and a soft glow of ineffable joy came over me. The fever was gone, and I rose and dressed myself, in a normal condition of health." In this personal experience there perhaps lies the key to the curious Eddyite doctrine of the superiority of the feminine principle in matters of religion. At any rate, in terms of the psychology of religion, this conversion was of the optimistic type. It was also precocious; two years before the average of adolescence the change came and Mary Baker left her old church because she was 'not satisfied with a man-like God.'

Succeeding this time of spiritual unrest and recovery there came a period of physical suffering: "When quite a child we adopted the Graham system for dyspepsia, ate only bread and vegetables; and drank water. Following this diet for years we became more dyspeptic, however, and, of course, thought we must diet more rigidly; so we partook of but one meal in twenty-four hours, and this consisted of a thin slice of bread, about three inches square, without water; our physician not allowing us, with this ample meal, to wet our parched lips for many hours thereafter; whenever we drank, it produced violent retchings. Thus we passed most of our early years, as many can attest, in hunger, pain, weakness and starvation."

Perturbation over theological infelicities, accentuated by ascetic practices, had as a correlate the visionary seizure. "This phenomenon of spiritual discernment," Mrs. Eddy has recently said, "appeared in my childhood, is associated with my earliest memories and has increased with my spiritual increase. It has aided me in healing the sick, and subordinating the human to the Divine. While this metaphysical phenomenon puzzles poor philosophy, and is not in the slightest degree theosophy, hypnotism, clairvoyance or an element of the human mind, I regard it as a component part of the Science of Mind not yet understood." It is significant that this condition of ecstasy, for such it will appear to be, occurred in a locality infested by an American species of illuminati. The author of the 'Autobiography of a Shaker,' who had turned from being a materialist to being a new light, tells how "one night I heard a rustling sound and found that a singular mental phenomenon was going on. I was positively illuminated. My reasoning powers were enhanced a hundred-fold. I came to a conception of the inner world as being the most substantial, and of the inner man as being the real man; the outward world being only the shadow of the invisible world of causation."

The period of mysticism was followed by a period of invalidism; the quietism of malnutrition by the restless pursuit of health. After her first marriage in 1843 the anæmic visionary tried in term allopathy, homeopathy, hydropathy, electricity, spiritualism and mesmerism. These 'medical experiments' in what were bitterly denounced as the various 'isms of mortal mind were succeeded by the 'emergence into light.' "The trend of human life was too eventful to leave me undisturbed in the illusion that this so-called life could be a real and abiding rest." In these health cure experiences is to be found the first step toward the 'great discovery' of 1866. As long ago as 1844, Mrs. Eddy asserts, she was convinced that mortal mind produced all disease, and that the various medical systems were in no proper sense scientific. The further statement that for twenty years prior to the discovery there was an attempt to trace all physical effects to a mental cause has an ex-post-facto air. As will be seen, the Eddyite system of mental healing was more of an invention than a discovery, was due rather to chance than to cogitation. Nevertheless, the physical side of these experiences may be accepted as valid. They betray, as nothing else could do, the psychopathic condition of the woman. Early in life she had acquired what might be called the sanitarium habit. She next took up vicious schemes of self-cure and began her dabblings in mental healing. Out of these grew an abnormal sensibility. She became squeamish about herself and oversensitive toward others. "We were an homeopathist because of our aversion to the dissecting room. * * * In years past we suffered greatly for the sick." What Mrs. Eddy terms the 'perpetually egotistical sensibility' is doubtless a reason for her present refusal to attempt the cure of others. The mental exertion, the concentrated attention needful for a demonstration would bring a return of this morbid self-consciousness. Hence she says: 'My own personality afflicteth me not willingly, for I desire never to think of it.'

Further details of these days of the search after health are given in certain suppressed letters. In 1861 Mrs. Eddy's second husband, Dr. Patterson, wrote to Dr. Quimby, a magnetic healer of Maine, asking aid of Quimby's 'wonderful power' in behalf of his wife, who had been for six years an invalid unable to sit up. Mrs. Patterson next writes herself from a New Hampshire water cure that she hopes to reach Quimby, as she is 'sufficiently excitable' to live through the journey. That this correspondence is substantially true is borne out by further letters in the public prints. As to her invalidism she says: "In 1862 my name was Patterson; my husband, Dr. Patterson, a distinguished dentist. After our marriage I was confined to my bed with a severe illness, and seldom left bed or room for seven years, when I was taken to Dr. Quimby, and partially restored." But regarding the success of this mental healer, Mrs. Patterson had given more positive testimony in the Portland *Courier* of 1862. "Three weeks since, I quitted my nurse and sick-room en route for Portland. The belief of my recovery had died out of the hearts of those who were most anxious for it. With this mental and physical depression I first visited P. P. Quimby, and in less than one week from that time I ascended by a stairway of one hundred and eighty-two steps to the dome of the City Hall and am improving *ad infinitum*."

According to her 'Miscellaneous Writings,' that *journal intime* for the faithful, the mid-period of Mrs. Eddy's life was a chapter of accidents. She married in haste and repented in the divorce courts; she met the man from whom she is alleged to have pilfered her system of drugless healing; she finally suffered a fall which injured her seriously. Four years after this there befell her an accident, the effects of which are now visible in a lameness of limb and a palsied shaking of the head. A letter of 1866 from Lynn, Massachusetts, to a pupil of Quimby reads in part thus: "Two weeks ago I fell on the sidewalk and struck my back on the ice, and was taken up for dead, came to consciousness amid a storm of vapors from cologne, chloroform, ether, camphor, etc., but to find myself the helpless cripple I was before I saw Dr. Quimby. The physician attending said I had taken the last step I ever should, but in two days I got out of my bed *alone* and *will* walk; but yet I confess I am frightened, and out of that nervous heat my friends are forming, spite of me, the terrible spinal affection from which I have suffered so long and hopelessly. * * * Now can't *you* help me? I believe you can. I write this with this feeling: I think that I could help another in my condition if they had not placed their intelligence in matter. This I have not done, and yet I am slowly failing. Won't you write me if you will undertake for me if I can get to you?"

In passing to the fourth period in the life of the founder of Christian Science we may employ a phrase from Hawthorne's romance of transcendentalism: "The weakly maiden, whose tremulous nerves endow her with Sibylline attributes, has now become the Veiled Lady." This desire to be mysterious explains the discrepancies between the earlier and later accounts of the accident. The time of recovery was made more rapid, the process more magic, until it was counted the turning point leading to the 'great Discovery of the spiritual science of

Mind-healing.' According to the original document the patient was still suffering two weeks after the fall. Another version says that, three days after her accident was pronounced fatal, she read Matthew IX., 2, and thereby learned that life was the sole reality of existence, that mortal thought evolves a subjective state which it names matter. The third and fourth accounts should be given at length, since the one exhibits the writer's manner of qualifying previous damaging statements, and the other presents the developed theory of metaphysical healing. "At Swampscott, Mass., in 1866, we recovered in a moment of time from a severe accident, considered fatal by the regular physicians, and regained the internal action that had stopped and the use of our limbs that were palsied. To us this demonstration was the opening of the new era of Christian Science. We then gained a proof that the principle, or life of man, is a divine intelligence and power, which, understood, can heal all diseases, and reveals the basis of man's immortality. But the minds around us at that time were unacquainted with our mental theory." The fourth account is the fullest and has a glossary tacked to it. "My immediate recovery from the effects of an injury caused by an accident, an injury that neither medicine nor surgery could reach, was the falling apple that led me to the discovery how to be well myself, and how to make others so. Even to the homeopathic physician who attended me, and rejoiced in my recovery, I could not then explain the *modus* of my relief. I could only assure him that Divine Spirit had wrought the miracle — a miracle which later I found to be in perfect scientific accord with divine law. I then withdrew from society about three years, to ponder my mission, to search the Scriptures, to find the science of mind, that should take the things of God and show them to the creature, and reveal the great curative principle, Deity. The Bible was my text-book. It answered my questions as to how I was healed; but the scriptures had to me a new meaning, a new tongue. Their spiritual signification appeared; and I apprehended for the first time, in their spiritual meaning, Jesus's teaching and demonstration, and the principle and rule of spiritual science and metaphysical healing, in a word, Christian Science. I named it *Christian*, because it is compassionate, helpful and spiritual. God I called *Immortal Mind*. That which sins, suffers and dies I named *mortal* mind. The physical senses, or sensuous nature, I called *error* and *shadow*. Soul I denominated *substance*, because soul alone is truly substantial. God I characterized as individual entity, but his corporeality I denied. The real I claimed as eternal; and its antipodes, or the temporal, I described as unreal. Spirit I called the *reality*, and matter, the *unreality*."

The humdrum sources of this curious amalgam of medicine, metaphysics and religion are to be traced with some exactness. Mr. Eddy's own opinion of her system should first be given. She declared it to be the 'first purely metaphysical system of healing since Apostolic days,' founded on a 'basis so hopelessly original' that all other systems of mental healing are plagiarisms from "the precious book 'Science and Health.'" As to the origin of this volume Mrs. Eddy explains: "I should blush to write of 'Science and Health with Key to the Scriptures' as I have, were it of human origin, and I, apart from God, its author; but as I was only a scribe echoing the harmonies of Heaven in divine metaphysics, I cannot be supermodest of the Christian Science text-book."

But this is anticipating: before the period of printer's ink and money-making there was a period of struggle and fear. Before 'Science and Health' reached its three hundredth edition the rediscoverer of 'divine' healing was making a precarious living as a mental healer by performing 'recuperative incidents.' By 1877, the year of her marriage to Mr. Eddy, she was apparently in a state of panic dread lest she be hoist with her own petard. When the hopeless invalid besought the magnetizer Quimby to visit her in his 'omnipresence,' she evidently believed that mental therapeutics was both local and long distance. This anticipated doctrine of telepathy, by a peculiar sort of feminine logic, was considered a reversible doctrine. By the 'law of opposites' it might do evil as well as good: "A mind taught its power to touch other minds by the transference of thought, for ends of restoration from sickness, — this mind, by misusing its freedom, reaches the degree of total moral depravity. * * * In coming years the person or mind that hates his neighbor will have no need to traverse his fields, to destroy his flocks and herds, and spoil his vines; or to enter his house to demoralize his household; for the evil mind will do this through mesmerism; and not in propria personæ be seen committing the deed. Unless this terrible hour be met and restrained by science, mesmerism, that scourge of men, will leave nothing sacred when mind begs to act under direction of conscious power." It is interesting to note that Mrs. Eddy acknowledged her doctrine of malicious animal magnetism to be a sort of recrudescence of the Salem witchcraft. More pertinent is the evidence of her earlier writings as to her pathological state. "Our readers," she says in the third edition of her text-book, "may feel an interest to learn something of the indications of this mental malpractice, or demonology. So far as we have traced it, it has not outward signs, such as ordinarily indicate mesmerism, and its effects are far more subtle, because of this. Its tendency is to sour the disposition, to occasion great fear of disease, dread and discouragement, to cause a relapse of former diseases, to produce new ones, to create dislikes or indifference to friends, to produce sufferings in the head, in fine, every evil that demonology includes and that metaphysics destroys. If it be students of ours whom he attacks, the mental malpractitioner tries first to produce in their minds a hatred of us, even as the assassin puts out the light before committing his deed."

But to leave this Iliad of woes. The successive stages of adolescent brooding, of trance, of invalidism, of experiments in health cures, of social ostracism had culminated in a state of neural instability and morbid subjectivism. Now the busy objective world relieved the woman from what might have been serious mental impairment. Mrs. Eddy's native ability and Yankee shrewdness had a chance to assert itself, and Boston saved her brain. Soon after the founding of the Church of Christ, Scientist, considerable forethought was shown in the substitution of readers for preachers and in the frequent disbanding of the organization. Personal exposition, private interpretation of the new Scriptures might have led to heresy and schism. Hence the order went forth that the pastors of the church were not to expound but simply to read 'Science and Health' and always in the latest edition. Then there was the scheme of local decentralization — a scheme, as some one has cynically observed, as effective in preventing disaffection as the constant disbanding of a Boer commando. These measures cannot positively be taken as a test of the mental calibre of the veiled prophetess; they may have emanated from her male advisers. Yet she herself originated one of the most paying of the Christian Science institutions. In 1867 she began by teaching one student Christian Science Mind-healing. From this seed grew the Massachusetts Metaphysical College in Boston, chartered in 1881. This institution for drugless healing was successfully opposed by the regular practitioners, the matter-physicians not the meta-physicians as they were dubbed. It was closed by the so-called anti-diploma law of 1883, 'after accomplishing the greatest work of the ages,' said its originator. It throws some light on the character of the spiritual healer to read her explanation of the size of her fees. "When God impelled me to set a price on Christian Science Mind-healing, I could think of no financial equivalent for the impartation of a knowledge of that divine power which heals; but I was led to name three hundred dollars as the price for each pupil in one course of lessons at my college; a startling sum for tuition lasting barely three weeks. This amount greatly troubled me. I shrank from asking it, but was finally led by a strange Providence to accept this fee. God has since shown me in multitudinous ways the wisdom of this decision."

This period of prosperity needs a further word of description as introductory to the last period in the life of Mother Eddy, the period of practical deification. In this we are not concerned with the mentality of the follower but of the founder. The obligatory purchase by each disciple of the latest expensive edition of the text-book, the sending of five-hundred-dollar cheques for absent treatment may be cause for astonishment. Yet back of the financial there was the psychic element. That Mrs. Eddy has been successful in treating many nervous, non-organic troubles cannot be denied. For all that there are strange contradictions in the business. That she and her devotees could not inhibit by will power the activities of the wilful microbe is virtually acknowledged in the recent manifesto from Concord; that in case of contagious diseases, and in order to keep within the law, a regular practitioner may be called in. However, the arch-healer still holds to her assertions that she has cured consumption in its last stages, the lungs being mostly consumed; that she has healed carious bones which could be indented with the fingers, while the doctors were preparing their instruments for operation; and that she has healed at one visit a cancer that had so eaten into the flesh of the neck as to expose the jugular vein, so that it stood out like a cord.

Suggestive therapeutics, the mental factor in control of disordered physiological functions, goes far to explain the last stage in the life of the high priestess, her elevation to a more than human plane. Thus a disciple tells how, in a public meeting in 1888, the audience rose after Mrs. Eddy's address; the people were in the presence of the woman whose book had healed them, and they knew it. They came in crowds to her side begging for one hand clasp. A mother held her sick baby up. Others touched the dress of their benefactor. A palsied woman held up her shaking hands and went away healed. The other side of this picture is given by the thaumaturgist herself. "It was not uncommon in my own church for the sick to be healed by my sermon. Many pale cripples went into the church leaning on crutches who went out carrying them on their shoulders."

In subsequent meetings Mrs. Eddy simply appeared on the stage, bowed, and disappeared. In 1889 she withdrew to her native state and her invisibility increased her reputation. Lest by any means this withdrawal should mean a loss of influence, in the absence of the sacred person, the sacred thing was substituted. A modern form of fetish worship arose. There was the Mother Eddy souvenir spoon which the faithful were urged to buy and mark for their inward digestions. There was also the booklet entitled 'Christ and Christmas' in which the author is represented with a halo around her head. Of this one reader asserted, 'looking at the pictures in your wonderful book has healed my child.'

Another step in the process of deification now occurred. Mrs. Eddy claimed inspiration for her utterances. In her 'Miscellaneous Writings' she says, "the works I have written on Christian Science contain absolute truth. * * * Christian Science is the soul of divine philosophy and there is no other philosophy. * * * It is not a search after wisdom, it is wisdom. * * * When God speaks I listen." The final step in this process was the assumption of a superhuman personality. As 'Mother' Eddy she has cautiously but virtually acknowledged herself to to be the 'feminine principle of the Messianic expectation.' For example, she says: "Jesus' immortal words were articulated in a dead language. Christian Science was to interpret them and woman to awaken the dull senses. * * * Jesus was the masculine representation of the spiritual idea. The Divine Science was the immaculate idea represented first by man and last by woman. * * * No person can take the place of the author of 'Science and Health,' the discoverer and founder of Christian Science."

Thus far Mrs. Eddy has given this picture of herself: in youth indifferently educated, anæmic and subject to trances; in middle age an unhappy invalid whose condition was ameliorated by the suggested mental treatment of another and by her own strong will; lastly in old age the acknowledged leader of what she styles 'the seventh modern wonder, a mighty system of metaphysical healing.' The task now before us is to compare this system with various contemporary records and show how the more bizarre doctrines of the day worked their devious way into the mind of the New Hampshire seeress. The doctrines may be intrinsically worthless, but their excogitation affords a glimpse of the mental processes of a sectary. Primitive Christian Science, as given in the first edition of 'Science and Health,' was a back-door philosophy. Its sources were provincial, its derivation accidental. To put some method into its madness, in a tentative way its theology may be traced to the Shakers; its therapeutics to the mesmerists; its metaphysics to the homeopathists. That this eclecticism had a dash of New England transcendentalism is true only if we accept Emerson's definition of that movement as the Saturnalia or excess of faith, wanting the restraining grace of common sense. Furthermore, this summary statement is to be modified by the personal equation. Christian Science was of the feminine-mystic type; it was expounded by the 'Reverend' Mary Baker Glover Eddy. Its method of cure was hieratic; it was 'a pathology of spiritual power, a science to meet sin and uncover it.' Lastly, its philosophy, if such it had, was neurotic. It was a crude immaterialism based on youthful hallucinations and persistent trances.

The whole system was called by its inventor 'hopelessly original.' In truth it was to one ignorant of the elusive courses of abnormal psychic visitations. As to the earlier lucubrations there is no need of charging Mary Baker with downright prevarication. The most unique of her philosophizings appear to have been the product of unconscious mentation, expressed in terms of a perverted emotional sensibility. In two respects she resembled Emerson's description of the heroine of the 'Blithedale Romance'; she was self-deceived by her own phantasms; she possessed the dangerous vertigo nature. Along with this abnormal personality we may expect the concomitants of a more or less subnormal activity. There was a youthful amnesia; there was also a certain subconscious retention of vaguely apprehended fragments of knowledge. Mrs. Eddy's 'spiritual syntax' was said to be due to the fact that Mary Baker's book-knowledge 'vanished like a dream.' At the same time in the production of the sacred writings there was the insistence of the fixed idea, the claim of an impulsion 'by a power not one's own.' That there was here mentation of an automatic order the writer evidently had no inkling. Whether the first pastor of the Church of Christ, Scientist, is to be considered an inspirational speaker depends upon the attitude of the hearer. Nevertheless, her books still bear the ear-marks of inspirational writing, vapid generalization, vain repetition and crude alliteration: 'matter is manifest mortal mind * * * pleasure and pain are mists of mortal mind.' If these tricks of style betray a poverty of normal thought, there were other ways of getting ideas. A half unconscious borrowing of current opinions explains the welter of notions in the early documents. So to return to origins and to trace the first of the sources of Christian Science.

To the 'unspiritual and ungodly' the Eddyite theology is said to be 'dark and difficult'; to the psychologist it appears but the recrudescence of latent memories, the more or less unconscious transcription of stored impressions. Bearing in mind that during the period of her youthful trances Mary Baker lived near the Shakers, it is to be presumed that she unwittingly imbibed many of the doctrines of this peculiar people. At the outset there are many minor resemblances between 'Science and Health with Key to the Scriptures' and the 'Holy, Sacred and Divine Roll and Book of the United Society of Believers.' There is the same tirade against rationalism and materialism, the same symbolical interpretation of the Bible, the same prediction of a last dispensation of spiritual healing. This manual of the 'Church of Jesus Christ and Mother Ann' also claims that 'Shakerism is the only religious system that teaches Science by Divine Revelation.'

But chapters sixteen and seventeen afford the most striking analogies. They are headed thus: "Christ's second appearing in the female, her office as Spiritual Mother. The Female had never been revealed in her sacred order before this dispensation. * * * Of what was manifested in Mother. In her was displayed my sacred and saving power to search the hearts and penetrate into the souls of such as came before her." Of the Englishwoman Mother Ann Lee's pathological experiences — her trances, visions and distressing ascetic practices, it is unnecessary to speak. Our concern is with the strange reappearance of her doctrines and their incorporation into an American cult. We need only put in brief parallels some of the statements of Shakerism and Eddyism to see how intimately connected they are. To Ann Lee was given 'the endearing term of Mother'; to Mrs. Eddy was given 'the endearing term of Mother.' To Ann Lee, the Woman clothed with the Sun was 'the divine spiritual intuition as representing the Mother in Deity'; to Mrs. Eddy she was 'the spiritual idea or type of God's Motherhood.' To Ann Lee marriage was 'the celibate spiritual union of the sexes'; to Mrs. Eddy it was 'the spiritual unity of male and female.'

Here, it appears to me, is the clue to the curious dualistic notions found in Christian Science. Like Joseph Smith and his 'Book of Mormon,' Mary Baker inadvertently absorbed certain environmental beliefs of the baser sort. In another Shaker compendium are given the grounds of this dualism. "In the last cycle," it was asserted, "Christ would come again to some other individual. This second coming must of necessity have been to a woman because the race is female as well as male."[1]

The second source of Christian Science was mesmerism and the allied methods of mental healing. Mrs. Eddy acknowledges as much: 'Take away the theology of mental healing and you take away its science, leaving it a human mind cure that produces the effect of mesmerism.' The writer's familiarity with this black art of the day has been already mentioned. How it reached her is a bit of back stairs history. Animal magnetism proper had been discredited throughout the land by the efforts of Benjamin Franklin, who was one of those signing the Report of the Commission of the French Academy of Sciences. But two generations later it gained a footing in America. It was in 1837 that a Frenchman, Charles Poyen, who had been cured 'mesmerically' of a nervous disorder, published his 'Progress of Animal Magnetism in New England.' In this he tells of his travels in the very towns of Massachusetts, Maine and New Hampshire, where Mrs. Eddy later lectured and practised. That she was conversant with what he taught is likely from the similarity between his six mesmeric phenomena and the elements of her teaching. His 'suspension of the external sensibility' was her 'unconsciousness.' His 'intimate connection with the magnetizer' was her 'mesmeric connection between you both.' His 'influence of the will' was her 'will power.' His 'communication of thought' was her 'absent treat-

[1] The two Shaker books from which I have quoted were written near Mrs. Eddy's home and before her first marriage. She may yet have had other than literary sources to depend upon. Her brother worked in the law office of General Franklin Pierce who was counsel for the Shakers in their trial at Concord in November, 1848. To those who care to go further into the affinities between esoteric Christian Science and Shakerism, the chapter on 'Marriage' in the first edition of 'Science and Health,' should be compared with 'Holy Mother's promises given by Inspiration' as contained in the 'Report of the Examination of the Shakers of Canterbury and Enfield before the New Hampshire Legislature.'

ment.' His 'clairvoyance' was her statement that 'the mesmerized subject sees with closed eyes.' His 'faculty for seizing the symptoms of disease and prescribing the proper remedies for them' was what she meant by 'healing the sick without seeing them.'

In the same year as Poyen's book came Durant's 'Exposition, or a New Theory of Animal Magnetism with a Key to the Mysteries.' In this animal magnetism is declared to be a 'branch of electricity, a science which gives a new life to the religious principle (furnishing unconquerable weapons to Christianity against materialism), creates a new method of pathological investigation and settles therapeutics on a basis hitherto unknown to the medical world.' Within another generation an Americanized form of mesmerism — Dr. Grimes' electro-biology — was known to high and low. The poet Whittier was familiar with it, while Mrs. Eddy wrote in the Portland *Courier* of 1862 as follows: I have employed electro-magnetism and animal magnetism, and for a brief interval have felt relief from the equilibrium which I fancied was restored to an exhausted system, or by a difference of concentrated action, but in no instance did I get rid of a return of all my ailments, because I had not been helped out of the error in which opinions involved me.' This confession has been used by the opponents of Mrs. Eddy in the attempt to prove that she stole her doctrines from the Portland magnetic healer Quimby. At first sight Eddyism might seem to be nothing but Quimbyism. He taught a 'Science of Health'; she wrote 'Science and Health'; both employed the term 'Christian Science.' Again Mrs. Eddy has her 'reversed statements,' propositions which are offered as self-evident because they read backward. She propounds this concatenation: 'There is no pain in Truth, and no Truth in pain; no matter in mind and no mind in matter; no nerve in Intelligence and no Intelligence in nerve; no matter in Spirit and no Spirit in matter.' Similar patent reversibles are to be found in Quimby's 'Science of Man': 'Error is sickness, Truth is health; Error is matter, Truth is God; God is right, error is wrong.'

Finally there are many points of resemblance between Quimby's Portland circular of 1859 and Mrs. Eddy's defence of his system in 1862. The former alleged, "My practice is unlike all medical practice. * * * I give no medicines, and make no outward applications, but simply sit by the patient, tell him what he thinks is his disease, and my explanation is the cure. If I succeed in correcting his errors, I change the fluids of his system, and establish the truth or health. *The truth is the cure.*"

Mrs. Eddy's apology for her master ran thus: "My operator believed in disease independent of the mind; hence, I could not be wiser than my teacher. But now I see dimly at first, and only as trees walking, the great principle which underlies Dr. Quimby's faith and works; and just in proportion to my light perception of truth is my recovery. This truth which he opposes to the error of giving intelligence to matter and placing pain where it never placed itself, if received understandingly, changes the currents of the system to their normal action; and the mechanism of the body goes on undisturbed. That this is a science capable of demonstration becomes clear to the minds of those patients who reason upon the process of their cure. The truth which he establishes in the patient cures him."

But it is needless to go further into this muddy question of plagiarism. There is no real issue between the Eddyite and the Quimbyite, because the head of each school drew their philosophic trickles from a common source. One of the chief ex-Christian Scientists himself grants that there was a near approach to the theory and practice of Dr. Quimby in the contemporary investigation of John Bovee Dods, who believed that electricity was the connecting link between mind and matter, that disease originated in the electricity of the nerves and can be cured by a change of mind. Turning to the 1851 edition of Dr. Dods' book, the 'Philosophy of Electrical Psychology,' there can be found passages reflected in both the writings of Dr. Quimby and Mrs. Eddy. Thus the former was said to change the 'currents of the system to their normal action,' while the latter asserted that electricity is the essence of mortal mind, the least material form of consciousness.

But to turn the study of Christian Science into a study of comparative literature is a trifle too ponderous. It was from the vaporings of itinerant magnetizers that Mrs. Eddy sucked in her primary notions. The quacks were drawing full houses in New England at the very time she was on her pilgrimage after health. Hence the 'true history of Mental Science' is to be traced, not to the only original Quimby, but to the indiscriminate practice of mesmerism along in the forties and fifties, and to the crude attempts of Yankee philosophizers to explain the borderland phenomena of hypnoidal states. Some of these crude attempts are to be found in Mrs. Eddy's primitive publications. Thus, she says: "In Mesmerism pain is only a suggested belief. * * * That sight is not in the eye is apparent when the mesmerized subject sees through different portions of the head. * * * Any mental link touching mind, though bodies are leagues apart, is sufficient to reproduce these to the clairvoyant; if the individuals have passed away, their aroma of thought is left, which is mentally scented and described. Mind has senses sharper than the body."

Out of this perception of a real hyperæsthesia with spiritualistic trimmings there was evolved a doctrine of supersensible apperception: "Clairvoyance is mind-reading alone, whereas science in contradistinction to clairvoyance reveals truth through the understanding, by which we gain the principle and explanation of phenomena; these are distinctly opposite standpoints whence to obtain information; and the right interpretation of cause and effect, belongs alone to science. Clairvoyance reaches only the fancied realities of mortal mind, whereas science admits none of these things, but admits truth, outside of mortality and error." This is only the beginning of the author's attempt 'to meet the deep demand of the Science of Psychology.' As steps to her doctrine of immaterialism she cites anything that happened to puzzle her mind, for example, 'the phenomenon of mediumship' and 'the mischievous link between mind and matter, called planchette.'

If in any sense Mrs. Eddy may be said to have transcended the current philosophy below stairs, it was only by kicking over the ladder by which she mounted. Her method of attaining the heights of truth was to spurn the assertions of her contemporaries. As her reminiscences insinuate, theirs was the imposition, hers the demonstration. 'In contradistinction to the 'ism of spiritualism,' she put as her 'scientific basis' of mind: that 'soul gives utterance to itself when sense is silent. Soul sets man free, which explains the phenomena of impromptu poets and uneducated orators.' In brief, the Eddyite theory of knowledge here appears to be an approach to the automatism of inspirational speaking and writing. But this would be a fatal admission; it would fit too closely the sing-song style and alliterative tricks of her who denies ever being a trance medium.

This feminine logic, this happy way of employing many premises and avoiding any conclusions, might be given as a proof of the authenticity of 'Science and Health'; at any rate it makes it unnecessary to linger much longer over it. There are, however, some further biographical details which illustrate the manner in which the female sage of Concord worked up her system of blank immaterialism. Having done with spiritualism and mediumship the author next takes up medicine. "The physical side of this research was aided by hints from homeopathy, sustaining my final conclusion that mortal belief, instead of the drug, governed the action of material medicine. I wandered through the dim mazes of *Materia Medica*, till I was weary of 'scientific guessing,' as it has been well called. I sought knowledge from the different schools — Allopathy, Homeopathy, Hydropathy, Electricity, and from various humbugs — but without receiving satisfaction. I found in the two hundred and sixty-two remedies enumerated by Jahr, one pervading secret, namely, that the less material medicine we have, and the more mind, the better the work is done; a fact which seems to prove the principle of mind-healing. One drop of the thirtieth attenuation of *natrum muriaticum*, in a tumblerful of water, and one teaspoonful of the water mixed with the faith of ages, would cure patients not affected by a larger dose. The drug disappears in the higher attenuations of Homeopathy, and matter is thereby rarefied to its fatal essence, mortal mind; but immortal mind the curative principle remains, and is found to be even more active."

These hints from a diluted Hahnemannianism do not give the earliest steps to the Eddyite adumbration of the really real.

Like a contemporary Shaker with his 'inexpressible something' she obtained in certain mystic experiences her first glimpse of what she called 'the eternal somethingness': "There have always attended my life phenomena of an uncommon order, which spiritualists have called mediumship, but I clearly understand that no human agencies were employed. * * * The recognition of the power of the Spirit came to me through the spiritual sense of the real, and of the unreal and mortal sense of things." These two fragments of experience demand their psychopathic interpretation. As the 'voices not our own' were auditory hallucinations, the 'phenomena of an uncommon order' were mere trances. They were veritable psychoses, yet the ecstatic having but a confused remembrance could with difficulty recall and describe them. Inasmuch as they transcended normal thought and experience they were ineffable. Hence if the ordinary reader desires to grasp the Christian Science epistemology he is informed that, as a literature, Christian metaphysics is hampered by a lack of proper terms in which to express what it means. This prairie-dog way of getting out of difficulties by falling into a hole leads to the other subterranean experience. The second passage concerning the Eddyite theory of spiritual insight has a similar interpretation and an equal significance in the building up of the system. The 'recognition of the unreal sense of things' may be laid to a temporary absence of conæsthesia. In this loss of the sense of the compact reality of the bodily organism is to be found the final flimsy basis of this latter-day doctrine of immaterialism.

In tracing the sources of Christian Science by isolating special factors in the psycho-physical development of its founder, her 'Science of Mind, the All-in-all of Spirit' is seen to be in large part the morbid outgrowth of an abnormal personality. Yet this sketch would not be complete without a reference to the saner influences which left their mark on her system. Besides the Shakers, the mesmerists and the homeopathists, the New England transcendentalists indirectly contributed to her 'divine metaphysics.' There is no need of Mrs. Eddy's denying her indebtedness to Emerson. In comparison with her subjective denial of matter, of evil, of sin, of everything that did not suit her personal fancy, his solipsism was a common-sense dualism. Yet between the two Concord schools of philosophy there was an essential, albeit a perverted resemblance. The transcendentalism of the New Hampshire cult has succeeded for much the same reasons as those given for the success of the Massachusetts cult. The earlier movement has been described as possessing "all the chief qualifications for a gospel. Its cardinal facts were few and manageable. Its data were secluded in the recesses of consciousness, out of the reach of scientific investigation, remote from the gaze of vulgar scepticism; esoteric, having about them the charm of a sacred privacy, on which common sense and the critical understanding might not intrude. Its oracles proceeded from a shrine, and were delivered by a priest or priestess, who came forth from an interior holy of holies to utter them, and thus were invested with an air of authority which belongs to exclusive and privileged truths, that revealed themselves to minds of a contemplative cast. To the pure transcendentalist the soul when awakened utters oracles of wisdom, prophesies, discourses grandly of God and divine things, performs wonders of healing on sick bodies and wandering minds."[1]

Certain more formal resemblances between these two movements may be given as final proof of the experiential and eclectic nature of Mrs. Eddy's system. It would be too much to say that she ever reached the great upper air currents of Emersonian idealism, yet in so far as her morbid nature allowed she tried to breathe the atmosphere of supersensible forms of thought, attempted to approach the realm of the absolute right, the unconditioned excellence, the eternally true. How far original Christian Science was influenced by contemporary literature it would be hard to say. These were books of the day which had a sort of anticipated Eddyite terminology. Out of the narrow circle illuminated by the Boston pundits there came such works as James Walker's 'Philosophy of Man's Spiritual Nature,' Frothingham's 'Philosophy as Absolute Science,' Henry James' 'Shadow and Substance.' But a comparison be-

[1] O. B. Frothingham, 'Transcendentalism in New England,' New York, 1876, page 303.

tween these works and 'Science and Health' turns into a contrast. From internal evidence alone we may be sure that Mrs. Eddy's only hovered about the penumbra of Cambridge culture. If any literature influenced her mortal mind it must have been a variety of philosophic chap-books such as the anonymous pamphlets 'The Panidea or an Omnipresent Reason,' and 'The New Science or Mental Theology.' Some of these crumbs from the feast of reason were undoubtedly within the reach of the wandering seeress. As she had been an experimenter in systems of mental hygiene, she now became a taster of random idealistic speculations. A twisted transcendentalism gave direction to her religious convictions and the result was that 'immortal sentence' in answer to the question: 'What is the Scientific statement of being? *Answer*. There is no life, substance, or intelligence in matter; all is mind; there is no matter. Spirit is immortal truth, matter is mortal error. Spirit is the real and eternal, matter the unreal and temporal. Spirit is God, and man is His image and likeness; hence, man is spiritual and not material.'

THE CASE OF JOHN KINSEL. II.

BY GEORGE B. CUTTEN, M.A., PH.D.

The summer following his graduation, he took an agency which required him to be away from home some of the time. During this time he was 'asleep' a week at a time, and thus met with some disagreeable experiences. One of these resulted in the breaking of the engagement between his fiancee and himself. He was in a neighboring town, working on his agency when he went to 'sleep.' The next move was to get beastly intoxicated, and then he boarded the train and went to see his sweetheart. She knew nothing of his trouble and naturally considered this a voluntary intoxication; but she considered also that he had been deceiving her, for she had not been told that he drank, and further considered it inconsistent with the ministry. Taken all together she immediately broke the engagement, and no explanations would suffice to atone for the way in which he presented himself on this occasion.

In the fall John entered the Divinity school, but on the advice of his physician he did not complete the first term, but returned to his home on the farm. Since that time he has spent most of his time at his home, working on the farm, and teaching school in his district. He has been away for short intervals on agencies of different kinds. He had one or two attacks of his abnormality while in the Divinity school, but after he returned home the attacks became less frequent, and of less duration when they did come, until in the spring following his graduation from college he had his last attack up to the present time.

John's eyes were a source of constant trouble to him, and probably contributed not a little to his other trouble. In the college town one oculist assisted in the frequent change of his glasses, enabling him to get 20-50ths vision. This treatment lasted from November 10, 1894, to January 11, 1897. The oculist who had most to do with this case resided in a neighboring town, not far from John's home. He has kindly sent the following details: "My acquaintance with Mr. Kinsel dates from October, 1883, when he was eleven years old. The story was that he was always considered extremely near-sighted, from his evident bad distinct vision, and from his way of holding a book within a few inches of his eyes. A year before he came someone had discovered a whitish appearance of his pupils. I found both crystalline lenses somewhat opaque, especially at their posterior parts (posterior polar cataract). His vision was right eye 15-70ths of normal, and his left eye 10-70ths. With his right eye he could read No. 6 (fair book) print, and with his left eye No. 16 (very large) print, at about six inches. I advised no treatment for six months, and that he should come again then to let me see what changes if any were going on.

"I next saw him April, 1888, and found the changes were very slight for five years. At this time his vision was, for right eye 15-50ths, and for the left 15-80ths.

"Again I saw him in September, 1899, after his graduation from college. His vision at this time was, for right eye 20-70ths, and for the left 20-100ths, with convex glasses. I advised operation on the left eye by the method known as 'needling.' The first operation was done on November 6, 1899, and was followed by no unpleasant reaction. Three more operations were done on this eye at intervals, all without subsequent pain or trouble, and on December 18, 1900, I found vision of the left eye with appropriate glass — to take the place of the (dissolved) lens — was 20-30ths +, or almost normal; and he could read the finest type at ten inches. I realized, as he did not, the dangers he had so fortunately escaped, and inclined to advise him to be satisfied with one good eye; but he insisted that the other one be operated on.

"On December 30, 1900, I did the first operation on the right eye; there was no reaction. On March 1, 1901, I did the second. This made the fifth operation on him, and as there had been no reaction following any of them, I think I overestimated his resisting power, and in my desire to get on with a less number of operations than had been required for the other eye, I operated more freely than I had formerly done. As a result he had within twelve hours an attack of acute glaucoma, for which there is but one thing to do, viz., the operation of iridectomy. This stopped the fearful pain and quieted the inflammation, but left him with vision of only 1-70th in this eye due to the thickening of the lens capsule. I was unwilling to attempt further operation till March 1, 1902, when I made a section of the capsule, and on March 8, 1902, he had vision of 20-50ths with prospect of further improvement.

"During the last year the vision of the right eye has fallen off to 20-70ths, owing to the thickening of the capsule. A recent needling operation has restored the vision to 20-40ths.

"These cases of posterior polar cataract are not usually considered favorable for operation, as they are apt to be accompanied by some diseased condition of the nerve. This in his case proved not to be so; certainly not in the left eye and I think not in the right one. They are not cases in which the conservative oculist often urges an operation, he usually waits until the patient demands it, as was the case with our friend. He * * * was eager to make an experiment of his worst eye. I consented with some misgivings, and am glad now that I did."

As has been said, the abnormal states in their usual form stopped in the spring of 1898, but in the fall of 1900 he developed a real dipsomania. Let us look at the alcoholic history of his case which has been reserved for this place. His heredity, which has been given, reveals that his maternal great-grandfather, grandfather and great-uncles all drank heavily; but neither his mother nor any of her brothers or sisters drank. All the Kinsels drank, and there is a direct history of alcoholism on this side of the family as far as it can be traced. Add to this the unstable neurotic diathesis, and we have splendid soil for the development of dipsomania.

John drank cider at home when a boy, but was never intoxicated but once. When seven years of age, his mother being away, he climbed up in the pantry and took from the shelf some cider brandy and drank enough to intoxicate him. He never drank anything but cider and this cider brandy until he was fourteen, when he had a glass of beer. When twenty-one years old, being in the sophomore class in college, he drank on one occasion beer and wine, and on another occasion claret lemonade. On the former of these occasions he became 'happy.' The following summer he drank cider at home.

In the junior year he only drank when in these abnormal states. His beverage was mostly beer, very little of which would intoxicate him. During the summer vacation, between his junior and senior years, he drank nothing at all, not even the cider at home. In the senior year he drank a great deal, but only in the abnormal states, except that sometimes he would come out of the abnormal state into the normal, partially intoxicated; then he would continue drinking in the normal state. But never was drinking initiated in the normal state.

During the summer following graduation he drank twice in his abnormal state, and both times was very much intoxicated. In the autumn while in Divinity school he drank some, but not sufficient to cause intoxication. He knew that he had been drinking by the smell of beer on his breath when he awoke. This is very interesting in showing the severance of the two states, for normally we cannot smell anything that we have partaken. After leaving the Divinity school, in January of the following year, he drank some cider when normal as well as during the abnormal state. This was the first of a desire to drink when normal, and was gratified by cider. He also smoked very heavily during this time.

In April of that year, 1898, he went fishing with some men who had wine and whiskey, and here we have the first voluntary intoxication. He drank cider all that summer and autumn, but not to intoxication, and continued very light or no drinking during the following winter, spring and summer. The following autumn, 1899, he drank considerable cider, on one occasion sufficient to intoxicate him; he also got intoxicated on beer. The subsequent winter he got 'happy' several times on cider, drank cider all the spring and summer, and in the early fall developed a true dipsomania with monthly periods, which continued all the fall and winter, the last one being on March 19, 1901. During these periods whiskey or any intoxicants were drunk.

On March 20, he came to the writer for treatment. He had had two drinks that morning, but as he had previously been hypnotized, and was an excellent subject, no difficulty was experienced in hypnotizing him. He slept deeply and appropriate suggestions were given. He came again on the following day and on April 16, 17 and 18, the last three being just previous to the time for his next outbreak. He went away and the next day the craving appeared on time, contrary to suggestion. He went to a saloon and procured a glass of beer, went to another saloon and got another glass. He went into a third saloon, purchased and drank a third glass of beer, when he began to know (he expressed it, "Hear, feel or remember, I can't tell which. It seemed like all three,") what I had suggested to him.

The next day he wrote very hurriedly as follows:

Saturday.

Dear Cutten: I won't come Monday, I believe I don't need any more treatments, that they would be harmful. I'll explain. Yesterday I was seized with a mighty desire to drink. I yielded. As I drank my third beer I became sick as a horse. I could hear your voice far away saying, "You cannot drink, it will make you vomit, you can't raise your hand to your head, it is impossible. You don't want it, you can't take it. You can't smoke, tobacco tastes like wormwood. You will be ambitious, * * * ." Is this what you said? I seem to remember it all. Tell me if I had better be treated any more, or see if I remain in this state, this third self. Write to me and tell me what to do; I will come anytime you say if you think best.

Yours sincerely,

JOHN KINSEL.

P. S. The very thought of drink makes me heave. If this only lasts I will be a model man you may be sure. Do you think it will? J. K.

On June 2, again, he came and was hypnotized. From that time until November, a period of six months — he did not touch any kind of intoxicating liquors, but then he got a quart of whiskey and became intoxicated. Since that time he has had no desire, and where the smell of liquor used to engender a great desire for a drink, now it has no effect, or if any, it causes a repugnance.

Mr. Kinsel very kindly allowed the writer to examine him while in the hypnotic condition, and some interesting results were obtained. When hypnotized, he was asked if this was the same state as his 'sleep'; he said that it was neither the same as his 'sleep' nor as his waking state, but a third state in which he could remember the events of both states. He was asked why he drank in the abnormal state, to which he replied: "When I was awake it seemed very wrong for me to drink, but when I went to sleep it not only seemed all right for me to do it, but the thing that I ought to do. It was the same way when these monthly spells came on (dipsomania)." We see here the change in the judgment, all scruples were removed, and it became a duty for him to drink where before it was a sin.

The hypnotic state was used by the writer to ascertain some facts concerning his case, but the statements were always verified, and it was found that most of the statements were correct; still he could not be trusted in everything that he said. An attempt was made to get a reproduction of the doggerel composed in the sophomore year, eight years previous, of which he could remember normally only twelve lines which had been told to him while he was awake. Not being familiar with the original, it was thought that he was showing a remarkable exaltation of memory, and in some respects he was; but it was also discovered that he could produce doggerel as well, and as rapidly in the hypnotic state as in his 'sleeping' state, and that he was interpolating much that did not belong in the original.

After finishing the doggerel, he said to the writer: "That's pretty bum poetry isn't it? You wouldn't hardly compare it to Homer." The writer replied that it was all right, when John made some further remarks. He was then asked if all that he had given was in the original, to which he replied, "Do you remember G— S—m—?" "Yes." "Well, he said, 'Gentlemen, -ah- you -ah- did not get that from the text, but from the context.' Well, I did not get it all from the text but from the context. I changed the meter, but I can give it to you in any other meter, or any other damned old poem; I feel like making poetry this morning." The writer then said to him, "Make some poetry on our baby, any meter you like, and I will write it down." The last four lines are presented to show what he calls his best effort:

> **"And great big Cutten, noisy, strong, is happy when he hears the song**
> **That sings his baby into rest, asleep upon her mother's breast.**
> **And Cutten moves so quiet, still, you'd think he'd hypnotized his will;**
> **He fears he'll wake his little girl, and spoil the luster of his pearl."**

He remarked after he had finished, "That's pretty bum. You hadn't ought to give me a subject like that, that's too good a subject. You hadn't ought to ask a man to write doggerel poetry on a baby. I tried to do well on that." These verses were spoken just as fast as they could be taken down, and on other occasions the rhymes were not recorded, and he talked right along without stopping with about the same success. There are three characteristics shown very plainly in his doggerel regarding the boat-sail. The first is the relative memory of the normal and abnormal states while hypnotized. While in the hypnotic condition he remembered all of the poem which he did in the normal state, and weaved it in. He remembered some of the poem as he used to recite it in the abnormal state, but not all of it; much more, however, than he could remember in the normal state. The second characteristic is the power of spontaneous versification while in the hypnotic state, as being equal to that of the abnormal state. The material which he composed at the time, he tried at first to pass off as the original. In the third place, it shows the vulgar state of mind when he is hypnotized. He is fully as vulgar in the hypnotic state as in the abnormal state, and very much more so in both these than in the normal state.

The second poem, that on the baby, shows what he called doing the very best he could in the way of rhyming; but in this as in the other there is a change of meter. Here, different from his usual mood in an abnormal state, the friendly and flattering way in which he speaks of the writer, is quite noticeable. It will be remembered that in the abnormal state he was usually sullen, and unkind with everyone, here we have the direct opposite. It shows the emotional extremes when not in the normal state.

Mr. Kinsel is at present teaching in a preparatory school. Apparently he is quite well, his eyes in the best condition they have been, and free from attacks of epilepsy, double personality, or dipsomania. Although the samples of his English given here while in the hypnotic condition, and just afterwards, were far from chaste, he has a remarkably fine appreciation of good English, and power of composition. He is also said to be a successful teacher.

Part II.

The statement was made in the beginning of Part I. that that part was wholly descriptive, and the terms there used were not intended to imply any theories. It remains for us now to discuss briefly a few points in the description. This discussion is not in the form of conclusions from the case, or theories in regard to the case, nor are such claimed for it; it is simply meant to be suggestive.

1. Attention is directed to the power of spontaneous versification of which some examples have been give. This abnormality — this genius — has shown itself in some quite famous persons, who have been otherwise quite normal. The following is quoted:[1]

"It seems highly probable that the extraordinary powers of impromptu versification shown by Theodore Hook and the late Mr. Serjeant Payne, were of the same intuitive kind as the faculty of musical improvisation. * * * I think it probable that if Mr. Hook or Mr. Payne had been asked the question, they would have said that their conscious mental participation in the performance was confined to that of a listener, and that they were wholly unable to say how it was produced, or to foresee the termination of a verse at the beginning of it."

Both in the cases quoted and in that of Mr. Kinsel, we see the exaltation of the normal faculties, in the power of attainment as well as in the rapidity of completion, so characteristic of abnormal conditions, *e. g.*, hyperæsthesia of the senses, and histrionic skill of hypnotics. Mr. Kinsel was the most spontane-

[1] Thomas Beckwith, 'Duplex Personality,' *Proc. Soc. Psy. Research*, VI., 84.

ous subject the writer has ever seen. Usually these subjects in the deeper stages are passive, answering at times unwillingly the questions or remarks addressed to them, but otherwise intent on sleep. This was not the case with John; he would volunteer remarks, make jokes, and laugh heartily on his own initiative.

2. If, as is probably true, the abnormal states were the equivalent of epileptic seizures, the dipsomaniacal attacks were the equivalent of the abnormal states, or better expressed, were another equivalent of the epileptic seizures, for dipsomania is usually associated with an epileptiform condition. This craving for alcohol was exhibited in true maniacal form; for twenty-seven days of the month he did not care anything about alcohol, during the remaining three days he could hardly be kept away from it. One other published case of double personality showed in the normal state a great craving for alcohol. In the case of Louis V., Mr. Meyer[1] says: "Though he had before the attack been a total abstainer, he now not only drank his own wine but stole the wine of other patients." The effects of alcohol appear to satisfy some abnormal nervous conditions in the morbid state. The fact that to the disordered judgment it seemed right for him to take alcohol at that time, means little more than that he felt the need, and he justified himself in this manner. Of course he did not recognize that it was simply a physical craving, for it had been woven into the diseased consciousness in the form of an ethical judgment. Two further abnormalities connected with his dipsomania and the hypnotic state, were the delay in action (he drank two glasses of beer without any discomfort) and the reappearance of the suggestions in conscious memory when they became active. Usually the subject who goes as deeply asleep as John did, when he takes the first glass of alcoholic beverage becomes nauseated at once, not knowing why, and not remembering any suggestion which had been given him. Here we see a closer bond as far as memory is concerned, between the normal state and hypnosis, than between the normal and the abnormal states; but this may be accounted for on the theory advanced by some,

[1] 'Multiplex Personality,' *Proc. Soc. Psy. Research*, IV., 496.

that when the post-hypnotic action in obedience to the command of the hypnotic state is carried out, there comes a temporary hypnotic condition during the time occupied in observing the command.

3. The phenomenon of repeating his awakening signal after he had awakened, without ability to stop, is interesting but not unique. In some experiments in automatic writing and other automatic movements, when the movements had been induced by repeated action, they would continue, after all initiative had been withdrawn, sometimes over one hundred times.[1]

4. The form of this case, as far as memory is concerned, is not unique, although usually the memories are entirely distinct. In the second state of Félida (there were three separate states), a case studied by Azam, the identical conditions of memory between the normal and abnormal states were found.

5. The predisposing cause of the abnormal states was undoubtedly epilepsy. The exciting cause was complex. One thing which contributed was the continuous study under unfavorable conditions; this put such a strain upon his mind that his brain and whole nervous system were affected. Another contributing exciting cause was his eyes. The cataracts directly did not affect the case to any appreciable extent, but indirectly they did. The cataracts were in the center of the lenses, and he endeavored to look around them. To accomplish this he walked along with his head down and eyes turned up, causing a heavy strain on the muscles of the eyes, and thereby putting himself in the position used by Braid and others in hypnotizing. The likeness of the abnormal state to the hypnotic trance would confirm us in placing this as a very important exciting cause, and in positing the case as closely allied to autohypnosis. Notice the similarity. First, he passed into the state as a hypnotic subject might do; at first in the form of a sleep, taking some time in getting well asleep. At these times he was only suggestible. As he became more and more accustomed to it, he went to sleep much quicker until he passed into the abnormal state instantaneously; he also became more spontaneous, not waiting for suggestions, but doing things on his own initiative.

[1] A. Binet, 'Alterations of Personality,' pp. 98 f.

Second, a loss of memory of what happened in the abnormal state, but full memory of both states when in the abnormal state. Third, his suggestibleness. Coupled with this is his dramatic skill under suggestion. Fourth, the exaltation of his normal powers while in this abnormal state; this has been referred to in another connection.

6. The chief interest in this and similar cases must center around the question of personality. There has been considerable written lately by certain psychologists, concerning a hypothetical entity called by various names, *e.g.*, 'Subliminal Self,' 'Subconscious Self,' 'Soul,' 'Other Self,' 'Second Personality,' 'Real Ego,' etc. The story is bruited abroad, that although the self and this mysterious individual of many names are keeping house together, they are not always congenial. According to some persons who claim to have seen her, she is sly, tricky, difficult to approach, murderous and thoroughly untrustworthy. Others affirm that she is very good, pure and refined, and that she will live after the death of the body and the self, *i. e.*, that she is the soul of man. If one were asked to decide between these two contradictory statements, and to say which one is correct, he might well say, "Both, it depends on the company which she keeps, it depends on the person with whom she lives." Unfortunately for those who desire to multiply mysteries, she does not exist, it is all gossip. A large amount of the material which has been presented on this subject lately, has been very pernicious in interfering with the integrity of the self. If there is more than one self there are not less than ten thousand selves residing in the one body, and dependent upon one brain and nervous system.

Binet[1] gives the distinguishing characteristics of selves as two in number, memory and character. Character implies so much that these two can be accepted as the dividing points. Let us look first at the connection between memory and personality. Can we say that we can be the same persons without self-conscious recognitive memory? Or is it necessary to remember all past experiences in order to be the same person? The ideal person holds a middle position between the two, for with no one person is either experience complete. In the life

[1] 'Alterations of Personality,' p. 345.

of everyone, there are certain mental experiences of whose validity we are in doubt when they come before us in memory; they may be dreams or they may be some of those strange experiences classed under what we call paramnesia. Is it memory, or is it imagination? The past alone holds the secret. Or it may be that it is impossible to tell whether or not we remember a certain experience as happening to us; and the question may come, in regard to some events, is it memory of the events as others have related them, or as they have really happened to us. Take the example of childhood experiences; our mothers have related the incidents so frequently, and we have retold them a number of times, until it is doubtful if we really remember them as experiences to the self. On the other hand, we must have some certain recognitive memory. What does it mean to be the same self without some recognitive memory? Yet to say that this must be continuous and complete is overstepping the mark on the other side.

We all recognize that one requisite of a good memory is the ability to forget well. The ideal person must forget eclectically. To remember every detail of life would so clog our minds as to render us unfit for the duties of life. We recognize this abnormality in some; the details are remembered so thoroughly that the important things are left out, and the individual is therefore unfitted for the responsible duties. So we swing from this extreme also; to be one and the same person it is not necessary to remember all of our past experiences.

But those who posit multiple personalities claim that it is the system of memories that makes the difference. A. B. remembers all the events of periods 1, 3, 5, 7, . . . of his life at a certain time 9; but at 10 he only remembers the events of periods 2, 4, 6, 8, . . . At 10, or any period of even numbers, he cannot remember any events of period 1 or any period of odd numbers; and at the odd periods the events of the even are equally shut out from him. This of course is the extreme case, and it is affirmed that in periods 1, 3, 5, 7, . . . he is a different person from what he is during periods 2, 4, 6, 8, . . .

Let us take a more familiar example. At nine o'clock in the evening I remember to mail a letter which my wife gave me for that purpose this afternoon. I remembered about mailing it at four o'clock and six o'clock when I was not near a letter-box, but at five o'clock and seven o'clock when I passed the postoffice and the associations might be considered more favorable for remembering, the matter was not present in consciousness. Now, am I the same person which had possession of my anatomy at four o'clock and six o'clock, and a different person than the one which had charge at five and seven? But, you object, that is only a case of spontaneous memory; your thoughts were busy with other things at five and seven; it was not that you could not possibly remember it if you had been asked concerning it. That is very true, but after all, that difference is but one of degree. We would not have to imagine much to suppose that if my wife were riding past the postoffice with me, and should ask if I had mailed the letter, that for a time I could not remember and should reply, "What letter?' and not until I had thought for some time could I remember it. This is a very simple example of lapse of memory, yet it differs only in degree from those lapses that are supposed by some to separate personalities. Examples without number could be given of total lapses of memory concerning certain subjects, under certain conditions, *e. g.*, fatigue; under dissimilar conditions, *e. g.*, rest and recuperation, the memory would be perfect.

Let us take another example; similar ones can be found in the lives of all. A gentleman after dinner sits down in his easy chair before the fire; his work is finished for the day, business is banished from his mind, and he thinks of the days of boyhood and young manhood. He thinks of his school-days in the 'old red school house,' — how familiar they are to him! In thought he skips a few years of life as the preparatory school looms up before him, — how vivid it is. His freshman and senior years in college now take his attention, and from these he passes to the years in the law school. His memory has been concerned with only certain experiences, not continuous in time, but with years separating some, and nothing in between is recalled. During this time other memories have fled, and all events around him are unnoticed. He sees not the children as they pass through the room, he hears not the call of his wife; but when she arouses him sufficiently to ask a question concerning the events of yesterday, he is unable to answer it, and affirms that he does not remember, although in an hour's time, or after he has been further aroused, he may be able to answer the question fully and correctly. Is he a different person while sitting here and remembering only certain things, or is he the same person? This example illustrates a further truth: memory after all is a matter of associations, if the right spring can be touched the experiences can be remembered. In some cases the trouble is in touching the right spring.

One further example from a late work.[1] A man drank quite heavily and went out driving with a friend. After a while the friend left him and he drove by a circuitous way back to the livery stable, stopped to allow another team to pass, delivered the horses, and went to a hotel some blocks away. Here he was aroused by a porter pounding on the side of a closet which he had entered. He remembered nothing of the past three hours. This was not a drunken sleep, for although he moved about apparently a trifle queer, he performed the complicated duties connected with the team in a competent manner, and went directly to the hotel. Was he a different person during these three hours? The memory of them was restored to him later by what is known as the hypnoid method.

Enough has been said to show what is wished — that memory, or lapse of memory (from whichever side we wish to view it), is a matter of degree, and depends on the control of the associations, and the coördination or harmony of the different elements of the personality. If necessary, a graded list could be gathered showing an unbroken line from the simplest lapse of memory to that which is supposed to separate personalities. Take four stages: (1) The common experience given above regarding the mailing of the letter. (2) The case given above of the abnormal state of the man who drank, but whose memory was afterwards restored. (3) The case of John Kinsel, who when in one state entirely forgot the events of the other state; but when in the other state remembered the events of

[1] 'Psycho-pathology.'—Boris Sidis.

both states. (4) Common cases of 'double personality' where the memories of the two states are apparently entirely separated. Between these four we can fill in cases to make a gradual scale. Where is the line to be drawn between the amnesia of one person, and the distinction between two persons? How much would a person have to forget, and remember again under other circumstances, to become two persons? As has been suggested,[1] we have been too willing to confuse amnesia with unconsciousness or other consciousness, instead of calling it what it really is, simply amnesia.

In dealing with the factor of character we have very much the same problem as we had concerning the memory, *i. e.*, that of embracing separate and distinct states in one personality. It is claimed that in 'double personality' there is usually an entirely different character. Let us look at our characters after the analogy of chemical compounds, made up of varying quantities of certain identical or different elements. A few molecules more or less change the nature of the whole compound; the proportion between two or three elements may be changed only in one atom or molecule in a single element, and yet it makes the difference between a health-producing potion and a poison. Very little change in one element of the character causes such a difference in proportion that the whole character is altered from a brilliant, joyful, buoyant one, to a dull, morose, depressed one. Thus in laying emphasis on the great change of character in people, let us realize that in reality it may only be small and apparently insignificant after all.

The secret of character is control. A person has a real, true character only as he has real control, and he lacks this character as he lacks control. We found with Mr. Kinsel that his surliness in the secondary state was largely a matter of control. In the abnormal state he was particularly disagreeable to those whom he disliked normally, but who never knew it because then he was able to control himself. With other cases of 'double personality' there may seem to be a great change of character, but it could probably be traced to the lack of control in some one element of life.

[1] 'Psycho-pathology.'—Boris Sidis.

Let us present two examples, both of which apply to the memory as well as to the character. A man is insulted and gets angry; contrary to his usual actions he talks loudly, calls vile names, swings his arms, and makes threatening gestures. His friends restrain him and take him away. The next day he is entirely ignorant of the details of the encounter; he denies that he swung his arms or talked loudly. Here is a lapse of both character and memory, but is the man a different person therefore, or is he the same person in a different mood?

The other example is more closely allied to 'double personality'; it is that of dreams. A very small proportion of our dreams are remembered for longer than a minute after waking; then they are gone beyond recall, and probably the great mass of our dreams are not remembered at all. For the time that we do remember them, and in those that we remember more fully, do we not recognize that WE dream so and so, and not that someone else has dreamed them and communicated them to us? We recognize ourselves in our dreams, the experience happened to us. Our characters are entirely changed in dreams; we do things that we could not normally allow ourselves to do. Here, as is true with all character, it is a matter of control; our characters are less because we have less control, less will. Sometimes the dreams are forgotten and we are unable to recall them, but on successive nights the dreams may be taken up and continued so as to make a systematized experience, as in the case of Mr. Kinsel, yet we may be unable to recall the details, only remembering that the dreams are continued. In all this we do not recognize another personality; even if memory and character fail us, we still recognize the same personality as in the normal state.

We have found memory to be a matter of degree, ranging in a graded scale from simple amnesia to that separation of memories common in 'double personality.' Even here we do not admit a complete separation of memories, but only an apparent one. Certain states of one condition greatly influence the states of the other condition, and memory in a small degree is undoubtedly present. Some time ago it was thought that the deeper stages of hypnosis were entirely shut off from the normal states as far as memory was concerned, but now it is recognized that the memory although weak is not entirely absent; and this we would posit for all cases of 'double personality' whether artificially induced or not. We have found character dependent on the amount of control, and therefore also a matter of degree; adding these distinguishing characteristics together, we would thus suggest that personality is a matter of degree. It is not a question then of the number of personalities, but of the degree of personality according to the amount of control and harmony. Therefore the term 'double' or 'multiple' personality has no real meaning, but instead we must state the particular stage of personality, ranging from complete or perfect personality to that of absence of personality, the extreme stages being purely hypothetical, and not found in real life. The very essence of personality is unity, and thus the term 'double personality' is a paradox; leave out unity and you destroy personality, and in proportion as unity is destroyed, personality is annihilated, not multiplied.

But a real objection enters here. One says, "I agree that personality is a matter of degree, that as memory loses and character loses, so personality loses; as the life is cut short in these respects, so the personality is dwarfed. Grant this, but what will you do with the separated portions? This is the whole question. When they are pieced together do they not make another personality in their systematized form?" No, we hold to our original suggestion. We doubt the complete change of character, at most it is only partial; and we also doubt that these memories are completely cut off. There is some disassociation, but we would warn again against the confusion of amnesia with other consciousness. It is but an exaggeration of the common working of association. The mention of one book leads us to the consideration of one line of topics, while another name would lead us in the opposite direction. These would never cross, one would never lead to the other. If a common association could be obtained, the two lines of thought could be merged. We find this well illustrated in hypnosis; in the deeper stages there is little or no memory when the subject awakes. Experiments of this kind have been tried: let the subject be awakened when he is washing his hands, or performing some act that can be continued, and the hypnotic experiences are remembered. Here the common association is found, and instead of two personalities we find only one; had not this association been found there would still be only one personality, dwarfed by simple amnesia. Successfully have the amnesic experiences been recalled by hypnotic and allied methods, and no distinct personalities have been found, but the same personality suffering from amnesia, a certain group of facts awaiting the proper stimuli to be aroused. We then would say in answer to the objection, we have no second personality, but the same personality which, like all other persons, is able to recall certain events under certain stimuli and associations, and when other stimuli and associations act, then other events come to mind; a stimulus or association common to both trains of associations would arouse both streams of thought of this one person.

We would suggest also that what sometimes seems like another personality is the mechanical action of the physical processes.

THE PLACE OF PLEASURE AND PAIN IN THE FUNCTIONAL PSYCHOLOGY.[1]

BY DR. WARNER FITE.

In Professor James' chapter on 'Instinct' we are told that all human activity is the outcome of preformed instincts; in the chapter on 'Will'[2] we learn that the operation of instincts may be modified by experiences of pleasure and pain. The question is then suggested, can pleasure and pain be regarded as modifiers in a system which refers activity to instinct? And this leads to the more general question, What is the place of pleasure and pain in a functional psychology? These questions will be the subject of the following discussion.

I.

As a basis for discussion it will be necessary first to make an outline sketch of the functional system. The functional view, as I shall call it, is represented by the tendencies common, say, to James, Stout and Dewey,[3] and is to be contrasted with the structural psychology of the association school. It is the view which is sometimes called 'pragmatism.' The raw material for a functional system may be found in James' chapters on 'Emotion,' 'Instinct' and 'Will' — more particularly in the chapter on 'Instinct.' Here it is shown that human instincts are far more numerous than had been commonly supposed, and, indeed, that so many of our activities have their origin in preformed tendencies as to warrant the supposition that this is the origin of all. The development of our activity is then as a whole nothing but a process of modification of the original instincts through inter-

[1] Read at a meeting of the Western Branch of the American Psychological Association, at the University of Chicago, December 6, 1902.

[2] 'Principles of Psychology,' Vol. II., p. 549.

[3] See Stout's 'Analytic Psychology'; Dewey's papers on 'Theory of Emotion,' PSYCHOLOGICAL REVIEW, Vol. I., No. 6; Vol. II., No. 1; 'The Reflex Arc Concept in Psychology,' PSYCHOLOGICAL REVIEW, Vol. III., No. 4; also his 'Study of Ethics, A Syllabus.'

action, or, in Professor Dewey's words, all activity is originally impulsive. The chapter on Emotion then deals with a special aspect of instinct. The emotion is the consciousness of an instinctive reaction; but the emotional reaction as such ends, so to speak, with the body of the agent, whereas the specifically instinctive reaction goes further and deals with an external object. The chapter on 'Will' shows that voluntary choice, or will, merely decides which of a plurality of competing instincts is to be allowed to prevail, or what is to be the nature of the compromise between them. We never do anything absolutely new; all that we do is to direct or modify instincts already formed.

Here we have the substance of the functional system. But to make our outline complete, two further considerations are needed. The first of these relates to the factor of conflict. Professor James makes it clear enough that the instincts we are talking about work not alone but in company — and thus either in harmony or conflict — but he fails to consider the bearing of conflict upon the distinction between instinct and emotion. Why is it that some instincts end with the agent's body, while others go beyond? Evidently because, after an instinct has set out to deal with an object, it has been checked half way. But what could have checked it? Evidently, if you accept the functional view, nothing but another instinct. And so the fundamental distinction between instinct and emotion is this: every instinct sets out to deal with an object; an instinct in the narrower sense reaches its original goal unhindered; if it is checked half way by another it becomes an emotional reaction and as such the activity is confined to the body of the agent. To illustrate — the instinct to strike another is set in motion by an insult, but a second instinct — you may call it self-respect or it may be fear — gets in the way and the result is nothing more than a clenching of the fist, etc. The obstacle may be overcome, and the first instinct may succeed in attaining its object, but in the act of doing so the emotion, *i. e.*, the anger, disappears.

The other consideration is this: It is part of the functional view to hold that without conflict there is no consciousness. If this be true, it is clear, on the basis of the foregoing, that purely instinctive reactions do not come to consciousness at all. If the instinct works itself out wholly without opposition, the agent remains unaware of his own action. If the impulse to strike my fellow-man meets with no more opposition from the rest of my nature than the impulse to brush away a fly, I shall be as unconscious of the former as I frequently am of the latter. It is only as an instinct is opposed by another and becomes emotional that it becomes a part of consciousness. In other words, confining our attention for the moment to the process of conation, consciousness begins with emotion. But it also ends with emotion. As we have just seen, the voluntary act is, in the functional view, the final adjustment of competing instincts; and as such it marks the end of both consciousness and emotion. Accordingly, taking the conative process as a whole, the functional view may be summarized as follows: Every conative consciousness begins with a conflict between instinctive reactions; it continues throughout the conflict, and ends with an adjustment of the conflict in the form of voluntary choice.

We have so far confined our attention to conation. It will be unnecessary for our purpose to define the functional view of cognition,[1] except to note the parallel, in the functional scheme, between emotion and reflection. If all consciousness is occasioned by conflict, then all cognitive consciousness, including even sensation, must be in some sense a process of reflection — a balancing of alternative possibilities. Hence, in reflection we have the cognitive parallel to emotion. And, carrying out the scheme, the following will be the functional view of the conscious act as a whole; every process of consciousness begins with a conflict, which is both emotional and reflective, and ends with a coördination, which is both voluntary choice and conviction.

II.

Such, it seems to me, is the scheme of relations presupposed in Professor James' three chapters. Now, after we have worked out these consequences, we are surprised to learn in the chapter on 'Will' that action may be modified by pleasure and pain. Professor James admits that pleasure and pain cannot initiate

[1] The substance of the functional view of cognition is given in James' chapter on 'Reasoning.'

action—that is to say, the infant cannot be influenced by pleasure and pain until he has once performed an act and noted the hedonic character of the results; hence, if he is to perform it in the first instance, he must be provided with the appropriate instinct. But the question still remains, How (upon a functional basis) can pleasure and pain even *modify* action? Where is the source of their power? They are neither instincts in themselves, nor are they part of that mysterious *fiat* which, according to Professor James, may tip the balance between competing instincts. Apparently, in a view which makes all activity impulsive and conceives of mind as a system of activities, they have no motive power. If we give them the character of positive factors in the mental complex, we depart from the functional view and adopt the standpoint of the associational or structural psychology which regards the mind as a *quasi*-chemical compound of substantive elements, among which is the element of pleasure-pain.

Nevertheless, whatever theory of psychology you adopt, pleasure and pain are facts of our psychical life. The question remains then, What is their position in the psychical life, if you accept the functional theory?

In seeking an answer to this question, we must use the functional *method*. From the functional standpoint, mind is a system of activities. Now, speaking empirically, some mental phenomena are more obviously activities than others; reasoning and choice are more obviously active than sensation and habit. The former are, therefore, for the functional psychologist, more typically the real thing. The functional method is, then, to begin with an analysis of the more obviously active phenomena of mental life and, regarding these as typical and real, to use the analysis thus obtained as a basis for the explanation of mental life as a whole. Applying the method to the case of pleasure and pain, it means that we first study the pleasures and pains of activity, in the narrower and more obvious sense, and then apply the resulting analysis to all the other forms of pleasure and pain.

Now, among the conditions determining the pleasures and pains of activity the most significant for our purpose is the before-mentioned condition of conflict. After calling conflict a condition of consciousness as such, it may seem a truism to repeat that it is a condition of pleasure and pain. But in a functional interpretation of pleasure and pain, the implications of conflict are in need of special emphasis. In the more common view, conflict belongs only to pain, pleasure presupposing its absence; and the rather striking coincidence of pleasurable excitement and struggle is treated as an unimportant variation due to special circumstances. The point to be urged here is that, if you take the functional view, struggle and conflict must be regarded as essential to both pain and pleasure; for with regard to the pleasures and pains of activity, you find that without conflict there is no feeling whatever. Consider, for example, the pleasures and pains of a game, let it be tennis or chess.[1] All the feeling lies between two points — between that at which your opponent becomes strong enough to offer appreciable resistance and that at which he has become so strong that any effort on your part is futile. Outside of these limits there is no struggle; if your opponent is too weak, struggle is unnecessary; if he is too strong, struggle is useless. It may be objected, indeed, that both of the excluded cases are cases of pain; if the opponent is too weak you are bored, if too strong you are disheartened. But upon examination you will find that this really is not the case. We are never chagrined at being defeated by an invincible opponent. Rather, in recognizing him to be invincible, we admit that we are not in his class; we cease to make any pretensions to an efficiency comparable with his, and cease to regard the matter of defeat or victory as having any importance. Nor are we necessarily bored by a too feeble antagonist. It is no hardship when you are comfortably seated enjoying your after-dinner cigar to play a game of checkers with a child. The attention required is so insignificant that while playing your part of the game, you may still enjoy the luxury of your own thoughts. It is when the activity positively

[1] The process of playing a game, assuming it to be played earnestly, furnishes a comprehensive illustration of a mental activity. Wherever we are active there is a struggle and an attempt to get the better of an opposing force, which may be the resistance offered by our neighbor, by material conditions, or by the inertia of our own habits.

interferes with some more engrossing object — when, for example, you are eager for a stiff game of checkers or tennis, or when you are impatient to take up your work, or for that matter to take a nap — it is then that the feeble opponent becomes a nuisance. But here we have again a condition of conflict, only now it is not a conflict between factors within the game, but between the whole matter of the game and something outside. Accordingly, from the functional standpoint, we have a triple parallelism between consciousness, conflict and pleasure-pain. Not only is conflict a condition of consciousness, but it is specially a condition of pleasure-pain.[1]

With this point granted, the distinction between pleasure and pain becomes relatively obvious; pleasure is succeeding, pain is failing in the process of resolving a conflict. I use the participles 'succeeding' and 'failing' because I wish again to emphasize the condition of conflict. Pleasure is not success, pain is not failure. In other words, neither is a substantive mental state, but only a transient phase of a process. When we have brought the process to an end in a definitive success or failure, the whole matter is dismissed from the mind and there is no more feeling of either kind. If we continue to rejoice over victory, it is because in our imagination the struggle is still on — in other words, we have to an extent ignored the certain fact of victory and put ourselves back into the standpoint from

[1] Upon rereading Stout's chapter on 'Pleasure and Pain' in the 'Analytic Psychology' I find that I have followed his analysis more closely than I was aware. I wish, therefore, to call special attention to the dependence of pleasure upon conflict, since Stout, though holding that conflict is essential to all feeling, seems also disposed to compromise with those who attribute pleasure to the absence of conflict, by assigning less conflict to pleasure than to pain. According to him pleasure is the 'smooth,' 'prosperous' and 'uninterrupted' progress toward attainment (pp. 271, 287). This suggests Spiller's assignment of pain to 'opposed disturbances,' pleasure to 'semi-opposed.' My point is, the fiercer the conflict the more intense the pleasure. This is supported, it seems to me, both by the logic of the functional view and by observation of the more active pleasures. If you connect pleasure with consciousness of activity, then the fiercer the conflict the more intense will be the consciousness, the feeling, and — if the feeling be pleasurable — the pleasure. And as a matter of fact there is certainly a more intense pleasure in removing a stubborn obstacle than a pliant one. Pleasure depends not merely upon the prosperity of our undertakings, but upon the size of them, as estimated by the difficulties presented.

which it was still in doubt; it is only from this standpoint that we can feel pleasure. If, again, we continue to feel the bitterness of defeat, it is because the struggle is not really ended. We have not yet accepted the fact of defeat: some hope impels us to keep up the contest and fight it out to the bitter end.

It is to be noted, however, that the estimation of success or failure does not refer necessarily to the external marks of victory or defeat. One may enjoy a game in which one is being badly beaten; or a brilliant victory, as estimated by the score, may be quite unsatisfactory for the agent in question. He may feel that he has virtually failed — that is, that he has failed to accomplish what he set out to do. The estimation of succeeding and failing proceeds always from the standpoint of the agent and of the stage reached in his self-development. From this standpoint, succeeding is progress, failing is retrogression. If, in playing a game of billiards or tennis, I feel that I am acquiring a greater precision in the estimation of angles and distances and a greater control over my muscles — if, in a word, I feel that I am making progress in the solution of the problem before me — then the process is pleasurable, whatever the objective results. Conversely, if my thoughts refuse to flow as quickly as before, and my muscles to respond as accurately — if I feel that I am less proficient in dealing with a given situation — then, even though the score is clearly in my favor, the process is unpleasant, or painful.

This suggests to us a general definition of the conflict to be resolved. While succeeding in certain activities I may be failing in others, and the pleasure attendant upon success in a game may be dimmed by the consciousness of failure in the larger matters of family and professional interests; so that the life problem as a whole shows no progress toward solution and my feeling as a whole is that of unhappiness. The question then arises, What is it that sets the life problem, causes the conflict and determines our happiness or unhappiness as a whole? There are various ways of stating and answering this question. A convenient method is to conceive of the conflict as equivalent to a disturbance of equilibrium;[1] pain and pleasure would then

[1] See Marshall, 'Pain, Pleasure and Æsthetics,' p. 221 ff.; Spencer, 'Data of Ethics,' § 36.

be the earlier and later phases of the progress from first disturbance to final restoration of equilibrium. But the question remains, What produces the disturbance? Does it arise from within or without? The functional view emphasizes the internal factor. According to this view, mind is an activity; psychical development is the issue of inner capacity into activity. Where an equilibrium has been reached, it may, indeed, be disturbed by such a narrowing of external opportunity as to threaten a backward movement in the matter of capacities already realized, but it is certain to be disturbed by the coming to consciousness of further capacities which call for further realization. Briefly, then, the functional view is teleological. The conflict is brought about by the increasing demands of the life purpose as opposed to conditions that stand in the way of its realization; and the resolution of the conflict means that the obstructions are either removed or discovered to be definitively insurmountable. Pleasure is then the consciousness of removing the obstruction, pain the consciousness of failing to remove it, but the consciousness that it is removed or definitively irremovable is neither pleasure nor pain. In other words, pleasure is a succeeding, pain a failing, from the standpoint of a teleological life process, but definitive success or failure produces as such no feeling whatever.

As an illustration of the pleasure-pain process, let us notice briefly the following series, which is the basis of an argument between Brentano and Stout: sorrow — longing after an absent good — hope, that it may come into our possession — desire to procure it — courage to make the attempt — voluntary determination which issues in action. This series may be taken to illustrate the transition, through pain and pleasure, from a disturbed equilibrium to an equilibrium restored through a satisfactory solution of the problem. And all the members of the series, with the possible exception of voluntary determination (according as we conceive the determination as in process or as accomplished), presuppose a condition of conflict. Supposing the series to be reversed, it would be true even of sorrow; for sorrow means that we have not accepted the situation as final. And this holds even of those cases where sorrow is due to loss by death, and where, it would seem, the irrevocable nature of the loss could not be contested. The sorrowing person still protests and struggles against his fate, refusing to believe it possible that a calamity of such proportions should be an accomplished fact. When he finally arrives at the matter-of-fact view that the loss *is* irrevocable, and that no protest on his part will make it less so, sorrow is succeeded by resignation, and with resignation there comes an end of suffering, if not, indeed, of memory of the object lost. So too, on the other side, the joy of final success lasts only while the dread possibility of failure is still in some degree present to the mind. When success is so assured as to be self-evident and, so to speak, axiomatic, it becomes a matter of course and passes out of consciousness. Thus we find that it is not the possession of wealth that confers happiness, but the getting possession of it after a period of poverty, and not health that we really enjoy, but convalescence. And, finally, the man who enjoys to the full the sense of mere living is he who has just narrowly escaped death.

III.

The object of the foregoing is simply to point out the implications of the functional standpoint and method, without bringing into question the final validity of the functional hypothesis. The exclusive validity of the functional view would depend upon the possibility of extending our formulation of the pleasures and pains of activity to cover the relatively passive pleasures and pains of sense; and the possibility of such application would be a question of physiological detail. The pleasures of activity, as commonly recognized, are those connected with the gross activities of the organism; if the sense-pleasures represent an activity, *i. e.*, the overcoming of a difficulty, the factors by whose conflict the difficulty is created must be sought in an analysis of the physiological process, since they are not clearly distinguished in introspection. But the presence of such conflicts is clearly suggested. Our common feeling, or mood, shows constant alternations between rest and unrest, satisfaction and need, problems solved and to be solved. And it is clear that these alternations are not purely periodic, like the alternation of hunger and satiety, but that some at least are due to the presence of new demands incident to the growth of the organism. It may even be doubted whether any return to equilibrium leaves the organism in the same condition as before the equilibrium was disturbed. As a result, we find that nothing is invariably pleasant.[1] The pleasantness of a given object depends upon the stage of appetite at which the object is secured; and in all cases it is the getting satisfied that is pleasant, rather than the being satisfied. This is true of the pleasures which are apparently the most passive, such as the pleasure of a warm bath. It is not the warmth, but the getting warm, that is pleasant; when the point of adaptation is reached between the temperature of the body and that of the bath, the bath is no longer distinctly pleasant. It is, indeed, a sufficiently obvious fact, that the pleasures and pains of sense are conditioned by the existing state of the organism; it remains for the functional psychology to show, by a reference to physiological detail, that the existing state is a state of conflict between organized habits and the further demands of organic growth.

Among the remoter consequences of the functional view I shall mention the following two: first, its bearing upon the experimental investigation of pleasure and pain, and, secondly, its consequence for ethics. (1) If pleasure is merely a transitory phase of a process, it is evident that we must take into consideration the stage of activity at which an object is felt to be pleasant or painful, as well as the nature of the object presented. Most of our experimental investigation, in other subjects as well as in emotion, rests upon the theory of structural elements as a working-basis. This is rather clearly illustrated in what Titchener calls the 'method of impression' as applied to the affective qualities. Here the attempt is made to construct a scale of pleasantness among, say, colors, by having the subject decide

[1] It is true, however, that for human beings, some objects, such as red-hot iron brought in contact with the skin, are invariably painful (though, perhaps, not always painful to the same degree). It may be possible to find a functional explanation of the fact. The invariably painful objects are related, apparently, to needs long since established in all human organisms. The absence of the object produces no pleasure because such absence is presupposed in the existing scheme of life, but its presence introduces a general derangement and thus gives pain. Pleasure, on the other hand, presupposes that the need of the object is in actual process of development.

which of a pair of colors presented is pleasanter; and the assumption underlying the method is that a given pleasure-pain quality is inherent in a given sensation-quality, that any failure of the pleasure or pain to manifest itself will be due not to any want of pleasantness in the object *per se*, but to the presence of other pleasures and pains more intense, and that the effect of such disturbing elements will be neutralized in a number of trials. It is clear, however, that from the functional standpoint, the method would be impracticable, or at least rather blind. We should not expect any sensation, or object, to have a constant affective quality, and we might expect to wander indefinitely through the maze of varying conditions without lighting upon the determining factors. From this standpoint the significant conditions would be those of the general culture of the subject, as well as his condition immediately preceding the experiment. We should expect to receive from different persons widely varying judgments and from the same person judgments varying somewhat according to the needs which were momentarily active. And it is possible that at no time would a single color be pleasant *per se*. The standpoint may perhaps be better illustrated in the realm of tones. We know that the pleasantness or unpleasantness of given combinations of tones — as contained in a given piece of music — varies with different persons, with the same person from time to time, and with different stages of musical culture; what is harmonious to one is discordant to another and perhaps flat and tasteless to a third. A functional psychology would then give up the notion of assigning harmony or discord to given combinations and endeavor to relate the combinations of tones in a genetic series corresponding to the development of musical taste.

(2) The ethical consequence of the functional view is to render it inconceivable that we should choose pleasure as an end, and hence, impossible to set up pleasure as the end to be sought. According to the functional view, the motive power of action is instinct, and it is the object implied in the instinct which constitutes the end. In this system there is no room for the motive of pleasure. Pleasure is simply an abstracted phase of the process of satisfaction — an indication that the object is

being attained in the presence of a difficulty. In other words, pleasure is not an active force or function, but a mere phenomenon. The desire for pleasure, if conceivable at all, would be irreconcilable with the desire for the object; for since pleasure exists only while success is deferred, pleasure as such could be prolonged only by sacrificing the object originally sought.

DISCUSSION.

MORAL FEELING AS A BASIS OF THE PSYCHOLOGY OF MORALS.

It is common to speak of such sentiments as sympathy, reverence, affection, etc., as moral feelings. If we consider the basis of this use of the term moral, we shall find it to be either the belief that these feelings tend to promote some end regarded as moral, or else the presence of some element of feeling associated intimately with all feelings called moral. From a strictly psychological point of view the former reason does not serve, of itself, to make feelings moral at all. On the latter ground we may rightly speak of moral feelings just as we speak of angry feelings, or feelings of fear. No one ever had, I suppose, a feeling of pure anger, or of pure fear, or of pure morality, but the abstractions may be regarded as elements of an emotional state. If the psychology of religious sentiment is to be balanced by a psychology of moral sentiment, it should be possible to identify (not of course fully to describe) the moral sentiment *per se*. This, so far as I know, has not been done.

Professor Leuba's article on the psycho-physiology of the moral imperative, in the *American Journal of Psychology*, Vol. VIII., No. 4, has been of much service to me in the study of this subject; but it seems to me that the conclusion he arrives at is unsatisfactory. His thesis is: The moral imperative is the psychic correlate of a reflective, cerebro-spinal, ideo-motor process, the efferent end of which is organized into motor tracts coördinated for specific action. The chief points are: that the process is reflective, not reflex; has no felt reference to the sympathetic system, and ends in a 'clear-cut coördinated motor conclusion, prompting to action.' That is, the idea of the action tends to fulfill itself, if it be only in speech, without effort or delay.

To meet the obvious objection that this description applies quite well to logical activity, Professor Leuba adds that only in the case of the moral intelligence is the idea that of an action that would modify the 'experiencer's relation to some existence.' As, however, the motor conclusion of the moral process may, it is admitted, affect the speech organs only — an effect realized also in a purely logical process — it appears that the distinction between moral and logical sentiment is reduced to a difference between the relations found within the ideal content accompanying the feelings. This distinction clearly will not serve for psychological purposes.

The moral imperative exists, Kant tells us, because of the opposition between our rational and sensible natures. Though Professor Leuba emphasizes the contrast between the moral imperative and all desires and cravings — a distinction which applies also to the sense of logical necessity — he fails to insist that this contrast must be *felt as a part* of the moral imperative if that is to have a distinctive character *per se*. It is the *sense of* that contrast that makes the moral claim not necessary, but imperative; not 'must,' but 'ought'; while the moral is distinguished from other imperatives by the clearness and coldness of its demand. It is never heard, Professor Leuba tells us, in the din of passion. On the contrary, surely it is there that it is primarily present. It is 'a rod to check the erring and reprove.' It is *not* calm, but it 'calms the weary strife.'

Though there are many feelings called moral in which a sense of actual restraint or imperativeness is not present, the classic representations in art and philosophy of the moral sentiment portray it as preëminently restraining, imperative. If the term moral is really one, it must be so by virtue of some one feeling which is at the bottom of all its uses. I wish to identify for psychological purposes a certain sense of restraint as generally receiving in and for itself the name of moral sentiment; and to indicate, further, how all uses of the term moral can be explained through association with this sentiment, getting their moral character from a reflection or memory of the moment of restraint and conflict. This identification should serve as a basis for a consistent psychology of morals, at present lacking.

This restricted use of the term moral corresponds with the term conscience in the narrowest sense. The universality of conscience is indicated by the common description of it as the law, as the voice of reason or of God. To say that it refers to an ideal of character (Wundt) or to an ideal self (Bosanquet) narrows it unduly. I wish to propose that in the first instance it is essentially nothing but the sense of a restraint of emotional by intellectual activity. If I were to adopt Professor Leuba's physiology I should say that it is the psychical correlate of the restraint of muscular activity, associated with the sympathetic system, by cerebral activity (perhaps also by muscular movements associated with the cerebral activity and antagonistic to the muscular activity first mentioned). This account would rest on the assumption that the association and awakening of cerebral activity on a large scale must materially diminish the energy available for those movements associated with the sympathetic system. As to the term intellectual I shall here again quote Professor Leuba. "Observations made indicate that even the mental activities which feel most purely intellectual exercise a measurable influence on blood circulation and visceral activity. But the experiencer does not recognize an effective tone, an emotion. The undistinguished bodily echo of the felt intellectual process does not concern us here. Moreover, as introspective observation declares, the moral imperative is the purer, the more exclusively intellectual it feels." (I should here say, the more exclusively intellectual the *restraint* is felt to be.) Provided the distinction thus drawn between intellectual and emotional activities will aid us in the identification of the moral sentiment, its precise physiological accuracy may be left unquestioned for the present.

The universality of conscience must be distinguished from mere generality. We may speak of sympathy, for example, as a universal sentiment, meaning thereby only that all men, without an observed exception, possess it. This is not the kind of universality we apply to the moral judgment; for some men appear incapable of it. So we regard a logical judgment and, I think, an æsthetic judgment as universal, though some men, in other respects estimable, fail to recognize it. We regard the moral demand as universal, because it rests on the nature of man. Kant says we value men because they are rational, and not merely sensible, that is, part of nature. Criticism of this way of expressing it overlooks Kant's insistence on the imperative character of rationality due to man's participation in a nature both sensible and rational. A man who lacks sympathy is a hard man; but he that lacks conscience, or rather the capacity for subjection to moral restraint, is a beast or a god, and not a man at all. In like manner we demand the recognition of logical necessity from all *men*. But it is in the case of moral restraint that universality appears in most immediate relation to action, and is most impressive.

This association of universality with rationality or intellectuality is endorsed by common usage; the emotional impulse with its bodily reference is recognized as individual. Where no flaw can be detected in a plea for any moral judgment we regard it as weakened if we can detect in him that proposes it any emotion, religious, social or directly selfish, that seems to be the spring and source of his demand.

Supposing this attempted distinction between intellectual and emotional activity to be sufficiently clear, I shall now give an account of what I think may be taken as a typical instance of moral restraint.

Suppose A. to be angry with B., and that he is about to express his sentiments; if he is checked by fear we do not call his emotion a moral one in itself. Suppose a sense of B.'s weakness strikes compassion to the heart of A. and thus his action is checked. There is a sense in which we may call this restraint moral, but if my analysis so far has been correct, this use of the term moral must be secondary, derived. Suppose A. realizes that he is angry and that his wrath can fit in with no general idea or system of things or of conduct; then the restraint may be purely moral. He places the idea of his present conduct in relation to a system of ideas and seeks in vain for some universal judgment under which it may be placed. Thus *thinking*, his hand falls or his face relaxes, and the threatening words are not spoken. Mere cerebration may be regarded as absorbing his power. Suppose that the succeeding action is merely ideo-motor. Then, if A. carries over to that act the memory of the former restraint, the act itself may be dubbed moral; but the application of this term is secondary. If some emotion, for example, if affection terminates the restraint in the direction opposed to the original impulse, that emotion, though not in itself moral, may take on, because of the memory of the state of restraint, a moral character.

There may be present in the act succeeding the moral restraint a deep sense of the fact that that act is thoroughly in accordance with the widest possible point of view. This sense of completeness, however, is rather æsthetic than moral. It is conceivable that such a sentiment may be present without any antecedent moral restraint. It is only by virtue of such antecedent moral restraint that this sense of perfection, of completeness, can be moral. Thus, moral enthusiasm belongs only to an imperfect individual.

Certain emotions that are seen consistently to tend towards the awakening, the support, or the final effectiveness of the intellectual restraint in a direction opposed to the original impulse may be and are classed as moral by virtue of this repeated association. This association of the moral sentiment may be carried still further in man's efforts to build up a system of thought which may serve to restrain him in critical moments. The whole system is described as moral. The ideal, which, in itself, as in the case of the great systems of Spinoza and Aristotle, may be æsthetical and logical, and even avowedly non-moral, may, because of this association, be felt as moral. All religious ideals are thus moral. These æsthetic and logical associations again strengthen the moral sentiment. For it is obvious that, like every other feeling, the moral restraint may be regarded not only as the result of conditions, but as being also active, as the restraining power.

In classifying the feelings that are called moral, this theory requires a distinction between those feelings that tend to call up intellectual restraint and those that tend to *end* the restraint in the direction that is in harmony with the intellectual activity. In case of pure moral activity the former class includes only the moral restraint itself seen from its effective side. Duty or conscience is the calling up of the intellectual activity, or the cause of that calling up, or its effect, just as anger is the excitement of certain bodily actions, or the bodily activity itself, or its effect, according to the way in which the matter is approached.

Reverence and obedience, however, are feelings which are closely related to duty. The distinction seems to be as follows: The intellectual activity and the reference to it, must be colored by the nature of the unconditioned basis of the individual's thinking. That such a basis is necessary for rational activity hardly needs to be said. That the character of that basis depends on the nature of the individual, and will color therefore the restraint as well as the thinking which restrains, seems obvious. Where that basis is practical, that is, rests on the perception of the *necessity of acting according to some idea*, then we have duty at its purest. Where the basis depends on an *æsthetic satisfaction*, as in the case of many religious natures, the emotion may best be called reverence. Where the basis is anthropomorphic, whether social or religious, the influence is obedience.

Mere self-control differs from a sense of duty in that the former usually implies a feeling that there is associated with the intellectual restraint some emotion which has associations non-moral or immoral. It is implied in duty; and prudence therefore has a moral character through its relation to self-control as well as through its connection with the objective ideal. Mere prudential restraint however *always* merges in an emotion that is felt as bodily, and as individual; for example, self-approval, love of others, fear, etc.

These remarks indicate some of the ways in which I believe this identification of the primitive moral sentiment will serve to assist in the classification of moral characteristics from the psychological point of view. It may aid also the genetic study of moral consciousness. Briefly to summarize my position: The essential or primitive moral sentiment is that which characterizes the restraint of an emotional tendency to act by a 'purely' intellectual activity whose ideal aspect is that of a general and more or less systematized view of reality; whose emotional concomitants, also, are not distinguishable or individual, have no bodily localization. Every proper use of the term moral is traceable to association with a restraint of this character, and the validity of the use may be tested by the closeness and genuineness of this connection.

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PROFESSOR STRONG ON THE RELATION BETWEEN THE MIND AND THE BODY.

In common with others I have been interested in Professor Strong's work just published, 'Why the Mind Has a Body.' The book is an attempt to solve that old but fascinating problem, 'The relation of the mind to the body.' The author as the result of an exhaustive discussion adopts the theory that consciousness is the reality of brain-processes or the thing-in-itself. We who believe in pan-psychism, or mind stuff as Clifford defined it, welcome all new converts to this doctrine and particularly any one who will expound it as clearly and with such accurate use of language as Professor Strong has done. But I write this letter[1] at the suggestion of Professor William James to call attention to the fact that the *argument* as laid down and developed by the author is not new nor, therefore, now presented for the first time as might be inferred from Professor Strong's work, but I believe was anticipated by me in a work now possibly forgotten by the philosophic world, entitled 'The Nature of Mind and Human Automatism' (published in 1885).[2] The same views were later set forth in two articles, which I published in *Brain*[3] and in the *Journal of Nervous and Mental Diseases*.[4] The argument which I expounded at some length in the two former publications was as follows:

The theories of 'functions' and of 'aspects' (as expounded by Huxley, Tyndall, Fiske, Bain, Spencer and others) as explanations of the relation between psychical processes and cerebral processes, are insufficient and lead to fallacies of all sorts, especially in parallelism and the denial of causative relation between the mind and the

[1] This article was written as a letter to the Editor of this REVIEW.

[2] Published by J. B. Lippincott Company, Philadelphia.

[3] 'Hughlings-Jackson on the Connection Between the Mind and the Brain,' *Brain*, 1891, p. 250.

[4] 'How a Lesion in the Brain Results in that Disturbance of Consciousness known as Sensory Aphasia,' *Jour. of Nervous and Mental Diseases*, 1885.

body. It was then argued that the key to the puzzle was to be found in a proper understanding of the nature of the problem to be solved, and that "the gist of the whole question under consideration" lay in the interpretation which we give to the term *matter*, and particularly in what we mean by cerebral motion (brain-processes). Then there followed an analysis of our conception of 'matter,' the postulation of 'things-in-themselves,' the ultimate nature of mind, the identification of mind with things-in-themselves, and finally the psychical character of the real universe.

By the term *matter* is sometimes meant the unknown realities existing outside our minds, the thing-in-itself; at other times is meant the state of consciousness which is our perception of this unknown. At other times again is meant both these things. In whatever sense the term is used, the important point is the facts which the term denotes and connotes. All three conceptions represent the facts. Now what is true of the rest of the universe is also true of our brain-matter. Keeping in mind this analysis of the term matter and all the facts which it stands for, it follows that what I mean by your brain is the unknown thing-in-itself, the real brain, and my perception of that thing-in-itself. This perception is my mental picture, or my reaction to this thing-in-itself, and it is this perception which we call the material brain. The same is true of brain motion; there is the thing-in-itself and the state of consciousness which is my picture of (or reaction to) it, and which is what we call motion. Now by the hypothesis the brain process-in-itself is consciousness, and that which we call the physical brain-process is my consciousness or perception of it.

"In other words, *a mental state and those physical changes which are known in the objective world as neural undulations are one and the same thing*, BUT THE FORMER IS THE ACTUALITY, THE LATTER A MODE BY WHICH IT IS PRESENTED TO THE CONSCIOUSNESS OF A SECOND PERSON, *i. e.*, to the non-possessor of it" (p. 55).[1]

"The real question is, not regarding the transformation of matter into mind, but how *one state of consciousness comes to be perceived as another state of consciousness*, or how a subjective fact comes to be perceived as an objective fact; how a feeling comes to be presented to us as a vibration" (p. 55).

"*Physical changes* [the thing-in-itself] *occurring in a foreign body*, *as a piece of iron*, *though giving us our experience of it*, *must be absolutely unknown to us. Physical changes occurring in our*

[1] The quotations, unless otherwise stated, are from 'Nature of Mind,' etc. Italics always in the original.

brains are clearly known to us; they are our thoughts, our sensations, and our emotions" (p. 60).

The problem was restated in the following form in the article in 'Brain' above referred to (pp. 256 to 266, Vol. XIV.).

"That every mental act is accompanied by a physical change in the brain has become an accepted axiom, and yet, having due regard to the accuracy of language, it may be said that such a statement is absolutely false, and is the basis of most of the fallacious views on the subject. This may seem paradoxical, and probably will be challenged by every reader who has followed me thus far, and yet it is a fact which I hope to be able to make clear before I have finished.

"Does the mental action follow the physical action, or does the physical action follow the mental?" How do neural vibrations in the brain become transformed into thought and feeling? These are questions which are frequently asked, and the questioner shakes his head and tells us there is no use trying to answer them. The essayist, if he be an essayist, says that these questions are insoluble, and that we are as far as ever from obtaining a solution of the mystery. This is perfectly true. We are as far as ever from doing this, and it is safe to say that we shall never answer these questions, for the simple reason that the questions themselves are mere nonsense. They involve assumptions which are not true, and therefore the questions are absurdities and cannot be answered. We cannot say how brain action is transformed into thought, because it is not 'transformed' at all. Nor shall we be able to discover whether mental action follows the physical, or vice versa, because neither follows the other. The erroneous assumption here is that there are two different facts (the mental and the physical), each having a separate and independent existence in the same individual, whereas there is in reality only one fact, and one thing cannot very well be transformed into or follow itself. * * * For the purposes of clearness of presentation let us consider another person's pain. This is a real fact. Cerebral molecular motion is a group of conscious states, not in that person but in ourselves, and therefore it is a symbol of a something else in that other person. This something else is his pain. This pain is the thing-in-itself; the neural vibration is the visual picture which one person would have if his optic apparatus could be affected by the mental state of pain in another person. Mental states are the reality of molecular vibrations in the brain.

"* * * But if the mind is the reality of cerebral processes, it is obvious that the statement with which we started out is true, viz., that there are not two separate processes occurring synchronously side by side, and correlated with one another *in the same individual.* There is only one process — the psychical. The physical is only an objective presentation of it in another person, just as a man pictured in a mirror is only a representation of the real man in the street. There are not two men. The real man is the real agent in the chain of circumstances making up his life. Therefore cerebral processes (which as we know them are a picture) are not transformed into, nor do they precede nor follow the psychical process, which is the real thing."

This theory gets rid of the difficulties of parallelism. There is no parallelism in the same organism because there is only one process (consciousness) and the other process is only the way it is apprehended by the second organism; 'only the sensations of, or effect upon, the second organism, when acted upon (ideally) by the real substance, mind' (p. 29).

"The common expression that 'every state of consciousness is accompanied with a molecular change in the substance of the brain,' * * * *must be regarded as unfounded and as leading to great confusion and misconception. A feeling is NOT accompanied by a molecular change in the same brain; it is 'the reality itself of that change.'* You may say, if you prefer, that a feeling in you may be ideally perceived by me as a molecular change, or that your feeling is ideally accompanied by my notion of molecular changes. *But you cannot correctly say that a feeling is accompanied by a molecular change in the same organism, because this implies two distinct existences and leads to all the fallacies of materialism*" (p. 66).

"*The parallelism is between your consciousness and my consciousness of your consciousness, or, what is the same thing, between the consciousness in you and the picture in my mind of neural vibrations.* The former is the reality, and the latter the symbol of it. There is an invariable concomitance of these facts" (p. 59).

"* * * The problem is found really to be not how molecular changes become transformed into consciousness, but how consciousness comes to be apprehended as physical changes. If the views that have been advocated above are accepted, this can be readily understood. It must be distinctly understood that it is not a question of translation or *transformation* at all, but of *identification.* Physical changes are not transformed into states of consciousness, nor are there 'two processes' which occur 'side by side' in the same person. *There is only one process*" (p. 65).

Inquiring now into the nature of mind, we find it an 'ultimate' (pp. 61–64), 'a reality' and the reality of a particular portion of this universe, namely, what is called a brain process.

The necessary deduction from all this is pan-psychism.

"It is not only possible, but in the highest degree probable, that those activities, the sum of which we call consciousness, are of a kindred nature to those activities which are the reality of phenomenal matter. Just as organic matter is made up of the same physical atoms and molecules which make up inorganic matter, combined and recombined in varying proportions, so there is every reason to believe that states of consciousness are the resultant of the combination and recombination of the elementary activities which are the realities of the physical atoms and molecules" (p. 67).

"By a still further combination of the activities underlying the properties of the simplest form of living substance, a lump of protoplasm, and manifesting themselves in its vital functions, the primitive germs of consciousness arise, and we obtain for the first time a glimpse of what these forces of the unknown universe may be" (p. 68).

"The whole universe, then, instead of being inert is made up of living forces; not conscious, because consciousness does not result till a certain complexity of organization appears, but, using figurative language, it may be said to be pseudo-conscious. It is made up of the elements of consciousness" (p. 154).

The argument by which these conclusions are reached is essentially the same as that by which Professor Strong arrives at the same result, but the writer who above all others should be credited with first setting forth the doctrine in a definite specific form is Clifford, and it is strange that Professor Strong should not realize this fact but should think that Clifford in some way held divergent views, yet I must admit on rereading Clifford's articles I do not find that his exposition is as clear as I at one time thought. There is no doubt however about his conclusions. My own argument differs essentially from Clifford's exposition, as indeed I was not familiar with his essays at the time of writing, although I took pains, just before going to press, to call attention to them in my book.

What sticks most people, or at least those who have not quite grasped the full meaning of the theory, is the difficulty of understanding how a state of consciousness can be apprehended objectively as motion.

"If cerebral motion is only a picture of the real thing, say a musical sound, why is it that the picture appears as *motion*, a so-called objective fact, while the real thing is a musical sound? The answer is not difficult, and those who have followed me thus far must have anticipated it. Let us suppose that one of us should show a kaleidoscope to some one, who not only had never seen such a thing before, but was totally ignorant of the effect of prisms, mirrors, etc., on light, in fact, was totally ignorant of the most rudimentary knowledge of optics, and of the physical sciences. Such a person might be an inhabitant of some uncivilized island in the Pacific. Suppose such a person, looking through the kaleidoscope, to see a wonderful variegated mosaic of colours. We can easily imagine how his curiosity would be excited by what he saw. He would, probably, at once examine the other end of the tube, but only to find there, in a glass box, a lot of little pieces of coloured glass thrown higgledy-piggledy together, and nothing at all like the beautiful mosaic that he has seen. He will, probably, still ask where is the picture he has seen? and if you point again to the little box of broken glass he will not believe you. You will tell him to look once more through the tube, and as he looks you turn the kaleidoscope, and he sees a succession of changing pictures, each a symmetrical pattern of brilliant colours. Again he takes his eye from the tube to examine the farther end, but as before, he finds only a handful of pieces of broken glass, comparatively dull in colour, tumbling over each other without order or method. You tell him that is what he sees, but, incredulous, he shakes his head, and will not believe you. He insists there is something inside the tube which he sees. You continue to expostulate and tell him there is nothing there, but he retorts that there must be two things, not one. He sees an ever-changing panorama of symmetrical and orderly designs, brilliant with transmitted light, and you are only showing a lot of dull glass tumbling about, and without brilliancy. He says there are two processes totally unlike one another. He can find no similarity between the two, and therefore is confident there must be something inside the tube. So you take the apparatus to pieces, and show him there is nothing inside but some mirrors arranged in a peculiar way. If you have amused yourself sufficiently with his curiosity, and if you think him sufficiently intelligent, you will probably explain to him why it is that these pieces of glass appear so differently when viewed through the tube. It is an optical delusion. They can only appear as if arranged symmetrically because they are seen through an apparatus which, arranged according to the laws of optics, compel anything seen through it to appear in a form different from that when seen with the naked eye. So it is with mind and a cerebral molecule. If we could *optically* experience a feeling as a pain, or a musical sound, in ourselves or another person, as we can ideally, that feeling could only appear to us as it would be modified by our optical apparatus —

just as the pieces of glass were modified in the kaleidoscope. If our visual apparatus were affected by a state of consciousness in another person, it would be an absolute necessity that we should see that state of consciousness (pain, sound), not as it really is but as a visual image, viz., a molecular motion. The *reality* of that motion would be the other person's state of consciousness. This would be the thing-in-itself. Hence it follows that cerebral molecules and motion, which are commonly said to be correlated in each person's brain with his physical processes, are only the modes by which *one* person apprehends *another's* conscious states. Consciousness is not correlated with molecular motion in the same individual, but only with states of consciousness (*i. e.*, the mental picture of molecular motion) in another person" (*Brain*, pp. 268–9).

There is a very interesting deduction from this theory which I believe I was the first to carry to its logical conclusions. Let us suppose a person has a sensation of color. Another person is investigating that conscious state, color, through his visual apparatus (as with a microscope); if affected he would become conscious of it in terms of vision, and would perceive it not as color but as neural vibrations, a conscious state and symbol of the color. A third person again investigates that symbolic mental picture (a conscious state) called neural vibrations, he would perceive it in his turn as neural vibrations also, and so on ad infinitum. We would have then the following as a result of these conditions:

"In organism A:	Sensation of color; an actuality and the reality of,
"In organism B:	Cerebral tremors; a conscious state, and as such also a reality, but also commonly known as phenomena or matter when projected outside of the organism and given objective existence in A. It is the form in which color in A is symbolized in B.
"In organism C:	Cerebral tremors; a conscious state, and as such an actuality, and the form in which the conscious state in B is symbolized in C.

"Cerebral tremors, then, are a conscious state, which may be a form of apprehending in a second organism either,

"1. An unlike conscious state— sound, color, thought, etc.

"2. A similar conscious state or cerebral tremor. In this instance of C, then, we are brought to what seems at first the surprising fact that that conscious state called cerebral tremors, which is the cognition of the thing-in-itself, and known as phenomena, and the thing-in-itself, also cerebral tremors in B, are similar though separate facts. And under the conditions just mentioned it might almost be said that neural tremors exist outside of us as such; or in other words, *that such phenomena exist practically as we see them.* I say practically, for although the conscious state, neural motions, possessed by one organism, may be perceived by another also as neural motions in the brain of the former, still it does not follow that these first motions would be perceived as the same kind of motion. They would be perceived as motion of some kind, but not necessarily as the same kind. For instance, taking the same illustration used above, A's sensation of color might be perceived by B as undulatory motion; the conscious state of undulatory motion in B might be perceived as circular motion by C; which again might be represented in D's consciousness by spiral motion, and so on. I do not mean to say that these particular motions do actually exist. That would depend upon physical conditions not yet understood. All I mean is that some kind of motion or physical change may under some conditions be the mode of apprehending a motion which may or may not be the same in kind; and we perceive the thing-in-itself as it really exists" (pp. 81–82).

We are brought then to a conclusion of far-reaching consequence which if developed might prove to be a field of rich investigation.

Another point which I would emphasize, and which I believe I was the first to insist upon, is that this doctrine is in entire harmony with the law of conservation of energy which has been urged with telling force against every other explanation. Let us take a nervous circuit, to wit, a sensory stimulation ending in a brain process, which in turn excites an outgoing excitation along the motor nerves to the muscles. According to this law a certain amount of nerve motion in the outgoing nerves is exchanged for an equivalent amount of motion in the brain, which in turn is exchanged for an equivalent amount of motion in the outgoing nerves. The conservation of energy is complete, but *feeling* has not entered into the circuit. Of course not. If we wish to introduce feeling into the circuit we change one of the terms and then we must change all the terms of the equation. We can do it in this way:

"Letting x, then, stand for the unknown changes in the sensory nerves, and y for those in the motor, we can say that *unknown* x becomes transformed into an equivalent amount of consciousness; that consciousness becomes again transformed into an equivalent amount of *unknown* y, and with each metamorphosis a certain amount of the one factor disappears, to be replaced by an equivalent amount of the succeeding factor. We have here, then, a circuit of ultimates corresponding to and identical with the dynamic nervous circuit, and the principle of 'correlation of forces' becomes applicable to the facts of consciousness" (p. 88).

As I said, I am led, at the suggestion of others, to once more call attention to these earlier writings of my own, partly because they are not referred to by Professor Strong (although the doctrine was fully worked out and carried even farther than has been done in Professor Strong's work), and partly in the hope that this interesting and important question may under the stimulation of Professor Strong's book awaken renewed discussion.

We, who for a long time have contended for this panpsychic doctrine and with these arguments, welcome any new comer who will break a lance for the faith, and we doubly welcome Professor Strong as a new knight.

It is important that any doctrine should be constantly restated until it has gained general acceptance or has been rejected. Professor Strong's book is a capital restatement both of the doctrine and of the facts and reasoning on which it is based. That it is a restatement is a fact worth insisting upon as evidence that the doctrine is no new one but has already received acceptance by competent writers on both continents.

MORTON PRINCE.

458 BEACON ST., BOSTON.

PROFESSOR HAMMOND ON ARISTOTLE'S PSYCHOLOGY.[1]

Professor Hammond, in his Preface, indicates the limits of the task which he has set himself. The aim of his translation and introduction is 'to make easily accessible to English scholars the scientific content of these Aristotelian treatises, and thereby to facilitate inquiry into the history of philosophical and psychological ideas.' In making it, he was, therefore, 'not interested primarily in textual questions, excepting in so far as the establishment of the text was ancillary to the establishment of doctrine,' has aimed 'to avoid the

[1] 'Aristotle's Psychology: A Treatise on the Principle of Life: (De Anima and Parva Naturalia).' Translated with Introduction and Notes by William Alexander Hammond. London, Swan Sonnenschein and Company, Lim.; New York, The Macmillan Company. 1902. Pp. lxxxviii + 339.

accumulation of notes of a purely scholastic kind, which in the present volume could only be marks of a diligent pedantry,' and has 'rigidly excluded all such matters as had no real interest for the doctrinal exposition of the treatises in hand, or for the history of science.'

Professor Hammond has displayed on the whole excellent judgment in his choice of a text and in the general faithfulness of his translation. The text is in the main that of Biehl. Where he departs from Biehl's text he generally observes the rule laid down in the Preface and calls attention to the fact in a footnote.[1]

The points of variation seem to me generally well chosen. Thus de An. III. 8. 431 b 27 *ταῦτα*, which is the reading of S and other MSS. and is supported by Sophonias and the old Latin translation, is preferable to the *ταὐτὰ* accepted by Biehl or the *ταὐτὸν* of E L, accepted by Bekker. It is difficult to see in what sense one can say that the *αἰσθητικὸν* and the *ἐπιστημονικὸν* are 'potentially the same,' or 'in potentiality, are the same.' De Sommis 2. 460 b 17–18 the reading of L S U et al. *αἴτιον δὲ τοῦ συμβαίνειν ταῦτα τὸ μὴ κατὰ τὴν αὐτὴν δύναμιν κρίνειν τό τε κύριον καὶ ᾧ τὰ φαντάσματα γίγνεται* is better than that of the E family, accepted by Biehl, * * * *τὸ κύριον καὶ τὰ φαντάσματα γίγνεσθαι*. The motive of the latter reading would seem to be an unwillingness to admit that *τὸ κρίνειν*, *i. e.*, the apprehension of difference, is a function of *φαντασία*, which it undoubtedly is. Aristotle carefully explains de An. III. 3. 429 b 10–22. 7. 431 a 20–b1, the difference between the *κρίσις* of *νοῦς* and that of *αἴσθησις* and *φαντασία*. De An. III. 6. 430 b 3, Professor Hammond rejects Biehl's conjecture ⟨*καὶ*⟩ *τὸ μὴ λευκὸν* ⟨*λευκὸν*⟩ *συνέθηκεν*, and again de Sensu 448 a 13–19 he follows in the main the text of Bekker, although apparently rejecting, with Biehl, *τῷ εἴδει*. In neither case is there any good reason for departing from the older text. He is also undoubtedly right in rejecting the *οὐ* inserted by Biehl de An. III. 1. 425 a 15 ⟨*οὐ*⟩ *κατὰ συμβεβηκός*. In some cases, however, Professor Hammond adopts the less satisfactory reading. De Mem. 2. 453 a 14 Biehl adopts Christ's emendation *ἐπέχοντες* for the *ἐπέχοντας* of the MSS., and reads *ὅτι δ' ἐστὶ σωματικὸν τὸ πάθος καὶ ἡ ἀνάμνησις ζήτησις ἐν τοιούτῳ φαντάσματος, σημεῖον τὸ παρενοχλεῖν ἐνίους, ἐπειδὰν μὴ δύνωνται ἀναμνησθῆναι καὶ πάνυ ἐπέχοντες τὴν διάνοιαν, καὶ οὐκέτ' ἐπιχειροῦντας ἀναμιμνήσκεσθαι οὐδὲν ἧττον, κτλ.*

[1] There are a few exceptions to this rule, *e. g.*, 417 a 32 *ἀριθμητικήν*, with Torstrik, for *αἴσθησιν*; 431 b 27 *ταῦτα* for *ταὐτά*; 434 b 5 *οὐχ ἕξει* for *ἕξει*; 437 a 4, *καὶ καθ' αὑτήν*, for *καθ' αὑτήν*; 445 a 16, Professor Hammond seems to follow Bekker's text in lieu of that suggested by Biehl in the 'Corrigenda,' though not printed in the text.

Professor Hammond reverts to the older reading and translates * * * 'is proved by the fact that many persons are made very restless when they cannot recall a thing, and when quite inhibiting their thought, and no longer trying to remember, they do recollect nevertheless.' His choice here seems to have been determined by a misapprehension of the meaning of ἐπέχειν τὴν διάνοιαν. 'Inhibiting their thought' is not another way of expressing 'no longer trying to remember'; it in fact implies just the opposite. The 'inhibition of thought' which Aristotle is here talking of is that which he elsewhere (Phys. VII. 3. 247 b 17) denotes by καθίστασθαι τὴν ψυχὴν ἐκ τῆς φυσικῆς ταραχῆς and (Phys. I. 9. 192 a 15) by ἀτενίζειν τὴν διάνοιαν, *i. e.*, it is that arrest of the desultory idea-trains which is involved in any vigorous effort of attention, such as the effort to recall. That recall is dependent upon a physiological process of some kind Aristotle would prove by two considerations — we sometimes cannot recall when we try to, at other times do recall when we are not trying to.

In general Professor Hammond holds firmly to the happy mean between extreme literalness and extreme freedom. The exceptions are but few. He sometimes varies his mode of translating a word with no apparent reason save the desire to avoid repetition, *e. g.*, de An. I. 1. 403 b 2 the words, * * * καὶ ὁ λόγος. ὁ δὲ λόγος, are rendered 'or reason of the thing. For the notion,' and in de An. II. 8. 421 a 1, the words, τύπτει τὸν ἐν τῇ ἀρτηρίᾳ πρὸς αὐτήν, are rendered 'produces an impact of the air already in the windpipe against the trachea itself.' More serious is his tendency, when breaking up Aristotle's complex sentences into shorter English sentences, to omit the particles by which the relations of the different elements are indicated. The long sentence de An. II. 2 414 a 4–14 is a good illustration. Aristotle first asserts that the phrase 'that by means of which we live and preceive' has two senses; then gives illustrations of the two senses drawn from other fields — we know by means of knowledge and by means of the soul, we are healthy by means of health and by means of the body; then applies this method of analysis to the case in point, 'the soul is that by means of which we in the first instance live, perceive and think, consequently it would be a kind of Ratio and Form, not Matter and the Subject.' This sentence is broken up by Professor Hammond into four sentences, the interrelations of which are far from obvious, even after repeated readings.

Seldom does one find the translation of a passage apparently influenced by a preconceived idea as to what the passage ought to mean, although a few cases occur which can hardly be explained otherwise. De

An. III. 4. 429 b 30 Aristotle remarks δυνάμει πώς ἐστι τὰ νοητὰ ὁ νοῦς, ἀλλ' ἐντελεχείᾳ οὐδὲν πρὶν ἂν νοῇ, and earlier in the chapter he uses almost the same language, 429 a 22 ὁ ἄρα καλούμενος τῆς ψυχῆς νοῦς * * * οὐθέν ἐστιν ἐνεργείᾳ τῶν ὄντων πρὶν νοεῖν. διὸ οὐδὲ μεμῖχθαι εὔλογον αὐτὸν τῷ σώματι. ποιός τις γὰρ ἂν γίγνοιτο, * * * ἢ κἂν ὄργανόν τι εἴη, * * * νῦν δ' οὐθέν ἐστιν. These words seem to import that the νοῦς of the individual, before it is actualized in the act of intuition, is in actuality nothing whatever. Professor Hammond translates the significant words as follows: — (I follow the order above given and the italics are mine) — 'The mind is potentially the object of thought, though *perhaps* not actually *so* until thought takes place.' 'What we call reason in the soul * * * is, prior to the exercise of thought, *no reality at all.* It is therefore wrong to suppose that reason itself is mixed with the body. For in that case it would have certain qualitative distinctions * * * or it would be a sort of instrument * * *. But in point of fact it is nothing *of the kind.*" All these modes of translation represent Aristotle as saying that νοῦς is not this, that or the other — not the object of thought, not reality, not quality, not an instrument. In only one of the three passages does Aristotle limit his assertion that νοῦς is nothing — in the second he says it is οὐδὲν τῶν ὄντων, which is here rendered, 'no reality at all.' Whether this is to be regarded as right or wrong depends upon the meaning one attaches to the word 'reality.' It suggests to my mind that, while the νοῦς in potentiality is not a real thing, it may very well be 'of such stuff as dreams are made of' — sensations, memories and other mental images. But by ὄντα Aristotle means, not merely τὸ ὂν καθ' αὑτό, but also all the ὄντα κατὰ συμβεβηκός, and in declaring that νοῦς in potentiality is οὐδὲν τῶν ὄντων he implies that one can attach to it no predicate whatsoever in actuality. It is difficult to resist the inference that Professor Hammond has here been influenced by his theory, set forth in this book in the introduction, that by νοῦς παθητικὸς Aristotle meant, 'the content of the central sense — memory and phantasy' (p. lxxxi). Upon this raw material or matter, according to this theory, the active reason, which is itself without content, operates and produces the νοήσεις, νοήματα, 'intuitions.' The intuition then is just as much a σύνολον, or composite of matter and form, as is the statue or any other material thing, and that which is potentially such an intuition is no more to be described as οὐδὲν or as οὐδὲν τῶν ὄντων than is the bronze which is potentially the statue. This interpretation seems to me something more than questionable, but the subject is too difficult for discussion in a review. Whatever Aristotle's meaning, it would have been better in passages of such importance to adhere more closely to his language.

Professor Hammond's treatment of the technical terms calls for little comment. Most of the equivalents chosen are appropriate, but he seems in each passage to have been guided in his choice rather by the feeling of the moment than by any definite system of equivalents.

He has made little effort to reconstruct the conceptual background, which one must possess if he is to understand Aristotle's elliptical diction. Very seldom indeed is the sequence of thought made clearer by the insertion of a clause in brackets, and the notes are brief and are often lacking when most needed. As a rule they are designed to explain allusions or to summarize the purport of the text. Many of them, those of the latter type in particular, are excellent. But when one comes to the really difficult passages, they give but little aid. Thus de An. II. 12.424 a 17–b 3 contains a summary of the theory of sensation; III. 2. 425 b 26–426 a 26 contains an explanation of the senses in which the objective quality and the subjective sensation are said to be the same and different; III. 6. undertakes to show how *νοῦς* apprehends as unities mental contents consisting of parts, such as the judgment (430 a 26–b 6), things extended in space and time (b 6–14), the *εἴδη* possessed by such objects (14–20), negations of extension or of an *εἶδος* (20–24). All bristle with difficulties of the most formidable kind, yet the notes are of the scantiest. Such notes could scarcely be regarded as 'marks of a diligent pedantry.'

But by far the most serious criticism than can be made of Professor Hammond's book will be based upon a certain lack of thoroughness and of accuracy which it presents. This is sometimes manifested in small things — a few typographical blunders remain uncorrected,[1] some words and phrases of the text are omitted in the translation,[2] and occasionally Greek words are mistranslated in a way that can be ascribed to negligence only.[3] But one is often forced to the conclusion that the translator has not himself taken the pains to master the meaning of the text before putting it into English. Some renderings, while correct enough from the grammatical point of view, fail to make

[1] 405 a 24, first for finest ; 408 b 9, sort for part (?) ; 414 b 22, derivations for derivatives (?) ; 432 a 1, in for is ; p. 181 N. 2. 213 for 231 (?); 413 a 27 processes for places (? *τόπους*).

[2] 402 a 18, *δεήσει γὰρ λαβεῖν περὶ ἕκαστον τίς ὁ τρόπος* ; 406 a 19, *κινεῖται γὰρ καὶ ταῦτα ἀλλὰ κατὰ συμβεβηκός* ; 408 a 31, *καθάπερ εἴπομεν* ; 420 a 10, *ἀκριβῶς* ; 424 a 31, *τοῦτο δ'ἦν ἡ αἴσθησις* ; 426 b 3 *ὡς λόγου τινὸς ὄντος τῆς αἰσθήσεως* ; 426 b 27, *οὐ μέντοι ὅτι νῦν ἕτερον* ; 433 a 15, *οὐ γὰρ ἡ ὄρεξις, αὕτη ἀρχὴ τοῦ πρακτικοῦ νοῦ* ; 439 b 4, *καὶ προσιοῦσι.*

[3] 419 b 15, *κοῖλα* 'hard'; 428 a 11, *σκώληξ*, 'worm' ; 404 b 8, *κινητικώτατον* 'most mobile' ; 405 a 10, *κινητικόν*, 'easily moved' ; 420 b 31, *τὸ τύπτον*, 'the impact' ; 402 b 7, *ἤτοι*, 'whether' ; 413 b 17, *φαίνεται ζῶντα*, 'appear to live' (and elsewhere) ; 447 a 5, *ὑπὸ τοῦ ἀλλοιοῦντος*, 'through its own alteration.'

the meaning clear, while others convey a false impression of the meaning. A few illustrations must suffice.

Among the common meanings of *λόγος* is that of 'ratio.' The proportion Aristotle always, so far as I have been able to discover, terms *ἀναλογία*, or *τὸ ἀνάλογον* — never *λόγος* or *μεσότης*, although the latter term is used for the proportion by the later arithmeticians, as Nicomachus and Theo of Smyrna. In dealing with compounds the essential nature of which depends upon the quantity of each component found in them, Aristotle frequently represents the *λόγος* 'ratio' which these quantities bear to one another as the *εἶδος* 'form' of the compound. Many allusions to this doctrine occur in his psychological writings, and the rendering of some of them in this translation fails to convey a clear notion of the meaning. Aristotle says, de An. III. 2 426 a 29, that the *συμφωνία* or harmonious concord, *e. g.*, the fundamental and its fourth, fifth, octave, etc., is a *λόγος*, meaning that the string lengths, pipe lengths, etc., which produce the component notes, bear to one another a ratio constant in each, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{1}{2}$, etc. Here we have merely 'relation of parts.' In de Sensu, 7, 448 a 8–9 he describes the same concords as *λόγοι ἀντικειμένων* 'ratios of contrasted (qualities).' It is here translated, 'their ratios have the characteristic of contrariety.' In de An. III. 4, 429 b 15 Aristotle says 'by the perceptive (part or power) of the soul we distinguish hot and cold *καὶ ὧν λόγος τις ἡ σάρξ*, *i. e.*, 'and (to generalize, the sense qualities) of which the flesh is a kind of ratio.' The peculiar nature of flesh, as of any other homogeneous substance, consists in the fact that in it the primary sense qualities, hot and cold, wet and dry, are combined in quantities bearing to one another a constant ratio. The phrase is here translated, 'of which flesh is a kind of register.' A misapprehension which relates to the same theory is found in the notes upon de Sensu 3. In that chapter Aristotle discusses the three conceivable ways of compounding the extremes black and white in the derived colors — by juxtaposition, superposition and blending, and rejects the first two in favor of the third. We have here the notes: (The theory of juxtaposition) 'viz., the number theory which Aristotle rejects.' "The first two theories, viz., the numerical and superpositional, are here rejected in favor of the theory of substantial mixture." But all three theories are equally 'numerical,' and Aristotle nowhere rejects, but explicitly accepts, the numerical aspect of the third theory.

The term *τὸ εἶναι* means, in Aristotle's technical phraseology, the immediate content to consciousness of the thing which the word in question denotes. Thus *τὸ ἀγαθῷ εἶναι* is 'the essential meaning to consciousness of the word *ἀγαθόν*,' and this, again, Aristotle regards as the ultimate or real being of the thing which *ἀγαθὸν* denotes. The constantly recurring phrase, *ταὐτὸ μὲν ἀριθμῷ, τῷ δ' εἶναι ἕτερον*, means, 'the terms or expressions in question are distinct when one considers their immediate content or significance to consciousness, but the traits they denote are both presented by one and the same thing,' as for example 'the road from Athens to Thebes' and 'the road from Thebes to Athens' denote the same road, but the expressions differ in *εἶναι*. To take a modern illustration, 'Theodore Roosevelt' and 'the President of the United States,' denote the same individual, but the terms differ in *εἶναι*.

Aristotle frequently makes use of this principle in solving the puzzles of his day. Thus, Physics III. 3, he undertakes to show that *ποίησις* and *πάθησις* are but two ways of thinking one and the same thing, namely, the *κίνησις* in question, and as the latter is undeniably located in the *κινούμενον*, he comes to the somewhat paradoxical conclusion that the actualization of a creative factor is located, not in the creative factor itself, but in the thing upon which it operates. The instructor is an instructor in actuality only when instructing, but 'instructing' is only another way of conceiving the process which we also call 'learning,' and learning is a kind of change which takes place *in* the learner.

De An. III. 2, 425 b 26–426 a 26 Aristotle applies this latter principle to the problem raised by Democritus, Protagoras and others, "Do objective qualities exist at all? Are not all sense-qualities mere subjective sensations existing in the sense organ only?" He first admits that we are here dealing not with two things but with one thing conceived in two ways, 425 b 26 *ἡ δὲ τοῦ αἰσθητοῦ ἐνέργεια* (*i.e.*, the objective quality, *e. g.*, *ψόφησις*) *καὶ τῆς αἰσθήσεως* (*i. e.*, the subjective sensation, *ἄκουσις*) *ἡ αὐτὴ μέν ἐστι καὶ μία, τὸ δ' εἶναι οὐ τὸ αὐτὸ αὐταῖς*, and then applies the general principle 426 a 2 *εἰ δή ἐστιν ἡ κίνησις καὶ ἡ ποίησις καὶ τὸ πάθος ἐν τῷ ποιουμένῳ, ἀνάγκη καὶ τὸν ψόφον καὶ τὴν ἀκοὴν τὴν κατ' ἐνέργειαν ἐν τῇ κατὰ δύναμιν εἶναι· ἡ γὰρ τοῦ ποιητικοῦ καὶ κινητικοῦ ἐνέργεια ἐν τῷ πάσχοντι ἐγγίνεται.*

"If then (whenever anything is being made) the motion, the making, the modification (exists) *in* what is being made, necessarily both the (objective) sound and the (subjective) hearing (when existing) in actuality, exist *in* that (which was) potentially (sound before sound was actualized, *i. e.*, the sense organ)." Sound then really exists in the ear. When we think it as so existing, we call it in modern terms subjective, in Aristotle's terms ἄκουσις. When we think it as caused by something external, we call it objective, *i. e.*, ψόφησις. But it is the same sound. Or in Aristotle's own concise language (Phys. III. 3 202 b 21) τὸ γὰρ τοῦδε ἐν τῷδε καὶ τὸ τοῦδε ὑπὸ τοῦδε ἐνέργειαν εἶναι ἕτερον τῷ λόγῳ. " 'The actualization *of* this *in* that,' and, 'the actualization *of* this *by* that,' are different in definition (or concept, *i. e.*, in εἶναι or content)."

The first of the two passages which I have quoted (425 b 26), Professor Hammond translates correctly enough, but adds the note, 'The one is the condition of the other,' which is far from Aristotle's meaning. The second passage he translates as follows:

"If, then, movement, activity and passivity are implied in the produced object, it must be that actual sound and hearing exist in a potential state. For creative and motive activity is given in antecedent passivity." Who could gather Aristotle's doctrine from this? And his translation of a parallel passage (de An. II. 2. 414 a 11) δοκεῖ γὰρ ἐν τῷ πάσχοντι καὶ διατιθεμένῳ ἡ τῶν ποιητικῶν ὑπάρχειν ἐνέργεια is equally obscure — realization is supposed to attach to that which has power to effect changes and is found in a passive and recipient subject.'

De Sensu, 6. 446 b 17, Aristotle raises the ἀπορία, "Can two individuals, one with his proper organs of sense and another with his, both simultaneously see, hear, smell one and the same thing?" To this he replies ἢ τοῦ μὲν κινήσαντος πρώτου, οἷον τῆς κώδωνος ἢ λιβανωτοῦ ἢ πυρός, τοῦ αὐτοῦ καὶ ἑνὸς ἀριθμῷ αἰσθάνονται πάντες, τοῦ δὲ δὴ ἰδίου ἑτέρου ἀριθμῷ, εἴδει δὲ τοῦ αὐτοῦ (leg. αὐτοῦ), διὸ καὶ ἅμα πολλοὶ ὁρῶσι καὶ ὀσμῶνται καὶ ἀκούουσιν.

"All perceive one, numerically, and the same original source of the stimulus, *e. g.*, a bell, incense, fire — but (each perceives) his own (percept, and this is) numerically distinct (from) but in kind the same (as that of his neighbor); consequently many (individuals), simultaneously too, (*do*) see, smell, hear (one and the same thing)." Which Professor Hammond translates as follow: "The primary stimulus, as a bell, frankincense, or a fire, is perceived by all as numerically one and the same, but in its peculiar qualities it is perceived with numerical differences, though in its essential nature as one and the same thing; for which reason many persons see, smell, or hear the same thing at the same time."

De An. III. 1. 425 a 30. τὰ δ' ἀλλήλων ἴδια κατὰ συμβεβηκὸς αἰσθάνονται αἱ αἰσθήσεις, οὐχ ᾗ αὐταί, ἀλλ' ᾗ μία, ὅταν ἅμα γένηται ἡ αἴσθησις ἐπὶ τοῦ αὐτοῦ, οἷον χολὴ ὅτι πικρὰ καὶ ξανθή· οὐ γὰρ δὴ ἑτέρας γε τὸ εἰπεῖν ὅτι ἄμφω ἕν.

"(When) a sense perceives *per accidens* (or mediately, qualities the perception of which is) peculiar to another sense, (it does so), not in so far as (it is) itself, but in so far as (it and the other sense are) one, (*i. e.*, are unified in the Common Sense, and this takes place) whenever (both senses) are applied to the same (object), as, for example, (when taste and vision simultaneously perceive) that bile is bitter and yellow, for (in that case) it certainly is the function of none other (than the Common Sense) to affirm that both (qualities) are one (object)." In Professor Hammond's translation this is rendered: "A sense, however, perceives accidentally the qualities that are peculiar to a different sense, not in their own nature but because of the unity of those qualities, as when two sense-qualities apply to the same object, *e. g.*, in the case of bile that it is both bitter and yellow. Now, it is not the function of either particular sense to say that both these qualities inhere in one thing," etc.

De An. IV. 4. 429 b 10–22, Aristotle undertakes to show that if one admits that the concrete individual and its Real or Conceptual Being are different in the real world, the mental content by which we distinguish from other things and apprehend the individual must be different from that by which we apprehend its Real Being. Or at least I shall interpret the passage upon that hypothesis. His language is elliptical in the extreme, and could be interpreted upon the hypothesis that he is arguing, not from a difference in the real world to a difference in apprehensive content, but from a difference in apprehensive content to a difference in the power or 'faculty' to which that content owes its birth. He first (10–17) considers gross concretes of sense. He then turns to the semiconcretes of abstraction, *i. e.*, the units, points, lines, surfaces and solids with which arithmetic and geometry deal. All these involve either discrete or continuous quantity and cannot therefore be regarded as wholly free from the concrete.

(18) *πάλιν δ' ἐπὶ τῶν ἐν ἀφαιρέσει ὄντων τὸ εὐθὺ ὡς τὸ σιμόν· μετὰ συνεχοῦς γάρ· τὸ δὲ τί ἦν εἶναι, εἰ ἔστιν ἕτερον τὸ εὐθεῖ εἶναι καὶ τὸ εὐθύ, ἄλλο· ἔστω γὰρ δυάς. ἑτέρῳ ἄρα ἢ ἑτέρως ἔχοντι κρίνει. καὶ ὅλως ἄρα ὡς χωριστὰ τὰ πράγματα τῆς ὕλης, οὕτω καὶ τὰ περὶ τὸν νοῦν.*

This I would translate as follows, substituting for the untranslatable *σιμὸν* the parallel term 'aquiline,' which, indeed, Aristotle himself uses elsewhere (278 a 28 sqq.) to illustrate the same point.

"Again, in case of the things which exist in abstraction, 'the straight' corresponds to (that which I have just illustrated, in dealing with the concretes of sense, by) 'the aquiline,' for (in 'the straight,' 'straightness' is conjoined) with 'continuum' (precisely as in 'the aquiline' 'archedness' is conjoined with 'nose'), but the Conceptual

Being, if the 'Being of straight' is other (in the real world) than 'the straight,' is (in consciousness) something else (— not 'straight continuum'). Let (us, with certain philosophers, assume) it, then, (to) be Two, (upon the ground that straightness is implied in Two, because the shortest line connecting any *two* points is necessarily straight, while that connecting any three or more is not straight necessarily). Then one distinguishes ('Two') by (a mental content) other (than that by which one distinguishes 'the straight') or by the same (content) in another condition. And, to generalize, we may infer that in what (soever) way the (Real) Things (*i. e.*, the Real or Conceptual Beings of concrete or semiconcrete objects, are) separable (in the real world) from the Matter (with which in those objects they are conjoined), in that way (are) the (mental contents) in the realm of Reason (by which we distinguish their Real Beings, distinct from those by which we distinguish the objects)."

How widely Professor Hammond's understanding of the passage differs from mine his translation shows:

"We refer the straight line as we do the snub nose to abstract entities, for they are both associated with the continuous. But the essential notion of a thing, if straightness and the straight line are different (and they are two things), is apprehended by a different power. The mind, then, judges in the two cases by means of a different power or by means of a power differently conditioned. In a word, therefore, as there are things abstracted from matter, so there are things that concern the reason."

De An. III. 12. 434 a 31 Aristotle gives a brief formula for the theory of chance which he has worked out in detail Phys. II. 5 and 6. * * * *εἰ μηθὲν μάτην ποιεῖ ἡ φύσις. ἕνεκά του γὰρ πάντα ὑπάρχει τὰ φύσει, ἢ συμπτώματα ἔσται τῶν ἕνεκά του* which runs, in Professor Hammond's translation, as follows: "* * * if it is true that nature creates nothing in vain. For everything in the natural world exists for a purpose or is the condition of something that exists for a purpose." But *σύμπτωμα* does not mean 'condition.' It means 'coincidence' or 'concurrence.' Aristotle conceives of Nature as a complex system of processes, each of which has its normal purpose or end and will reach that end unless blocked by some obstacle. If then we find, as we sometimes do, that a process fails to reach its end, or that an end which properly belongs to a given process appears as the final term of another, the phenomenon must be ascribed to the fact that different processes *συμπίπτουσιν*, 'fall in with,' and hence interfere with, one another.

An analogous formula, found in the eleventh chapter of the same book, is intended to cover the essential features of the practical syllogism:

434 a 16, *ἐπεὶ δ' ἡ μὲν καθόλου ὑπόληψις καὶ λόγος, ἡ δὲ τοῦ καθ' ἕκαστα (ἡ μὲν γὰρ λέγει ὅτι δεῖ τὸν τοιοῦτον τὸ τοιόνδε πράττειν, ἡ δὲ ὅτι τόδε τοίνυν τοιόνδε, κἀγὼ δὲ τοιόσδε), κτλ.*

It is here translated as follows:

'Since we have two principles in conduct, on the one hand the general conception and notion, on the other the particular notion (of which the one says that a man of such and such a kind shall act in such and such a way, and the other that this particular man — and I am that particular man — shall act in a given way),' etc. This quite misses the point. The practical syllogism requires, besides the general law, two particular propositions, one applying the law to the individual man and the other to the individual act. For example:

'All religious men fast on Friday;
I am a religious man;
I should fast on Friday.
To-morrow is Friday;
I should fast to-morrow.'

The clause in parenthesis should read 'of which the one says a man of such and such a kind should act in such and such a way, the other that *this* is the way in question and that *I* am a man of the kind in question.'

A few similar instances of misapprehension occur in the notes. To one or two of these I have already referred. Another is found in de An. III. 9. 433 a 4. Aristotle remarks that the mere possession of knowledge — *e. g.*, that of the physician — does not suffice to produce action in accordance therewith, *ὡς ἑτέρου τινὸς κυρίου ὄντος τοῦ ποιεῖν κατὰ τὴν ἐπιστήμην*. This 'other something' is, of course, *βούλησις* or *ὄρεξις βουλευτική*, 'settled, deliberate wish' or 'will' to attain the end the means to which are prescribed by the knowledge in question. The note here reads, 'It is not science but nature, the principles and laws of whose operation are formulated by science, that heals.'

But in the notes such slips are exceptional. In the text they are, unfortunately, much more common — sufficiently common to impair its value to a very considerable degree. And more common still are passages, which, although translated correctly enough, fail to make Aristotle's meaning as clear as it might be made.

The introductory outline of Aristotle's psychology is on the whole excellent. It is clear, brief, adequate and generally accurate. At some points it is open to attack, but these are for the most part points upon which difference of opinion is to be expected. The account given of Reason is to me the least satisfactory part of it. As I have already remarked, it differs very widely indeed from Aristotle's theory as I understand it.

A good English translation of Aristotle's psychology is needed, and there is a deal of good work in this book. As it stands, Professor Hammond's translation of the de Anima, if inferior to the (French) translation of Rodier, is indefinitely better than that of Wallace, and hitherto we have had none other in English. Perhaps Professor Hammond may find the opportunity in a future edition to remove any blemishes and thereby to put the English-speaking student of Aristotle yet more deeply in his debt.

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PSYCHOLOGICAL LITERATURE.

Genetic Psychology for Teachers. Charles Hubbard Judd. (International Educational Series.) New York, Appletons. 1903. Pp. xii + 329.

Professor Judd's work is neither a text-book on genetic psychology nor a systematic exposition of its underlying principles, as its name might seem to imply. It is rather an application of genetic principles to the peculiar interests and domain of the school teacher. The author believes that the average teacher, dealing constantly with immature minds that have reached only a limited stage of development, stands in great danger of mental stagnation — a danger which can best be removed by the cultivation of new interests in connection with his work. The study of the child's mental processes and their growth has already attracted much attention among teachers. But the author urges that the meaning of results so obtained can only be properly appreciated by comparison with the corresponding processes in the fully developed mind. In other words, the teacher should study himself as well as his pupils. The well-established fact that the child sees only the points of interest to himself in his environment and interprets these in many cases wrongly, stands in a new light when we discover that the adult is liable to a similar error. Such an error in adult interpretation is illustrated by the long list of optical illusions, many of which are cited by the author with drawings and explanations. Again, the common motor habits have become so automatic with the adult that he is often unable to trace their antecedents in the simpler forms of motor activity exhibited in childhood. In order to establish the connection the author recommends an experimental analysis of the separate muscular elements which enter into such habits, with a view to determine just what distinct motor factors contribute to the finished product and how they are coördinated. Handwriting and reading are analyzed at considerable length by this method in three chapters, with descriptions and graphic records of actual experiments. Another chapter deals with the 'third R,' arithmetic, principally in the way of discussing the phylogenetic origin of the idea of number.

Interwoven with these concrete problems are several chapters devoted to discussion of educational ideals in general, their origin, meaning, development, and present tendencies. Taken as a whole, the book, though somewhat lacking in unity, is well adapted to serve as a stimulus to the practical study of genetic psychology, and will tend to broaden the teacher's professional vision and elevate his ideals.

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The Laws of Imitation. G. TARDE. Translated by E. C. PARSONS. Preface by F. H. GIDDINGS. New York, Holt. 1903. Pp. xxix + 405.

In this translation we welcome M. Tarde's well-known and influential work. It was the beginning of the series of studies, both psychological and sociological, on this and kindred topics, which are still appearing. To M. Tarde's volume we owe much of the current interest in social psychology; and we predict for it the cordial welcome of students in more than one field. The general character of the work is too well known to require extended analysis; and its views have been too well apprized already in the literature to require commendation or criticism. Those who do not read French should look into M. Tarde's little book *Social Laws*, translated by Professor Warren, for the relations of the theory of imitation to the author's later-developed views.

But why should the Holts issue a book so badly printed? The print is from unclean type and the impression most uneven. The back of the book, also, quite unjustifiably reads *Tarde's Laws of Imitation — Parsons;* not agreeing with the title page. From this the reader would expect an original treatise by Parsons on Tarde's work!

J. M. B.

The Psychology of Expectation. CLARA M. HITCHCOCK, Ph.D. Mon. Sup. to The Psychological Review, No. 20 (Vol. V., No. 3), 1903.

This monograph is an attempt to show the psychological nature and importance of the process of expectation in the construction of the inner life of consciousness and the external world of reality. The introduction contains some general and suggestive remarks on these points, followed by a brief historical sketch which shows that expectation, unlike memory and other mental processes, has received scant mention in the writings of early philosophers; while modern psychologists are, for the most part, content with a brief account of its workings. It is classed by them variously, sometimes with belief, sometimes with feelings or emotions and, again, with the cognitive processes.

The writer places it in the latter class; first, because its reference is distinctly objective, pointing to that which has definite time and space relations beyond the individual mind, whereas emotion pertains to the subjective side of an experience. Again, while expectation forms the intellective element in various prospective emotions such as hope, fear and dread, it is not itself an emotion. The bodily resonance so essential to emotion may or may not be present in expectation. It is true that anticipations which have special interest for us are accompanied by marked feeling; but there are multitudes of commonplace anticipations that serve their purpose of preparation in the routine of life and pass unnoticed. Thirdly, expectation cannot be ranked as a feeling because, of itself, feeling cannot anticipate any more than it can remember, since it has no time direction; this belongs entirely to ideas.

The relation of expectation to feeling is the same as that of memory or any other cognitive process; that is, it may be accompanied by an affective tone either pleasant or unpleasant. Feeling alone cannot form the object of expectation. To anticipate a pleasant time, for example, is to have an immediate experience of a pleasure feeling in connection with an idea that has a future reference.

Structurally, expectation is composed of a complex of sensory and motor images and organic sensations having a peculiar relation to the presentative element in consciousness. This relation is due to the vibrations of attention and forms the basis of the time consciousness. Functionally, expectation is of great importance. 'It rests upon habit, but not habit understood as the forging of a chain of isolated occurrences; but as the repetition of steps leading to some known end.' Hence it serves the purpose of preparing the mind for coming events. Besides this practical work of preparation, anticipation has a distinctly cognitive function, that of assuring 'the mind of the real existence of objects not present to sense perception.'

Future reference, in the form of expectation, precedes any reference to the past. Biological considerations demand that the earliest reactions in the developing consciousness be for the preservation of the organism, and this would force primitive ideation into the prospective form. All analogy points in this direction also. Would it not be, queries the writer, a strange reversal of the natural order if, in the mental life, one must first look backward and then later learn to look forward? Careful observations of the child's developing mind show that it continually turns forward; that it is prospective rather than retrospective.

There are various forms of expectation ranging from 'a simple, mechanical process as immediate as perception, through mediate forms which are at first reproductive, depending on the mechanism of association, then apperceptive and productive, up to the highest forms of construction such as the hypotheses of science and the ideals to be realized in art and conduct.'

Expectation is held to be the basis of various more complex intellective processes, particularly of conception and reasoning. The supplementary images in perception are anticipatory, and illusion is but the impossibility of realizing such images. Expectation is also a factor in volition.

The prospective reference is implicated in many of the categories. For example, substance implies the continuous exercise of capacity in one or more directions, and capacity, after all, means a belief in our expectations that certain changes will take place under certain circumstances. The principle of sufficient reason is analyzed into the categories of causality and finality, and the dependence of both upon expectation is pointed out.

Not only does our immediate knowledge of the world about us depend on this process, but it is involved in the ultimate test of all knowledge. "Is it true?" does not mean merely, "Is it consistent with experience up to date?" but also, "Will it realize anticipations of future experience." The latter question can be answered only by experiment, whether this be of the practical or scientific sort. If successful, these rational expectations, technically called assumptions, may become the established laws of nature. To quote the concluding words of the monograph, "What are the laws of motion but the expectations of reason concerning the position of bodies in space? We are thus justified, not only in saying that all complete knowledge involves anticipation, but also in affirming that all rational expectation is knowledge."

THE AUTHOR.

The Perception of Number. J. FRANKLIN MESSENGER. Psychological Review, Monograph Supplement No. 22, June, 1903.

The work is a continuation of an article published in the Harvard Psychological Studies Vol. I. (Monograph No. 17). It deals with the sense of touch and the sense of sight, and attempts to throw some light on the method of perceiving objects in groups. It treats of 'fusion' in order to show the difference between real fusion of sensations and the recognition or non-recognition of a number of objects presented at once. The following classification is given: There are three kinds of combinations which are fundamental in explaining the relations of consciousness to the external world; (1) elements may be inseparably connected for perception yet separately conceivable, (2) elements which can be perceived separately may be combined so as to produce an entirely new and distinct quality of sensation, (3) different qualities of sensation may be combined to form a single object. The perception of objects in groups does not fall within this classification, and is an experience of an altogether different sort. That experience is the result of analysis and not of synthesis. The process is this: (1) the perception of an undivided whole, (2) the analysis of that whole into parts — this involves temporal succession; (3) setting the parts into a correspondence with the number series for the convenience of description and fixation by association. When this has been done enough times to firmly establish the association the process is no longer necessary, and the observer may come to think he directly perceives the number.

The tendency to move in order to aid perception is attributed to the desire to restimulate the sense organs, and is not regarded as evidence of a motor element in sensation. The observer moves his muscles because he has control of them and he has not control of the stimulating object. If he had control of the latter he could accomplish the same thing by moving it. Movement has a direct effect upon sensation only in so far as it is an object of sensation.

No attempt is made to discover how many things can be perceived at once. The question is rather, what are the factors which influence the perception of number.

The Author.

SOCIAL AND INDIVIDUAL.

Le Mensonge; Étude de Psycho-Sociologie Pathologique et Normale. G. L. Duprat. Paris, Alcan, 1903. Pp. 190. 18mo.

This study of lying has grown out of a lecture delivered primarily in the interests of the 'Société libre pour l'étude psychologique de l'enfant.' It includes a classification of various kinds of lying, and a study of the psychological, physiological and social causes which produce the habit of deception. The primary classification is that of positive and negative, the former including inventions, fictions, simulations, additions, distortions and exaggerations, the latter dissimulation, denial, suppression of evidence, omission, mutilation and attenuation. In concrete cases, there are of course frequently both aspects of falsification. In an examination of cases undertaken by the society above named, there were found to be 42 negative, 131 positive, and about 75 mixed cases. More interesting is the study of conditions which give rise to lying. The study of pathological cases and of children leads the author to this formulation: "Lying may be due either, on the one hand, to all sorts of tendencies, appetites or repulsions, which act either as impelling forces, or as obsessions, or as principles of systematization in minds imperfectly organized as regards the coherence of their ideas, and their psychological continuity, or on the other hand, to a defect of higher intellectual or social sentiments." It is notable that in 136 cases of lying observed among children, 75 were caused by fear and 24 by pride, self-love or boasting. The conclusion from the first figures, for the parent and educator, is very evident: that if he wishes to combat successfully the lying habit in children, he must beware of appealing to fear, since this is by far the chief cause of the habit.

Lies which may be classed under the general head of boasting or more or less æsthetic modifications of bare facts are closely allied to the free use of imagination in poetry and fiction. The 'make-believe' element which is common to both is a part of the natural play-impulse of the child and is one avenue through which freedom of mental functioning finds expression. As this in general means a breaking up of the ordinary or habitual associations in favor of free invention, it is favored by complexity of experience, so that Ribot's law applies here: 'The tendency to spontaneous variation (invention) is always in inverse ratio to the simplicity of the environment.' The complicated situations of modern life tend, therefore, to supply the conditions for greater invention on the child's part and therefore for greater ability to deceive.

Among social causes for lying are mentioned especially the pressure of group or collective life in party or church or in the less organized crowd, the conventions of society, and the organization of the press, which though composed of men who individually are honest, is very deceptive in its reports of news. Contrary to frequently expressed opinions, the author finds no decided racial preeminence in either lying or truthfulness. Different races merely have different forms. Savages especially exhibit craft or cunning not so much among themselves as in dealings with the greater strength of civilized races. Whatever of truth underlies the common opinion that women are less truthful than men, is traced not to any sexual difference but rather to the necessity which woman frequently experiences of meeting a greater physical force, or else to the artificial conditions of present civilization which tend to check her free development. In the latter case, as in the cases of members of sects and parties, above noted, the most potent cause is seen to be lack of independence. The educator or parent, therefore, who wishes successfully to combat tendencies to deception, so far as they proceed from this source, should foster independence of judgment, both intellectual and moral, in every way possible. Where, on the other hand, lying arises from egoism or cupidity, the reliance must rather be upon the cultivation of the higher sentiments which are able to inhibit the lying impulse. It is, however, noteworthy that a certain sort of lies which are very common among children spring not from egoistic tendencies but rather from the spirit of solidarity in the group, which prevents the child from betraying his companions.

The study is one which deserves the reading of both parents and teachers.

L'Ennui, Étude Psychologique. ÉMILE TARDIEU. Paris, Alcan. 1903. Pp. viii + 297.

This is a very elaborate classification and exposition of the various forms of ennui, especially those which are prevalent in modern life. The ennui of exhaustion, the ennui caused by lack of variety, by satiety, by narrowness of interests, the ennui peculiar to various ages, to woman, and to various characters, are treated in detail and illustrated from literature and life. A few brief suggestions as to remedies for ennui are added in a concluding chapter, but the author is apparently not very sanguine as to their effectiveness, for he concludes that 'ennui will go on ever increasing and opposing itself to the final triumph of the good.' If ennui is a less pressing problem to the American than to the citizen of the old world, the study is nevertheless interesting to the psychologist and the student of literature.

Essai de Classification Naturelle des Caractères. CH. RIBÉRY. Paris, Alcan. 1902. Pp. xxiv + 199. 3 fr. 75.

Among the various signs — religious, psychological, metaphysical and pedagogical — of the shift in stress of attention toward the voluntary and emotional aspects of experience, may be included the studies of 'characters,' several of which have recently appeared: Perez, 'Le Caractère de l'enfant à l'homme,' 1891; Paulhan, 'Les Caractères, 1894; Fouillée, 'Temperament et caractère,' 1895; Malapert, 'Les Éléments du caractère,' 1897; not to mention incidental treatment in works of more general type, such as Ribot's 'Psychology of Feeling.' Of the above, Perez classifies characters as quick, slow, intense, etc., using the motor organization as a basis. Fouillée and Malapert make three main classes, based on the relative dominance of feeling, intellect or will, while Paulhan distrusts the possibility of finding sufficiently constant connections between the various tendencies found in different individuals to justify any such simple scheme of classification. The present author, in a somewhat extended preface, opposes the view of Paulhan, and in the body of his work proceeds from two fundamental principles: (1) The basis for character must be sought in physical temperament; (2) intellect cannot be assigned a position among the primary characteristics. Intellect is at most of secondary importance; sensibility and will are fundamental. The function of the idea is merely to provide for the organization of the sentiments or volitions.

A classification of temperaments is first offered, based on the general tests of relative promptness and intensity of reaction in the sensitive and motor systems respectively, and to the various combinations afforded by these criteria is added a class 'Amorphous,' to include those capable neither of deep nor permanent impressions and with low capacity for motor reaction. Ranged on the basis of relative stability and unity the temperaments would then take the following order: Amorphous, sensitive (subdivided into 'reaction prompt and less intense, and reaction slow and intense'), sensitive-active, active (subdivided into choleric, with reaction prompt and intense, and phlegmatic, with reaction slow and less intense), temperate or well-balanced. The characters corresponding to these temperaments are:

- Amorphous.
- Sensitive
  - Affective.
  - Emotional
    - Unstable.
    - Stable.
- Sensitive-Active
  - Affective-passionate.
  - Emotional-passionate.
- Active
  - Passionate
    - Unstable.
    - Stable.
  - Dispassionate
    - Feeble.
    - Strong.
- Well-balanced.

In the above terms, it should be noted perhaps that the author uses affective to indicate a sensitive nature which responds promptly and feebly, while an emotional nature implies a deeper seizure and slower reaction.

As regards the propriety of excluding the intelligence from the classification of characters, it would seem to be a question largely of how much one wishes to include. That some persons are so predominantly reflective and analytic in their temper as to have this properly included in a characterization, seems to me unquestionable. Such persons are placed by the author in the class of 'strong, dispassionate' natures, but this certainly does not necessarily carry with it the other implication. Many interesting literary and historical illustrations are presented by the author.

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VERBAL ASSOCIATION.

Zur Psychologie der gebundenen und der freien Wortstellung. P. Barth. Philosophische Studien, Bd. XIX. 1902. Pp. 22–48.

In almost all languages the order of words in a clause is strictly conventional, The order is fixed in all the Indo-Chinese languages, in the Chinese at least since Confucius, in the Ural-Altaic family of tongues, in the Bantu family, in the languages of America (both in those which, like the Iroquois, distinguish between the sentence and the word and also virtually in those of the intercalative type) in the Malay-Polynesian family, in the Semitic family and in the non-poetical Sanscrit prose. The word-order of the clause is, of course, predetermined in such languages as the Chinese, which is so lacking both in prefixes and suffixes that the grammatical relations of subject, verb and object can be indicated only by the sequence. It is more remarkable that the order should be rigidly conventionalized in languages so rich in inflection as is the Arabic and especially as is the Sanscrit. With the fixed order of the Sanscrit prose, the varied sequences of the classical Latin and Greek seem in marked contrast. Greek clauses show a greater freedom in the arrangement of words than is permitted in any other language, and the literary Latin also allows a wide variation. Nevertheless in the Latin of the common people, as we find it in Plautus or in aphorisms or legal maxims, and in the colloquial Latin of the educated classes, as we find it in Terence, there is a marked tendency toward uniformity. The order is subject, verb, object, or subject, object, verb. In language at large this is the most common order in declarative sentences. The Malay-Polynesian and Semitic languages are exceptional in their tendency to put the verb before the subject.

Although the order of all the Indo-Germanic languages seems originally to have been flexible, the word sequences of all the modern European languages have been progressively conventionalized. The change may be seen, for example, in comparing Dante with the modern Italian poets. As one might expect, the highest degree of rigidity has been attained by the Romance languages. The French, more rigid even than the Italian or the Spanish, allows almost as little variation in poetry as in prose, and the English has been so influenced by the Norman as to allow scarcely more liberty than the French. Yet in spite of the influence of the Greek, the modern Russian among Slavic languages has become fixed as compared with the old Russian. As for the Polish, a literary language has grown up which is more stereotyped than the vernacular. In all the ancient Germanic dialects the order was entirely free, that is, both in principal and in subordinate clauses, the verb might stand at the beginning, in the middle, or at the end. Thus, Ulfilas in his Gothic translation of the Scriptures could keep strictly to the order of his Greek text. But although the freedom of modern German remains exceptionally great, it has become subject to such limitations in prose as the rule that the verb must stand at the end of a subordinate clause or that the emphasized word must stand at the beginning of a question.

The progress of usage in the arrangement of words offers tempting problems for psychological explanation when, as in German, it can often be traced step by step. Of the rule that the verb must stand at the beginning of the principal clause if the sentence opens with a subordinate clause, the writer offers the following explanation, as an addition to the suggestions of Wunderlich upon similar points and as an illustration of legitimate explanatory methods. In general, the arrangement of sentences is based upon association and apperception. The word or phrase will stand first which represents the emphasized notion, that is, the notion which at the outset of the sentence is 'apperceived' or thrown into the focus of consciousness. For the Germanic mind this word is normally the grammatical subject. The placing of the principal verb directly after an adverbial clause or phrase may first have occurred in series of sentences with the same subject. The primitive speaker, fond as he is of talking about himself, retains through long stretches of his narratives the same grammatical subject. In such cases, the subject notion, because recurrent, loses its rank, and the changing circumstances in which the person or thing is placed, represented by the adverbial modifiers, come to the front. The grammatical order, following the order of mental emphasis, will thus be adverbial modifiers, verb, subject. This order may then easily have been transferred to sentences with a new subject. The transfer would be due not to any logical realization that the grammatical relations remained the same, but rather to a sameness of affective accompaniments. At the end of an adverbial clause there is a feeling of completeness, followed by a feeling of strain. This strain gives place to relief as soon as the verb of the principal clause has been uttered. The relief is doubtless due to the fact that in the kind of sentence most common with primitive German speakers this verb was the most important part of the remainder of the sentence. These feelings, however, follow the wording of an adverbial clause whether a new subject for the principal verb is to be introduced or not, and the sameness of feeling may well have led by association to similarity in the arrangement of words.

This law of association through the 'analogy of feelings,' so largely used by Wundt in his 'Völkerpsychologie' as an explanatory principle, the writer believes to be one of the most important factors in producing uniformity of speech. Another exceedingly important factor has been the desire to save time in expressing oneself. This object has become increasingly important as the number of ideas to be expressed has multiplied. The demand for speed leads to the perpetuation of habits in the arrangement of words because the mechanical action is the one most rapidly performed. The mechanical process affects most decidedly the expression of traditional ideas and the language of ordinary life. It is very probable that a thorough study of Greek aphorisms or of the language of the Athenian common people would reveal conventionalities even in the Greek order.

In view of this almost universal tendency to uniformity, the great flexibility of all the pure Germanic languages is especially significant. This flexibility contributes no little to the energy of the German tongue. When the Frenchman must say in two clauses, *C'est lui que j'ai vu*, the German may say with the force of simplicity *Ihn hab' ich gesehen.* The flexibility of the German, moreover, makes it, like the Greek, an excellent medium for translating other languages.

It should be possible to infer much in regard to racial traits from linguistic phenomena, although the student must be careful both to exclude accidental peculiarities and to verify his inferences by comparing linguistic with social characteristics. Thus, it seems but fair to say that on the one hand, the rigidity of the richly inflected Sanscrit is a special instance of Oriental bondage to tradition, and that on the other, the plasticity of the German is a special instance of Teutonic individualism.

Contribution à l'étude de l'individualité dans les associations verbales. B. BOURDON. Philosophische Studien, Bd. XIX., 1902, pp. 49–62.

The object of this investigation was to determine the average amount of agreement between different individuals and the majority of the same individuals in the first verbal associations suggested by given sets of words, and to explain individual variations from this average.

In the experiment reported, two series of one hundred words each were used. These series were printed in vertical columns on large sheets of paper. The subjects were required to take the words and columns in order, and to write in the blank space opposite each word the first word or phrase suggested by it. A signal was given for the writing to begin. That the rate of progress might be timed, some of the subjects were required to mark, at a signal given at the end of each minute, the point reached on the paper. There were one hundred subjects in all, most of whom were 'students or professors.'

The average amount of agreement between the several individuals and the majority was determined as follows: Words which gave less than ten or more than thirty-nine identical results were thrown out. This procedure left seventy-five words in one series and eighty in the other. The association which occurred most often was taken as the standard result for each word. For each subject the number of agreements with the standard words or phrases was counted. Singulars and plurals of the same word, the same noun with and without an article, and so on, were reckoned as identical. This method showed, for the whole number of subjects and out of the total number of possible cases, an average agreement of 29 per cent. for the one series and 31 per cent. for the other. From the following facts, it may be assumed that the figures for the several subjects, if taken in connection with the average, represent individual tendencies very fairly. Eighty-one out of the one hundred subjects stood either above or below the average for both series alike. The figures for ten out of the other nineteen subject differ very slightly from one series to the other. Moreover, nearly the same figures resulted for thirteen out of fifteen persons who were tested three times over with the same series of words at intervals of some weeks.

The report closes with an explanation of individual variations from the average of agreement. On the showing of the time intervals and of a special test for speed in writing, it cannot be supposed that the subjects who wrote down the greatest number of uncommon associations took the longest time to think. Neither can peculiarities of education or experience explain peculiarity of associations. Every subject must, for example, have oftener met the phrase *de paille* in connection with *chapeau* than the word *chaussure* or *casque*. The writer accepts the following suppositions: A larger amount of individual peculiarity than the average must, in some cases, be explained by special interest in unusual combinations of ideas. In a few cases, it may be explained by special interest in phonetic or graphic similarities between words. Such associations as *saveur-savon*, *narine-marine* were, however, rare. In many cases, the excess is caused by a weakness of the verbal memory which minimizes the formation of fixed verbal associations. On the other hand, a smaller amount of individual peculiarity than the average is, in some cases, due to that mechanizing of verbal associations which conditions correct and facile speech. In other cases, it is a special manifestation of a more fundamental lack of individuality.

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VISION.

Beiträge zur Analyse der Gesichtswahrnehmung. III. F. SCHUMANN. Zeitsch. f. Psych. u. Phys. d. Sinnesorg., Bd. 30, pp. 241–291, 321–339.

The theoretical motif for this article is a theory of the processes that are involved in the successive comparison of impressions. Schumann's thesis is that judgments of that type are immediate and do not involve the recall of any memory image of the first when the second is presented. The statements of Külpe and Angell are cited in support of the position, and the author reinforces the position with results of his own introspections in connection with all the investigations he has conducted, notably in relation to the time sense. Two general considerations point to the same conclusion: in many senses successive comparison is more accurate than simultaneous, and since the memory image is always of less intensity than the original impression, comparisons of successive sensations on the basis of the memory image would always lead to illusion. In the experiments performed ou visual space comparisons only one subject was found who made use of a memory image in the judgment, and he was a man of much training in drawing.

If the memory image is to play no part in the comparison it is evident that some other set of criteria must be discovered to base the comparison upon. Schumann finds these in some concomitant phenomena (Nebeneindrücke) which are however not very clearly analyzed. One experience that seems in part to form the basis of the comparisons of visual extents is a swelling of the field of attention if the second object is too large and a shrinking of the field if the second be the smaller. But whatever the nature of these auxiliary processes may be they must be assumed to furnish the basis of the judgment. In the early comparisons of childhood there is undoubtedly always a superposition of the parts. Later successive fixation takes the place of the direct comparison. At this stage the auxiliary processes begin to show themselves, and in the adult the verbal judgment follows so closely upon them that analysis of their more intimate nature is impossible.

The later parts of the article are devoted to an application of the theory to the explanation of the general optical illusions of distance. The illusion of the arcs of concentric circles, the various forms of the Müller-Lyer figure and many other related illusions are explained as due to the effect upon the judgment of the lines which are to be compared of other parts of the total figure. The explanation reminds one very forcibly of Müller-Lyer's original explanation in terms of confluxion and contrast except that the secondary parts are said to work upon the judgment instead of merely being seen. Both point to the fact that the illusion disappears if the attention is restricted to the lines to be compared as evidence of the truth of their hypotheses.

The explanation of the overestimation of filled as compared with empty space deserves separate mention, although it is another illustration of the same principle. It is said that since the eye can clearly perceive but two or three and even with effort but four or five dots, that when a space is filled with a number they must be perceived successively, and this successive apprehension induces the same mental state as a large extent. The hypothesis is further supported by the fact that when there is but a single dot between the two limiting ones there is an underestimation in place of the overestimation. This is partly due to the ease with which the small number of dots can be grasped, in part to the fact that there is a tendency to compare each half of the line with the whole of the empty line.

Incidentally throughout this article there is a tendency to refer all peculiarities of perception to the laws of attention rather than to eye-moments, although in this as in the preceding article, many of the so-called laws of the attention seem to be suggested by the facts to be explained. The word attention, too, is admittedly used in a vague way, and when defined is given a meaning that is not clearly consistent with the use that has been made of the term in the text.

These three articles are all alike in their method of attacking the problem. All start with unaided introspection. All seem to end in about the same result, the recognition of a problem, with the statement that many factors contribute to the production of a certain end; but that it is very difficult to say what the elements are. In the first paper we are told that visual form is but the recognition of a unity in the constituent lines, but what that unity may be can only be hinted at. In the second we are told that the estimation of visual space is dependent upon the way we attend to the extents, but there is no indication as to why we attend in one way to one figure and in another way to a second figure. Finally in the last article the same set of illusions is explained as due to the different Nebeneindrücke, which affect the comparison, but again there is little success in connecting specific effects with definite causes. While the outcome can hardly be said to be satisfactory, still if recognition of the problem is the first step to its solution, we must regard Schumann's sharply defined statements of these problems as a valuable contribution.

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Ueber Unterschiedsschwellen bei Mischungen von Kontrastfarben. G. HEYMANS. Zeitsch. f. Psychol. u. Physiol. d. Sinnesorgane, XXXII., 38–49, 1903.

In this article the author gives measurements of the perception of differences when contrasting colors are mixed. The colors were mixed by rotating discs with varying amounts of each color, *e. g.*, 0° red, 360° blue green; 60° red, 300° blue green; 120° red, 240° blue green, etc.; and the threshold was determined by the method of minimal changes. The results are summarized as follows:

1. On mixing red and blue green a minimal difference threshold (3.8°) was found when a disc with 55° red and 305° blue green was used. From this point the least perceptible difference increased towards both blue green and red. With 300° red and 60° blue green the threshold (10.8°) was found to be about two and one half times greater than with 55° red and 305° blue green.

2. The minimal threshold for yellow and blue (2.9°) was calculated to be with 214.4° yellow and 145.6° blue. It also increased towards pure yellow and pure blue. When pure blue was used the threshold was 7.4° and with 300° yellow and 60° blue the threshold is about fifty per cent. greater than the minimum.

3. With black alone (black-white series) the minimal threshold was found (0.2°), and the threshold increased almost proportionally

with increase in added white. 300° white and 60° black gave threshold of 3.5°.

Ueber Dunkeladaptation. H. Piper. Zeitsch. f. Psychol. u. Physiol. d. Sinnesorgane, XXXI., 161–214, 1903.

In this article we find a carefully conducted series of experiments upon the adaptation of the eye to darkness. The subject placed in a dark room looked with the peripheral portion of the eye (10°–15° from fovea) at a square (10 cm. wide, and 30 cm. from the eye), which was illuminated at times from the outside. The intensity of the source of light could be changed and the subject determined the least intensity that could be perceived. Measurements were made every few minutes and until this intensity remained at a constant minimum, at which time it was judged that full adaptation had occurred. Binocular and monocular observations were made with variations of light- and darkness-adapted eyes. The results of the experiments thus give an account of the course of adaptation as well as the time of adaptation.

It was found that the sensitivity of the retina in darkness (*i. e.*, the adaptation of the eye), after having been in a condition of good light-adaptation, increased slowly during the first ten minutes, then more rapidly until the maximum was reached. This maximum sensitivity or this ability to perceive slight amounts of light, taken as the time of full adaptation, averaged, for eighteen subjects, 51 minutes.

A series of experiments upon the author (1) with binocular adaptation and monocular determinations of adaptation time, (2) monocular adaptation and monocular determinations, and (3) monocular adaptation and binocular determinations, gave practically the same results. Considerable difference was noted for (4) binocular adaptation and binocular determinations.

The author concludes that his experiments support an hypothesis which considers the adaptation apparatus distinct and different from the intensity and color-perceiving mechanisms.

Ueber das Sehen durch Schleier. Chr. Ladd Franklin and A. Guttmann. Zeitsch. f. Psychol. u. Physiol. d. Sinnesorgane, XXXI., 248–265, 1903.

Mrs. Franklin and Dr. Guttmann here give a preliminary account of some experiments upon visual acuity when objects (test-types) are seen through netting.

Monocular determinations of visual acuteness were made with ladies' veiling (mesh of .57 mm. sq. and quite irregular), with iron gauze (1 mm. sq. mesh), and with two kinds of brass gauze (0.27

mm. sq. mesh, very regular; and 4 mm. sq.), interposed between test-types and the eye of an observer. Variations of the experiment were made by using homatropine or atropine in the observing eye. All the individuals and all the results show a decrease in ability. With the type at a distance of 10 meters, the greatest decrease was found with the nettings at a distance of from 30 cm. to 90 cm. from the eye. The acuteness was greater than this when the nettings were nearer or farther from the eye. In only a very few cases was the ability as great as without the nettings.

Several curves show that a large amount of the loss in ability may be overcome by practice.

The authors suggest for a later article the effects of colored veils, of colored lights and the differentiation of the physical and physiological factors.

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ATTENTION.

Untersuchungen über die sogennannten Aufmerksamkeitsschwankungen. III. E. WIERSMA. Zeitschr. f. Psych. u. Phys. d. Sinnesorg., Bd. 31, Hft. 2, pp. 110–126.

Wiersma continues his investigations, by extending them to cover comparisons of the periods of perceptibility of slight stimuli during the different moods of the insane. Records for the audibility of weak sounds were taken for five hysterical patients during periods of different degrees of depression and it was found that the capacity for perception was uniformly least when the patient was in a depressed mood. No definite parallel could be drawn for the moods of exaltation. In the same way it was found that in a neuræsthenic there was always a reduced perceptive capability during the times when his flights of ideas were most marked. A second neuræsthenic showed a similarly reduced capacity when she suffered most from fixed ideas. The results substantiate Wiersma's earlier contention that the fluctuations of attention are of subjective origin.

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UNIVERSITY OF MICHIGAN.

NEW BOOKS.

Philosophie de l'Effort, essais philosophiques d'un Naturaliste. A. SABATIER. Paris, Alcan, 1903. Pp. 480. 7 Fr. 50. [Twelve essays, ranging all the way from 'Energy and Matter' to 'Prayer,' by the well-known zoölogist and dean of the faculty of sciences of the University of Montpellier.]

La Psychophysique. M. FOUCAULT. Paris, Alcan. 1901. Pp. 491. 7 Fr. 50. [A very interesting and valuable exposition and criticism of psychophysics in the sense of Fechnerism and its development in recent research. Had this work reached us promptly it would have had full notice in the literary reviews of 1901.]

The Higher Realism. D. KEMBLE. Cincinnati, Jennings & Pye; New York, Eaton & Mains, 1903. Pp. 167.

Dei Caratteri e delle Tendenze della filosofia contemporanea. G. VILLA. Pavia, Bizzoni, 1903. Pp. 37.

Evolution, a Lecture. C. WHEDON. Medina, New York, the Author, 1903. Pp. 32.

The Definition of the Psychical. GEO. H. MEAD. Decennial Publications, University of Chicago. From Vol. III. Pp. 38.

Weight of the Central Nervous System of the Frog. H. H. DONALDSON. The Same. From Vol. X. Pp. 15.

The Finer Structure of the Neurones in the Nervous System of the White Rat. S. HATAI. The Same. From Vol. X. Pp. 14.

Scientific Method in Education. ELLA F. YOUNG. The Same. From Vol. III. Pp. 15.

Studies in Logical Theory. JOHN DEWEY (and others). The Same. Second Ser., Vol. XI. Pp. xiii + 388. $2.50.

The Religious Education Association: Proceedings of the First Convention, Chicago, 1903. Chicago, The Association, 1903. Pp. viii + 422.

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Fundamental Problems. P. CARUS. Third Edition. Chicago, Open Court Co., 1903. Pp. xii + 373.

Natick Dictionary. J. H. TRUMBULL. Bull. 25 of the Smithsonian Institution. Washington. Govt. Print. Office, 1903. Pp. xxviii + 347.

Psychological Norms in Men and Women. HELEN B. THOMPSON. Contributions to Philosophy, Vol. IV., No. 1, University of Chicago, 1903. Pp. vii + 188. [Published also in book form as *The Mental Traits of Sex.*]

A Plea for Hedonism. J. C. PALMER, JR. Thesis for Doctorate, University of Wooster. Wooster, Ohio, Herald Printing Co. Pp. 67.

The New Philosophy. S. S. HEBBARD. Chesterfield, Ills., the Author. Pp. 315–408.

Le Problème de Déterminisme social. D. DRAGHICESCO. Paris, Editions de la Grande France, 1903.

Report of the Exploration of the Hayes' Creek Mound, Rockbridge Co., Va. E. P. VALENTINE. Richmond, Valentine Museum. No date.

Recherches sur la vie oscillante; Essai de biodynamique. J. NOÉ. Paris, Alcan, 1903. Pp. 241.

Hypnotism, its History, Practise and Theory. J. MILNE BRAMWELL. London, Grant Richards, 1903. Pp. xiv + 478. 18s. net.

Les Phénomènes psychiques. J. MAXWELL. Préface by CH. RICHET. Paris, Alcan, 1903. 5 fr.

Gehirn und Seele. P. SCHULTZ. Leipzig, Barth, 1903. Pp. viii + 55. M. 1.

La Polemica Leopardiana e C. Leopardi. R. MAZZORI. Rome, Operaia Romana. 1903. Pp. 72.

The Place of Values. G. R. MONTGOMERY. Bridgeport, Conn., the Author, 1903. Pp. 62.

La Morale religieuse et la Morale laïque. CH. LEJEUNE. Paris, Giard et Brière, 1903. Pp. 60. 1 fr. 50.

The Groundwork of Psychology. G. F. STOUT. London, Clive; New York, Hinds and Noble, 1903. Pp. vii + 248.

David Hume and his Influence on Philosophy and Theology. JAMES ORR. New York, Scribners, 1903. (The World's Epoch-Makers.) Pp. ix + 246. $1.25.

Le Mouvement. R. S. WOODWORTH. Paris, Doin, 1903. Bib. intern. de Psych. expér. Pp. 421. 4 fr.

Notes on Child Study. E. L. THORNDIKE. Second edition. Col. Univ. Contr., Vol. VIII., 3–4. [A 'program' publication, useful no doubt in its way, but principally notable for its incompleteness.]

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Les Phénomènes d'Autoscopie. P. SOLLIER. Paris, Alcan, 1903. Pp. 176. 2 fr. 50.

The Laws of Imitation. G. TARDE. Translated by E. C. PARSONS. Preface by F. H. GIDDINGS. New York, Holt, 1903. Pp. xxix + 405.

Social Origins. A. LANG. *Primal Law.* J. J. ATKINSON. New York and London, Longmans, 1903. Pp. xviii + 311.

L'Année psychologique. A. BINET. 9e Ann. Paris, Schleicher, 1903. 666.

Morals: the Psycho-Sociological Basis of Ethics. G. L. DUPRAT. Trans. by G. L. GREENSTREET. Contemp. Science Series. London, Walter Scott, 1903. Pp. xv + 382.

NOTES.

We regret to announce the death, on September 18 at Aberdeen, of Professor Alexander Bain, the psychologist. Professor Bain represented the highest traditions of British thought, and in him culminated one of the great movements in the history of modern psychology. His two volumes 'Senses and Intellect' and 'Emotions and Will' will always remain classics.

M. Ch. Renouvier, the prominent French philosopher, founder of the movement known as *Néo-Criticisme*, died on September 1. A necrological notice and bibliography may be found in the *Revue Philosophique*, October, 1903.

Professor E. W. Scripture has resigned his position at Yale University. Professor Stearns has resigned the chair of Philosophy in the University of Wisconsin.

Professor Witmer, of the University of Pennsylvania, is temporarily filling the chair in Psychology at Lehigh University.

Dr. J. E. Wallace Wallin, of the University of Michigan, has been appointed Demonstrator in Experimental Psychology in Princeton University.

Professor J. Mark Baldwin of Princeton has accepted a call to the new chair of Philosophy and Psychology in the Johns Hopkins University.

After November 1, all editorial communications for the Review, together with books and papers for notice, should be addressed to Professor J. McK. Cattell, Garrison-on-Hudson, New York. Business communications should be addressed as heretofore to Professor H. C. Warren, Princeton, N. J.

INDEX OF NAMES.

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