(5)

910cm 2 7324

THE AMERICAN JOURNAL OF PSYCHOLOGY

III

EDITED BY

G. STANLEY HALL

E. C. SANFORD, Clark University. AND E. B. TITCHENER, Cornell University.

WITH THE CO-OPERATION OF

F. ANGELL, Stanford University; H. BEAUNIS, Universities of Nancy and Paris; I. M. BENTLEY, Cornell University; A. F. CHAMBERLAIN, Clark University; C. F. HODGE, Clark University; A. KIRSCHMANN, University of Toronto; O. KUELPE, University of Würzburg; W. B. PILLSBURY, University of Michigan; A. D. WALLER, University of London; M. F. WASHBURN, Vassar College.

119799 / 14/12/11

VOL. XX

CLARK UNIVERSITY, WORCESTER, MASS.

FLORENCE CHANDLER, Publisher

1909

THE COMMONWEALTH PRESS
O. B. WOOD,
WORCESTER, MASS.

14

TABLE OF CONTENTS

THE AMERICAN
JOURNAL OF PSYCHOLOGY

Founded by G. Stanley Hall in 1887

Vol. XX | JANUARY, 1909 | No. 1

THE PSYCHOPHYSICS OF CLIMATE

By E. B. Titchener

I owe to Professor Cleveland Abbe a number of references to papers of psychophysical import, published for the most part in meteorological journals, which so far as I am aware have escaped the notice of experimental psychologists. Such further search as I have been able to make has brought to light other papers of like character, and I have no doubt that a systematic student of meteorological 'literature' would discover still more. I desire, however, in the present note, simply to call attention to a few points of importance, and to print a very tentative bibliography.

The 'method of classification' or 'method of the psychophysical series', which in theory goes back to Fechner's essay *Ueber ein psychophysisches Grundgesetz und dessen Beziehung zur Schätzung der Sterngrössen*[1], is usually referred, as a method of laboratory practice, to Ebbinghaus (1887) and Jastrow (1888).[2] I find, however, that as early as 1876 experiments were made by J. W. Osborne, of Washington, D. C., upon the classification of sensible temperatures.[3] Osborne's method "consists in obtaining from a sufficiently large number of intelligent persons, their individual estimate of the sensible temperature for certain fixed hours in each day, and then deducing means from the whole record. . . This was accomplished by conceiving the total range of sensible climatic temperature to be divided into twenty equal parts, counting upwards from the extreme of cold. To each of these divisions

[1] *Abhandlungen d. kgl. sächs. Ges. d. Wiss.*, iv., 1859 [1858], 457-532. See my *Exper. Psychol.*, II., ii., 1905, 419.

[2] See my *Exper. Psychol.*, II., ii., 1905, 85, 89, 91.

[3] Determinations of Subjective Temperature, in *Proceedings A. A. A. S.*, 25 Meeting (August, 1876), 1877, 66 ff.

a descriptive expression was affixed conveying in as unambiguous a way as possible, the idea of a progressive elevation of temperature at every step. The observer, in the open air, and sheltered only from the direct rays of the sun, selects the expression which most nearly describes his appreciation of the sensible temperature at the time, and records, not the expression, but the number attached to it. From such records, means and generalisations can be obtained which increase in value as the observers increase in numbers, and gain experience''. The scale of observations, as printed on the outside of the record-card, is as follows: 20 Intolerably hot; 19 Excessively hot; 18 Very hot; 17 Tolerably hot; 16 Very warm; 15 Decidedly warm; 14 Agreeably warm; 13 Mild and soft; then, after an interval, 12 Mild and fresh; 11 Quite fresh; 10 Very fresh; 9 Decidedly cool; 8 Very Cool; 7 Moderately cold; 6 Cold and fine; 5 Cold and sharp; 4 Very cold; 3 Bitterly cold; 2 Painfully cold; 1 Unbearably cold. A foot-note adds: ''However qualified, these expressions must be understood to refer to equal gradations of sensible temperature only''. Under the heading of 'Remarks' are given three scales of letters, which provide for a general characterisation of the weather in terms of wind, humidity and sunshine.

The results obtained from 25 to 35 observers over a period of 9 weeks from June to August are shown in tables of daily observations and means and of weekly means. The writer points out that, ''as was to be expected, the observations do not always coincide for the same day and period [time of day]. This is due to the fact that the system at best is but approximate, that individuals differ in their estimates of temperature, and that living in different localities, some exposed to one wind, and some to another, they are not affected similarly. Nevertheless the means obtained from this large mass of somewhat crude material, have exhibited so remarkable a degree of consistency and harmony as to justify great confidence in the method, which certainly furnishes information relative to climate that has not been hitherto obtained''. Unfortunately, no attempt is made in this paper to compare the subjective estimates of temperature with the ordinary meteorological records. What is significant, however, from the psychophysical point of view, is the writer's assumption (roughly verified by the facts) that the average educated man or woman is able to 'place' a given experience of climatic temperature upon a subjective temperature-scale made up of 19 equal sense-distances between the limits 'intolerably hot' and 'unbearably cold'.

Another and perhaps a still more striking instance of 'psychophysics by common sense' is found in two papers by W. F. Tyler, entitled respectively ''A Scheme for the Comparison of

Climates"[1] and "The Psycho-Physical Aspect of Climate with a Theory Concerning Intensities of Sensation".[2] The writer's aim is to lay down a scale of some sort for the intercomparison of climates, as regards, *e. g.*, their bracing or relaxing character. "It has been assumed that the principal factors forming climate are some five in number", rainfall, soil, altitude, aspect, wind. These five may be reduced to the four: humidity, temperature, sunshine, pressure. And these, again, may practically be reduced to two: humidity and temperature. "It seems likely that temperature and humidity are so incomparably more important and of more effect than the others, that, eliminating these other comparatively unimportant factors, a law may be found (sufficient for practical purposes) connecting the sensation scale with temperature and humidity alone".

"The observations were made as follows: A number of persons of normal condition and regular habits were requested to estimate daily at noon the degree of 'hyther' on a scale of 0 to 10. The word hyther was introduced to indicate the sensation caused by a warm climate [the original observations were made at Shanghai], and supposed to be due to the combined effect of heat and humidity. *Ten* represents the very worst day an observer remembers to have experienced in Shanghai—hot, damp and enervating; while 0 represents an ideal summer's day—warm of course, but bright, brisk and bracing, when, suitably dressed, one suffers no discomfort from temperature and humidity". The hyther or 'hydrotherm' is thus taken as the unit of mugginess.

As to the possibility of a sensation scale, "it is maintained that the mind has an innate, but generally dormant, faculty of subdividing a sensation or emotion". "Many, if not most sensations, have instrumentally measurable conditions corresponding to them in their varying intensities, and the whole object of the writer's idea in regard to sensation scales generally is the establishment of the laws connecting equal differences of these sensations with the instrumental variations corresponding to them, and thereby enable (*sic*) the formation of scales in terms of instrumentally measurable quantities, the degrees of which correspond to equal differences of sensation". A terminology is then worked out. The innate faculty of the mind to graduate is termed indicativeness. "*Sensation* is the physiological effect, of which we are cognizant, of one or more physical causes. A *Sensation Scale* is a means whereby the

---

[1]Journal of Balneology and Climatology, VIII, January, 1904, 17 ff.
[2]London, John Bale, Sons and Danielsson, Ltd. 1907. Reprinted from the *Journal of Tropical Medicine and Hygiene*, April 15, 1907. This second paper in large measure repeats the earlier essay of 1904.

intensity of a sensation can be named. A *Sensation Increment* is the minimum appreciable alteration in the intensity of a sensation. A *Physical Increment* is the amount of variation of the physical cause producing a sensation increment''. Sensation increments are equal (1904, p. 23; 1907, p. 9) and may be summed up; the physical increment is "a variable quantity, and by analogy alone it might be assumed that it is some function of the intensity of the physical cause''. "*Equal Differences of Sensation* are those which have the subjective effect of a similar degree of change''.[1]

Here surely is Fechner *redivivus*,—although Mr. Tyler has never heard of Fechner! The adoption of the just noticeable difference as the unit of sensory measurement; the doctrine that all just noticeable differences of sensation are equal; the contention that just noticeable differences (or other unit-differences of sensation) may be summed up; the standardisation of the sense-scale by reference to a corresponding stimulus-scale: all these things are commonplaces to the reader of the *Elemente*. The writer even vacillates, as Fechner does, between the notion that the single sensation is a measurable magnitude and the notion that it is merely the limiting term of a sense-distance; though it is fair to say that he seems to come nearer than his unknown master to the modern point of view. And indeed, if it were worth while, still other and more detailed parallels might be drawn between Mr. Tyler's positions and the traditional teaching of the psychophysicists.[2]

Fechner, as we all know, at first believed himself to be the discoverer of the law which is called by Weber's name, and only later found that he and Weber had, both alike, been anticipated. And he lays especial emphasis on the century-old classification of the fixed stars by visible magnitude. "Es

---

[1]All of these definitions, except that of equal differences of sensation, are taken from the paper of 1904. In 1907 the sensation scale is defined as "a progression of stimulus intensities such that the differences of corresponding sensation between any consecutive pairs are equal to one another" (p. 9).

[2] The psychophysics of the paper of 1904 was worked out "quite independently," without any sort of technical knowledge of what the author, in his blindness, still calls "the little known subject of intensity of sensation" (1907, 1). Before printing his second paper, Mr. Tyler had been made aware of Weber and Fechner, and had read Landois and Stirling's "Text Book of Human Physiology", Schäfer's "Text Book of Physiology", Mercier's "Psychology, Normal and Morbid", and Ladd's "Psychology, Descriptive and Explanatory". All, alas! secondary sources. He has, however, "seen no reason to alter anything he has written in consequence of what he has read", though he has "embodied in the paper [of 1907] extracts from the authorities named, with his own remarks concerning them",—remarks that are both instructive and amusing.

giebt . . . eine . . . Bewährung des Gesetzes an mehr als nur eben merklichen Unterschieden, zugleich die erste, die überhaupt für das Gesetz existirt, und zwar wiederum auf jenem hohen Beobachtungsfelde, dem die zuerst angeführten Bewährungen entnommen wurden, nämlich in der Schätzungsweise der Sterngrössen''.[1] Mr. Tyler, now, finds outside confirmation of his 'sensation scales', of precisely the same kind. He appeals, first, to Beaufort's scale of wind force. This is a scale of twelve numbers introduced into the British Navy about the year 1805 by Admiral Sir F. Beaufort (1774-1857), for use in recording the apparent strength of the wind. The 0-point of the scale denotes calm, the 12-point a hurricane. "The general impression, even among meteorologists of eminence, appears to be that, because Captain Beaufort distinguished between wind forces according to the amount of sail his vessel [a full-rigged man-of-war] could carry, therefore the division of his scale was a mere arbitrary one. The writer's idea, however, is that, probably unconsciously, he selected twelve wind forces which differed from one another by an 'equal difference of sensation' and he in effect used the sails carried merely as identifying labels''. Eventually, the velocities corresponding to Beaufort's numbers were ascertained, as the means of a large number of estimates; and Mr. Tyler shows that "these velocities are, with only one exception, within 1.5 miles, functions of the corresponding Beaufort numbers, being expressible by the formula:

$$v = 3 + 5\,n + \frac{n(n-1)\ (n-2)}{60}$$

where v is the velocity and n a Beaufort number. This indicates with certainty that the estimate of the wind force by Beaufort's numbers was not according to some arbitrary method, but that there is a definite relationship between the several intensities of sensation. For want of a better way of expressing it the writer says that sensations due to the different degrees of Beaufort's scale differ from one another by an equal degree of sensation''. How Beaufort actually obtained his scale appears not to be certainly known; "several professional meteorologists'' have accepted Mr. Tyler's interpretation (1907, p. 4).

A second bit of confirmatory evidence—which is, however, of a much more doubtful character, and upon which Mr. Tyler accordingly places less reliance—appears in the graduation of bath thermometers. "Certain bath thermometers are marked according to Dr. Forbes' Specification. Concerning who Dr. Forbes was and how he arrived at his graduation the writer

[1] *Elemente der Psychophysik*, i., 1889, 158.

has no knowledge.[1] It always appeared to him that these graduations were obviously empirical. A comparison, however, of them with those obtained [in the writer's own experiments with cold and warm water] by the method of physical increments shows a partial conformity in regard to differences which perhaps points to more than a mere coincidence''. Mr. Tyler, in fact, finds equal sense distances represented by 27, 10, 6 and 4 degrees F., where Forbes' Specification has Cold-Cool, 21 degrees; Cool-Temperate, 12; Temperate to Tepid, 9; and Warm to Hot, 7 degrees.[2]

It is time, however, that we returned to our hythers. With them it fared about as well as could have been expected. The several hyther numbers are plotted on diagrams, in which the temperatures are laid off along the axis of x and the differences between the wet and dry bulbs along the axis of y. In Mr. Tyler's own case, the numbers group themselves into fairly regular zones; in the case of four other observers the zones are distinctly indicated; in the case of seven observers we can say no more than that the higher and lower numbers are divided by a line, and that in all instances the line has approximately the same slope. If, however, the data from the eleven assistants are combined in a single diagram, the resulting curve is very similar to Mr. Tyler's; ''the remarkable conformity between these two curves tends to show that the estimates of No. 1 observer [the writer] were taken with considerable accuracy. There is rarely as great a difference as 1 [one hyther number] between them.'' ''As far as any conclusion can be come to on

[1] The Dr. Forbes in question is no less a person than Sir John Forbes, F. R. S. (1757-1861), whom I am proud to claim, in virtue of his long residence in my native city, as a fellow Cicestrian. See *The Cyclopædia of Practical Medicine*, edited by J. Forbes, A. Tweedie and J. Conolly, i., 1833, art. Bathing, p. 245. Forbes merely says that his scale is ''founded on practical indications''. It is laid out, not in single values (as on current bath thermometers), but in ranges; cold 33-60, cool 60-75, temperate 75-85, tepid 85-92, warm 92-98, and hot 98-112 degrees F.

[2] At the risk of too much detail, I cannot refrain from mentioning that Mr. Tyler attempted to construct a subjective temperature scale by two different methods: that ''of estimating relative intensities in respect to two standard intensities'',—the method of the psychophysical series, or the method of equal sense distances; and that of deducing ''the relative intensities by means of experimentally ascertained 'least observable differences' '',—the method which has been modernised as the method of limits. ''And now'', he says, ''is brought to light a disappointing fact. The two methods of forming a sensation scale result in two different scales''. Not only, then, did Mr. Tyler invent two of the recognised metric methods of psychophysics, but in seeking to correlate their results he has found, in strictly orthodox fashion, the discrepancy that we connect chiefly with the name of Merkel. A step further, and we should have had him discussing absolute impression, and the *R*-error, and Merkel's Law!

the very limited data provided, it would appear that temperature and humidity are certainly the factors of paramount importance in our appreciation of climate, but that some other factor or factors occasionally have appreciable effect.'' Mr. Tyler was unable to connect observations for pressure, wind or nebulosity with the irregularities in the hyther numbers. He offers the tentative formula:

$$H = \frac{d - 1.2\ (d - w) - 66}{3},$$

where H indicates hyther on a scale of 0 to 10 (indicates, that is, the degree of discomfort due to the mugginess of the day), and d and w represent respectively the readings of the dry and wet bulb thermometer. Since for comparatively high humidities d and w are approximately the same, the formula may be simplified to:

$$H = \frac{w - 66}{3},$$

and the degrees of discomfort are thus brought into direct relation to the wet-bulb readings.

It would be superfluous to print, in this *Journal*, a technical criticism of Mr. Tyler's views, methods and results. To the psychologist, the significant thing with him, as with Mr. Osborne, is the effort of psychophysical construction. But it is not superfluous, I hope, to urge the better acquaintance of the psychophysicist and the meteorologist. Experimental psychology has already begun, from its own point of view, the study of the weather: Dexter's *Conduct and the Weather* came out in 1899, and Lehmann and Pedersen's *Das Wetter und unsere Arbeit: experimentelle Untersuchungen über den Einfluss der meteorologischen Faktoren auf die körperliche und seelische Arbeitsfähigkeit* came out in 1907. Now we learn that the meteorologists have these many years been engaged, for their purposes and from their point of view, upon the problem of 'subjective climate' or 'sensible temperature'. The solution of that problem, however, demands a combination of the two techniques and of the two standpoints. I have no doubt that solution, at any rate in the rough, is possible with the means at hand. Something can be done by the mutual printing of essays and reviews in the professional journals, by the exchange of papers, by personal correspondence. But I should like to see some of the younger generation of climatologists pass through the drill of the psychological laboratory, and I should like to see some student of experimental psychology take up the meteorological problem. The running analysis of the titles given below in the bibliography may help to indicate precisely where that problem lies.—

Just as I am about to send the MS. of this note to the printer, I receive the *Zeits. f. Psychol.*, xlix., Heft 3-4, which contains an article by J. Plassmann on "Astronomie und Psychologie." It is clear that the technical 'literature' of astronomy offers to the psychophysicist a mine of unworked materials at least as valuable as those that I have found in the works upon meteorology. The extraction and refining of the metal, in both cases, will require time and labor; but it is greatly to be hoped that some interested student may undertake the task.

References[1]

1826. W. Heberden, An Account of the Heat of July, 1825; together with Some Remarks upon Sensible Cold. *Phil. Trans. Roy. Soc. Lond.*, 1826, pt. II, 69-74. [Suggests that the way to estimate 'sensible cold' would be to warm a thermometer (on the assumption that the radiating surface of the instrument is analogous to that of the body) "to a height something exceeding the natural heat of the human body, and then to observe at what rate the quicksilver contracted upon exposure to the air".]

1853. J. C. Houzeau, *Règles de climatologie*, pp. 59 f. [Proposes to take the readings of the wet-bulb thermometer as measures of sensible temperature.]

1860. C. Martins, Des causes du froid sur les hautes montagnes. *Annales de chimie et de physique*, 3d series, lviii., pp. 208-243. [Emphasises the importance, for subjective temperature, of the wind and of the direct action of the sun, in addition to the temperature of the air.—See also Du froid thermomètrique et de ses relations avec le froid physiologique. *Montpellier Acad. Sci. Mém.*, IV., 1858-60, pp. 251-301. Conditions subjectives qui modifient la sensation du froid. *Brown-Sequard's Journ. de Physiol.*, III., 1860, 597-603.]

1873. J. C. Houzeau, *Patria Belgica*, 1 re partie, p. 12. [Again recommends the wet-bulb thermometer, and gives tables.]

1876. J. W. Osborne, On a New Meteorological Instrument. *Proceedings A. A. A. S.*, 24 Meeting (August, 1875), 1876, Part 1, Physics, 59-70. [Refers sensible climatic heat to the three factors: temperature of the air, relative humidity, and force or velocity of wind. Proposes to measure sensible temperature by the time required for a mass of water, heated to the temperature of the human body, and contained in a paper cylinder of special construction, to cool under varying conditions.]

1877. J. W. Osborne, Determinations of Subjective Temperature. *Proceedings A. A. A. S.*, 25 Meeting (August, 1876), 1877, 66-74. [The paper referred to in the text. The author, at the end, expresses the hope that he may be able, on a future occasion, to compare his subjective results with the readings of the instrument described in the previous year. I have not been able to find any such comparison.—See also *Bulletin of the Philos. Soc. of Washington*, ii., p. 63; *Smithsonian Miscellaneous Collections*, xx., 1881.]

---

[1]For aid in the compilation of this bibliography I am especially indebted to the Messrs. Cleveland Abbe, Sr. and Jr., to the librarian of the Surgeon-General's Office, Washington, D. C., and to Professor R. de C. Ward, of Harvard University.

1879. G. Forbes, On Observations relative to the Temperatures to which the Human Body is Exposed. *Journal of the Scottish Meteorological Society*, N. S., V., 1880, Nos. 49-62, pp. 273-4. [Describes an instrument similar to that of Osborne, and devised for the same purpose.]

1882. A. Borius, *Les maladies du Sénégal*, Baillière, Paris. [I have not seen this work. According to Vincent, 1907, the author argues (pp. 124-240) that the depressing or relaxing character of tropical climates is due to excessive humidity. He proposes, but does not use, a sensation scale: 0 Very cold, 5 Indifferent, 10 Extremely hot. Intermediate degrees are not defined.]

1885. C. Abbe, Meteorology. In *Smithsonian Report* for 1883, 483-569. [Report of scientific progress during 1883, with bibliography. Declares that 'exhilarating' and 'depressing' weather may be regarded as functions of air temperature, wind, barometric pressure and relative humidity. Refers to Osborne: p. 491; advocates special observation of days in which the human organism experiences such special feelings as are defined by the expressions: close, oppressive, harsh, raw, penetrating, chill, mild, soft, soothing, invigorating, exhilarating, stimulating, balmy, gloomy, cheerful, nervous, restless.]

1887. C. Smart, The Thermometer as a Climatological Instrument. *Trans. Internat. Medical Congress, 9th Session*, Vol. V., pp. 172-178. [Determines the combinations of air-temperature and wind-velocity that produce, in the unit of time, the same drop of the thermometer (from 98.4° F.) as a given degree of cold in a calm atmosphere. Argues that the "cooling effect" or the "demand on the powers of the system" in the two cases is the same.]

1890. J. Vincent, La détermination de la température climatologique. Brussels, 1890. Reprinted from *Annuaire de l'Observatoire Royal pour 1890*. Abstract in *Ciel et Terre*, 10e année, 1889-1890, p. 515. [I have not seen this paper. According to Hann, pp. 43 f., "Vincent undertook an extended series of investigations in order to show the relation between the temperature of the exposed surface of the skin (T); the air temperature (t); the excess of the temperature as indicated by the actinometer over that of the air, in degrees (d); and the velocity of the wind in meters per second (v)." He found the approximate formula:

$$T = 26.5° + 0.3° \, t + 0.2° \, d - 1.2 \, v.$$

The degrees are C. According to Van Bebber, p. 133, Vincent established further a seven-degree scale of temperature sensation as follows: 1 Very hot, copious sweat, discomfort, T over 37.5°; 2 Hot, sweat, little or no discomfort, T 34.5°-37.5°; 3 Warm, no sweat, T 31.5°-34.5°; 4 Moderate, indifferent condition,—one may sit in the open air without an overcoat,—T 29°-31.5°; 5 Fresh, cold, but not unpleasantly cold to the hands,—one cannot sit in the open without a coat,—T 26°-29°; 6 Cold, unpleasant to the hands, T 22°-26°; 7 Very cold, unbearable to the hands and unpleasant to the face, T under 22°.]

1889. H. F. Blanford, *Climates and Weather of India, Ceylon and Burmah*, Macmillan & Co., London, p. 48. [Contrasts the oppressiveness of a humid Red Sea temperature of 90° with the dry hot-wind season of the upper provinces of India at a temperature of 112° to 118°, when the conditions, "if not exactly agreeable, are borne without serious inconvenience."]

1892. M. W. Harrington, Climate and Meteorology of Death Valley, California, *U. S. Dept. of Agriculture, Weather Bureau Bulletin, No. 1*, pp. 50. [Remarks, p. 23, that the "most interesting form of humidity by which to judge of the comfort to be found in residence at a place" is the relative humidity. The low relative humidity of the valley, together with the large air-motions, causes evaporation to proceed with very great rapidity. Again, p. 30, "a good measure of the rigor of a climate is to be found in the mean daily range of temperature." The diurnal ranges are great in the valley, but are equalled and surpassed elsewhere.]

1892-3. A. Piche, Le déperditomètre. *Assoc. française pour l'avancement des sciences*, Session de 1892, i., 195; ii., 296-300. [Describes an instrument that should measure the volume of gas or alcohol consumed in maintaining for a definite length of time a known volume of water at a uniform temperature of 37° C. under varying meteorological conditions.—The description is repeated in the *Bulletin mensuel de l' Observatoire Carlier d' Orthez*, 7e année, No. 1, January, 1901.]

1894. M. W. Harrington, Sensible Temperatures. *Intern. Med. Mag.*, iii., August, 481-485. Also published separately, as paper read before the American Climatological Association, Washington, D. C., May, 1894: pp. 7, with Discussion and three Plates. Abstract in *Amer. Metl. Journ.*, July, 1895, 93-95. [Sensible temperatures depend on evaporation; and when evaporation takes place they are invariably lower than the shade temperatures given in meteorological tables. In actual practice, the temperature of evaporation is taken by means of a wet-bulb thermometer. The author gives illustrative charts of (1) the reduction of meau temperatures over the United States, for a given period, due to evaporation, and (2) the resulting sensible temperatures.]

1894. C. Abbe, Humidity. *Monthly Weather Review*, xxii., 407 f., 453 f., 496 f. [Note on wet-bulb or sensible temperatures, with references. Printed for the first time in Oct., 1894; reprinted, with slight modification and abridgment, till Octr. 1895, when doubt is cast upon the correlation. In xxiii., 368, we are told that "a complete ezpression for the relation between atmospheric conditions and nervous sensations is under consideration, but has not yet been obtained." See also Summary for 1895, xxiii., 491; the wet-bulb readings are given for "the sake of certain studies in Hygiene." In xxiv., Jan. 1896, 3 the statement is made that the sensation of temperature depends on the temperature of the air, its dryness, the velocity of the wind, and the suddenness of atmospheric changes, all combined with the physiological condition of the observer. A complete expression of sensible temperature has not yet been obtained. See also Summary for 1896, xxiv., 488.]

1895. W. J. van Bebber, *Hygienische Meteorologie*. Enke, Stuttgart, pp. 124-145. [Discussion of the "Hygienische Bedeutnng der Wärmeerscheinungen." Very hot temperatures are endurable, if the air is dry and, more especially, if it is in motion; a moist day is oppressive with a temperature of only 30° C. (p. 130). Dry-air baths do no harm at 80°, while steam-baths may be injurious at 50° or less (p. 136). The injurious effect of low temperatures is due to the cold, to the dryness of the air (ct. Hann, pp. 54 f.), and to a violent air-motion (p. 138); yet we are especially liable to take cold in damp, tropical climates and in a climate "in welchem *nass*kaltes und windiges Wetter vorherrscht" (p. 143). Heat-prostration is due mainly to air-temperature and humidity; windlessness also plays a part (p. 137).]

1895. C. Abbe, Sensible Temperatures. *Monthly Weather Review*, xxiii., March, 93 f. [Historical note.]

1895. B. S. Pague, Sensible Temperatures, or the Effect of Heat on the Body in California. *Amer. Metl. Journ.*, xii., No. 6, Oct., 1895, 196-198. [Equates wet-bulb readings with sensible temperatures. Applies this correlation to the climate of Death Valley, etc. Sunstroke is due to heat and humidity.]

1896. W. F. R. Phillips, Sunstroke weather of August, 1906. *Monthly Weather Review*, xxiv., Novr., 409-413. [Besides heat, "evidently there must enter into the case another factor, namely, the accommodation of the individual to average physical environment, or the climatic equation."]

1896. W. F. R. Phillips, Sunstroke in California and Arizona. *Monthly Weather Review*, xxiv., Dec., 454-456. [These sunstrokes are "not caused by the traditional high relative humidity." References.]

1896. W. F. R. Phillips, Sensible Temperature. *Trans. Amer. Climatol. Assn.*, xii., 16-25. [Opens with an historical review of the subject. The writer correlated Osborne's subjective results with the synchronous meteorological observations, but "was unable to discover more than a very general agreement." He admits, nevertheless, that the uniformity of the records "does appear to suggest something more than accident." He discusses "the principal factors concerned in the determination of temperature sensation" and shows incidentally "why failure (to establish an instrumental register of sensible temperature) has so far been inevitable."]

1896. W. L. Moore. Some Climatic Features of the Arid Regions. *U. S. Dept. of Agriculture, Weather Bureau*, pp. 19, with 5 Plates. Communication to National Irrigation Congress, 5th annual session, Phœnix, Ariz., Decr. 15-17, 1896. ["The meteorological instrument that registers the temperature of evaporation and thus in a great measure the actual heat felt by the human body, is the wet-bulb thermometer." On Sept. 20, 1895, instructions were issued "to the observers of the weather service to begin the telegraphing from observation stations of the readings of the wet-bulb thermometer, more popularly known as the 'sensible' temperature. This is about the temperature felt by animal life." Charts are appended, showing the distribution of average actual and sensible temperatures over the United States.]

1896. W A. Glassford, Why Summer in the Apparently Hot Arid Region is Comfortable. *Monthly Report, Oregon State Weather Service*, Jan., 1896, 23-29. [Equates 'sensible temperature' with the reading of the wet-bulb thermometer.]

1896. I. M. Cline, Influences of Climatic Conditions and Weather Changes on the Functions of the Skin. *Proc. Texas State Med. Assn.* Reprint, pp. 8, with a Plate. [Considers the functions of the skin, as influenced by weather changes and climatic conditions, under the headings of Secretion and Excretion, and Regulation of the Evolution of Heat. Both sets of functions are affected mainly by temperature and absolute humidity. The influence of these climatic factors is discussed in detail for the six types of climate in the United States: low damp warm, low damp cold, high dry, between low damp warm and low damp cold, between low damp warm and high dry, and between low damp cold and high dry.]

1897. W. F. R. Phillips, Clothing and Temperature. *Monthly Weather*

Review, xxv., May, 200-201. [Thermometrical observations, paralleled by estimate of subjective sensations. References to Rubner and van Bebber.]

1897. M. Rubner und V. A. Lewaschew, *Archiv f. Hygiene*, xxix., pp. 1 ff. Abstract in *Meteorologische Zeitschrift*, xv., 1898, pp. 148 f. [I have seen only the abstract of this paper. It contains a report of experiments on the human subject, carried out in the respiration chamber. At low temperatures, 14-15° C., dry air is more comfortable than moist. Between 24° and 29° dry air appears cooler than moist; high temperatures are not uncomfortable if the air is very dry; visible sweat begins at 29° with a relative humidity of 22%. A relative humidity of 96% makes a temperature of 24° unendurable for any length of time. Evaporation (loss of water-vapor) is a continuous function of temperature both in dry and in moist air, but the course of the function varies very considerably with the relative humidity.]

1898. C. Abbe, Sensible Temperatures and the Curve of Comfort. *Monthly Weather Review*, xxvi., August, 362 f. [Suggests that diagrams be prepared, with a vertical scale of relative humidities from 0 to 100 and a horizontal scale of air-temperature from *minus* 10 to 100, and that the observer make a record whenever he feels like saying: "Well, this weather is just perfect."]

1899. C. Abbe, Sensible Temperatures. *Monthly Weather Review* xxvii., 18. [Brief comment on a suggestion in the *N. Y. Times* that the Weather Bureau "combine the figures indicating temperature, humidity and velocity of the winds into a single figure that would express just what people mean when they say and feel that the weather is hot or cold."]

1899. A. Lancaster, *De la manière d'utiliser les observations hygrométriques*. Rapport lu au Ve Congrès International d' Hydrologie, de Climatologie et de Géologie Médicales à Liège, 1898. Vaillant-Carmanne, Liège, 1899. [A comparative study of the combined effect of air-temperature and relative humidity at Brussels and at Vivi in the Congo Free State. The author sought to determine, in calm weather at Brussels, what combinations gave certain high temperatures, in his own case, an oppressive feeling of heat. He gives a numerical table of results. According to Tyler, 1907, 31, "Lancaster's oppression curve coincides very closely with the position which the curve for Hyther 1 would occupy. Now Hyther 1 is a condition when very lightly dressed in a hot climate one begins to feel discomfort. The same condition in Belgium experienced by an observer with heavy European clothing might well be said to be oppressive."]

1900. E. G. Ravenstein, The Geographical Distribution of Relative Humidity. In *Report of 70th Meeting of Brit. Ass. Adv. Sci.*, 817-818. [In temperate weather we can bear great humidity with equanimity, while the same degree of humidity accompanied by great heat may be unbearable. The writer maps the earth according to sixteen hygrothermal types of climate.]

1903. J. Hann, *Handbook of Climatology. Pt. i., General Climatology.* Translated by R. de C. Ward. New York, The Macmillan Co., 1903. pp. 43-46, 82 f. [Brief review of previous work on sensible or subjective temperature. The temperature which we actually experience depends upon air temperature, air movement, insolation and the humidity of the air. "In hot climates, and also in the summer of middle and higher latitudes, when the body is usually covered with perspiration, the temperature which is actually felt depends to a great extent upon the dryness of the air, or, to put it more plainly, upon the reading of the wet-bulb thermometer. . . In the case of hot climates it would be advisable to include, among the climatic elements, the readings of the wet-bulb thermometer as a convenient index of the degree of heat which is actually felt by the human body". Refers critically to Abbe's proposal of 1883 (to classify days as harsh, raw, mild, etc.) and to Osborne's sensation scale of 1876. States that both very hot (p. 45) and very cold (pp. 54 f.) climates are more endurable when the air is dry.]

1904. W. F. Tyler, The Sensation of Discomfort. *Monthly Weather Review*, xxxii., 217. [Letter written from Shanghai, communicated by the editor, who adds comments on the writer's Scheme for the Comparison of Climates, and refers to Osborne. The editor remarks that the use of the wet-bulb reading as a measure of sensible temperature has "long since been given up."]

1904. W. F. Tyler, A Scheme for the Comparison of Climates. *The Journal of Balneology and Climatology*, viii., January, 1904, pp. 17-44, with plates. Not offprinted: price of number 2/-. [Paper referred to in the text.]

1904. R. de C. Ward, "Sensible Temperatures." *Bulletin of the American Geographical Society*, xxxvi., no. 3, 129-138. [Summary of factors concerned in sensible temperature. General review of work done, with appended bibliography.]

1906. R. Börnstein, *Leitfaden der Wetterkunde*, 2d ed., Braunschweig, p. 48. [Emphasises the importance of relative humidity for sensible temperature.]

1907. W. F. Tyler, Hythers and the Comparison of Climates. *Monthly Weather Review*, xxxv, June, 267 f. [Letter written from Shanghai in 1905, briefly discussing the 'comfort curve', the use of hythers, etc. The editor appends a bibliography.]

1907. W. F. Tyler, The Psycho-Physical Aspect of Climate with a Theory concerning Intensities of Sensation. London, John Bale, Sons & Danielsson, Ltd. Pp. 45, with plates. 5/-. Reprinted from the *Journal of Tropical Medicine and Hygiene*, April 15, 1907. [Paper referred to in the text.]

1907. J. Vincent, Nouvelles recherches sur la température climatologique. *Annales météorologiques* de l'Observatoire royal de Belgique, année 1907, nouvelle série. In extract, pp. 120. Abstracts in *Ciel et Terre*, xxviii., 1907, p. 25; *Annuaire météorologique* de l'Observatoire royal, 1908, p. 462. [In this paper Vincent modifies his formula of 1890 to read (with the omission of the d-values, which are discarded as empirical only):

$$T=30.1+0.2t-v\ (4.12-0.13t).$$

This formula "permet de calculer la température de la peau à l'ombre, lorsque l'on connait la température de l'air et la vitesse du vent. On ne doit y recourir que lorsque t est supérieur à 17°".—The most important point of the paper, however, is the ruling out of humidity as a factor of sensible temperature, under the conditions of the author's observations. His general conclusion reads: "lorsque la température superficielle de la main de l'homme est inférieure à celle qui provoque la production de sueur visible, ce qui est le cas le plus fréquent dans les climats tempérés, l' humidité de l'air n'a aucune influence sur notre sensation thermique et ne doit pas être considérée dans l'étude de cette sensation." And he writes in detail: "l' humidité de l'air . . . . n'a aucune influence sur la température superficielle de notre corps, dans les conditions atmosphériques ou nous avons opéré. . . . Le thermomètre mouillé, même immobile, suit bien les fluctuations de l'humidité; il devrait en être de même de la surface de notre corps, si cette surface était réellement comparable à un linge mouillé; mais qui ne voit qu'en général elle ne l'est pas? . . . . La capacité de l'air ambiant pour la vapeur d'eau, capacité que la chaleur propre du corps élève et empêche de devenir nulle, est toujours suffisante, quelle que soit l'humidité relative, pour vaporiser immédiatement et complètement l'eau qui atteint la surface de notre corps. Lorsque la température de la peau vient à s'élever suffisamment sous l'action des influences atmosphériques, les glandes sudoripares deviennent très actives et déversent à la surface de la peau la sueur qu'elles ont secretée abondamment. Alors seulement notre corps peut être comparé jusqu'à un certain point au thermomètre mouillé, puisqu'il s'y accomplit une évaporation dans le sens habituel du mot, sur l'activité de laquelle l'humiditè de l'air doit, cette fois, exercer une influence. Il ne faudra, en tout cas, jamais perdre de vue que notre corps a sa chaleur propre, ce qui constitue une différence importante avec le thermomètre mouillé. . . . D'ordinaire, notre peau ne peut être assimilée à un linge mouillé: cela est clair, puisqu'elle est presque parfaitement sèche. On raisonne pourtant à son propos comme si elle était toujours couverte de sueur. . . . La température de la peau mouillée est notablement inférieure à celle de la peau sèche; elle est, par contre, supérieure, et de beaucoup, à celle du thermomètre mouillé; les différences sont, du reste, loin d'être constantes." Vincent further discusses, with approval, Heberden's indirect method, and gives a bibliography of previous work, with critical notes. He advises that, whatever form of objective method be adopted, the observer should append to his records a statement of the subjective temperature in the terms 'very hot', 'hot', 'warm', 'mild', 'cool', 'cold' and 'very cold.' This scale may be roughly standardised as follows: Very hot, 37.5° and over; Hot, 34 5° to 37.4°; Warm, 32.4° to 34.4°; Mild, 27° to 32.3°; Cool, 26.9° and under. The point of transition from Cool to Mild is uncertain; the points of transition from Cool to Cold and from Cold to Very cold are still more uncertain. These determinations have, however, little practical importance, as we meet cold weather by appropriate clothing.]

A DEMONSTRATIONAL COLOR–PYRAMID

By E. B. TITCHENER

It is usual, in text-books and lecture courses, to represent the system of visual qualities by means of some tridimensional figure, sphere or double cone or hexahedron (double three-sided pyramid) or octahedron (double four-sided pyramid). I have long felt the need of a model of this figure,—a large model that should show, by actual pigment, the distribution of hue and tint and chroma in the visual system. In the course of a series of lectures on light and color, delivered during the summer of 1908 to a class of art-students, I was fortunate enough to secure the interest and cooperation of Mr. Louis Agassiz Fuertes, whose skill as a colorist requires no advertisement. For the immediate needs of the class, we constructed in cardboard a small water-colored model of the regular octahedral double pyramid; and this model did such good service that we judged it worth while to build a larger and more durable pyramid for regular use in class-room and laboratory.

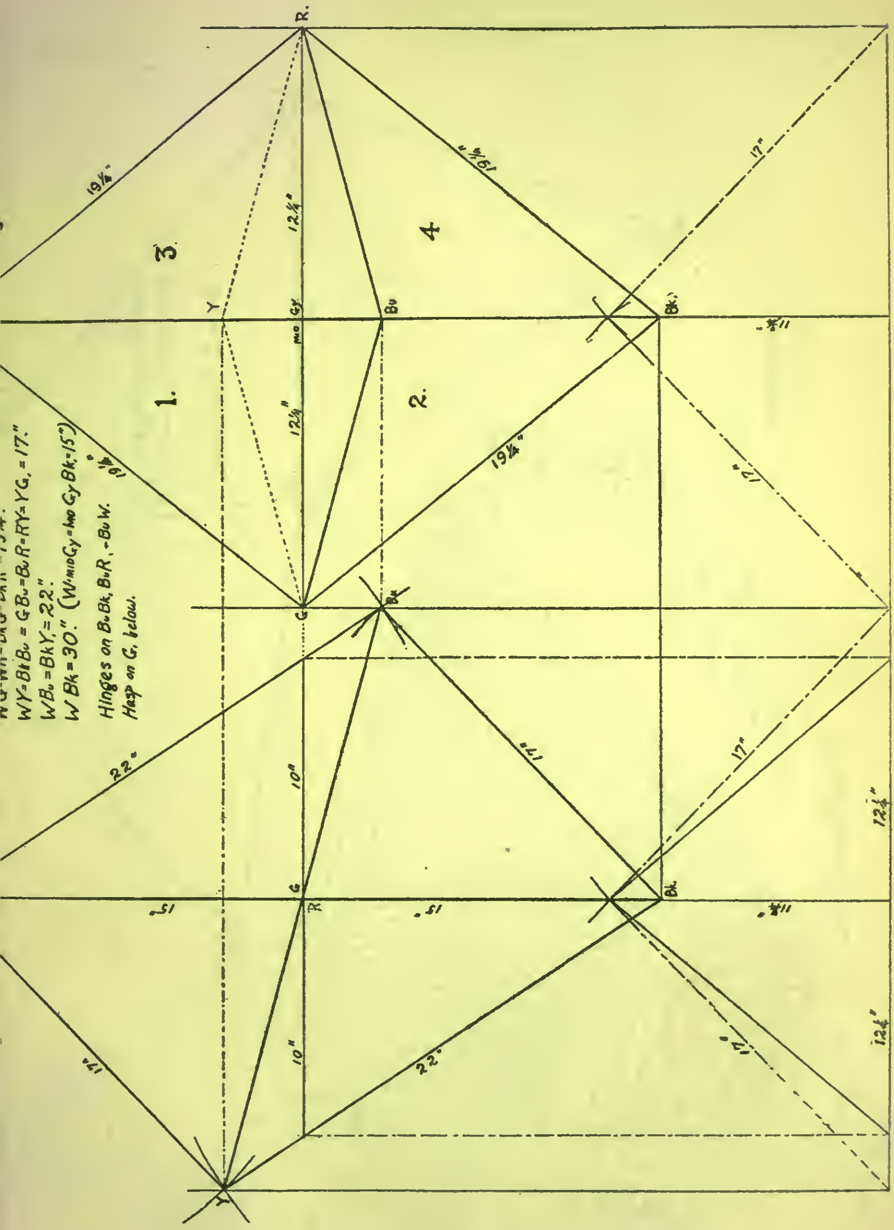
The earliest colored *chart* with which I am acquainted is Lambert's single three-sided pyramid (J. H. Lambert, Beschreibung einer mit dem Calauschen Wachse ausgemalten Farbenpyramide, etc., Berlin, 1772). Aspects of the color-sphere are shown in colors by Runge and Steffens, in their Farbenkugel oder Construction der Verhältnisse aller Mischungen der Farben in der Natur, Hamburg, 1810. Many books contain colored plates that may, at a pinch, be useful to the lecturer: I mention, *e. g.*, the chromatic scale of tones printed as Plate v., and the chromatic circle of hues printed as Plate vi., in Chevreul's Principles of Harmony and Contrast of Colors, 3d ed., London, 1890. The Prang charts and Nendel's Farbenkreis should also be referred to; and the painted metal plate of equated Urfarben and grey, contained in Hegg's Die invariablen aequivalenten Oelpigmente zur Farbenperimetrie, is indispensable. Since, however, the system of visual qualities is in fact tridimensional, colored charts can never be wholly satisfactory; they give, at the best, a series of typical aspects which must be imaginatively combined by the spectator.

In this connection I am tempted to remark that the two-dimensional representation of a tridimensional system in Ross' Theory of Pure Design (1907) has, in my opinion, brought the author into serious conflict with psychological fact. Ross admits three variables in the color impression, value, color and intensity (tint, hue and chroma). His classificatory diagram 4 (facing p. 140) is, however, essentially two-dimensional, though it can be translated into a color pyramid if we ignore the extreme length of the lines R-Y and B-R (due apparently to the existence of the color-names orange and violet) and the extreme lowering of V (due in part, perhaps, to the exigencies of the diagram itself, and in part to the darkness of V in the ordinary dispersion spectrum: see p. 139). No one can quarrel in principle with empirical rules of procedure; but one may object to the distortion of theory, *i. e.*, in this case, of psychological observation.[1] There seems, indeed, to be not even an empirical reason for the lowering of V; such, at any rate, was Mr. Fuertes' experience in the painting of our model.

A *model* of the double cone, 12 in. from pole to pole and 8 in. in diameter at the base, was put upon the market ($4.00) by E. G. Willyoung & Co. in 1896. I am informed that the model showed no color, but simply copied, in black and white, Fig. 88 of Scripture's Thinking, Feeling, Doing, 1895, p. 158 (*cf.*, however, The New Psychology, 1897, pp. 345 ff., Figg. 87, 88). Two models, a "Farben-Oktaeder, zerlegbar in die acht Oktanten" and a "Farben-Doppeltetraeder", are listed by Höfler and Witasek in their Psychologische Schulversuche mit Angabe der Apparate, Leipzig, 1900, p. 4 and 1903, p. 5. The former "zeigt am Umfange des horizontalen Achsenschnittes die gesättigten Farbentöne (Grund- und Mischfarben), an den Spitzen der vertikalen Achsen Weiss und Schwarz, an den Innenschnitten die Anteile verschiedener Grau an den nicht gesättigten Farben". Details of construction are promised, in both editions, for a forthcoming number of the *Zeitschrift f. Psychol.*, but the article has not appeared. The cuts suggest small models of painted wood or plaster; the price (Kr. 10.60 and Kr. 4 respectively: say $2.25 and $0.85) seems to indicate that the colors and greys are only roughly represented.

The octahedral double pyramid, which embodies the fundamental facts of the 'antagonistic' theory of visual sensation, was figured in 1897 by Höfler and Ebbinghaus. Höfler's pyramid has the form of a regular octahedron (*Psychologie*, 1897, p. 113, Fig. 12; *Grundlehren der Psychologie*, 1897, p. 35, Fig. 9). Ebbinghaus brings the figure into better accordance with psychological requirement by tilting the Y-angle of the base up towards W, and by rounding off the poles and base-angles (*Grundzüge d. Psychol.*, erster Halbband, 1897, p. 184, Fig. 15; or i., 1905, p. 199, Fig. 18). We decided to tilt the base of our model, partly because the relative displacement of Y and its antagonistic B helps the student to grasp the meaning of 'tint' as applied to color, and partly because the painting of the model in this form is much easier; theory and practice are, so far, in agreement. We did not, however, round off the poles and the corners of the base. There would have been no appreciable gain, that we could see, on the technical side; the model would have been more expensive; and there would have been incorporated in it a point of exposition which, as experience shows, may more safely be postponed. Our own pyramid, then, as painted by Mr. Fuertes, finally

[1] *Cf.* A. H. Munsell: A Color Notation, 1905, 12. "Two dimensions fail to describe a color. Much of the popular misunderstanding of color is caused by ignorance of these three dimensions or by an attempt to make two dimensions do the work of three." Munsell uses hue, value and chroma for the three qualitative attributes that I term hue, tint and chroma.


WY=BkBu = GBu=BuR=RY=YG, =17."
WBu =BkY, =22."
Hinges on BuBk, BuR, -BuW.
Hasp on G, below.
1.
2.
3.
4.

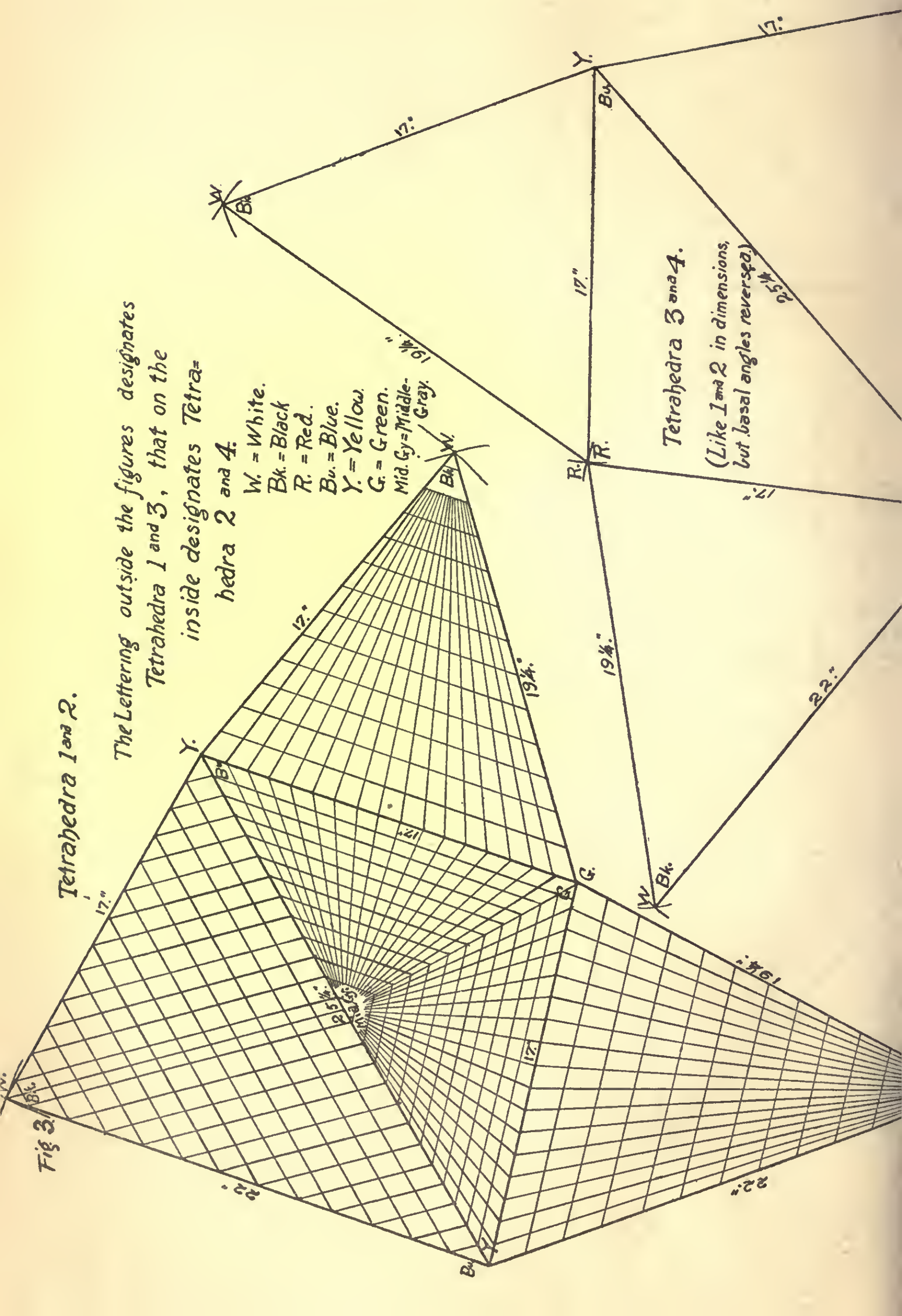

Tetrahedra 1 and 2.
The Lettering outside the figures designates Tetrahedra 1 and 3, that on the inside designates Tetrahedra 2 and 4.
W. = White.
Bk. = Black
R. = Red.
Bu. = Blue.
Y. = Yellow.
G. = Green.
Mid. Gy = Middle-Gray.
Tetrahedra 3 and 4.
(Like 1 and 2 in dimensions, but basal angles reversed.)
17."
19¼."
22."
25¼."

Fig 3.

PLATE I

assumed the form shown in the accompanying photographs. I append details of its manufacture, in the hope that they may be of value to other psychologists in lecturing to large undergraduate classes: the model saves at least a full hour of classroom time, and a good deal of unnecessary puzzling on the part of the student. In case the cost still appears excessive, I suggest that the figure may easily be constructed (from the plans here given) of heavy white cardboard, with white cloth hinges, and that a very moderate degree of skill in water-coloring will suffice to produce a serviceable model.

To construct the unit-tetrahedra, lay off the triangle BuYG: BuY = 25.25, YG = 17, GBu = 17 in. On BuY as a base, lay off BuW = 22 and YW = 17 in. On YG as a base, lay off YW = 17 and GW = 19.25 in. Finally, on GBu as a base, lay off GW = 19.25 and WBu = 22 in. These four triangles placed together will make tetrahedra 1 and 2; with the same measurements, but with the angles reversed, they will make tetrahedra 3 and 4. The four tetrahedra, fitted together as in Figg. 1 and 2, form the double pyramid required.

The material used is specially selected, trebly seasoned 'state' white-wood, ⅝-in. thick. Attached firmly to the angle GBk is a forked walnut standard which, seated at the base in a strong swivel socket, allows the model to be revolved about a vertical axis. The standard is fixed to a cast-iron base, 16 by 16 by 1.5 in. Two small pin-hinges, set as closely as possible to the surfaces, are placed on the edge BuBk; two others on BuR; and two others on BuW. These, while holding the model together in one piece, allow of its being opened to show a vertical section through W, Bu, Y, Bk and to show a peripheral section through R, Y, G, Bu and middle grey. A single pin-hasp, hooking from G slightly below the periphery and across the edge BkG, locks the entire model.

To ensure uniformity of gradation in the coloring, the entire surface of all the tetrahedra was plotted, as shown in Fig. 3, on a unit of one inch, taken on the sides of the base triangles and thence projected upon the remaining three faces. On the surfaces of section Bk, Bu, W, Y, the figures thus formed are rhomboids which decrease by twos in the direction from base to apex; on all the other surfaces the figures are rhomboids which show the same number (17) in all rows. The plotting was done with a hard pencil-point on the raw wood.

To prepare for the coloring, the whole of the upper pyramid and the upper half of the lower pyramid were first painted with a good coat of flake white mixed in pure boiled linseed oil. The lower part of the lower pyramid was simply given a thorough oiling, the white being brushed out evenly along the line of junction. After this treatment, the model was allowed to stand for three weeks. It was then entirely dry, and no color could 'sink' after application.

The colors used were J. H Hatfield's hand-ground oil colors, and the palette was selected by Mr. Hatfield with special reference to our requirement of pigments that should produce purest hues of maximal chroma, and should not interact chemically or change on long exposure to light. The paints selected were: zinc white, flake white, ivory black, vert emeraude, aureolin yellow, light cadmium yellow, cadmium orange, orange vermilion, fast deep vermilion, rose madder, cobalt violet, ultra-marine blue, American cobalt blue and French cobalt blue. These colors, lightened with white only, make it possible to render the outer surfaces at high chroma, even at very light tints; they also furnish a high chroma for the lower pyramid. It was found that the blues required a good deal of lifting even to reach the 'pure blue' level.—The addition to the above list of a tube of burnt sienna simplifies the complicated mixing for dark yellows, which tend strongly to fall over towards green or red.

The first step of the painting was to work out carefully the colorless series along the BkW axis. This was laid off in 25 stages, with pure W and pure Bk at the poles, and 23 graded neutrals between. Enough of the neutrals was mixed to modify all the chromas of Bu and Y, between the limits of black and white, on the vertical section, and enough of middle grey to modify all the hues of the peripheral section.

In painting the outside faces, the periphery was first established, all four tetrahedra being thus far carried on together. The start was made at Y with light cadmium, which was taken to express the maximal chroma of yellow. This was worked, step by step, into cadmium orange, pure, and thence through orange vermilion into fast deep vermilion, which expresses a nearly perfect red at maximal chroma. The f. d. vermilion was then worked through rose madder (with white) and cobalt violet to cobalt blue (with white) which forms the note at Bu. This mixture gives a very good middle blue, which works evenly into a green-blue with the addition of vert emeraude, which in turn is nearly perfect (needing only a little white) for blue-green. If the white is now gradually replaced by light cadmium, an excellent high-chroma green is obtained for G; and serial reduction of the green and increase of the light cadmium gives the GY line. These colors form the first row on both planes of all edges of the periphery.

The faces towards W are produced simply by serial addition of white to the notes already established. Those towards Bk offer a much more serious problem; the mechanical mixture of black with the hues of the periphery does not result in the required gradations. Red must be worked down through rose madder *plus* orange vermilion, with black added where necessary. Blue, as the white component of Bu is diminished, becomes darker in tint without loss of chroma to a point nearly (if not quite) half-way towards Bk. Vert emeraude is nearly as dark, and must be treated in nearly the same way. The green at G can be developed down a few steps by reduction of the light cadmium; then this must be replaced by aureolin *plus* cadmium orange; and then the black increment must begin. The series downward from G-Y is the most difficult and complicated of all, and must in large measure be left to the skill and eye of the painter; the changes, as the different colors are employed, are very instable, and the proportions must be sought empirically. Especial care must be taken with the colors between green-yellow and Y, if they are to be kept clear, and not to lean over towards red or green. A little burnt sienna will tend, as was said above, to warm and clear the difficult series of ambers and browns between Y and Bk; it also works well into the dark oranges between YBk and RBk.

The painting of the sections (both surfaces of which were covered) was comparatively easy. On the vertical section, the chroma of the peripheral blues and yellows was decreased by adding the serial increment of the neutral found at the middle line in the same row. On the peripheral section, very little besides middle grey was required to neutralize all the hues; when the grey component becomes predominant, the color lies so near the horizontal plane that middle grey may be used without appreciable error. To have laid other sections through the model would have impaired its stability, and would have added considerably to its cost. On the other hand, there is no difficulty, when once the faces described have been painted, in coloring

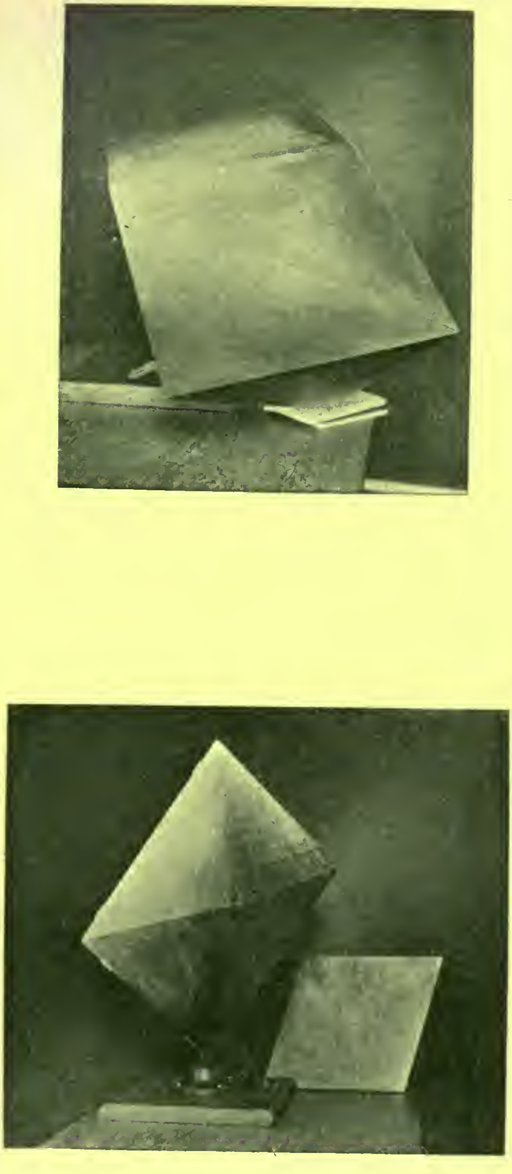


PLATE II

other surfaces that represent planes of section in any direction. We have worked out only one of these: the horizontal plane that passes through R, G, and middle grey, and therefore also through a high blue and a low yellow. A separate rhombus of wood was made to the required dimensions; the hues were laid off along the periphery by reference to the painted pyramid; and the surface, showing all hues in all chromas at the tint of middle grey, was treated in the same way as those of the peripheral section. It is probably unnecessary to prepare more of these sectional surfaces; students who have followed the construction of the pyramid on the blackboard, and have been led to the complete model by way of the Hegg plate and the separate rhombus, find no special difficulty in following imaginary lines and covering imaginary planes within the color system.

The price of the wooden model was $26, and of the iron base, $3; the cost of the pigments, brushes, etc., a little over $7. Given the necessary skill in painting, the model can therefore be reproduced for $37.

Explanation of Plates

Plate I. Color-pyramid, closed, seen from back. Shows pin-hasp at G, and forked support.

Color pyramid, open. Shows vertical section.

Plate II. Color pyramid, open. Shows peripheral section.

Color pyramid, closed, seen from front, the R-angle facing observer. Separate rhombus (horizontal section) at side.

THE ASSOCIATION REACTION METHOD OF MENTAL DIAGNOSIS (*Tatbestandsdiagnostik*)

By ROBERT M. YERKES and CHARLES S. BERRY

In connection with a course in Applied Psychology, given in the Harvard Summer School during the session of 1908, the association reaction method of mental diagnosis was employed for the purpose of class demonstration. The discovery of this method of studying the content of consciousness is claimed by both Wertheimer and Jung,[1] who, with other psychiatrists and criminologists, have discussed its applications and values from several points of view. Without previous experience in the use of the method and with little hope of success in our efforts to adapt it to the demands made upon a class demonstration experiment, we planned and executed the experiments whose results constitute the material of this report. The success which rewarded our efforts to demonstrate the value of the method to a class of elementary students of Psychology would seem to justify a brief account of our work. We wish to have it clearly understood, however, that we are not attempting a critique of the method of Jung and Wertheimer, and that we do not consider our results in themselves as of special importance. It is our aim merely to call attention to a useful adaptation of the method in the teaching of Psychology.

EXPERIMENT I

We attempted, in our first experiment, to discover which of two possible acts a person had performed. In a room of the laboratory we placed two small boxes, one of which contained a dancing mouse, and the other a pack of playing cards. The boxes were covered, so that their contents could be discovered only by raising the lids. In another room of the laboratory, the classroom, we had arranged a Hipp chronoscope, electrically connected, with a single hand key in the circuit. After the class had assembled in this room we explained briefly what we proposed to do, selected one member of the class to serve as subject for the experiment, and then directed that individual, in the company of the remainder of the class, to go to the room which contained the boxes, open one of them, examine its contents, and, without disturbing the other box, return to

[1] For the literature on "Tatbestandsdiagnostik" see bibliography at the end of this article.

the classroom to be tested by the association reaction-time method.

We had previously prepared two lists of thirty words each. In one of them occurred several words which referred directly to the contents of the mouse-box. This we may hereafter call the mouse-box list. In the other occurred certain words which similarly referred to the contents of the card-box. This we may therefore designate as the card-box list. These lists, of experiment 1, appear in tables 1 and 2.

The subject, upon returning to the classroom, was given a seat at one side of the table which carried the chronoscope. On the opposite side sat the experimenter, whose task it was to call the words of the list and manipulate the reaction key. At the end of the table, so seated that he could readily read the chronoscope, was an assistant experimenter. The subject having been directed to conceal, if possible, knowledge of the contents of the box which had been opened, was told to respond to each word of the list, as it was pronounced by the experimenter, by speaking as quickly as posssble the idea which came into consciousness.

The experiment was then conducted as follows. The chronoscope having been set in motion by the assistant, the experimenter gave a ready signal and then pronounced distinctly the first word of the mouse-box list, at the same instant closing the key of the chronoscope circuit and thus starting a reaction-time record. The instant the subject pronounced an associated word (reaction word) the experimenter released the key and thus stopped the recording hands of the chronoscope.[1] Immediately the assistant read and recorded the position of the hands on the dial of the chronoscope. As soon as this had been accomplished, the experimenter pronounced the second word of the mouse-box list, and so on until the chronoscope had run down. Usually five reaction times could be measured without

[1] The experimenter was an experienced reactor and the uniformity of his reactions made it seem desirable that he react to both the stimulus word and the reaction word. Had the subject instead of the experimenter reacted to the reaction word the reaction times might be somewhat shorter, but they would also be less reliable because of lack of previous training.

It may be objected that the experimenter's knowledge of the contents of the boxes influenced his reaction times, and thus determined the results of the experiment. We appreciated and met this danger in our second experiment, for in it the experimenter had no knowledge of what the subject had done and was unable to distinguish the significant from the irrelevant words of the list. As our results were positive in both experiment 1 and experiment 2, we feel confident that the experimenter's knowledge of the conditions of the first experiment did not render the results unreliable.

interruption for winding the instrument.[1] As soon as the mouse-box list had been completed, the card-box list was given in the same way.

Upon the completion of this initial experiment we tabulated and examined our results in the presence of the class, and, in the light of them, we were able to state with considerable confidence that the subject had opened the mouse-box instead of the card-box. As this conclusion proved to be correct it will be worth while for us now to examine the facts upon which our judgment was based.

In the accompanying lists of words some, it will be noticed, are marked with an asterisk. They refer to the contents of the box. To distinguish them from the other words of the series, which are properly described by the term *irrelevant*, we may speak of them as *significant*. The mouse-box series contained eight significant and twenty-two irrelevant words, but as the word squeak was misunderstood by the subject there were in effect only seven words which referred to the contents of this box. We planned to have one significant word to two irrelevant words in each series.

Beside the number of each word given (stimulus word) and the word itself the tables present the associated words (reaction words) given by the subject, and the reaction time. The latter we may, in order to be on safe ground, consider as reliable only to the second decimal place. Careful study of the associations which the two lists of words called out gives us little if any suggestion as to which box had been examined. But when we turn to the reaction times we meet with a strikingly different condition. As is shown in the general results for experiment 1, the average or mean for the irrelevant words of the mouse-box list was 1.775″ while that for the significant words was 2.487″. Apparently the significance of the words rendered their reaction times considerably longer than they would ordinarily have been. For the card-box list the difference between the reaction time for the two groups of words is only 0.125″. Furthermore it is important to note that the variability, as well as the range, of reaction time is greater for the mouse-box list than for the card-box list.

Turning to individual words and their reaction times, we

---

[1] It has occurred to us since these experiments were performed that the Münsterberg chronoscope (readable to hundredths of a second) would be more satisfactory than the Hipp, because it can be used for ten minutes or more without interruption for winding. Professor Münsterberg, who, in his employment of the association reaction method for diagnostic purposes has made use of a stop-watch which could be read to fifths of a second, states that his readings are sufficiently accurate for practical purposes.

note that the irrelevant word rain, number 19 of the mouse-box list, was reacted to with the word clouds in 2.419″, that the five consecutive significant words, small, white, dance, tail and rat, were reacted to with the words faucet, sink, book, horse and cat in 2.981″, 2.830″, 2.233″, 1.948″, and 3.208″ respectively, and finally, that the immediately succeeding irrelevant word book was reacted to with the word paper in only 2.205″. The word rat, following as it did a series of significant words, caused an inhibition or delay of reaction which was very noticeable indeed.

The attitude of the subject toward the experiment is indicated by the following brief introspective report. "I tried to give, as I understood I was expected to, the first association that came to mind. I endeavored, however, at the same time,

TABLE I

Mouse-Box List of Experiment 1

No. of Word	Stimulus Word	Reaction Word	Reaction Time
1	House	Dog	1.670 sec.
2	Sky	Blue	1.067
3	Tree	Green	1.340
4	Sun	Yellow	1.740
5	Flower	Blue	1.293
6	Boston	City	1.637
7	Man	Tall	.955
8	Harvard	Yard	1.758
9	Knife	Cut	1.600
10	Psychology	Study	1.675
11	*Box	Wood	2.117
12	*Movement	Machine	2.090
13	Squeak[1]	Sugar	3.146
14	Woman	White	1.906
15	Drink	Water	1.733
16	Room	Wood	2.098
17	Study	Book	1.381
18	Consciousness	Brain	2.234
19	Rain	Clouds	2.419
20	*Small	Faucet	2.981
21	*White	Sink	2.830
22	*Dance	Book	2.233
23	*Tail	Horse	1.948
24	*Rat	Cat	3.208
25	Book	Paper	2.205
26	Money	Color	1.206
27	Crime	Murder	2.205
28	Ground	Yellow	2.206
29	Companion	Woman	1.667
30	Home	House	1.697

[1] This word was understood to be sweet; hence the associated word sugar.

TABLE 2

Card-Box List of Experiment 1

No. of Word	Stimulus Word	Reaction Word	Reaction Time
1	Iron	Metal	1.731 sec.
2	Wolf	Animal	1.666
3	Wine	Red	1.296
4	Salad	Lettuce	1.845
5	Railroad	Rails	2.379
6	Orange	Yellow	1.214
7	Ticket	Railroad-train	2.612
8	Journey	Steamer	1.499
9	Lonesome	Child	3.109
10	*Play	Earth	2.344
11	*Ace	Cards	1.480
12	*Diamonds	Brilliant	1.894
13	*Clubs	Ace of Clubs	2.690
14	Trump[1]	Box	3.116
15	Wind	Blow	2.057
16	Window	Light	1.416
17	Dog	Yellow	2.100
18	Apple	Red	1.475
19	Fish	Swim	1.706
20	*Box	Trunk	2.146
21	*Lid	Trunk	2.271
22	*Open	Book	1.384
23	Black	Coat	1.072
24	*King	Queen	2.025
25	*Jack	Cards	1.398
26	*Hearts	Cards	1.798
27	Sleep	Tired	1.664
28	Morning	Light	1.387
29	Home	House	1.365
30	Friends	Many	1.655

to put out of mind all thought of my experiences with the boxes."

In the light of the reaction times of this experiment there seemed to be good reason for concluding that the subject had opened the mouse box: hence our statement to that effect.

The unexpectedly definite and convincing results of this preliminary experiment led us further to test the value of the method in a slightly different way three days later, with the same class. In the meantime the members of the class, being deeply interested in the experiments, had discussed with the assistant experimenter and among themselves the method, our results, and various ways in which a subject might render the method useless. It is significant of the importance of the

[1] This word was understood to be trunk; hence the associated word box.

TABLE 3

Quantitative Results of Experiment 1

	Reaction-Time Values for Mouse-Box List		Reaction-Time Values for Card-Box List	
	Irrelevant Words	Significant Words	Irrelevant Words	Significant Words
Mean	1.775″	2.487″	1.818″	1.943″
Mean Var.	.366	.445	.449	.352
Difference in Means	+0.712		+0.125	
Extremes Short	.955	1.948	1.072	1.384
Extremes Long	2.914[1]	3.208	3.109[2]	2.690
Max. Range	2.253″		1.618″	

method, that, despite this discussion, it continued to yield positive results.

EXPERIMENT 2

Our task in experiment 2 was to discover which of two subjects had performed a certain series of acts. With the apparatus arranged as previously, and with the list of one hundred words which appears in table 4 prepared, we chose two members of the class, Mr. L. and Mr. W., as subjects. These individuals were then directed to take a folded note and leave the classroom. When out of sight and hearing of those of us who remained in the classroom they were to decide which one of them should open the note and carry out certain written directions which it contained. They were clearly told that only one of them should know anything about the contents of the note, and that they should make it their business to prevent the experimenters from discovering which of them had the information that would be gained by following the written directions which are appended.

"Go to the shop, place a hammer in a drawer which is indicated by the mechanic, and carefully examine the articles in the drawer so that you can answer correctly the following

[1]Omitting the reaction time for word number 13 (squeak) because it was misunderstood.

[2]Omitting the reaction time for word number 14 (trump) because it was misunderstood.

questions: How many keys are on the key ring? What is the label on the ink bottle? Is the ink bottle empty? Is the stop-watch running? What is the number on the back of it? How many blades has the knife? Are they sharp? What is the color of the handle? Can you pull small nails with the hammer?"

One at a time, the two subjects returned to the classroom and were tested for their knowledge of this series of questions.

The results which appear in table 4 demand little comment, but they deserve careful study. We may briefly call attention to the chief grounds of our conclusion that Mr. L. instead of Mr. W. carried out the directions.

First, the significant words caused considerable delay of reaction in Mr. L.'s case and none whatever in Mr. W.'s. Second, the variability of the reaction time to the significant words was greater than that for the irrelevant words in Mr. L.'s case, whereas it was less in Mr. W.'s case. Third, the range of reaction times was considerably greater for Mr. L. than for Mr. W. And, fourth, certain significant words caused very obvious inhibition in the reactions of Mr. L.

Especially important in this experiment are the reactions of Mr. L. to the three significant words Carter's, stopper, blotter which immediately followed the irrelevant word hawk (No. 21). For the latter the reaction time was only 1.873″, for Carter's it increased to 2.279″, for stopper it was 2.059″, for blotter 3.386″, and for the irrelevant word meat, which followed blotter, it suddenly dropped to 2.129″. The inhibition of reaction caused by the word blotter is explained by the introspective report of Mr. L. He had prepared himself for the experiment by thinking over words which might appropriately be associated with the objects which he had seen. For the word blotter he had, as it chanced, no word ready. It took him completely by surprise, and his reaction time was so long that he at once decided that his case was lost. This change in Mr. L.'s attitude toward the experiment manifested itself in the reaction times to the remaining words of the list. The results of the second part of table 5 show that after the first twenty-five words, among which occurred the words stopper, Carter's, and blotter, the significant words of the list caused little delay in reaction. These averages for the reaction times of the series by twenty-fives are worthy of careful examination. Among the first twenty-five words of the list there were twelve significant and thirteen irrelevant words. For the former the reaction time was more than half a second longer than for the latter in the case of Mr. L., and a twentieth of a second shorter in the case of Mr. W. The second group of twenty-five words contained eleven significant and fourteen irrelevant words. Mr.

TABLE 4

Results of Experiment 2

	RESULTS FOR MR. W.			RESULTS FOR MR. L.	
No. of Word	Stimulus Word	Reaction Word	Reaction Time	Reaction Word	Reaction Time
1	Canary	Bird	1.021	Dog	1.289
2	Tea	Yellow	.979	Coffee	1.287
3	Bread	White	1.886	Butter	1.528
4	Dog	Animal	1.022	Cat	.876
5	Lily	Green	1.143	Pond	1.353
6	*Bottle	Green	1.227	Water	1.637
7	*Empty	Crack	1.412	Full	1.448
8	*Black	White	1.054	White	2.057
9	*Write	Figure	1.286	Dog	2.024
10	*Fluid	Paper	1.334	Water	1.580
11	Sparrow	Bird	1.229	Bird	1.170
12	Coffee	Black	1.506	Tea	1.655
13	Cow	Milk	1.274	Milk	1.169
14	Plum	Fruit	1.820	Purple	1.733
15	Door	Open	1.243	Closed	1.812
16	*Lock	Open	1.355	Key	2.017
17	*Rusty	Green	1.233	Nails	1.922
18	*Ring	Key	1.439	Key	.824
19	*Number	White	1.105	Two	2.771
20	Window	Open	1.506	Glass	1.543
21	Hawk	Black	1.187	Bird	1.873
22	*Carter's	Ground	1.375	White	2.279
23	*Stopper	Chair	.963	Bottle	2.059
24	*Blotter	Red	1.269	Paper	3.386
25	Meat	Cotton	1.136	News	2.129
26	Horse	Brown	.999	White	1.806
27	*Ticks	Clock	1.203	Watch	1.595
28	Floor	Open	1.562	Wax	1.893
29	Peach	Stone	1.196	Stones	1.795
30	*Keyhole	Open	1.309	Door	1.733
31	Apple	Seeds	1.573	Seeds	1.439
32	Violet	Violet	1.621	Purple	1.649
33	*Face	Brown	1.007	White	1.654
34	*Hands	Door	1.196	Clean	2.225
35	*Stop	Watch	1.120	Clock	2.165
36	*Set	Open	1.213	Gone	1.798
37	Chocolate	Brown	.971	Cream	1.719
38	Potatoes	White	1.496	White	1.463
39	*Blades	Green	1.696	Sharp	1.643
40	Cat	Grass	1.102	Dogs	1.559
41	Hen	Knife	1.589	Scratch	1.589
42	Soda	White	1.194	Water	1.279
43	Sugar	Glass	1.135	Sweet	1.120
44	*Second	Sugar	1.341	Hand	1.811
45	*Hour	Minute	1.331	Minute	1.637
46	Mouse	Cat	1.058	Trap	1.239
47	Orange	Tooth	1.245	Yellow	1.459
48	Ceiling	White	1.180	White	1.509
49	*Sharp	Dull	1.502	Corn	1.592
50	*Color	Color	1.513	Green	1.810

TABLE 4.—(Continued)

Results of Experiment 2

RESULTS FOR MR. W.				RESULTS FOR MR. L.	
No. of Word	Stimulus Word	Reaction Word	Reaction Time	Reaction Word	Reaction Time
51	Ostrich	Book	1.030	Plumes	1.464
52	Lemonade	Lemonade	1.648	Sweet	1.834
53	Pickles	Ball Game	1.561	Sharp	1.417
54	Elephant	Animal	1.278	Trunk	1.285
55	Daisy	Flower	1.174	Ox-eye	2.168
56	*Open	Closed	1.118	Shut	1.260
57	*Handle	Door	1.359	Blades	2.155
58	*Cut	Rat	1.102	Dog	1.340
59	*Three	Green	1.587	Six	1.900
60	Roof	Wall	2.613	Green	1.828
61	Pike	Toll	1.749	Road	1.595
62	Dove	Round	1.064	Fly	1.619
63	Beer	Floor	1.128	Foam	1.615
64	Soup	Tomato	1.761	Turtle	1.976
65	Wolf	Wall	1.063	Snarl	1.817
66	Grapes	Glass	1.313	Purple	1.705
67	Wind	Hard	1.562	Blow	1.624
68	*Time	Short	1.404	Minute	1.731
69	*Clean	Board	1.355	White	1.420
70	Chimney	Short	1.530	Smoke	1.302
71	Shark	Smoke	1.306	Bite	1.568
72	Wine	Green	1.426	Milk	2.026
73	Chicken	Hen	1.309	Hen	1.463
74	Rat	Cat	1.081	Trap	1.219
75	Cherries	Stones	1.665	Red	1.708
76	*Hatchet	Axe	1.514	Cut	1.368
77	*Drawer	Red	1.270	Open	1.239
78	Whale	Yellow	1.199	Bone	1.638
79	Ale	Wine	1.103	Beer	1.705
80	Pepper	Potatoes	1.314	Sharp	1.817
81	*Pickup	Key	1.160	Drop	1.809
82	*Open	Paper	1.098	Close	1.237
83	Book	Door	1.053	Shut	1.734
84	Place	Table	1.004	Room	1.155
85	Close	Paper	1.056	White	1.260
86	Steps	Pen	1.192	Down	1.346
87	Swan	Pencil	1.236	White	1.279
88	Goat	Table	.929	Butt	1.751
89	Salad	Bird	1.100	Dressing	1.128
90	Water	Tool	1.150	Drink	1.417
91	Figs	Sharp	1.150	Sweet	1.729
92	*Nail	Box	1.185	Door	1.278
93	*Drive	Hatchet	.955	In	1.283
94	*Pull	Crayon	1.013	Out	1.106
95	Ocean	Hinges	1.246	Green	1.386
96	*One	Pencil	1.046	Six	1.585
97	Ship	Pencil	1.004	Sail	1.464
98	*Ten	Hammer	1.144	Dozen	1.336
99	Boy	Door	.940	Girl	1.214
100	Home	Door	1.359	Down	1.765

TABLE 5

Quantitative Results of Experiment 2

	Results for Mr. W.		Results for Mr. L.	
	Irrelevant Words	Significant Words	Irrelevant Words	Significant Words
Mean	1.293″	1.258″	1.551″	1.729″
Mean Var.	.224	.145	.221	.343
Difference in Means,	−0.035″		+0.178″	
Extremes: Short	.929	.955	.876	.824
Extremes: Long	2.613	1.696	2.168	3.386
Max. Range	1.684″		2.562″	

Means, and Differences in Means, for the series of Reaction Times by quarters.

	Mr. W. Irrelevant	Mr. W. Significant	Mr. L. Irrelevant	Mr. L. Significant
1st Quarter (25 words)	1.304″	1.254	1.494″	2.000″
Difference	−0.050		+0.506	
2nd Quarter	1.280	1.312	1.537	1.788
Difference	+0.032		+0.251	
3rd Quarter	1.435	1.321	1.644	1.634
Difference	−0.114		−0.010	
4th Quarter	1.127	1.154	1.493	1.360
Difference	+0.027		−0.133	

L.'s reaction time to the former was about a quarter of a second longer than his reaction time to the latter; Mr. W.'s times differed by only thirty-two thousandths of a second. In the third and the fourth groups of twenty-five, the significant words were reacted to by Mr. L. even more quickly than the irrelevant words. This marked difference in the results for the first and the second halves of the list of stimulus words is intelligible in the light of Mr. L.'s introspective statement. "I tried," he writes, "to put myself into another state of mind in order not to reveal my knowledge of the facts. I thought over words that might be associated with the objects I had seen, and tried to make my association reaction time slow and uniform. I had not thought of the blotter and was unprepared for it. The delay which it caused made me feel that there was no use in trying to conceal my knowledge."

Mr. W. gives the following account of his attitude toward the experiment: "When I entered the classroom I was not guilty of carrying out the directions. I tried to make my associations quickly and uniformly. Several times a word, generally the name of some object before me, came to my mind before the stimulus word was given by the experimenter, and I pronounced this word, although possibly it had no connection whatever with the word given to me. I did not try to trick the experimenters except when I tried to think of some objects which Mr. L. would be likely to see in the shop. I mentioned some of these objects with the idea that possibly the experimenters would be led to think I knew a little about the series of acts. I had no reason for acting in any other than my ordinary manner."

The basis for our judgment concerning the facts revealed by this experiment is even more satisfactory than that furnished by experiment 1. Indeed the case against Mr. L. was clear before the experiment had been completed.

Subsequently, still further to test the reliability of our results, we repeated these two experiments with subjects who had considerable knowledge of psychology.

Experiment 3.

Experiment 3 is essentially a repetition of experiment 1, but with the substitution of certain words which promised to be more satisfactory than some that occurred in the lists of tables 1 and 2. In each list of thirty words ten significant words were given. The subject, a graduate student in Harvard University, was told to give the associated idea quickly, but to do his best to prevent the experimenters from discovering which of the two boxes he had opened. The test was conducted as the others had been, except that there was no class present in the experiment room.

An examination of the averages, variabilities, and ranges for this experiment as they appear in table 6 indicates that we have a less satisfactory basis for our conclusion than in either of the previous experiments.[1] There are no indications of the inhibitory influence of the significant words of either list. The greater variability and range of the reaction-times to the mouse-box series might indicate that it had been opened; but, on the other hand, since this series was the first to be given it might reasonably be urged that lack of practice on the part of the subject accounts for the greater variability and range. And so we are left uncertain. As a matter of fact both experimenters finally decided, incorrectly, that the card-box had been opened. But they also decided (and this is of prime importance) that the subject had not given associations that were suggested by the stimulus words. Examination of the reaction words indicated that most of them were suggested either by objects in the room or by earlier words of the series. In the light of this fact the only possible conclusion would be that fear of detection of the truth had caused the subject to refuse to follow the directions which were given. The experiment is inconclusive so far as knowledge of which box had been opened is in question, but it furnishes very interesting and important information concerning the subject's mental content.

TABLE 6

Quantitative Results of Experiment 3

	Results for Mouse-Box List		Results for Card-Box List	
	Irrelevant Words	Significant Words	Irrelevant Words	Significant Words
Mean	1.542″	1.471″	1.324″	1.389″
Mean Var.	.330	.223	.174	.112
Difference in Means,	−0.071″		+0.068″	
Extremes Short	.805	.768	1.006	1.184
Extremes Long	3.037	1.994	2.340	1.616
Max. Range	2.232		1.334	

Had this subject given us true associations instead of ideas which had come to him before the stimulus word was given, we should almost undoubtedly have obtained results similar to those of experiments 1 and 2.

[1] The detailed results of experiments 3 and 4 are not presented, since they offer little in addition to what appears in the other tables.

Perhaps the most interesting feature of the general results of experiment 3 is the high variability of the mouse-box reaction times. If practice had been excluded by previous training, this would have suggested knowledge of the contents of that box. Although we did not get what we set out to get in this experiment, the results would seem to indicate that no matter what method a subject chooses to conceal his knowledge, he is sure to give the experimenters some clue which if followed up skillfully will reveal the truth.

Experiment 4

Experiment 2 was repeated, as experiment 4, with the list of words reduced to fifty, of which twenty were significant. The list was otherwise changed slightly from the original, by the substitution of certain words. The subjects for experiment 4 were Mr. G., the subject in experiment 3, and another graduate student in Harvard whom we may refer to as Mr. M.

The results of this experiment were definitely positive, for it was clear that Mr. M. instead of Mr. G. had knowledge of the series of acts. This was indicated as strongly by the fact that many of his associations referred to objects in the drawer, as by the inhibitory influence of the significant words and the great variability of their reaction times.

Table 7

Quantitative Results of Experiment 4

	Results for Mr. G.		Results for Mr. M.	
	Irrevelant Words	Significant Words	Irrevelant Words	Significant Words
Mean	1.340″	1.369″	1.853″	2.269″
Mean Var.	.251	.159	.181	.307
Difference in Means	+0.029		+0.416	
Extremes Short	.860	1.063	1.479	1.773
Extremes Long	2.585	1.764	2.278	3.106
Max. Range	1.725		1.627	

Finally, in table 8, we have brought together the mean variabilities and maximum ranges[1] for the reaction times of

[1] It is to be noted that as the maximum range we give the difference between the shortest and the longest reaction-time of the series.

the several experiments. As experiment 3 did not conform to the conditions prescribed for the tests its results have not been included in the general averages of this table.

TABLE 8

General Quantitative Results of Experiments

	Objects or Acts Known to Subject		Objects or Acts Unknown to Subject	
	Mean Variabilities		Mean Variabilities	
	Irrelevant Words	Significant Words	Irrelevant Words	Significant Words
Experiment				
1	.366″	.445″	.449″	.352″
2	.221	.343	.224	.145
3	.330	.223	.174	.112*
4	.181	.307	.251	.159
Average	.256	.365	.308	.219
	Maximum Range		Maximum Range	
Experiment				
1	2.253		1.518	
2	2.562		1.684	
3	2.232		1.334*	
4	1.627		1.725	
Average	2.147″		1.642″	

*Results for Experiment 3 are not included in the averages.

In conclusion we would say that although there are obviously many environmental and subjective factors whose relation to the results of an association reaction-time experiment must be known before the method can be considered reliable, this does not seem to us a fit place to enter upon a discussion of them. It has been our sole purpose in this paper to show that under ordinary conditions and with ordinary skill in experimentation we have succeeded in demonstrating to a class, much to the surprise and satisfaction of its members, that a simple list of associations and their reaction times may reveal intensely interesting facts concerning the content of consciousness.

LITERATURE ON TATBESTANDSDIAGNOSTIK

BINSWANGER, L. See Jung, C. G. Diagnostische Assoziationsstudien. XI. Beitrag.

BLEULER, E. Diagnostische Assoziationsversuchen. Vorwort. Ueber die Bedeutung von Assoziationsversuchen. Jour. f. Psy. u. Neurol., 1904, Bd. 3, S. 49-54.

——. See Jung, C. G. Diagnostische Assoziationsstudien. V. Beitrag.

BOLTE, —. Assoziationsversuche als diagnostisches Hilfsmittel. Allg. Zeit. f. Psychiat., 1907, Bd. 64, S. 619-630.

FÜRST, E. See Jung, C. G. Diagnostische Assoziationsstudien. X. Beitrag.

GRABOWSKY, A. Psychologische Tatbestandsdiagnostik. Beilage z. allg. Zeitung (München), Jahrg. 1905, Nr. 289, S. 497-499.

GROSS, A. Zur psychologische Tatbestandsdiagnostik. Monatsschr. f. krim. Psy. u. Strafrechtsreform, 1904.

——. Die Assoziationsmethode im Strafprozess. Zeit. f. d. ges. Strafrechtswiss., 1905, Bd. 26, S. 19-40.

——. Zur psychologischen Tatbestandsdiagnostik als kriminalistisches Hilfsmittel. Beiträge z. Psy. d. Aussage, 1905, Bd. 2, S. 150-153.

GROSS, H. Zur Frage des Wahrnehmungsproblems. Beiträge z. Psy. d. Aussage, 1905, Bd. 2, S. 128-134.

——. Zur psychologischen Tatbestandsdiagnostik. Arch. f. Krim.-Anth. u. Kriminalistik, 1905, Bd. 19, S. 49-59.

ISSERLIN, M. Die diagnostische Bedeutung der Assoziationsversuche. Münch. med. Woch., 1907, Bd. 54, S. 1322-1326.

JUNG, C. G. Die psychologische Diagnose des Tatbestands. Zent. f. Nervenheil. u. Psychiat., 1905, Bd. 28, S. 813-815.

——. Experimentelle Beobachtungen über das Erinnerungsvermögen. Zent. f. Nervenheil. u. Psychiat. 1906, Bd. 17, S. 633-666.

——. Diagnostische Assoziationsstudien. Beiträge zur experimentellen Psychopathologie. Bd I. Leipzig, Barth, 1906.

——. On psychophysical relations of the associative experiment. Jour. of Abn. Psy,, 1907, Vol. 1, pp. 247-255.

——. Zur Tatbestandsdiagnostik. Zeit. f. angewandte Psy. u. psy. Sammel., 1908, Bd. 1, S. 163.

—— und RIKLIN, F. Diagnostische Assoziationsstudien. I. Beitrag. Experimentelle Untersuchungen über Assoziationen Gesunder. Jour. f. Psy. u. Neurol., 1904, Bd. 3, S. 55-83; 145-164; 193-215; Fortsetzung und Schluss, Bd. 4, S. 24-67.

II. Beitrag. (Wehrlin, K.) Ueber die Assoziationen von Imbezillen und Idioten. Jour. f. Psy. u. Neurol., 1904, Bd. 4, S. 109-123; 129-143.

III. Beitrag. (Jung, C. G.) Analyse der Assoziationen eines Epiletikers. Jour. f. Psy. u. Neurol., 1905, Bd. 5, S. 73-90.

IV. Beitrag. (Jung, C. G.) Ueber das Verhalten der Reaktionszeit beim Assoziationsexperimente. Jour. f. Psy. u. Neurol., 1095, Bd. 6, S. 1-36.

V. Beitrag. (Bleuler, E.) Bewusstsein und Assoziation. Jour. f. Psy. u. Neurol., 1905, Bd. 6, S. 126-154.

VI. Beitrag. (Jung, C. G.) Psychoanalyse und Assoziationsexperiment. Jour. f. Psy. u. Neurol., 1906, Bd. 7, 1-24.

VII. Beitrag. (Riklin, F.) Kasuistische Beiträge zur Kenntnis hysterische Assoziationsphänomene. Jour. f. Psy. u. Neurol., 1906, Bd. 7, S. 223-252.

VIII. Beitrag. (Jung, C. G.) Assoziation, Traum und hysterische Symptom. Jour. f. Psy. u. Neurol., 1906, Bd. 8, S. 25-60.

IX. Beitrag. (Jung, C. G.) Ueber die Reproduktionsstörungen beim Assoziationsexperiment. Jour. f. Psy. u. Neurol., 1907, Bd. 9, S. 188-197.

X. Beitrag. (Fürst, E.) Statische Untersuchungen über Wortassoziationen und über familiäre Uebereinstimmung im Reaktionstypus bei Ungebildeten. Jour. f. Psy. u. Neurol., 1907, Bd. 9, S. 243-278.

XI. Beitrag. (Binswanger, L.) Ueber das Verhalten des psychogalvanischen Phänomens beim Assoziationsexperiment. Jour. f. Psy. u. Neurol., 1907, Bd. 10, S. 149-181; 1908, Bd. 11, S. 65-95; 133-153.

KRAMER, F. und STERN, W. Selbstverrat durch Assoziation. Beiträge zur Psy. d. Aussage. 1906, Bd. 4 S. 1-32.

LEDERER, M. Die Verwendung der psychologischen Tatbestandsdiagnostik in der Strafrechtspraxis. Monatsschr. f. Krim.-Psy. u. Strafrechtsreform, 1906.

——. Zur Frage der psychologischen Tatbestandsdiagnostik. Zeit. f. d. ges. Strafrechtswiss. 1906, Bd. 26, S. 488-506.

LIPMANN, O. und WERTHEIMER, M. Tatbestandsdiagnostische Kombinatonsversuche. Zeit. f. angewandte Psy. u. psy. Sammel., 1908, Bd. 1, S. 119-128.

MEUMANN, E. Ueber Assoziationsexperimente mit Beeinflussung der Reproduktionzeit. Arch. f. d. ges. Psy., 1907, Bd. 9, S. 117-150.

MÜNSTERBERG, H. On the witness stand. Essays on psychology and crime. See especially "The detection of crime," pp. 73-110. New York, The McClure Company, 1908, pp. 269.

RIKLIN, F. See Jung, C. G. Diagnostische Assoziationsstudien, I und II Beiträge.

STERN, W. Psychologische Tatbestandsdiagnostik. Beiträge z. Psy. d. Aussage, 1905, Bd. 2, S. 145-147.

——. Selbstverrat durch Assoziation. Beiträge z. Psy. d. Aussage, 1905, Bd. 3, S. 150-155.

WEHRLIN, K. See Jung, C. G. Diagnostische Assoziationsstudien. II. Beitrag.

WERTHEIMER, M. Experimentelle Untersuchungen zur Tatbestandsdiagnostik. Diss. Würzburg, 1905. Arch. f. d. ges. Psy., 1905, Bd. 6, S. 59-131.

——. Tatbestandsdiagnostische Reproduktionsversuche. Arch. f. Krim.-Anth. u. Kriminalistik, 1906, Bd. 22, S. 293-317.

—— und KLEIN, J. Psychologische Tatbestandsdiagnostik. Arch. f. Krim.-Anth. u. Kriminalistik, 1904, Bd. 15, S. 72-113.

WRESCHNER, A. Die Reproduktion und Assoziation von Vorstellungen. I. Teil. Zeit. f. Psy. u. Physiol. d. Sinnesorg., 1907, Erg.-Bd. 3, S. VI+328.

STUDIES IN RETARDATION

As Given in the Fatigue Phenomena of the Tapping Test.

By Frederic Lyman Wells, Ph. D.
Assistant in Pathological Psychology in the McLean Hospital, Waverley, Mass.

CONTENTS

1. Retardation and the Curve of Work

The experimental problems presented by this striking mental symptom are important and numerous. Definition adds little to what is implied in the term itself; in its pure state it is simply *a loss of the promptness of response to the given situation.* It does not necessarily carry with it any mental confusion; thus a long interval may elapse between a question and its answer, but when the answer does occur it may imply as perfect comprehension and insight as would be expected in the normal state. Motor reactions are delayed, ineffective, and give a superficial appearance of extreme difficulty and (usually) hesitation.

While extremely similar conditions occasionally appear in other psychoses, retardation is *par excellence* a symptom of manic-depressive depression, the depressed phase of the Kraepelinian manic-depressive insanity. In this disorder it is perhaps the most familiar objective symptom, and it is only in its relation to this disorder that it need be considered here. Its physiological correlate is unknown, and of its intimate psychological nature much still remains to be cleared up, especially in the analysis of its factors, and the relative part which sensory, associative and motor elements may play in the ultimate product.

The present study proceeds mainly along the lines first suggested by Hoch. Clinical observation indicates that under the continued activities of the waking day the characteristics of the retarded state in circular depressions tend to become less profound, and that these activities effect a change in such cases far more marked in character and amount than in the case with normal individuals. From this fact the inference is suggested, that if we could standardize the activities of such cases over a certain period, introducing controllable and measurable forms of activity, we should find in the measures thus obtained characteristic deviations from the normal corresponding to the clinically observable changes in the patient's condition. One would thus be afforded at once a quantitative measure for the extent of retardation, as well as a means of detecting its presence and changes in its amount so slight as to escape ordinary clinical observation.

The experimental problem then, is a study of the work curve in retarded individuals, but the various psychological tests that can be applied to the problem of the work-curve are not equally well suited to the purposes in hand. The measures most frequently employed in problems of this nature are those of the ergograph and the addition test. No one will dispute the value of the mass of data which Kraepelin's pupils have accumulated about these measures, which the present studies largely confirm at their points of coutact. Certain advantages may perhaps be urged for the present method. The ergograph is strong where the addition test is weak, namely in the objectivity of the results, simplicity of procedure, the control of co-operation, and in the amount of time consumed in the experiment. The addition test is strong where the ergograph is weak, namely in the simplicity of the apparatus, and the detail and minuteness with which it is possible to trace such factors as warming up, reflex inhibition and the *Antriebe.* The tapping test, however, while retaining the objectivity and precision of the ergograph, sacrifices none of the detail and minuteness possessed by the addition test under sufficiently refined conditions of measurement. In the matter of its demands upon co-operation, the tapping test is easily the superior of both. Further, there is probably no motor measure which is less dependent upon muscular factors.

The method and conduct of the experiments is identical with that described in a previous paper.[1] Each individual curve consists of 30″ of continued tapping, evaluated according to number of taps executed in each 5″ interval during the 30″ period. Between each of two 30″ periods of tapping there is a 2′ 30″ pause. Six successive 5″ *intervals* thus constitute a *series* (or individual curve), and five 30″ series with one hand form a *record.* The two records of right and left hand constitute the single *experiment.* There are given the results of two experi-

[1] Normal Performance in the Tapping Test, *American Journal of Psychology*, XIX, 1908, pp. 437-83.

TABLE I

Number of taps executed in 30″ periods. In each experiment the subject executes five 30″ series with one hand, then five series with the other hand. The second experiment follows one week upon the first.

First Experiment (Right Hand Preceding)
Rt. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st Series,	160	185	203	160	151	182	242	154	72	174	202	173	168.3	192
2nd "	164	183	200	165	139	171	225	160	73	164	209	168	164.4	190
3rd "	145	188	209	172	140	179	232	162	75	174	181	173	167.6	194
4th "	153	191	208	175	138	190	236	165	76	191	187	171	172.3	199
5th "	144	188	218	177	137	198	239	164	85	189	204	150	173.9	199
Av.	153.2	187.0	207.6	169.8	141.0	184.0	234.8	161.0	76.2	178.4	196.6	167.0	169.3	195

Lft. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st Series,	148	167	196	179	143	171	213	148	69	192	187	162	162.6	173
2nd "	136	166	196	170	140	156	210	154	80	181	190	149	158.9	172
3rd "	141	167	196	177	140	155	215	152	83	184	177	154	161.0	174
4th "	135	175	192	180	138	158	215	153	79	190	173	151	161.5	172
5th "	149	167	206	166	149	148	215	153	78	190	168	152	162.1	173
Av.	141.8	168.4	197.2	174.4	142.0	157.6	213.6	151.6	77.8	187.4	179.0	153.6	161.2	173.0

TABLE I (Cont.)

Second Experiment (Left Hand Preceding)

Rt. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st Series,	178	197	188	128	144	186	238	156	89	168	202	147	167.2	189
2nd "	188	188	194	131	134	191	234	155	89	179	203	158	168.3	191
3rd "	188	188	198	137	147	195	234	159	93	170	204	144	170.9	195
4th "	184	188	207	139	152	200	233	160	84	185	203	155	173.2	197
5th "	195	191	212	146	150	198	250	156	91	175	200	149	176.4	196
Av.	186.6	190.4	199.8	136.2	145.4	194.0	237.8	157.2	89.2	175.4	202.4	150.6	171.2	194

Lft. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st Series,	152	170	191	144	143	152	206	141	87	177	181	146	156.3	178
2nd "	137	169	189	135	143	151	193	135	92	164	185	137	151.0	175
3rd "	154	170	187	162	147	159	196	140	88	171	191	129	157.4	177
4th "	161	171	193	143	149	164	202	144	85	160	178	165	157.2	176
5th "	152	168	202	144	142	170	211	145	85	161	185	160	158.0	175
Av.	151.2	169.6	192.4	144.6	144.8	159.2	201.6	141.0	87.4	166.6	184.0	147.4	155.8	176

TABLE II

Average number of taps in each 5″ interval of the five thirty second series with either hand in each experiment. Fatigue is shown in the decrease in the number of taps as the later intervals are reached; "reversal" or interserial warming up in the failure of the successive intervals to regularly decrease.

First Experiment (Right Hand Preceding)

Rt. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st interval, 0″-5″	24.0	31.0	38.0	29.4	22.8	30.2	41.4	27.6	12.8	30.2	32.0	27.2	28.7	35.3
2nd " 5″-10″	24.8	32.8	36.0	29.4	23.8	31.6	41.2	27.0	12.8	31.0	34.0	28.2	29.0	33.5
3rd " 10″-15″	25.8	32.0	34.2	28.8	23.2	31.0	39.0	26.6	12.2	30.2	32.6	27.8	28.3	32.3
4th " 15″-20″	26.8	30.6	34.2	28.6	24.0	30.6	39.0	27.0	12.8	29.0	34.2	28.0	28.2	31.5
5th " 20″-25″	25.8	30.8	32.8	27.4	23.6	29.8	38.0	26.4	12.8	29.2	31.8	28.2	27.7	31.0
6th " 25″-30″	26.0	29.8	32.4	26.2	23.6	30.8	36.2	26.4	12.8	28.8	32.0	27.6	27.3	30.7
Index of fatigue *f*	1.08	1.01	.89	.96	1.04	1.02	.93	.97	.99	.99	1.03	1.03	.98	.90

(*f* is the average of the last five intervals divided by the first.) With reference to this measure, Cf. *American Journal of Psychology*, XIX, 1908, pp. 467 *et seq.*

Lft. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st interval, 0″-5″	23.8	29.6	36.8	32.0	23.4	28.2	38.6	26.6	12.8	33.8	31.2	24.6	28.6	32.0
2nd " 5″-10″	24.0	29.4	34.4	30.8	23.6	27.0	37.0	25.4	13.0	32.2	30.6	25.8	27.7	31.1
3rd " 10″-15″	23.8	28.8	32.8	29.8	24.0	26.0	36.0	25.2	12.8	31.4	29.0	26.0	27.1	28.7
4th " 15″-20″	23.4	27.0	30.8	28.6	24.0	26.0	35.0	25.0	13.2	30.6	29.8	25.8	26.4	28.0
5th " 20″-25″	23.8	26.6	31.2	26.8	23.2	25.6	33.4	25.2	13.0	29.8	29.6	25.2	26.9	27.3
6th " 25″-30″	23.0	27.0	31.2	26.4	23.8	24.8	33.6	24.6	13.0	29.6	28.8	26.2	25.7	26.9
f	.99	.94	.87	.89	1.01	.92	.91	.94	1.01	.91	.95	1.06	.93	.89

TABLE II (Cont.)

Second Experiment. (Left Hand Preceding)

Rt. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st interval, 0″-5″	31.2	32.6	37.0	22.4	24.2	33.0	44.0	26.4	14.6	30.0	32.8	24.0	29.5	35.0
2nd " 5″-10″	32.0	33.0	34.4	22.2	24.0	33.0	41.4	26.2	14.6	30.0	33.6	24.6	29.1	33.2
3rd " 10″-15″	31.4	31.8	33.4	23.4	24.2	32.6	39.6	26.4	15.0	29.8	33.4	25.2	28.8	32.2
4th " 15″-20″	31.4	31.8	32.2	22.2	24.4	32.4	38.8	26.4	15.0	29.8	34.2	25.6	28.3	31.6
5th " 20″-25″	30.2	31.0	31.8	23.4	23.8	31.4	37.0	25.8	15.0	28.8	33.6	25.4	27.8	31.4
6th " 25″-30″	30.4	30.2	31.0	22.6	24.0	31.6	37.0	26.0	15.0	28.0	34.8	26.0	27.6	30.7
f	.99	.97	.88	1.02	1.00	.98	.88	.99	1.02	.98	1.03	1.06	.96	.91

Lft Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st interval, 0″-5″	26.0	30.0	35.0	24.6	22.8	28.2	35.2	25.2	14.0	29.8	31.4	23.2	27.1	32.3
2nd " 5″-10″	26.0	29.6	34.4	24.2	25.0	27.2	34.4	24.2	14.6	28.6	32.4	24.4	26.8	30.8
3rd " 10″-15″	25.6	28.6	32.0	24.4	24.0	26.6	34.2	23.2	14.4	28.4	30.8	24.4	27.0	29.3
4th " 15″-20″	24.8	27.6	31.2	24.4	24.4	26.4	33.2	23.0	14.6	27.6	29.6	25.2	25.7	28.3
5th " 20″-25″	24.6	27.0	30.0	23.6	24.2	25.6	32.6	22.6	14.8	26.6	30.8	25.0	25.2	28.1
6th " 25″-30	24.2	26.8	29.8	24.4	24.4	25.2	32.0	22.8	15.0	25.6	29.0	25.2	25.0	27.5
f	.96	.93	.90	.98	1.07	.93	.94	.92	1.05	.92	.97	1.07	.96	.89

ments upon each of twelve subjects. *In the first experiment the right hand precedes, in the second the left hand precedes.*

The statistical aspect of the results is summed up in the accompanying tables. Table I gives the results in terms of the total number of taps in each of the five 30″ series in each experiment with each hand. In this table the decreased total efficiency of the retarded records as compared to the normal average is the principal thing to note. In Table II the figures are given according to the averages of all the series for each 5″ interval, in order to convey an idea of the fatigue phenomena which the series show. It will be noted that the series for the depressed subjects, while slower in rate, show very much less fatigue than does the normal average. In these two facts lie the principal deviations of the depressed subjects from the normal.

It will be observed that in the averages for the intervals (Table II) the first interval is often surpassed in rate by a later interval, which is not the case in normal subjects. To this characteristic phenomenon we shall apply the name of *reversal*. It is an in*tra*-serial warming up, warming up within the individual series, as distinguished from the in*ter*-serial warming up, warming up between the series, discussed at some length previously.[1] As a marked illustration of this *reversal* may be quoted the figures from Table II of Case I's right hand in the first experiment; in the first five seconds he averages 24.0 taps, in the second five 24.8 taps, in the third 25.8, in the fourth 26.8, in the fifth and sixth intervals 25.8 and 26.0. That is, there is regular "reversal" to the fourth interval, the opposite of the normal curve. Case III on the other hand shows no "reversal" at all.

The index of right handedness[2] shows an analogous phenomenon, to which is applied the name *transference*. In the normal individuals it was noted that the index tended to no regular fluctuations according as the right or left hand preceded. Here, however, many of the cases show a very pronounced tendency for the index to be lower (*i. e.* a relative superiority of the right hand) in the second experiment, when the right hand follows. The indices in the depressed cases are given in the table on the next page.

The indices show that in most cases the work with the preceding hand *transfers* its warming up effect to the work with the following hand.

Cases I-X are under the diagnosis of manic-depressive depression. In Case XI the depressive picture is complicated with

---

[1] *Am. J. Psych.*, XIX, 1908, pp. 446-47, 457-61.
[2] *Ibid.*, pp. 446, 454-56.

Index of Right-Handedness, (Lft. Hd.÷Rt. Hd.)

Case..	I	II	III	IV	V	VI	VII	VIII
First Experiment, Right Hand Preceding	.93	.90	.95	1.03	1.01	.96	.91	.98
Second Experiment, Left Hand Preceding	.81	.89	.96	1.06	.99	.82	.85	.90

Case..	IX	X	XI	XII	Average	Average of 10 Normal Subjects
First Experiment, Right Hand Preceding	1.02	1.05	.91	.92	.95	.89
Second Experiment, Left Hand Preceding	.98	.95	.93	.95	.91	.91

traumatic traits, in Case XII with psychasthenic symptoms. These two cases do not therefore present a manic-depressive picture sufficiently unequivocal to make it advisable to include them in the averages of the other ten. Their experimental correspondence with them, however, justifies their separate quotation. In addition, experiments have been made upon some thirty other pathological subjects, representing various disease groups.

To any one familiar with cases of this type, it will be scarcely necessary to say that, although presented under one and the same diagnosis they really afford very diversified clinical pictures. These individuals are in all respects much more different from one another than the ten normal subjects to whom we perforce compare them. These individual differences have naturally a great influence on the character of the results we obtain from them. Many striking peculiarities in the individual records would become much more intelligible if these differences were made clear. To do this, however, would expand the paper to more than three times its present proportions, and even so would intrude much matter in a form significant only to the clinician who could read between the lines of the histories and physicians' notes. It has seemed preferable, therefore, to dispense for the present with any detailed treatment in connection with the individual pictures, which are best presented primarily from the clinical viewpoint. It may be said at the outset, however, that no subject figures in the present data whose co-operation would not have been regarded as entirely trustworthy in a normal subject.

2. The Results as the Product of Retardation

In gross rate there is an entirely fundamental difference in favor of the normal individuals. Taking the normal standards for right and left hands as 194 and 175, those for the depressed subjects become 170 and 159. It is thus also seen that in the depressed subjects there is rather less difference between the right and left hands than in the normal subjects. In the relationship of the five successive series to each other, the groups show no difference worth mentioning. In both normal and depressed subjects inter-serial warming up is noticed in the right hand, not in the left. As regards individual differences in the two groups, these are distinctly greater in the depressed subjects. The m. v. of the average "total efficiency" figures for the normal and depressed subjects in each experiment, as well as for the average indices of fatigue, or *f*'s, are as follows:

	M. V. of Gross Rates				M. V. of *f*'s	
	R1	R2	L1	L2	R	L
Normal	14	18	14	16	.038	.030
Depressed	29	31	27	22	.043	.041

The depressed individuals, then, differ more among themselves than do the normal individuals. Since this was true also of age, and of other characters irrelevant to the depression, it is not possible to say how far this greater variability is in direct relation to the depression; to some extent, it probably is so.[1]

In the average gross rate for the normal individuals, the preceding hand is the more favored; in the depressed individuals, on the contrary, it is the following hand which is the more favored. The final figures expressing this relation are as follows:

	R1	R2	L1	L2
Normal	195	194	173	176
Depressed	169	171	161	156

In R1 and L1 the left hand follows, in R2 and L2 the right hand follows. Each hand is better when it follows than when it precedes in the depressed subjects. Each hand is better when it precedes than when it follows in the normal subjects. We noted this tendency of the following hand to be the more favored in many of the depressed subjects individually, under the name of "transference"[2] and we now observe that this tendency is sufficient to be reflected in the averages.

[1] The same observation is made by Hutt (*Ps. Arb.* V., p. 354), the women being less variable than the men. It may be noted that the left hand is here less variable than the right, as was also found in the normal group.

[2] *Cf.* the figures for the index of right handedness, p. 45

Finally, there is the fundamental difference between the two groups in the matter of their relative immunity to fatigue within the individual series. In considering the individual cases we meet time and time again with fatigue phenomena absolutely foreign to what has been observed in normal individuals. Not only is the *f* in the depressed cases abnormally high, differing from the normal *f* considerably beyond the limits of the probable error, but there are noted in many indvidual cases fundamental alterations in the shape of the fatigue curve itself. In these cases the curve of work does not begin at its maximum efficiency with gradual loss by fatigue, as in normal subjects, but there is a prolonged period through which the performance actually increases to a level above its initial efficiency. The most marked instances of this phenomenon are to be found in Cases I, IV, and XII. We have here a set of favorable influences operating on the curve of work which are sufficiently potent to more than counterbalance the fatigue losses which normally have the preponderating influence. The degree to which these influences operate naturally varies with the different subjects. In the most extreme series with Case XII the increase in rate was continual throughout the 30 seconds, and it is impossible to say how much longer the gain would have continued before fatigue overbalanced it; in Case II, on the other hand, the gain was apparent only in the right hand and then not further than the second or third interval. In other cases, as VIII and X, these favorable influences are not sufficiently strong to make the later intervals actually faster than the earlier, and their effects are seen merely in a raising of the *f* towards the limit 1.00, which is surpassed in such cases as XII or V.

Only with great caution, however, is an *f* below 1.00 to be accepted as evidence of retardation in the absence of reversals. An *f* above 1.00 cannot occur without reversal; but an *f* below 1.00 can occur either with reversal, or merely a very slow fatigue loss. This latter condition may mean, as above indicated, a true retardation, but it may be also the result of indifferent co-operation, and we usually have no evidence which. One objective criterion of co-operation we occasionally do have: if during the course of the tapping the energy directed to this work "overflows," as it were, into other motor pathways, and the subject shows generalized body movements in addition to the tapping, co-operation can ordinarily be accepted without further question; but entirely trustworthy co-operation occurs without this being observed at all. Beyond an unmistakable lowering of gross rate, the only justifiable criteria of retardation in these experiments are reversal and transference. These are essentially unaffected by differences in co-operativeness.

Either reversal, transference, or both, is observed in all the twelve preceding subjects save Case III. This case gave the appearance of some retardation; there is certainly more ground for surprise at not finding evidences of it in the results than there would have been at finding them. It remains exceptional among the present observations.

Turning attention now to the more general aspect of the results as given in the above special and particular manifestations, it is evident that the common factors in all the anomalies noted in the depressed records are a lowered gross efficiency of performance and an increased responsiveness to the favorable influences operative upon the curve of work. The lowered efficiency of performance is manifested in the decreased gross rate, roughly to 89% of the normal. The increased responsiveness to favorable influences operative upon the curve of work is manifested in the greater efficiency of the following hand as such (transference) and in the phenomenon of intra-serial warming up (reversal).

The general slowing up of all responses which forms so prominent a feature in a large number of depressions, is described under the generic term of *retardation;* and in the present results we are afforded a quantitative index of certain phases of this symptom. Retardation is to be regarded as the most important factor in the lowered gross rates in these cases, nor should we in its absence observe the increased responsiveness to favorable influences which plays so great a part in the present experiments.

With regard to the exaggerated transference effects as a phase of the retardation may be cited the observations of Hoch.[1] These experiments were made on the ergograph, with exceptionally careful technique, and consist each of three "exhaustion" curves executed at intervals of 15 minutes. For comparison with the present experiments, therefore, each curve corresponds best with a record with one hand and the 15 minutes to the 2′ 30″ pause between the hands; save that 15′ was known to be the *günstige Pause* for the ergograph, while we do not know the relation of the 2′ 30″ pause to the tapping test.

The retardation was considered to be given in the fact that the depressed cases showed an increase in the number of times the weight was lifted (*Hubzahl*) throughout the three curves on a single day, though the *Hubzahl* shows no practice increase from day to day. The increased efficiency of performance of the following curves over the preceding ones is entirely parallel to the more favorable position of the following hand noted

[1]On Certain Studies with the Ergogaph. *Journal Nerv. Ment. Disease*, XXVIII (1901), pp. 620-628.

in the present subjects. In Hoch's terminology, we may speak of the preceding hand overcoming certain "resistances," which brings about a greater efficiency in the later series with the following hand. Under this interpretation the "resistances" overcome must be of a general nature since the one hand benefits from the work of the other. The only difficulty in the analogy between the two experiments is, why do not the earlier series bring about greater efficiency in the later series of the same hand, *i. e.*, an exaggeration of inter-serial warming-up? We do not obtain this in any significant degree save in certain other experiments with Cases IV and XII, where there is exceptional overcoming of retardation; in this respect the normal and depressed subjects are practically on an equal footing. Still, when the fundamental differences in the methods are taken into account, this discrepancy cannot be considered a very formidable one. The same general process is at work in both sets of results.

The phenomenon of intra-serial warming up, *i. e.*, the gain in the later intervals over the earlier (reversal), is more closely paralleled in the experiments of Hutt.[1] The object of inquiry in these experiments is practically identical with the present, Hutt making use of the addition test. The depressions are 15 in number, 5 men and 10 women. The properties of the addition test under these experimental conditions in normal subjects are fairly well known. When the ten minutes of work are continuous the number of additions performed in the second 5′ is regularly less than that of the first 5′. If, on the other hand, a 5′ pause is inserted between the two halves of the experiment, the second 5′ is regularly better than the first 5′. Hutt's depressions, like the present ones, showed a lowered efficiency of performance; but whether a pause was inserted or not, the second 5′ showed an unequivocal tendency to be better than the first 5′ The improvement in the second 5′ without the pause corresponds to the improvement in the later intervals over the earlier (intra-serial warming up, reversal) noted in the present experiments. With regard to the days with the pause, the situation is complicated. Normal individuals gain in the second 5′ with the pause, but, according to the normal results given by Hutt from the researches of Plaut and of Rehm (p. 356) *they do not gain so much as the depressed individuals when these latter gain at all.* With considerable frequency occur depressed individuals who lose after the pause, and statistically there is very wide variation in Hutt's Table I, Column 3, (p. 355), between those who gain after the pause and those who lose. All loss after the pause is abnormal, and those

[1]Rechenversuche bei Manisch-Depressiven. *Ps. Arb.* V, 3 (1908), pp. 338-370.

4

who gain after the pause very frequently gain more than the normal. This abnormal gain after the pause is another aspect of the abnormal gain from 15′ rest to 15′ rest noted in the ergographic experiments of Hoch, and probably also of the hand to hand "transference" gains in the present experiments. Those who lose after the pause suggest the "more pronounced" case mentioned by Hoch (Ps. Bull. I, 255), in which both "warming up" and practice were absent.

For their behavior with respect to the pause, Hutt divides his material into four groups. (a) Those who lose without and gain with the pause. This is the normal relationship, but quantitatively, *the loss in the depressions is less, and the gain is more.* (b) Those who gain with and without the pause (by far the most numerous). (c) Those who gain without and lose with the pause. (d) Those who lose both with and without the pause. Though small, these last two groups are of some interest from the interpretational point of view. Be the experimental approach the ergograph, addition test, or tapping test, the interpretation "overcoming of initial resistances through the warming up effects of continued effort" will cover all cases in which the work curve of depressed subjects (a) loses less than the normal loses, (b) gains more than the normal gains, (c) gains when the normal loses. The remaining cases are those who both gain when the normal should lose (without the pause), and lose when the normal should gain (with the pause), and those who lose in both instances. For the loss after the pause Hutt (p. 360) suggests that the cessation from the effort for a sufficiently long period allows the retardation to again establish itself. (*Cf.* also Hoch, p. 624.) The resistances, now added to the fatigue induced by the previous 5′, suffice to make the second 5′ poorer than the first. There are only two cases who, losing with the pause, also lose without it. In neither of these cases does Hutt lay any special stress on the loss without the pause, attributing it to chance *Antriebswirkungen* in the first 5′; but this interpretation cannot be well reconciled with the loss after the pause, for which he in vokes in the second case "rasches Schwinden der Arbeitsbereitschaft durch *Wiederauftreten*[1] der Hemmungen in der Pause." *Hemmungen* cannot *wieder auftreten* which have not been already *beseitigt*, and the experimental records show no evidence of any *Beseitigung*. Rather do we have here a complete failure of response to any "warming up" influences. "In der Tat," Hutt continues, "bot auch dieser Fall eine sehr stark ausgeprägte Denk- und Willenshemmung dar."[2]

[1]The italics are mine. F. L. W.
[2]*Cf.* p. 53.

But whatever may be the correct interpretation of such exceptional cases, the fact that the same general result is reached in these independent investigations, by as many different modes of experimental approach, points strongly in favor of the similar interpretations suggested in each case. It is interesting to note how closely the phraseology of Hutt parallels that of Hoch, of whose work Hutt seems to have been unaware. He writes, "Wie uns scheint, vermag hier die klinische Erfahrung einen Fingerzeig zu geben, in welcher Richtung etwa die Erklärung für das abweichende Verhalten unserer Kranken zu suchen sei. Die alltägliche Betrachtung lehrt uns, dass sie vielfach Hemmungen unterliegen, die durch eine längere Beschäftigung mit ihnen allmählich an Stärke abnehmen. Gilt dies zunächst für die depressiven Kranken, so tritt uns bei den manischen eine ganz ähnliche Erscheinung in der Erfah-rung entgegen, dass sich bei ihnen die Erregung durch dauernde motorische Betätigung in sehr auffallender Weise zu steigern pflegt. Uebertragen wir diese Gesichtspunkte auf die hier vorliegenden Versuche, so würde uns ihr Ausfall unter der Voraussetzung verständlich erscheinen, dass durch die fortlaufende Arbeit selbst Hemmungen beseitigt werden, die den Ablauf der geistigen Arbeit zunächst verlangsamt haben. Daraus würde sich die geringe Durchschnittsleistung unserer Kranken in den ersten 5 Minuten ihrer Arbeitszeit und das Anwachsen der Leistung in den zweiten 5 Minuten an den pausenlosen Tagen befriedigend erklären. Die bei Gesunden hier überwiegenden Ermüdungserscheinungen würden bei ihnen überreichlich wieder ausgeglichen werden, dass sie im Laufe der Arbeit Hemmungen überwinden, die ursprünglich die Leistungsfähigkeit weit unter die Norm herabdrückten."

So far as the depressions are concerned[1], these remarks apply equally well to the above mentioned experiments of Hoch and those of the writer. The final indication seems to be, that the curve of continued work shows in retarded patients an abnormality characterized by a low efficiency of initial performance, which the overcoming of the retardation through the activity of the work either (a) raises to a higher level, or (b) maintains at or near the initial level longer than would normally be the case.

3. The Method as a Measure of Retardation

Inasmuch as the alterations in the work curve observed in

[1] Hutt's manic cases (8 in number) are distributed among the first three groups of his depressions, though he does not give them quite the same interpretation. In the tapping test the manic performance is the absolute antithesis of the depressed. This apparent contradiction will be discussed under a separate treatment of the manic cases.

retarded individuals are of the same general nature independently of the class of tests we employ, the most reliable results are probably to be expected from the test which is intrinsically the most accurate psychological measure. The advantages of the tapping test in this respect the writer has already endeavored to point out. We have now to inquire to what extent the alterations in the fatigue curve may afford a quantitative measure of individual or personal differences in the extent of retardation. It is necessary to consider two aspects of the curves; first the amount of lowering of gross efficiency; second, the transference and resistance to fatigue, or intra-serial warming up. The first factor would indicate the absolute amount of retardation, the second, how far this retardation tended to be overcome. For comparisons between different individuals, the method is very limited indeed. It is difficult to say whether A is more retarded than B from a comparison of their gross rates; the fundamental individual differences are too great. (*Cf.* Hutt, p. 353.) Nor does the amount of intra-serial warming up or "reversal" afford a reliable index of whether one individual is more retarded than another, *e. g.*, these phenomena were most marked in Subject XII, in whom retardation was hardly, if at all, clinically evident.

A much more practical question is whether the method affords a means of measuring changes in the degree of retardation and thus reflecting changes in the condition of the same individual. Its behavior in this respect is a much more constant one. Increased gross rate regularly accompanies improvement in condition, decreased gross rate a drop in the condition. It is interesting to note that it does this with rather more regularity than seems to be the case in normal individuals. Through the thirty experiments reported in a previous paper,[1] the writer could detect no relationship between gross rate and subjective condition, and this result was entirely confirmed by unsystematic inquiries made in the case of Subject II. Among the present subjects, in Case III alone is the clinical change opposite to that of the gross rate. In Case VII, the gross rate does not reflect a slight clinical change. In Case V there is neither clinical nor experimental change. Changes in gross rate occur in the absence of *positive* clinical observations in Cases I, VI and XI. More careful and extended observations on Cases VIII and XII as well as Cases II, IV, IX and X reflect a correspondence with entire clearness.

Knowing the gross rate of a depressed subject in the normal state, the performance in the depressed condition should afford

---

[1] *Loc. cit.*, pp. 480-1.

an indication of the extent of retardation as well as its presence, but as these experiments are made we do not know the normal rate. Only as the rate actually found changes with the patient's condition is any evidence of retardation found in the rate alone. Owing to this limitation, we can be much surer of the presence of retardation when we find the characteristic alteration in the shape of fatigue curve (intra-serial warming up, reversals and transference) brought about by the overcoming of its resistances. The relation which is indicated beween the gross rate and the warming up phenomena is complex, but consistent. Generally speaking, the lowered gross rate indicates a condition of retardation. The warming up phenomena indicate the extent to which the continued activity of the test tends to overcome this retardation, *i. e.*, the responsiveness of the the individual to warming up influences. We shall not, of course, expect abnormal warming up phenomena in the absence of retardation; the hypothesis that resistances are overcome postulates resistances present to be overcome. The important thing to remember is that they may be present without being overcome; and that we may have considerable retardation without the slighest trace of warming up effects. Indeed, there is evidence that these cases, in which we may obtain a fatigue curve quite resembling the normal in form, may present rather the profounder degree of retardation. A slight retardation then, will show a somewhat lowered gross rate and decreased susceptibility to fatigue, with reversal perhaps in the second interval (*Cf.* Case II); when the retardation becomes deeper, the gross rate is further lowered, and the immunity to fatigue is more marked, with reversals in the later, even up to the final intervals; with yet more profound retardation the warming up effect fails, and we obtain in the later intervals a loss approaching the normal. Hutt's Figure III probably illustrates these phases better than do the here quoted results. Considering of course, only the results without the pause, we obtain first the three depressions who lose during the second 5′, just as normal individuals would do (Group a), then a group of eight depressions gradually gaining more and more during the second 5′ (Group b), then another group of three depressions who, while they still gain during the second 5′, yet lose with the pause, showing a more profound degree of retardation than the preceding, (Group c), and finally two depressions (3 records) who again lose during the second 5′, (Group d), just as did the group (a) at the other extreme. The present observations, when considered in connection with the clinical data, are entirely confirmatory of Hutt's findings on this point, and certain individual cases, notably IV and XII, illustrate the matter rather more strikingly, owing to the greater precision of the method. Such interpretations, however, have no relation to the comparison of the depth of depression in different individuals, applying only to the different conditions of the same individual.

4. Retardation and Depression

With this result the strictly experimental portion of our inquiry ends, but in closing, a few words may be devoted to the psychological relations of retardation to the general symptoms of the manic-depressive disorders that seem to maintain the best correspondence with the observed facts of clinical experience. We shall make a grave mistake if we start out with the idea of finding any single formula that will express the relationship of depression and retardation in all manic-depressive states. We shall have to distinguish at least three forms in which retardation may occur, as follows:

1. Cases in which the depression seems to be the fundamental thing and such retardation as is clinically observed can be amply accounted for as secondary to the depression. Case VIII is an excellent example of this type.
2. Cases in which the retardation is fundamental, even to the extent of swallowing up the depression. The present observations contain no really classical case of this type; its best representatives are probably Cases IV and VI.
3. Cases in which the retardation is independent of the emotional condition, including other conditions than manic-depressive insanity.

In the first group we have a class of cases that may be most instructively compared with the reactions in our own depressed moods. Here, as there, the essential of the state is its emotional tone. Of the physiology of the emotions we know little enough, and still less of their pathology; but that emotional states are the product of physiological adjustments of some sort, and do not exist in the absence of these adjustments, few will be found to deny. Every physiological adjustment of the organism carries with it its own essential emotional tone. Such readjustments are, of course, brought about in various ways. In the emotional sphere, it is usually central causes which are the immediately determining factors, though the influence of organic processes, such as digestion, is, of course, far from negligible. The same physiological adjustment as is brought about through a severe mental shock, may be brought about through a specific disease process, just as bodily secretions are influenced alike by mental causes, and by the action of drugs. In normal life, these adjustments occur as reflex responses to the various situations that we meet, and the emotional state is given immediately in the bodily state ensuing upon the response, because this itself constitutes the emotional state. In the psychosis, a certain adjustment is maintained through a pathological condition, and the world is seen through dark glasses.

The former condition results in the transitory depressions which many of us know only too well; the latter, in the fundamental depressions of the psychoses. The appearances which the two conditions present to ordinary observation are really very similar. The "feeling of inadequacy" is beyond question a concomitant of our own depressed moods, and the same must be said of retardation in so far as it is the direct offspring of inadequacy. The changes in speech and gait are especially illustrative in this respect. In sensitive and unstable individuals more serious manifestations may occur, as delusional interpretations of trivial occurrences, and the contemplation of suicide. But the basic difference between the two states remains, in that the former is the product of external, mental causes, and would disappear upon the removal of these causes,[1] while the latter is an internal organic manifestation that responds little, if at all, to mental influences, and disappears when the disease process ceases to be active. It is also true that there is less clouding in the normal depressions, but we must remember that the delusional content in the manic-depressive depressions is often secondary, and results from the delusional coloring, by the depression, of insignificant happenings, or through a feeble attempt to account for the existence of the depression. Moreover, the very group of psychoses in which confusion is the most prominent, and would have the best title to be assigned a fundamental value, are precisely the conditions in which *hallucinosis* is also the most prominent, and this upon a specifically toxic history; namely, the acute exhaustion and the drug psychoses. Here the delusions may be largely secondary to the hallucinations. Altogether, no great stress can be laid on delusions as a primary symptom in manic-depressive insanity, and a considerable number of cases will fail of correct interpretation without the full recognition of the fundamental character of depression and the multiplicity of symptoms, including retardation, which can be secondary to it, as well in normal life, as in the psychoses.

The second group is not so easy to interpret from the standpoint of our normal reactions, but we are forced into making such a group on account of the great disproportion that often exists between the retardation and the depression. We cannot

---

[1]As in Banquo's reply to Macduff on learning of the murder of Duncan,

> "Dear Duff, I prithee, contradict thyself,
> And say it is not so."

satisfactorily account for all phenomena of retardation on the basis of its being secondary to depression. Indeed, some points of view regard the retardation as the one fundamental quality, and the depression as secondary to it. It may be questioned, however, whether such a formula does not involve a conception of the term retardation more limited and specific than is yet justified in clinical knowledge. If an individual becomes depressed at finding himself primarily retarded, clearly the brain-processes which give the consciousness of the retardation do not share to any great extent in the retardation. It must be obvious that a fundamental and diffused retardation must in time result in a general dulling of mental faculties, in which the emotions share. This would give us ultimately a condition resembling apathy rather than depression, and is one that we not infrequently see. In its profounder manifestations, every trace of affective life may be completely engulfed in the retardation. Characteristic motor symptoms may ensue, such as negativism, stereotypies, catalepsy and the like, which often give the case an extremely præcox-like appearance. This, and not affective depression, is the logical result of a generalized retardation.[1] If the reader will pardon a somewhat loose employment of the adjective, a "peripheral" retardation is the normal psychomotor result of a depression (at least in younger individuals); but that a depression might be secondary to a retardation, the retardation, while it might involve some element of a pure thinking disorder, would usually be one involving only relatively low levels of psychomotor activity, a retardation *die sich im Rückenmark abspielt*, to borrow a phrase of Jung's.

But while maintaining the existence of retardations of the above general type and their special influence on the emotional tone, this specialized type of retardation may after all be the best conception of those cases in which the retardation appears to preserve an absolute independence of the affective life. It is very difficult to understand how a fundamental, generalized retardation could have for its affective result any other condi-

[1] Here, again, we have a not inept parallel in normal life. The fatigue following physical exertion is accompanied by a condition not dissimilar to retardation, in which the emotional life shares to a peculiar extent. If while we are in such a condition we are met by a situation of considerable affective value, be it of an elevating or depressing nature, it is quite noticeable that the affect is, under these conditions, considerably less than when we are fresh and unwearied. There is teleology in this, for the promptness, decision and general efficiency of the outward response to a situation tends to be inversely related to the affective power of the situation, and it is precisely in situations likely to arise under such circumstances that a relative efficiency of the reaction may be most urgently demanded.

tion than apathy, and if we are to consider retardation as *secondary* to all the affective states with which it is clinically associated, we shall indeed make of it a psychiatrical *fils de vingt pères*. Kraepelin analyzes the mixed states into variations of three fundamental symptoms, associative retardation or thinking disorder (*Denkhemmung*), motor retardation (*Willenshemmung*), depression (*Verstimmung*), and their opposites. The mixed states are given us in the different combinations of these traits. Inasmuch as in the regular pictures of manic-depressive depression the three run a largely parallel course, the writer has employed the term *retardation* to cover both the first and second of these symptoms; but in the mixed states we have to keep them separate, as we see them occurring independently, and must assign to each a fundamental value. We may find a flight of ideas and either a motor excitement or retardation, combined with either an emotional exhilaration or a depression; we may find a slowness in thinking (thinking disorder) and either motor activity or retardation, combined with either emotional depression or exhilaration. In these states it is difficult to conceive of any one symptom being secondary to another, because they are all so independent of each other. A psychomotor retardation is not what we should expect in the presence of euphoria (manic stupor), and that it does so occur argues its fundamental quality; in like manner do the cases of difficulty in thinking (thinking disorder), emotional depression, but motor activity (*Depressive Erregung*), and those of flight of ideas, emotional exhilaration, but motor quiescence, argue a retardation in the motor sphere independent and distinct from the intellectual as well as the emotional activities. This is the retardation, "*die sich im Rückenmark abspielt.*"

In sum, the immediate relation between retardation and depression may be threefold. A fundamental depression may give rise to a secondary inadequacy or retardation; a fundamental retardation not involving the higher psychic centres may conceivably give rise to a secondary inadequacy and depression. Both symptoms may appear in their fundamental quality, each presenting its special picture independently of the presence and character of the other. The experimental criteria of retardation that have been adduced do not depend for their existence upon phenomena of consciousness; they are merely special aspects of what may be observed under conditionsin which consciousness is experimentally out of the question, as in the decerebrate frog. Our conception of retardation must, therefore, be more objective than something exclusively secondary and dependent upon the mood; the entire phenomena of retardation are the most easily comprehensible from the physiological point of view.

5. Summary and Conclusion

The phenomena of retardation are here studied according as they affect the curve of continued work. The form of experiment is the "tapping test," each individual curve consisting of 30″ continued tapping, evaluated according to the number of taps made in the successive 5″ intervals of the entire 30″ period. The alterations of the work curve that appear especially associated with retardation are evident not only in the decreased amount of work performed (found in the lowered average efficiency of the group studied) but also in the improvement of the work under conditions in which normal individuals would show a fatigue loss or no characteristic change (*reversal* and *transference*). Among the manic-depressive depressions distinct phenomena of retardation, shown mainly in *reversals*, appear in Case I; in Case II they are less prominent; in Case III they are absent. In Cases IV and V they are again quite characteristic, in Case VI less so. In Case VII they are again absent, but there is a trace of *transference*. Transference is especially prominent in Cases VIII, IX and X. Cases XI and XII show especially prominent phenomena of reversal; in Case XII they are the most clear-cut observed. It is not made out whether reversal and transference represent different types of retardation; they occur largely independently, but simultaneously in Cases X and XII. These results, representing as they do increased responsiveness to the warming up effects of continued effort, are paralleled in the observations of Hoch with the ergograph and Hutt with the addition test. The rate of tapping tends to increase with improvement in condition and to decrease with a drop in condition; but the phenomena of reversal and transference may be swallowed up in a very deep retardation, appear as the retardation becomes less profound, and finally disappear as the retardation itself disappears. Kraepelin builds the manic-depressive depression upon three fundamental symptoms of *Denkhemmung*, *Willenshemmung*, and *Verstimmung*. The term *retardation* includes the first two of these; but the present phenomena are presumably the product of *Willenshemmung* plus *Denkhemmung* or *Willenshemmung* alone. In certain cases *Willenshemmung* and *Denkhemmung* may be secondary to *Verstimmung;* and in other cases *Verstimmung* may be secondary to the other two. All three, however, may and probably do have a fundamental and independent existence, especially in the mixed states. The "feeling of inadequacy" is itself never fundamental, but always secondary to either retardation or depression; it forms a bridge between the two. Whatever its origin, the retardation measured in the present experiments is to be regarded as the product of immediately physiological conditions.

That free use has been made of the hospital records and physicians' notes in the interpretation of the individual cases, is scarcely necessary to add. The writer is also under obligations to the physicians of Boston Insane Hospital, through whom access was obtained to Cases IX and X. Special acknowledgments are due to the writer's colleagues in the McLean Hospital, Dr. E. Stanley Abbott and Dr. Frederic H. Packard. Such clinical suggestiveness as these studies may possess is largely due to them.

CHAPTERS FOR A BIOLOGICAL-EMPIRICAL PSYCHOLOGY

Nihil in Mente nisi prius in Mundo

By **Revere Randolph Gurley, M. D., M. Sc.**
Sometime Fellow in Clark University

INTRODUCTION

Rectification of frontier between science and philosophy; Penultimate and Ultimate Philosophies, respective aims and subject-matter. Penultimate "explanations" merely re-statements in successively simpler terms; their advantage, progressive shifting of difficulties where they belong (to Ultimate Philosophy). Biological and psychological, wrongly identified with philosophical problem. Biological psychology and existing systems; its predominantly genetic character, utilization of introspection, exactitude, empiricism; it subsums under Penultimate Philosophy and conforms to Penultimate Logic. Consciousness no peculiar mystery, only one phase of the All-Mystery; its subordinate place in a penultimate philosophy of the cosmos, apparent nature and *modus operandi*, survival value. Foundation assumption of scientific psychology, a rigid correspondence of vividness of consciousness with amplitude, of quality of consciousness with length, of dynamic-inseparable wave. The psychologic problem, tracing of evolution of "mind," but the mystery not that of mind but of consciousness. Anomalous electrical conductivity of carbon. Universality of law of conservation of energy not impugned by any psychic fact.

Clear ideas on psychology are first possible after sharpest distinction between matter of *psychology* and matter of *philosophy;* more precisely, between *Penultimate*-philosophic and *Ultimate*-philosophic questions. Penultimate Philosophy is not concerned with noumenal problems, or with "spiritual" or "moral" meaning for the universe. Such questions are, or may be, legitimate for Ultimate Philosophy. Penultimate Philosophy has a different task; to rear a superstructure, our "elements" *being given*. For penultimate purposes, the ultimate nature of these last may be anything,—God, corpuscles, mental figments, energy turned other side to. In this philosophy, "substance" and "energy" denote merely quality-complexes *absolutely definite and constant* within the limits (temporal and spatial) of our experience with them; and "carbon" and "hydrogen" could as well be replaced by C and H, by algebraic symbols for unknown quantities, or by paragraphs enumerative of their known properties. Scientific (and no less common) language is thus stenographic, cryptographic, hieroglyphic, algebraic, symbolic; everywhere expressive of sums total of sensible (and deducible) properties, nothing more. The final values of our symbols must come from Ultimate Philosophy. But for penultimate purposes, the language of substance-energy is all-sufficient, those purposes being fulfilled when problems are explained in penultimate terms, terms of "matter" and "motion." True, this "explanation" is no explanation at all, but merely restatement of fact in simpler terms, a tracing back of the chain of "continuity" up to, and reformulation of problem in terms of, some earlier link; a *resolution* as distinguished from a *solution*, as in algebra we resolve one equation into another with roots differing by any assignable quantity without getting nearer the values of the roots,—in short, it is a *penultimate* solution. But such resolutions have great advantage; they *progressively shift our difficulties where they belong*, on the *jenseits* of substance, to Ultimate Philosophy.

But it is no less true that *all* the realm of substance-energy lies on the hither side of the resurveyed scientific-philosophic (penultimate-ultimate) boundary. Some will still have "life" and "mind" beyond the jurisdiction of science, but this paper aims to show that these subjects fall within that jurisdiction, and that the biologic and psychologic have been wrongly identified with the philosophic problem.

Each makes his psychology (also his philosophy) in the loneliness of his own mind, and offers it to his fellows to try on. So is offered this attempt of a biologist weary of the stones proffered when in the psychological proletariat he cried unto the Chairs for bread, to bake on his own account. This psychology differs from existing systems in several respects. In conception, methodology, exegesis, it is predominantly biological; mental phenomena are subject-matter of a department of dynamic biology, "mind" is an evolution integration-product. The human (no less the existent avian, the existent reptilian) mind is but the latest ephemeral term in the integration series, and nothing is to be accepted as existing in it[1] which could not have evolved into it. Given a problem in human mind, biological psychology asks: How could such condition have come about, what must have been its tributary

---

[1] One qualification only: If as the outcome (at the end, not at the beginning) of investigation, some irreducible residuum inexplicable on evolutional principles, should emerge, then first would it be time to talk of a "soul." Only in this *a posteriori* (and not in any *a priori*) way, could science come in contact with this "entity," which otherwise remains an exclusively ultimate-philosophic creation, amenable solely to the canons of evidence, whatever they be, of Ultimate Philosophy.

forerunners in the pre-human, what the biological necessity[1] in this case and the neurological condition meeting it, and, lastly, does introspection throw light? The oil immersion of psychology, introspection, yields valuable hints (chiefly suggestive, confirmatory, cautionary), but is liable to all the fallibilities of interpretation inseparable from immersion work; whereas the low-power lens of the sciences (particularly comparative neurology and neuropathology), though less delicate, is interpretatively more certain. This psychology utilizes introspection within its radius of validity, and holds that had psychologists reached a consensus as to what does and what does not constitute matter of introspection, much argument about need for an Ego to appreciate likeness and difference, could have been spared.

Like all sciences, biological psychology conceives its subject-matter as exact, and subsumming under Penultimate Philosophy, it presupposes that philosophy, as specific characters presuppose the generic. For that philosophy, *why* is an illegitimate (being an ultimate) question; its *why* always means: How comes it that? This psychology, then, no more knows why the human is conscious, than the physicist why bodies "fall" together when not propped apart; as he accepts fall as matter of fact, this psychology accepts consciousness as datum of introspection. True, consciousness is a mystery, but no peculiar mystery; only one outcrop the more of the All-Mystery. We do not know what it is, but no more do we know what anything is. But answers essential for a penultimate philosophy of consciousness (characteristics and apparent nature, relations to other things, *modus operandi*), this psychology tries to give. The consciousness-series appears to be the psychic-inseparables[2] of a kinetic series finding nearest affinity in the heat series; therefore, inferentially referable to the infra-red (near Blondlot's N waves?). Like all energy, consciousness is *immanent*. Its action is neither dominant nor epiphenomenal, but reactive-dynamic and resistance-reducing. Though its omnipresence, marvels, and all-importance, form

[1] *Biological necessity:* That which *must* "somehow" come about if an organism is to survive; a condition *sine qua non* for survival. Showing biological necessity in any given case is entirely different from showing how (neurological condition) that necessity comes to be met.

[2] *Psychic-inseparable:* The "subjective aspect" ("inner face," etc.) corresponding to the various members of the kinetic series which arises through latentization of nerve energy; that which is experienced as consciousness.

Dynamic-inseparable: The kinetic species of which consciousness is the psychic expression.

Consciousness- (sensation-, thought-, feeling-) *di:* consciousness, dynamic-inseparable aspect of.

proud capstones in pretentious intoxication-philosophies, available evidence tends to show that for a penultimate philosophy of the cosmos, it is far less important; for, it is less than coextensive with the cosmos, with "life," with nervous energy even, and seems a derivative from the last under certain special conditions. Its office[1] accords with this. In evolution and individual alike, it appears not for itself, but as part of the plasticity-syndrome (plasticity-complex). The survival value of this is two-fold as anticipator of environmental happenings, and as enabler of multiple activity of one structure (through multiplicity of plastic operating mechanisms). Query whether the syndrome might have equally evolved without the conscious ingredient, is idle; *de facto*, it evolved with it (and, to judge from its *modus operandi*, as a *necessary*-incidental constituent). In the syndrome, the part inseparable from consciousness, is "profiting by experience," but a penultimate philosophy of consciousness will *not* say that such profiting is the outcome of the features in the psychic situation to which greatest significance seemingly attaches upon introspection, namely, the fore-imaging (fore-"knowing") "power" of mind. Rather will it say: The foundation assumption of scientific psychology being that every supra-liminal intensity of dynamic-inseparable has its attendant fourth-dimension absorption-spectrum (for inapt metaphor) in, and as, consciousness of a vividness in rigid correspondence with the amplitude, of a quality in equally rigid correspondence with the length, of the dynamic-inseparable wave; then, *given an awareness-consciousness at all*, along with organization of nervous connections *sine qua non* for existence (through rendering possible profiting by experience), there must have proceeded a mosaic-ing of the discrete psychic-inseparable mirror-facets into the environment-parallel awareness-patterns which make up mind. Profiting and fore-imaging are, thus, parallel concomitants (co-expressions, co-functions) of nervous organization. The psychological problem is tracing the evolution of mind. But *the* "mystery" (ultimate-philosophic question) is not that of mind but of consciousness; not why mind-mirror now images environment so perfectly, but why energy happens to have an awareness feature linked with it. But, *for psychology*, it suffices that this is datum of introspection.

Biological psychology conforms to Penultimate Logic, which, like Penultimate Philosophy, sums to acceptance of *de facto*. This logic propounds but one riddle: Is there evidence (demonstration, probability of all grades down to possibility, room

---

[1]*Biological Office*: The net effect, rôle; what teleologists meant by "purpose."

for legitimate hypothesis) showing whether, *as matter of fact* this condition does or does not obtain? Before experience, nothing; from observation, facts; from facts, inductions, deductions, hypotheses, the last to be verified by observation, or where impossible, to be tested with regard to agreement (or at least non-conflict) with established fact or generalization,—that is Penultimate Logic (simple straightforward logic of science). In this logic, it matters not that the condition be unimaginable ("unthinkable"); that is beside the penultimate question. Everything is alike unimaginable; determine facts *as facts*,—then *credo*, *nec impossibile*.

This essay aims at a theory as to nature and mode of action of consciousness which shall be all-embracing, conflict with no established fact or generalization, accord with the law of the conservation of energy, conform to accepted modes of scientific reasoning, and conflict with no datum of introspection. It shows how an anomaly of carbon hitherto ignored, explains (1) why carbon compounds alone admit of that complication of molecular structure characteristic of "organic" compounds; (2) why the period antecedent to the emergence of "life" may well have been one of progressive molecular complication; (3) how, with transit of impinging energies, only carbon compounds undergo modification in such manner that resistance is less for subsequent transits than for the initial transit, which modification being the basis of the most fundamental *sine qua non* for existence, profiting by experience, in carbon compounds alone can the phenomena summing to "life" come to be manifested; and (4) how (*a*) in the intensification-stage of this process in nerve tissue, involving emergence of consciousness (as a critical phenomenon when the relation between nerve energy and resistance comes to exceed a certain "limen" value), the reactive resistance-reducing effect of consciousness eventuates in unlimited profiting by the individual experience, and (*b*) subliminal recession of the emerged consciousness takes place in connection with expenditure in intra-molecular work. With these conditions as bases, and utilizing electrical principles, attempt is made to explain the principal varieties of nervous action; conduction in the infra-liminal ad-cortical and ab-cortical areas, and especially in the labyrinthine mazes of the cortical-psychic zone. Above all else, solution of the cortical-psychic problem is sought, that the equation of the conservation of energy within the living body may be knit up. Three "mental elements" are recognized, *and defined*; and analysis forces admission of distinct molecule-types to account for non-lapse of sensations and their dependency of function upon intensity of stimulus, for lapse of thought, and for non-lapse and dependency of function upon intoxication conditions, of feelings.

And it is found that no need exists by reason of any psychic fact, for impugning the universality of the law of the conservation of energy; rather, there is every warrant for holding that equally with all physical facts, all psychic facts subsum under that law.

Despite dogmatic tone, inevitable considering the brevity, this essay is *all theory*,—just like Professor So-So's big good volumes, but here theory is labeled. No preliminary attempt like this can be the last scientific word on consciousness; ample indeed would it be should this attempt prove the first such word. But one thing is certain; if it be not such first word, that word yet remains to be spoken. *The* desideratum for psychology is *some* hypothesis which shall bring mental phenomena into a connected whole; for no science can progress in advance of formulation of its basal theory.

Had conditions admitted, blemishes inevitable with defective vision would have been avoided. But the facts appealed to are well known, and the only especially original feature is the view point; orientation of data round the electrical anomaly of carbon and dynamic-neurological conception. The rest is culture-material whose source is often forgotten, and for which I cheerfully admit indebtedness to nearly every one.

1829 Washington Ave.,
New York City, 1908.

CHAPTER I

Biodynamics and Biogenesis

Anomalous electrical behavior of carbon; explaining why carbon alone forms basis of "organic" and *a fortiori*, of "life" compounds, its compounds alone being susceptible of modification under impact of energies, in such manner that reaction to subsequent such impacts is less difficult than reaction to the initial impact ("profiting by experience," origin of "genetic" series). Presumable difference in degree of anomaly among the various carbon compounds, affording a basis for the line of increasing molecular complication leading, in absence of microorganisms to effect decomposition, to compounds capable of manifesting $n—a$ of the phenomena summing to "life," to which might come to be added the remaining a properties. Conceivable modes of development of reproduction and motion. Lines along which, *de facto*, variations came to accumulate; the plant line and the animal line, and the dynamic characteristics of each.

Carbon exhibits an anomaly all-important for explanation of "life" phenomena, its electrical resistance *decreasing*, whereas with every other conductor[1] resistance *increases* upon heating. And it carries this anomaly into at least some of its compounds (wax, paraffin, gutta percha, vulcanized rubber,

[1] Except antimony, an exception to be supplied *wherever* "carbon compounds alone" (or equivalent phrase) appears.

ebonite). Decrease in resistance upon heating must likewise take place with gases, they conducting better when rarefied; such decrease also occurs with sulphur (a "non-conductor"), and is a feature of electrolytic conduction.

Comparing, now, behavior toward the electric current, of metallic salts, *e. g.*, zinc chloride, with that of carbon compounds, supposing elements to carry into their compounds all their properties (however masked these may be), the heat emerging upon passage of the current will entail increased resistance in the zinc, but decreased resistance in the chlorine. This assumption by the component atoms, of opposite conductional phases, accords with (and probably explains) their observed readiness to separate, in electrolysis. For, if conduction mean capacity for harmonic vibration, and if molecular integrity be contingent upon the same capacity, then such assumption will introduce vibrational discord incompatible with the continued existence of the molecule.

The case must be the same with carbon compounds such as carbonates, into which a metal enters; accordingly such molecules remain quite simple and are not classed as "organic." Quite different would seem the case of compounds of carbon with elements in which resistance decreases upon heating; here, the heat effect of the current tends to bring all component atoms alike (though probably not in equal degree) into that harmony of vibration implied in the better conduction. Consequently, in such molecules, conduction will come increasingly to supplant decomposition, any resistance tending to become eliminated through reactive effect of the entailed temperature-increase. We thus reach molecules less prone to shattering under action of impinging energies; the possibility of indefinite molecular complication and of the colloidal state.

But some carbon compounds must possess capacity for reacting to energy-impacts in the way of conduction as against decomposition, in higher degree than others; to maintain the contrary, is to assert that the other atoms contribute nothing towards the properties of the molecule. And when such a molecule enters into higher combinations, the resulting super-molecules will be, as compared with the parent-molecules, either equally capable of conduction and decomposition, less capable of conduction but more so of decomposition, or more capable of conduction and less so of decomposition.

Neither of the first two exhibits nearer approach to the vital condition, but the third possibility does; and if amid numerous compounds of this type, one emerged in which in the presence of certain energy-conditions, an atom-group should become split off, with which in the presence of other energy-conditions, the molecule-remainder could recombine, we should already have a crude metabolism. And just such compounds are the enzymes.

These ferments, indeed, possess metabolism (capability of back-and-forth play of take-up and let-go), resembling that of "living" compounds in (1) being quantitatively unlimited as long as (2) undue accumulation of products of their action is prevented, such accumulation inhibiting further activity, with ferment and living compound alike. And the only difference of moment between the two classes of compounds is that the living possesses capacity for "growth." For, of characters considered distinctive of "life," reproduction is a derivative of growth, and motion seems a physical-chemical reaction to environment.[1]

But growth is merely predominance of intake over output; instead of *immediately* after assimilating a molecule, disgorging in decomposition-products its exact equivalent, the life compound disgorges products equivalent to only a portion of the intake, the remainder being held combined, for a longer time.

Motivated by the growth force, the life compounds tend to grow till some counter-force comes into play; but mass increasing as the cube, surface (digestive, respiratory) only as the square, growth must have *some* limit. The sphere representing minimal surface for given mass, larger masses can accumulate in elongate-flattened form. But this form involves frangibility leading to fracture under impact of incident forces (wave action). The fragments, however, can grow to the original size, and reproducing the original frangibility, will repeat the fracture.

Crude as such reproduction would be, its importance cannot be overestimated; for the elongate-flattened forms will thenceforth increase by geometrical progression, while non-fracturing spherical forms will increase only by accession of newly evolved molecules. But causes of decrease (food scarcity) will operate about equally, elongate-flattened forms having even here the advantage incident to greater surface. Thus, even before the advent of carnivorous metabolism, there will be a steady trend toward numerical predominance of elongate-flattened forms. But when over-increase comes to entail extinction, variations in the direction of optimum orientation of the plane of division will accumulate through Preponderant Survival;[2] and when nuclear substance appears, that optimum will lie in such orien-

---

[1] *Cf.* Bütschli, *Ueber Schäume*; abstracted by Dolbear, *Woods Hole Lectures* for 1894.

[2] Preferred to "natural selection," as expressing nothing but the facts, and avoiding implication of selecting agency; also, as available equally in the form, Preponderant Extinction, which emphasizes the real

tation of the division-plane, as entails equal substance in each daughter-cell.

Paradoxically, defeat of "spontaneous generation," furnishes an argument in its favor. To-day it might be impossible for "life" to develop *de novo*, unless under artificial conditions; but why? Because the necessary forerunners, the long series of gradually complicating molecules, would be destroyed by microorganisms. So accustomed are we to this result of their activity, that we speak of "easily decomposable substances," forgetful that there are no more stable compounds than these same, in the absence of an efficient cause for their decomposition; and the myriad laboratory and commercial sterilizations prove that the only such cause is activity of microorganisms, no combinations of physical energies, sufficing. But such compounds never breaking down "of themselves," the period antecedent to the emergence of "life," may well have been one of progressive complication of carbon molecules; for, decomposing agencies absent, such molecules may have remained unaltered indefinitely, till change in environmental conditions led to further complication, however delayed the advent of that change may have been.

Among these earliest forms, variations toward increased rapidity of growth and a prodigious prolificness,[1] would accumulate, provided with such variations there did not happen to be associated others overbalancingly contra-adaptive ("unfavorable"). But variations in the direction of rapid growth and reproduction do not exclude other variations, simultaneous or successive. Consequently, individuals predominating numerically in virtue of prodigious prolificness may perpetuate variations arising haphazard (*de facto*, from unknown causes), till, sometime, those variations either (1) disappear, or (2) become functional through (*a*) increase in size, or (*b*) emergence of some new haphazard variation which with the previous such variation, constitutes a combination having survival value. But, become functional, the previous variation, *viewed retrospectively*, is termed an "incipient structure," a name convenient enough if it do not imply difference between the variations which emerge only to disappear, and those which emerge later to increase. Both are alike variations, and subsequent disappearance of the one set, and subsequent elaboration of the

---

factor, "selection" of some organisms, really meaning their presence as living representatives, by reason of extinction of the remainder. But having most to do with living forms, the affirmative phrasing will be most frequently used.

[1] Here, prolificness refers only to number produced (fecundity); not to proportion surviving to maturity.

other, are equally incidents of prevalent environmental conditions.

In the beginning, successive integrations of structure must have been limited to accumulation of haphazard adaptive variations; especially would haphazard *combinations* of adaptive variations accumulate. At first,[1] such combinations must, on the doctrine of chances, have been exceedingly rare; and consequently, only among innumerable progeny of prodigiously prolific individuals, would they occur in sufficient number. Their elements need not, however, have appeared simultaneously, if one element were sustained by the carrying power of prodigious prolificness, till such time as the other (or others) appeared.

Organisms, then, first predominating numerically by reason of prodigious prolificness, might come later to survive preponderantly (be "selected") in virtue of other characters originally acquired incidentally. But sprung from prodigiously prolific forms, such organisms would, nothing opposing, remain prodigiously prolific,—whence, the condition exhibited by protozoa and protophyta: adaptive reactions and structures in combination with enormous prolificness, the last considered adaptation to conditions involving enormous extinction, though it is perhaps not only (and possibly not mainly) referable thereto, but may be a relic of the original sustaining factor.

Probably the characters considered distinctive of "life" did not emerge all at once; the primordial organisms most likely possessing metabolism only, eventuating in increase of mass ("growth") and reproduction. But the advent of carnivorous metabolism may have been long delayed. Whether that advent ante-dated, accompanied or post-dated that of motion, is indeterminate and relatively unimportant; combination of these activities opens up the illimitable vista of the struggle for existence with its incidental outcome, preferential survival of particular individuals.

Motility may have emerged through accumulation of haphazard variations. If among innumerable offspring of prodigiously prolific individuals, some varied in the direction of greater tremulousness of the protoplasmic jelly, and if among offspring of these individuals such greater tremulousness persisted indefinitely, till later (one or a million years) in some tremulous individuals, variations arose in the direction of correlation of tremulousness into reaction to impact of energies ("stimuli"), such oriented tremulousness would be "motility,"

---

[1] At first; progressive preponderant survival of individuals in which adaptive combinations have, *de facto*, arisen eventuates in a race prone to emergence of such combinations.

the problem in the origin of which seems not so much that of change of form, as association of such change into reaction to impinging energies. But capacity for response to environmental happening being the only element in external reactions of survival value, and accuracy of response the final measure of that value, variations in the direction of greater accuracy would accumulate; variations, that is, in the direction of integration of structure, in proportion as they conduce to such accuracy, to improved sensori-motor connections.

Accumulation of haphazard variations thus eventuates in integration of structure to a point at which the organism becomes able to react adaptively to recurrent environmental happening; when, perfect adjustment reached, modification ceases. But subsequent change in environment will entail consecutive change in habitant organisms, via the same process,—accumulation of haphazard variations adaptive under and to the new conditions. As with linking of variations into combinations, so with linking of new variations on to those combinations, the process must have been exceedingly slow, dependent as it was upon emergence of variations, and extinction of the vast host not possessing them; and only among offspring of the prodigiously profilic would sufficiency of variations occur.

But with every winnowing out of individuals unpossessed of survival-requisite variations, the "successful" remainder comes to be more and more exclusively made up of individuals in which environment-responsive variations have, *de facto*, emerged; therefore, of individuals more uniform in type, as being out of many the particular ones which happened to coincide in adaptation to common environment. An incidental outcome of successive winnowings must then be an ever more pronounced and apparent definiteness of evolutionary trend, series ever narrowing into closer conformity with environmental conditions, necessarily eventuating in an environment-parallel race. And the evolution of that race will thenceforth exhibit a definiteness of trend in keeping with whatever definiteness of trend happens to obtain in environmental conditions. Whence, no presumption of a "plan of evolution" (equally none as to design in the universe) is inferable from this definiteness (secondary and derivative) of biological trend, however successfully or unsuccessfully it may be inferred from the parental definiteness of environmental trend.

Prodigious prolificness involving frequent re-divisions of the protoplasm with corresponding brevity of the nutritive stage, precludes growth to larger mass. But when the original numerical predominance of prodigious prolificness gives place to preponderant survival in virtue of integration of haphazard adaptive variations into combinations (capture and digestion of prey), further accumulations of such variations, in the direction of increased surface (digestive), may eventuate in larger mass with its ulterior possibilities in the way of differentiation of structures. More abstractly, the carrying power may come to pass over from the prodigious prolificness to the adaptive combinations, obviating previous hindrance to growth to larger mass.

From the prodigiously prolific mass whence descended our protozoa and protophyta, variations accumulated, *de facto*, along two main lines, eventuating in plants and animals. The momenta (organismal dynamic factors) determining trend of evolution along the plant line, seem to have been those previously operative,—successive emergence of haphazard adaptive combinations sustained by a prodigious prolificness. For the momenta distinctive for plant evolution are: Enormous expansion of surface in association with fixed habitus and hard protective structures, and relatively prodigious prolificness, eventuating in larger mass; with, probably, proneness to emergence of adaptive combinations, bespeaking descent from individuals in which such combinations had, *de facto*, been wont to occur. At least greater frequency of "sports" in plants, and the fact that most if not all asserted cases of genesis of new species come from botanists, speak for this view.

The momenta determining trend of evolution along the animal line are: Moderate expansion of surface, such sufficing in virtue of association with active habitus, the latter sustained by emergence of structures admitting of alternate and multiple uses of the same organ and of "profiting by experience," eventuating in larger mass; and, following the advent of the psychic factor, progressive decrease in prolificness in connection with an ever-increasing capacity for self-protection, securing of the opposite sex, and care of offspring, summing to a continually rising multiplication rate. Otherwise put, the sequelæ of the emergence of the psychic factor were a steadily increasing tendency to readjustment of survival values, toward a shifting over from survival of the more prolific to that of the more intelligent, and an incidental outcome, integration of "mind," has been the steady trend toward individualization.

CHAPTER II

Neurodynamics

Neuromolecular readjustments consecutive upon environmental happenings, whereby optimum motor response to such happenings comes about; optimum as regards kind, intensity, and rapidity of response. Interpolated inquiry whether nerve energy is electro-chemical. Parallelism between reactive effect of heat in the electric circuit upon the conductor, and that of consciousness-*di* upon nerve substance; indicating the *modus operandi* of consciousness as neither dominant

nor epiphenomenal but reactive-dynamic, and further indicating the nature of consciousness, its mode of genesis out of nerve energy, and its subliminal recession as incidental to its expenditure in intramolecular work,—all in harmony with the conservation of energy.

In way of external nervous reactions, the one *sine qua non* for survival is accurately adjusted motor response to environmental happenings. Loosely termed "profiting by experience," such adjusted response is the outcome of several neurological processes, interblended and separable only in analysis. But clear understanding of reactions ("actions") of animals, can be attained only through preliminary analytical separation and ultimate synthesis, of these processes. Motor response can become adjusted to happenings as regards kind, intensity, and rapidity, of response.

Adjustment as regards kind of response. The following are involved:

Depositional-registrative processes:

1. Registration of environmental happenings. *Imprintation.*

The real type, of which 1 is an abstraction, is, however,

1 *a*. Registration of imprints, *chained in series*, consequent upon environmental happenings chained in series. *Serialization.*

Resuscitative-interpretative processes:

2. Appreciation of environmental happenings *as having been experienced before. Recognition.*

2 *a*. Appreciation of environmental happenings as having been experienced before, not as individual happenings, but *as members of a chained series. Diagnostic recognition.*

All these processes find explanation on ground of that property (variation in resistance with temperature) of conductors, in virtue of which mere passage through them of the electric current entails change in the *future relations* of current and conductor. And with a current dividing itself among several carbon conductors, the ultimate distribution will be different from the initial distribution, a disproportionate amount of current coming to pass via those conductors which, originally offering greatest resistance, suffered most heating. True, there is no evidence whether with carbon itself, one passage of the current would entail lessened resistance for subsequent passages (initial temperatures equal). But that with certain carbon compounds, one passage might entail lessened resistance for subsequent passages, follows from Spencer's conception of the colloid molecule as one in which complexity of play and cross-play of the intra-molecular forces is so great as to induce molecular sluggishness (inertia, viscosity, so to speak), a condition in which change takes place less readily and goes less far than with crystalline molecules under like conditions of impact; but likewise, as with a molecule in which, change once occurred, return to the original condition is correspondingly difficult and imperfect, the molecule retaining some imprint of its experience. Whence, here we reach the origin of "genetic" (irreversible) series, the philosophic import of which can, therefore, be no greater than that attaching to the underlying physico-chemical change in the substrate. But among the carbon molecules manifesting such *plasticity*, those exhibiting "life," are the ones in which it is manifested in highest degree, and among these, the molecules forming the nervous substrate are the ones in which it is manifested in highest degree of all.

Of the above catalogued processes, the plasticity of the carbon molecule explains Imprintation and Serialization,—the former, as transit of energies through such molecules modifies them for subsequent such transits; the latter, as mechanisms once associated in action are thenceforth interconnected by an imprinted line of lessened resistance.

Respecting Recognition, recalling Spencer's conception and such phenomena as isomerism and polymerism, we may picture flow of energy through the nervous system thus: Impinging energies in undergoing absorption, entail unequilibration in the peripheral sense organ, eventuating in a nerve "impulse." Concerning the nature of this, if one will have any working hypothesis (any principle of coherence among his ideas, that is), but one is possible in the present state of science,—nerve impulses are *some* form of undulatory energy. Among known forms of energy, their affinities being closest with electricity, electrical phraseology is admissible, pending emergence of inconsistency or *reductio ad absurdum*. Depicting the changes ensuing in an undifferentiated neurone[1] upon the advent of nerve-impulse waves, potential accumulating to the irruption point, discharge along the neurone occurs, under resistance, entailing emergence of heat which reacts, bringing about such modification of orbit form (vibration system) of the unit-molecules of the neurone as corresponds to diminished resistance (say the ellipsoidal). On account of the unwieldiness, sluggishness and viscosity of the molecules, reorientation of orbit form is at first slow and imperfect, and on cessation of stimu-

[1]Nerve cell plus "fibre;" unit of nervous architecture. The neurones of the intervertebral ganglia extend, one process down to feet, the other up to the medulla. The neurone theory is here used for convenience of presentation; present speculations are not dependent upon it for their validity, much of this line of reasoning dating back to the days of the vaguer "tracts."

lation, the orbit form swings back toward the original figure (say spherical), return being, however, incomplete. But with each recurrence of similar stimulation, the orbit form becomes warped further in the same direction, and every back-swing is less.

While subsequent conduction thus becomes more easy for the same stimulus, it becomes less easy for stimuli of other type; for orbit forms modified in one direction will be less susceptible of modification in other directions than will original unmodified orbit forms,—the experience-imprinted molecule becomes specialized.

Though sometimes, Recognition is not necessarily (or even generally) accompanied by consciousness; most movements being automatically adjusted, *e. g.*, proportioning of pulls of the various leg muscles to unevennesses of the ground. But such proportioning presupposes Recognition as an indispensable interpolation in the sensori-motor progress. How explain such automatic proportioning? On sensory stimulation, nerve-energy waves surge up to the terminal arborizations of the first tier of afferent neurones where they heap up a certain "gradient."[1] If, now, nerve energy be some form of wave motion, different varieties of that energy (those initiated by light waves of different lengths and eventually arousing different sensations) may be assumed varieties of such motion, with different lengths or different vibration rates. And the only explanation accordant with accepted scientific theory, of the better conduction along previously used routes, is that it is a phenomenon of harmonic-vibration type, and the non-conduction along routes otherwise specialized, a phenomenon of interference.[2]

If, then, among the innumerable molecule-chains constituting the second tier of neurones, one has through repeated imprintation under the influence of identical (perhaps also of nearly identical) stimulus waves, become specialized for transmitting them, they will outflow along that neurone in preference to others, not necessarily that they might not eventually have heaped a gradient steep enough to admit of their irrupting into those others, but because *at a time antecedent* to that at which such gradient could be attained, a gradient was

[1] "Gradient" for consistence of phraseology; "unequilibration," less specialized, would answer as well.

[2] I have seen in successful operation a device for calling at will any particular subscriber on a "general circuit," consisting of armature-tipped metallic reeds, one at each subscriber's, and at the telephone exchange duplicate reeds, each attuned to unison with that of one subscriber. When the electric current was switched through any reed, only the bell of the subscriber with the correspondingly attuned reed, rang, interfering pulsations preventing transmission of current through the other reeds.

reached at which the specialized neurone was "permeable." Specialized neurones are, thus, the *antecedently permeable,* meaning thereby that to every increment of rise of potential, there corresponds a vast number of approximately permeable neurone-conductors, and the one among the many possibles which a particular wave enters, is that permeable for it at lowest (first-reached) potential. But, discharge having occurred along the lowest-potential route, such further rise of potential as might have admitted of irruption into less permeable routes, is precluded. Antecedent permeability is the key to conduction-conditions everywhere throughout the nervous system, and it means that with every flow of nerve energy, certain preferential routes will be followed. But this is Recognition, Diagnostic Recognition, the essence of which is *discriminative* preferential conduction, discriminative with respect to the shunting of the initial member of a stimulus-series into the route traversed by its identical (or closely similar) predecessors.

If the stimulus be an unhabitual one, no specialized neurone existing, potential will rise to a gradient at which irruption can take place into an unspecialized plastic neurone, the resistance of which, though greater than that of a neurone specialized for a particular stimulus, for that stimulus, is less than that of a neurone so specialized for a stimulus other than its own.

Adjustment as regards rapidity of response. But the only direct value of the nerve apparatus in the struggle for existence being its ability to fore-parallel environmental happenings, admitting of protective reaction not to the happening of the moment, but to its coming sequels in advance of their advent, 2 and 2*a* (an analytical abstraction *en route* to 2*b*) have no survival value except as they enter in 2*b*.

Accelerative processes:

2*b*. Appreciation of environmental happenings as having been experienced before, not as individual happenings, but as members of a chained series, *the remaining members of which are yet to come—Diagnostic Recognitive Fore-triangulation.*[1]

But the value of this process would be rather limited, did not an identical one occur on the motor side.

3. Ability to execute more rapidly movements performed before—*Agilization.*[2]

---

[1]Hereafter termed *Anticipations; Expectations* of Huxley (*Life of Hume*).

[2]The essential features here are: Increase in rapidity and in efficiency (co-ordination plus power), which we observe increase *pari passu.* "Greater ease upon repetition" is the same thing in psychical terms.

But happenings varying widely in exigency, only organisms can survive which have become able to fore-parallel environment. Neurologically, increase of rapidity upon repetition is the outcome and expression of a progressively decreasing time-consumption upon subsequent, as compared with that upon the initial passage of the nerve-energy current, in reorienting the viscous, sluggish, unwieldy molecules, which undergo reorientation *seriatim* through, and only in proportion to, heaping in the first molecule, of potential to a point at which latentization takes place, whereupon a certain amount of reorientation of that molecule ensues, admitting of passing on of the current, in quantity, to the next similar molecule, in which the same process then goes on.[1] The time-consuming element in the process is the latentizing, which necessarily takes place constantly at expense of potential.

This explanation of increase in rapidity is supported by parallelism between progressiveness of that increase, and that of the attendant psychic phenomena ("greater ease" and diminished vividness of consciousness), to which latter the foregoing explanation is particularly apt.

But imprintation makes for rapidity, not only by impressing the reorientation-habit upon successive molecules of one neurone, but more by perfecting inter-neuronal connections ("associations"), and most of all by entailing the turn-table reorientability of the plasticity zone (which ensures concentration of conduction indispensable for rapidity), and replacement of conduction via conscious, by conduction via reflex tracts. But all these are incidental and inevitable step-by-step elaborations of the one process of molecular reorientation-imprintation.

But when rapidity of conduction along the happening-indented, environment-paralleling imprint-chains comes to exceed the rate at which happenings succeed one the other in the environment, on advent of stimulus annunciative of happening *a* of the happening-series *a-n*, the entrant waves sweeping along the much-imprinted nerve trellis-work ahead of the drama-development, before the advent of the stimulus annunciative of *b*, the organism is already fore-reacting to the impending threat inherent in *k* ("instinctive warding off"). But this is Diagnostic recognitive fore-triangulation and Agilization. When accompanied by consciousness (not at all necessary), the process is heralded as "fore-knowledge" (fore-imaging), but conscious or unconscious it equally consists, in

[1] O. J. Lodge (*Internat. Monthly*, 1900 (May), I, p. 495) takes essentially the same view (as far as this point goes), thinking conducting substance comparable to a series of condensers, charged *seriatim* to full capacity; and this is, likewise, his explanation of rate of transmission. His "capacity effect" is my "reorientation."

essentials, in resuscitation of fossil experiences imbedded in the imprint-chains, and eventuates in repetition of the motor response which, *de facto*, sufficed in former emergencies.

INTERPOLATION: IS "NERVE"-ENERGY ELECTRICAL?

Eccentric, bizarre theses apart,[1] nerve energy is a kinetic species, either distinct from all, or identical with some, species operative outside the body. So long as it is conceived as a species of the energy genus, interchangeable with other species at exact (as yet undetermined) equivalent, the difference between the alternatives is not fundamental; still they have very unequal merit. If one elect the first and, without the necessity being proven, begin multiplying energy-species, where will he stop? If "nerve" energy shall be a distinct species:

Hypothesis. Difference in microscopic structure and in function constitutes a valid criterion of distinctness of associated energies, or—

Hypothesis. Such difference does not constitute such a criterion.

On the first, nerve, muscle, gland (etc.) energies are distinct kinetic species. But if to every large difference in structure and in function corresponds difference of species, for minor (but well-marked) differences must be admitted varietal differences; four varieties for the connective-tissue group, three for the muscle group, two for the nervous system,—more for glands, mucous membranes, etc.

The second hypothesis avoids this confusion, leaving clear way for assuming the foregoing factitious species and varieties to represent extra-corporeal energies, here peculiar in *facies* by reason of peculiarity of substrate; an assumption in accordance with all known facts. For, intra-corporeal heat, electricity, light, differ in no wise from extra-corporeal; and every chemical advance makes increasingly manifest the identity of intra-corporeal with extra-corporeal chemism. And the substrate which, in homologous muscles in allied genera and other muscles in the same fish, generates "contractile" energy, is in certain muscles of some fishes modified to an electricity-generating substrate.

---

[1] As that nerve energy is a Something-not-energy. Our meek patience with vagaries where consciousness is concerned does not extend to such theses *in re* nerve energy; and fondly we hope the "philosopher" of the future will be limited to the alternative of admitting consciousness to be, or of denying that nerve energy is, a kinetic species. Whoso wills to maintain the latter must explain:

1. Potential energy of food excluded, *ex hypothese*, from serving as source of this Something-not-energy—(*a*) whence is this Something derived in embryo, from spermatozoon, ovum, or both? (*b*) in growth from embryo to adult, what is its source of increase? (*c*) from what source is it kept up from day to day?

2. (*a*) On the assimilative side, where do ordinary forms of energy cease to give place to this Something? (*b*) not interchangeable with, what is the relation of it to, ordinary forms of energy, and their relation to it; and how is the equation of the conservation of energy within the living body, all at loose ends, to be knit up? (*c*) on the dissimilative side, where does this Something cease to be, and ordinary energies again come into play, and what is the destiny of this Something after it has done its work?

3. Are nerve, muscle, gland (etc.) energy merely different modes of manifestation of one Something-not-energy, or distinct Somethings-not-energy? If the former, then muscle energy being in some cases demonstrably electro-chemical, the case falls. If the latter, heavy *onus probandi* lies on the asserter, to show wherein the case for nerve energy differs from that for muscle energy; and if histological differentiation shall serve as criterion of distinctness of Somethings-not-energy, to show where multiplication of such Somethings is to stop (striated-muscle Something, smooth-muscle ditto, heart-muscle ditto, and what not).

"Trivial!" I think it is myself, but for outspoken declaration that consciousness is Something-not-energy, yet can control energy, see Minot (*Pop. Sci. Mo.*, 1902, lxi, 289-303.)

Eccentric bizarre theses apart[1], the hypothesis on which one can correlate ("explain") most facts with fewest difficulties, is that nerve energy is electro-chemical-electric,—meaning consisting of waves of electric (or near-electric) type, set going in the sense organs by stimuli, which waves excite in nerves (perhaps mostly in the cells) chemical changes generating tributary reinforcing similar waves.

To the view that nerve energy *may be* electricity adequate objection has never been made. Difference in speed has been appealed to; *meaning* difference between speed of nerve energy in nerve, and of electricity in wire. Speed of electricity in nerve has never been measured (how could it be?); what has been measured is speed in nerve of an electrically generated nerve impulse. But transit of electricity through a series of sluggish, unwieldy, viscous molecules which have to be reoriented (via the heat effect) *seriatim* as it goes along, presents a condition not at all comparable with transit along a wire.

Also, nerve energy is said, unlike electricity, never to cross a gap (between apposed proximal and distal ends of a severed nerve). Even if undisputed, this would not necessarily invalidate the theory of identity; too many elements of uncertainty existing (impossibility of excluding diffusion of current around cut ends, improbability of apposition of original neurone segments when the cut ends are brought together). Further, the only way that, in view of the phenomena of electric currents in nerve, the natural inference of the identity of nerve energy with electricity has been staved off, is by asserting (very probably correctly) that the currents are "currents of injury," the outcome of changes taking place over the exposed cut ends of the nerve. But this explanation cannot be worked double; if the cut ends are sufficiently altered to generate "currents of injury," they may well be so altered as not to transmit nerve energy. Finally, cases of rapid restoration of function have been accumulating in recent surgical literature to an extent to render it probable that the whole question of non-transmission across a gap may yet come up for re-hearing with the ultimate verdict in doubt.

If nerve energy be electrical, everything clears up,—its source is the chemical energy of the food, it enters the equation of the conservation of energy, it can induce muscular contraction as electricity induces such contraction. Moreover, its latentized form (the psychic aspect of which we know in and as consciousness) reacts upon nerve molecules as the similarly generated by-product of electricity, heat, reacts upon carbon molecules. Consciousness here finds no mere analogy, but a true homology of relations, and its first parallelisms of relations, former comparisons being mere fanciful illustrations, ikons to assist the imagination of the vulgar to "understand" unintelligible things. The homology may be thus shown:

---

[1]As that nerve, muscle, gland energy are merely different manifestations of "vital force."

Hypothesis. "Vital force" is Something-not-energy.

Hypothesis. "Vital force" is a kinetic species.

The first requires answer to all questions in note, p. 77; and, in particular, explanation how in electric fishes muscle Something-not-energy can be replaced by electricity.

Though the second seems expressly designed unerringly to miss every advantage and infallibly to catch all difficulties in the situation, though exceedingly improbable (almost demonstrably false); so long as things fall within the equation of the conservation of energy, we, become old and patient, might make shift to live with it, cursing it roundly for the clumsiest contrivance it was ever our ill-starred fortune to meet.

Electricity	Nerve Energy
Working against resistance, heat emerges.	Working against resistance, consciousness emerges.
Heat diminishes resistance in carbon molecules.	Consciousness diminishes resistance in nerve (carbon) molecules.
In such molecules, the heat of the electric circuit tends to eliminate itself.	In such molecules, consciousness tends to eliminate itself.
Better conduction is the result.	Better conduction ("profiting by experience") is the result.

Adjustment as regards intensity of response. But how do the resistances become so nicely graded as to account for all the minutiæ of adjustment observable? Environmental happenings differing widely in violence, only organisms can survive which come to be able to react to different such happenings with very different intensities of motor response; horses must not waste on escape from flies energy needed for flight from the wolves, salmon bound up-stream on sexual mission must expend every kinetic unit to surmount the cascade. That is, graded happenings must be met by graded response. But intensity of response is determinate as (variations in intoxication-condition apart) the result of the kinetic outpour of the plasticity zone. That outpour is, however, determinate, as the resultant, in magnitude and direction, of the forces undergoing fusion (superfusion, etc.). But those forces are determinate, as resultants of amount of nerve energy entering, and of its lines of transmission within the plasticity zone; these factors involving (as dependent function) liberation of a determinate amount of energy by the cell elements of that zone. Amount of entrant energy and lines of transmission are, however, determinate; amount as a dependent function of intensity of stimulus, lines as quotients inversely proportional to the resistances. Whence, graded motor response can come about only through grading of intensity of stimulus, of resistance, or of both. But, being environmental, stimulus is for the organism a fixed quantity and ultimate factor; consequently, graded response can come about only through grading of resistance. And stimuli being graded, it must come about. For, among carbon molecules differing in initial resistance and capacity for ultimate modification, preponderant survival will occur of individuals offering the optimum, which will lie where initial resistance suffices to develop just the amount of heat which will reduce resistance to optimum conduction, measured in terms of final œcological value, motor response; just that heat which will expend itself in intra-molecular work. For, if by reason of too small, or by reason of too great, initial resistance, heat relatively insufficient emerges, conduction along that molecule-chain will be deficient, eventuating in deficient response. Contrariwise, heat relatively too great will entail unduly great conduction along that particular molecule-chain and consequent disproportionately great response of certain muscles (inco-ordination).

So far, the hypothesis demands a separate molecule-chain for every intensity of every stimulus. But biological economy (most needed in the brain with whose rapidity of enlargement the fundamental process of bone-development in cartilage has been unable to keep pace, whence cranial-vault development in membrane) suggests that each molecule-chain may transmit several (or many) intensities of stimulus; that each chain has a certain polyvalency.

CHAPTER III

PSYCHODYNAMICS

I. Factors Determining Direction of Energy Flow: Fundamental dynamic constitution of nervous system (stabile factor); its persistence temporarily, spatially, zoölogically. Intoxication condition of nervous system (labile factor); principal variations in reaction type. Stimulus (environmental, annunciative factor).

II. Cortical-Psychic (Conscious) Activity: No thaumaturgy, but biological resultant contains reactive as well as incoming components. Phylogenetic divisions of cortex. Bioneuropsychodynamic relation. Definition of consciousness-facies,—sensation, thought, feeling; their differences referable back to differences in underlying arousing conditions; their respective dynamic rôles. Law of lapse, of fade. Taxonomic importance of lapse.

A, *Pre-Compositional:* Non-lapsing facies take rise in elastic-molecule structure. Sensations, non-lapsing by reason of biological necessity for space representation; latter, outcome of interproportionment of simultaneous sensations parallel with interproportion of arousing stimuli. This requires constancy of resistance in sensation area; which constancy ensures projection forward into coming composition of forces, of stimulus waves environment-representively interproportioned. If nerve energy is electrical, heat-generation in nerve circuit is directly as energy-radiation in environment, fulfilling biological necessity for environment-representation in cortex; comparison of Fechner's with electric-heat formula suggests his constant measures resistance of sensation area, his logarithmic function judgment-coefficient from distance-relations of moving objects, and correlation of psychic-inseparable with dynamic-inseparable on basis of increment for increment. Inheritability; Baldwin-Osborne-Morgan "factor" superfluous. Consciousness penultimate-philosophically.

B, *Compositional:* Plasticity-zone action divisible into Route, Fusion-Composition-Reorientation-Psychogeny; repetition of previous activity except for differences the outcome of imprintations or of variations in "functional condition." The last apart, Route is strictly a dependent function of imprintation conditions; determines character of reactive forces but not their amount. Fusion: Abutment area; inequality of imprintation inevitable outcome of unequal frequency of happenings, leads to formation around abutment area, of fusion focus. Imprintation along intra-, exo-, and extra-focal routes, contrasted. Attuning of molecular orbit forms; reciprocal modifying action of wave on orbit form, of orbit form on wave. Beginning reconcentration involves fusion. Composition of forces: Only another side of fusion; amplitude of fusion wave sum of intensities of component waves. Superfusion-supercomposition. Reorientation: Incident to fusion-composition; its essence, dynamically, redistributions in kinetic field whose outcome is optimum motor response.

C, *Concurrent-Compositional:* Given sensation (space-orientation) and feeling (reaction-regulating) ingredients, organisms could oppose appropriate response to happenings; reaction-sequences *sine qua non* for existence could come to be motivated through adjustment of second factor, via Preponderant Survival. Feeling and thought independent variables; their respective offices; that of feeling, rendering of tracts temporarily reflex, ensuring, without fatigue, certain prompt performance of sequences too long-lasting and complicated for execution via reflex action. Alternative, that migration-sequence thought-motivated, untenable. Thought, *the* prophetic sense. Introspection and self-consciousness. Allocation of feelings, to motor area or special affluents of it. Independent of intensity of stimulus, feelings cannot be "in the sensation;" not lapsing, cannot take place in thought tracts. Sensations presumably tributary, some to pleasantness, some to unpleasantness, tracts. Dynamic interrelation of feeling and thought tracts; probably former on resistance-plateau accessible to energy-inflow when intoxication lowers resistance; while energy can flow from feeling to thought tracts, reverse flow contingent on intoxication-lowering of resistance. Relative permeability of feeling and thought tracts, outcome of accumulation toward optimum intoxication conditions and optimum plasticity, respectively.

III. Infra-Liminal ad-Cortical and ab-Cortical Neurones: Constant but low resistance; composition cannot take place by reason of biological necessity; this molecule type probably phylogenetically the older.

I. FACTORS DETERMINING DIRECTION OF ENERGY FLOW

The factors determining large fluctuations in energy flow, are divisible into two classes,—those which go to make up the selective receiving capacity of the nervous system, and those which fall under the head of annunciative value of stimulus. The first comprises a constant and a variable; the former being the fundamental dynamic constitution of the nervous system in the species (smaller or larger zoölogical group), the latter, the intoxication condition of that system at any given moment (the two summing to the "functional condition" at that moment). The actual reaction of an individual will then be the resultant of one constant and two variables; and we might epitomize, paradoxically, the respective shares by saying that the fundamental dynamic constitution determines form of reaction, subject to the qualifying effect of a "predisposing cause" (intoxication condition), and the dominant rôle of the "exciting cause" (stimulus).

Fundamental dynamic constitution of nervous system. To the operation of this factor are referable those reactions (consequently also, the habitually preter-active reorientation proclivities necessarily precedent thereto) which are approximately constant in an individual from age to age, season to season, and especially those common to him and others of the same species (particularly of the same sex) or narrower or wider zoölogical group. Examples: The herbivorous and carnivorous habits, and a chain of habits tributary to the latter (lying in wait, chasing, seizing, killing). The essential characteristic of the reorientation-proclivities referable to this factor is persistence in space and time; over large zoölogical groups, wide areas, and (inferentially from fossil structure) extensive periods of geologic time. This factor is, thus, the element in animal reactions, which makes for stability.

Intoxication condition of nervous system. The most striking characteristic of animal (especially human) reactions is caprice-like super-variability often defying successful prediction and leading to belief that they are "free," though they conform closely to the law of the average.[1] Of late, fore-falling shadows of the deterministic hand on the wall, have led to much subtle trimming as to just how free, "free" is. Leaving that to those interested, apart from fatigue (in which diminished activity in unduly used mechanisms incidentally involves shift over to other mechanisms), nothing yet brought forward sufficiently explains this pre-eminent variability. But fatigue represents one case of a wider category, the intoxication condition of the nervous system, the sum total of the *chemical* forces acting on the organism. These embrace habitual and unhabitual products of metabolism, habitual saline intake; secretions of glands (sex, adrenal, thyroid, pituitary); products of micro-organisms (toxins, ptomaines, leucomaines); and the motley host of unhabitual chemicals called "drugs." The two-sided relation involved in emergence of consciousness explains similarity of psychic action of substances widely remote in composition; some intoxicants supposably increase or decrease nerve energy, some increase or decrease resistance, and some act simultaneously on both factors in the same or opposite directions, whence the possibility of all necessary combinations of effect.

To the operation of this factor upon the fundamental dynamic constitution are referable those reactions which vary from sex to sex and season to season; and many of those which in one individual vary from age to age, moment to moment, and from conditions, of health to those of disease. This factor is, then, the psychic variation-inducing factor, *par excellence*. The following comprise the principal variations in reaction type:

Variations from sex to sex: Everywhere observable (collection of instances in Darwin's *Descent of Man*). Throughout the vertebrates, the male exhibits mental attributes summing to aggressiveness, whether in combat or the sexual pursuit; whereas the female shows a relative backwardness, qualified by the periodic emergence of "mother-love." All psychic characters being more apparent the greater the mental development, these variations are well-marked in the human, and have given rise in all peoples to sexual-social customs and rituals reaching extreme elaborateness. These variations are explicable as the outcome of unisexual intoxication.

Variations from individual to individual: These are seen in all animals including man, in which in connection with greater brain development the "principle of individualization" attains most marked expression. Some are addicted to gastro-intestinal matters; others turn from these to respond to sex stimu-

---

[1] *Cf.* Morselli, *Suicide*. Yet if any act be "voluntary," suicide is that act.

lation; still others recoil from this to engage in its artistic or religious equivalent; while some react only to a particular congeries of stimuli ("hobby") entirely peculiar to themselves.

Reactions strictly personal to the individual and constant in him from age to age, season to season (etc.) are referable to his *Anlage* and mostly to the fundamental dynamic constitution of the nervous system.

Variations from age to age: Under this head come reactions in connection with dependence of offspring upon parents, protective concealment of defenceless young not cared for by parents; and the predominantly gastro-intestinal and motor interests of the young, in contrast with the sexual and later parental interests of the adult. These variations are referable to age variations in intoxication conditions, whether in the way of surplus or deficit, except where they are the outcome of imprintation ("experience").

Variations from season to season: Seasonally recurring ruttings, combats, migrations, abstinence from food on migration, are among a host of instances. These are referable to seasonal variations in intoxication conditions.

Variations from moment to moment: These are continually observed, especially in the young ("distractibility"). Most of these are in reaction to the perpetually shifting environmental stimulus-succession; but the hour-to-hour variations ("moods") into which the moment-to-moment changes are in-woven, are, in my experience, largely the outcome of shifting intoxication conditions.

Variations from health to disease: These range from restlessness of fever to wildest delirium or to deepest coma; from the normal feeling of well-being, to the delusions of grandeur of paretic dement or to the unrelieved gloom of the melancholic. Many of these variations are now recognized by medical writers as due to intoxications.

Finally, marked change in mental condition frequently follows critical discharges (colon lavage, ejaculation, abscess evacuation, etc.).

Stimulus. In what sense can kind and intensity of stimulus be said to enter as elements in determination of form of reaction and of reorientation precedent thereto? Any classification of stimuli from the standpoint of their reaction-evoking (reorienting and psychogenic) action will be incommensurate with classification based on their physical or chemical properties. This is exemplified by difference of reaction of different nervous systems to the same stimulus, the visual stimulus-complex, "cat" evoking promptest reaction from a mouse, none from a by-standing cow. Still, this incommensurateness does not mean that reaction-evoking (reorienting, psychogenic) action of a stimulus is independent of its kind and intensity, but that these attributes enter as elements of effectiveness not by reason of their physico-chemical values, but only as they are necessary elements toward identification; toward Recognition, of the stimulus as one experienced before, as a member of a chained series, the remaining members of which are yet to come. Effectiveness of stimulus thus corresponds to an *œcological* value, the annunciative power. This value, however, bearing no direct and definite, but only an incidental relation, to the physico-chemical properties of the stimulus, reorienting (reaction-evoking, psychogenic) capacity of stimulus is not, properly speaking, a dependent function of stimulus at all, but is an adaptive acquisition (susceptibility) of the organism.

II. CORTICAL-PSYCHIC (CONSCIOUS) ACTIVITY

We now approach the psychologist's ultima thule; his holy of holies, presided over by Vishnu-Ego, to whom the brain submissively hands up all the data received by it from the sense organs, and Who, having duly deliberated thereupon, vouchsafes to Its neurone-minions Its behests. We shall, however, find no something-from-nothing device; but only that the final mechanical resultant discharged is not the mechanical resultant of the incoming forces of the moment alone, but a resultant into the composition of which enter, *as components*, certain other, reactive forces, contributed by the organism itself. These forces represent no necromantic thaumaturgy, but merely the static forces (partly rheostatic, partly reactive-dynamic) of the organism, accumulated through Preponderant Survival; forces which contribute to determine the only form of reaction of which its particular structure is capable.

Phylogenetic Divisions of Cortex. In man, the association tracts have wedged far apart (yet bridge over the hiatus between) two great brain segments, the sensory and motor areas. But as we descend the zoölogical scale, this intrusive wedge narrows up and practically disappears, and we reach the original state of approximation. Consequently, the cortex comprises the following regions:

- Palæogenetic.
  - Sensory area.
  - Sensori-motor bridge.
  - Motor area.
- Neogenetic.
  - Association tracts (as developed in higher vertebrates).

But "association tracts" being used in an unduly wide sense, the term *plasticity zone*[1] will hereafter be used.

Classification of Consciousness-Facies. The first requisite for understanding of mental action is trenchant distinction between mental process and mental content. There is but one mental process, embodied in the bioneuropsychodynamic relation: *In carbon molecules the heat incidental to conduction under resistance reacts upon the molecules in such manner that resistance is less for subsequent than for initial conduction; the intensification stage of this process in nerve tissue may surpass the critical, limen point, and involve the emergence of consciousness.*

There is but one mental content,—consciousness. Calling sensations, thoughts, feelings, "elements of mind," risks missing the *real* question: How comes it that consciousness, as introspectively observed, exhibits sometimes characters diagnostic for sensation (variations in quality and in intensity), sometimes those distinctive of feelings (fluctuation over the pleasantness-unpleasantness gamut), sometimes only the colorlessness of thought? The natural answer is: Differences in the aroused consciousness are referable back to differences in the evoking agencies; peculiar combinations of conscious characters are the outcome of correspondingly peculiar combinations among those agencies. Thus, consciousness *in forma* sensation differs in quality according as it is aroused by waves of auditory-nerve, optic-nerve (etc.), length; in vividness with the amplitude of the arousing waves. And the various sensation-facies assumed by the aroused consciousness are mainly outcomes of different combinations between these two independent variables.

What, now, do we *mean* by consciousness *in forma* sensation, *in forma* thought, *in forma* feeling; what are the underlying conditions entailing these introspectively observed differences in facies (of *content*); and what the respective dynamic rôles?

Consciousness *in forma* sensation: The pictorial, environment-representative ingredient in mind, exhibiting variations in quality and in intensity; exempt from laws of lapse and of fade;[2] office of dynamic-inseparable, introduction into composition of forces, of environmentally (spatially) interproportioned

---

[1] *Plasticity zone:* The locus of plastic neurones, embracing besides the association tracts, at least those parts of the sensory and motor areas, destruction of which obliterates memories.

[2] *Law of lapse:* Repetition of actions originally accompanied by thought involves progressive diminution (to disappearance) of that thought.

Law of fade: The rise-and-fall curve of feeling, parallels, as a dependent function, the rise-and-fall curve of the excitant intoxication; consequently, in the last analysis, the evolution-involution curve of the intoxicant organ or focus.

components; seat, sensory area (*pars*); palæogenetic; condition of arousal, critical (limen) value of relation between nerve energy and resistance in elastic molecules.

Consciousness *in forma* thought: That which lapses, the cognitive ingredient in mind; exempt from law of fade; office of dynamic-inseparable, reactive-dynamic effect on plastic nerve molecules, eventuating in "profiting by experience;" seat, plasticity zone; neogenetic; condition of arousal, critical (limen) value of relation between nerve energy and resistance, in plastic molecules.

Consciousness *in forma* feeling: The affective ingredient in emotion, exhibiting variations between opposite affective poles; exempt from law of lapse, subject to law of fade; office of dynamic-inseparable, intensification or minimization of reaction; seat, special affluents of motor area; palæogenetic or palæo-neogenetic; condition of arousal, critical (limen) value of relation between nerve energy and resistance, in intoxication-prepared elastic molecules.

Taxonomic value of lapse. But, it might be argued, sensations not lapsing (they are equally vivid however often experienced), are not consciousness at all; and, *de facto* we *think* not *in* but only *of* red. Lapsing is, however, nothing inherent in consciousness *qua* consciousness, but is an incident of the type of structure in which in man consciousness (*in forma* thought) happens to be chiefly manifested. And sensations not lapsing must be assumed to take rise in structure different enough to account for the radical difference in behavior.

The alternative view offers insuperable difficulties. If sensations are in a distinct category, they either react dynamically and so back-flow into the ebbing nerve-energy current, or do not so react and back-flow. If the former, possessing in common with thought, awareness, they agree with it in all important features except lapse (otherwise explicable), and the category falls. If the latter, there is no conceivable way they can satisfy the equation of the conservation of energy, and they are left without conceivable office.

The prime taxonomic importance of lapse consists, then, in its being an index of plastic structure. Lapse appears, indeed, not for itself, but incidentally, *en route* to profiting by experience; and thought lapsed, can, on occasion, revive as readily as it lapsed.

A. Pre-Compositional

Sensations. Sensations do not lapse by reason of biological necessity. Motor response appropriate to environmental happening can come about only as a sequel to accurate spatial orientation; consequently, only organisms can survive in which there exists *somewhere* antecedent (in the sensori-motor progress) to the composition zone, an area in which the intensities of the incoming stimulus waves bear to one another proportions representative of the proportions obtaining among the environmental objects among which the organism has to move. This area must be antecedent to the composition area, as only so can the stimulus waves carry into the coming composition of forces a series of kinetic values so interproportioned as to be space-representative. This area will be the sensation area. For, vividness of sensation being a dependent function of intensity of stimulus, the interproportions of the vividnesses of sensations simultaneously aroused, will be (secondarily) a dependent function of the interproportions of the initiating stimuli. But this interproportion (of light, shade, color, *e.g.*) being derived from, will be representative of, existing conditions and interrelations of the environmental content of the moment. The sensation area is thus an intra-cranial camera-obscura shield-mirror, in which is perceived the direction in which the environmental Gorgon is to be struck.

The sensation area is the last stage in the sensori-motor progress, in which the intensities of the stimulus waves bear to one another proportions representative of environment; immediately succeeding this stage, these waves enter into composition with the reactive forces of the organism, forces different in every zoölogical group and individual. But the interproportion representative of environment is carried forward from the sensation into the composition stage. For, interproportion of vividness of sensation being a dependent function of interproportion of intensity of stimuli, interproportion of dynamic-inseparables, and consequently interproportions of reactive-dynamic effect and of reductions in the denominator of Ohm's formula,—all are equally dependent functions of the intensities of those stimuli. Whence, environment-proportional sensation-values, being multiplied by proportionals, remain environment-proportional.

So much for the biological necessity for constancy of relation between cortical-psychic processes and environment; what neurological conditions meet the necessity? What must be the molecule-type in the sensation-yielding neurones? One offering:

Constant resistance upon successive conductions.

Relatively *high* resistance.

Only with resistance constant can vividness of sensation be a dependent function of intensity of stimulus. But constant resistance upon successive conductions means a molecule yielding temporarily under the reorienting effect of the latentized nerve energy (*in forma* sensation), but *without undergoing imprintation* and reassuming its original condition upon the cessation of the reactive-dynamic action; in short, an *elastic* molecule. This is no contradiction of the main thesis; carbon molecules *may be* plastic, but even in the body many are not (cartilages yielding temporarily to pressure do not yield more readily and extensively, and the cornea transmits light not better but only equally well, time after time). And carbon molecules differing in degree of plasticity, accumulation will, according to the biological necessity, take place in the direction either of plasticity or of aplasticity.

Fechner's law: If 1, 2, 3 (etc.), units of energy fall successively on 1 sq. cm. of body surface, the energy on each constituent small square of the centimeter-square (consequently, on each peripheral sense organ), is the square root of 1, 2, 3 (etc.); and the impinging energy initiates a current as the square root of the energy radiated on to body surface by the environmental source of energy, unless sense-organ activity alter proportions. But heat in the electric circuit[1] being as square of current strength, if the current initiated be electrical, *heat-generation in the nerve circuit is directly as energy-radiation in environment* fulfilling the biological necessity for environment-representation in the cortex, and showing how extensity in environment becomes polarized in one plane, as intensity, in the cortical-psychic zone, entailing qualitativeness of consciousness (more or less) in correspondence with the biological advantage of limitation of decision, at each moment, to choice between alternatives. Comparing Fechner's with the electric-heat formula, the constant corresponds to the resistance. Two facts favor their identity,—they are multipliers of homologous quantities, the constant is invariable despite varying strengths of stimulus, resistance invariable despite varying strengths of current. Invariable for the same sense but varying from some senses to others, the constant should be a specific-energy sense coefficient, but peripheral or cortical? Sense-organ activity varying *pari passu* with stimulus, the constant not so varying, can hardly be peripheral (anyway, unless the 1:1 relation between energy-radiation in environment and heat-generation in cortex be tossed aside, the true constant is the square root of Fechner's). But, if a cortical coefficient, invariability of constant within the same, and its varying on change from some sense-realms to others, is intelligible as indicative of equivalence of dynamicity among cells of the same realm, and difference of dynamicity between cells of different realms, which

---

[1] .0009477 C^2rt British thermal units, C being current (amperes); r, resistance (Ohms); t, time (seconds). Varying inversely as cube of radius of conductor, the heat generated in neurones and neuro-fibrils can be no negligible quantity. Its destiny can only be *work*, physical or chemical, probably both in different proportion in different cases and species.

accords with experience that while stimulation of one sense is never appreciated in psychic terms belonging to another sense (visual impressions never heard), vicarious interchange may take place within the same realm in hysterical reversal of the retinal color fields (explicable as intoxication-alteration of resistances, entailing cross-deflection *en route* with ultimate projection of color fields on to unhabitual cells). The constant would, then, measure the resistance of the sensation zone.

Did Fechner's logarithmic function formulate the quantitative relation between psychic and physical worlds, consciousness could not be compared with energy, no form of which interchanges with others on a logarithmic basis of equivalence. But the function seems intra-psychic; an orientation of judgment acquired through estimation of distance and speed relations of moving objects. If an animal perceive an enemy approaching from 200 meters distance, successive fractional distances traversed and distance-remainders yet to be traversed, are continually being estimated. Estimated how? Through intercomparison of sensation-vividnesses varying in dependent relation with shifting energy-impingement values incident to approach or recession in line of sight. Such intercomparison may be either of absolute sensation differences, or of the proportionate increase (or decrease) between one sensation and its predecessor. Supposing now, sensation-vividness correlated increment for increment with cortical heat, the latter equalling total energy-impingement on body surface, from a series of ascertained sensation-vividnesses we can calculate the distances whence the respective impingements were radiated[1] and thus test the comparative accuracy of the two modes of comparison. That scale would be best in which sensation-differences are proportional to distances traversed. But energy-impingements being as the inverse squares of distance-*remainders*, environmental conditions preclude *any* scale from being so proportional. Whence this most instructive case of two-story adaptation, where a series of judgment units is superimposed upon and sifts out the series of sensation units; proving that mental parallelism of environment exists not for sake of such parallelism, but because in general parallelism corresponds to maximum adaptation.

The table shows that at 200—199, for both series, the sensation-difference is 0.01, the "just noticeable difference" for vision; whence, 200 marks the diverging point of the two series.

---

[1]If with 200 meters distance, impingement (i) $= 1$, for any point, p, distant less than 200, $i = \left(\frac{200}{p}\right)^2$, and $p = \frac{200}{\sqrt{i}}$

Comparison of sensation-differences and distances traversed.

BOTH SERIES		COMPARISON BY EQUAL SENSATION-INCREMENTS OF 0.01			COMPARISON BY LOGARITHMIC INTERVALS		
Number of term	Distances traversed should be	Distance-remainders (p)	Distances traversed ($200-p$)	Separate Comparisons per meter	Distance-remainders (p)	Distances traversed ($200-p$)	Separate comparison per meter
41	40	169.03	30.97	—	163.92	36.08	—
81	80	149.07	50.93	2	134.35	65.65	—
121	120	134.84	65.16	2	110.11	89.89	—
161	160	124.04	75.96	4	90.246	109.754	2
201	200	115.47	84.53	5	73.965	126.035	—
241		108.46	91.54	6	60.621	139.379	3
281		102.6	97.4	—	49.685	150.315	4
321		97.59	102.41	8	40.722	159.278	—
361		93.25	106.75	9	33.375	166.625	6
401		89.444	110.556	10	27.354	172.646	—
441		86.068	113.932	12	22.42	177.58	8
481		83.046	116.954	13	18.375	181.625	10
521		80.323	119.677	15	15.06	184.94	12
561		77.85	122.15	16	12.343	187.657	15
601		75.593	124.407	18	10.116	189.884	18
1001		60.303	139.697	26			
5001		28.006	171.994	125			
10001		20.	180.	625			
40001		10.	190.	3000			

Clearly only organisms can survive in which, where complicated distance judgments (animal, enemy, or both, moving) are in question, sensations do *not* enter for their face values into, but find *some* reactive coefficient of correction in, the composition of forces. For, the sensation-increment does not even roughly approximate distance traveled; the sensation difference per meter differs widely in different portions of the line of approach (200—199, 0.01, 20—19, 10.8); and the number of separate comparisons is prohibitory. Further, as no animal could continually bear in mind the initial 200—199 increment as a referendum, intercomparison must come to be with *some* later term. But the later term, comparison with which involves greatest accuracy, is the next preceding. Such comparison cannot, however, be on the increment basis, the distance-value of the increment varying too widely in different portions of the line of approach (200—199, 1 meter for 0.01; 20—19, only 0.001 for 0.01). Consequently, intercomparison must tend toward proportionality; toward the logarithmic. But such continual referendum to the next preceding sensation-value as a basis applies to logarithmic comparison exactly the corrective required to make it closely approximate to distance traveled, as that referendum makes that term the initial member of a new series with corrected base. The logarithmic series is seen to cover the whole line of approach with a series of sensation-differences not accurately proportional to distance traveled, but near enough for practical purposes; the sensation-differences vary far less widely in different portions of that line; and the number of separate comparisons is practicable. But judgment being in animals more exercised on distance and speed relations of moving objects (enemies, prey) than on all else together, all discrimination of sensation will come to be similarly oriented; toward logarithmic comparison. But such discrimination will not continually be attained by re-decision, but consciousness will lapse to the optimum limen, the "just noticeable difference," the *judgment* unit, not the sensation unit.

Meeting this biological necessity are composite-photograph imprintations in the plasticity zone (all the "learning" machinery) ensuing upon the innumerable trial-and-error experiences; (and, *de facto*, the young often exhibit imperfection in distance judgment) reinforced by elimination of the unready and inexact, and eventuating in a race in which judgment approximates the logarithmic type.

But if this be accepted as good and sufficient explanation of Fechner's law, then, successful explanation of that law hitherto mysterious, reflects retrospective lustre on the hypotheses which enabled such explanation,—that nerve energy is electricity, and that sensation is correlated increment for increment with cortical heat.

What does logarithmic proportionality mean dynamically; what neuromolecular conditions operate to magnify the small sensation-increments into the larger hiatuses of the J. N. D.? Unless I give up all won so far, or assume a psychic-cortical correlation for the plasticity zone different from that assumed for the sensation area, I must reason: Discontinuity in sensation-discrimination means discontinuity in plasticity-zone heat-generation; but with C^2 and r increasing subcontinuously, the heat increases by small increments (retaining visual discrimination as the example, a, $1.001a$, $1.002a$, . . . $1.009a$); and while the relation r not $> \frac{1}{C^2}$ must *somehow* obtain in order that neurone A shall yield sensation *a* upon impingement *a*, nevertheless r and C being independent variables, appeal must lie to discontinuity-elements in the substrate.

Hypothesis. Sensation-discriminating neurones (or neurone-grouplets) of the plasticity zone constitute a step-by-step graded series, A, B, C, ... Z, each neurone of which can rob (via collateral afflux consequent upon preponderant resistance-reduction) the territories of the other neurones according to circumstances and within narrow limits; for current values making $C^2r =$ successively to a, $1.001a$, $1.002a$, . . . $1.009a$, neurone A holds its own as against B, but the value $a\ (1+.01)$ reached, B robs A, but holds its own as against C so long as the value $a\ (1+.01)^2$ is not reached, at which point C becomes able to rob B (also all other neurones). Motor connections of constituent neurones are peremptory in proportion to the robbing power of such neurones. Each neurone consists of molecules specialized (via optimum lapse and Preponderant Survival) in such manner that r not $> \frac{1}{C^2}$, but r and C being independent variables, that limitation of value cannot come about through purely physical causes, but for neurone A (specialized for sensation a), supplementary heat-values $1.001a$, $1.002a$, . . . $1.009a$, *some chemical* alteration (presumably slight isomeric) must occur involving absorption in intra-molecular work.

This explains the disproportionate "attention" and reaction to strong stimuli. Also, it accords with the biological advantage of mental shock in arousing attention (meaning dynamically, promptness in robbing other previously active tracts, thereby turning nerve energy and consciousness in the newly needed direction quickly).

Inheritability of "learning" structure. Sensation-discrimination thus seems a matter of trial-and-error ("learning") adaptation, reinforced by "selection" among carbon molecules differing in initial resistance and capacity for ultimate modification; in plasticity, that is. Romanes believed the question of inheritability of acquired characters would be settled in the psychic realm, if anywhere. But there seems no necessity for assuming such inheritability in that realm; and the Baldwin-Osborne-Morgan "factor," the subtlest device for getting something out of nothing in matters psychic I have ever met, is superfluous. The "factor" is (1) in each successive generation the young learn anew a survival-requisite special artifice the learning of which saved the parents to the breeding period; until such time as (2) variations in the same direction appear in the germ. But *can function outrun structure?* Bodily structure, yes; plastic structure, no. "Learning" *does not come from nothing;* learning capacity is imprintation capacity which is a dependent function of the plasticity interrelation, the relation between initial resistance and ultimate modifiability. But equally with other brain structure, plastic molecules present in the parents can, on *any* theory of heredity, reappear in ("be transmitted to") and vary in the offspring which, as compared with the parents as regards degree of perfection of the plasticity interrelation, may be on a par with, inferior to, or superior to them. Pending contrary proof, however, these differences are variations strictly comparable with other variations ("spontaneous," *de facto*, and from unknown causes). But the "acquired" imprintation-improvements of the parents cannot, so it is said, be "transmitted." Waiving discussion of what I know naught about, here at least it is not necessary that they should be; for "transmission" of learning-ready machinery entails learning-readiness of offspring on occasion (the "variations in the same direction" are *already* "in the germ"). Thus incidentally and in a manner the reverse of the Baldwin hypothesis, the young of ready-learners of the artifice, re-learn it, thereby surviving in largest percentage to the breeding period and leaving largest percentage of offspring. And obviously the limit of the process is a readiest-learning race, eventuating in one which is *pre*-ready, not needing imprintation-improvement of the plasticity interrelation, one which is able to execute appropriate motor response the first time occasion demands. Several degrees in the approximation-series can be seen in young animals of different species.

Consciousness. The logarithmic function relegated to the distal side of the sensation-thought boundary, no objection remains to translating the "rigid correspondence" of page 63, into: Correlation increment for increment; and consciousness and the cortical heat-like energy fall, dynamically, within the limits of one kinetic species, the name of which may as well be consciousness as anything else. And Penultimate Philosophy will say: Consciousness is the dynamic factor (the cognitive integral) in "profiting by experience." The consciousness wave-series probably adjoins (or more or less overlaps) the lowest of the heat series, extending thence indefinitely downwards. This position is, dynamically, most striking as in keeping with the character of consciousness as a *compromise* energy; one (perhaps the only one) into which can be directly and readily compositioned all the diverse energy-types impinging upon organisms,—the molar-motion type (all mechanical impacts including olfactory); aerial sound pulses; the several etheric-vibration series. Conveniently analyzable as dynamic- and psychic-inseparable, it may well be that "inseparable'' should be *identical*. For, there being no antecedent indication as to what properties waves referable to hitherto unknown portions of the spectrum should have, awareness may come to be accepted as *de facto* inherent in these particular waves, as is expansion-induction in those of the termic series, and silver precipitation in the ultra-violet, no one property being any *more* unintelligible than another.

B. Compositional

Recapitulating, every moment innumerable waves initiated by sensory stimulation are surging up through tier after tier of afferent neurones. At every successive millimeter of the upward progress, the waves are divisible into two classes,—waves for which antecedently permeable routes exist in the transverse sensori-motor connections at that level, and waves for which the antecedently permeable route is upward. The waves making up the progressively diminishing residual of the second class come ultimately to be deflected into the sensation area, where increased resistance involves friction with consequent latentization of nerve energy beyond the limen point, whence emergence of sensation, followed by the passing on of the waves, space-representatively proportioned.

Like other nerve action, plasticity-zone action is merely repetition of what it has been in the past, with two differences,—differences the outcome of past imprintations, and differences due to variations in ''functional condition'' (intoxication, including fatigue). And it is just because such differences cannot be excluded, that the sketch to be given will depart from our actual experience of mental action. But now the constants in nerve action are under analysis. Plasticity-zone action falls naturally under: Route, and the three-faced indecomposable fundamental fusion-composition-reorientation process, the distinctive feature of such action.

Route. This is strictly a dependent function of the several resistances, and (variations in functional conditions apart) these are strictly such function of the imprintation conditions. Thus is explained how the ''experiences'' of the individual, though past, can still enter, as a present factor in reactions. For, route incidentally determines which among the myriad host of cortical cells shall be impinged upon and stimulated. Route thus determines *kind* of nerve energy liberated (so to speak). But imprintation conditions have nothing to do with the *amount* of nerve energy liberated, that amount being exclusively an outcome of accumulations via Preponderant Survival, and in present reactions varies *pari passu* with different intensities of the same stimulus.

Fusion. Fusion-focus:—Sufficient gradient accumulated, the sensation waves penetrate the plasticity zone, everywhere along the antecedently permeable routes. Penetrate how far? Till they can penetrate no farther; till they impinge in the abutment area of a fusion focus, a rheostatic *cul-de-sac*, exit from which is impossible for the waves *as such*, and possible only for their fusion wave. How could such a "focus" come about? Take the most difficult case possible (rather impossible). Suppose a system of neurones, all unimprinted and all equally plastic, every neurone forming terminal arborizations round the cell of every other neurone; neurone A arborizing round B, C, D, etc.; neurone B round A, C, D, etc.; neurone C round A, B, D, etc. Then A arborizes round B and B arborizes round A, A round C and C round A, A round D and D round A, B round C and C round B, etc. In such a system, impinging energies will start waves radiating in all directions; stimulation of A, *e. g.*, will be followed by radiations out to all its terminal arborizations. But stimulation of the circum-arborized neurones (B, C, D, etc.), will start secondary waves radiating out from each such neurone to all its arborizations, thereby stimulating all those circum-arborized neurones to radiate tertiary waves,—till waves of higher order fail to evoke further energy liberation. But all those waves imprint; and some environmental happenings recurring more frequently than others, certain stimulus waves recur in particular combinations, correspondingly more frequently; whence, inevitable inequality of imprintation conditions. For, in the neurone system supposed, with incoming waves corresponding to the ingredient sensations, *a*, *b*, *c*, of a concept, *abc*, impinging on neurones A, B, C, the routes radiating out from all the neurones of the system will fall as regards the type of the resulting imprintation, into three classes,—*intra-focal* (A-B, B-A, A-C, C-A, A-D, D-A, B-C, C-B, etc.); *exo-focal* (A-E, B-E, etc.); and *extra focal* (E-F, F-E, E-G, etc.). And the imprintation types will be:

ROUTE A-B	ROUTE B-A
1. Primary waves generated in cell A under stimulation of incoming sensation waves (of A type).	Primary waves generated in cell B under incoming sensation waves (of B type).
2. Secondary waves generated in cell A under stimulation of B-A primary waves.	Secondary waves generated in cell B under stimulation of A-B primary waves.
3. Tertiary waves generated in cell A under stimulation of B-A secondary waves.	Tertiary waves generated in cell B under stimulation of A-B secondary waves.
Etc.	Etc.

And what takes place along routes A-B and B-A takes place equally along other intra-focal routes. Very different is the imprintation along the exo-focal, and still more along the extra-focal routes.

ROUTE A-E	ROUTE E-A
1. Primary waves generated in cell A under stimulation of incoming sensation waves.	No primary waves.
No secondary waves.	Secondary waves generated in cell E under stimulation of A-E primary waves.
Tertiary waves generated in cell A under stimulation of E-A secondary waves.	No tertiary waves.
No quaternary waves.	Quaternary waves generated in cell E under stimulation of A-E tertiary waves.

Consequently, the routes interconnecting A, B, C, and D, being each time more frequently and more intensely imprinted, will with every new advent of the particular stimulus-combination, tend more and more to routes of lessened resistance, as compared with the exo-focal routes; whence, diffusion will ever be more limited to the forming focus, thereby accentuating the difference between the intra- and exo-focal resistances; leading to stricter limitation to the focus, etc. The depth of such imprintation and super-imprintation will depend both upon frequency of recurrence and degree of plasticity; and as the process in question involves the only element of survival value in plasticity-zone action, variations in plasticity will accumulate via Preponderant Survival.

A fusion focus is a physiological rather than an anatomical entity, and denotes merely a number of neurones which in the presence of certain particular sensation waves, act together, but any or all of which neurones may, in presence of other sensation waves, act in connection with any extra-focal neurones to form other similar foci. Consequently, the form and size of such foci may be anything; the focus may consist of closely contiguous neurones, or of neurones separated by the length and breadth of the plasticity zone.

Fusion :—The intra-focal routes are thus imprinted by primary waves, and super-imprinted by waves of higher order. The latter, generated in the same cells under stimulation of different character, may be assumed (pending contrary proof or emergence of discrepancy) to differ in *some* respect from the corresponding primary waves.[1] But, if they differ at all, fusion seems certain. For, the plastic carbon molecules of route A-B, would, if imprinted exclusively and repeatedly by primary A waves, attain to an orbit form vibrating in unison with those waves. The subsequent action of a different wave type upon an orbit form so modified would tend to modify it in another direction, that leading to an orbit form vibrating in unison with those second waves. As, however, the molecules are not subjected exclusively and repeatedly to the action of the second waves, but are subjected alternatively and repeatedly to the action of the first and second waves, the only possible outcome, these waves being different, is the emergence of an orbit form vibrating in closest harmony with both the remoulding waves, one whose proper note (so to speak) is an harmonic of both their vibration rates.

Waves vibrating in unison with an orbit form will be transmitted without change. But where their vibration rate differs from that proper to the orbit form, more or less interference with consequent absorption will ensue. With absorption, however, heat will emerge and modify the orbit form toward unison (or closest harmony) of vibration rates. Except for unison waves, then, every transmission will be the outcome of reciprocal readjustment (of a vibrational composition, so to speak), orbit form modifying wave, wave modifying orbit form; the amount of change undergone by orbit form and wave, respectively, differing in different cases. But with every additional imprintation and super-imprintation, the proportion which a single new imprintation bears to the sum total of all past imprintations and super-imprintations is continually diminuendo. Consequently, the effect of such new imprintation upon an orbit form once established is likewise continually diminuendo; or the effect of orbit form on wave is continually crescendo. Whence, waves impinging on orbit forms perfectly attuned, either, if too remote in vibration rate (too dissonant) are not transmitted at all, or, if harmonically reducible, undergo absorption sufficient to admit of their vibration in harmony with the orbit form become relatively inelastic (or elastic within comparatively narrow limits) through repeated modification in one direction.

---

[1]Parenthetically, the orbit form of route A-B, being imprinted by the more intense primary waves, and super-imprinted only by the less intense waves of higher order, will, though modified to an harmonic ratio of B, remain fundamentally A in type; meaning that its vibration rate will approximate to unison with that of A type, but only to harmonic ratio to that of B type. Conversely, the orbit form of route B-A, will be fundamentally B in type, and only harmonic to A. And what takes place along routes A-B, B-A, takes place equally along other intra-focal routes.

But potential raised in the abutment area, discharge must occur somewhere; either the immediate final discharge from the focus, or a mediate discharge on to other intra-focal neurones. On the second alternative (as the more difficult case), discharge will be on to more or fewer neurones (discharge on to the same number only postpones the question). If it be said that discharge from the abutment area is primarily on to more neurones, and that reconcentration (discharge on to fewer neurones) first begins later, it comes to the same thing, the contention merely changing the question to this: Is the abutment area the place where for these particular waves, the principle of the increasing complication of paths reaches its limit, or does that principle first reach its limit in some post-abutment (but still intra-focal) area? The abutment area denoting the last point to which the waves can without change penetrate the plasticity zone, the question lacks standing ground unless it mean contention that harmonic reduction confers further penetrating power, a contention unimportant concerning which no presumption arises, pro or con. *The* question lies between:

Hypothesis. Discharge from the focus occurs with ever increasing diffusion, no reconcentration on to fewer neurones taking place.

Hypothesis. Discharge from the focus occurs via reconcentration on to fewer neurones.

On the first alternative, energy existent in the abutment area would be dissipated correspondingly to the diffusion; whence, its psychic-inseparable would dwindle in vividness, all the time persisting *in jorma* sensation. But we observe the energy concentrate to reaction, and the sensations integrate to a concept; whence a presumption in favor of the second alternative, which, besides, corresponds with what obtains for the plasticity zone as a whole, where discharge via fewer neurones is demonstrable anatomically in the diminishing number as we pass down the motor side, and marked psychically by concentration of "deliberation" into "will" and "effort." Since, then, the law for the part cannot be presumed different from that for the whole, discharge via fewer neurones is legitimate hypothesis. But such discharge means simultaneous impinging of two (or more) different sensation waves, reduced to harmonic ratios, upon the same neurone. As, however, its orbit form cannot, at one and the same time, vibrate in unison with each wave separately, the only possibility will be the assumption by it of a vibration rate to which the rates of both (or all) the waves can be assimilated; a rate which includes all rates, a common (and inferentially the least common) multiple of the separate rates,—which means vibrational composition to a fusion wave.

Composition of forces. This is involved in (is only another side of) fusion. Unless the conservation of energy be repudiated, the amplitude of the fusion wave must be assumed to represent the algebraic sum of the intensities of the components; and that of the super-fusion wave to correspond to such sum of all the waves undergoing super-fusion.

In reaction to these incoming sensation waves, and more especially to their fusion (super-fusion, etc.) wave, the plasticity-zone cells liberate a vast amount of energy, an amount the liberation of which in the past sufficed, *de facto*, to ensure motor response adequate to the exigency announced or presaged by the stimulus. But (variations in "functional condition" always apart) being compounded and super-compounded from equals, the final resultant will equal the predecessor resultant, whence exit having been possible for it, will equally be possible for the present resultant along the route imprinted by its predecessor in its exit. Consequently, discharge will ensue on to those motor mechanisms on to which that predecessor discharged, and discharge on to which sufficed to preserve the organism in former similar emergency, or to preserve its ancestors to the breeding period.

In different cases, the energies contributed by the organism bear very different proportions to the incoming energies; the former may predominate or be insignificant. The *biological* resultant (that of the composition of *all* the forces) will, then, be incalculable, not because it departs (as far, at least, as there is any evidence) from the path of mechanical exactitude, but incalculable by reason of our ignorance of the values to be assigned to the intra-organismal forces in any given case. Especially will these forces differ widely in amount, since each organism reacts not to the immediate happening announced by the stimulus, but to its coming sequels; to sequels which are members of œcological series which though having the initial term in common, nevertheless differ for every species or other zoölogical group.

Superfusion, Supercomposition. Fusion-composition taken place, the fusion wave following the exit route imprinted by its predecessor will undergo either final discharge from the plasticity zone, or reach some limit within that zone progress beyond which is impossible for it as such wave, another abutment area round which another (superfusion) focus will build up. The latter (more frequent) case underlies all those congruous serializations of associations constituting "trains of thought." In the new focus, fusion-composition is repeated,—and so on, till final discharge on to the motor mechanisms.

Reorientation. Is incidental to fusion-composition, the direction of exit of the fusion wave being given between its vibration rate and the imprintation conditions along the exofocal routes. The essence, dynamically, of reorientation, lies in redistributions in the kinetic field, the outcome of which is discharge on to motor mechanisms whose action saved in similar emergency. Reorientation and reaction are thus merely consecutive phases in one process, of which psychogeny is but another aspect.

But why do mechanisms already acting give over their activity upon the advent of stimuli arousing activity in other mechanisms, "attention" coincidently moving over? No organism can at the same moment "compose" (attend to, deliberate and decide upon) all the inthronging stimuli annunciative of happenings at that moment (*de facto* attention to plurality of subjects is difficultly acquired and is unknown among animals). Consequently, those organisms only can oppose to happenings appropriate response, which have accumulated selective receiving capacity. Through biological necessity and Preponderant Survival, such capacity must come to be oriented parallel with the œcological-importance scale of happenings. What neurological condition meets this necessity? Graduation of the antecedent-permeability scale parallel with the œcological-importance scale. Being variable, selective receiving capacity cannot lie in the sensation area (the locus of constancy of relations), but must be a plasticity-zone or feeling-tract matter. In the plasticity zone, it is the outcome of the fundamental dynamic constitution of the nervous system and of the imprintations (except to the not small extent to which intoxication conditions affect zone action). Stimuli entering the zone in order of œcological importance, if stimulus *a* enter neurone A, it will (supposing the stimulus continue acting) continue to flow along such tract as long as resistance along that tract remains less than along tracts entered by later stimuli (*b*, *c*, *d*, *e*). For, with resistance along A relatively minimal (otherwise *a* would not have entered it in preference to other tracts) and further lessened by the conditions involved in functioning (resistance-reduction consequent upon heat of passage of current), energy will inflow on to tract A from all collateral points. But with the advent of stimulus *f* into tract F in which *f* encounters less resistance than does *a* in tract A, direction of energy flow will become reversed, and F will rob A. Energy will, however, flow in from points at which potential previously stood highest, from tract just active, where, consequently, consciousness was most vivid. But with such drafting off from those tracts, consciousness there will first pale and then vanish, while it will as steadily rise to a bright glow (so to speak) in the newly active area. Whence, we shall have just that shifting over from consciousness of the previous to that of the present subject, which we introspectively observe.

In the feeling tracts, pre-antecedent permeability is an outcome of graduation of intoxication conditions parallel with the œcological importance scale. Consequently, the supplanting of a former stimulus, by a subsequent one, is evidence of intoxication preparation (resistance-reduction).

C. *Concurrent-Compositional*

Some think mind originated in "some form of sentiency," a vague phrase for: Sensation plus feeling. With these ingredients, simply environed organisms would be in position to oppose to happenings appropriate response, since they would possess a space-orienting and a reaction-regulating (intensifying or minimizing) factor. Before the intrusion of the thought-wedge, reaction-sequences, *sine qua non* for existence, might come to be organized through adjustment of activity of the second factor, via accumulation of optimum gradations in intoxication conditions (the first factor, environmentally determined, is for every organism a fixed quantity and the point of departure). Neurologically, this means a vast number of neurones organized into a connected whole, with the resistances of the constituent neurones just such as to ensure optimum serial motor response. What would be the psychic condition along such tracts? Sensations and feelings being non-lapsing conscious states, the tracts should be conscious tracts. The gradual intrusion of the thought wedge will introduce comparatively little (though as we ascend the zoölogical scale, ever progressively more) modification; and that in the way of inhibitive regulation or control. For, the biological necessity remaining the same, unless motivation of the established reaction-sequence should come to pass over to the thought wedge, it will remain where originally organized. But the offices of feeling and of thought (rather of their dynamic-inseparables) differ radically. That of feeling is ensuring of certainty of performance of serially connected, and promptness of execution of suddenly necessary, reactions, thus contributing to survival in face of emergency. But the survival value of thought lies in the fore-warning accruing in connection with it, so long in advance of emergency, that there will be none. If the senses be arranged in order of the warning afforded of happenings, touch and taste afford warning only upon contact; smell and hearing cover a range of at most a few miles; sight, somewhat or much more; but the warning in connection with thought is without assignable limit. Which *means* this, and *no more*,—that the conditions involving emergence of thought are equally those involving "profiting by experience," involving boundless possibilities of resurrecting experiences (and experience-fragments in new juxtapositions and combinations), to extemporize a color-scale (so to speak) against which to match any incoming stimulus-complex, to the end that its ground tint being ascertained, its coming sequels may be "inferred" from the *de facto* sequels of its ground-tint match, long in advance of their advent. Thought is thus *the* prophetic sense.

Analysis thus reveals between sensation and motor function *two* bridges, a sensation-feeling-motor and a sensation-thought-motor bridge, differing in antiquity and in office. Otherwise put, besides the two recognized types of nerve action, reflex and conscious, a third type exists, reflex-though-conscious or pleasantness-unpleasantness action, which is in no sense intermediate between the other two, being altogether differently (namely, intoxication) motivated.

Feelings not lapsing, the underlying molecule type must be assumed elastic. Feelings being a dependent function of intoxication, the predisposing dynamic-inseparable must be assumed to originate through intoxication-induced metabolic processes. But to this *basis* dynamic-inseparable comes to be super-added, reinforcing dynamic-inseparable arising through latentization of nerve energy radiated in from the sensation area, entailing intensification beyond the limen point and emergence of feeling. Were feelings wholly the outcome of such latentization, however, they would be dependent functions of sensations and consequently (composition not occurring in elastic-molecule tracts) dependent functions of intensity of stimulus.

It now becomes intelligible why feeling and thought are independent variables, some exhibiting predominance of one ingredient, some of the other; also why in the same individual, intense thought and intense feeling alternate, rarely coincide. The intoxication substrate also explains the feeling variations classified on pp. 81-83. In salmon, *e. g.*, paralleling seasonal development of the reproductive organs, inshore movement begins, the initial term in a months'-long concatenated reaction-sequence, each member in which involves progress of the organism to a point where it will encounter the next supersessory stimulus. What share has intoxication? Given a sequence too long-lasting and complicated for successful execution via reflex action, the only way all available energy can remain concentrated for months within narrow channels, without fatigue, is through an elastic-molecule system maintained in constant reorientation by continuous intoxication-induced generation of dynamic-inseparable, whereby one series of tracts becomes *temporarily* (cyclically, seasonally) reflex, in constant pre-readiness for response to those stimuli which initiated in the parents motor responses, whose eventual outcome was successful spawning.

The only alternative is that the salmon "knows." What does this *mean;* to what commit one? That the salmon *thinks:* "perceiving my ovaries swell, I know my time approaches, I must turn inshore, enter the river, ascend to those gravelly stretches near the headwaters, there build my nest and spawn my as yet hardly begun eggs." Absurd! More important, *why* absurd; only by analyzing out justification for contemptuous repudiation can one advance toward precision of thought. Ascription to salmon of such thoughts is unwarranted because (1) only in the Columbuses of the human does thought attain to fore-triangulation of a mental chart whereby migration *per saltum* across a hiatus can be steered. Because (2) such thoughts involve appreciation of interrelation of organism and environment amounting to self-consciousness which is an outcome of introspective observation of the sequential relation between sensations and mental operations, and between those operations and motor (glandular, etc.) response,— which observation sums to recognition that, psychically, body surface represents the locus of all points in the universe from which originate initiators of psychic ingredients (sensations) which form the first members of series of which feelings and thoughts are subsequent members; and that body surface represents, coincidently, the locus of the limit points to which can be followed out the sequential relation of feelings and thoughts to motor response. More briefly, body surface represents the limit points of sensation and "will," equally whether the universe be mind-constructed or only mind-reflected. But then self-consciousness cannot antedate phylogenetically the rise of introspection, the only distinctively human psychic trait.[1] Be-

---

[1]Psychologists make far too little of introspection, James merely saying it is "mysterious." Introspection offers no special difficulty. Thought emerges whenever the relation between nerve energy and resistance comes to exceed the critical limen point, *irrespective of the source* of the instreaming energy. For every superfusion focus, that source is some other part of the plasticity zone. But the superfusion consciousness comes, *de facto*, to include appreciation of the sources and relations of the instreaming energies; to include, *as superfusion content*, the consciousnesses which those energies simultaneously arouse or immediately precedingly aroused. The consciousness of simultaneous or immediately precedent consciousness is thus inherent in superfusion; every superfusion focus is potentially introspective. But introspection involves two complementary conditions, the "power of volition," and *sufficient continuance* of the observed and observing consciousnesses. Neglecting the first condition as *flatus vocis*, so long as environmental (especially escape and food) needs press so continuously and insistently as to necessitate biologically (and conse-

cause (3) "attention" could not be maintained for months unwaveringly concentrated upon efficient performance of the details of the migration. And because (4) did the enormous amount of energy expended pass through the plasticity zone, imprintation on a phenomenal scale must occur. But in species migrating year after year, following cessation of the breeding intoxication, the individuals revert to seek-food reactions, contra-indicating phenomenal imprintation. In such species, too, and also where all the parents die before hatching, the young perform, untaught, the first migration as well as the old; whence here, profiting by experience (plasticity-zone action) does not enter. From the foregoing, then, migration cannot be thought-motivated.[1]

Allocation. Not lapsing, feelings cannot take rise in thought tracts; independent of intensity of stimulus, they cannot belong in the sensation area. Were feelings in *any* case "in the sensation," that case would be the preputial dermal sensation; yet even here, pleasure is not "in" the *sensation*, as the latter is *always* present, pleasure on preputial retraction present only sometimes (erotism) and *post-ejaculationem* pain is "in the sensation."

Whence, inferentially neither is in it, but sensations fall, as regards pleasantness-unpleasantness relations, into two groups, —sensations tributary, in general, to pleasantness tracts, and sensations tributary, in general, to unpleasantness tracts. And the abrupt shift-over *post-ejaculationem* from pleasurable to painful means de-intoxication alteration of resistances whereby sensation waves previously tributary to pleasantness tracts become tributary to unpleasantness tracts. The sensation is tributary during erotism to pleasantness tracts because such tracts are those involved in innervating evacuation (observable correlation between pleasure-increments and rhythmical pre-ejaculatory pulsations). Whence, pleasure fluctuates synchronously with the *motor* side.

Similarly, pain (a sensation, being equally vivid, however often experienced) is, in general, tributary to unpleasantness tracts, not because pain *qua* pain is necessarily unpleasant, but

---

quently through Preponderant Survival to entail) the preter-lability of orientation finding psychic expression in ultra-distractibility, such sufficient continuance remains impossible. But when plasticity-zone development attains a stage admitting of fore-calculation covering a range far enough in advance of happenings to involve *leisure* (pastoral, agricultural stages), integration of mind has reached the introspection-possible stage.

[1] When my "Habits of Fishes" (*Amer. Journ. Psychol.*, XIII, 1902, pp. 408-25) was written, I had not recognized the existence and importance of the intoxication factor. That factor complements and completes the explanation there given.

because being the specialized danger-signal sensation, it must, in accord with biological necessity, be tributary to the escape (drawback, run-away, etc.) tracts. Pain is not always unpleasant. In slight dental periostitis, while never losing the pain quality, it is associated with pleasurable (lustful) tingle; an exaggeration, through inflammatory over-sensitiveness, of the frequent association of biting with lust. In severe grades of the periostitis, both this tingle and pain-unpleasantness are commingled in different degrees.

The close connection between feelings and the efferent side appears from the striking prominence of motor elements in those complex pleasantness-unpleasantness states termed "emotion," and from the relief of pent-up emotion through its motor expression; from the culmination of sexual pleasure coincidently with a motor act (evacuation); and from coincidence of the pleasantness gamut with high efficiency, and of the unpleasantness gamut with inefficiency (fatigue, inflammation, etc.) of the motor apparatus.[1] Such close connection suggests allocation to the "motor area or special affluents of it," which affluents are the "feeling bridge." Like the fusion focus, this bridge is (at present, at least) rather a physiological than an anatomical entity; but unless the preceding analysis is to go for naught, to this bridge corresponds the bulk of the cortical sensori-motor connections in vertebrates whose hemispheres are limited to the oldest basal convolutions (hippocampus, etc.). If, then, feelings be regarded as consciousness-facies taking rise in neurone-systems in close connection with the *efferent* (particularly the ad-muscular) apparatus, and as facies whose predisposing condition of arousal is intoxication and whose office is intensification or minimization of motor (or other) response, we come near a theory of feeling.[2]

Dynamic interrelation of feeling and thought tracts. Can "feeling cause thoughts," in the unphilosophic argot of the day; meaning, can energy which in transit through feeling

---

[1]Likewise other bodily organs; most striking with the musculature, because its innervation is associated with consciousness.

[2]Feelings *may* be naught else than consciousness arising in the motor area itself, consciousness of height of potential in (state of efficiency of) that area. This position (distal, in the sensori-motor progress, to "attention") would accord with the elusiveness of feeling upon introspection. Feelings could then be thought of (enter the composition of forces) only as modification of thought-*content*, the outcome of repeated experiences of the relative ease or difficulty of inhibiting their motor expression; and, *de facto*, paralleling the biological necessity, we are able to gauge the character and force of the coming movement in advance. But, tentatively, it suffices if the connection of feeling with the *efferent* side be accepted.

tracts has aroused feeling, subsequently pass into the plasticity zone and arouse thoughts? Introspection warrants an affirmative; repeatedly deliberation is influenced and decision warped by feeling components. More convincing, feelings are remembered, and memory is explicable only as an imprintation phenomenon. Feelings are remembered, then, in terms of their association values.

Can energy coursing in the thought watershed overpass the divide into the feeling watershed ("thoughts cause feelings")? This question can first be answered after introspective restudies with due consideration of the problems here raised, particularly that of the paramount importance of intoxication processes. For myself, the more I have come to realize the wide range of that influence, the more I trace in cases, in which formerly I should have replied to the above question unhesitatingly in the affirmative, intoxication changes underlying and accounting for the newly appeared deflection of energy flow. Therefore, I suspect the future will say: Failing intoxication-induced lowering of resistance, the feeling tracts as a class have a relative higher resistance than the thought tracts as a class; whence, to the extent that intoxication does not enter as a resistance-reducing factor, energy entrant from the sensation area will trend predominantly into the thought tracts, and to the extent to which intoxication does so enter, energy so entrant will trend predominantly into the feeling tracts. And the relative extent to which that energy passes into the two classes of tracts has been determined through accumulation via Preponderant Survival,—in the former case of increments in the direction of optimum plasticity, in the latter of increments in the direction of optimum intoxication conditions.

III. INFRA-LIMINAL NEURONES[1]

The infra-liminal ad-cortical and ab-cortical neurones must consist of molecules offering:

Constant resistance.

Relatively low resistance.

Resistance must be constant, otherwise sensation could not be a rigid dependent function of intensity of stimulus (and space-representation would be impossible); and, on the downward side, co-ordination would be impossible. Resistance must be low, at least to the extent of always being infra-liminal.

There is no reason to think this molecule type the outcome of lapsed consciousness; rather it is apparently phylogenetically older than the molecule type of the plasticity zone.

---

[1] *Infra-liminal: Permanently* subliminal.

ON THREE TYPES OF BEHAVIOR

THE MECHANICAL, THE COERCITIVE (MAGIC) AND THE ANTHROPOPATHIC (INCLUDING RELIGION)[1]

By PROF. JAMES H. LEUBA, Bryn Mawr College

In his dealings with the different kinds of objects or forces with which he is, or thinks himself, in relation, man has developed three distinct types of behavior. A concrete illustration will bring them before us more forcibly than an abstract characterization. A stoker in the hold of a ship, throwing coal in the furnace, represents one of them. His purpose is to produce propelling energy. The amount of coal he shovels in, together with the air draught, the condition of the boiler and other factors of the same sort, determine, as he understands the matter, the velocity of the ship. The same man, playing cards of an evening, and having lost uninterruptedly for a long time, might get up and walk around the table backwards in order of change his luck. He would then illustrate a second mode of behavior. If a storm threatens to sink the ship, our stoker might be seen falling on his knees, lifting his hands to heaven, and addressing in passionate words an invisible being. These are the three differentiated kinds of responses he has learned to make, the three ways by which he endeavors to make use of the forces about him in his struggle for the preservation and the enrichment of life. We may designate them as—

1. The mechanical behavior.
2. The coercitive behavior, or Magic.
3. The anthropopathic behavior, which includes Religion.

The mechanical behavior differs from the anthropopathic by the absence of any reference to personal beings. In the sphere in which it obtains, threats and presents are equally ineffective. It implies instead the practical—not the theoretical—recognition of a fairly definite and constant quantitative relation between cause and effect. If science is to be provided with an ancestor, and only with one, it should be this first type of behavior rather than Magic. For the moment the existence of the fixed quantitative relations, implicitly acknowl-

[1] A portion of a little book "The Psychological Origin of Religion", soon to be published by Archibald Constable & Co.

edged in the first type of behavior, is explicitly recognized, science is born. Magic separates itself, on the one hand, from the mechanical behavior by the absence of implied quantitative relations, and, on the other hand, from anthropopathic behavior, by the failure to use means of personal influence; punishment and reward are just as foreign to Magic as to mechanical behavior. As to the anthropopathic type of activity, it includes the ordinary relations of men with men as well as those with gods. One's frame of mind and behavior when dealing with a human person, especially if exalted far above us, resembles Religion so closely that it is proper to place them in the same class.

Mechanical behavior and Religion are, obviously, by far the most common and important modes of activity among civilized peoples, whereas in primitive culture the coercitive behavior (Magic) is everywhere in evidence and Religion may be practically unknown. As one ascends from the lowest stages of culture, Magic gradually loses official recognition. Among us, though it leads only a surreptitious existence, it has by no means lost all influence. The list of magical superstitions that have retained a hold among us would be found tediously long. A numerous class of them includes the gambler's methods of securing luck. So-called 'religious' practices may really be magical. The cross, the rosary, relics, and other accessories of Religion, acquire in the mind of many Christians a power of the coercitive type; that is, for instance, the case when the sign of the cross, of itself, without the mediation of God or Saint, is felt to have power; or when 'saying one's beads' is held to possess a curative virtue of the kind ascribed to sacred relics by the superstitious. And even when the symbolism of the sign of the cross, and the meaning of *Ave Maria* are realized, it happens not infrequently that signing oneself and saying one's beads are regarded as acting upon the Virgin Mary, Jesus Christ, or God, in the manner of incantation, *i. e.*, magically.

It has been the habit of most students of the origin of Religion to concern themselves exclusively with the origin of the god-idea, as if the belief in the existence of gods was identical with Religion. They have ignored its other essential components: the motives or desires and the feelings, as well as the means by which, in Religion, the gratification of desire is sought. But the limitation of the problem of origin to that of the god-idea is not entirely amiss. For there are neither specifically religious motives, nor specifically religious feelings. Any and every human need and longing may, at some stage or other, become a spring of Religion, and conversely the feelings and emotions met with in any form of Religion appear also in non-religious experience. As to the practical means of securing the favor of the gods, it is agreed that they were at the beginning essentially the same as those men were already in the habit of using in their relations with their fellow-men. It is the Agent or the Power with which man thinks himself in relation, and through whom he endeavors to secure the gratification of his desires, which alone is distinctive of religious life. And so the origin of the idea of gods, though not identical with the origin of Religion, is at any rate its central problem.

In the preceding remarks, as also in practically all writings on the origin of Religion, it is assumed that the god-concept precedes, in the mind of man, the establishment of Religion. This opinion is, as we shall see, the correct one. But it cannot be taken as a matter of course. Actions may become established in other ways. Our first problem is to discover how Religion arose, and what psychological capacities and conceptions it implies.

A comparative study of the three modes of behavior is, after all, the shortest way of gaining a satisfactory understanding of the origin of Religion.

What are the abstract conceptions necessary to the establishment of the three modes of behavior?—There is usually little difficulty in determining what end any particular action is intended to secure. It is quite otherwise if one wishes to ascertain the nature of the power from which the desired effect is supposed to proceed. The philosopher, suffering from the illusion to which his class is subject, is in danger of imagining the presence of highly abstract notions where much simpler mental processes actually take place. A comparatively easy way of getting oneself disentangled from these high-flown interpretations and of ascertaining what is the intellectual minimum really involved in these types of behavior, is to examine them in the least developed men known to us, or, better still—if they are to be found there—among animals. Let us accordingly turn for a moment to animal behavior with the intention of determining what ideas of power, or of agency, are involved in their modes of action, and thus take a preliminary step towards the solution of our problem.

Apes, dogs, beavers, in fact all the higher animals, show by their behavior a 'working understanding' of the more common physical forces. They estimate weight, resistance, heat, distance, etc., and adapt their actions more or less exactly to these factors when climbing, swinging at the end of boughs, breaking, carrying, etc. I remember observing a chimpanzee trying to recover a stick which had fallen through the bars of his cage and rolled beyond the reach of his arm. He looked around, walked deliberately to the corner of the cage picked up a piece of burlap, and threw the end of it over the stick. Then, pulling gently, he made the stick roll until near enough for him to get hold of it with his hand. This ape dealt successfully with physical forces. Towards animals and men, animal behavior is quite different. A dog will beg from a man; he will not beg from a ham suspended out of his reach. Towards animals and men, animal behavior is similar to that of men when dealing with invisible anthropopathic beings.

One may well believe that the inner experiences of animals differ in these modes of behavior as much as their external movements. The feelings and emotions which appear in a dog's intercourse with his master are of the same species, if not of the same variety, as those felt by man when he deals with his fellow-men and with superhuman beings. Certain highly gifted animals feel blame and approbation, independently of physical punishment or reward, and attach themselves to their masters with a devoted affection possessing all the marks of altruism. The higher animals do, then, without any doubt, practise both the mechanical and the anthropopathic types of behavior, but they exercise the latter only towards *actually present* persons or animals. We shall have to consider subsequently the significant psychological difference to which this fact points.

But, is there no trace in animal life of the coercitive behavior? I know of none, though some perplexity might be caused by certain reactions animals learn under the tuition of man. What shall be said, for instance, of a dog who has learned to raise its forepaws when he wishes to be liberated from confinement under circumstances making the person causing the door to open invisible to him? Is this magical behavior? There is certainly no quantitative nor any qualiitative relation between lifting up the forepaws and the opening of a door, neither is there any visible continuity between cause and effect. That the dog's action is not determined, in this instance, in the same way as that of a magician, appears when it is observed that whereas the latter would perform the same magical rite in a great variety of external circumstances, the dog will seek liberation by lifting its paws only when in the particular cage in which he has learned the trick, or in one very much like it.[1] But more about this pres-

[1] H. B. Davis has this to say on the power of generalization of the raccoon, a very intelligent animal: 'When an animal [raccoon] is forced to approach a new fastening from a new direction, it is often as much bothered by it as by a new fastening. Nevertheless, in course of time the animals seem to reach a sort of generalized manner of procedure which enables them to deal more promptly with any new fastening (not too different from others of their experience).' 'The Raccoon: A Study in Animal Intelligence,' *Amer. Jr. of Psy.*, Oct., 1907, p. 486.

ently. It is not to be overlooked that without the interference of man, the dog would never have learned to perform this quasi-magical trick. This illustration serves, if no other purpose, at least to indicate how apparantly slight is the impediment which prevents the higher animals from setting up a magical art.

It may be a matter for astonishment that two complicated and effective modes of reaction are arrived at by animals iu the absence of abstract ideas about forces. Yet, so it is; before any speculation on power, before any induction or deduction, before any abstract notion of the nature of spirit and matter, animals have learned to deal quite well with what we call physical and personal forces. How did they do it? The study under experimental conditions of the establishment of new reactions in animals reveals the process very clearly. Imagine a cat shut up in a box, the door of which can be opened by pressing down a latch. When weary of confinement the cat begins to claw, pull, and bite, here, there, and everywhere. After half an hour, or an hour of this purposive, but unreasoned, activity, he chances to put his paw upon the latch and escapes. If again put into the cage, he does not seem to know any better than before how to proceed, Yet, something has been gained by the first experience. For now he directs his clawing, pulling, and biting more frequently towards the part of the cage occupied by the latch. Because of this improvement he finds himself released sooner than the first time. The repetition of the experiment shows the cat learning to bring his movements to bear more and more exclusively upon the door or its immediate surroundings. Ultimately he will have learned to make just the necessary movement and no other. In this gradual exclusion of useless movements, the cat is guided entirely by results. The psycho-physiological endowment required for acquisitions of this kind involves no abstract ideas but (1) the desire to escape; (2) the impulse and ability to perform the various movements we have named; (3) an indefinite rememberance of the positon occupied when success was achieved, combined with a tendency to repeat the same movements when in the same situation.

The method illustrated above by which animals learn to deal with forces in the midst of which they live has a much wider range of application in human existence than is generally supposed. Man's fundamental mode of learning is also the unreflective, experimental, one in which frequent blind attempts and chance successes slowly lead to the elimination of ineffective movements. Would you convince yourself of the vastly exaggerated rôle ascribed to abstract ideas and to logical processes in ordinary human behavior, inquire how 'power' is conceived of by those who use it. What is in the mind of the stoker when he thinks of the power of coal? What is in the mind of the gambler when he tries to coerce fate? What in the mind of the necromancer when he summons the shades of spirits? Nothing definite beyond a knowledge of what is to be done in order to secure the desired results and the anticipation of these results themselves. The stoker thinks of what he sees and feels: the coal, in burning, gives heat; the heat makes the water boil; the steam pushes the piston-rod, and so forth. Each one of the successive links in the chain is vaguely thought of by him as striving to bring about the following one. That is how he understands the coal-power. And what does the ordinary person know, for instance, about electricity? Simply what is to be done in order to start the dynamo, light the lamp, switch the current, and what the effect will be in each case, nothing more. The superstitious person, whether belonging to a primitive tribe or to the Anglo-Saxon civilization of the twentieth century, understands in no other than this practical way the forces he deals with. I remember the delight shown by an elderly lady when a brood of swallows fell down our sitting-room chimney. 'It will bring luck to the household,' said she. I did my best, patiently and in several ways, to ascertain the sort of notion the lady had regarding the nature of the power that was to bring about the fortunate events predicted, and also to discover her idea of the connection existing between the fall of the swallows and the exertion of the 'power' in our behalf. I had to come to the conclusion that there was no idea whatsoever in her mind beyond those expressed by 'swallows-down-the-chimney' and 'happy-events-coming.' These two ideas were in her mind directly associated. When I declared my inability to see the causal connection between the two, she complained of my abnormal critical sense! Nothing more than the immediate association of an antecedent with its consequent need be looked for in the mind of most civilized, superstitious persons, and, of course, nothing more in the mind of a savage. That is sufficient for practical purposes.

The words 'matter' and 'spirit' wield a very considerable influence among us; what do they mean to most of those who use them? Physical science ascribes either extension alone, or extension and weight, to physical substances. Non-material forces are, then, according to science, both spaceless and weightless. I will venture to affirm that not one educated person in a thousand is acquainted with this distinction. Most of the few who have known it have forgotten it. So that the words 'matter' and 'spirit' mean different things to the philosopher and to the layman. In the popular mind, if spirits are not perceptible it is because the senses are not sufficiently acute. Spirits are here or there, diffused over wide areas or concentrated in narrow spaces. The average Christian, whatever he may say to the contrary, is, theoretically speaking, a materialist, and, I might add, a polytheist. Whatever matter and spirit mean to him, and they certainly have a substantial meaning, the distinction made by the philosopher is for him non-existent. The following facts may be of some interest in this connection. A few years ago, in a conversation with a shop-clerk, I happened to mention a lead coffin made hermetic with solder. He was shocked, and objected to a dead body being shut up in a coffin of that description because it prevented the escape of the soul. This man had had an ordinary grammar-school education. Here are two quotations taken from answers of American College students to questions requesting a description of their idea of God. It should be added that the questions were given only to classes which had not yet taken up, or were just beginning the study of philosophy. 'God, to me, is a being of flesh and blood, for without this form he would seem unnatual and unsympathetic as our leader.' (Female, twenty years old.) 'I think of God as real, actual flesh and blood and bones, something we all shall see with our eyes some day.' (Male, twenty-one years old.) Together with these, and from the same classes of students, came a great number of very different answers; for instance this, 'God is an impersonal being. . . . I think of him as the embodiment of natural laws.' Descartes's conception may serve as a point of comparison: 'What the soul itself was, I either did not stay to consider, or, if I did, I imagined that it was something extremely rare and subtle, like wind or flame, or either, spread through my grosser parts.'[1]

If the philosophical distinction between matter and spirit is not ordinarily made, these terms express none the less a very definite practical meaning of prime importance: they mark the difference between forces that are not responsive to psychic influences (desire and emotion, ethical and æsthetic considerations) and those that are.

The trial-and-error method which serves to establish the efficient modes of behavior observed in animals, is so far-reaching in its possibilities that one might be tempted to regard it as accounting for the existence of Magic and of Religion. Were this theory tenable, the origin of the three modes of human behavior would have been brought back to one method of learning, the unreasoning, trial-and-error method. But, even a superficial consideration discovers insuperable obstacles

[1] *Meditationes*, ii, p. 10, Amsterdam, 1678.

in the way of this enticingly simple explanation, and compels the admission that magical art and Religion involve the operation of mental powers not required for the estalishment of the mechanical, and of the non-religious anthropopathic behaviors.

The first of the two differences I intend to bring out, is that if a particular action is to be learned by an animal, the gratification of the actuating desire must follow immediately, or nearly so, upon the performance of the successful act, and be frequently repeated at short intervals; whereas in man, as far as Magic and Religion are concerned, the results may follow quite irregularly upon the performance, often only long after, and, not infrequently, not at all. Had not the door opened every time the cat pressed the latch, but, let us say, only once every ten times, or, if every time, one week after the movement, he would never have learned to make his escape. No more would he have acquired the trick, had he not been placed in the cage repeatedly and at short intervals. An interesting instance of the gradual undoing of a habit in consequence of the absence of the sensory results for the sake and under the guidance of which the action had been learned, is reported by Lloyd Morgan.[1] He had brought up in his study a brood of ducks. They had had a bath every morning in a tin tray. After awhile, the tray was placed empty in its accustomed place. The ducks got into it and went through all their ordinary ablutions. The next day, they again enjoyed the missing water, but not as long as on the first day. On the the third day they gave up the useless practice of bathing in an empty tray.

In three days ducklings eliminate a habit which has become useless, whereas generations after generations of men have gone through innumerable, time-wasting, often costly and painful ceremonies for results rarely secured, and, as we think, never directly secured by the magical or the religious ceremonies themselves. There is here a curious point of psychology: animals establish habits under the guidance of immediate results while man develops the magical art and Religion *despite* the usual absence of the results sought after. The very possibility of deceiving himself reveals the superiority of man over animals, for self-deception requires a degree of independence from sense-observation, a capacity of constructive imagination, a susceptibility to auto-suggestion, not to be found in animals. That the first glimmer of these capacities should have plunged man in the darkness of primitive Magic and Religion, and made him the ridiculous fool he appears to be by the side of the matter-of-fact, intelligent animal, is, however, a very striking and singular fact.

[1] C. Lloyd Morgan: *Introduction to Comparative Psychology* (The Contemporary Science Series, 1894), p. 89.

If the constant and immediate appearance of the desired results does not seem necessary to the establishment of Magic and Religion, it should not be thought, however, that these arts are altogether useless. On the contrary, they are, even independently of the results at which they aim, of a most substantial value to the cause of individual and social development. Let it be said first concerning the expected results that they happen more frequently, perhaps, than I may have seemed to imply. When, for instance, the rain ceremonies are performed during a spell of dry weather, success, more or less distant, always crowns the efforts of the magicians: the rain does come and the earth does bring forth its fruits. The ceremonies for the healing of disease are often followed by the recovery of the patient, however absurd the treatment may have been. One should not forget, in this connection, the considerable effect of suggestion upon the credulous savage. Many cures are, no doubt, performed in this manner by the medicine-man. Davenport, speaking of tribes of Puget Sound, says: 'Their cure for disease consists in the members of the cult shaking in a circle about a sick person, dressed in ceremonial costume. The religious practitioner waves a cloth in front of the patient, with a gentle fanning motion, and, blowing at the same time, proceeds to drive the disease out of the body, beginning at the feet and working upward. The assistant stands ready to seize the disease with his cloth when it is driven out of the head! And they are able to boast of many real cures.'[1] A psychologist is not inclined to doubt the report of Curr, that among the aborigines of Victoria persons who knew themselves to have been devoted to destruction with magical ceremonies have pined away and died,[2] nor that of Howitt, who, alluding to the habit of the medicine-men of certain tribes to knock a man insensible in order to remove the kidney fat for magical purposes, writes, 'In the Kurnai tribe men have died believing themselves to have been deprived of their fat.'[3]

But the intended results form only a part, and that perhaps not the most important, of the gains to be credited to the practice of Magic and Religion. The most noteworthy of these unsought by-products are:— (1) The gratification of the lust for power. The Magician and the Priest are mediators between superior, mysterious powers and their fellow-men.

---

[1] F. M. Davenport; *Primitive Traits in Religious Revivals*, Macmillan (1905), p. 36: quoted from the Fourteenth Annual Report of the [Amer.] Bureau of Ethnology, p. 761.

[2] E. M. Curr: *The Australian Race*, iii, p. 547, as quoted by Frazer, *Golden Bough*, 2nd ed., i, p. 13.

[3] A. W. Howitt: *The Native Races of South-East Australia* (1904), p. 373.

The sense of mastery over, or communion with, these powers, and the respect and fear with which Magicians and Priests are regarded, are, of themselves, almost sufficient to keep up these practices. (2) Both these modes of behavior, but especially Magic, appeal to the gambling instinct. All men crave excitement; the savage is no exception. In the daring game in which the rain-maker or the disease-healer engages, the high tension of the gambling-table is, to a certain extent, present. (3) Less obvious, perhaps, than the preceding advantages, but not less valuable, is the general mental stimulation induced by Magic and Religion. Magic is the great social play of the savage. If animal plays serve a highly valuable purpose in affording practice in sense-observation and motor-co-ordination, Magic makes its chief call upon the imagination; in this consists one of its most far-reaching values. It becomes a training for the achievement of those higher mental syntheses requiring the momentary disregard of the actual sense-impressions, from which it is so difficult to liberate oneself, in behalf of the accumulated experience of a whole life.

The second objection to the assumption that the trial-and-error method could have led to the establishment of magical and religious habits, arises from the inability of animals to act towards unperceived objects as if they were actually present. A dog never welcomes by gambols or licks the hand of an absent friend while Religion, and at times Magic, show primitive man in more or less systematic relations with powers he has never sensed. When the Shaman draws lines upon the sand, describes various curves with his arms, utters sundry incantations, he does not address a power he perceives, nor even one he has really seen, although he may believe that he, or some one else has seen it. That animals are moved to action by memories of past perception is, of course, not open to doubt. Their whole life is a long testimony to that ability. Any one will recall instances of chains of concerted actions indicating clearly, on the part of some one of the higher animals, domesticated or wild, the anticipation of a particular person, object, or event. What they never do, is to behave as if the remembered object was really present, though not sensed. H. Spencer, discussing adversely A. Comte's opinion that fetichistic conceptions are formed by the higher animals, relates the following observation concerning a retriever who had learned for herself to perform 'an act of propitiation.' She had associated the fetching of game 'with the pleasure of the person to whom she brought it', and so, 'after wagging her tail and grinning, she would perform this act of propitiation as nearly as practicable in the absence of a dead bird. Seeking about she would pick up a dead leaf, a bit of paper, a twig, or other small object and would bring it with renewed manifestations of friendliness. Some kindred state of mind it is, which I believe, prompts the savage to certain fetichistic observances.'[1] So far the dog could go, but she could not have imagined the presence of an unseen being and behaved toward him in the same manner. Another significant point is that the absent objects towards which animals may direct their actions are always, so far as one may judge, identical with those actually sensed by them at some time, *i. e.*, their behavior never shows that they have transformed, imaginatively, objects with which their senses have made them familiar. Whereas, man can not only believe in the presence of unseen objects, but he can also imagine beings never actually sensed by him, and behave towards them according to the traits and capacities with which he has endowed them.

There are observations on record which compel the qualification of the assertion, I may have seemed to make in the preceding paragraph, of a clean break between man and animals. Certain dogs are thrown into paroxysms of fear by peals of thunder, and run into hiding. Darwin relates how his dog 'full grown and very sensible,' growled fiercely and barked whenever an open parasol standing at some distance was moved by a slight breeze. He is of the opinion that the dog 'must have reasoned to himself in a rapid and unconscious manner, that movement without any apparent cause indicated the presence of some strange living agent, and that no stranger had a right to be on his territory.'[2] Romanes, in a short and interesting paper entitled 'Fetichism in Animals,'[3] after reporting the preceding illustration, relates this observation touching a remarkably 'intelligent,' 'pugnacious' and 'courageous' dog. 'The terrier [Skye] in question, like many other dogs, used to play with dry bones, by tosssing them in the air, throwing them to a distance, and generally giving them the appearance of animation, in order to give himself the ideal pleasure of worrying them. On one occasion, therefore, I tied a long and fine thread to a dry bone, and gave him the latter to play with. After he had tossed it about for a short time, I took an opportunity, when it had fallen at a distance from him, and while he was following it up, of gently drawing it away from him by means of the long and invisible thread. Instantly his whole demeanor changed. The bone which he had previously pretended to be alive, now began to look as if it really were alive, and his astonishment knew no bounds. He first approached

[1] *Principles of Sociology* (3d edition, 1885), i. Appendix A, p. 788.
[2] *The Descent of Man*, 2d ed., i, p. 145.
[3] *Nature*, xvii (1877-1878), pp. 168-169. Comp. Lloyd Morgan, *Introd. to Comparative Psychology*, p. 92 ff.

it with nervous caution as Mr. Spencer describes, but as the slow, receding motion continued, and he became quite certain that the movement could not be accounted for by any residuum of the force which he had himself communicated, his astonishment developed into dread, and he ran to conceal himself under some articles of furniture, there to behold at a distance the uncanny spectacle of a dry bone coming to life.' Certain instances of instinctive fear of harmless things may help to interpret the preceding observations. G. Stanley Hall mentions a little girl who would scream when she saw feathers floating through the air. To keep another child in a room, it was sufficient to place a feather in the keyhole.[1]

Shall we hold that these animals interpreted the unusual experiences reported above as the work of hidden beings of the kind known to them, or shall we agree rather with Lloyd Morgan, Romanes, Spencer, and others, in thinking that their behavior indicated merely surprise, astonishment, and fear at the unexpected movements of familiar objects? That explanation is probably sufficient. The failure of an object to fit in with the psycho-physiological attitude of expectation which past experience has taught us to assume brings about the sudden disturbance called surprise, astonishment or fear. It is in subtance what would happen to any person if, on opening his bed in the dark, his hands came in contact with some object concealed in it. Personalization of the unexpected object is not necessary to cause fright. And yet, who shall say that in none of these instances is there anything corresponding to the anthropomorphic interpretation of natural events so common among men of low culture? Does not the growling of Darwin's dog indicate as much? It would seem to me an unjustifiably dogmatic assertion to affirm that no animal can think of thunder as caused by a being like those with which his senses have made him familiar. Were he to do so, he would do as the savage who projects his ordinary notion of animated beings behind inanimate phenomena. Creative imagination is not any more required for such an interpretation than for the belief in survival after death when it is suggested by apparitions in dreams or trances. It is quite in point, at any rate, to affirm that man and beasts are much nearer to each other, regarding the possibility of interpreting animistically certain striking natural events, than most people are willing to admit.

The most significant difference between men and animals is not found in the fact that animals may be unable to interpret animistically certain striking natural phenomena—an opinion

---

[1] *A Study in Fears*, *Am. Jour. of Psy.* (1897), viii, p. 166.

open to question—but in their inability to *fix* by means of communicable signs any fleeting animistic interpretation which might chance to cross their mind. Without the advantage conferred by speech upon even the lowest savages to hold, clarify, keep alive, and bring to fruition impressions of this evanescent nature, I do not see how a stable belief in animism could have been established. The decisive rôle played by language appears forcibly when one considers the part it takes in introducing dream experiences into waking life. The baffling evanescence of dreams caught sight of on awakening is familiar to every one. Unless clothed in linguistic form they are soon completely lost; verbal expression makes them part and parcel of our mental possessions.

Animals practice the mechanical and the anthropopathic behavior but, the latter, only towards beings present to their senses. Religion—anthropopathic behavior towards unperceived and, in lower religions at least, always personal powers—and Magic—the art of making use of an impersonal, non-mechanical power, to coerce things or persons—are found only in man. This fateful difference between animal and man is due to the latter's relative independence from slavery to sense-impressions. He is able (1) not only to sporadically imagine unseen personal agents but, thanks to speech, to keep their existence in mind, and (2) to establish and maintain practical relations with these imagined unseen beings, as well as to make use of suppositious impersonal forces, in the face of delayed and of oft-repeated failures on their part to produce what is expected of them.

A CRITIQUE OF PROFESSOR WIRTH'S METHODS OF MEASUREMENT OF ATTENTION

By L. R. GEISSLER

I have been working, for the past two years, upon the problem of the measurement of the degree of attention; and I have sought to approach the problem in the way that has already led to success in the case of intensity of sensation, by the assignment of numerical values to introspectively differentiated degrees of clearness. The problem has already been attacked by experimental psychologists, and notably by Professor W. Wirth, of Leipzig. Wirth's methods are entirely different from my own: but his general formulation of the problem, as that of measurable degrees of clearness, is the same. As I have found Wirth's articles both difficult and obscure, and as my criticism of his work must be given with some fullness if it is to be itself intelligible, I have thought it well to devote a special paper to the discussion of his contributions to the subject.

There is a difficulty at the outset, due to the fact that Wirth expresses himself ouly incidentally as to the relation of the field of attention to the field of consciousness. Wundt has made us familiar with the distinction between attention and inattention, apperception and perception, inner point of regard and inner field of regard. Wirth, on the other hand, appears (although I wish to speak on this point with great caution) to identify the range of attention with the range of consciousness. He aims to give "a precise determination of the range of simultaneous mental processes."[1] The title of his article in the Wundt *Festschrift* is "Zur Theorie des Bewusstseinsumfanges und seiner Messung." He speaks of the "Versuch einer Wiedergabe des gesammten Bewusstseinsumfanges."[2] Phrases like these seem to show at least that he is concerned with a total simultaneous consciousness, with both focal and marginal processes, even if they do not in themselves show that he draws no definite distinction between centre and periphery of consciousness. But we find, later, a passage like this: "the mere determination of the possible number of (tachistoscopically) isolated elements in maximal clearness does not at all show how high the absolute clearness-degrees may rise, or how many objects of the same clearness may be added to the average number of five isolated single objects, provided that all of them together form a familiar, associatively related whole."[3] Or again: "our measure of range really presupposes only a uniformly graded and highest possible average clearness of the whole complex.[4]" Here we have, appar-

---

[1] *Philosophische Studien*, xx, 489.

[2] *Op. cit.*, 493.

[3] *Op. cit.*, 524. "Durch die Feststellung der möglichen Zahl derartig (tachistoskopisch) isolirter Elemente in maximaler Klarheit ist jedoch noch gar nichts darüber bestimmt, wie hoch sich nun die absoluten Klarheitsgrade belaufen bezw. wieviele Objekte noch in der gleichen Klarheit wie bei ca. 5 isolirten Einzelobjekten zu diesen hinzutreten können, wenn sämmtliche Objekte zusammen ein geläufiges, unter sich associativ verbundenes Ganzes ausmachen."

[4] *Op. cit.*, 524. "Zunächst ist ja eigentlich nur die gleichmässig abgestufte und im Mittel möglichst erhöhte Klarheit des gesammten Complexes zu unserer Messung des Umfanges vorausgesetzt."

ently, an identification of attention and consciousness. And this inference is borne out by a passage in which Wirth discusses the relation between the apperceptive and perceptive regions of a simultaneous consciousness, and declares outright that there is no sharp difference between the two, but that on the contrary there are a number of transitional stages as we pass from the higher to the lower level. It is evident, he says, to unaided introspection "that the contraposition of an apperceptive and a perceptive region is not to be thought of as a mere dual division of consciousness. Within the simultaneous whole, several stages of attention and of clearness may always exist side by side, according as at any moment a larger or smaller number of unitary complexes forms the immediate experience of the subject. Although under certain conditions, which favor a kind of dual division, it is possible that a region, to which a fairly uniform attention is given, may be opposed to a 'background' or 'periphery' of consciousness, to which attention is as uniformly denied, nevertheless the concept of apperception ordinarily denotes a general direction of the process which goes on over the whole field, though at different places with unequal completeness."[1] Apperception is thus, as it seems, simply a general tendency to conscious self-realization, common to all the contents of a consciousness, but carried farther in the case of certain mental processes than in that of others simultaneously present. The antithesis of 'dunkel bewusst' and 'klar bewusst' practically lapses, as antithesis, and the difference between perception and apperception becomes merely a matter of degree. If it is objected that, even in Wundt himself, this difference has never been anything more, we reply that while the objection may be formally sound, in terms of a strict definition of the conscious states known as clear and obscure, nevertheless the distinction has played so important a part in the Wundtian system as to be, to all intents and purposes, a distinction of kind. This generic, Wundtian difference seems to be given up by Wirth, without defence or discussion; and we may add that Wundt himself, in his brief discussion of Wirth's experiments,[2] vacillates between the expressions 'range of consciousness' and 'range of attention' as if he too saw no reason to distinguish them.

The question at issue, however, is more than a question of personal belief or of an individual psychological system; it is a question of fact, of observation. That Wirth would have done well to face it as a question of fact will come out, I believe, in the course of the present paper. The above interpretation of Wirth's attitude is offered with all reserve, since his language—if I understand it aright—is not always consistent. I turn now to a consideration of his experimental investigations.

Of the three sets of experiments reported[3] the first is of a preliminary character, and is of no importance for the determination of degrees of clearness. In these experiments, a standard visual complex of 25

---

[1] *Op. cit.*, 493. "Hierbei zeigt sich vor allem noch, dass die Gegenüberstellung einer apperceptiven und perceptiven Region nicht etwa bloss als eine einfache Zweitheilung des Bewusstseins zu denken ist. Innerhalb des simultanen Ganzen sind jederzeit mehrere Stufen der Beachtung und der Klarheit neben einander möglich, je nach der grösseren oder geringeren Zahl von Einheitsbildungen, auf welche sich das Subject augenblicklich im unmittelbaren Erleben bezieht. Wenn auch unter Umständen in einer Art von Zweitheilung eine ziemlich gleichmässig beachtete Region einem ähnlich gleichmässig unbeachteten 'Hintergrunde,' bezw. einer 'Peripherie' des Bewusstseins gegenüberstehen kann, so bezeichnet doch der Begriff der Apperception mehr eine allgemeine Richtung des Processes, welcher innerhalb des ganzen Blickfeldes an verschiedenen Stellen in ungleicher Vollkommenheit durchgeführt ist."

[2] Phys. Psych., III, 1903, 358-360.

[3] Philos. Stud., XX, 1902, 635-659; Psych. Stud., II, 1906, 30-88; A. Kästner und W. Wirth, *ibid.*, III, 1907, 361-392, and IV, 1908, 139-200.

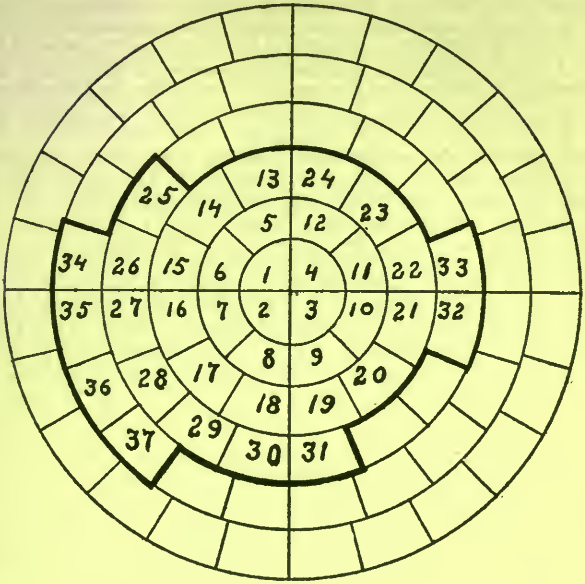
or less small geometrical objects was observed, with maximal attention, until all its elements had been clearly apprehended. During a short interval, usually of less than a second, the complex was slightly varied, in one or several different places unknown to the observer. Then it was tachistoscopically re-exposed for another fraction of a second, and the observer was asked to say whether, and if possible where, a change had taken place. Wirth's argument is as follows: "So far as, under these variable conditions, a correct judgment of difference remains possible, so far at least does the range of the simultaneous visual consciousness extend. But the difference necessary, at any given point, to arouse this correct judgment of difference, or in other words the differential limen under these special conditions of attention, is at the same time a measure of the clearness-degree which obtained at that particular point in the standard complex".[1] That is to say, if the change made at a certain place in the complex is noticed during re-exposure, then the object was in consciousness at some moment of the primary exposure; and the amount of change needed to arouse notice is inversely proportional to the degree of clearness of the mental content. The results show that, if the complex contained only 12 or 13 figures, any change in them could always be judged correctly; whereas, if the complex consisted of 25 figures, many errors occurred. With a momentary original exposure of a standard complex of 3 to 5 figures, changes in the re-exposure field were recognized with correspondingly greater or less certainty. Thus the results confirm and extend those of previous tachistoscopic experiments upon the range of attention proper.

After this preliminary investigation, Wirth comes back to the "problem of the completest possible determination of the range of consciousness",[2] which leads him more especially to "a quantitative determination of the simultaneously present clearness-degrees or degrees of consciousness of the greatest possible number of elements".[3] Nothing is said of the apperceptive and the perceptive regions; and it is only by a close examination of the experimental methods and results that we shall be able to decide whether Wirth has measured different degrees of clearness within the apperceptive region only, or whether he is dealing with the whole series of assumed transitional steps between the upper and the lower levels of consciousness. Wirth devised a special apparatus, whose main features are as follows. The field of vision for the left eye consisted of the funnel-shaped and uniformly illuminated surface of a glass cone 50 cm. in diameter at the base and 25 cm. high. The left eye was placed at the centre of the perimetrical field. The whole surface was divided into 6 concentric rings, the peripheral ring being again divided into 24 regions, the next into 20, the next into 16, the next into 12, the last into 8, and the inner disc into four regions. Of the 84 regions thus demarcated, 37 were selected for experimentation. The arrangement is shown in the Fig. By reference to this schema, the point of change could be easily identified. The conical surface was observed continuously,

[1]Philos. Stud., XX, 593 f. "So weit sich unter den übrigen, bereits bekannten Bedingungen noch ein richtiges Unterschiedsbewusstsein bei einer solchen Variation mit Sicherheit einstellt, so weit reicht mindestens der Umfang des simultanen optischen Bewusstseins. Die Differenz aber, welche zur Erzielung eines solchen Unterschiedsbewusstseins an einer bestimmten Stelle nothwendig ist also die Unterschiedsschwelle unter diesen speciellen Aufmerksamkeitsbedingungen, ist zugleich ein Mass des Klarheitsgrades, welcher an dieser Stelle bei der Verteilung innerhalb des betreffenden Complexes herrscht."

[2]Psych. Stud., II, 30 f.; "die Aufgabe einer möglichst vollständigen Bestimmung des sog. Bewusstseinsumfanges."

[3]*Op. cit.*, 31.; "der quantitativen Bestimmung der gleichzeitig vorhandenen Klarheitsgrade oder Bewusstseinsgrade möglichst vieler Elemente."



with different distributions of attention, while by the manipulation of lenses and mirrors any one of the 37 regions could be gradually changed until its brightness was noticeably different from that of the rest. The first problem, then, was to establish the 'normal' differential limen for each region while the apex was fixated and the particular region was observed with maximal attention. If, for example, the brightness of the whole field of vision at a given moment was 174.5 photometric units, while the observer was attending to region no 2, and if the additional brightness necessary for the region to become just noticeably different was 29 photometric units, and the duration of the change was 6.75σ, then the normal limen for region no. 2 was $\frac{29 \times 6.75}{174.5} = .91$. The values for each of the 37 regions were similarly determined. The next question was to find out how the normal limen was changed by varying concentration of attention, and how the changes could be made to express or measure degrees of clearness. To answer it, Wirth divided the normal limen of any given region by its 'distraction' limen, that is, by a limen for the same region with a prescribed direction of attention. "A comparable value for the degrees of clearness (*Beachtung*) realized with a total distribution of attention over the various elements, each having its variously favorable position in the complex, may evidently be obtained without serious error if we divide the limen for maximal attention to a known region of change by the liminal value (for the same region) resulting from the method without knowledge, that is, from the method employed for the determination of the range (of consciousness)".[1] Or, more explicitly: "a more delicate differential or

[1]Philos. Stud., XX, 598. "Einen vergleichbaren Werth der Grade der Beachtung, welche für eine und die nämliche Gesammtvertheilung der Aufmerksamkeit mit

variational limen will correspond to a higher clearness degree; and the ratio of this limen to the normal limen for the same region as known in advance and made the object of attention will offer the greatest approximation to a comparable measure of degrees of clearness."[1] Thus, to return to our previous example of region no. 2; if we determine its distraction limen, while attention is distributed over the whole area, we find the value $\frac{30.3 \times 25}{146.4} = 2.06$. The degree of clearness corresponding to the distraction limen 2.06 is then expressed by the ration 0.91:2.06. The clearness degrees for the other regions are determined in the same way. We thus obtain 37 different ratios, which are made comparable by reduction to the same basis of 100; *e. g.*, in the case of region no. 2, the proportion $0.91:2.06 = 100:x$ results in the value 226 as the clearness index for a totally distributed attention. Wirth has determined in this manner the clearness indices of all the 37 regions for 6 different distributions of attention, and has presented them in 6 different schemata of the kind shown in the Fig., each region containing a ratio which represents its clearness for a certain distribution of attention. As to the psychological interpretation of these schemata, he says simply: "the value of these diagrams is quite independent of our psychological interpretation, which might, *e. g.*, be given as well in terms of purely dispositional factors."[2] Nevertheless, he certainly assumes that the numerical values of the schemata are, in some way and to some extent, definite quantitative measures of degrees of clearness. In order, then, to determine whether and in how far the results may be accepted as indications of differing clearness-degrees, we must examine the diagrams more closely. And we begin by considering them, so far as possible, from what we conceive to be Wirth's own point of view.

Wirth gives, first, a schema of minimal normal values,[3] which he employs for the calculation of the ratios with the six different distributions of attention. We should expect that these values would show normal characteristics as regards their magnitude, their relative frequency, and their spatial distribution. Now the lowest of them is 74, the highest 173, the general average 119, and the mean variation 19.3. The most frequent values (11) occur between 91 and 99, while 26 (or almost three-fourths of the 37) occur within the limits of 74 and 116, and the 11 highest values range between 123 and 173. The most regular spatial distribution is evinced by the four quadrants constituting the three innermost and complete concentric areas, and consisting of 24 regions. The left lower quadrant has the lowest average value, 89 ± 5, and the right upper quadrant the highest, 126 ± 7; the other two are nearly equal, 100 ± 7 and 102 ± 7 for the left upper and right lower quadrants respectively. However, if we look at the extreme values in each of the five concentric areas, we find that they range, as we travel in the centrifugal direction, between 91 and 107, 74 and 137, 87

---

verschieden günstiger Stellung der einzelnen Elemente zu Theil werden, gewinnt man dann offenbar ohne grossen Fehler, wenn man den Schwellenwerth bei maximaler Beachtung der wissentlich variirten Stelle mit dem Schwellenwerth dividirt, der bei unwissentlichem Verfahren, also den eigentlichen Umfangsbestimmungen, gewonnen worden ist."

[1]Psychol. Stud., II, 31. "Dem höheren Klarheitsgrade wird hierbei eine feinere Unterschieds- bezw. Veränderungsschwelle entsprechen, deren Verhältniss zu der 'Normalschwelle' für die Veränderung des nämlichen im voraus bekannten und maximal beachteten Elementes die grösste Annäherung an ein vergleichbares Mass des Klarheitsgrades bieten dürfte."

[2]*Op. cit.*, 73. "Der Wert dieser Karten ist ganz unabhängig von unserer psychologischen Deutung, die z. B. auch im Sinne rein dispositioneller Verhältnisse erfolgen könnte."

[3]*Op. cit.*, 71, Fig. 8.

and 137, 97 and 150, and 123 and 173. The difference in the central area is the smallest (16), in the next area the largest (63), and in the other areas nearly equal (about 50). In spite of these irregularities, Wirth thinks that "there appears, after all, to be a sufficient degree of uniformity in the minimal values"[1] to warrant their use as 'normal' values. He therefore proceeds to divide them by the 222 distraction values, obtained with the six different directions of attention. It is obvious, however, that, if the normal values themselves show so little regularity, the 222 ratios partly depending upon them must be still less uniform as regards magnitude, relative frequency and spatial distribution. Besides, they are further affected by the fact that, while a certain part of the visual field is fixated, and another part observed with greatest concentration of attention, a liminal change of brightness may be expected in any possible region. Surely, such complications do not promise very satisfactory results. We find, in fact, that of the 222 values the lowest is 86, the highest 216. The extremes occur in the parts attended to as well as in those distracted from. For convenience of comparison we have arranged the figures in Tables I and II under the heading *Range*. The second table differs from the first in having the 9 high values above 200 eliminated. If, now, these

TABLE I

Attention to—	Total Field of Vision			Area Attended to			Area Distracted from		
	Range	Av.	MV.	Range	Av.	MV.	Range	Av.	MV.
Total Field	108–284	165	30	108–284	165	30	—	—	—
Left Half of Field	86–181	131	23	86–181	130	24	88–178	131	20
Right Half of Field	106–186	136	17	110–173	136	20	106–186	136	16
Left Upper Quadrant	103–206	138	20	103–171	127	18	105–206	142	20
Left Upper Periphery	91–302	147	28	135	—	—	91–302	148	28
Fixation Point	93–162	123	15	—	—	—	93–162	123	15

TABLE II

Attention to—	Total Field of Vision			Area Attended to			Area Distracted from		
	Range	Av.	MV.	Range	Av.	MV.	Range	Av.	MV.
Total Field	108–200	151	19	108–200	151	19	—	—	—
Left Half	86–181	131	23	86–181	130	24	88–178	131	20
Right Half	106–186	136	17	110–173	136	20	106–186	136	16
Left Upper Qu.	103–198	136	19	103–171	117	18	105–198	140	19
Left Upper Per.	91–192	141	22	135	—	—	91–192	141	22
Fixation Point	93–162	123	15	— —	—	—	93–162	123	15

[1] *Op. cit.*, 72. "So ist schliesslich eine hinreichende Gleichmässigkeit der Minimalwerte vorhanden."

values are really expressions of degrees of clearness, we should expect to find a marked difference in their distributions; the low values ought to be found in the parts attended to, the high values in the other parts of the field. Or, if unknown and occasional errors or the irregularity of the normal values be allowed for, we should still expect to find the *average* values for the parts attended to lower than those for the rest of the field of vision.

The tables show no sign of such a distribution. Wirth himself explains the disappointing character of the results as follows: "the effort of the special activity to bring about the right distribution of attention makes itself felt as a competitive factor. With distribution of attention over the whole field, it is probable that the lowering of the clearness values is for the most part due to this disturbing factor. But the distribution of attention to a certain region also carries with it the possibility that a higher clearness-degree may be assigned to some particular point which, under the circumstances, happens to be the object of maximal attention. [For these reasons] the arrangement of the values within the observed area gives no constant picture of a determinate clearness-relievo in the visual field under steady fixation."[1] Such an admission seems, however, to be fatal to the whole investigation. If the difficulties encountered were so great as entirely to obscure the results, then the experimental method employed is not applicable to the problem in hand.

The third set of experiments upon clearness-degrees is reported by A. Kästner and W. Wirth. Here recourse is had, not to just noticeable brightness differences, but to a plainly supraliminal brightness-change. The clearness-indices are expressed by the times of quickest possible reaction to the discrimination of change. Otherwise the experimental arrangements remain the same as in the previous work. The following groups of experiments were made: I *a*, simple reaction with knowledge of the region to be changed and maximal concentration upon it (to furnish a series of 'normal' reaction times corresponding to the normal brightness determinations in the former investigation); I *b*, complete reaction, without knowledge, and with attention distributed over some one quadrant; I *c*, the same with total distribution of attention; II, the same, with attention upon a region in the left upper periphery; III, selective reaction without knowledge, attention being totally distributed, and the reacting forefingers being assigned either to the right and left halves of the visual field, or else the one to a particular region and the other to all other regions. "It seems justifiable to assume that a proportional change in the reaction time will occur, if at all, then assuredly in connection with its dependence upon the degree of clearness with which the motive to reaction is apprehended."[2] The clearness-index is expressed by a number which is found by multiplying the reaction time of a given region, with a certain distribution of attention, by a fraction whose numerator is a constant (the average of the 37 normal values, 223.8σ) and

---

[1] *Op. cit.*, 87. "Indessen beginnt bereits die Anstrengung der besonderen Tätigkeit zur eigentlichen Verteilung der Aufmerksamkeit sich konkurrierend geltend zu machen. Bei der Verteilung auf das ganze Sehfeld fällt wahrscheinlich der Hauptanteil der Erniedrigung des Klarheitswertes dieser Störung zu. Doch trägt die Verteilung der Aufmerksamkeit auf ein Gebiet jederzeit die Möglichkeit in sich, einem beliebigen bei der Verteilung gerade maximal beachteten Punkt einen höheren Klarheitsgrad zukommen zu lassen, als ohne diese Verteilung. Die Art der Anordung der Werte innerhalb des beachteten Gebietes lässt keine Konstanz eines bestimmten Klarheitsreliefs im Sehfeld bei festgehaltener Fixation auffinden."

[2] Psych. Stud., III, 390. "Wenn irgendwo, so scheint nun gerade für die Abhängigkeit der Reaktionszeit vom Klarheitsgrade der Motivauffassung, die wir hier im einzelnen untersuchen wollen, eine solche proportionale Veränderung der zunächst von beliebigen Unterschieden der Raumlage modifizierten Reaktionszeiten angenommen werden zu können."

whose denominator is the normal average value (of 5 individual values) for a given region. Thus, if the reaction time for region no 18 is 216σ, with total distribution of attention, while the normal average reaction for the same region is 221, then its clearness-index will be $216 \times \frac{223.8}{221} = 218$. Hence, with total distribution of attention, 218 represents the degree of clearness for region no. 18, independently of its position in the visual field.

The results of the normal reaction experiments are uniform. The times vary only between 200 and 245, and show a fairly regular distribution over the whole area. On the other hand, the results with various distributions of attention are so irregular that the authors are satisfied with the general comparison of the average values for the whole area, without, as a rule, giving due consideration to the large mean variations, or even distinguishing between the regions attended to and those not attended to. Their conclusion runs: "no matter how much the distribution of the times varies, over the whole visual field, for the different adjustments of attention, the general averages agree almost perfectly in cases of approximately equal practice; their mean is 282. This time is, therefore, in a certain sense the expression of a constant energy expended in the control of the whole perceptual field of possible motives to reaction,—an energy that, under the different distributions of attention, manifests itself in the time of preparation even for varying spatial positions; so that, on the average, a higher degree of expectation of a certain group of motives lowers correspondingly the preparedness of the observer for all the other possibilities."[1] It is difficult to see what relation such a conclusion can sustain to the problem of measuring either the range of consciousness or the degrees of attentional clearness. Some of the irregularities are, it is true, more or less adequately explained, as due to special circumstances, while others are termed simply "Zufälligkeiten."[2] The latter designation seems to apply, for instance, to the case of total distribution of attention, where the four regions of the most extreme concentric area contain not only the longest and the average reaction times of the whole series, but also the two quickest reactions of that particular series. However, it is not of vital importance that we should know the reason for the numerous irregularities. The fact that they occur at all, and prevent the appearance of the expected results, is sufficient to prove the inexpediency of the method. Indeed, in their closing sentence, the authors virtually admit the inadequacy of the investigation. "Further enquiry must determine how far a more detailed differentiation of the co-ordinations, with more than two possibilities of choice, may avail to furnish a more accurate representation of the clearness-relievo within any given group of motives to reaction."[3]

Thus far we have sought to review Wirth's investigations entirely from what we regard as his own standpoint; and our conclusion is

---

[1] Psych. Stud., IV, 163. "So verschieden als die Verteilung der Zeiten auf das ganze Sehfeld bei den verschiedenen Einstellungen der Aufmerksamkeit ist, so stimmen die Gesamtmittel bei ungefähr gleichen allgemeiner Uebung fast vollständig überein und ergeben im Mittel hier 282. Diese Zeit ist also gewissermassen der Ausdruck einer konstanten Energie zur Beherrschung des ganzen Wahrnehmungsbereiches möglicher Reizmotive, die bei den verschiedenen Verteilungen der Aufmerksamkeit in der Vorbereitungszeit auch in verschiedener räumlicher Aufteilung zur Geltung kommt; so dass die grössere Erwartung der einen Motivgruppe die Bereitschaft für die übrigen Eventualitäten im Mittel um ein Entsprechendes herabsetzt."

[2] *Op. cit.*, 148.

[3] *Op. cit.*, 200. "Es wäre nun weiterhin zu untersuchen, inwiefern eine weitere Differenzierung der Zuordnungen in einer mehrfachen Disjunktion ein genaueres Abbild des Klarheitsreliefs innerhalb eines Motivbereiches ergibt."

that he has failed to solve his problem. The difficulties of his method and the many factors that complicate his observations seem to obscure the real nature of his results and to rob them of any decisive significance. It may, however, be possible to give the results a new meaning by an interpretation from that standpoint which, in opposition to Wirth's, makes a sharper distinction between the apperceptive and the perceptive levels of consciousness.

There can be no doubt that Wirth's experiments have dealt with certain degrees of clearness. The question is whether they covered the whole range of a simultaneous consciousness, from the clearest region of apperception to the obscurest region of perception, or whether they were merely concerned with certain relatively small differences of clearness within the level of apperception itself. Wirth assumes that his low values represent a high degree of clearness. He has to admit, however, that his high values do not necessarily represent a low degree of clearness. They were due, at least in part, to distracting factors. While, therefore, the low values may possibly furnish a numerical statement of the clearness of processes on the apperceptive level, Wirth has been unable either to measure the clearness of processes on the lower level, or to prove the existence of his assumed transitional steps between the two levels.

Let us consider once more the liminal brightness-determinations obtained with various distributions of attention. The 222 values represent 100 actual ratios. Now Wirth certainly does not mean that each separate ratio measures a new degree of clearness, so that there are at least 100 stages between the two extremes of his experiments. If he had thought this, he would not have disregarded the large mean variations, and have contented himself with general averages. On the other hand, if his values do embrace the whole range of consciousness, and if they demarcate in a rough way some smaller number of intermediate steps, then we should expect them to fall into natural groups of greatest frequency, clustered around certain averages representing these steps, and thus to form a multimodal frequency curve. In this event, the number of such groups will indicate the number of intermediate steps, and the difference between their averages will be a rough measure of the size of the steps. But if we examine the total frequency distribution of the 222 values, irrespective of the various kinds of attention under which they were obtained, we find only the slightest indication of grouping between the values 90 and 160. The probable meaning of this we discuss later on. It is very significant that of the 100 different values only

2	occurred	7	times	(namely 130 and 144)
1	"	6	"	(" 146)
7	"	5	"	(" 103, 110, 119, 125, 137, 149, and 157)
12	"	4	"	
21	"	3	"	
44	"	2	"	

Although the figures vary, as was stated above, between the limits of 86 and 302, 90% of them (or 200 out of the 222) occur between 90 and 180; and, what is more, 75% (or 165) lie between 90 and 160. That there is no multimodal distribution may be easily shown by constructing a frequency surface, in which, *e. g.*, groups of 10 possible values are represented in each unit of the abscissa. The same fact is brought out in the following table:

Within the limits of	81	and	90	there occur	2 cases
"	91	"	100	" "	10 "
"	101	"	110	" "	26 "
"	111	"	120	" "	31 "
"	121	"	130	" "	32 "
"	131	"	140	" "	26 "
"	141	"	150	" "	30 "
"	151	"	160	" "	21 "
"	161	"	170	" "	13 "
"	171	"	180	" "	12 "
"	181	"	190	" "	6 "
"	191	"	200	" "	4 "
"	201	"	302	" "	9 "

Thus our expectation of finding natural groups of values is not realized. We find instead that the 222 (minus the 9 extreme cases above 200) group themselves around one general average of 136, with 75% of them clustering closely about 122. A similar distribution is found in Wirth's reaction times, as may again be seen from the construction of a frequency surface. This fact would seem to indicate that, during most of the experiments, the observer managed to maintain a fairly constant and fairly high degree of concentration, while during the remainder the difficulties of which we have spoken exercised a dominant influence upon the results. Such an assumption becomes still more plausible if we compare the frequency distribution of Wirth's reaction times with those of other experimenters upon the sensorial reaction. We may select for instance the results of Alechsieff as typical, since he also took sensorial or complete reactions to visual stimuli.[1] We call attention to the curves for his 4 observers A, K, F, and S. These observers were required to concentrate their attention maximally upon the stimulus. Their "Einstellung" thus differed as widely as possible from that required by Wirth. Nevertheless the distribution of their results is strikingly similar to Wirth's; so similar that the agreement can hardly be due to accident. It therefore seems fair to suppose that Wirth, after all, maintained a uniform concentration of attention upon the field of vision, and that his degrees of clearness—in so far as they are correctly indicated by his lower values—are only small variations upon the apperceptive level.

With this interpretation in mind we may come back to the slight indications of grouping between 90 and 160. Near our corrected average value of 122 there is, as has been said, a relatively large frequency of similar values. We notice further, small groups at 110 and 103, where we may perhaps assume slight rises in clearness above the common or normal level. We notice also small groups at the lower values 128, 137, 146, and 156, which in a similar way we may perhaps assume to represent slight decreases of clearness. The same thing appears on inspection of the reaction times. It is true that we are not here dealing with simultaneity in consciousness, since the values are taken from the different distributions of attention. And if separate curves are drawn for the separate distributions, they will be found to show both fewer and less marked deflections from their general apperceptive levels (especially in the case of the brightness-values, where all values above 160 should probably be eliminated). It would plainly be wrong, then, to lay any great emphasis upon these groups, in view of the extreme complications of Wirth's experimental conditions. They are at best, only very slight, and are mentioned merely in order to do full justice to the results.

[1] Philos. Stud., xvi, 1900, 1 ff.

All these considerations force upon us the conclusion that Wirth's numerical values, in as far as they may be admitted to measure degrees of clearness, refer to slight clearness variations on the apperceptive level only; that he has not measured the range of a total consciousness; and that he has given us no evidence of the existence of transitional degrees or steps between the apperceptive and the perceptive level. It may be added that, so far as can be seen without repetition of the experiments, Wirth's experimental arrangements were most unfavorable to a solution of his problem. All tachistoscopic experiments (and the conical field is, in essentials, tachistoscopic) tend to exaggerate the difference between the two main levels of consciousness. Under Wirth's conditions, the whole world of extraneous lights and colors was, objectively as well as subjectively, excluded from the observer's eye and mind. The difficulties of concentration of attention upon the various parts of the visual field, with maintenance of a constant fixation, must have required an almost hypnotic state of attention, in which the isolated tachistoscopic objects appealed to the observer with such exclusive force that the mental processes which would normally have occupied the perceptive level were driven towards or below the limen of consciousness. This is probably Wirth's justification for their neglect. But such an extreme and almost abnormal state of attention is, evidently, not the most favorable condition for the solution of his problem, in whatever form he meant this problem to be understood. It is too far removed from the conditions of daily life, where the most natural and frequent occurrence seems to be the more or less clear apperception of a single mental process or small unitary group of processes, upon a background of other, more or less obscure but still noticeable mental processes.

There are one or two other points that call for criticism. In the first place, practically the only observer, especially in the last two sets of experiments, was Wirth himself, so that there is no possibility of comparing results obtained from different individuals. The excuse offered is lack of time which, however, can hardly be accepted as valid. In the second place, comparison is the more necessary in this case since it is practically impossible to repeat the experiments anywhere except in the Leipzig laboratory. And yet they must be repeated, unless we agree that the methods are so faulty and so unpromising as to spare us the necessity of repetition. This, at any rate, is clear: that Wirth has failed to solve his problem. For, if we try to interpret his numerical values from his own standpoint, we find them meaningless; they are obscured or invalidated by complicating factors. And if we look at them from our own point of view, and eliminate into the bargain all doubtful cases, they appear to disprove his assumption of the existence of transitional steps between the apperceptive and perceptive levels. The main reasons for his failure seem to be uncertainty and ambiguity in the formulation of the problem; the impossibility of overcoming difficulties of observation; and the restricting conditions of his experimental arrangement, which was rather unfavorable than favorable to the type of consciousness under investigation.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

X. A Study of Retinal Rivalry in the After-image

By Alma de Vries and Margaret F. Washburn

If a stereoscopic slide be constructed having on each half a small square of differently colored paper, and if such a slide be looked at through a stereoscope from which the middle partition has been removed, double images of each square will be seen. If the distance between the squares has been properly adjusted, the two innermost images will coincide, producing a binocular image that fluctuates in color, displaying the ordinary phenomena of retinal rivalry. Now, when after prolonged fixation of the central square, the eyes are closed, negative after-images of the colored squares appear. The two images on either side show each in the color complementary to that of the square on its side of the slide; the middle image alternates between the colors complementary to those which alternated in the middle square seen on the slide. In other words, the retinal rivalry continues in the negative after-image. For instance, if the slide has a blue square and a red square on it, when seen through the stereoscope it will show three squares, a blue and a red one on either side and in the middle a square which is alternately blue and red; the negative after-image shows a yellow square in place of the blue one, a green square in place of the red one, and in the middle a square which alternates between yellow and green.

There is, of course, nothing remarkable about this phenomenon. Attention was called to it by Breese in his work on Inhibition. Working with a light of approximately constant intensity transmitted through red and green gelatine, he found (1), that "the rate of fluctuation in the rivalry of after-images is much slower than in the case of direct stimulation;" and (2), that the lengths of the phases varied on different days. Seven tests only were made, and the duration of fixation of the stimuli was one hundred seconds. Breese found also, as a result of his study of rivalry in general, that conscious effort increased the length of time during which the phases seen with open eyes lasted. But he did not observe what effect such 'voluntary' lengthening of phase had upon the phase of the corresponding negative after-image, and it was to this problem that we addressed ourselves in the work to be described.

Our methed of experiment was as follows: The observer looked through the stereoscope for forty seconds at a slide on which were two squares, one of green paper and one of blue paper, the saturated green and blue of the Bradley series. These squares were one cm. a side. The observer's hand rested on a telegraph key, by pressing down which the line drawn by an electric marker on a smoked drum was lowered. Time was registered on the drum by a Jacquet time-marker. The light was ordinary daylight, which of course varied in intensity; the experiments were always performed at the same time of day. During the forty seconds' fixation the observer recorded the time of the fluctuations of the middle image by means of the key, while the experimenter recorded on the drum the nature of the changes as verbally reported by the observer. The experimenter at the end of forty seconds gave a 'Now' signal, at which the observer closed her eyes and the fluctuations of the middle after-image were recorded in the same way as those of the original image. After completing such an experiment, a rest of three minutes was allowed. The after-image, it should be said, was observed as long as any trace of it remained. When three minutes had elapsed, the experiment was repeated, but this time the observer was instructed to make every effort to hold the blue color in the middle image during the period of actual stimulation. When the eyes were closed, the after-image was allowed to take its course as before without effort at control, the object being to see whether any lengthening of the blue phase by effort in the original image would affect the duration of the yellow phase in the after-image. Another three minutes' pause was then allowed, and a third experiment performed in which it was attempted to prolong the green phase in the original image. These three experiments were usually all that were demanded of the observer during a sitting, as the work was quite fatiguing. From six to ten such complete experiments, of three parts each, were made with each of the three observers. Later, the colors red and blue were used on the cards, five complete experiments being made with each observer. The observers were V. and W., the writers, and R., a young woman with a semester's previous training in introspection.

The numerical results appear in the accompanying tables. The first horizontal row, under each observer, marked 'Uncon.', gives the average figures for the experiments where there was no effort to control the rivalry in the original image; the second horizontal row in Table I gives the average results for the experiments where the observer attempted to prolong the blue phase, and the third row those where the attempt was to prolong the green phase. In Table II the second and third horizontal rows give the results of effort to control for blue and red respectively. 'L' stands for the average duration in seconds of each appearance of the color under which it is placed; 'N' for the average number of such appearances. 'M. V.' of course refers to the mean variations of these averages.

The conclusions to be drawn from these tables may be briefly stated as follows:

(1.) In the rivalry of the original stimuli an effort to see either one of the two colors resulted in a lengthening of the average duration of the appearances of that color. The average length of the phase of a given color was longer when the effort was made to hold that color, in every case but two. In one of these exceptions, observer W., when the effort was made to hold the red phase against the blue, obtained an average red phase (2.2 seconds) which was longer than was the case when the series was controlled for blue, but shorter than the red phase in the experiments without attempt to control (4.7). The other exception occurred with observer R., who in the experiments with red and blue got a shorter average phase when she attempted to hold red than under any other circumstances. In the other ten sets of experiments where there was attempt to control, the attempt was successful so far as a lengthening of the average duration of the separate appearances of the colors was concerned. This result is in accord with those of Breese.

(2.) Another of Breese's results is also confirmed. He says, "The rate of fluctuation in the rivalry of after-images is much slower than in the case of direct stimulation." We also find that the average length of a color phase is longer in the after-images than in the original images.

(3.) A third confirmation of Breese's results relates to his statement that by effort to hold one color in the original image, "the number of fluctuations in the rivalry could not be controlled." Inspection of our tables shows that in every case but two, the number of appearances of a given color was not increased by the effort to see that color, although the duration of the single appearances was. It is a curious coincidence, at least, that the two exceptions are identical with the exceptions to the increase of average phase with effort. Observer W., who did not get a maximum average red phase with the effort to hold red against blue, got the maximum number of appearances of the red phase under these conditions; and observer R., who got the minimum average length of the red phase in trying to hold red against blue, also found that the effort in this direction produced the greatest number of appearances of red. It looks almost as though the effort to control, which normally lengthens the time of each appearance without affecting the number of appearances, might on failing to produce the first effect expend its energy in bringing about the second.

(4.) It will be seen on comparing the figures that when through the effort to hold one color, the average duration of that color in the original image was lengthened, the negative after-image of the color in question was not seen any longer than when no effort at control was made. The lengthening of a color phase in the original image by effort to hold that color is accompanied, in general, by no correspond-

TABLE I

Colors: Green and Blue

	Stimulus								After-Image							
	Green.				Blue.				Red				Yellow			
	L	m v	N	m v	L	m v	N	m v	L	m v	N	m v	L	m v	N	m v
Observer W																
Uncon.	1.3	.45	9.	1.5	1.7	.22	9.5	1.	3.2	1.3	2.	.5	5.6	2.1	1.2	.3
Blue	1.2	.22	9.5	1.	2.2	.15	9.5	1.5	3.7	.8	1.5	.75	7.4	2.3	2.2	1.6
Green	1.5	.4	9.5	2.5	2.1	.55	9.2	1.8	3.3	.7	1.5	.5	9.5	3.4	2.7	1.7
Observer V																
Uncon.	2.2	.27	8.5	1.8	2.3	.8	8.7	1.5	3.3	1.3	3.2	.65	3.3	.9	3.8	1.1
Blue	1.1	.26	7.	1.6	4.4	1.2	7.4	1.4	2.4	1.5	2.7	1.1	3.7	1.3	2.7	.8
Green	3.8	.9	7.8	1.7	1.3	.58	7.2	1.2	4.2	1.2	1.9	.36	2.6	.7	2.1	.5
Observer R																
Uncon.	1.6	.47	11.	1.9	2.5	.87	9.9	1.9	1.8	.65	2.8	1.5	3.4	1.4	6.6	1.4
Blue	1.4	.34	9.7	1.5	2.7	.6	9.4	1.3	2.5	.97	2.2	.84	3.2	.78	5.9	1.5
Green	2.2	1.	10.1	1.3	1.8	.29	9.4	1.6	2.5	.98	2.4	.56	3.4	.56	6.	1.4

TABLE II

Colors: Red and Blue

	Stimulus								After-Image							
	Red				Blue				Green				Yellow			
	L	m v	N	m v	L	m v	N	m v	L	m v	N	m v	L	m v	N	m v
Observer W																
Uncon.	4.7	1.8	5.4	.7	2.2	.9	5.	.8	6.3	1.	2.6	.48	4.9	1.5	1.8	.38
Blue	1.8	.32	7.	.8	2.7	.5	6.8	1.	4.6	.7	2.4	.48	4.7	.8	1.8	1.1
Red	2.2	.42	7.8	1.	1.6	.28	7.6	.88	5.1	1.2	2.6	.48	6.	2.5	2.4	.48
Observer V																
Uncon.	1.8	.58	8.6	1.2	1.8	.24	8.6	1.2	6.1	1.9	6.1	1.1	2.4	.44	3.8	.64
Blue	1.2	.22	6.	.8	4.	.48	7.	1.5	4.2	1.	6.	1.	2.8	.78	3.2	.64
Red	3.	.76	7.	1.6	2.	.68	7.	2.	4.5	1.1	5.2	.8	2.8	.8	1.6	.48
Observer R																
Uncon.	2.9	.84	9.	.8	2.1	.78	8.2	.64	3.5	1.2	5.8	1.5	3.4	.98	6.	2.
Blue	2.9	.62	7.4	1.5	2.3	.54	8.2	1.2	3.2	.26	5.2	2.1	2.6	.32	5.1	1.6
Red	2.5	.53	9.6	1.	1.5	.6	9.4	1.1	3.1	.37	5.8	1.7	3.9	.63	4.4	1.6

ing lengthening of the negative after-image of that color. Only two out of the twelve sets of experiments show any trace of such an effect on the after-image. These are the experiments of observer V., with blue and green, which do give a lengthening of the after-image phase corresponding to the control of the original image. As observer V. was the most successful of the three observers in controlling rivalry, it may be that very complete control of the rivalry in the original image might carry with it an effect on the after-image. V.'s control in the red and blue experiments, however, was just as good, and here there was no influence on the after-image.

On the whole, these results are such as we should expect if motor processes condition rivalry. Evidently, when an observer is directed to make an effort to hold a certain color, there is an amount of muscular strain, in addition to the eye-movements which Breese observed under such circumstances, sufficient to make the motor conditions quite different from what they are when the rivalry is merely observed in a passive attitude. As this latter was the attitude during the course of the after-image, it is only natural that there should be little or no correspondence between the phases of original image and after-image.

Another question which demands an answer is this: What is the duration of the average phases in the *uncontrolled* original image, as compared with that of the average phases of their corresponding after-images? The tables show that in the averages there was a certain amount of concomitant variation. A survey of the first horizontal line of figures under each observer indicates that when in uncontrolled rivalry the red phase, for instance, was on the average longer than the blue phase, the average green phase in the after-image was longer than the yellow one. While this is true of the averages, the detailed records show that it was not true in a majority of the individual experiments, which rather oftener than not fail to point to any correspondence. Perhaps the nearer approach to correspondence between original image and after-image shown in the uncontrolled experiments may be due to the fact that the motor conditions were here more nearly alike in the two halves of an experiment, where no special effort was made in either half; while the fact that a perfect correspondence is very far from existing even in the uncontrolled series may be accounted for by the difference in motor conditions, such as convergence, for example, that must still exist between the part of an experiment where the observer with open eyes looks through a stereoscope, and the part where he watches the after-image changes with closed eyes.

PSYCHOLOGICAL LITERATURE

A Treatise on Facts, or the Weight and Value of Evidence, by C. C. MOORE. Northport, Long Island, N. Y., Edward Thompson Company, 1908. Two vols., pp. clxviii, 730 and 731-1612.

In *Law Notes* for October, 1907, there appeared a rather personal little article by Mr. Charles C. Moore, the author of the work under review, in which the vials of contempt were poured upon Professor Münsterberg's "Nothing but the Truth," a paper which came out in *McClure's Magazine* for September of the same year. I quote a few characteristic passages. "The judge need not exercise, nor even possess, any reasoning faculty. All he needs is faith in the expert. But there's the rub. Infidelity is rife on the bench." "On almost every topic that has a proximate and practical relation to the trustworthiness of testimony delivered in court, the judges have the psychologists 'beaten a mile.'" "We never find a judge citing or quoting a psychologist." "A treatise on Attention and Memory that would be more voluminous than any entire work on Psychology ever written, could be compiled from the opinions of judges." "Suppose a witness declares he did *not* observe an occurrence. By what reliable means would the psychological expert test that witness in support of a theory that he did observe but has forgotten?" And so on.

The 'psychological expert' was not, of course, seriously affected by this criticism. He knew, first of all, how difficult and how unwelcome is the task of popularizing psychology. There is an old and crusted psychology of common sense which everybody carries about with him; so that the psychologist can hardly open a newspaper, or glance through a magazine, or even sit through a dinner, without receiving dogmatic instruction in some field of his specialty. He could admire, then,—this psychological expert,—Professor Münsterberg's courage in attempting to bring into general notice a growing and practically unknown branch of psychological research; and he could make due allowance for the exaggeration into which the attempt at clearness had led the author of the *McClure* articles. Moreover, he knew that the serious work in the *Psychologie der Aussage* had been published in other languages than English; he knew that this work was being taken seriously by the legal profession in other lands; and he knew that the outside reader would look in vain for any account of it in the pages of Mr. Spencer, President Porter, Dr. Ladd, Ribot, or Dr. James, if only for the reason that an author, however well-reputed, cannot give an account of what does not yet exist. And further, he knew, being a psychologist, that every man is tempted to magnify his own profession; he could make allowance for the legal bias of the student of law; he could take Mr. Moore's *sesquipedalia verba* with a grain of tolerant salt. Finally he knew, what perhaps was more important than anything else, the true character of the original *Aussage*-work, its laborious, cautious experimentation and its hesitant, qualified conclusions. He could be content to let the legal critics furiously rage together, in the assured confidence that his methods would ultimately prove themselves and, so far as they should be proved valid, would ultimately win acceptance.

In the meantime, while experimental work upon the psychology of evidence is still going on, Mr. Moore has published his two volumes on Facts, or the Weight and Value of Evidence, legally regarded. Here, then, is the lawyer's substitute for a psychology; or rather, here is his applied psychology, compiled in the main from court decisions, but leavened to some slight extent by quotation from non-legal (even psychological) authorities. To accomplish his task, the author "has scanned page by page some thousands of volumes of reports in the United States, Canada, and England." After a general introduction, he presents his materials under the headings: degree of proof; uncontradicted testimony; incredibilities and improbabilities; sound and hearing; light and sight; taste, smell and touch; distance; speed; the weather; course and bearing of vessels; presumptions, inferences and circumstantial evidence; handwriting and typewriting; observation; memory; age, sex, intelligence or insanity of witnesses; affidavits, sworn pleadings and depositions; credibility of witnesses in general; perjury and mistake; bias of witnesses: impeachment, contradiction and corroboration; declarations, admissions, confessions, oral contracts; positive and negative testimony; demonstrative evidence, experiments and photographs; identification of persons and things; expert and opinion evidence; construction of testimony and sundry topics; and weight of evidence in appellate courts. All this material, occupying (with the footnote references) 1,431 pages, lies before me for review. I have read nine-tenths of it, but I cannot do more than offer a few general remarks.

It is clear that no one but an experienced lawyer could tell, without turning up a multitude of references, whether Mr. Moore's compilation has the supreme virtue of works of its class, the virtue of accuracy. I can make no pretence to a judgment on this point. The volumes give every impression of scrupulous care; and I shall assume that this impression is correct. That being granted, the first thing to say is that Moore on Facts is a legal text-book of an entirely novel kind, and one that should prove of immense value to the lawyer in the practice of his profession. The second thing to say is that the making of the book must have involved a truly appalling amount of labor. "The fact that the topics discussed in these volumes have seldom been treated by text-writers, and even more rarely with any citation of judicial authority, is undoubtedly attributable partly to ignorance of the treasures buried in the reports, and partly to lack of courage and industry to pursue a laborious quest for them." The list of cases quoted occupies 116 pp. of double-columned fine print; the list of legal authorities referred to totals, on a rough count, nearer three than two hundred. And the third thing to say is this: that the layman in matters of law cannot but marvel at the range of cast and the fineness of mesh of the legal net, at the scope of the topics discussed and at the minuteness of detail with which these topics are treated. On these three points Mr. Moore's books are deserving of unstinted praise. Their author has also read widely in non-legal literature, the psychological included; he quotes from writers so recent as Thorndike and Stratton and Baird.

Here, however, is the point at which criticism must set in. Mr. Moore has read widely, but he has not read critically, in non-legal literature. The objection is not serious, since one cannot expect a critical judgment from the non-expert; it is, however, regrettable that advice was not sought from—Professor Münsterberg, or some similarly competent psychologist. Modern psychology is so much a matter of the technical magazines, and so little a thing of text-books, that the uninformed reader is exceedingly likely to go astray. Nevertheless, this objection, as I said, cannot be regarded as serious. Much more important is the fact that Mr. Moore has confined himself exclusively to works and translations in English, and makes no reference whatever to the other modern languages. From the point of view of psychology, and more particularly of the psychology of evidence, this defect is most grave. It is, moreover, a defect that might easily have been overcome by the help of the psychologist *vom Fach*. The literature of the *Aussagepsychologie*—what shall be the English equivalent? the psychology of record, or of report, or of evidence?—this literature is scattered, but it is not bulky.

For the rest, I can, as I have said, only set down a few critical impressions. It is no reflection upon Mr. Moore or upon the law to say that the contents of these volumes are empirical. That, in the nature of the case, they must be. But there can be no doubt that Professor Münsterberg was right in his contention, however severely handled in *Law Notes*, that the applied psychology of the court of justice is "the primitive psychology of common sense." And this it need not be. Mr. Moore's reader is inevitably reminded of the recent controversy between Wundt and Delbrück. The great philologist had remarked, in effect, that he saw a good deal to praise in Wundt's psychology, but that, for his own part, he got along very well with Herbart. And Wundt replied that Herbart was psychologically dead; and that, if the philologists wanted to apply psychology at all, they had better go to something that was alive. Now there is, evidently, a vast deal of applied psychology in the law. Why not, then, apply a modern psychology, a living psychology? Why try to apply that common-sense psychology which it is the first effort of the teacher of psychology to break up in his hearers? Modern psychology has learned, for instance, to draw a clear-cut distinction between experience and report of experience, between the adequacy or reliability of perception, memory, etc., and the adequacy and reliability of the verbal report upon things perceived and things remembered. It has learned, also, to group its materials; it would not have counselled the putting of observation in ch. xvi and the bias of witnesses in ch. xx; or, indeed, the co-ordination of the general method of observation (for observation is simply method) with the specific function of memory. This sort of criticism might be multiplied a hundredfold; but it will do little good until the legal profession shall have been convinced, by aid rendered in concrete instances, that it is at all worth while to transcend the psychology of common sense and to master the rudiments of mental science.

Another general impression that I have gained from the reading of Mr. Moore's volumes is that the position of the expert, in legal regard, is extremely unsatisfactory. First and foremost, I think, because the law makes small distinction between one expert and another, between the empirical familiarity of the handwriting expert and the knowledge of principles possessed by the man of science. But secondly because the expert is often tied down to what I must call illegitimate questions, questions that he would never put to himself and that he can answer, if at all, only conditionally. Every scientific man knows that the outsider's questions are those most difficult to answer; they are for the most part questions of casuistry, and they are more likely than not to presuppose an attitude or a set of postulates which the scientific man cannot adopt or recognize. I feel that I am, in this matter, upon unfamiliar ground, and I hope that I am doing no injustice to legal procedure. My impression is as I have given it; and I cannot help thinking that a body of trained middlemen, versed both in psychology and in the various forms of its application that the law demands, would be as useful here as in education or in psychiatry.

Something, at any rate, is wrong, when Mr. Moore can head a section: 'Expert Opinion Evidence Not a High Grade of Evidence,' and can state that "'expert evidence, while useful in many cases, is dangerous in all,' is a judicial rubric found in substance in many reported opinions." The retort is obvious that this is, itself, an expert opinion, and therefore open to mistrust. But that apart, it is clear that the existence of a body of trained intermediaries between the different sciences and the law would go far to remove "the little confidence [reposed] in the opinions of experts and professors, who often have more knowledge than judgment."

I pass to another point. Modern psychology teaches that the mind is a stream of fluid and changing processes, but that these fluid processes are without exception governed by law, that they evince thorough-going uniformities. A large number of laws or uniformities have already been formulated; and where formulation is still, from our ignorance, impossible, we nevertheless believe that uniformity of occurrence is present. The mind which I seem to see pictured in Mr. Moore's pages is, on the contrary, a static and discrete mind; a mosaic of informations and prejudices; a mind whose furniture, while subject to decay with time, is in meaning and intention permanent, as it were bought to last; and a mind, finally, which is governed by rules that have exceptions. In saying this I am, of course, merely saying in a slightly different form what has already been said: that the psychology of the courts is that of common sense and not of science. The common-sense view of mind comes out, however, with such fatal clearness in Mr. Moore's book that it seems worth while to give this aspect of legal theory a separate mention.

Let me take, by way of illustration, the chapter on memory, which fills 260 pp. and contains more than two thousand citations of cases. The opening section is entitled "The Laws which Regulate the Human Memory." Yet the only law of which one finds mention is the law that memory is obscured by lapse of time. And the passage which the author regards as the most important in this chapter, "or perhaps in the entire work," runs as follows: "Judges prefer to consider all the attending circumstances, and then decide for themselves whether the witness was likely to have remembered, or on the other hand to have overlooked or forgotten." Comment is needless; and it would appear that the author's numerous quotations from psychological text-books are superfluous. As a matter of fact, however, Mr. Moore's opening section does him injustice, for there are many indications of the laws of memory—its dependence on interest and attention, on physical and mental constitution, on age and sex, on the nature and time-relations of stimulus,—in the course of the discussion. As an example of the rule with exceptions we may take the treatment of memory of unpleasant experience. There is a tendency, the author declares, for pleasing reminiscences to take precedence of those that are offensive. *But* there can be no safe conclusion as to the impermanency of disagreeable or painful impressions. I give this illustration, not because psychology has established the laws of memory in respect of pleasantness and unpleasantness, but precisely for the opposite reason, that it has not, and for the collateral reason that it very well can. Here is just the place where the expert, the psychologist of application, might step in, to discover the uniformities that underlie the rule-with-exception of the courts. The unsatisfactoriness of that rule can hardly escape the notice even of the most empirically inclined.

In the above reflections I have been criticising the present status of legal theory and practice rather than the contents of Mr. Moore's work. My excuse must be that he himself challenged such criticism in his article in *Law Notes*. He invited a comparison between the psychology of memory and attention, as set forth by the psychologist, and its psychology as set forth in the law reports. There is, in fact, no comparison. The psychology of the reports is scrappy and out-of-date. This verdict is less harsh than it sounds, since the same thing might be said of a good deal of recent educational psychology, medical psychology, linguistic psychology, social psychology. All these psychologies, however, are emerging, more or less quickly, from the shadow of tradition and common sense; and with the appearance of the *Aussagepsychologie* the practical psychology of testimony is on the path of progress. Meanwhile, let no jurist suppose that the psychologist underestimates either the magnitude of the task before him or the high level of empirical achievement to which the legal profession has attained. Mr. Moore's work amply testifies, both to his own training and industry, and to the mental caliber of the judges whose views he has assembled. P. E. WINTER.

Buddhism and Immortality. The Ingersoll Lecture, 1908. By WILLIAM STURGIS BIGELOW. Boston and New York, Houghton Mifflin Co., 1908. pp. 75. Price c. 75.

The lecture outlines the teaching of the Buddhism of Northern India and Japan, which is closely allied to Brahminism, as regards human immortality. Briefly put, that teaching is that the self is a resultant of mortal and immortal factors, the former being sensations and emotions, and the latter the will. There is constant conflict between the two opposed forces, and we may assist our own progress towards immortality either indirectly, by the performance of good actions, or directly, by the turning of will upon character. After life in this world we pass to a sort of angelic state, where existence is still personal; and beyond that we reach the ultimate, impersonal peace of nirvana.

The point of greatest interest to the psychologist is, perhaps, the writer's account of the transmigration or re-birth of souls. Sensations are perishable, but sensations often repeated lead to habitual, automatic and reflex action; they thus gain in length of life. Western science then speaks of heredity or atavism, the persistence of a parental or ancestral type; Eastern thought finds illustration of re-birth or re-incarnation. In comparing the two views, Mr Bigelow writes as follows.

"First. If material constitution, that is, inheritance . . modified by the tendency to variation, is the cause of character, then, as the laws of matter do not vary, we have no way of accounting for the tendency to variation itself . . . . whereas, if the psychical characteristics . . . are the dominant factor, the tendency . . . follows as a matter of course.

"Second. If material constitution is the cause of character, the range of variation ought to be equally great in different forms of animal life. . . Whereas [if the soul is dominant], we ought to find the greatest variation where the characters are most complex,—which we do.

"Third. Family resemblance often asserts itself most clearly in the second generation. [And it appears most clearly when the grandparent has been dead less than ten years. Moreover, it appears once only, however many the grandchildren.] Heredity by transmission offers no explanation of either fact. Whereas, from three to ten years is the ordinary interval for re-incarnation, and the single resemblance is the natural result of the re-birth of a single soul." Mendel's Law?

"But it is, perhaps, too early to be sure just what is behind Mendel's law." M. W. WISEMAN.

Die Prinzipien und Methoden der Intelligenzprüfung. Von TH. ZIEHEN. Berlin, S. Karger, 1908. pp. 61.

A reprint, with notes, of a lecture read before the International Congress of Psychiatry at Amsterdam, 1907. The lecture gives an interesting account of the methods used in the testing of defective intelligence in Professor Ziehen's Berlin clinic, accompanied by psychological commentary. First in order stand the tests of retention or deposition: here the author discusses the value for the test of school knowledge *versus* everyday knowledge, the difference between retention of single ideas and retention of ideational complexes, the status of retention in the normal uneducated individual as referred to his everyday knowledge, the relation of retention to *Merkfähigkeit*, etc. A characteristic test is the following: First, a problem in the simple multiplication of one-place figures is given; then six one-place numbers are read off, and the patient repeats them; then six other numbers are read off, and repeated as before; and, finally, the patient is asked to recall the problem set him at the beginning of the test. A typical visual test, in which a geometrical figure is shown for 15 sec. and drawn from memory after an interval of 15 sec., is also described. Secondly, the author speaks of tests of ideational development and differentiation. He instances typical questions for the testing of power of generalization and specification, typical stories told for the testing of the patient's capacity for abstract ideas, questions whose answers presuppose analysis or synthesis or discrimination, etc. Questions that call for a definition he regards as, in general, unsuitable. Thirdly, we have tests of reproduction, carried out by the ordinary methods of the association experiment. Fourthly and lastly come tests of combination. These have a wide range: tests of the patient's orientation in his novel surroundings, tests of inversion of association (months said backwards, *e g.*), tests with puzzle blocks, simple arithmetical tests (easy equations, rule of three), Ebbinghaus' mutilated texts, reproduction of the main feature or main causal relation embodied in a story or picture.

As important conclusions we may single out the following. The question of time, of the rate of intellectual achievement, plays but an inconsiderable part in clinical investigation of the kind here described. There is no certain method of eliminating the influence of grave derangements of association and of emotion. Every test of intelligence should be preceded by a test of attention (cancellation of letters, tachistoscope, mean variation).

Psychologically, the lecture appears somewhat too formal and too clean-cut in its distinctions. The psychology of intellect is still in a very backward condition. To the practical psychiatrist, on the other hand, Professor Ziehen's descriptions and comments will be exceedingly useful. It seems possible, too, that the careful sifting out of tests of defectives may leave a remainder, of valid forms of test, which will be of service to normal psychology as indicating the principal easily differentiable aspects of intellectual function, and thus furnishing rubrics for the study of the normal subject. L. TURLEY.

La pathologie de l'attention. Par N. VASCHIDE et R. MEUNIER. Bibliothèque de psychologie expérimentale et de métapsychie. Paris, Librarie Bloud et Cie, 1908. pp. 117.

The authors of this little essay begin by asserting that we have as yet no normal psychology of attention, and that it will be useful to bring together the experimental results derived from the study of the attention in abnormal cases. They point out that the only writer upon the psychology of attention who has devoted thought and space to the question of the abnormal is Ribot, and they accordingly summarize the treatment of the subject found in the *Psychologie de l'attention* of that author. The body of the essay is then taken up with the exposition, along with more or less of running commentary, of the methods (reaction time, perimetrical measurement, mental work, fluctuation, æsthesiometry, etc.) employed by various investigators of abnormal attention from de Sanctis to Consoni, Marie, Wiersma, etc. Special chapters are assigned to the work of A Rémond at Nancy (1888) and of Raymond and Janet at the Salpêtrière. The names of experimenters, state of their subjects, and reported state of attention, are brought together in a useful chronological table on pp. 108-110. The authors phrase their conclusions as follows. (1) All states of intellectual inferiority, congenital or acquired, stationary or progressive, are accompanied by hypoprosexia. (2) Certain neuropathic states may be accompanied, at least momentarily and exceptionally, by a hyperprosexia, which is itself a condition and not a disease. (3) All mental disorders that take the form of delirium are characterized by paraprosexia. With the experimental means now at our disposal, we cannot give quantitative expression to these qualitatively different morbid or abnormal states of attention. (4) Distraction is either merely a transient hypoprosexia, or an incapacity of attention to maintain itself under some determinate form, *i.e.*, a mental disorientation.

It is clear that these conclusions are very general, and that they give us little insight into the mechanism of disordered attention. Of attention itself we are told only that it stands to the intelligence as reflex irritability stands to the nervous system, and that it presents many different aspects, according as it is spontaneous or voluntary, conscious or automatic, emotive or intellectual, and in these cases conscious or subconscious, etc. A deeper going analysis may be expected from the forthcoming *Psychologie de l'attention* by M. Rivière.

W. JENKINS.

Notes on the Development of a Child. II. *The Development of the Senses in the First Three Years of Childhood*, by MILICENT WASHBURN SHINN. The University Press, Berkeley, Cal., 1907. 258 p.

In this, the second volume of her Notes on the Development of a Child, Dr. Shinn has made a most important contribution to the psychology of infancy. While the source of the original data for the work, as in the case of the author's earlier publications, is the carefully kept record of her own observations upon her niece, this has been supplemented by not only the few scientific records which are available in printed form but by a number of manuscript records obtained through the agency of the Association of Collegiate Alumnæ, of whose child-study committee Dr. Shinn has for many years been chairman. The result of this carefully collated material is the most systematic and complete record of the development of the senses that has yet been contributed to psychology. In the introduction there is a brief discussion of the methods of child study, the principles of classification which have been used by different students of child psychology, including the author's own, of which the guiding idea is that of "*a progressive movement consisting of the integration of simpler activities into more complex and the differentiation of specialized ones out of generalized*, and an anticipatory summary of conclusions.

Under the principle above mentioned the book is divided into four parts. Part I deals with the sensibility of the new born, bringing together and tabulating for comparison the tests and observations of the best workers in this field on visual, auditory, dermal, taste, smell, muscular, organic and general sensations. The following conclusions are reached.

The condition of the senses in normal infants at birth is as follows:

"1. *Sight* is only a dim and passive feeling of light and dark, without sense of distance or direction.

2. "*Hearing* appears with more or less delay and then only as a dull sensibility to auditory jar, rather than to sound.

"3. *Dermal feeling* includes a sensibility to *contact*, lively about the face (especially the lips and eyes) and duller over the rest of the body; a lively sensibility to decidedly *cold* touches though scarcely to diffused cold; and an exceedingly dull sensibility to pain."

"4. *Taste* and *smell* appear under normal conditions to be almost wholly wanting but can be excited by intensive artificial stimuli. There are also indications that taste, smell and touch are not perfectly differentiated.

5. "Sensations of *motion*, *muscular activity*, *fatigue* and *equilibrium*, *hunger*, *thirst* and *organic pain*, are distinctly felt, but are probably individually feeble. There may be also a faint undercurrent of *general sensation*.

"6. The content of consciousness in the new born is chiefly made up of the sensation of light and of touch and muscular sensations about the mouth and eyes."

These early experiences unassociated with each other or with reproductions of their earlier recurrence, represent the simplest form of consciousness and, since each has its own specific quality, furnish the material for the development of a complex psychic life. At this stage Dr. Shinn finds nothing that "parallels any phylogenetic stage of sense development though traces of the phylogenetic order are evident in the imperfect differentiation of smell, taste and touch, in the low sensibility, to dermal pain and the delayed appearance of cochlear hearing.

Part II discusses the synthesis of sense experience, through which the isolated sensations become fused, grouped and organized into a unified "stream of consciousness." The main lines of this fusion and grouping are as follows:

1. In eye movements, associations are formed between the muscular and visual sensations in the automatic movements of eyes, by means of which the power of voluntarily directing and accommodating the eyes to distance is attained and this is followed by the ability to trace outlines with the eyes leading to the perception of plane form, and rudimentary perceptions of distance, direction and objects as such.

2. During this same period a series of associations is formed between tactile and muscular sensations, especially those which occur in the automatic graspings of the mouth and hand, through which the power of active touch is attained.

3. By a complex process of association, the whole visual-motor group of sensations becomes linked with the tactile motor group experienced in grasping, and through this, aided by locomotion, the perception of solid form and the space-feeling itself are slowly acquired.

4. Auditory sensations become associated with visual and muscular sensations, and associations are also formed between sounds heard and the vocal organs producing them, thus laying the foundation for the development of speech. Admirable summaries and tables of the stages of learning to see, of the development of the active use of the touch organs, and their synthesis are given at the conclusion of these chapters. The development of the auditory sensations, the associ-

tion of sound with sight, and with sound-producing movements are likewise discussed in detail and short chapters are devoted to the associations of the minor special senses and to the development of the feeling of the bodily self, through the growing consciousness of muscular control, equilibrium feelings, exploration of the body surface and organic and general sensations.

Part III, deals with development in descrimination and interpretation and here comes the chapter on the development of color vision in infants, the largest contribution which has yet been made to this almost *terra incognita* of psychologists. As a result of her own observations and their agreement with the reports of other observers, Dr. Shinn concludes that, for at least three weeks after birth, a baby does not see color. Even after sensibility to light had become marked she found no reaction to even the most brilliant field of color. The questions next considered are: (1.) How long does this condition of color blindness last? (2.) By what steps does the child emerge from it? (3.) To what extent dces color perception become developed during the first three years of life? To these questions correspond roughly three stages of the development of color vision in the child. The first is that in which there is no evidence of discrimination and the question to be tested is whether the child really *sees* color or not. Here brightness and contrast must be sharply discriminated from color in order that reactions to color surfaces may not be mistaken for reactions to color, when, in fact they are merely reactions to light. This period extends over the first year and a quarter. In the second period, to be looked for somewhere about the middle of the second year, signs of definite color concepts should appear, if they exist, though this is, of course, quite apart from any ability to understand or use the color names. In the third period, beginning in the latter part of the second year or certainly early in the third, the problem is to ascertain how far the color vision of the child is identical with that of the adult or how far it may still be limited. But few series of experiments have been made on the color vision of young children, those of Prof. Baldwin, covering the ages from 9 to 15 mos. being the first systematic ones. With these the author has compared her own careful experiments and others to which she has had access in manuscript form. The conclusions which she has reached through these data are best given in her own summary.

"The subject is far from being cleared up; but we are at least justified in the following conclusions:

First. The child is insensitive to color at birth and may continue so for several months, though here we have only negative evidence.

Second. Feeble color sensations, beginning at the lower end of the spectrum, and developing progressively upward begin to be felt, certainly within the second half year, and perhaps earlier. These include all the long-wave color sensations by the end of the first year, and probably short wave ones follow soon after, but there is no actual proof of the existence of these before the 18th month.

Third. By the third year, it may be by the latter part of the second, the child has all the color perceptions of the adult, and can be taught to name and discriminate them quite perfectly, and to notice color in the world about him.

Fourth. Colors are seen by the infant more feebly than by the adult as if in a lower illumination. This difference grows progressively less and has nearly disappeared by the third year. It is probably due in part, at least, to the restricted area of the color-sensitive tract of the retina.

Fifth. Pleasure in light precedes pleasure in color; next pleasure in color appears, depending jointly on their light-richness and their "warmth;" but by the third year the warm colors in some cases lose their advantage, and the cold ones may give as much pleasure. But for color *harmony* no feeling is to be found in the little child."

In this chapter the various space concepts of form, distance, size, direction, locality and solidity, and the child's understanding of pictures are also discussed.

Discrimination in hearing, musical sensibility, sense of rhythm, spontaneous musical expression, and other allied topics, each receive due consideration in the chapter on hearing; touch and the minor dermal senses, taste, smell, muscular, organic, and general sensations are all included among the topics discussed in Part III and the recapitulation of this section, which does not lend itself well to tabulation, is given in 6 month periods, making it exceedingly convenient for reference.

Part IV gives the pedagogical results. These have been foreshadowed by the previous chapters and may be very brifly summarized as follows. In the earliest stage of development, that which comes before grasping, the human presence seems to be the one thing of educational value. As the baby's powers develop the line must be carefully drawn between neglecting to provide necessary stimulus and the danger of overstimulation. In general, the largest possibility of free action is the secret of wholsome and happy development. During the first three years it is in only a slight degree that any formal education can be begun and yet in this period certain psychological foundations for the future may be laid, and a few simple but valuable suggestions are given illustrative of the general principles of such informal instruction.

As a whole the book represents an amount of scientific work and thoroughness that place it in the front rank of psychological investigation. THEODATE L. SMITH.

Ueber Lesen und Rezitieren in ihren Beziehungen zum Gedächtniss, von L. WITASEK. Zeitschr. f. Psychol. u. Physiol. d. Sin., 44 Bd., 1907, pp. 161-185, 246-282.

Groups of ten nonsense syllables were presented visually at the rate of one syllable per second to seven adult observers, who read them aloud in trochaic rhythm. In reciting a group from memory the observer was given the first syllable, and he attempted to recite the remainder in the same rhythm and at the same rate used in the presentation. In case of failure to recall a given syllable, ten seconds were allowed, and then the correct syllable was given him, with the repetition of which he continued the recitation. Designating the number of successive readings in the presentation of a group by Roman numerals, and the number of successive recitations by Arabic gives the following twelve series of different combinations of readings and recitations that were used.

VI+0	VI+5	VI+10	VI+15
XI+0	XI+5	XI+10	XI+15
XVI+0	XVI+5	XVI+10	
XXI+0			

An hour after giving a series it was re-learned through recitations alone, carried out in the same manner as before and repeated to a point where the observer recited the group in ten seconds without error. We may call these the *re-learning*, and the former the *learning* recitations. The degree of memory induced at any point was measured by the time taken to recite, and by the number of forgotten syllables or number of times aid was required in a recitation.

In agreement with results of previous investigations the author found that the impression value (effectiveness for memory) of successive readings decreased very rapidly, this holding true in both the learning and the re-learning recitations. The impression value of successive recitations decreased quite in the same manner. The impression value of a recitation in inducing immediate memory as compared with that of reading far exceeded the latter in all cases. It was greater after the hour's interval when the initial degree of impression already present had been obtained by reading and recitation instead of by reading alone. Likewise, the memory permanency, as measured by the first recitation alone after the hour's interval, was greater for the series that included recitation with the reading. The combination of readings and recitations that resulted in the quickest re-learning showed a complex order, from which the following conclusion is drawn: (1) The optimum total time for reading equals about one-fourth of the total time spent on reading and recitation together. (2) The optimum division of the total time spent for recitation between learning recitation and re-learning recitation is one that gives less than half to the former.

The figures on which these generalizations are based are in most cases derived in several different ways, the possibility of which may be seen from the nature of the series that were given.

F. KUHLMANN.

Expériences sur le rôle de la récitation comme facteur de la mémorisation, par M. DIMITRE KATZAROFF. Archives de Psychologie, 1908. pp. 225-258.

Couplets of nonsense syllables, eight to ten in a group, were presented visually at the rate of one couplet per two seconds to six adult observers, who read the syllables aloud in trochaic rhythm. The presentation of a group was combined with recitations in which the first syllable of a couplet was presented visually, while the observer tried to recall the other, and failing to do so was given the term orally. The final recall, in which the first term of a couplet was again presented as in a recitation, followed 24, 48, or 72 hours after the learning. Using Roman numerals to designate the number of consecutive presentations and Arabic figures for the number of recitations, gives the following combinations, with the intervals before recall, that were employed:

Combinations				
A	B	C	D	E
X+1+V	VIII+VII	VIII+VII	IV+VI	IV+VI
X+1+5	VIII+7	VIII+1+VI	IV+6	IV+3+III
			IV+1+I+1+I+1+I	IV+1+I+1+I+1+I
Intervals				
48h.	72h.	72h.	24h.	24h.

The degree of memory induced in the different series was measured by the number of terms recalled, and by the time taken to recall one. Thus measured, the author found that the fixation value of a recitation is greater in all cases than that of a presentation. Syllables that were recalled in the first recitation after the first group of presentations (four to ten) were remembered best, in the final recall, when the relative number of subsequent recitations was greatest. This held true also for syllables that were not recalled in the first recitation, but were recalled in the later ones. The number of syllables recalled in the first recitation but forgotten in the final recall, was greater than the number not recalled in the first recitation, but learned later and remembered in the final recall. This is explained by assuming that the first recitation in a series shows the observer what he has learned and what he has not learned, resulting in a subsequent concentration of the attention on the latter and a neglect of the former. The best combination of presentations and recitations seems to be one that includes at least two recitations, one for showing the observer what he has already learned and another for memorizing proper, this number depending on the amount of material and the number of previous presentations. Then at least two readings should follow to reinforce the perception of the material as a whole. The recitations should come together, instead of alternately with presentations. Further explanation of the superiority of the recitations is found in (1) a difference in the attitude of the observer in the presentations and the recitations; (2) in the unfamiliarity of an isolated term of a couplet when it has not been presented alone before as in a recitation, (3) in the fact that the neural processes are probably the same in the recitation and the recall, while in a presentation they differ from both. F. KUHLMANN.

Ueber Vorstellungstypen, von L. PFEIFFER. Pädagogische Monographien, II, Bd., 1907, pp, 1-127.

The first eighty-five pages of this monograph are given to review and discussion. The methods and results of investigations on ideational types in verbal, and in concrete thinking are comprehensively and critically considered. This includes studies whose aim and methods were not primarily directed to the investigation of ideational types, but from whose results some inferences as to ideational types can be made. The next twenty-eight pages are given to the presentation of results of the author's own experiments, followed by a discussion of the theory of ideational types, and of their practical significance to the teacher.

The experiment consisted of the subjects' writing down the first suggestion (indicating also the nature of the imagery) aroused by a word pronounced to them by the experimenter. Four classes of words were used, a group of ten of one class being presented at one sitting per week. (1) Nouns with a predominant visual and auditory content. (2) Verbs with a predominant visual and auditory content. (3) Nouns with a predominant kinæsthetic content. (4) Verbs with a predominant kinæsthetic content. Two groups of ten words of each class were used. The test was made the first year on a class of fifteen girls with an average age of ten years. It was repeated on the same girls (with several changes in the class) the second and third years, using the same words as material. Classifying the suggestions aroused by these words as visual, auditory, and kinæsthetic gave the following percentages belonging to each:

	Visual	Auditory	Kinæsthetic
1st year	56	31	12
2nd "	47	31	21
3rd "	50	30	20

It is to be remembered that half the words used were designed to suggest kinæsthetic imagery. Taking the middle values for the class for the number of times each kind of imagery was suggested gave

	Visual	Auditory	Kinæsthetic
1st year	44	25	10
2nd "	39	26	17
3rd "	40	23	14

Taking these middle values as a norm gave the following percentages of the number of children whose imagery exceeded this norm in the three classes:

	Visual	Auditory	Kinæsthetic
1st year	47	33	20
2nd "	40	25	35
3rd "	47	18	35

The author concludes that the ideational type is fairly constant, but discusses the small changes that, according to the figures, occurred. The monograph is a valuable one chiefly for its critical survey of methods and previous results. F. KUHLMANN.

The Nervous Correlate of Pleasantness and Unpleasantness, by M. MEYER, Psychological Review, July and September, 1908.

The author considers that the present confusion in the psychology of feeling is due in no small measure to the lack of attempts to determine the nervous correlate of feeling, which for him is the same as the nervous correlate of pleasantness-unpleasantness. To emphasize this confusion he summarizes the views of Lagerborg, Marshall, Stumpf, Fite, Lipps, Alechsieff, Miss Calkins and Pikler.

He then prepares the way by a theory of the structure and function of the nervous system, stated in mechanical terms, the essential point of which is a comparison of the nervous system to a very complex system of pipes filled with fluid, so interconnected through higher centres that an impulse given to the contained fluid at any point can be transmitted through the ramifications of the pipes to any other point. It is assumed that the resistance of a pipe or series of pipes often used will decrease, while that of a series seldom used tends to increase. If, for any reason, two stimuli varying in intensity are given simultaneously, the more intense tends to attract the lesser to its own path, whose resistance is thus decreased until it becomes the path preferred whenever possible, causing thus possibly a motor reaction different from that expected. From this point of view the author explains the phenomena of habit, variation and sensory and motor condensation.

This current within the system of pipes is evidently the nervous correlate of sensation. But feelings of pleasantness-unpleasantness arise only when two simultaneously existing currents meet in the higher centres in such a way that the total activity is increased, (causing pleasantness), or decreased (causing unpleasantness). The more complicated the structure, the more opportunity for pleasantness-unpleasantness to arise; it thus belongs in its definite form to a high stage of evolution.

The author points out that this view explains the fact that certain usually unpleasant sensations may through habit or purpose become pleasant, and *vice versa*. It also explains the lack of images of pleasantness-unpleasantness as well as the fact that these states cannot occur without perception; but conceivably, through complexity of structure, pleasantness and unpleasantness may exist at the same time. Emotions, according to this view, are not entirely derivable from pleasantness-unpleasantness, and may, indeed, exist without them. It is also evident that pleasantness and unpleasantness are not, and cannot become, sensations. H. W. CHASE.

A Theory of Mind, by JOHN LEWIS MARCH. Scribner's Sons, New York, 1908. 453 p.

The writer tells us "that many believe that the next great advance should take place in psychology, and that this advance should be the result of a clarification of the field chiefly by the modern science of biology," which has hitherto had an extraordinarily slight influence in this direction. "Biology and psychology still stand almost rigidly apart." The fault, however, is with the psychologists, and the author here proposes a theory of instinct intended to meet this situation and show what the fundamental misconception has been and in so doing to break the deadlock and allow the scientists to come together. In the words of the author, the theory "has been accomplished almost in solitude and I have little to say in the way of special acknowledgments." He assumes that both mind and matter are found in unities that are similarly limited in space and in their complex forms in time. The simpler unities of mind are found in connection with the simpler unities of matter, and the complex unities of mind with the complex unities of matter. Mind is not known to exist apart from matter. The phenomenon of the latter can be fully explained with no reference to mind, which knows nothing of matter except through experience. Mind seems affected only through physical means. Instead of saying of matter and mind that either controls the other it is better to say that matter to itself is mind, mind as it reveals itself to another mind is matter, and so they are thus to be considered as identical. The atom has an impulse to fuse with certain other atoms, to influence and be influenced by them. Now an impulse is identical with the setting free of force. The influence emitted in an impulse is such as to bring about a complimentary impulse in a suitable other mind and to make this impulse definite. The act of satisfying an impulse is identical with the movement in space, and the force tends to the motion necessary to bring about the satisfaction of the impulse. When an impulse is satisfied or its results broken up, the equilibrium of forces is accompanied by a feeling of pleasure or pain. In the building up of mind, impulses are specialized out of more generic ones by fusion and subfusions. The results show that memories are not inherited but impulses are. Instincts are thus not the results of experience but are original. Instincts are in a general way similar, and many of our impulses are satisfied indirectly or are not entirly satisfied. We have a cell material and a cell personal instinct, a body material, personal and social instinct, recognition instincts, thought instincts, ideal instincts and many combinations of these. Thus out of the individual, whether cell or personal, higher unities are ever being evolved.

The writer seems to us to be correct in recognizing the biological foundations of psychology and also in his feeling that when, in the good time coming, human instincts shall be treated in the same objective way as those of animals, the chasm between the two will close. But his method seems to us abstract, formal, and far too little illuminated or even informed by facts. To our thinking, the way to demonstrate his fundamental thesis would be to parallel, step by step, the latest situations, for instance, of human society, and those of animal social organizations from the ant up, and so with all the rest; and the success of such an effort will depend entirely upon the mastery of the facts in both these fields, and that this author hardly seems to us to possess.

Principles of Psychic Philosophy, by CHARLES B. NEWCOMB. Lothrop, Lee, & Shepard Co., Boston, 1908. 199 p.

This book is dedicated to those "who are beginning to understand that in this present mortal life man has the opportunity of unfolding all the powers and realizing all the privileges of any spiritual plane." To realize these higher powers of man the author suggests that "two days be given to the careful consideration of each chapter." This would require twenty-four days as there are twelve chapters, viz., God, Nature, Man, Psychism, Suffering, Selfishness, Responsibility, Adjustment, Power, Freedom, Healing and Fulfillment." If we take the first chapter as an example, we must recognize everlasting arms beneath us; a universal force superior to ourselves governing life, intelligent, benevolent, supreme, silent, irresistible, must discover every point of development. The telescope and microscope are harmonious. Supreme good, love, wisdom, law, are synonyms. Every alternative is between law or chaos, design or accident; and GOD GEOMETRIZES. This will perhaps suffice to show the general point of view of the author and his method. He wishes every one to achieve the highest good, realize the highest knowledge, evolve himself or herself to the uttermost, and the method of accomplishing these magnificent ends is to meditate along the lines of his pregnant sentences until all the truth that they contain is irresistibly borne in upon the soul, which will thereby be greatly advanced toward its goal.

Ueber Theodor Lipps' Versuch einer Theorie des Willens. Eine kritische Untersuchung, zugleich ein Beitrag zu einer dynamischen Psychologie, by JULIUS PIKLER. Barth, Leipzig, 1908. 50 p.

This critical investigation is at the same time a contribution to a dynamic psychology and is directed chiefly against Lipps's "*Vom Fühlen, Wollen und Denken.*" The author expresses the wish that his treatise be read especially by those who have already perused his work on "*Das Beharren und die Gegensätzlichkeit des Erdlebens.*" He then proceeds to treat, 1—Striving and the conviction of possibility, 2—Striving as the actuality of objective tendency to realization. And in a supplement he treats of the relation between ideas and experience.

Der Begriff des Ideals; eine historisch-psychologische Analyse. WILHELM ENGELMANN, Leipzig, 1908. 136 p.

This historico-psychological analysis is a doctor's thesis at Erlangen. The writer first traces the history of the various theories of the ideal beginning with Spinoza and ending with Ribot, Wentscher, Cohen and Ricardo. The author thinks light can be shed upon his general problem by means of two questionnaires which he appends, both of which cover several pages and are too lengthy to be reproduced here. He wishes to know how many people think seriously over sense impression, whether they love society or solitude, are fond of expressing themselves, really suffer from doubt, what kind of a youth they spent. This to the end of getting their personal ideals. The second questionnaire is characterological and pertains to selfishness, pedantry, faithful memory, presence of mind, casuistry, fancy, receptivity, excitability, friendship. To these are appended other queries of a still more detailed character, as to the attitude of the individual toward ideals.

Vortex Philosophy, or the Geometry of Science, by C. S. Wake. Published by the author, Chicago, 1907. 36 p. Diagrammatically illustrated.

This is a geometry of science, diagrammatically illustrated. It has been evolving in the author's mind since 1892 and was suggested by Mr. J. J. Van Nostrand. This is the epitome of a manuscript of 600 pages that was burned, and hence there is much for which the scientific data is not given in this pamphlet. The author expresses some obligation to Ribot, Haeckel and Royce and has a mechanical device illustrating his theory. This paper is illustrated by many rather intricate cuts and diagrams, some of them colored, and everything from the motion of primitive elements up to sexuality is explained.

Questions in General and Educational Psychology, by GUY MONTROSE WHIPPLE. C. W. Bardeen, Syracuse, N. Y., 1908. 197 p. (Cornell Study Bulletins for Teachers, No. 3.)

The range of these questions is wide and interesting. It covers the nature and scope of psychology, mind and body, heredity, acquired forms of response, sensory-motor mechanism, the general principles of mental elaboration, perception and observation, memory, imagination, conception and language, judgment, apperception, the affective process, formal discipline or general training, abnormal and anomalous psychoses. This bulletin is the outgrowth of an attempt on the part of the writer to supply his students with the means for checking up their progress in psychology, and for securing their intelligent assimilation of the material supplied by lectures, reading, and classroom discussions. Believing as we do that this is a very great need in the teaching of this subject at the present time, especially in view of the prevalence of the lecture method, we can but welcome this text, even though it seems to us, from a rather cursory reading, to be somewhat uneven in its merits.

Ethics, by JOHN DEWEY and JAMES H. TUFTS. Henry Holt & Co., New York, 1908. 618 p.

This text strives to awaken a vital conviction of the general reality of moral problems and the value of reflective thought in dealing with them. The historical material in Part I is subordinated to this end. Part II is devoted to different types of theoretical interpretation and Part III to typical social and economic problems which characterize the present. It is hard for the student of morals to get the subject objectively and definitely before him, so that the problems seem real. Conduct is so intimate that it is hard to analyze. Hence all must be concrete, and yet the classical conceptions of moral theory are of remarkable importance in illuminating the obscure places of the moral life and in giving the student clues that will enable him to explore it for himself. The authors do not aim to inculate a ready-made system, and they recognize that in the political and economic portions of Part III no definite treatment is yet possible. It should be added that the first two hundred pages, or Part I, are by Professor Tufts; and the third part (pp. 427-606) by both authors. For each main topic, a list of important literature is given, and there is a well-made index.

Psychotherapy. A course of reading. Combining sound psychology, sound medicine and sound religion. Vol. I, No. 1. Centre Publishing Co., New York, 1908. 100 p.

This admirably printed journal as edited by W. B. Parker and published by the Centre Publishing Company, 30 Church Street, New York, contains articles by Dr. Richard C. Cabot, J. J. Putnam, Rev. L. W. Batten, Prof. J. R. Angel, F. T. Stimpson and Rev. Lyman P. Powell. Others whose co-operation is enlisted are Prof. Jastrow, Bishop Fallows, Prof. Royce, Prof. Woodworth, and Dr. F. Peterson. The opening article is by Dr. Cabot, who speaks of psychotherapy in Europe, strangely enough, without even a mention of the Freud school, but declares that psychotherapy "has its place, not instead of, but by the side of, chemical and physical methods." He advises those interested to associate with the Harvard psychologists and thinks we need team work. Dr. J. J. Putnam holds that man is a psychophysical organism, that the right philosophy is important, that materialism and pessimism are current and should be resisted, refers to the work of Loeb, Sherrington, Bergson, and also has a word to say about pragmatism, will, faith, etc. Another article by Dr. Batten characterizes healing in the Old Testament. Professor Angel treats of the relations between mind and body, especially the brain, and concludes that mental healing is possible and that its methods deserve recognition, emphasizing suggestion and also speaking of caution in view of dangers. Dr. Stimpson then describes the nervous system beginning with the neurone and then characterizing the brain as a democracy, speaking of evolutionary centres, the powers of those that are high and low, brain architecture, influence of conscious mind on lower centres, the secondary self or dual personality. And finally Dr. Powell describes psychotherapy in Northampton, how he applied the Emmanuel method and some of the results achieved, among which was one hopeless case.

All these articles are broken up into brief sections with captions, are accompanied by notes, collateral reading, editorial summaries, etc., so that it is all brought down to the most elemental mind by all kinds of notes; and a glossary of technical terms, such as catalepsy, cell, cerebellum, cerebral, cerebrum, ganglia, neural, prognosis, phonation, soporific, sensory nerves, etc., follows.

Das Sexualleben des Kindes, von DR. ALBERT MOLL. Walther, Berlin, 1909. 313 p.

Moll ranks with Havelock Ellis, Krafft-Ebing, and Freud perhaps in expert knowledge of this subject. After a historical introduction, the subjects treated are as follows: the organs and the instinct of sex; sexual differences in childhood; symptomatology, pathology, etiology, and diagnostics; the meaning of sexual life for the child; the child as an object of sexual treatment; education in sex. The topics are treated in a very practical and able way.

Das Sexualleben der Australier und Ozeanier, von DR. B. SCHIDLOF. Hallberg, Leipzig, 1908. 314 p.

This is a very painstaking and luminous contribution to what the writer calls "the new science of sexual psychology." The chief topics discusssd are: the sexual life of early childhood, the ceremonies of pubertal initiation for boys and for girls, the sense of shame and modesty, prostitution and concubinage, sexual aberrations, diseases, ideas of beauty, erroticism in costume, dancing, song, love charms, virginity, marriage and its forms and violations.

Race or Mongrel, by ALFRED P. SCHULTZ. L. C. Page & Co., Boston, 1908. 396 p.

This is a brief history of the rise and fall of the ancient races of the earth, and a theory that the fall of nations is due to intermarriage with alien stocks. The argument is that national strength is due to racial purity, and the writer concludes that America will sink to early decay unless immigration is rigidly restricted. The twenty-nine chapters take a wide range, from the Hamites, Phœnicians, Egyptians, Jews, Gypsies, Greeks and Lombards to the Anglo-Saxons, the present yellow races, the German-Americans, the pan-Europeans in this country, and the American negro. No historic race was ever destroyed by inbreeding; and no race that practiced it was ever destroyed from any cause. The English to-day are the strongest European race. They crossed with Danes and Normans but the immigration never amounted to an inundation and the immigrants were pure and closely related, while the absorption was slow. Race is everything. A world language is bad because it bastardizes those who speak it, and every man's tongue is no man's tongue. It is a language spoken by mongrels, and every mongrel is worthless. If uniformity, eternal peace and bastardization of all nations is devoutly wished, then let us spread the English language.

Le Spiritisme dans ses Rapports avec la Folie, by MARCEL VIOLLET. Librarie Bloud & Cie., Paris, 1908. 117 p. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

The first chapter considers spirits—normal, predisposed and foolish; in successive chapters, the phenomena and the doctrine of spiritism are discussed; later comes mediumopathy, external and internal, and mediumomaniacs. The insanity of spiritism is classified as 1. deliriums of spiritistic origin; 2. follies not of spiritistic origin but colored with delirium. Then follow certain conclusions.

Les Hallucinations Télépathiques, par N. VASCHIDE. Librarie Bloud & Cie., Paris, 1908. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

This is a very interesting study which the author carried on for nine years on two series of people: the first 21, and the last 13, in number, using in all, in the first case, 1,011, and in the last, 363 determinations. He found many veracious cases, but strongly advocates the view that there are no spiritual agents involved, but that credulity, the social *milieu*, cleverness and experience, and malobservation on the part of the beholder, are sufficient to account for all the facts. The lowest class of people in France are just nine times as prone to believe in supernatural interpretation as the most cultivated class.

Diseases of the Nervous System, by ALFRED GORDON. P. Blakiston's Sons Co., Philadelphia, 1908. 487 p.

This work, with its one hundred and six illustrations, is designed especially for the general practitioner and for the student. The writer believes that neurology has not been made sufficiently attractive, owing to the too technical treatment it has had. This work aims to give a plain, practical account of the diseases of the nervous system. The first chapter is on anatomy and physiology; the second, methods of examination and diagnosis; then follow chapters respectively on cerebral localizations, apoplexy, encephalitis, Jacksonian epilepsy, aphasia, hemianopsia, tumors, hydrocephalus, diseases of the basal ganglia, circulatory troubles, diseases of the cerebellum, medulla, pons, cord, periphery, syphilis, paresis, functional troubles, diseases of the sympathetic system, and intoxication.

Neurological and Mental Diagnosis, a Manual of Methods, by L. PIERCE CLARK and A. ROSS DIEFENDORF. The Macmillan Co., New York, 1908. 188 p.

This volume is designed to aid the student and general practitioner to make thorough and systematic examinations in nervous and mental diseases. A definite method of procedure is laid down in both lines of examination, in order that proper analysis may be made easily and readily in routine case study, either in hospitals or private practice.

Untersuchungen zur Kenntnis der psychomotorischen Bewegungsstörungen bei Geisteskranken, von DR. KARL KLEIST. Leipzig, Klinkhardt, 1908. 171 p.

This is a careful work dedicated to the memory of Wernicke. It is based on a clinical study of cases in the nervine hospital at Halle. It treats of motility, akinesis and psycho-motor, as opposed to cortical, transcortical and ideational apraxia. Motor aphasia is also laid under tribute. Psycho-motor disturbances of innervation are dwelt upon with reference to their low position. And here imperative movements and attitudes are discussed along with asthenia. The basis of Wernicke's body consciousness is the reflex system. These disorders, with their complex symptoms of strain and excitement, modify feeling, attention and thought, all of which are connected with each other and with the cerebellar and frontal brain system.

The Psychology of Advertising, by WALTER DILL SCOTT. Small, Maynard & Co., Boston, 1908. 269 p.

This work is by the author of "The Theory of Advertising," published in 1893, and is dedicated to American business men who wish to be scientific. After treating the memory, feelings and emotions, the writer points out how an advertisement must appeal to the customer's sympathy, must show human instincts, be suggestive, start up the will, favor the habit of reading advertisements following the law of progressive thinking, must be scientific in rightly estimating the attention value of small and of large spaces, etc. He then discusses the mortality rate of advertising, the psychology of food advertisements, the unconscious influence in street railway advertising, questionnaire method illustrated on newspapers; and the work ends with a bibliography of the subject, which occupies some twenty pages. The book abounds in shrewd and rather new suggestions, and is illustrated.

The Story-Tellers' League. The Story Hour. Vol. 1, No. 1. November, 1908. Edited by W. C. Ruediger, Ph. D., with Richard T. Wyche as Consulting Editor. Published ten times a year, Washington, D. C. Subscription price, one dollar. Address—The Story Hour, 406 5th St. N. W., Washington, D. C.

It is to our thinking high time that the Story-Tellers' League, numbering some five thousand people in different parts of the country, should have its own organ, and it is well also to begin in this modest way. The articles in this number are not ambitious but are telling. There are reports of the League work in different centres, a number of book reviews, the constitution and by-laws of the League, and one or two sample stories are told.

BOOK NOTES

Beobachtungen über die Psyche der Menschenaffen, von ALEXANDER SOKOLOWSKY. Neuer Frankfurter Verlag, Frankfurt a. M., 1908. 78 p.

This is a careful study of the psychic life of the gorilla, chimpanzee and the orang utan, made at first hand. The writer has had many of these animals under observation and has studied them carefully. The conclusion of his very interesting study is that apes and man differ from each other morphologically and psychologically only in degree and that the divergence is on a unitary basis.

The Hearing of Primitive Peoples, by FRANK G. BRUNER. The Science Press, New York, 1908. 113 p. (Archives of Psychology, No. 11, July, 1908.) (Columbia Contributions to Philosophy and Psychology, Vol. XVII, No. 3.)

This study is divided into upper mediums of hearing and auditory acuity. The historical sketch introduces each part, followed by characterization of instruments. The data are from the Indians, Filipinos, Ainus and Pygmies. In general, the Filipinos lead, then follow the Ainus, with whites lowest of all.

A Study of the Influence of Custom on the Moral Judgment, by FRANK CHAPMAN SHARP. 1908. 144 p. Bulletin of the University of Wisconsin, No. 236.

The writer has here made rather suggestive use of questionnaires upon various affairs in which custom affects our judgment, and finds a good deal of diversity of opinion and a wide, general distinction between honest, personal judgments on the one hand, and custom on the other.

The Problem of Form in Painting and Sculpture, by ADOLF HILDEBRAND. Translated and edited by Max Meyer and Robert M. Ogden. G. E. Stechert & Co., New York, 1907. 144 p.

The author discusses vision and movement; form and appearance; idea of subjects and its visual expression of balance and depth; conception of relief; form as an interpretation of life; and sculpture in stone. The book has few illustrations.

Les Synesthésies, par HENRY LAURES. Librarie Bloud & Cie., Paris, 1908. 97 p. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

The author first treats of colored audition; then of the other synesthesias of the same type; then of those of an emotional character.

L'Audition Morbide, by A. MARIE. Librarie Bloud & Cie., Paris, 1908. 146 p. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

The first class of morbid auditions are those by deficit or hypoacousias, and the next are those by excess or hyperacousias. These are described in an interesting way, and a general bibliography follows.

Les Préjuges sur la Folie, par la PRINCESSE LUBOMIRSKA. Librarie Bloud & Cie., Paris, 1908. 146 p. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

The prejudices are characterized as follows: 1. that insanity is of supernatural origin; 2. that fools show their folly; 3. that insanity is dangerous; 4. that it is incurable; and 5 that it is contagious.

Concerning Lafcadio Hearn, by GEORGE M. GOULD. Fisher Unwin, London, 1908. 303 p.

This is an interesting and in some respects ideal biography, describing heredity, the gruesome period, getting a soul, the development of the author's poetic talent, with style, appreciation, etc.

Twenty-sixth Annual Report of the Bureau of American Ethnology, 1904-5. Gov't Printing Office, Washington, 1908. 512 p.

This work is chiefly devoted to a study of the Pimas Indians by Frank Russell, with forty-seven plates and over a hundred figures; and to the social conditions, beliefs and linguistic relationship of the Tlinkit Indians, by J. R. Swanston.

Ursprung und Wesen des Menschen, von KARL CAMILLO SCHNEIDER. Franz Deuticke, Leipzig & Wien, 1908. 125 p.

The writer first gives the basis of phylogeny and the story of humanization. The second part discusses spiritual substance, idea, understanding energy, logic, exonœsis and endonœsis, will, feeling, ego, non-ego, idea and work, and finality. The work contains various cuts and tables.

Technical Aspects of Experimental Psychopathology, by FREDERICK LYMAN WELLS. Rep. from American Journal of Insanity, Vol. LXIV, No. 3, Jan., 1908, pp. 477-512.

Chemical Problems in Hospital Practice, by OTTO FOLIN. Rep. from the Journal of the American Medical Association, May 2, 1908. Vol. L, pp. 1391-1394.

On the Variability of Individual Judgments, by FREDERICK LYMAN WELLS. Rep. from Essays Philosophical and Psychological in Honor of William James. New York, 1908. pp. 511-549.

Die Wirkung von Suggestivfragen, von OTTO LIPMANN. Johann A. Barth, Leipzig, 1908. 169 p.

NOTES

THE CONGRESS OF THE JAPANESE SOCIETY FOR CHILD STUDY

The Japanese Society for Child Study, which was established twenty years ago and was recently re-organized, held its congress on May 10 and 11, 1908, at the Imperial University of Tokio with an attendance of about 250 members of various callings, school teachers, physicians, psychologists, ministers, criminologists, lawyers, etc. Dr. Y. Motora, Professor of Psychology in the University and President of the Society, spoke, in his address, on the purpose of the society and of its past services. Papers were presented by the members on the following subjects: K. Sugawara—On the æsthetic feelings of school girls for cherry blossoms. Dr. S. Warashina—On hysteria in infancy. I. Miabe—On backward children in the common schools. S. Sawaki—On psychopathic feeble-mindedness. G. Roseki—On the mental states of school children. Y. Ohara—On vacation colonies. Dr. S. Kurahashi —Children and poetry. A Sennichi—On the heredity of myopia, Kishibe—The crying of children and its treatment. Dr. K. Yoshida —New view-points of child study in Germany. F. Mayeda—Mimetic expressions of the child. Dr. T. Ishiwara—Mental development of the child. Dr. I. Shimoda—Children in the city and in the country. N. Miyamoto—Convulsions in children. Dr. H. Takashima—On the influence of pedagogy upon the mental states of youth. Dr. Y. Fujikawa—On nervousness in infancy. Dr. A. Miyake—On criminal youth as result of disease. Dr. S. Asoh—Present condition of the higher education of women in Europe. Dr. T. Yamada—On the question of overtaxation. Dr. T. Fukurai—"Isolated" psychical functions. Dr. Y. Motora—On mental gymnastics. Dr. H. Miyake—Some remarks on child study.

THE AMERICAN
JOURNAL OF PSYCHOLOGY

Founded by G. STANLEY HALL in 1887

VOL. XX APRIL, 1909 No. 2

CONTRIBUTIONS TO THE STUDY OF THE AFFECTIVE PROCESSES[1]

By TAIZO NAKASHIMA

The writer's interest in the psychology of the affective processes dates from the year 1896, when he read Wundt's *Grundriss der Psychologie*, and in conjunction with Professor Motora began a translation of that work into the Japanese language. Hence, when opportunity arrived for the undertaking of advanced work, in the laboratories of Harvard and Cornell Universities, he naturally chose the feelings as a subject of special research. The experimental results obtained in the Harvard laboratory, under the direction of Professor Münsterberg, will be published in another place. The present paper gives the results of three investigations pursued in the Cornell laboratory during the years 1907-1909.

It is to be said at the outset that these investigations have not been made upon any systematic plan. In the present state of affective psychology, no such plan is possible, save to those who adopt the attitude and theories of a particular school. The writer entered upon them with an open mind, and believes that he has impartially recorded their outcome. He has simply sought to experiment at points where experimentation appeared feasible and desirable. So far as space allowed, he has given the observers' judgments and reports in their own words. Nevertheless it may quite well be the case that his interpretation and conclusions seem to the reader to lay undue

[1] From the Psychological Laboratory of Cornell University.

emphasis here and to show undue neglect there. The handling of a large body of introspective material, obtained in experiments that continued for several months, is at best a difficult task; and there is something misleading in the printed account, even if it is objectively correct, since all the sentences stand out at the same level, without the shading and subordination that appear in the spoken reports. Hence the writer desires to say here, at the very beginning, that no interpretation or conclusion is offered in the following pages that is not acceptable to the observers themselves. Wherever there was doubt as to the meaning of a report, the observer was consulted, and was asked to supplement or explain.

The paper falls into four parts: I. a study of the mechanism of the affective judgment; II. a study of the *Reizmethode;* III. an account of incidental results, bearing upon current affective problems, which were obtained in the course of the preceding studies; and IV. a study of affective process by the method of the discriminative reaction.

I. The Affective Judgment

When an observer, in work done by means of some one of the regular metric methods of psychophysics, makes a record of 'like' or 'different', 'present' or 'absent', we say that he has passed a sensory judgment. When the observer makes this record in work upon the affective processes, we say that he has passed an affective judgment. The phrases are parallel, and suggest an identity, or at any rate a similarity, of mental mechanism. Nevertheless, our knowledge of the two sorts of judgment is very different. Stumpf, for instance, bases his whole psychophysical theory upon the fact of the universality of the sensory judgment, and defines psychophysics itself as the concluding chapter of a "quantitative science of judgment".[1] Whether or not we accept this view, we cannot doubt that the position is tenable and arguable; enough is known of the sensory judgment to make its facts available for theory. On the other hand, our knowledge of the affective judgment is practically *nil.* Scattered hints towards its psychology may be found in the various investigations carried out by the method of impression. But no one appears to have asked, or to have sought experimentally to answer, the question: What precisely goes on in consciousness when I judge 'pleasant', 'indifferent', 'unpleasant', or 'more (less) pleasant', 'more (less) unpleasant'?

There are, of course, many cases in which this question is rendered nugatory by the writer's general conception of affective process. If affective tone or algedonic tone is merely an

[1] C. Stumpf: *Tonpsychologie*, i, 1883, §§ 1-7; Titchener, *Exper. Psychol.*, II, ii, 1905, clxi ff.

attribute of sensation, co-ordinate with intensity and quality, then the affective judgment is a sensory judgment akin to that of 'louder' or 'fainter', 'blue' or 'red'. If affection is itself sensation, organic sensation or centrally excited concomitant sensation, then again the affective judgment is a sensory judgment, akin to judgments of kinæsthesis or of synæsthesis. The former hypothesis, however, may be summarily dismissed; it cannot be seriously maintained, in the present state of psychology, that feeling is an attribute of sensation. The latter hypothesis is now on trial, in various forms, and has been accepted by many psychologists of standing. But there are at least as many, of equal standing, who assert the independence of affection as an elementary mental process. We cannot, therefore, assume without examination that a study of the affective judgment is superfluous, or identify that judgment outright with sensory judgment.

Experiments with Tones

Of the three principal forms of the method of impression, the serial method, the method of single stimulus, and the method of paired comparisons, the last was chosen in the present experiment as the method of procedure. As regards the selection and duration of stimuli, the interval between observations, and general experimental conditions, Titchener's method in his harmonical experiment was repeated as closely as possible, except in a few points where differences were necessitated by the divergent aim of the present investigation. The reasons for repeating these experimental conditions are as follow. Titchener's article in the Wundt *Festschrift*[1] contains definite indications as to the nature of the affective judgment, and Hayes' study[2] by the same method strongly confirms Titchener's conclusions. But these indications were brought out by way of evidence for the dual theory, as against Wundt's tridimensional theory, of affective process. Hence, although the two papers in question contain many suggestions with regard to the nature of affective judgment, and although the conclusions are based upon careful introspective records, yet the results are in so far indirect as that they were obtained incidentally, in the course of work upon a wider problem, the problem of the number of elementary affective qualities. On this account it is important to repeat Titchener's and Hayes' experiments, and to confirm or to refute their conclusions.

Another and, from the standpoint of systematic psychology,

[1]Titchener: *Ein Versuch, die Methode der paarweisen Vergleichung auf die verschiedenen Gefühlsrichtungen anzuwenden*, Philos. Studien, XX, 1902, 382 ff.

[2]Hayes: *A Study of the Affective Qualities*, *Amer. Journ. Psychol.*, XVII, 1906, 358 ff.

a still more important reason for the repetition of these experiments lies in the fact that the nature of their 'subjective evidence' suggests the existence of simple, elementary affective processes, and tells against the view that makes feeling a resultant or mediated experience. Again, however, the introspective indications do not demonstrate the independent existence of an affective element, nor did the authors directly approach this problem.

There can, then, be no doubt that we have here a definite point of departure for a further experimental study of the affective problem. It remains to decide how, in detail, the method of paired comparisons shall be applied.

Titchener applied his method to two of the three Wundtian dimensions; while Hayes, for sufficient reasons, extended it, and attempted to take into account, for one and the same set of stimuli, all three of the affective dimensions. In the present study, the observers were required to pass judgment only in terms of pleasantness and unpleasantness (P-U), except when the affective reaction was 'doubtful'. This limitation is justified by the considerations: (1) that while no one denies the validity of P-U as an affective dimension,[1] all other dimen-

[1] In the *Journal of Philos. Psychol. and Sci. Meth.*, iv, 1907, 190, C. H. Johnston remarks that "it is not quite accurate to say that 'no one denies the validity of P-U as a feeling dimension'". He cites the work of C. Minnemann, Atmung und Puls bei aktuellen Affekten (Martius' *Beiträge*, i, 4, 1905, 514 ff.), asserting that the author "is inclined to give up the P-U hypothesis as not serviceable, in that there are found for them [*i.e.*, for P and U] no distinguishing characteristic physiological symptoms". The reader will not find this statement in Minnemann. This writer is discussing emotions, not simple feelings; and he is examining them in the light of their expression in pulse and breathing. What he says is that, if you set to work to classify emotions in the light of physiological *Affektbilder*, you will not get much beyond the old division into sthenic and asthenic states. Johnston is probably quoting a remark made, not by Minnemann, but by Martius (*ibid.*, 512), to the effect that "festzustehen scheint auch, dass Lust und Unlust in keiner Weise bestimmte Symptomenkomplexe besitzen, durch welche sie sich voneinander unterscheiden". But this fact, if fact it be, has nothing to do with the validity of P-U as an affective dimension. Johnston further says that "Royce . . . finds many affective states which do not exhibit the P-U dimension". So he does, and so does Wundt; but neither denies the existence of the P-U feelings; see Royce, *Outlines of Psychol.*, 1903, 178. To maintain that P-U is an affective dimension is not to maintain that all affections are necessarily comprised within that dimension; and, similarly, to maintain that some feelings are feelings neither of P nor of U is not to maintain that P and U are not feelings. "It will be recalled further," says Johnston, "that for Binet also the fundamental feeling opposition is not of the P-U type, but rather of the nature of an activity-repose dimension." If the reference is to the work of Binet and Courtier, in *Année psychol.*, iii, 1897, 65 ff., it is no more relevant than that to Minnemann. The *Affektbilder* are, again, dependent rather on the intensity than on the quality of the emotion; but the writers speak, without hesitation, of emotions and ideas that

sions are matters of dispute, and (2) that our immediate problem is not that of the number of the affective qualities, but that of the determination of the nature of the affective judgment.

The method employed, therefore, is the same as that described by Titchener and Hayes, save for the limitation just stated, and for one important and a few minor differences, to be mentioned below. The same harmonical and noiseless pendulum, marking seconds, were used. The 24 tones in the three octaves *C-c* (64-128vs.), c^1-c^2 (256-512vs.), c^3-c^4 (1,024-2,048vs.) were combined in all possible ways, thus giving a series of 276 pairs of tones. The series was first formed by chance, and then so rearranged that the same tone should never occur in two successive pairs. Which tone of a pair should be given first was also determined by chance. In the earlier studies, this series was given 12 times to each observer; 6 times upward (lower tone first), and 6 times downward (upper tone first); so that there was a total of 3,312 experiments for each observer, exclusive of 'Make-up' experiments. In the present experiment, the series was not repeated, and only the total number of 276 pairs of tones was given to each observer. Number of experiments was here less important than full and accurate observation, since we were to make no appeal to the 'curves' which present in quantitative form the course of the affective judgment, but had introspective analysis as our primary object. The whole series of 276 pairs was given in precisely the same order to both observers. In most experiments, the two observed together sitting with their backs to the harmonical, about 2 meters from it, with a screen between them. The experiments were made at weekly intervals during the months of November, December, 1907, and January, 1908;[1] the hour, 2-3 P. M., was kept constant. The observers were instructed to judge of the relative pleasantness of the tones in the pairs sounded. They were told to listen to each tone separately, and to analyze their judgments introspectively, as fully and accurately as possible; especially they were to note the basis, motives, cues, in short, the means by which these judgments were or seemed to be effected. After ten pairs of tones had been presented, the observers began to write down their introspections; no limit of time was set, but in general a period of ten to fifteen minutes was found sufficient. The experimenter had intended to allow five minutes for rest as near the middle of the hour as the work permitted, but the observers

---

are 'gaies' and 'tristes'. Finally, Johnston refers to Mach; but see *Analyse der Empfindungen*, 1900, 14!

[1] The interval of a full week was chosen with a view to the avoidance of any possible habituation, of which, we may here say, there was not the slightest trace throughout the experiments.

declared that this interruption was unnecessary, and for the most part continued throughout the hour without any relaxation.

The actual procedure of an experiment is as follows. The experimenter stands before the harmonical with one foot upon the raised pedal, the paper containing the series of the tones in his left hand, the pendulum bob in his right. At the signal "ready," he releases the bob and presses down the first note to be sounded. After two full swings of the bob (2 seconds), he treads once quickly and once slowly upon the pedal, and keeps a strong, even tone sounding for two swings of the bob. Then, after two seconds of silence, the second tone is sounded for two seconds in the same way. An interval of from 2 to 6 seconds is allowed, between the pairs of tones, for the recording of the judgment. After the 141st pair, this interval was increased to 30 sec., to allow of the writing of short notes upon important points of observation, and to prevent a possible lapse of memory during the remainder of the ten comparisons. This arrangement was continued until the 171st pair, when the observers decided that the interval was unnecessarily long, and the time was accordingly reduced to 15 seconds for the rest of the series.

The occasional unevenness in the intensity of the tones, the noises from the mechanism of the organ, the occasional creaks of the pedal in certain kinds of weather, noises within and without the building, the peculiar character of a few tones (c was apt to sound suddenly and at high intensity; b^3 and c^4 were rather faint; c^3 now and again developed a tremolo)—these disturbances and variations in the stimuli were of no material consequences for our problem. Moreover, the high degree of training of the observers showed itself in the rapid development of power of concentration, and in the complete ignoring of the disturbing factors. We were, therefore, not obliged to repeat in 'Make-up' series the occasional tests into which these disturbances and variations entered.

The results of the experiment are embodied in the introspective records of the observers. G's records are presented below in their entirety, so that any possible arbitrariness of quotation and conclusion is out of the question. Insight and accuracy are individually different, and the same materials may admit of different conclusions. But the introspective records here quoted stand for the facts observed, and represent the actual reaction of the observer. Observer G is Mr. L. R. Geissler, assistant in the psychological laboratory. G had had 3 years experience in experimental work, and had already taken part in extended affective studies. He is musical.

Introspective record I. 1st to 10th pair

1 (a^3-e^3) first Judgment usually very difficult. There was hardly

2 (c-c^4) first
3 (a^3-a^1) second
4 (d^3-d^1) second
5 (g^1-D) second
6 (B-e^3) first
7 (E-g^1) second
8 (C-g^3) first
9 (F-g^1) first
10 (e^3-f^3) second

any inherent affective tone in the stimulus. Judgment was arrived at by reasoning about the different attributes of the two tones; for instance, the one tone was smoother, more evenly blown (intensively). The highest tones usually call forth a frowning and twitching in the eye-lids. I am afraid I am not yet in the right mood; I am somewhat constrained in my bodily attitude.

In the above record, the figures in the first vertical row indicate the number of the pair sounded for comparison, while the word 'first' or 'second' in the third row indicates the observer's judgment that the first or the second tone of the pair was the more pleasant. In the remaining records, these data are omitted.

Record II. 11th to 20th pair

This series was rather irregular because of disturbances. I never could get into a truly affective mood. My judgments were still made intellectually, by deciding on the different attributes of each tone.

Record III. 21st to 30th pair

Closed my eyes, took the most comfortable bodily position, and let myself go entirely. Kept all foreign ideas out of my mind, and tried to keep myself in a perfectly constant and unchanged mental and bodily attitude. Now whichever of the two tones was more confirming in this, or whichever was less disturbing, was felt as the more pleasant.

Record IV. 31st to 40th pair

Was more difficult than last time to get into an entirely restful, quiet attitude. Consciousness was several times disturbed by external noises and ideas associated with them. Otherwise, I tried to make judgments on the same basis as before. Of my body I seemed to be unconscious altogether. The tone which disturbed me the less by fewer parallel other sensations and associated ideas was called the more pleasant. The tone whose qualities could easily be analyzed or forced themselves singly into focus was unpleasant.

Record V. 41st to 50th pair

In these cases the pleasantness was usually decided on the basis of some quality in the two tones. I was not yet able to get into the right mood. Associations aroused by the tones also sometimes decided the affective judgment. There seems to have been an entire absence of organic sensations, as far as I remember; *i. e.*, the judgment must have been based only on the external stimulus.

Record VI. 51st to 60th pair

I am at a loss to say why I liked the one tone better than the other. I know I am somewhat biased in favor of the lower tones anyhow, but I try to neglect this. My judgments are the conclusions drawn from an intellectual comparison of the different qualities in the two tones, the tone which comes nearer to a pure tone being called more pleasant. But I am unable to discover a purely affective elementary process in my consciousness.

Record VII. 61st to 70th pair

Tried to let tones make their impression without writing down the judgment, but found that I could not get much, because the tones did not have much affective quality in them, and associated ideas tried to take the place of the affective judgment; only two or three pairs seemed to have something essentially different in the affective coloring of their respective members. But this shows that compulsory affective judgment is artificial.

Record VIII. 71st to 80th pair

Was still trying to quiet down after a fast walk; the lower tones were more quieting and soothing than the higher, and therefore more pleasant. By more quieting I mean that they seem to have a slowing effect upon breathing (and perhaps on pulse as felt in temples).

Record IX. 81st to 90th pair

Much more difficult to judge this time. Decided mainly by the qualities of roughness or smoothness in tones. I am not sure whether the lower tones are not more conducive to my general mood of quiet phlegmaticism. The lower tones do not disturb my organic equilibrium, do not seem to call for any kind of conscious muscular readjustment, while the higher tones do, sometimes very distinctly so.

Record X. 91st to 100th pair

I don't know whether there was anything peculiar in the different octaves. I know the tones made very little impression upon me one way or another; they hardly changed my general mood, and I simply judged them more pleasant by the presence of greater smoothness.

Record XI. 101st to 110th pair

Most notes were hardly pleasant at all, only in so far as my general mood was not disturbed by them. Only the very high tones are less pleasant distinctly, and always cause an involuntary frown. Occasionally I noticed my breathing and thought it rather deeper and longer than commonly.

Record XII. 111th to 120th pair

I was somewhat distracted by various things, and my mood was not very constant. The judgment was made more or less on the basis of secondary qualities in the tones; *i. e.*, they are logical constructions rather than indications of feelings. The whole situation as such was not very pleasant; there was much anxiety about the unsuccessfulness. This anxiety seemed to be an irregular flow of ideas, sometimes smoother and easier than at other times.

Record XIII. 121st to 130th pair

This time the general mood was better, almost indifferent, and the tones that disturbed it less were judged more pleasant. For the first time I noticed a mass of organic sensations trying to claim my attention, especially in the intervals between the pairs. I cannot say whether these organic sensations had anything to do with the general mood or the various feelings in particular; I simply noticed their presence.

Record XIV. 131st to 140th pair

The tones were more indifferent than ever before; even the highest ones no longer cause any great affective disturbance of the general mood. I had to guard myself against perfunctory judgments and asked myself again and again: Which tone do I actually like the better?

Record XV. 141st to 150th pair

My general mood of undisturbed indifference was very little or sometimes not at all changed by the various tones. The judgments were made on an inferential basis. Sometimes a few organic changes were noticed, but they did not seem to influence or be influenced by the general mood or even the tones.

Record XVI. 151st to 160th pair

The long intervals between the pairs (30 sec.) allowed me to fall into a kind of sleepy drowsy mood of a distinctly pleasant character. Now whichever of the two tones disturbed this mood more was judged more unpleasant. Some tones seemed to be surprisingly different either as to loudness or smoothness or both, and thus their affective tone was more easily determined by the greater surprise which they set up and which was unpleasant. Also the momentary awakening and writing down of the judgments was more or less unpleasant.

Record XVII. 161st to 170th pair

The situation was very similar to the one before. I find my previous observations confirmed, and merely tried to rule out the element of surprise as far as possible. I noticed also that the more unpleasant tones sometimes evoke a tendency to some facial muscular reactions, but only once did I feel an actual frowning, perhaps because my attention was not especially concentrated on this element.

Record XVIII. 171st to 180th pair

The only way I seem to be able to judge real pleasantness and unpleasantness is to let myself go entirely, to become passive, and to remain in a very constant, even half-sleepy mood and observe which of the two tones arouses me more or less to a normal state and which leaves me in the same mood as before. The tone which leaves me in the drowsy mood or disturbs me less in it, is the more pleasant. The tone which brings me back to a more normal mood of everyday life is more unpleasant.

Record XIX. 181st to 190th pair

Somewhat similar to last series, although the mood was not so phlegmatic and drowsy. I cannot remember that any organic sensations disturbed me or came into notice at all. The general feeling of indifference seems indescribable. The judgments arouse sometimes ideational associations, but these come rather after the figure [indicating judgment] is written down. Sometimes also, after the writing, I catch myself reflecting on other qualities of the tones compared.

Record XX. 191st to 200th pair

This time my attention seemed more labile; several times I found myself thinking of other topics. Hence the tonal judgments were made entirely on the affective basis. Only once I was doubtful, and began to reflect on the other qualities of the two tones compared. Toward the last the question occurred to me: Which of the two tones would disturb me less, or to which of them could I adapt myself more perfectly and more easily? The answer coincided with the more pleasant tone.

Record XXI. 201st to 210th pair

It seemed much easier to make the judgments, except in the doubtful cases. My attention was more concentrated than before, and ideational associations were easily kept out. My general mood was an easy let-go feeling; no organic or other sensations disturbed me.

Record XXII. 211th to 220th pair

Was less restful than before, also disturbed by wandering attention. But most of the judgments were easily made; perhaps because there was a great difference in pitch between the tones in the pairs.

Record XXIII. 221st to 230th pair

I have not yet got into a really comfortable mood of indifference, and it was harder for me to make the judgments than before. Still I think I made them in the same fashion as before, letting myself go, giving myself up to the tonal pair and letting myself be affected by them, either the one way or the other. Once or twice I noticed with very high tones a strain in the right tympanic membrane.

Record XXIV. 231st to 240th pair

This time it was easier for me to make my judgments. Nor was I bothered by organic sensations. I just feel comfortable, and if a tone does not disturb me in this feeling I call the tone more pleasant. I don't remember any associated ideas or trains of thought; I was simply passively listening.

Record XXV. 241st to 250th pair

The situation was much as before. There is hardly anything to add. Twice, after the judgment was made, it occurred to me that I have heard the same tonal interval once before to-day, but this consideration did not influence my judgment.

Record XXVI. 251st to 260th pair

My general mood was not quite so stable this time. The noise outside from the wind seemed to make me feel somewhat restless. Nevertheless, I tried to be as calm as possible, and to make my judgments purely on the tones themselves, abstracting from the noises.

Record XXVII. 261st to 268th pair

Was very much less disturbed this time. Do not remember having heard the wind. Hence judgments were made more easily. My mood was quite restful and calm, and neither organic sensations nor trains of ideas were noticed.

Record XXVIII. 269th to 276th pair

Much the same as before. Only toward the very end it occurred to me that these were the last judgments, and I felt somewhat anxious about whether I had completely analyzed the situation or not. But when I look back I cannot find anything I left out, and I know now just as little as at the beginning why I like some tones better than others. It is just simply the way they affect me; and I imagine under other circumstances they would affect me quite differently.

We now proceed to an examination of the above introspective records.

I. In the case of this observer, the following five factors in, or determinants of, affective judgment may be distinguished.

(1) Reflection on the different attributes of the two tones. "Judgment was arrived at by reasoning about the different attributes of the two tones; for instance, the one tone was smoother, more evenly blown (intensively):" Record I. This factor is also mentioned in Records II, V, VI, IX, X, XV, XVI, XXII.

(2) General condition or mood. "Now whichever of the two tones was more confirming in this, or whichever was less disturbing, was felt as the more pleasant." Record III. "Which of the two tones would disturb me less, or to which of them could I adapt myself more perfectly and more easily? The answer coincided with the more pleasant tone." Record XX. This factor is referred to in all records, except VI and VII. In IV, VIII, XXII, XXIV, XXV, and XXVIII, the word 'mood' is not employed, but the operation of the factor is distinctly implied.

(3) Associations. "Associations aroused by the tones also some-

times decided the affective judgment:" Record V. See also Record VII.

(4) Bias or prejudice. "I am somewhat biased in favor of the lower tones anyhow:" Record VI. See also Record XIV.

(5) Breathing and pulse. "By more quieting I mean that they seem to have a slowing effect upon breathing (and perhaps on pulse as felt in temples)." Record VIII. Such facial expressions as frowning, twitching in the eyelids, etc., are also recorded, but are looked upon as concomitants only. And it should be noted that during the experiments of Record VIII, the observer was trying to quiet down after a fast walk.

II. Critical changes in the course of introspection. In accordance with his understanding of the instruction given, and in accordance also with his personal interest in discovering the basis of the affective judgment, the observer entered upon the experiments in a distinctly analytical attitude. He had often spoken in conversation of the 'inferential' nature of pleasantness and unpleasantness; and he now set systematically to work to find a 'reason' for the affective judgment. Such a 'reason' was first found in the different attributes of the two tones. In the first 20 comparisons, judgment was for the most part passed on this basis.

At the 21st comparison, however, the observer abandons this reflective attitude for an attitude that must be characterized as its direct opposite. The following quotations will show what is meant. "Closed my eyes, took the most comfortable bodily position, and let myself go entirely. Kept all foreign ideas out of my mind, and tried to keep myself in a perfectly constant and unchanged mental and bodily attitude" (Record III). "The only way I seem to be able to judge real pleasantness and unpleasantness is to let myself go entirely, to become passive, and to remain in a very constant, even half-sleepy mood" (Record XVIII). "My general mood was an easy let-go feeling; no organic or other sensations disturbed me" (Record XXI). The attitude is now that of passivity or receptivity; analysis and reflection have disappeared.

Under these conditions, the observer recognizes that the affective judgment is prompted directly by the external stimulus. "In these cases the pleasantness was usually decided on the basis of some quality in the two tones; there seems to have been an entire absence of organic sensations, as far as I remember" (Record V). It should be especially noticed that the records nowhere (save in the one doubtful case of Record VIII)) suggest the mediation of the affective judgment by organic sensations; see, *e.g.*, Records XIII, XV, XIX.

The search for a 'reason' has not, however, been given up. The observer is perplexed that the judgments attach thus directly to the stimulus, and mistrusts his observation: "I am at a loss to say why I liked one tone better than the other" (Record VI). He accordingly introduces a change in his method of observation: thinking that the explicit formulation of his judgment on the tones might lend an appearance of directness to the affective reaction, he now allows the tones to make their impression upon him without writing down a judgment at all. But the character of the judgment remains the same. Henceforth, therefore, the mood of passivity becomes the essential condition for the pure affective judgment, and the intellectual cues are relegated to a secondary place. Introspection furnishes the warrant. "The judgments arouse sometimes ideational associations, but these come rather *after the figure is written down*. Sometimes also, *after the writing*, I catch myself reflecting on other qualities of the tones compared" (Record XIX). And, this warrant once acknowledged, the whole task becomes simpler and easier. "It seemed much easier to make the judgments, except in the doubtful cases" (Record XXI. There were 27 doubtful cases in the total number of 276 comparisons). "This time it was easier for me to make my judgments" (Record XXIV). Finally, the observer declares that "I know now just as little as at the beginning why I like some tones better than other. It is just simply the way they affect me" (Record XXVIII).

III. Conclusion.—It seems clear, from this examination of G's observations, that he passes his P-U judgments directly and immediately on the basis of the stimuli and of these only. The ideal conditions for the experiment are a mood of passivity or listlessness; an undisturbed bodily attitude and state; and freedom from intruding or distracting ideas when the series begins. There is apparently no mediation of the affective judgment by organic sensations.

G himself, unwilling at first to believe that the affective judgment is prompted directly by the stimulus, and confirmed in his scepticism by the nature of the instruction, attempts to find—almost one might say to manufacture—an intellectual basis for the judgment. He thinks that there must be a 'reason' for it over and above the mere quality of the tones. So he has recourse to 'smoothness', or to the 'tone which is nearer a pure tone', etc., varying his report in all sorts of ways, but always seeking for some intellectual construction of the affective judgment. That this search for a 'reason' is a search for what does not exist is clearly shown by the later introspections. G gives up his endeavor to find the 'reason', even while he would still like to find it, and is finally satisfied to take the stimuli as they come. He has tried various logical constructions (associations, organic sensations, power to distract, general disturbance of mood, etc.), and his efforts having been unsuccessful, he renounces the mediation of the affective judgment, and at once the "work is easier". Here, then, is an observer who at the beginning distrusts the immediacy of the affective judgment, and tries to argue himself out of it by all possible means; but whose own introspections, given by the way, convict him of immediate judgment as it were against his own will.

The second observer, Miss M. C. West, is graduate fellow in the department of psychology. Like G, she had already taken part in extended affective studies. W is also musical.

An examination of the introspective records of this observer seems to justify the distinction of two main classes of affective judgment: the associative and the direct. The direct judgments are again divisible as (1) judgments prompted by the tones themselves, with or without certain concomitant processes (especially organic sensations), and (2) judgments prompted by other motives than the tones.

I. The associative judgments

1. The tones were judged on the basis of their 'fitting in with the mood': they seemed to be what the observer wanted; she wanted to sing them; while in the tones themselves, as qualitative processes, there was nothing unpleasant, and nothing strikingly pleasant.

2. Certain middle tones were pleasant, because the observer had the same sense of ease (or absence of strain) and rest, in hearing them, that she would if she were trying to sing them.

3. Tones were unpleasant because agressive, forcing themselves on the observer, seeming not to fit in with or belong to the disposition of consciousness.

4. Tones were unpleasant because too loud or sudden, 'ruffling' the observer 'all up'. Or because harsh and rough, 'keeping her on edge'. Or, again, because nasal and twanging. In all these cases associations were set up, along with the obvious arousal of organic sensations. "The ominous association with the sound in some of those lower notes seems to make them more distasteful than even very shrill, thin high notes that I usually dislike most" (Record III). "The first tone in the pair 75 had an arousing effect through the association of a trumpet call that was solemn in nature and therefore unpleasant" (Record XVIII). "I am pretty sure that the gruffness and roughness of some lower notes may be unpleasant partly through associations with hoarseness and with people of rough low tones in speech, who are often cross and disagreeable people" (Record XXV). "Some of the deep rough tones were judged so not by strains but by a sort of mood they threw me in, of depression and fear" (Record IX). "Many of the notes are judged pleasant or unpleasant by associations, I think, such as Christmas horns, whining animals and people, roaring lions, etc., according to the associated mood these objects set up" (Record XIII). "None but sweet notes seem pleasant to me unless because of associated mood" (Record XVIII).

II. The direct judgments prompted by tones

The nature of these judgments will be clearly seen from the following quotations. "I don't know by what means I judged that I liked it. Maybe I thought 'clear, full, sweet,' maybe I thought of flute notes; but I don't think I did. Maybe I judged by the absence of things that made me shrink" (Record IX). "The high notes seem unpleasant in themselves, but it may be only that the unpleasant strains are more local, in the region of the ear. I can't make out whether the unpleasantness is the strains or not" (Record VI). "The unpleasantness seems to come in at once. There is a kind of jarring which is unpleasant, though I don't think I call it that; I could as easily call it jarring as unpleasant, but I don't think I call it anything. It is just a feeling that I can't sit still and listen to it" (Record VI). "They seem unpleasant from the first, and I can find no word or judgment in consciousness. I caught myself making a mental grunt" (Record VI). "Can't tell what made 71 [the first tone in the pair] pleasant or sweet" (Record XVIII). "I think some tones are pleasant and unpleasant to the ear itself, not counting strains around the ear or unpleasant vibrations, which are not always present even when the tone is unpleasant" (Record XX). "Some very high tones are unpleasant just because they are high, without being shrill or causing any particular associations" (Record XX). "The organic sensations in the case of pleasant tones seem to follow the affective judgment and do not seem to be its basis. They seem to create a desire for more, and a restlessness; their pleasantness is not created by this relaxation, etc." (Record XXV). "Caught myself saying 'don't like it,' but generally noticed a shrinking, wincing, turning of head away without any words or thought; but I can't tell whether those movements were the unpleasantness, or their sensations were the unpleasantness, or whether there was a distinct process of unpleasantness" (Record V). "There were three tones there that I just liked in themselves, and when I asked why, the words 'full, rich, sweet' came. It had something to do with the pitch being a medium one (not associated with the strain of high pitch), but it was more than that. They were accompanied by no perceptible organic sensations and no ideational associations. They seemed to satisfy, and a deep breath followed as a result. The ear lingered on them and tried to hold them and one wished they would continue" (Record XXIII). "In the pair 262, there was not anything except the tone itself which was agreeable. I liked the tone. I am not even sure that it was full or resonant or had any of those associated attributes which might be a basis of judgment. I noticed that the nasal quality of both tones in the pair 264 [g^1 b^1] set up no detectable vibrations and strains and had no observable associations, but when I asked why unpleasant, I merely answered 'nasal.' It seemed as if the inharmoniousness of the tone itself were directly unpleasant to the ear drum. Nor was the roughness in the first tone [*F*] in the pair 266 [*F-e*] unpleasant because of associated hoarseness; but it seemed to have thickness and roughness, unpleasant just as the nasal tone was to the drum itself" (Record XXIII). "I tried to think what made 33 so pleasant to me but I could not. I think it put me in a pleasant mood but why, or why the mood was pleasant, I do not know. I know that relaxed organic sensations had something to do with it" (Record XIV). "1 in each case gave no disagreeable strain sensations and was in no distinguishable way otherwise unpleasant than by the gloomy mood it induced" (Record XV). "The very deep tone in 57 seemed to be judged disagreeable instantly from a general dislike of very deep tones, not because it was intrinsically very disagreeable" (Record XVI). "The judgment is nearly always made on the basis of degrees of unpleasantness. No tone seemed really pleasant. It was a matter of release from unpleasant. [*c* and g^3] in 61 and 62 were rather sweet, possibly because of no strain" (Record XVII).

III. The direct judgments prompted by other motives

The principal motives to judgment in this class were organic sensations of ear strain, nose tingling, tendency to shut ears, vibrations in head, nose, ear or chest, hitting or piercing of ear-drum or head, head-wincing, etc. "The shrill notes that I did not like were sometimes judged so by ear strains that seemed to go down to my toes" (Record IX). "In some cases I judged it was unpleasant by means of strains, as in the pair 100, a high shrill tone that made my nose tingle and my eyes shut, and I did not like it because of the strain, I think. On the other hand there were twangy tones that were unpleasant because of something else, I don't think it was strain, but an unpleasant vibration set up somewhere in the head, nose or ears" (Record X). "One note seemed to pierce the ear drum and made me want to put my hands to my ears. My head distinctly winced. Other twangy tones set things vibrating around nose and ears, which was unpleasant. Other rough low tones caused a disagreeable vibration somewhere (could not localize it); seemed perhaps in the chest; it was unpleasant. In all these cases it seemed more the organic sensations that were unpleasant than the tones" (Record XI). "Very disagreeable to the ear, induced reflex of head, felt as if things were hitting the drum and piercing the head" (Record XV). "Sometimes the judgment is entirely based on organic sensations of strain and disagreeable vibrations in face and ear; sometimes apparently localized in drum, those seemed squeaky; sometimes across nose and face, those seemed twangy" (Record XVI). "Based largely on strains, contractions of muscles and ear and nose vibrations; sometimes in body (chest particularly) also" (Record XXII). The last quotation that we shall make shows a mixture of motives. "In every instance the unpleasantness seems to be derived from something else, organic sensations or a foreboding mood. It is possible that the pleasant tones also were judged so by comparison with a standard of clarity of tone which I considered beautiful and which was gained from opera singers and from musical instruments. I think the absence of jarring organic sensations is one thing in the pleasantness of a tone. I don't know whether the rest is an inference according to some standard or not" (Record XII).

Conclusions from W's Observations

This observer is much more liable than G to organic sensations. And she has fallen into a rather natural mistake: she thinks that, if she can put her finger on some accompanying organic sensation, she has found a 'reason' or 'basis' for the affective judgment. But, of course, we have to ask how an organic sensation is judged pleasant or unpleasant, just exactly as we ask how a tone is thus judged. In fact, if we abstract from the cases of association, the affective judgment seems to be immediate (there were only five 'doubtful' cases out of the total number of 276 judgments). The immediacy is, however, of two kinds. The affection may attach (1) to the tone itself, without there being any assignable 'reason' for it to do so; it just belongs to the tone: or it may attach more prominently (2) to the organic reverberation set up by the tone. It is clear, indeed, that this observer wished to find a basis for judgments in organic sensations, just as G wished to find a basis in intellectual factors. She succeeds better than G, in the sense that she often finds the P-U judgment attaching to organic sensations set up by the tone, while G had no success with his logical reflection. But she offers no 'reason' for this attachment of P-U to organic sensations; so that really the judgment is as immediate as it is in the many instances in which the tones themselves are pleasant or unpleasant.

W's introspective records are not given here in full, since they are far more homogeneous than those of G, and offer no similar possibility of varying interpretation.

We have now to compare the general result, gained from the study of these introspective records, with the conclusions drawn by Titchener and Hayes, so far as these are relevant to our problem. "Judgments of P-U," these authors report, "were easy, direct and natural. It was exceptional to find any reason, any basis for these judgments: the stimuli were intrinsically pleasant and unpleasant, more pleasant or more unpleasant than their neighbors: and when a reason, or a basis was found, outside of intrinsic affective tone, it lay in the organic reaction set up by the stimulus employed."[1] So far as directness or immediacy of judgment is concerned, the outcome of the different investigations is strictly comparable. And even on the score of ease and naturalness there is some possibility of comparison. It is true that a judgment might be difficult, straining, and yet be immediate: that would be the case, *e.g.*, when the state or process to be judged lacked clearness, was faint or indefinite or fleeting. On the other hand, since it must be impossible to analyze, or to pass mediate judgment upon, a mental process which is in reality simple and ultimate, judg-

---

[1] Titchener: *Psychology of Feeling and Attention*, 1908, 165. Hayes: *A Study of the Affective Qualities*, *Amer. Journ. Psychol.*, 1906, 389.

ment under such circumstances will become unspeakably easier and more natural as soon as the attempt at analysis is given up. In this sense, ease and naturalness of judgment become a test of the simplicity of the process in question, and thus a test of the immediacy of judgment itself. The test is especially applicable to the situation in which the observer G found himself placed during the latter part of the experimental series reported above.

As regards the primary issue, of immediacy of judgment, the evidence of the earlier studies was, of course, not conclusive, and was not stated as conclusive. The spirit of the instruction given, and the mode of treating the observers' judgments (the procedure of 'make-up' series), may even be said to have put a premium upon the immediate affective judgment. The stimuli judged directly affective under these conditions might prove to be only indirectly affective under another form of instruction. Hence it was plainly necessary to supplement the earlier work, in which judgment was called for on the ground of the stimuli, by experiments in which the observer was required to introspect the affective judgment, with a view (if possible) to the discovery of its 'basis' or 'reason.' If the immediacy of the affective judgment is still attested, with this special direction of attention, then it may be regarded as established with some degree of certainty. But this is precisely the result to which we have been led. And it is a result, be it remembered, which is entirely opposed to the initial expectation of the two observers.

But, it may be urged, these observers speak in fact of several bases or reasons for their judgments, over and above the mere quality of the tones, and beside the organic reverberation set up by the stimuli. They appeal to various secondary attributes of the tones, such as their smoothness, purity, etc.; to general disturbances of mood; to associations; to a bias or prejudice of general like or dislike; to the sense of fitness; to the mood temporarily aroused by the stimulus; to some relation, of contrast or what not, to other affective or sense qualities; to motor tendencies, and to actual movements. That is true. It is, however, equally true that these bases or reasons are for the most part directly attributable to the interpretation put upon the instructions given, an interpretation dictated by the observers' personal interest in the explication of the mechanism of the affective judgment. The immediacy of that judgment is, nevertheless, plainly brought out, even against their inclination, and in spite of a reflective attitude that was unfavorable to any immediate experience. Had they passed their judgments, throughout the experiments, under the ideal conditions of passivity and receptivity, practically all of these judgments would probably have been immediate.

We do not deny that there are cases in which an indifferent stimulus may be affectively judged by association, or in terms of some bias or prejudice. An indifferent impression may be judged affective through its relation to some other, vividly affective experience; a 'blunt meaningless tone', for instance, may become unpleasant by contrast (the term in its widest sense) with a 'full rich tone', the bluntness and meaninglessness being simply the result of association. Or again, the impression may be more or less affectively colored, intrinsically, and yet may be distinctly modified in its affective intensity or quality through bias or contrast or any other form of association. There are many cases, among these associative judgments, in which the observer cannot be sure whether or not there was any affection attaching to the stimulus as such, but is aware only of the affection attaching to the associated ideas. All these facts we admit. But we assert that, in general, these mediated judgments tend more and more to disappear as the experiments progress and the observers gain in practice. Associated ideas, permanent likes and dislikes, the 'sense' of fitness or congruity with the present contents of consciousness,—all these things are secondary criteria, which the observer at first, in his comparative helplessness, avails himself of, but which he learns to disregard as his familiarity with the problem in hand increases. The very same stimuli which are now judged by their associates are presently ranked in terms of their intrinsic pleasantness or unpleasantness. There can be no doubt of the immediacy of the affective judgment; there can, we think, be no doubt that the various experiences offered from time to time by our observers as the 'basis' or 'reason' of the affective judgment are of the nature of disturbing factors, that should so far as possible be eliminated from an experimental study of that judgment; precisely as, in psychophysical work, the influence of association and the admission of secondary criteria are ruled out by experimental arrangement and by the shaping of instructions. The tones are judged pleasant or unpleasant with the same directness with which they might be judged loud or faint.

Experiments with Colors

In the earlier studies of the affective tone of colors, the problem was the determination of preference, and its systematic expression in quantitative terms. Neither Cohn nor Major, for instance, gives us any definite information as regards the mediacy or immediacy of the affective judgment. Thus, Cohn says only:

"Der eine [Beobachter] wird rasch zugreifend entscheiden, der andere lange hin und her schwanken, das einmal gegebene Urtheil wieder zurückziehen und schliesslich zu keinem oder doch nur zu einem unsichern Resultat gelangen. Häufig hört man gerade im Anfang, dass die Sache doch wohl bekanntlich subjectiv und unsicher sei, dass man sich der Verwendungs-Associationen gar nicht erwehren könne und dergleichen mehr. Wenn man dann wieder und wieder hervorhebt, dass es sich ja hier nicht um eine Geschmacksprüfung handele, dass jedes Urtheil gleichwerthig sei, es möge nun ausfallen wie es wolle, dass es nur darauf ankomme, den augenblicklichen, unmittelbaren Gefühlseindruck wiederzugeben, so gewöhnt man seine Beobachter allmählich an ein ruhigeres Verhalten. Und diese gleichmässigere Gemüthslage ist ein unabweisbares Erforderniss zum Gelingen der Versuche. Handelt es sich doch bei denselben um einen ziemlich complicirten psychischen Process. Der Beobachter soll die ihm gebotenen Sinneseindrücke möglichst ohne Reflexion auf sich wirken lassen und er soll dann diesen rein gefühlsmässigen Vorgang in die intellectuelle Form eines Geschmacksurtheils kleiden. Es besteht also die Gefahr, dass jener intellectuelle Urtheilsvorgang gewissermassen vorweg genommen werde, dass der Beobachter sich Theorien mache und unbewusst nach diesen sein Urtheil bilde."[1]

These sentences point out difficulties of method; they do not help us greatly to an understanding of the nature of the affective judgment. Major's notes, under the heading *The Affective Judgment*, practically coincide with those of Cohn. He noticed the tendency to theorize, to put reflection in the place of direct experiencing; and the difficulty of obtaining a 'gleichmässige Gemüthslage' from his observers, due partly to the confusion of the æsthetic with the affective judgment, the conviction of the 'subjectivity' and uncertainty of the experiment, etc., and partly to the unaccustomedness of the required judgment. He also noticed the influence of mood. Major's observers sent all impressions alike up towards pleasantness when they were in 'good spirits', working on a pleasant day after much wet weather, etc.[2] So Cohn's "vier verschiedene Beobachter erwähnten gelegentlich, dass sie dieses oder jenes Urtheil abgäben, wiel die betreffende Farbe oder Farbencombination ihrer augenblicklichen Stimmung besser entspräche. So wurde auch einmal ein Gleichheitsurtheil durch die Aeusserung motivirt: 'man würde je nach der Stimmung bald das eine bald das andere schöner finden; verschiedener Gefühlscharacter, darum sehr schwer zu entscheiden'. Ein Beobachter hob hervor, dass ihm bei guter Stimmung die Unterschiede viel deutlicher zum Bewusstsein gekommen seien, als bei schlechter".[3] For this reason, Cohn finally decided not to appeal to introspection. Indeed, both he and Major found it necessary to work towards mechanization of the judgment process during the experimental hour. They both noticed that the number of distracting associations decreased as the experiments progressed. But we are not informed whether or how the associations influenced the nature of judgment. Some of Cohn's observers, again, seemed to be influenced by the serial order of experimentation; a color or color combination might be judged pleasant simply because it was strikingly different from the preceding, unpleasant

---

[1] *Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten, und ihrer Combinationen*, Philos. Studien, X, 1894, 596-597

[2] *On the Affective Tone of Simple Sense-Impressions*, *Am. Jour. Psy.*, VII, 1895, 61-62.

[3] *Op. cit.*, 598.

color. But this sort of influence may fundamentally change the nature of the judgment.

Our own experiments with colors, like those with tones, were planned solely with a view to the better understanding of the affective judgment. The experience gained in the harmonical experiments made it possible for us to put a series of definite questions to the observers,—who, however, were notified that they should by no means consider themselves bound by the order of the suggestions, nor feel obliged to state their observations on all points during every introspective interval, but that they should observe and report as opportunity arose, and supplement the questions set in any way that occurred to them. The method employed was again that of paired comparisons. The judgment was passed in terms of P and U; the observers reported which of the two stimuli was the more pleasant, or (if both were unpleasant) which was the more unpleasant. The 26 (Milton Bradley) colors employed were combined in all possible ways, thus giving a series of 325 pairs.

The whole series of 325 pairs was given in precisely the same order to both observers, who in these experiments observed separately, sitting at a distance of 1.5 m. from the windows (3x3 cm.) of the neutral gray exposure apparatus. The experiments were made during the months of January, February, and March, 1908. They were performed in ordinary diffuse daylight in the same room of the laboratory; every care was taken to keep the light as constant as possible, by the adjustment of white curtains at the windows of the room. The time of exposure of the colors was 5 sec.; the interval between observations, during which the observers recorded their judgments and could make short introspective notes, was 10 sec. After every 5 comparisons, they were required to write out full introspective reports, without any definite time limit. Miss West and Mr. Geissler again served as observers. We begin with an examination of W's records.

Of the total number of 325 judgments passed by W, 266 were immediate. W now terms these direct judgments 'affective' or 'purely affective.' They are all phrased in much the same way; the following may serve as illustrations. "I think the judgments were certainly purely affective; I could give no reasons why, and felt no organic sensations throughout these experiments, except consciousness of deep breathing when I liked the color and of movement away from the color I disliked." "Both colors in the pair are horrid to me; one perhaps less than the other because 'less like purply' I said, but I really think it was a purely affective judgment. 'Less purply' was no reason." "I hated the dark buff [in no. 40]. It is a color seldom or never seen and I had no prejudice against it or association with it; it was an affective judgment."

There were 35 mixed reports of the following kind, in which, nevertheless, immediately affective judgments appear to be comprised. "There was a distinct prejudice against the color at the right hand side, but also there was an immediate dislike of it." "I think there was a distinct prejudice from preceding judgments that I should like the pure yellow better than the orange; but perhaps I also liked it immediately." "In the third pair there were also intellectual judgments that the oranges were too bright, but I think an immediate judgment that the red was the more pleasant came first, and a feeling of irritation with the orange." "After deciding that I liked the pure yellow there came the thought 'but see how bright they are,' which though I think it modified my liking did not make me find the yellow unpleasant." "In no. 4 I immediately liked the greenish yellow and then I thought 'but is not that the ugly bilious green you do not like?' but it did not make me dislike it." "I had an image of robin's eggs with the blue green, which was almost inseparable from the color itself, and made me like it better than I would otherwise." "In 56 the thought of yellow satin dresses and yellow ribbons, and the good times I had had wearing them, made me like better the yellow which I think I liked anyway." "The yellow in 68 I did not like at first, but suddenly there came a vivid recollection of the sunshine on the wet grass in the picture of a French artist whom I like, which made me like this green and the greens in 69 and 70." "As the blue violet reappeared in 74 I could hardly make myself look at the other color to see whether I liked it or not. I forced myself to ask, why do I like it? and I answered 'soap bubbles' and then 'Frenchman's pictures of the Seine and The Tower of London'; but these associations came later and brought a new kind of pleasure, not so absorbing but more acute." "There are certain shades I noticed that I dub 'artistic', I don't know why. This may be an individual feeling that a dull color that I like is artistic. It may be some forgotten association. I believe the soft shades, the mixed shades, may usually be called art shades and the dark soft shades especially so. I should not call the buff artistic though I like it. But the dull green in 44 as well as the pea-soup color may derive its likableness partly from codes of art got from custom. Yet I think there is a strong immediate affective judgment there too, as there is in the buff which is not called artistic." "The contrast in 90 made both less pleasant than they would have been separately, made them seem too dark and too light." "The contrast in 95 made both worse. In spite of the prejudice in favor of dark colors, the color at the right in 94 though dark seemed horrid to me; I could not bear to look at it." "The contrast in 137 made both worse. I could not decide which I disliked more, could not make myself continue looking at them, eyes passed rapidly back and forth and then fell." "I seem to have certain codes already established as to what I do or do not like in colors; I know I like dull colors and a blue violet. Yet I think that these decisions were made on a purely affective basis."

The remaining 24 judgments were, perhaps, more definitely associative in character. "These judgments were all mediate, and the associations of eggs and grass rendered it difficult to decide which I liked of the two in 21." "Usually the judgment is instantly made, perhaps largely by prejudices which may have been affective judgments at first or may have been determined by convention and usage. These prejudices are very decided and act at once and without hesitation." "There was a distinct association of biliousness and caterpillar juice with that yellowish green which made me reject it, without liking the alternative color especially. The bright greens and yellows seemed too crude, but the mixed green and yellow was nasty (whether it was unpleasant in itself I cannot say), and I don't think the associations crowded up with the yellow green; only I had evidently decided that that color was nasty and repulsive from its associated objects." "But these affective judgments have settled into such codes that the judgment may not be purely affective any longer." "When I had up the preferred dull against the preferred brighter but bluer violet, the lack of affective elements came out more strongly; it seemed to be a matter of which judgment of liking did I cling to more tenaciously, dark colors, or blue violets." "I think I do have an initial attitude of readiness to embrace or to be repulsed by the color. I think this attitude is followed by a movement toward or away from the color."

It seems fair to conclude that the affective judgments of color impressions are or may be immediate; the great majority of W's judgments were, as a matter of fact, passed without any mediation.

W states that her attitude in observation may be that of active attention, in which event she uses the terms "like and dislike", or that of passivity and receptivity, in which event the terms "pleasant and unpleasant" suit better. She says also that it may make a difference, not only in the ease and rapidity of judgment, but also in the judgment itself, whether the attitude is active or passive. "Most of these decisions are made instantly and with no trouble whatever in both the active and passive state, but when a difficult decision has to be made it is much harder and slower if the mind be passive. I think that more associations come with the passive state, but I am not sure of this. When pale blue and yellow were compared I liked the blue better in the passive state and the yellow better in the active. The blue caused me less excitation and was more pleasant. The yellow I liked better, and found more delightful if I took the trouble to enjoy it. There was not much I could do with the blue; it was just there."

The observer G at first assumed the reflective, analytical attitude of which we have already spoken; the result was that in the early stages of the experiment he often found very little to choose between the colors, and his judgments were frequently doubtful. However, he soon became convinced of the immediacy of the affective judgment, and thenceforward found it easy and prompt. Of his 325 comparisons, 272 were of the direct type. The following illustrations may suffice. "The greater pleasantness in nos. 16, 18, and 19 was quite pronounced. I felt really pleased by the colors as such; no secondary criterion seemed even to lurk in the background." "I had an idea that there would be many more doubtful cases than I have had so far. Usually, however, it was not difficult for me to decide. I noticed no bodily processes whatever, and cannot remember anything that entered consciousness during the attention to the colors." "It was not difficult for me to decide, but I am vexed if one asks me how and why I like one color better than another. I am sure in this series there were no associations, except perhaps that one chocolate-brown called up an image of the wrapping of Hershey's Chocolate. However this did not influence my judgment." "With one exception judgments seemed easy, immediate and unhesitating. Usually I had made my preference before the second signal came." "I believe I could reconstruct a great deal of the background of memory or reason or both, but don't have the faintest idea that this would help me in deciding why I liked one color better than another." "The pleasantness in these colors was not pronounced. Yet I had no difficulty in deciding which I liked better. There were no associated ideas, nor any verbal or visual images connected with the stimuli sometimes." "I am still at a loss to tell how I prefer and why I do so. The background processes of consciousness don't seem to have anything to do with it. The two colors are in the focus of attention, the rest is almost unconscious." "The judgments came quickly and immediately, without interference of associated ideas or any other conscious processes." "The degrees of preference seem to be quite distinct to-day. Nevertheless the judgments are made immediately and without deliberation. It is only after the decision that I begin to make distinctions as to greater or lesser degrees of preference."

The remaining 53 judgments were of the following kind. "In no. 13 the right color was constructed or synthetized out of the left *plus* a little red, just enough to destroy the pure saturation of the right in comparison with the purer left color. The same consideration influenced also my next judgment, where the same color was to be compared with another." "I think I judged more the saturation. In this, however, my judgment was immediate. But at the beginning I could not avoid a short reference of the two colors to circumstances under which they had been seen before. Also in nos. 24 and 25 I judged more the saturation." "Again the purer colors were preferred. The third pair was distinctly judged by saturation." "The judgments were easily made simply on the basis of color-preference, no other criteria seemed to enter except in no. 219, where the left was rejected on account of its poor saturation." "The preference was quite easy, based on an immediate æsthetic pleasure derived from one color, sometimes on account of its shade, where saturations were about the same." "This was a plain preference of better saturations. Why I like them better I am unable to say." "In the very first comparison the judgment was not so much on the color as on the brightness. The suggestion of clear and dirty influenced the decision. In a less degree also the second pair was judged in this way. The real affective tone was very slight."

W, as we have just said, reports an oscillation of active and passive attention in the color work. G, who in the harmonical experiments insisted strongly on the advantages of the mood of passivity and receptivity, now reports, throughout the series, an active concentration of attention upon the stimuli as most favorable for the passing of the affective judgment. As a rule, he entirely failed to notice what was going on in the background of consciousness while he attended to the colors.

For G, there is no difference in the judgments expressed by 'pleasant' and 'like', 'unpleasant' and 'dislike'. Here we seem to be in presence of an individual difference between the observers. The general tendency to a passive reception of tones and an active attention to colors may be ascribed, with some confidence, to the different impressiveness or attention-compelling character of these stimuli. The tones are, without doubt, more striking, less escapable than the colored squares.

The results of these two sets of experiments appear to warrant the conclusion that affective judgments may be and usually are as direct and immediate as the sensory judgments of psychophysics.

II. The Method of Single Stimuli

The experiments which we are now to report were planned at the same time as those described above, but were made earlier, namely in the months of October to December, 1907. They are postponed to this place, since they lead on naturally to the further experiments of Section IV, which were performed last of all.

Selected stimuli, twelve cutaneous and twelve olfactory, were presented to the observer in separate series, one at a time and in chance order, for 15, 10 or 5 sec. The method employed is thus that named by Brahn the *Reizmethode*.[1] Our object was, by fitting instruction to the observers, to secure reliable introspective data as to the nature of affective process and its difference from sensation. The stimuli were chosen with a view to their probable affective value, as well as with a view to their perceptual characters; and the observers were asked to take each one as it came, and to say what it did to consciousness,—to give their immediate conscious reaction upon it, without making an effort to identify or to analyze. We hoped that, if some of these reactions were definitely affective and some definitely perceptual, we should be able to appraise the various criteria of affection proposed by other investigators.

It may be said at once that the longer times of presentation of stimulus were rarely utilized by the observers. Only in the case of certain cutaneous stimuli, and in that of a few scents of extreme pleasantness or of unknown quality, was the limit of 15 sec. reached. The aim of the experiment was to secure the immediate conscious response to the stimulus, and in many instances this response was made in 1 sec. or even less. Hence the time was reduced from 15 to 10 sec., and in the latest series to 5 sec.

The cutaneous stimuli employed were (1) sandpaper, (2) heavy plush, (3) glass, (4) rough felt, (5) quicksilver lying in an enamel-ware pan, (6) rubber sheeting stretched over the mouth of a glass funnel, (7) fine sand spread in a flat wooden box, (8) smooth stretched leather, (9) a smooth brass plate heated to about 55° C., (10) ice water, (11) a blackboard eraser, and (12) polished hard wood. On the signal, the observer, seated at a table with closed eyes, made a pawing movement over the surface of the stimulus with the tip of the right forefinger. The olfactory stimuli, contained in similar glass bottles, were (1) carbolic acid, (2) mixed vanillin and cumarin, (3) benzole chloride, (4) listerine, (5) oil of anise, (6) sulphuric ether, (7) essence of peppermint, (8) valerianic acid, (9) violet water, (10) asafœtida, (11) nitro-benzole, and (12) musk. On the ready signal, the bottles were taken in the left hand, and at a 'now' were uncorked by the observer, raised to the nose, and inhaled. Here, as before, the observer had his eyes closed during the experiment; in no case was the

[1] M. Brahn: Experimentelle Beiträge zur Gefühlslehre, *Philos. Studien*, XVIII, 1903, 133. Titchener: *ibid.*, XX, 1902, 404.

introspective record influenced by sight of the stimulus. Every care was taken to avoid adaptation of the organ, or diffusion of an odor in the room in which the work was done.

The observers were Miss West and Mr. Geissler, of whom we have already spoken; Mr. W. H. Pyle (P), assistant in psychology; and Dr. Weber (Wb), a graduate of the University of Pennsylvania.

In their main purpose the experiments miscarried. Whether this miscarriage was the fault of the method itself, or of our selection or manipulation of stimuli, we do not know. We took every precaution that we could think of, and we naturally incline to blame the method. At any rate, what we got was not any clear-cut differentiation, on a positive basis, of affection and sensation, but rather a reflection of the theories and idiosyncrasies of our observers. Thus P entered upon the experiments with the idea that pleasantness and unpleasantness are attributes of sensation, and his introspective records are for the most part couched in terms of this opinion. W, as we have seen, has keen and varied experience of organic sensations; she began the work, not with any definite theory of the nature of affective process, but with the general expectation that it would turn out to be an organic complex; and organic sensations figure largely in her records. G was inclined to look upon affection, in Wundt's early manner, as something relational, perhaps even as the result of an unconscious judgment or inference. It is easily possible, in his as in W's case, to make the prepossession too explicit, to formulate expressly what was in reality only a sort of trend or tendency or line of least resistance; but it is at least safe to say that G's attitude was reflective, intellectually analytic, and not receptive. He was accordingly disposed to note whether the stimulus gave 'objective' knowledge, or whether it produced a 'feeling of change' in the sense-organ; whether it was accepted as informatory, or whether it aroused movement by attraction or repulsion, etc. Under these circumstances, the question which we had set ourselves to answer naturally remained unanswered. The observers did their best, under the instructions given; but the method, as we must think, showed itself inadequate.

We add that all three observers later changed their opinions, and declared for the independent status of the affective qualities. And this change was due, in some part, to the work by the present method. G, for instance, was struck by the objective immediacy of the affective coloring of certain smells and touches; his reflective distinction could not always be carried through. P was impressed by the fact, made known to him at the conclusion of the experiments, that the same smell might appear in his records now as pleasant and now as unpleasant. While, however, the method of single stimuli may be credited with the impulse to this change of view, it can hardly be credited with more. The change of view was by no means completed when the experiments were given up for the series described in Section I, and we have no reason to believe that the method would have availed to complete it within any reasonable time. The main value of the work—with a single exception to be mentioned later—lay in the practice it afforded for introspection of what were oftentimes vividly affective consciousness.

The remaining observer, Wb, manifested no special interest in affective problems. He had, however, a very strong recognitive tendency, which our instructions failed to inhibit. Wherever possible, he sought to identify the perception; and where this was impossible he turned his attention to a characterization of the stimulus in terms of intensity, duration and associated ideas. No doubt, the recognitive habit would have been broken up in time; but Wb was unfortunately called away from the laboratory in December. We should not, in any event, have continued work with the method of single stimuli, as the three months of practice had no such appreciable effect upon Wb as we observed in the cases of W, G and P.

The one positive result obtained from these experiments has to do with the time-relations of the arousal of sensation and affection. Wundt has recently maintained that the affective process may enter consciousness alone, as the herald of the sensory process with which it is connected.[1] Our results, so far as they go, definitely negative this doctrine. The following table speaks for itself.

			Simultaneity	Sensation first	Affection first
P	Touch	144 obs.	38	106	0
	Odors	144	111	29	1?
G	Touch	72	28	43	1?
	Odors	72	27	27	0
W	Touch	48	16	32	0
	Odors	48	26	21	0
Wb	Touch	96	10	86	0
	Odors	96	57	36	0

The discrepancies between the number of observations and the number of recorded judgments for 'odors' are due to the

[1] Grundriss, 1905, 262 (Engl., 1907, 243); *cf.* Ladd, *Psychology, Descriptive and Explanatory*, 1894, 181.

fact that the stinging, pricking character of a smell sometimes overpowered its odor, and further, in the case of G, to a temporary catarrhal condition which led to a number of reports of 'no smell'. It seems best to omit all observations to which this report was attached, though it is right to say that the pricking sensations always came with or before the associated unpleasantness.

Here, then, are 695 observations, in which there are only two, and those doubtful references to a temporal procedence of affective process. G's touch record (rubber sheeting) read simply "the most pleasant": nothing was said of the smoothness, softness or elasticity which is mentioned in the remaining five observations on the same substance. It seems clear, however, that the judgment was comparative, implying the presence in consciousness of ideas of stimuli previously presented; so that the independent appearance of the affection is not made out. P's odor record (valerianic acid) consisted of the single word "awful!" Here again there is no positive evidence that unpleasantness only, and not an unpleasant odor, was dominating consciousness.[1] On the whole, then, the results speak strongly for simultaneity or for the succession sensation-affection. Either the affection appears distinctly later than the perception, or the two factors are in James' phrase, "beaten up together in one consciousness."[2]

We had anticipated that the cutaneous stimuli would, in the main, set up typical perceptual consciousnesses, and the odors typical affective consciousnesses, although we chose the stimuli with the view of securing both types of consciousness within each sense-department. The anticipation is in so far fulfilled as that, with all four observers, the touch-experiments show a marked preponderance of sequence (sensation first) over simultaneity. Only P's results show, for odors, a similar marked preponderance of simultaneity. This may perhaps be due to P's theoretical attitude to the affective problem, which was, as we have seen, different from that of the remaining three observers. However, the observations are too few to allow of safe inference. Our excuse for their small number is the well-known duration of smell adaptation: we never succeeded in taking more than 12 observations with odors during an experimental hour, and this maximum was but seldom attained.

---

[1] It should be remarked that these reports are unusually brief, though (as the observers were instructed to give the immediate conscious reaction on the stimuli, and to disregard later reflection or association) the introspections rarely exceeded half-a-dozen descriptive terms.

[2] Psychological Review, i, 1894, 523 f.

III. Incidental Results of the Foregoing Experiments

Some of the incidental results of the experiments described in the preceding Sections have a bearing upon vexed questions of affective psychology. We now proceed to report them.

(*a*) *Qualitative Differences within P-U.* It is still a matter of controversy whether pleasantness and unpleasantness are the names of single affective qualities, or whether they are collective terms, covering a greater or less number of such qualities. Only one of our observers, W, finds anything like a qualitative differentiation. The remaining observers never reported differences of quality during the experiments, and when questioned on the matter, afterwards, they affirmed that they had found no evidence whatsoever for more than the single qualities, pleasantness and unpleasantness.

W's records for tones are as follow.

I found 94 very hard to compare, because the kinds of unpleasantness were so different. They [the tones] were both unpleasant: the first one caused strains, was thin and shrill; the other was deep and rough, and caused a sort of shudder.—Record X.

I don't know what makes the jarring sensations unpleasant. But I know that we are not speaking of the same thing when we speak of a jarring sensation being unpleasant as when we speak of the ominousness of a tone being unpleasant.—Record XII.

It was hard to say which I minded most, the melancholy mood or the ear strains; they did not seem comparable, but on the whole the strains seemed worse.—Record XV.

In 79 I noted the different unpleasantnesses of a shrill tone giving strain around ear and of a rough tone causing unpleasant vibrations of drum and nasal parts.—Record XVIII.

I noted a great variety of ways of being unpleasant through vibrations in ear and nose.—Record XXI.

Similar reports upon colors will be found under (*d*) below. When asked to comment on these records, at the conclusion of the experiments, W wrote: "I do not believe that I get any affective quality without obtrusive, though hardly analysable, organic sensation; the whole is inextricably blended, appearing as simple as a mood. My liking of red is (as I have said) altogether different from my liking of the color of a hepatica. With red I am excited and satisfied at once (whatever that means in terms of organic sensation; sensations of tingling come, from face and chest muscles especially). The blue-lavender color excites me in a different way: I have an expansion of the chest, with deep breathing, and an unsatisfied feeling: the emotion of 'longing' comes as near to this as I can describe, though the two feelings are not identifiable. I believe that I ought to say, on reconsideration, that the judgments of qualitative difference in affective quality refer always to this kind of blend of affection with (only very partially analysable) organic sensation. The organic sensations are very lively in me." It is, then, quite possible and even probable that the qualitative differences in question are sensational and not affective in their nature.

(*b*) *Mixed Feelings*. W is again the only observer who reports a mixed feeling, that is, a simultaneity of pleasantness and unpleasantness. The remaining observers are strongly of the opinion that mixed feelings do not occur. The records are as follow.

One of those tones, though unpleasantly twangy, had an associated idea of a bugle call to action, which was both pleasant and unpleasant, but was at least more satisfactory than the thin meaningless tone with which it was compared.—Record XXV.

There was something in the second tone of 162 that made it unpleasant, somewhat so, in spite of its sweetness; maybe a sort of prejudice against notes very high or very low.—Record XVII. The tone was g^3.

There were no similar reports upon colors. When asked to comment on these reports, at the conclusion of the experiments, W wrote: "I have never theorized or thought about mixed feelings; but I seem often to get them with tones, and I am surprised that they did not appear more frequently in the experiments. You must remember that 'feeling' to me means a blend of pleasantness or unpleasantness with organic sensations. It is possible that some of the organic sensations set up by the tones were pleasant, and some unpleasant, and that in recalling the experience I gave as simultaneous what was not so in reality. I cannot now say, and I do not think that (at any rate with an observer of my type) the method is suitable for settling the question." We had planned to make special experiments with W upon this and the previous point; but the observer was unexpectedly called away from the university, and we were therefore unable to do so.

(*c*) *Affective Localization*. A few cases of affective localization occurred in the experiments by the method of single stimuli.

G. Rubber membrane. The pleasantness seemed to be localized entirely in the finger-tips.

G. Carbolic acid. The affective tone seemed to be localized only in the mucous membrane.

P. Vanillin-coumarin. It seems as if the pleasantness is a separate sensation in the nose, different from the odor proper.

P. Valerianic acid. The unpleasantness seems to be localized in the upper part of the nose.

In three of these reports, the affection is plainly localized along with its sensation; consciousness has, to all intents and purposes, been narrowed down to a single sense-feeling (*cf.* Titchener, *Feeling and Attention*, 336). P's account of a separate, sensation-like affection seems at first sight to imply a separate localization; but there can be little doubt that the localized pleasantness attached, in this case, to sensations aroused by stimulation of the trigeminal endings. P often reported, with the pleasant odors, tickling sensations in the nose which were themselves intrinsically pleasant; and, with the unpleasant odors, tickling or tingling sensations. These last blended with the quality of the odor into a unitary complex.

We add, finally, a brief account of (*d*) *individual differences in affective reaction to colors.* The colors employed were: VR tint 2, VR, VR shade 2, R, OR, RO, O, YO shade 1, YO, OY tint 1, OY, OY shade 1, Y, GY, YG, G, BG shade 2, BG, BG tint 2, GB, GB shade 1, B, VB, BV, V, RV. The reactions of the observer G may be summed up as a preference for highly saturated colors that are not too bright, a special dislike for browns of low saturation, and a decided liking for YG. The dislike of little saturated browns was mentioned by the observer; he was not aware of his liking for the YG. Towards the end of the experiments, however, he reports: "The YG was strikingly unpleasant this time, though I remember having preferred it in other combinations." The reactions of the observer W are more differentiated, and appear to be worth a report in full.

Violet red, orange red, and violet blue. I find reds that I like in quite a different way from the violet and blue alone. I don't know whether there is a common element in the way I like all these colors, but I think there is in the way I like the red and the violet, though there is a difference too. The violet has a lighter, giddier, more exciting effect. The red seems merely to affect the retinas more.

Violet red, shade 2. The dull color in 157 seemed restful because of its dullness as well as pleasant in itself. I think there were two elements there, one affective and one organic from eye muscles relaxing or else pleasant to retina itself.

Red (the red is rather dark due to the oxidation of the pigment.)—There came in a faint associated idea that I did not like dark red dresses; it did not, however, make me dislike the dark red. And yet there was no active liking of it; it merely did not trouble me; my eye could rest on it easily; it was dark and faintly restful.

Orange red, red orange, orange, orange yellow, yellow green and blue green. The oranges were irritating and aggressive; the orange yellow was felt clear, pure and restful by comparison with the oranges.

The sort of disagreeableness from the bright oranges is different from that of the blue green or yellow green; the one antagonizes (fights) me and the other disgusts me.

A brighter yellow I did not like at all. I do not like the glare of the bright colors; they almost hurt my eyes.

Orange yellow, green yellow, and red violet. I cannot get anything but a feeling of disgust to those I really dislike, like that green yellow in 100; while the bright clear colors like the orange yellow caused no disgust; they only tired me, and I said the word "crude" with them, they seemed raw and rasping. They gave me a restless feeling of being on edge.

The red violet I disliked most and the green yellow was most disgusting. I know the difference in the feeling, but I cannot analyze it. The disgust was accompanied by movements in the chest and facial muscles, and a sort of dropping feeling. The dislike was more aggressive and the color seemed more aggressive.

Green blue and blue. The green blue in 22 brought a distinct sense of relief; especially when I looked at it from the yellow green.

In 180 there was no dislike to the blue, but a positive feeling of discomfort in the presence to my eyes of such a raw color (lacking gray or white).

Blue violet, and green blue shade 1. At the blue violet in 73 I was conscious of saying "ah" on a long breath, and I think I relaxed all over; I did something all over and it was not a start up. I think I had a typically pleasant feeling there; it was very absorbing and left no room for thinking anything; it was a feeling half of wonder and half of "ah, that's good," and my eyes felt as if they rested on the color and never wanted to leave it. I think there were breathing sensations, and it left a kind of hunger for more which I think was in the chest.

I liked both the colors in 203 and they seemed to present two different kinds of pleasantness. The green blue was restful and satisfactory, altogether good to look at, but unexciting. The violet was exciting. As I looked from the one to the other I felt a distinct change, a lift of the chest, I think, and tightening of facial muscles, and a catch in the inspiration when looking at the violet. There was more than this; a tickling feeling in chest; and besides these bodily feelings there was something else too.

Violet and red violet. In No. 76 I seemed to have succeeded in the discovery of the pleasantly affective attitude. It seemed to me the attitude was nearly that of attention; eyes seemed fixed to the spot and straining out of head; body seemed alert, breath drawn in with a little almost tickling sensation at bottom of chest; all this plus something which I cannot analyze, a sort of buoyant feeling. No crowding in of associations or thoughts that I was aware of. I did like the violet when it came, and I seemed to still expect to like the color in 77, for when 77 (blue vs. red violet) came, I had a feeling of disappointment, a let-down feeling and turning away of the head. I am not sure but that there were muscular contractions in throat as if to spit it out. I am sure there was a general movement of backing off, with turning of head away,

Red violet, and yellow orange shade 1. I got a distinctly unpleasantly affective quality in 85, but when I tried to analyze it only got quivery sensations running up and down my arms and legs. There was a strong disgust for both and it seemed exactly the same in the l. (yellow orange) as in the r. (red violet), although I had no prejudice in regard to the orange. And it seemed intensified as I tried to bring them in at one glance. I think there were feelings in the chest of contraction.

General observations on the affective values of the various colors. The clear, not too bright yellow gives a light feeling; I think it is an idea; an idea of lifting up and lightness; not an actual lifting of the chest and head, but these may be there. I think the eyes open wide, and you feel as if you could look through it; it is exciting, and quite different from the dull blue which is also pleasant. You want to look straight at the blue; it is commonplace but nice. The bright red, again, is pleasant in a different way; it is exciting, but not like the yellow. It is a little aggressive, and you don't want to look at it too long, or look through it. You could not bear to imagine the air red, but you could stand the air that clear yellow for a little while. The blue violet is exciting, again, but something is added; more organic sensations, and you want to live in it. The pale blue and pale yellow are again pleasant in a different way, but I cannot analyze this at all. The dark green and dark purple red are pleasant, more as the dark blue is, but there is an exciting element in the dark purple red. I should say that the dark purple red, the dark green, the dark blue, the pale blue, and the pale yellow excite æsthetic feelings; this seems to be due largely to the mixture of white and gray. Yet I do not find it in the pink.

Disagreeable blue greens and pink reds and yellow green have an affective quality, which is almost distinct enough to be a sensation; I don't know whether it follows on organic sensations or not. It seems to pierce you; to do something to you and to be very positive.

IV. The Discriminative Reaction

Two methods are at hand for the investigation of the time-relations of sensation and affection. There is, first, the graphic method, in which the observer may register the moment of appearance of the sensory or affective process. The writer made a series of experiments with a simple form of this method in the Harvard Laboratory in the year 1906-1907: the stimuli employed were colors, geometrical figures, and illustrative photographic pictures, and the result was that the affection set in noticeably later than its sensation. These experiments will be reported in a later paper. There is, secondly, the method of reaction. We took, during the months of December, 1907, to April, 1908, a series of discriminative reactions to cutaneous stimuli, which are now to be described. Discriminative reactions to odors were also planned, but for lack of time have not been carried out.

We selected five pairs of stimuli, with the characters: hard, soft; rough, smooth; sharp, blunt; wet, dry; hot, cold. Wooden blocks, 3 by 2 by 1 in., having a slightly raised rim, were used to hold the stimulating surfaces. For *hard* we inserted in the block a plate of sheet brass; for *soft*, a layer of cotton wool; for *rough*, coarse sandpaper; for *smooth*, a sheet of kid leather; for *sharp*, an even field of short tacks, point up (the points were slightly blunted, in order to avoid injury to the finger); for *blunt*, an even field of furniture tacks, head up; for *wet*, water-soaked cotton wool; for *cold*, a sheet of brass cooled in ice-water; for *hot*, a sheet of thicker brass heated to about 50° C. For *dry* we employed the smooth surface of the wooden block itself. The reactions were taken by means of the vernier chronoscope, model II.[1] The stimulus block rested partly upon a support, placed on the table, and partly on the button of the release-key of the longer pendulum. The pressure upon the button was so slight as not to interfere

[1] E. C. Sanford: Improvements in the Vernier Chronoscope, this *Journal*, xii, 1901, 592.

with the working of the instrument. The observer sat with his left hand in a specially prepared rest, grasping the sides of the stimulus block by his thumb and middle finger. The forefinger thus extended over the stimulus surface; and the stimulus was applied (and the long pendulum released) by a single pawing movement of the finger-tip downwards. The forefinger of the right hand rested, of course, upon the button of the release-key of the shorter pendulum.

The observer was given general directions for the performance of the discrimination reaction[1] to the characters hard-soft, rough-smooth, wet-dry, hot-cold, sharp-blunt, pleasant-unpleasant. In the actual experiment, the experimenter would say, for instance: "Hot or cold? Ready!" whereupon the observer would close his eyes, and adjust his two hands to the instrument. As soon as the observer was in position, the experimenter called "Now!" The observer then made the movement with the forefinger of his left hand which gave the stimulus and started the longer pendulum, and after discrimination of the character of the stimulus pressed the key of the shorter pendulum with the forefinger of his right hand. The experimenter counted off the swings of the two pendulums in the usual way.

Preliminary experiments made by the writer and Professor Titchener led us to believe that the necessary manipulations would readily be learned by the observers. As all had had practice in the ordinary reaction experiment, we planned to continue their special practice only to the point at which a set of 25 reactions could be made without error; and we expected that this point would be reached in three or four hours. We were over-sanguine. In all cases, the preliminary practice covered a period of several weeks; and in the case of R, W and C an errorless series could not be obtained at all, and we were forced to rest content with what appeared to be an irreducible minimum of mistakes (too light pressure upon the one or the other key, confusion of the tasks of the two hands). The number of these wrong reactions is shown in the Tables.

The observers were Mr. Geissler, Mr. Pyle, Miss M. G. Rand (R), Miss A. T. Waldie (W), and Mr. W. D. Clark (C). The three last had had two years of laboratory training in psychology. A complete set of experiments comprised 50 stimuli, 30 of which were presented for sensible and 20 for affective discrimination. Great care was taken to distribute the ten surfaces evenly, and to utilize them all in the affective work. The 50 stimuli were so arranged that the same surface was never presented twice in succession, for the two kinds of reaction; and the order of presentation was reversed from set

[1] E. B. Titchener: Exp. Psychol., II, i, 1905, 186 f.

to set. A half-set, of 25 reactions, was taken in the experimental hour.

Tables I and II give the results of 350 reaction-times obtained from the observers G and P; 210 times of sensible, and 140 of affective discrimination for each observer.

TABLE I

Observer G. Unit, 1/50 sec.

	Affective discrimination times						Sensible discrimination times					
Stimulus	Range	Median	MV	Average	MV	Wrong	Range	Median	MV	Average	MV	Wrong
Hard	74-88	80	5.3	80	5.5	0	50-58	54	2.8	55	3.1	0
Soft	56-88	70	10.1	72	11.0	0	34-48	42	3.0	41	3.0	0
Rough	62-86	79	6.9	77	7.5	0	54-76	66	5.1	65	4.9	0
Smooth	76-96	81	4.9	83	4.7	0	54-78	66	6.1	67	6.0	0
Sharp	32-50	40	6.4	43	7.3	1	28-38	34	4.1	35	4.6	2
Blunt	54-86	70	9.2	69	9.7	0	44-58	51	4.5	50	4.7	0
Wet	54-82	71	8.5	69	8.6	0	46-62	55	4.7	53	4.5	0
Dry	80-99	86	6.0	85	5.3	0	54-70	64	4.5	61	4.1	0
Warm	54-82	79	8.7	73	8.6	2	54-86	74	6.1	74	6.3	0
Cold	78-88	85	7.6	84	8.2	0	54-83	71	5.3	67	5.8	0

TABLE II

Observer P. Unit, 1/50 sec.

	Affective discrimination times						Sensible discrimination times					
Stimulus	Range	Median	MV	Average	MV	Wrong	Range	Median	MV	Average	MV	Wrong
Hard	72-86	81	4.6	82	4.4	1	52-72	60	6.4	61	6.8	0
Soft	58-72	62	6.2	64	6.0	1	38-50	42	3.8	44	3.8	0
Rough	80-108	93	12.0	96	12.0	1	50-68	58	5.2	58	5.2	0
Smooth	60-86	73	6.7	73	6.7	0	48-68	53	4.8	55	5.7	0
Sharp	52-64	55	6.0	57	4.7	0	38-48	40	4.1	42	4.0	1
Blunt	84-94	91	14.0	90	14.0	1	46-62	59	5.0	56	4.8	0
Wet	74-86	82	8.0	81	7.0	0	56-76	68	4.9	67	4.5	0
Dry	70-78	80	4.5	75	2.3	1	62-82	73	5.8	75	6.8	0
Warm	71-98	84	7.0	82	6.5	0	70-86	76	4.7	77	4.5	0
Cold	80-100	91	8.5	89	7.7	0	78-94	86	5.3	87	4.3	0

Inspection of these tables shows that the affective discrimination times are uniformly longer than the sensible discrimination times. In general, also, as might have been anticipated, the *MV* of the affective reactions is larger than the *MV* of the sensory; exceptions occur only for *G* with the stimulus *smooth*, and for *P* with the stimuli *hard* and *dry*. Nevertheless, the *MV* of the affective reactions is by no means abnormally large. And it can hardly be doubted that the affective times represent the observers' discriminative reactions in precisely the same way as the sensory times. The individual ranges are of the same order; and the total range of the sensory times, from 28 with *sharp* to 86 with *warm* for *G*, and from 38 with *sharp* to 94 with *cold* for P, are paralleled by the affective limits 32 (unpleasant) and 99 (pleasant) for the former, and 52 (unpleasant) and 108 (unpleasant) for the latter observer. The Tables, therefore, seem to show conclusively that the affective processes are amenable to the reaction method.

The observer P discriminated the *warm* stimulus sometimes as warm, and sometimes as hot. The average times are:

	Warm	Hot
Sensory:	20 reactions: 78±4.5	20 reactions: 98±7.1
Affective:	15 reactions: 80±6.0	15 reactions: 108±8.0

It would, of course, have been exceedingly difficult, in an experiment like ours, to keep the temperature of the stimulus constant; and beside taking an initial test and carefully regulating the time-relations of the experiments, we made no effort to do so. In so far, then, as these figures depend upon objective differences in the temperature of the stimulus, they are in agreement with Thunberg's statement that the simple reaction-time of heat is longer than that of warmth (*Zeits. f. Psychol.*, xlvii, 1908, 166 ff.). There were a few cases, however, when the effect of the stimulus was not simply warmth or heat, but when the sensory experience began with a warmth, which quickly passed over into heat. In these cases the observer might react at once to the warmth, or might wait for the development of the perception of heat; and the times naturally varied in accordance with his attitude.

The following Tables III-V show the results obtained with the three other observers.

TABLE III

Observer R. Unit, 1/50 sec.

	Affective discrimination times						Sensible discrimination times					
Stimulus	Range	Median	MV	Average	MV	Wrong	Range	Median	MV	Average	MV	Wrong
Hard	34-40	36	2.7	37	2.8	1	28-40	32	3.0	34	3.0	1
Soft	34-51	42	2.5	41	3.0	3	28-44	34	1.3	34	1.7	1
Rough	32-60	39	4.1	40	4.1	2	26-50	36	2.8	37	2.3	2
Smooth	36-55	44	3.5	43	3.5	1	30-46	40	2.8	38	3.0	1
Sharp	29-43	32	2.5	32	2.5	4	26-36	30	2.0	31	2.0	3
Blunt	32-44	36	3.3	37	3.4	2	24-30	28	1.4	28	1.4	1
Wet	32-53	40	3.2	38	3.6	2	30-48	32	2.1	36	1.8	1
Dry	38-48	40	3.5	43	3.5	1	30-44	34	3.3	35	3.4	2
Warm	34-55	40	3.2	41	3.8	2	32-46	40	3.0	39	3.0	1
Cold	46-74	52	4.1	50	4.0	1	30-47	40	2.4	41	2.5	1

TABLE IV

Observer W. Unit, 1/50 sec.

	Affective discrimination times						Sensible discrimination times					
Stimulus	Range	Median	MV	Average	MV	Wrong	Range	Median	MV	Average	MV	Wrong
Hard	44-60	48	4.3	48	5.1	2	30-44	36	4.0	38	4.6	1
Soft	48-66	58	4.8	59	5.3	6	28-38	34	2.0	34	2.0	5
Rough	26-50	46	5.6	43	6.6	5	28-44	32	3.5	34	3.5	4
Smooth	32-48	44	3.2	42	4.4	4	22-34	30	1.8	31	2.0	3
Sharp	34-50	38	5.5	40	5.0	6	26-34	29	2.7	30	3.0	6
Blunt	38-60	42	8.1	49	7.0	2	34-48	38	4.5	41	4.5	2
Wet	36-54	46	6.1	47	6.1	3	30-46	38	4.0	39	4.2	2
Dry	38-60	46	4.8	44	5.0	4	30-46	44	6.6	39	7.6	4
Warm	42-69	54	9.0	55	9.4	2	38-60	50	7.0	49	6.4	4
Cold	42-62	58	8.8	59	9.2	2	40-44	42	2.1	43	2.1	2

TABLE V

Observer C. Unit, 1/50 sec.

	Affective discrimination times						Sensible discrimination times					
Stimulus	Range	Median	MV	Average	MV	Wrong	Range	Median	MV	Average	MV	Wrong
Hard	84-130	99	15.0	108	12.7	3	40-70	54	6.7	55	6.8	1
Soft	54-108	82	22.0	85	17.4	3	46-66	58	5.0	56	5.1	2
Rough	66-150	84	28.0	106	21.3	3	58-90	68	9.4	69	9.0	5
Smooth	86-102	81	6.4	89	4.0	2	58-66	60	2.3	62	2.6	2
Sharp	58-114	76	15.0	80	15.0	2	32-54	50	5.6	46	6.0	4
Blunt	54-106	84	28.8	103	28.4	4	48-72	60	7.0	60	7.0	3
Wet	66-98	80	9.9	81	11.2	2	52-74	62	6.9	60	7.4	2
Dry	98-148	90	9.1	94	9.5	4	46-60	52	4.0	54	3.6	2
Warm	84-108	89	9.8	88	9.0	3	74-94	81	4.8	84	4.5	2
Cold	82-152	84	18.0	103	18.6	1	62-90	79	7.7	77	8.3	2

The general results are the same as those of Tables I and II. In all three Tables, however, there are many more 'wrong' reactions than we obtained with G and P; it must be remembered that these two observers had had prolonged special practice in work upon the affective processes, while R, W and C were new both to the problem and to the particular method of reaction. It is striking that the absolute times given by C obviously fall into the same class as those of G and P, while the times given by the two woman-observers R and W are, throughout, shorter. Thus, if we average the averages, for the sake of a rough comparison, we find:

	Sensory	Affective
G	56.8	73.5
P	62.2	78.9
C	62.3	93.7
R	35.3	40.2
W	37.8	52.2

There was no difference, that we could discover, in the nature of the 'wrong' reactions as given by C on the one hand and R and W on the other; practically all of these reactions were due to the observer's omission to move his finger down upon the stimulus, or to his making the touch upon the stimulus surface so light that the pendulum was not released. The sole point of difference that we could find was this: that C took the difficulties of manipulation easily and in a matter of course way, whereas R and W were a little afraid of the apparatus, and braced themselves anxiously for its management. It is possible, then, that they hurried over the reaction, and made it cognitive rather than discriminative. The time required for the cognition of a color is, for instance, considerably less than that required for the discrimination of two colors (Wundt, *Phys. Psych.*, iii, 1903, 456, 458). If this interpretation is correct, we have shown, though unintentionally, that the cognitive as well as the discriminative reaction may be applied to the affective processes. We had required no introspections during the course of the experiments; the observers were simply asked to follow to the best of their ability the instructions given. When questioned at the end of the work, however, and when confronted with a careful analysis of the cognitive and the discriminative attitudes, R and W inclined to the view that they had, in fact, reacted cognitively. At all events, the important thing is that the affective averages are without exception the larger.

The stimuli *hard* and *dry*, which in the sensible discriminations were contrasted with *soft* and *wet* were apprehended by all observers in the affective discriminations as *smooth*. Since this change in apprehension destroyed the parallelism of the two halves of our Tables, we made some additional experiments (21 valid reactions with P and G, and 18 with R, W and C) in which these stimuli were contrasted with a *rough*. The results are shown in the following Table.

Discriminative Reactions To Smooth

Observer	Stimulus	Range	Median	MV	Average	MV
G	Hard	52-64	60	4.7	59	5.4
	Dry	50-64	58	4.2	60	4.8
P	Hard	44-54	50	4.1	47	4.9
	Dry	46-58	50	4.8	51	6.2
R	Hard	32-40	36	2.8	37	2.8
	Dry	35-44	38	1.7	39	2.8
W	Hard	41-56	45	4.3	46	4.0
	Dry	36-52	44	5.0	43	5.0
C	Hard	57-71	65	3.1	63	3.0
	Dry	55-71	62	3.1	60	2.5

If we compare these times, first, with the times for *smooth* in the preceding Tables, we observe that G and P show a slight shortening, presumably due to practice; R and C give practically the same values as before; W gives a somewhat longer reaction, due (in her own belief) to the persistence of the connection between 'smooth' and the kid-leather feeling of the earlier series. If, secondly, we substitute them in the previous Tables for the sensible discrimination times under the headings *hard* and *dry*, we find that the difference between the two types of reaction is somewhat lessened for *hard* in the cases of G, W and C, lessened to 0 in the case of R, and somewhat increased in the case of P; while the difference for *dry* is somewhat lessened in the cases of R and C, increased in that of P, and left unchanged in those of G and W. It is, of course, entirely possible that affective reactions, had they been introduced into this supplementary series, would again have given uniformly longer times.

On the general question of the time-relations of sensory and affective arousal, two opinions are *a priori* possible. Since feeling is readily and regularly 'expressed' in movement, it might be maintained that the movement which registers the appearance of an affective process in consciousness should occur at least as quickly as that which indicates the appearance of a sensation. Since, on the other hand, affection lacks the attribute of clearness, it might be maintained that a longer time must elapse than in the case of sensation before the movement of response is made. The experiments performed in the Harvard laboratory tell directly, those just described tell indirectly, for the second hypothesis. Or at any rate they show that, whether the reaction movement itself be performed more quickly or more slowly, the formation of an affective consciousness requires a longer time than that of a sensory consciousness.

A full discussion of the question would here, where we are dealing only with indirect evidence, be out of place; it will be given in a later paper. What we have proved in the present Section is that the method of reaction is applicable to the affective processes pleasantness and unpleasantness. The affective discrimination times are, as we have said, times of the same general order, and show the same sort of variability, as the sensible discrimination times. It remains only to add that there was no single case, in the reaction experiments, when the affection appeared first in consciousness, as the herald of the connected sensory quality (*cf.* pp. 181 f. above). Whenever the observer reacted to affection, he reacted to a pleasant or unpleasant cutaneous perception.

ON THE ANALYSIS OF AUDITORY MEMORY CONSCIOUSNESS[1]

By F. KUHLMANN, University of Illinois

TABLE OF CONTENTS

[1]The work on this study was done at Clark University during the academic year and summer of 1906-07. The experiment as originally planned not being completed, the report of the results has been postponed in the hope of carrying out the experiment as at first intended. Change of circumstances, however, has led me to take up other investigations and I offer the results now as they stand. I wish to express my indebtedness to the Clark University authorities for supplying the rather expensive special apparatus required and for the liberal opportunity given to carry out the work, and to my observers,—Dr. W. F. Book, Dr. H. L. Brittain, Dr. W. L. Guard, Dr. J. Morse, Dr. G. Ordahl, and Dr. J. H. White,— for their careful and expert work.

I. Introduction.

A. *Aim of Study.* The general aim of this study has been the same as in preceding ones on the analysis of visual memory.[1] This is to determine the conscious processes that enter in memorizing and in recalling given sensory material, together with the description of their character and function. It has been discussed sufficiently in the former studies and will not be considered further here. In the analysis of auditory memory, however, a special difficulty is met in determining the detailed character of the auditory imagery and the changes it undergoes with the lapse of time and other conditions. This is the difficulty of describing a sound, or its auditory image. Drawings and verbal description may quite accurately portray size, shape and color in visual memory, but there is no correspondingly adequate means of describing in detail the durations, rhythms, and variations in pitch and intensity of even a very simple sound. The present results on the nature of the auditory imagery and the changes it undergoes are therefore of a somewhat general character only. On the other hand, the method has been quite adequate for determining the presence, nature and function of the non-auditory processes in learning and in recalling the auditory material. Our main effort will be directed to the description of these.

B. *Method and Procedure.* It was desired to study auditory memory under conditions that approached as nearly as possible those of every-day experience. This required means of reproducing at will verbal discourse and the sounds of more or less familiar things in their true character. The graphophone method was finally chosen as the most satisfactory.

The experiment falls into two parts. The material for the first part consisted of verbal discourse; for the second part groups of sounds of familiar things were used. For the verbal discourse five descriptive records were so chosen as to range from a simple monologue to quite complex scenes in which several persons are heard.[2] For the second part of the experiment two groups of seven sounds each were made up. These were cut out of Edison and Columbia cylinder records. The seven small rings containing the sounds wanted for a

---

[1] See "On the Analysis of the Memory Consciousness: A Study in the Mental Imagery and Memory of Meaningless Visual Forms". *Psych. Rev.*, 1906, and "On the Analysis of the Memory Consciousness for Pictures of Familiar Objects." *Am. Jour. Psy.*, 1907.

[2] The following are the records that were used for the verbal discourse: Edison records: 1. McKinley's address at the St. Louis Exposition. 2. Waiting for the Dinner Horn to Blow. 3. Closing Time at a Country Grocery. 4. Two Rubes and the Tramp Fiddler. 5. Louis and Lena at Luna Park.

group were then stuck together again, making a new cylinder record.[1] A scale graded to a hundredth of an inch was fastened to the front part of an Edison machine. A pointer and magnifying lens fastened to the reproducer arm travelled in front of the scale. With this arrangement a little practice was sufficient to enable the experimenter to drop and raise the reproducer so as to pick from each ring just the sound that was wanted and no more, while the observer listened to the group. The substitution of a high grade alternating current motor for the spring motor of the machine, and special ear tubes with sound modulating attachment and means of regulating the intensity completed the equipment for presenting the auditory material.

For the verbal discourse the procedure was as follows. A descriptive record was heard two to three times at the first sitting and not again later. It was recalled four times with full introspective description of the process. The first recall was immediately after hearing, the second to fourth with one, three, and six weeks intervals, respectively, between two successive recalls. Three observers participated. With the groups of familiar sounds three series of experiments were made. In the first series the sounds were heard twice in immediate succession with immediate recall. It was found that two presentations were just sufficient to induce the recall of all, without hesitation, immediately after hearing. Four weeks afterwards the character of the sounds had been entirely forgotten. The second series was then taken with the same sounds and the same three observers, in the same manner as in the first series, excepting that the sounds were now presented three times in immediate succession. In the fourth series three new observers heard the same sounds four times in immediate succession, the procedure being otherwise as before.[2]

---

[1]Group I consisted of the following sounds: 1. Ringing of a bell. 2. Song of an English Wren. 3. Snare Drum. 4. Crow of a bantam rooster. 5. Lowing of a cow. 6. Laughter. 7. Bugle call—taps. Group II consisted of: 1. Cheers of a crowd. 2. Bugle call—assembly. 3. Peep of a chick. 4. Steamboat whistle. 5. Call of a peacock. 6. Bellow of a calf. 7. Bark and snarl of a dog.

[2]This constitutes the series with the familiar sounds so far as their results will for the most part be reported at present. In the actual conduct of the experiment, however, three recalls were made in the first series, one immediately after hearing and the second and third with one and three weeks, respectively, between successive recalls. The sounds were not presented again after the first sitting. The second series followed immediately on the conclusion of the first, and the sounds were presented three times a sitting for four successive days, with recalls on the first and fifth days. In the third series the sounds were presented four times a sitting with two sittings a day, continued

The sounds of a group were always presented in the same order, with ten to fifteen seconds intervening between successive ones. The following was then required of the observer: (1) A general, semi-passive recall in which he was required merely to name the sounds and give the order of the different kinds of processes that entered. He was asked not to stop to recall anything further about them. (2) His observations on his manner of memorizing the sounds. (3) The recall of each sound, one at a time, as vividly and completely as possible, giving his observations on the order of the different processes entering and on their nature and function in detail. (4) Next he measured off by means of a noiseless switch key what he thonght was the duration of the sound, repeating the measurement three times in immediate succession,[1] and gave his observations on the mental processes in this measurement of the duration. (5) After the conclusion of a series with a group of sounds, each observer measured the duration of each sound while listening to it, in order to determine its actual duration plus the observer's individual reaction time. The main attention and time was given to the third of the above requirements.

II. Analysis for Auditory Verbal Discourse.

A. *Factors in the Recall.* I shall consider first the manner in which the words and other sounds heard in the descriptive records were recalled. There were three distinct ways in which this was done. First, the auditory imagery of the words appeared at once without any process preceding as an aid to its recall. Second, concrete visual imagery of the persons and things appeared first as a means of recalling the words. Third, the words were inferred from the context as already recalled. The first way needs no further description.

1. Character and use of the Visual Imagery. While the observer listened to a record he visualized the persons and scene

---

for four days. The recalls followed immediately after the first sitting of the first day, and preceded immediately the presentation in the first sitting on the following days. The object of the repeated recalls in the first series was the same as in the repeated recalls for the verbal discourse to be described below. The object of the repeated presentation in the second and third series was to determine what changes would thus be brought about in the manner of learning and of recall. As regards the recall, the effect of the lapse of time and of the repetitions of the presentation was not enough to be conclusive. All the figures and tables given in the present report are taken from results of the immediate recalls of the first sittings only. But the introspections of the observers for the later sittings are also taken into account.

[1]The key was set in circuit with an ordinary stop-watch and electromagnet whose attached lever started and stopped the watch as the key was turned in and out.

suggested in much profusion and detail. The details of dress and appearance of persons, their movements, facial expressions, and their surroundings were sometimes visualized in a manner so complete as to seem to leave out but little that would have been supplied if all had been actually present and seen as well as heard. This visual imagery played a very significant part in the recall of the words and other sounds heard. It frequently appeared simultaneously with or followed the auditory imagery, but it was difficult to determine its function in such cases. We shall consider it mainly only so far as it preceded the auditory imagery.

In all but the immediate recalls of a record the first thing that came to mind was as a rule visual imagery. The general setting was visualized, including the persons and the things suggested, their relative positions, and the arrangement of the general surroundings. Sometimes these would be reviewed in much detail so that a remarkably minute description of the appearance of persons and things could be given from this visual imagery. The clue to the recall of the words was sought in it, while all auditory imagery might remain absent for several minutes. With the recall of the first phrase or sentence this visual imagery dropped out for the most part. A very vague and incomplete visual sketch of the scene might remain in the background of consciousness, with a clearer focus that was immediately connected with the words that were being recalled standing out in detail. But more usually the former was entirely absent after some words were once recalled, and was returned to only when the recall became difficult. Degree of difficulty of recall of the words largely determined the nature and use made of the visual imagery throughout. If very easy, the visual imagery tended to drop out entirely, or, if present in this case, accompanied the auditory imagery simultaneously rather than preceded it. Besides degree of difficulty of recall, the nature of the record determined the presence and manner of use of the visual imagery. Naturally, the verbal discourse suggested visual imagery in different degrees, according to its nature. This difference appeared in the recall. This general manner of behavior of the visual imagery gave to it, when taken by itself, various degrees of continuity, both with reference to its temporal successiou and with reference to its representing a connected scene or event.

Besides being an aid to the recall of the auditory imagery of the words, it filled in the gaps left by words that could not be recalled at all. Where the words were entirely forgotten the visual imagery present often supplied the meaning. The actions of the persons speaking, the facial expressions as visualized indicated what they were saying, especially what replies were given to specific questions for which the words had already been recalled in auditory terms. But this filling in with visual imagery where words could not be recalled occurred even when no meaning was supplied with it. Such was the case when a person was visualized as finishing a sentence, or replying to a question, the observer seeing the lips move and the gestures in this visual picture, without any knowledge of the content of the forgotten words. Usually the observer would know fairly definitely the length of the forgotten remarks, but no more, from this visual imagery. This function of filling in the gaps so as to make a continuous and connected account of what was recalled came particularly to the fore when the recall of the words was very fragmentary so as to become confused and the order of remarks lost. In such cases the visual imagery was used to straighten out this confusion and to determine where each remark and phrase belonged.

2. Inference from the Context. I call the observer's determination of what the words were from what was already recalled an inference from the context. This process was peculiarly prominent. The nature of the introspective reports indicates that the observers were often not aware that this was the procedure, instead of real recall. But special questioning at such points usually revealed the true process. A definite quantitative statement cannot be made from the present data. But they justify attributing about a fourth, on the whole, of what was reported as recalled to this sort of inference. The descriptive records that were used were favorable to inference as a means of recall. Since all but one presented conversations between two or more persons, questions and answers were both readily inferred when either had been definitely recalled. This was often equally true of any preceding remark when the following one was already known. These inferences were made in two ways. (a) They were made from words that had been recalled, from the verbal context. (b) From the visual imagery. The first needs no further description. The second was apparently the more prominent. The remarks made were inferred from the actions, attitudes, expressions of the persons speaking, as visualized. Obviously, when this visual imagery was quite complete and connected, this could be done readily and correctly. That it might play a large rôle will become clearer after a description of the character of the auditory imagery and the changes which both visual and auditory imagery underwent with the lapse of time.

B. *Character of the Auditory Imagery.* The character of the auditory imagery presented three different aspects: (1) It varied with reference to the completeness with which a sentence was recalled directly in auditory terms. (2) It varied with reference to the degree in which the words were imaged in the quality of the individual voice. (3) Imagery of the voice in its true character sometimes appeared without the recall of any words.

1. Degrees of Completeness. It might have been supposed that the sentence, or part of it that independently suggested meaning, would always have been the unit of recall. This was not the case. The different remarks that had been heard were recalled in fragments much oftener than as a whole. The auditory imagery of certain words or phrases would flash out first, often remaining isolated from any further recall or any meaning for some time. With this for a start, the remainder would be filled in until the sentence was completed and recognized as correct in meaning and in words. A single sentence therefore frequently embodied all the different ways of recall, direct recall in terms of auditory imagery, the auditory imagery brought in associatively by preceding concrete visual imagery, or brought in by inference from the verbal or visual context. The amount recalled directly in terms of auditory imagery varied from the entire absence of the latter to the recall of all the words in this direct way.

2. Differences in Quality. The auditory imagery for the words recalled was of several different forms. (a) In some cases the words would ring out clear and intense, minutely in the character of the individual voice in which they had been heard. The imagery then approached the perceptive quality characteristic of all vivid recall. (b) As frequently the individual character of the voice was entirely absent in the imagery. The words would still be imaged distinctly, but in a sort of characterless form, resembling the voice of one no more than of the other person heard. Between these two forms all grades naturally appeared. As was the case with most of the records used, two of the voices in each contrasted quite strongly as bass and tenor. Perhaps the most frequent form of the auditory imagery was that in which the bass or tenor was still recognized, but not the particular bass or tenor concerned. (c) A third class of words is to be added; words that were reported as recalled without first clearly imaging them. The process here was similar to that of ordinary conversation, when sentences are formed without any definite auditory imagery preceding the spoken words.

The manner of recall was characteristic of these forms of auditory imagery. The first was usually recalled directly, sometimes suggested by preceding visual imagery, but never the result of inference from the context. The second was largely recalled directly, about as frequently suggested by concrete visual imagery, and in some cases preceded by inference. The third was solely a product of inference from the context.

From both the form of the imagery and the manner of its recall it might be expected that certainty as to correctness would be greatest for the first and least for the last. In the main this was the case, but with very frequent exceptions belonging to the second class. Words that were distinctly imaged in auditory terms, but without the individual character of the voice in which they had been heard were very often regarded as correct with as great certainty as were those of the first class.

3. Imagery of the Voice without Recall of the Words. An interesting phase of the auditory imagery appeared when in the recall of a sentence the voice was heard to continue beyond the point where the words were recalled. This was not an occasional occurrence, but rather the rule for sentences that were recalled in a fragmentary fashion. The nature of this imagery was by some of the observers compared with hearing voices at a distance, which may be recognized as the voices of certain friends, without being able to understand what is being said. In the case of this auditory imagery the individual character of the particular voice was quite often very clearly recognized. In it was also reproduced the accents and rhythms, so that the number of words that could not be recalled could sometimes be accurately stated. Undoubtedly this imagery was some aid to the recall of the missing words when these were later recalled, but there were numerous instances in which the middle phrase or so of a sentence was auditorily imaged with the beginning and end filled in with this auditory imagery of the voice quality alone as the final result. The latter might be accompanied by visual imagery of the person, moving lips, facial expression, etc., from which something of the meaning might be gleaned.

C. *Changes with the Lapse of Time.* It will be remembered that there were four recalls for each record, and that the time intervals between successive recalls were one, three and six weeks, respectively, the first being immediately after hearing a record. For these intervals the greatest changes occurred between the immediate and the second recall; and contrasting the first with the last recall showed in every case a peculiarly striking transformation in both the manner of recall and in the final result, the auditory imagery of the words. We may consider first the changes in the total process, the factors entering to produce the auditory imagery.

1. Changes in the Visual Imagery. We have to deal with changes in the amount and detail of the visual imagery, in its temporal continuity and associative connectedness, in its func-

tion, and in its clearness and vivacity. This will be done mostly by contrasting the immediate recall with the last two. In the immediate recall the visual imagery was not constantly present. It came in at special points, which were at the beginning, or where the words suggested changes in the scene or action, or where the recall was more difficult. It was therefore incomplete and disconnected as regards the action or event it represented. It also very largely accompanied the auditory imagery simultaneously rather than preceded it as a means to its recall. It might seem to have been largely superfluous at this stage. With this incompleteness went at the same time minute detail of another sort. This was of the persons heard in the record, of their dress, personal appearance and action. A large portion of this had apparently no connection whatsoever with what had been heard, but was merely the result of the tendency to complete a visual picture. It could therefore be of service in recall at any time only indirectly, by bringing up those other details of the visual imagery that had been directly suggested by the words. The first and prominent change with the lapse of time was in the manner of use of the visual imagery. Instead of accompanying the auditory imagery simultaneously it preceded it as a means to its recall. This change was greatest for the beginning of the recall of a record. In the second recall already, after a week's interval, the visual imagery representing the general scene and action was usually the first thing that appeared. Having once started the recall of the words, the visual imagery might recede again, much as in the immediate recall. For the last recalls this change in function was evident throughout the recall of a record. The visual imagery preceded the auditory in most cases. Other changes were connected with this change in function. In the first place, there was more visual imagery of the sort directly connected with the recall of words. It was at first absent in some places where the words might have suggested it, and it appeared at such places later. In the second place, it was temporarily more continuous and associatively more connected, both being largely the result of the new imagery appearing where it was at first absent. Thus, in the extreme cases, the visual imagery taken by itself represented the whole scene and action suggested by the words, developed in the right order and in a continuous manner. It supplied the whole framework for the meaning of the words.

The new visual imagery that appeared during the later recalls was used as a means to recall words. The unessential visual details, details that had not been directly suggested by the words, did not show the same tendency to change. On the whole, it seems to have been somewhat on the decrease.

The observations were to the effect that some of the unessential details had dropped out, while at the same time the descriptions of the ways in which the persons and things suggested were visualized were, when the observer was asked to describe his visual imagery, often as detailed in the last recalls as in the first.

The visual imagery that was used in the later recalls as a means to recall did not show any decrease in general clearness, but was reported as remaining the same; or, in a few cases, as having increased in clearness. Considering the conditions, this, indeed, was to be expected. Since it had not had its origin in visual perception and was repeated at intervals, there was no occasion for its losing in clearness.

2. Changes in the Auditory Imagery. The changes in the auditory imagery with the lapse of time may be inferred largely from the preceding description of its character, and from the changes in the character and function of the visual imagery. We may follow the course already outlined and consider changes in degrees of completeness, and changes in quality. Naturally, the auditory imagery was most complete, the sentences were recalled in less fragmentary fashion, in the immediate recalls. But several additional observations are to be noted. The later recalls were characterized more by whole sentences being left out rather than by certain phrases or words only of a sentence being recalled. Thus the fragmentary character of the sentences that were recalled did not change so much with time. But the meagreness of this direct recall in terms of auditory imagery when compared with the visual imagery present was always very striking after the six weeks interval. A sentence or two was sometimes all that was imaged in auditory terms without being derived from the visual imagery. Further, the fragments consisting of a few words or a phrase only in the immediate recall were often not completed at all, the meaning being lost. But in the later recalls such fragments recalled in auditory terms were more usually parts of sentences that were filled out in other ways, the words only and not the meaning also being forgotten.

The change of the auditory imagery to a form in which the individual peculiarity of the voice heard is absent is one that has been observed in the recall of nonsense syllables. It is the main change in quality to be noted here. The change for connected verbal discourse, however, seems to be more marked, probably because this individual character of the voice is in this case more vividly imaged at the beginning than it is for nonsense syllables. In the second place, the change occurred much more slowly. Even in the third recall the voices were sometimes still imaged in their individual character, so far as the observers could judge. We may also note several stages in this change. This will make the complete statement of the forms of the auditory imagery and the order in which the changes from one to the other occurred as follows: (a) The voice is imaged in its individual quality. (b) The voice is imaged as bass or tenor, but without any particular individual characteristic. (c) The voice is imaged in a somewhat characterless fashion. (d) No complete imagery of the voice at all, the sentences being formulated as is usually the case in ordinary conversation.[1]

III. Analysis for Groups of Sounds

Any one who has had his attention called to the matter will have observed the very striking difference in the amount and nature of associated imagery when he is looking at things when they are silent and when he is listening to the sounds they make when he cannot see them. The visual impressions of things have of themselves but a very slight tendency to arouse the memory of their sounds. But when we hear sounds we almost invariably visualize the things that produce them. In the previous study on the memory of pictures of familiar objects there was never the slightest tendency to auditory imagery while memorizing a group of pictures, and auditory imagery entered only in a very few individual instances in the recall of the pictures after long time intervals. How visual imagery enters when the sounds heard are human voices has been seen in detail above. In the second part of this study visual imagery was never entirely absent in listening to the sounds for the first few times, nor in the recalls immediately after hearing them. But this visual imagery does not constitute the only possible associated process that may be relatively absent or abundantly present in auditory perception. We have to deal always at least with vocalization and with other motor processes. This, if present at all in the memorizing and recall of verbal discourse, played such an insignificant part as to escape the observer's notice. But in the case of the groups of sounds both the process of memorizing and of recall was more complex. A brief description of the factors present in memorizing the sounds will throw some light on the analysis of their recall.

A. *Methods of Memorizing the Sounds.* The nature of the

[1]For the third form one observer noted that the words were imaged somewhat in the character of his own voice. This suggests that vocalization was probably also a factor in the recall in this case. But whatever part vocalization may have played, it was of such an incipient character as to escape observation.

series of experiments taken in this second part will be remembered from the description already given (pp. 195 ff. above). The methods of memorizing a group of sounds were different in the first series from what they were in the second and third. In the first series the small number of repetitions permitted little more than learning the group as a whole. Only a minor share of the attention could be directed to memorizing the details of individual sounds, if none were to be forgotten altogether. In this matter the observers all followed the plan used by those memorizing visual material. Details were attended to only after the recall as a whole was insured. Associated processes entered at once. For all the sounds that were interpreted at all the visual image of the thing was at once aroused on hearing the sound. While the sound lasted the attention was then divided between the auditory perception and the visual image. In the first repetition of a group immediately after the first hearing an incipient naming of the things, or an incipient imitation of the sound might accompany the hearing. By the third or fourth hearing imitation always entered. The ten to fifteen seconds intervals between successive sounds were used for the recall of what had just been heard. In this recall attention usually went to the names in an attempt to commit to memory the series of names for the group. With the recall of the name, the visual image usually came in of its own accord, while the auditory image entered less readily.

In the second and third series the larger number of repetitions of the presentations made attention to memorizing details possible. The general procedure of the observer changed. The first change was the frequent absence of the visual image while the sound was being heard. In the second place, imitation of the sound, while listening, increased. In the recall during the intervals between successive sounds the visual image was very readily eliminated, and the name disappeared with equal ease. The prominence of auditory imagery increased correspondingly.[1]

B. *The Factors in Recall.* 1. Associative Connections Between Sounds. Associative connections between two or more sounds were in the present case almost entirely absent, though such connections were very prominent in the memory for pictures of familiar objects. Only one form appeared. This consisted of a classification, two or three sounds being put into a class and fixed by a verbal class description. Such associative connections, however, as a means of recalling any of the sounds as

[1]These changes in the manner of memorizing became quite marked in the later sittings of the second and third series where the sounds were heard repeatedly on successive days.

wholes played a very insignificant part. Their function need not be considered further. But before comparisons are made two special reasons for their absence here must be noted. The first lies in the small number of sounds in a group. Such associative connecting was not so imperatively required in order to insure the recall of all. The second lies in the fact that the sounds were presented successively, while the pictures were presented simultaneously. Associative connections between the individual members of a group of stimuli doubtless always suggest themselves more readily when the stimuli are presented simultaneously.

2. The Order of Appearance of the Different Processes. The processes with which we are concerned here are: (a) the auditory imagery, the recall of which is the purpose of the whole; (b) the visual imagery of the things that the sounds suggest; (c) the verbal imagery of the names; (d) the motor processes involved in vocally imitating the sounds and in following out the rhythms with hands, head, and other parts. The function of these factors in the total recall process may be measured in part by the order in which they appear in that total process. This order will give an idea of their relative spontaneity, and hence of the relative ease with which they can serve the function of suggesting auditory imagery. In this only the results of the general, semi-passive recall will be taken into account. When the effort is made to recall the sounds as completely and vividly as possible the order and function of the several processes is quite different. This will be seen later. The following table gives the percentages of the number of times each process held each of the four places, or was absent entirely.

I[1]

	1st	2d	3d	4th	Absent
Visual	53	25	15	1	10
Auditory	35	39	29	6	3
Verbal	15	15	0	55	18
Motor	8	22	18	10	44

In taking these figures as an indication or the relative spontaneity of the several processes we must take into account the necessity of using the names of the things for the purpose of describing the process of recall. This brought in the verbal process for the fourth place when otherwise it would probably

[1]In those cases in which two processes were reported as haviug appeared simultaneously each was counted once for that place. This makes the percentages sum to a little more than a hundred for each process, except for the verbal the observation on which was sometimes overlooked.

have been absent in these 55% of the cases. The 18% in which it was absent include the cases in which the observer did not interpret the sound and therefore had no name for it. Aside from this, we see that half the time visual imagery is the first thing in recall, that the auditory imagery is distributed fairly equally over first, second, and third place, that the verbal process comes in either early or last, and that the motor process is most frequent in the middle of the total recall process. It will be remembered that in the recall during the memorizing of a group of sounds more effort was made to recall the names than for any of the other processes. Its relative lack in prominence here is significant as showing how readily a process can serve the function of fixing associated imagery so as to insure its recall and then itself drop out of the recall later.

This comparison of the relative spontaneity of the several processes gives only a rough idea of the relative part each played directly in leading up to the auditory imagery, since each might follow as well as precede the latter. This function of each is seen better in a direct comparison of the relative frequency with which each preceded and followed. The next table gives this comparison in percentages.

II

	Preceded	Followed
Visual	55	35
Verbal	24	58
Motor	13	43

It is understood, of course, that these several processes, visual, verbal, and motor might accompany the auditory imagery, simultaneously, or might remain absent altogether in this general, semi-passive recall, and also that any two or all three might in the individual case precede the auditory imagery.

Since in this part of the experiment the object was merely to determine the relative spontaneity of the different factors in the total recall process, no effort was made to get observations on the nature and degree of completeness of the auditory imagery. But it was evident that as a rule only a very small part of the total auditory imagery for a sound was aroused in this way, while the 43% of the cases in which motor processes of imitation followed auditory imagery is evidence that the process of recall did not always stop where it was intended it should. For, as we shall see later, the use of motor processes was the predominant means of recalling details. We may proceed now to a description of the character of these several factors in the recall, and the manner in which they were used to produce auditory imagery. We shall here consider both the semi-passive recall and the recall of the details of the sounds. In this recall of details the relative usefulness of the several factors is entirely changed.

3. Character of the Visual Imagery and Nature of its Function. From the standpoint of its function, the visual imagery that entered the recall of the sounds follows three types: (a) The visual image of the thing that produced the sound, which is used for the purpose merely of getting the auditory imagery as a whole or for starting the auditory process. (b) The visual image of the thing going through the motions it would make in producing the sound in question. In addition to being used in the same way as the first, it sometimes served the purpose of recalling the details of the sounds. (c) Visual sound analogues, used mainly for the recall of details of the sounds. The first class and its manner of use was by far the most frequent, so that in the semi-passive recall visual imagery was very prominent as an aid to recall, while in the recall of details it was quite infrequently an aid.

(a) The visual image of the thing tended to take on special characteristics that were suggested by special characteristics of the sound. In individual instances this was carried out to quite minute details. But frequently a visual image with such special characteristics to fit the sound in detail could not be found, or at least did not suggest itself spontaneously. In these latter instances what did suggest itself was not ruled out because of its shortcomings. It appeared as a means of recalling the auditory imagery as a whole although the discrepancy between the thing visualized and the sound with which it was associated forced itself vividly upon consciousness. A tendency exactly opposite to that of visualizing in detail was the appearance of only the essential parts of the thing in the visual imagery, the parts that immediately produced the sound or from which it was emitted. Possibly another form of this tendency was given in some cases in which it consisted merely of some sort of visual consciousness of the direction and distance from which the sound was regarded as coming. With the first suggestion of a part of the auditory imagery the visual imagery of this class at once dropped out. It sometimes appeared a number of times again before the auditory process was completed, but in such cases it came in entirely of itself and was of no service. If the observer tried voluntarily to recall it again and hold it, it interfered with the auditory imagery. Usually the latter dropped out at once when the viual imagery was thus attended to.

(b) For the second and third classes of visual imagery the behavior of the auditory imagery when the former was thus attended to was sometimes quite different. In some cases in which the object was visualized as going through the motions required in making the sound such visualization was an aid to the recall of the details of the sound. These cases were, however, not frequent even when considering only those instances in which they were quite possible. The observer also sometimes stated that its presence seemed to be of no aid to the recall; it merely accompanied the auditory imagery. The possibility of its being used in the recall of the details followed from the fact that in these cases the visual and auditory processes constituted two closely parallel series of changes. A change in position of the moving part was closely associated with a change in the character of the sound. That the visualization of these motions did not always play a part in the recall of details is very likely accounted for by its influence being outweighed by the distraction introduced in turning the attention to disparate imagery.

(c) The visual sound analogues came in less frequently to start the auditory imagery or to recall the sound as a whole, but when present were always used to recall details. Only three of the six observers reported them, but with two of these they were very common, some form being made use of in most cases. These analogues consisted, in the first place, of the visualization of some arbitrary form, marked off somewhat vaguely in the general field of vision by differences in brightness, in a few individual cases colors being introduced. The majority of these forms were cylindrical or eliptical in shape. Others consisted of coils, wavy lines, irregular patches. In addition to their general shape they possessed within further details of form in varying degrees. Likewise, while a difference in brightness marked them off in the general visual field, further differences in brightness and color marked off different parts or areas within them. Finally, either the form as a whole alone, or parts within, were visualized as in motion, or, what corresponded to such motion, the form was visualized as developing from one end to the other. Every detail in form, brightness, color, and motion, was patterned as an analogue to some characteristic in the sound. The two were associated so closely as often to seem to the observer like one process. The details of the auditory imagery were superimposed, so to speak, on the details of the visual, the two developing absolutely simultaneously. The auditory imagery, as one observer put it, was *visual*-auditory. In this way duration, rhythm, intensity, and pitch, were put into visual terms. The auditory part was never present without the visual analogue, but the latter often appeared without the auditory associate. In the latter case much could still be said about the nature of the sound, and its duration could be indicated quite as accurately as when the auditory imagery was present also. These visual analogues usually appeared without any effort on the part of the observer to recall them. But when attention was directed to the recall of their details it never hindered, but always improved, the auditory imagery.

4. Use of the Verbal Process. Only the names of the things that produced the sounds, and never any verbal description of the details of the sounds themselves were employed. The former could, from the nature of the case, be useful only in recalling the sound as a whole. From the figures in the tables already given we saw that the recall of the name preceded the auditory image in 24% of the cases, and that in 55% of the cases it held last place in the total process. In this 24% of the cases the orders verbal-visual-auditory, visual-verbal-auditory, and verbal-auditory-visual occurred with about equal frequency. But the meagre data do not allow much generalization. For one observer the order was very strongly visual-verbal-auditory. For another the order verbal-auditory-visual was equally prominent. The nature of the verbal process with reference to the kind of imagery involved, gives some additional insight into the part it probably played in producing auditory imagery. When the verbal held fourth place it was nearly always a purely motor process. When the observer had completed the recall he simply found himself incipiently or actually pronouncing the name. There was no visual or auditory image of the name. On the other hand, when the verbal process preceded the auditory, and especially when it preceded also the visual, it was much more likely to include an auditory image of the name. At this point in the recall the observer was not concerned with description and the verbal process would not enter from this cause. There are left, however, some cases in which it followed reflexly the visual image that preceded the auditory.

5. Character of the Motor Processes and Nature of their Function. The motor processes were by far the most important of the several factors in the recall of the details of the sounds. As regards the muscles that came into use, they were of great variety. But we may divide them into two classes on the basis of their function and briefly describe, first, their general character, and then consider the manner of their use. These classes are: (a) motor processes involved in the vocal imitation of pitch and quality, and (b) various motor processes involved in the imitation of rhythm and duration. In the former the same set of muscles were necessarily always concerned, while imitation of rhythm occurred in the variety of ways familiar to every one.

(a) Naturally, the pitch and quality of the sounds could not usually be accurately imitated. Very rough approximations only were the rule. Certain details of the sounds could be imitated. The rest would be 'slurred over.' This consisted mostly of a stationary tension in the vocal muscles with accompanying regulation of breathing. Sometimes another sound was substituted that was more easily imitated and which resembled the real sound. These substitutions were at times recognized as something familiar, and sometimes as constructions for the occasion. Imitating the sounds aloud was not permitted, but the vocalizing was nearly always much more than incipient. This was shown both by the direct observations and by laryngograph records that were taken on a few occasions. The care taken to imitate the sounds was apparently not greatly affected by the ease or difficulty met. Some vocalization of an imitative character entered in the recall when no sort of approximation even would seem to have been possible.

(b) Imitation of rhythm and duration was on the whole easier than imitation of pitch and quality. It was reported by the observer as considerably more prominent. For the irregular and difficult rhythms slurring over and substitution occurred in quite the same way as for pitch and quality. In like manner the very rough approximation to what was recognized as correct was followed out with as great care as the quite easy rhythms. One general and significant difference, however, is to be noted. In the imitation of rhythm there was a greater tendency to let details drop out, not because they were difficult, but because they were not needed for the purpose for which imitation of rhythm was used most. In this way imitation of the rhythm often became but little more than an indication of the duration of the sound. This consisted usually of a stationary tension in the muscles or of a single long-drawn-out movement equal in duration to the recognized duration of the whole sound. The regulation of the breathing in this way was especially prominent among these cases.

In describing the manner of use of both these classes of motor processes attention must be recalled to the distinction made between recalling the sound as a whole and recalling it in detail as vividly as possible. From the figures given on the order of the processes in the former kind of recall, it was seen that the motor processes preceded the auditory imagery in only 13% of the cases, while visual imagery preceded it in 55%. In the recall of the details of the sound this relation is entirely reversed. We have already seen how the visual imagery usually dropped out in the recall of details. But for *the motor processes in the recall of details* we may state at once as a general fact that they *were a constant and necessary means of producing auditory imagery of a sound in full.* The more they could be introduced, the more detailed and vivid, the more satisfactory in general would be the auditory imagery. The process was decidedly a *motor*-auditory complex, in which the two factors were even more inseparably united than the visual sound analogue and the auditory imagery associated with it described before. According to the various observations at different times, the auditory imagery would fail to appear if the motor processes were voluntarily inhibited, or if attention was directed to getting the auditory imagery directly; the latter was dependent on the motor processes. To one observer the two seemed like one unitary process, his generalization on one occasion being that: "The vocalization almost *is* the sound. I cannot separate the two at all." Even for the observer who succeeded best in getting independent auditory imagery the latter could not be carried through vividly to its completion, but remained fragmentary and hazy without resorting to vocal imitation. As regards the difference in the function of the two classes of motor processes described, vocalization of pitch and quality and motor imitation of rhythm and duration, it is to be noted further that the two were not independent. While imitation of rhythm was perhaps no great aid to the recall of pitch and quality, the muscles involved in vocalization were often involved in marking off the rhythms and in judging the durations. In the recall of the duration of a sound the muscles used in following out the rhythms were the predominant factor. We may turn now to a special consideration of the recall of duration.

It will be remembered that each observer indicated the duration of a sound as he recalled it by turning a noiseless switch key, and that he also gave his observations on the mental processes involved in making this measurement. The latter will be taken up first. It was found at once that this measurement from memory was not at all a matter of measuring the duration of the auditory imagery. Five of the six observers could not even approach such a degree of control of the total auditory imagery as to make its total duration anything like what they regarded as the duration of the sound. They made the measurements mainly from the motor processes. The auditory imagery was, indeed, entirely absent about as frequently as any was present. In the majority of cases when it was present it was regarded either as of no use at all or as being only of slight aid in judging duration. The details with which the rhythm was followed out varied with the observers and with other conditions. The tendency to slur them over was often noted, so that the duration was judged largely from merely the stationary tension of the muscles, or long drawn out movement, as already noted.

From this analysis of the mental processes involved in measuring the duration from memory, it becomes more proper to speak of the accuracy of the associated motor processes in making this measurement than to speak of the accuracy of the auditory imagery. The next table gives a fair idea of how serviceable these motor processes were for this purpose. It gives the averages for the six observers of the measurements in seconds, of the actual sounds while they listened to them, and for their percentage of error in measuring them from memory.

III

Order of sounds in a group,	1	2	3	4	5	6	7
Duration as heard, Group I,	6.0	7.8	4.1	2.0	2.1	2.2	21.1
Percentage of error in recall,	21	41	33	33	77	17	63
Duration as heard, Group II,	3.4	4.6	1.3	5.4	1.8	2.0	2.2
Percentage of error in recall,	50	50	24	41	35	15	8

The general average error from these figures is 36. That is, these observers in measuring the duration of these sounds from memory during the hour in which they had heard them made an average error of 36%. They were underestimated more frequently than overestimated. Disregarding many exceptions we may state it as a rule that the short sounds were usually overestimated, while the long were always underestimated. The slowness of the motor processes in imitating seemed to be responsible for the overestimation of sounds of less than two seconds duration. Substitutions of other sounds and their imitation for the original doubtless caused overestimation in other cases. The tendency to gravitate towards an average length, a law found to hold true for the intensities of lights as recalled, was probably also present. But no adequate analysis of the causes of these errors in the measurement of duration of the sounds can be made from the present data. The variation in the amount of error from observer to observer, and from sound to sound was very great indicating a high degree of complexity of factors. The main result that is of significance for our present purpose is the fact that the auditory imagery was found to be so much less efficient than the motor processes in estimating the duration from memory, and that with the latter such large errors were still made.

C. *Character of the Auditory Imagery.* The foregoing analysis of the recall process, of the aids to reinstating the auditory imagery, the character and manner of their use, brings us to the description of the auditory imagery itself. Here we meet again the difficulty noted at the outset: there is no adequate means of describing auditory imagery. A few general characteristics of the auditory imagery were determined from the direct observations, and some further inferences from the observations are evident. Our description will be limited to these.

1. Incompleteness. The characteristic most striking to all observers was incompleteness. What was present in auditory terms seemed to represent only a small part of the sound. If we combine this direct observation with measurements from memory of the sounds we see that the auditory imagery must often have been exceedingly fragmentary. These measurements of the duration were sometimes over 50% too short, but the auditory imagery took up only a small part of this measured duration. Indeed, direct observation in many special cases shows well enough this fragmentary character. We have, first of all, those instances in which the visual analogues or the motor processes were gone through in detail with no auditory imagery present whatsover. In some of these cases the observer was still able to measure the sound from memory with some fair degree of satisfaction to himself. Statements that only a trace of the auditory imagery, a bare suggestion of it, a mere 'flash', etc., could be obtained were very common. Its presence in such a degree of completeness as to seem to represent approximately all the details of a sound was very exceptional. The rule was that only a fraction of the sound, a fourth to a half, seemed to be imaged in auditory terms.

2. Its Course and Voluntary Control. Second in importance were certain characteristics concerning the course and voluntary control of the auditory imagery. A comparison of visual imagery of forms in these respects makes them the more striking. Its course was decidedly irregular. Certain parts only would come out, a note here and there, or larger part marked off more or less from the rest by its general character. These parts might be recognized as quite correct in quality, and were nearly always definitely placed in the total sound as being near the beginning, or end, or middle. The rest of the sound as remembered would then be represented by other than auditory processes. The means of its control has already been indicated. It was not direct, but through the aids to recall. In the effort to reinstate the auditory imagery in detail attention was not directed to that imagery itself directly, but to the motor processes of imitation, and to certain kinds of visual imagery in some cases. With attention thus directed the auditory imagery seemed to come in of itself if it appeared at all. When started or once obtained, it could not be held or developed by turning attention to it directly. It disappeared at once in spite of concentration, and the aids had again to be resorted to to make it return. As one observer put it, "Without the vocalization the auditory imagery does not run smoothly, the attention does not hold to it; it slips off and the

auditory image is gone." This means of control was employed for the different parts of the sound independently. Auditory details were repeated and modified to get them more correct by repeating and modifying more or less step by step the motor and visual processes for these details. Immediate repetition of the total recall decreased for the time being the ability to reinstate the auditory imagery. With such repetition it became vaguer, more incomplete, and required more attention to the aids to reinstate it. Apparently this is a fatigue effect not at all equally shared by visual imagery. Some auditory imagery sometimes came up more or less independently of motor or visual aids. This might be correct so far as it went, but sometimes it was recognized as wrong. In the latter case the observer often found that he could not control it so as to make it come up correctly. It consisted of short spotaneous 'flashes', belonging equally to easy and difficult sounds. These came in unexpectedly, when attention was not particularly directed to the recall of any detail, or just at the moment when the observer was relaxing his effort to recall. They disappeared at once and could never be reinstated by attempting to do so. The prominence of this kind of auditory imagery varied much with the different observers. For one a series of such flashes for a sound became the guide for directing and redirecting his motor processes by means of which he produced the auditory imagery in detail. Another observer, on the other hand, never reported such imagery.

3. Its Vividness and Quality. No direct measurement of the general vividness being possible, the observer was asked to compare the auditory imagery in this respect with his visual imagery of the things that entered. This comparison easily demonstrated a very great difference between the two classes of imagery. The observer whose auditory imagery was most prominent sometimes stated that it was as clear and vivid as his visual imagery of the associated things. But even for him such vividness was quite exceptional. For the other five the auditory imagery seldom even approximated the visual in vividness. This, indeed, becomes evident enough when we consider the general results given above, and compare them with our general observation on visual imagery.

As regards its general quality, the auditory imagery that was reinstated was of four different classes. These will need no special description. They were as follows. (a) Auditory imagery which included the characteristics peculiar to the given sound, and made it individual, instead of one of a class. For instance, the crow of the bantam rooster would be imaged in the distinctive way as heard, not as representative of the crow of any bantam rooster. (b) Auditory imagery representing largely only the class of sounds to which the one heard belonged. In these cases the observer was uncertain often as to whether the distinctive characteristics were imaged or not. Many of the substitutions for the original as heard belonged to this class. (c) Sounds considerably different from those heard were substituted, the observer being aware that his imagery represented a substitution, but accepting it as the nearest approximation he could make. (d) Some sort of auditory process which represented merely the general quality of the sound but included no detail or variations in pitch or intensity. This would distinguish the bugle from the whistle or from another musical instrument, for example. The usual statement of the observer in this case was that there was present a certain auditory quality that identified the kind of sound, but this mere quality did not materialize into a definite auditory image. With the exception of the third, as is seen, these are the same classes described for the auditory imagery in the recall of verbal discourse.

IV. Summary of Results

In the recall of verbal discourse the words were recalled (1) directly in terms of auditory imagery; (2) associatively through visual imagery of the persons and things that the heard words had suggested; (3) through inference from the visual or verbal context as already recalled. The second method of recall was by far the most prominent, and came especially into use at the beginning of the recall of a record, at turning points in the discourse, and at other points where for any reason the recall of the words was difficult.

The character of the auditory imagery varied with reference to (1) its completeness, (2) the degree in which the words were recalled in the quality of the individual voice; (3) imagery of the voice in its true character without the recall of any words sometimes occurred. As a rule only fragments of sentences and phrases were recalled directly in terms of auditory imagery, the rest being filled in by the other methods of recall. Sometimes the words were recalled in the quality of the individual's voice as heard. Usually this individual character was not represented in the auditory imagery while the latter was nevertheless distinct and clear. Sometimes the words were recalled without first definitely imaging them in a manner analogous to the process in ordinary conversation.

Changes with the lapse of time in both the manner of recall and in the nature of the factors entering were greatest from the immediate to the second recall, after a week's interval. Visual imagery, that at first accompanied simultaneously the auditory imagery of the words or followed it, later preceded it and became the means of recall. It increased in amount and continuity, so that the visual imagery alone presented the whole scene and event. The general clearness and vivacity of the visual imagery remained about constant throughout the several recalls.

The total amount recalled in auditory terms decreased markedly, leaving sometimes only a sentence or two that was thus recalled after the six weeks' interval. But the fragmentary character of the sentence recalled at all did not increase much. The quality of the auditory imagery showed progressively the following stages of changes. (1) The voice was imaged in its individual quality. (2) It was imaged as bass or tenor merely. (3) It was imaged in a somewhat characterless fashion. (4) No definite complete auditory imagery at all appeared before the words were formulated and stated in the recall.

The processes entering in memorizing a group of sounds changed with the repeated presentations. During the first presentation attention was well divided between the actual sounds and the visual imagery they aroused. The first repetition or two next brought in the processes of naming the sounds and of vocally imitating them. With still further repetitions the visual imagery and the naming readily dropped out, the motor processes of imitation increased for a while, but tended also to drop out finally.

In recalling the sounds in a semi-passive way without any effort to recall them in detail or vividly, visual imagery appeared first in 53%, auditory imagery in 35%, the name in 15%, and motor processes in 8% of the cases. The auditory imagery was preceded by the visual in 55%, by the name in 24%, and by the motor processes in 13% of the cases. This shows the relative spontaneity of the different factors in the total process of recall, but not at all their relative value as aids to the recall of the sounds in detail.

According to the manner of its use, the visual imagery was of three kinds. (1) The visual imagery of the things that produced the sounds, used solely to recall the sounds in a general way or to start the auditory process. Attention to it for the recall of details was detrimental to the recall. (2) Visual imagery of the things going through the motions they would make in producing the sounds. This was used like the first and also sometimes to recall details. (3) Visual sound analogues, consisting of arbitrary forms, sometimes including colors, whose characteristics were patterned after characteristics of the sounds. These were used mainly for the recall of details. The first class was by far the most frequent.

Only the names of the things and never any verbal description of the sounds entered the recall. From the nature of the case, therefore, the verbal processes present were never an aid to the recall of details.

The motor processes used in imitating the sounds were by far the most important factor in the recall of details. They were *inseparably connected with the effort to recall a sound vividly and minutely*, and constituted a necessary means to the same. They were the basis for measuring the duration of the sounds from memory, in which process all auditory imagery was entirely absent about as frequently as any was present, and when present was in the judgment of the observers no aid to making this measurement. The average error for all observers in these measurements ranged from 15% to 77% for the different sounds used. In general, the short sounds were overestimated, the long ones underestimated.

The auditory imagery itself was very fragmentary. According to the direct observations only a fourth to a half of the total sound could as a rule be recalled in auditory terms. Combining this with the observer's measurements of the sounds from memory, which were in individual instances often 50% too short, the indication is that the auditory imagery must have been often extraordinarily incomplete. In many individual cases direct observation verified this conclusion. The course of the auditory imagery was decidedly irregular, both with reference to its temporal continuity and with reference to the parts of the sounds it represented. It could not as a rule be voluntarily controlled directly, but only through the motor processes, or through visual imagery in some instances. Through these aids the sound was recalled fragment by fragment, leaving usually large gaps to be filled in by motor representatives alone.

MIRACLES OF HEALING[1]

By Charles W. Waddle
Sometime Fellow in Psychology, Clark University

In approaching the psychological study of miracles of healing one can scarcely be expected to define the term miracle in any narrow sense, for one must study every form of the so-called miraculous from the most elemental form of naturism to the highest type of scientific or pseudo-scientific psychotherapy. The term may cover now one and now another class of phenomena as we deal with a low or a high type of belief or practice. For the purpose of this study it is even, to a large degree, immaterial whether the miraculous occurrences, or supposed occurrences, passed in review be accepted as facts or viewed as mere superstitions. We may thus, perhaps, largely escape entanglement in the discussion of a phase of the subject which has elicited a wider divergence of view, been productive of more superstition and the basis of more heresies than almost any other discussion relating to human thought. Whether based upon accepted facts or not the miracle psychosis is the result of the mind's reaction to a phenomenal world which constantly baffles satisfactory explanation. The everywhere prevalent belief in miracles of healing as facts presents problems of the origin, nature, causes, and significance of the beliefs as such. It is in short the miracle psychosis that we are most interested in. This we still have with us if we reject all the physical phenomena connected with it as untrue or as falsely interpreted.

It has become a canon among modern anthropologists that while the reactions of the human mind under the same set of circumstances may be strikingly different among different races, mental organization is in all fundamental points the same for all humanity. All are capable of abstraction, inhibition, and choice though of very different types. Two authorities on this

[1]The writer wishes to express his obligation to President G. Stanley Hall for suggesting the topic of this study and for constant help while it was in preparation; to Dr. A. F. Chamberlain for frequent assistance and advice as to the literature of the anthropological phases of the subject; to Dr. E. C. Sanford, Dr. W. H. Burnham, Dr. Theodate L. Smith, Dr. Louis N. Wilson, and to many of his fellow students in Clark University for their friendly interest and other valuable assistance.

point must suffice: Brinton who says (15,[1] p. 6) that "the laws of human thought are frightfully rigid, are indeed automatic and inflexible" and that all minds deal "with nearly the same objective facts in nearly the same subjective fashion, the differences being due to local and temporal causes." (p. 9.) Again, Boas who in his discussion of the mind of primitive man says (10 b, pp. 1-11) "it would seem that, in different races, the organization of the mind is on the whole alike, and that the varieties of mind found in different races do not exceed, perhaps do not even reach, the amount of normal individual variation in each race." It is our purpose, in the first place, to show by a genetic study of the healing art that the ideas of the human race concerning disease and its cure establish this canon. In the second place, we hope, that as the study proceeds, we may discover and make somewhat more evident the laws of mind which condition its reaction to the phenomena which we study. In the third place, we shall hope to be able to show that the phenomena themselves while strikingly different when casually viewed from the outside, the real causes and effects being obscured beneath a mass of form, ceremony and superstitious or unscientific beliefs, are in reality dependent upon the action of certain now commonly recognized laws of mind, quite limited in number and of universal application. Further, we shall attempt, if these three points are well taken, to point a way to a more scientific view of the subject and to suggest some ways by which our knowledge may be made useful in the overthrow of superstition and to the positive benefiting of mankind both physically and mentally.

To the ends suggested our method is to be historical in so far as we study the development of beliefs regarding the healing art; comparative, that we may gather the common elements of the various beliefs; and psychological in attempting to discover some laws of mental life by means of which the phenomena observed may become more intelligible.

In order to determine with some degree of accuracy at what stage of his mental evolution man began to have thoughts about disease and its cure it seems necessary to sketch in brief outline the steps by which he developed, through reaction to a world full of movements, sights, and sounds, a mentality of a higher order than that of his animal ancestry.

The simplest normal human mind now to be found anywhere has its philosophy of life that, more or less crudely, attempts a solution of the great problems of fact and existence. For example, in all the history of modern anthropology no tribe of

[1]The numbers in parenthesis refer to the bibliography at the end of the paper.

people however low has been discovered which has not had some sort of religion. Many attempts have been made to reconstruct the mind of our earliest progenitors and make out the stages through which have been evolved thoughts of life, magic, medicine, religion, and philosophy. Too many of these attempts have been philosophical speculations based on few and often doubtful facts. It is only since anthropologists have advanced the view, noted above, that the mental constitution of man is essentially the same in every age and under every condition, that the importance of a careful, scientific study of existing customs, mythology, language, and archeology, as an aid to the retracing of the steps of psychic evolution, has been recognized. Since the adoption of this view there have been a few scientific men and women who have put themselves in a position to form an intimate acquaintance with the facts in these fields. It is in the main upon such studies and upon the further light to be gained from the modern "child study" movement that our conceptions of the naïve thought of the race should be based. The outline which follows is based upon such authority.[1]

It has been determined to the satisfaction of every competent authority that no artificial boundaries can be set up between the mind of man and that of the animal. There are stages of human thought so elemental as not to be capable of distinction from that of the animal. So nearly as we can determine, the consciousness of the animal is a nebulous, undifferentiated jumble of subject and object, of sensation and the thing sensed. According to Bogoras (10a), Clodd (20a), and many other authorities, such a state is to be found among low primitive tribes to-day. Environment and social intercourse have developed from this elemental state all the complex states of our now highly specialized consciousness. It is difficult to make out definite steps in such development, because the simplest conceptions remain and have the higher superimposed upon them. It is even possible that they develop side by side. In spite of this difficulty there is practical agreement upon some such development as we shall now trace.

The most elemental stage may be termed the *subjective* in which the objects and activities of the universe were not differentiated. The perception of motion and of the resistance to motion repeated again and again in the experience of the in-

---

[1]For the sake of brevity and because this discussion is not sufficiently germane to the general topic in hand we shall not quote authorities on the subject, although many have been consulted, but merely refer those who wish to investigate the grounds for our statements to a few of the best references in our bibliography viz.; 10a, 15, 18, 19, 20a, 37, 38a, 52, 53, 73, 79.

dividual led to a vague idea of *life*, in its earliest form little distinguished from motion itself. In this stage there was no conception of spirit. The object was self-active. The perception of motion suggested the invisible which aroused the emotion of fear which to Hobbes and to many since his day was the "natural seed of religion." The terrors of the storm, the resistance which his will met in all the objects and beings about him, the injuries which he received when he fell upon some object or it upon him, led man to a view which differs slightly and is, possibly, an advance upon the one just mentioned. This new view is the conception, still non-spiritual, of power or powers, everywhere evidently inferred from experiences with the self-activity of objects manifested in movement and life. Gradually the life of objects as manifested in power came to be considered as a thing in itself that could be conceived of as a transfigured form distinctly different from the ordinary, material form of the object. This transfigured form was not strictly a spirit but differed from the ordinary form as the body of man differed from the object. It was in short anthropomorphic, it acted as man acted, it embodied the living self-active power of the object. This conception had been made easier by mental note having been made of gross resemblances between the parts of the object and those of the human body. Next, probably through the phenomena of dreams, ecstasy, hypnotism, intoxication, insanity, death and the like taken with the observation of the shadow of his person and the echo of his voice which were sometimes perceptible and sometimes absent, man was led to conceive of the possibility of a separation of these two forms. The one becomes now purely material; the other spiritual. The self-active element of objects is now for the first time spiritualized. No permanent separation of the two forms, however, is yet thought possible. The spirit may leave its material envelope at will, temporaily, just as it was thought that the soul of man, now for the first time conceived of, could leave the body during sleep but must always return with awaking consciousness. The final step in this line of development is taken when a permanent separation of the two forms of the object is thought possible. The spirits resident in man's body and in every object or power about him are now free either to live in their material object or person or to separate themselves forever from their material form and continue an independent existence. All of these forms of thought are still prevalent among primitive peoples and some of them persist under the influences of the highest type of civilization.

Theories of the nature, form, and later the destiny of spirit naturally followed the conception of it. Religious thought still has much to do with such questions. As one or another theory has held the field the thought of the race has developed into fetishism, totemism, anthropomorphism, metempsychosis, polytheism, or monotheism. Some have believed in the future life; others in nirvana as the ultimate end of spirit. Some have combined various ones of these views in strange proportions. Scarcely any large group of people is wholly free from a number of the crudest of the views here outlined. It is beyond our present purpose to trace the higher lines of thought in detail.

It is impossible to say at just which stage of his thinking man began to attempt an explanation of sickness, disease, and death and to devise means for their treatment. It seems certain, however, that it came with the spiritualization of the objects and powers of the world and the conception of these spirits as capable of action for good or evil, an inference easily drawn from experience. We shall now turn to a closer study of the thought of man along this line, illustrating as fully as space will permit.

Theories of Disease

All who have studied the medical lore of primitive and ancient civilizations agree with Hewitt (18, 21st, p. 134) and Fewkes (*ibid.* p. 15), to quote only the latter, that "primitive man regards everything as possessed of magic power allied with what we call life, capable of action for good or evil." This magic power as we have seen has been, in the animistic stage, spiritualized so that we are prepared to find with Bartels that in answer to the question, "What is sickness?" one gets, from the primitive man the ready answer that sickness is the influence of evil or ill-disposed spirits. The words in which this answer would be given might to the uninitiated confer quite a different impression, but would on careful study be found to mean nothing else. To illustrate the various ways in which this same fundamental idea would be expressed by different tribes, and even by different members of the same tribe, we can do no better than condense the exhaustive and extremely suggestive outline of Bartels. (2, pp. 49-72.) He suggests four common ways in which the evil spirit itself is the direct cause of sickness: 1. Sickness *is* a demon or it may be several demons; 2. The angry spirit of some deceased person; 3. The spirit of some animal; 4. A vampire or werewolf. In thus causing sickness the evil spirit may enter the body of its own accord; by direction of a witch or magician; by permission or at the command of the gods as a curse or punishment for wrong doing; by transfer from another person through the use of contagious magic. Again the ill-disposed spirit may make an external attack upon a person or may assume human form to make such attack; it may steal away one's shadow, breath, life blood, or some vital organ; or by introducing into the victim's body some foreign object it may effect its evil purposes. The last of these views is very common and takes many strange forms. To enumerate only a few: disease is often caused by the presence in the body of something animate; as, a worm or other animal. Frequently the trouble is due to some inanimate body, visible or invisible, which is to be found on or in the body of the patient or the particular part affected. Within these two classes almost every conceivable object has been considered a cause. No purely physical agent, however, is ever sufficient in itself as an explanation to the primitive man. Each ailment is without exception dependent in some way upon the presence (possession) or action of some spirit.

Disease and Death Unnatural

To the mind of primitive man the idea of disease and death from natural causes is almost unknown. According to Decle (64, p. 117) the natives all over Africa have no conception of death as a natural occurrence. Numerous other observers confirm this opinion. The Cherokee myth (18, 7th, p. 319) which accounts for the origin of disease and death tells us that in the olden time the animals and man lived peaceably together and disease and death were not known. As time went on, however, men increased so rapidly that they began to crowd the animals and, not only so, but with the invention of the instruments of the chase, began to kill them for their flesh and skins. In self protection the animals held a conference and retaliated by sending death and all sorts of disease upon man. The same myth also accounts for the healing art which was made possible by the plant world which was friendly to man. To defeat the purposes of the animals each plant offered itself as a remedy for some disease. So the Cherokee, even to the present time, believes that disease and death are unnatural and that there is always some plant, if we could only know it and invoke its aid, whose spirit would come and defeat the evil influences of animal spirits, ghosts, and witches. The Shaker Indians (18, 14th) of Puget Sound believe that disease came as a punishment for lacerating the bosom of mother earth. "In Egypt," says Maspero (57), "man does not die, but some one or something assassinates him." Spencer and Gillen (78, p. 476) find no belief in natural death among the Central Australians, for "however old or decrepit a man or woman may be when this takes place it is at once supposed that it has been brought about by the magic influence of some enemy." Roth's study (72) bears out the same statement for other Australian tribes. An Australian native, according to Lumholtz, "is unable to conceive death as natural, while diseases and plagues are always ascribed to witchcraft and to hostile blacks''. In short, wherever the evil spirit theory is strictly held there is no room for the conception of disease and death as natural.

EVIL SPIRIT THEORY ILLUSTRATED

The universality of the evil spirit theory may, perhaps, be made more evident by some specific illustrations. The Malay belief is that mischiefs (*i. e.* spirits) reside in all objects animate and inanimate which get into man and cause disease. The ancient Egyptians divided the body into thirty-six parts, for each of which there was a particular demon. The Babylonians attributed all diseases to demons and the Assyrians had a special one for each disease. In China, Tartary and Japan the evil spirit theory was once universal. Mooney (60) finds that the folk-medicine of Ireland assigns three causes for disease—fairies, the evil eye, and witchcraft. Roth's extensive study (72) among the Australian tribes reveals the fact that most accidents, ills, and diseases are thought to be caused by a ''dooming'' of the victim either by nature spirits or enemies, living or dead. The demon theory has not been entirely outgrown even in the most civilized countries of the present day although the tendency everywhere is to attribute fewer diseases to it.

The belief in witchcraft is scarcely less universal and, indeed, is only one phase of the evil spirit theory. The Egyptian physician of the olden time, as Maspero (57, p. 213) shows, was credited with all the powers of magic, sorcery, and witchcraft by which he could cause as well as cure disease having by these means the mastery over evil spirits. The sorcerer believed that if he could procure something belonging to an individual he could exercise complete control over him. A few nail parings, hairs, a piece of his clothing, or a drop of his blood embedded in a wax doll made it possible for him to inflict on the individual anything that he could inflict on the image. This is the real meaning of the removal of the scalp lock of an enemy, its possession making the soul of the owner subject to the warrior procuring it. ''The illnesses to which the human race is prone,'' says Maspero, ''were not indeed all brought about by enchanters relentlessly persecuting their enemies, but they were all attributed to the presence of an invisible being, whether specter or demon, who by some supernatural means had been made to enter the patient, or who, unbidden had by malice or necessity taken up his abode within him.''

In China, Japan, India, and all those countries that came under the influence of the Buddhist religion the belief was universally held that disease was in many cases a punishment sent by the gods for some sin committed either in the present life or in some previous state of existence. Many Chinamen will refuse to rescue a drowning man for fear of interfering with the fates ordained by the gods and thus bringing misfortune on themselves. The Greeks believed the plague a direct visitation of the gods and therefore refused to use any means to stay its ravages. Bartels (2) cites several instances in which savage peoples attribute certain diseases—notably small-pox, measles, and other epidemics—to the anger of the gods, although this belief is not so common among primitive as among civilized peoples and those especially who have come under the influence of Islam and Christianity. Here the principle is the same although the spirits are good spirits angered or ill-disposed.

The anger of an enemy or a deceased relative is responsible for many ills. The Baganda of Uganda (50: 4, p. 124) believe that the spirits of dead relatives that have not been becomingly interred haunt their living kindred and cause sickness and even death if the offence is not in some way atoned for. The natives of Australia have a similar fear of the spirits of enemies killed in battle. In New Guinea a widow must observe carefully many rules in order that the spirit of her husband may not make her sick. In many countries the spirits of still born children, or of those that die very young, of those who die of small-pox or the pest, of those dying suddenly and the like are very much to be feared for their powers of causing many kinds of disease.

The evil eye is widely feared and persons reputed to be possessed of such power are scrupulously avoided and are usually regarded as witches. Mooney (60, p. 146) states that "the belief in the evil eye is general throughout Ireland as well as throughout the greater portion of Europe and Asia," but thinks it is too subtle and intangible an idea for most savages. The idea is not lacking in the current folk-lore and superstition of this country.

The belief, common to all grades of civilization, that many diseases may be transferred, no doubt owes its origin partly to the contagion of certain diseases although it is by no means confined to these. One may for example transfer his sickness to another by placing a piece of money, food, an article of clothing, or any of his belongings on a path or in some conspicuous place where it is likely to attract attention. The first person, or even bird or animal, that touches or picks up the object is immediately afflicted with the disease and the original sufferer restored to health. The underlying thought here is clearly that of contagious magic (38a, p. 2) by means of which the evil spirit is persuaded or compelled to take up a new abode.

There are beliefs scarcely capable of classification. The Yakuts of Northern Siberia (50:4) believe that man has three shadows; if he loses one he will fall sick, while if he loses all three he pays the penalty with his life. The Tchouktchis believe (10a) that man has many souls some of which he may lose withont serious trouble, but if he loses too many he becomes weak or ill. Many tribes believe that spirits of men and animals come stealthily by night and steal away a vital organ thus causing disease. The "doom" of the native Australian often means that his blood has been made bad or removed from his body by an enemy. By the same people consumption is thought to be due to the insertion by magic of a rope reaching from just below the Adam's apple down into the chest. Among the many other causes are the quartz crystals, pebbles, splinters, bones and the like which are inserted in the body by witchcraft; the magic wounds; the influence of bad winds, or water spirits, of charms, of fire, of the rainbow and other natural phenomena; the infringement of the *tabu;* and so through a list as numerous and diverse as it is possible for the minds of men to conceive. Our illustrations are necessarily limited in number and scope but are perhaps sufficient for our purpose which has been to show that everywhere the supernatural spirit theory is the prevalent theory of disease.

The Primitive Healing Art

Almost all observers are agreed upon the fact of the intimate relation of religion and medicine. Not only do we find among all primitive and ancient peoples a close relation in practice, but most of the accounts of the origin of the healing art are mythological and colored with religious ideas. A good example is that of the Menomini Indians (18 : 14th) who relate how a great "manitou" came to earth and chose a wife of the children of men. Of the four sons to which she gave birth, the first went to the spirit world when he arrived at manhood and learned all the mysteries of the "grand medicine." He then returned to earth and giving each of his family a medicine bag revealed to them the mysteries and told them to perpetuate the ceremonies into which he had initiated them, after which he departed and has since remained the intercessor with the Great Spirit. In ancient China, Japan, and Tartary we find a religion of shamanism, worshipping magical powers above all else. So, too, in Chaldea where Maspero (57, p. 780) finds that "consultations and medical treatment were religious offices, in which were involved purifications, offerings and a whole ritual of mysterious words and gestures." Among the Ojibwa, as with most other Indian tribes, the practice of magic and medicine is limited to those who belong to the great medicine society into which they must be initiated by religious ceremonies comprising four degrees. For the Romans Cicero declared: "the art of medicine has been consecrated by the invention of the immortal gods." Mooney (18) finds that "among the Indians the profession of medicine and religion are inseparable. The doctor is always priest, and the priest is always doctor"—an opinion from which few would dissent, even if it had been applied to all primitive peoples.

The Medicine Man. Since man has quite generally felt his inability to cope with disease and ill fortune unaided he has ever been ready to choose as an intercessor some one who possessed greater knowledge, wisdom, and power over the unseen agencies which his fancy created. The medicine man is almost universally raised above the common people, now trusted, now feared and hated, but always exerting a tremendous influence on all phases of primitive life. It is because in him are vested both medical and priestly functions that it is so difficult to separate form, ritual, and ceremony from the healing art. He is usually at once priest, physician, sorcerer, seer, and prophet.[1] He lives a life apart from his fellows; sleeps in a tent which stands apart and differs in structure from those of the common people; as a rule he does not work, but is supported by gifts and fees from his patients; he eats different food; wears robes which designate his station, and ornaments, paints, masks and many such things as are denied the common man. In some cases his office has come to him by heredity, or it may be some mark upon his person, even some deformity, has set him apart as a fit depository of divine power. Again some strange fact about his birth; a misfortune; a dream of his or of his friends about him; some manifestation of unusual power; or the supposed entrance of the spirit of a deceased shaman into him may set forth his fitness to become a great medicine man.

Whatever may be the nature of his call, he is usually prepared for entrance into his sacred vocation by fasting, prayer, and solitude; then, by hallucination or in a dream, his guardian spirit is revealed to him and by virtue of its power he does his mighty works. In some cases nothing further is required of him in preparation for his work, but with many of our North American Indian tribes he has still to serve an apprenticeship under an older man and learn by imitation the secrets of hypnotic influence, the methods of discovering and preparing remedies, the use of the drum, rattle, medicine lodge, and other

[1] In some tribes the religious "man of mystery" is to be distinguished from the magician—healer. See 50a, Vol. XVIII, pp. 269-275; also 18, Bul. 30, under "medicine man."

insignia of the office before he can stand the examination and approbation of his fellow shamans and be initiated into the "grand medicine society" with due form and ceremony.

Among the Indians, the Australian aborigines, and some other tribes the "medicine bag" is indispensable. The shaman carefully dries some medicine, prepared with due ceremony, and puts it, with other relics, as bones, pebbles, splinters, and the like, that have been extracted from some patient, into a bag made of the skin of his totemic animal with the hair on the outside and further decorated with beads, bones, feathers, porcupine quills, etc. This he always carries with him and about it centres a vast amount of mystery and superstition.

We have spoken thus far of men as shamans, but they are not always men; in fact, in some tribes, women are considered so far superior that the man who would be a good shaman must adopt the clothing and so far as possible the characteristics of the woman. This is especially true of the Yakuts of Siberia where the most powerful and most respected shamans are women.

Some shamans have medical books and all of them have a fund of lore that recounts chiefly the names of the evil spirits that cause disease and gives formulæ for their expulsion or advises offerings, plants, and other remedies for the same purpose. When the medicine man dies we usually hear little about it, or if we do it is that his spirit has entered into some young man and lives on with, perhaps, increasing power with the new incarnation.

Diagnosis. The diagnosis of disease consists in the first place in discovering whether the sickness is due to the anger of an offended deity, a broken *tabu*, the presence of a spirit, the loss of the breath-body, shadow, kidney fat, blood, or what not; or to the presence of some foreign object introduced, by witchcraft or magic, into the patient's body. In the second place the medicine man must discover what offering will propitiate the deity or evil spirit; what charm, incantation, or good spirit will restore the lost part, or counteract the malevolent spell under which the patient is suffering; or by what means the foreign object may be extracted, or the spirit exorcised and made thenceforth harmless.

The ways in which these objects are accomplished is what most interests us. The methods to be pursued and the means to be used are almost always supernaturally discovered. In some cases a prophet advises the medicine man as to the method of procedure; in others a woman is put into the hypnotic state, or a clairvoyant or spiritualistic medium is employed to "see" the cause and report to the shaman; or he may himself, in an ecstasy or a dream, see the cause and have the remedy suggested to him. A method which with some variations is used by nearly all our Indian tribes runs somewhat as follows: the medicine man, after a sweat bath and a stimulating drink, enters his medicine lodge where after much beating of drums and shaking of rattles, after prolonged dancing, shouting, groaning, screeching, beating of himself and invoking of his genius he works himself up into a condition favorable for the reception of the divine impulse, the spirits come and reveal to him the cause of the disease and a suitable remedy. Then, in the words of Charlevoix (18, 14th, p. 139), "full of his pretended divinity, and more like a person possessed by the devil than one inspired by Heaven, he pronounces in a positive tone of voice on the state of the patient, and sometimes guesses tolerably just."

Methods of Treatment, Exorcism, etc. If the first theory of disease was the evil spirit theory, the first reasoned treatment of it must have been entirely by magical means. The only methods admissible on such a theory are conciliation, transfer, or expulsion of the supernatural agent. The great variety of ways in which these ends were accomplished a few examples may illustrate. The shamans of the Algonquian, Ojibwa, Apache, Sioux, Sia, and other Indian tribes as well as the Malays, Australian blacks, and peoples in other parts of the world have a simple magical procedure known as the sucking method. Placing the mouth, a hollow bone, or a rope over the seat of disease they proceed to suck vigorously until the spirit, the immediate cause of disease is drawn out and disposed of in some way. The Malays (76) frequently construct a basket, which they fill with food and other offerings to attract the evil spirit. When it has accomplished this end, the basket and its occupant are set afloat on the river or taken to a desolate part of the forest and left. Others drown the spirit in a bowl of water or command it to leave the place. Among the Egyptians the disease demons were supposed to obey the gods, so when the healer was practising his art he assumed the personality of the divinity to whom the disease demon, with which he chanced to be dealing, was supposed to be especially amenable and was then ready to command the spirit as if he were the very god whom he personated. He even had the power to call to his assistance other divinities if he deemed it necessary.

Chaldea had sorcerers and exorcists who were experts in casting out, by magical means, demons and spirits which caused disease to the body they inhabited. The magician sometimes lighted a fire of herbs the clear flame of which was believed to frighten away the spirits and their evil influence. Accompanying this, to add to its effect, a prayer was offered in which the enchantments and their expected results were described.

In China, Japan, the Malay Peninsula, and among numerous primitive as well as with more civilized peoples, prayers and ceremonies are used to induce the destroying demon to remove his baleful influence. The most common method of treatment with the Bering Strait Eskimo is the incantation of the shaman. With the Malay and some African tribes the evil spirits are propitiated by offerings, or conjured or tricked from their human habitations in various ways. In one of the cases described by Skeat (76, p. 429) the shaman mixed some pulverized woods in water with which he washed the patient's body; then with a bunch of leaves, he brushed the patient from head to foot till at last the spirit was driven out; when with his dagger the shaman succeeded, after much ado, in destroying it. The Yakuts (50:4, p. 99) apply fire to the part where the *yor* is supposed to be until the bursting of the skin apprises them of the exit of the evil spirit.

The ancient Greeks believed that disease demons were to be conciliated by lustrations, invocations, prayers, offerings, and music; and one cannot fail to note the prominent place that music, or perhaps more truthfully noise, holds in the treatment of the sick by many primitive peoples. As a rule its purpose is ostensibly to frighten or conciliate the spirit of disease.

When disease is supposed to be due to any of the causes more indirectly traceable to the influence of evil spirits, the methods of treatment are not essentially different. The sucking ceremony mentioned above is often used to extract poison, a stone, a splinter of wood or bone, or other object that has been conjured into the body. Sometimes the Malay shaman cures by rubbing the body over with a piece of dough which gathers up the object causing the trouble (76). The same people often consider breathing on the patient or on the medicine an effectual means. "The miraculous cures of the Messiah," says Burton, "were, according to the Moslems mostly performed by aspiration." (Arabian Nights V, p. 30). The folk-lore of all peoples is full of magic means of many kinds with which to combat every sort of disease. White magic can suspend the law of destiny, combat the influence of secondary deities, the evil eye, and spells cast by evil persons. Some of these magic means deserve more detailed treatment.

Amulets, Charms, Talismans, Magic Formulæ, etc. In his study of the history of medicine Berdoe (4, p. 247) comes to the conclusion that, "in the ancient world as well as with savages, the whole art of medicine was in many cases the art of preparing amulets and charms." These things either cause, prevent, or cure disease according to the method of their employment; all are based on a conception of a supernatural origin of disease; and all depend for their success, where they have any, upon their effect upon the mind of the person invoking their aid. Erdman (34, p. 353) tells us that in ancient Egypt, men and even the gods, "wore amulets as a protection, and used magical formulæ to *constrain* each other." The dead wore amulets against the evils of the next world as the living against those of the present. Certain objects were given lasting magical power by having a formula recited over them, and as of old the modern Egyptian wears amulets and written charms and puts great faith in the magical preparations of the higher theurgy. Quackery and sympathetic cures by means of amulets, magic words, laying on of hands, symbolic washings and the like were very common in ancient Greece. In Chaldea one of the infallible cures was a charm knotted with seven knots which was bound to the patient who was then sprinkled with holy water. Throughout all their history, astrology, charms, amulets, and characts enter largely into Chinese and Japanese practice. "There is scarcely a disease," says Pettegrew (68, p. 78), "for which a charm has not been given." In the early Christian centuries Gnosticism was reponsible for the introduction of many wonder-working charms and amulets. Some of the old Anglo-Saxon monks made use of amulets and their countrymen have never ceased to do the same.

The most scientific men of Greece and other countries of antiquity were unable to free themselves entirely from a superstitious faith in magic formulæ. Even Galen the great Greek medical reformer and authority of the second century A. D. did homage to incantations, having been convinced against his previous conviction that "many of them are excellent severally and they reach their mark." But there were fine distinctions even in his day for he denounced Pamphilos for using incantations "not merely useless, not merely unprofessional, but all false." (4, p. 253.) Erdman (34, p. 353) states that "the belief that there were words and actions by which they could produce an effect on the powers of nature, upon every living being, upon animals, and even upon gods, was indissolubly connected with all the actions of the Egyptians." The formulæ used by the magicians were believed to be revelations from the gods themselves. They were made up wholly or in part of words from some foreign tongue or of a meaningless jargon, and the more mysterious and difficult of understanding they were the greater their power was thought to be. Written words were by many of the peoples of the olden times believed to have magic power. In Egypt even to the present time pieces of paper inscribed with texts from the Koran are swallowed and beneficial results are fully expected. In many places magic formulæ are written on pieces of wood or slate which are then washed with water and the water used as a powerful medicine. Some of the Jews believed that Jesus had learned the Mirific WORD (true pronunciation of the word Jehovah) and by its use was able to cure diseases. The early Christian church opposed these superstitions; but so deep seated was the faith in them among her converts that the best that could be done was to substitute Christian names for heathen ones and allow their use to be continued. Peters (67, p. 163) tells us that in the folk-medicine of Germany very many cures are yet made by magic formulæ; and that in many parts of the country "*Besprechen und Stillen*" are means of healing which the inhabitants still prefer, in cases of sickness, to the help of a physician.

Use, Preparation, and Discovery of Remedies. The early use of remedies was closely connected with the spirit theory of disease. Almost all ancient and modern peoples have used certain plants as remedies; but in early times, at least, this was from no idea of their physiological effect, but because they were supposed to be distasteful to the disease demons which afflicted men. In Chaldea as in Egypt the records show that greatest reliance was placed in grotesque and revolting remedies fitted to excite disgust in the tormenting demon that caused disease. Maspero (57, p. 782) finds that although the Chaldeans were not ignorant of the natural virtues and uses of herbs, their physicians esteemed more highly prescriptions "which pandered to the popular craving for the supernatural." Even natural remedies were only made effectual by supernatural means. There were certain days for gathering each plant, definite formulæ to be recited as the plant was pulled from the ground, others as the mysterious cooking, filtering, etc., were in progress with their accompaniment of murmuring, singing, and exorcising of evil spirits that might enter and render the preparation ineffective. Again, as they were being administered to the patient, suitable formulæ and incantations recounting the effects that were attributed to and expected from their action must accompany their use. In this way the employment of natural remedies was confined largely to the physician, for only the inner circle knew the necessary formulæ and charms without which no remedy would be of any avail. This conception prevailed also with the ancient Hindu and Persian and finds its place to-day among almost all primitive peoples and to a surprising degree among many more civilized races as we shall see.

Again, there have been times in the history of almost every people when sympathetic magic measured the value of many remedies. Under this conception, some real or fancied resemblance between the remedy and the bodily organ to be cured was the basis of evaluation. So far is this idea carried among the Cherokee that Mooney (18, 7th, p. 329) is convinced that "Cherokee medicine is an empiric development of the fetish idea." Of the plants used by them he finds that only one-third possess real medical virtue, the other two-thirds being inert, if not positively injurious. Thus we see that the doctrine of signatures had much more to do in directing to the choice of proper remedies than their actual physiological effect. This idea of "like cures like" carries us back to that stage of man's thought in which he sought out and gave special meaning to each slightest resemblance between himself and objective nature. The conservatism of human thought has preserved it, however, so that it still holds a prominent place in the folk-medicine of every race civilized or otherwise.

In this connection Bourke (13) has written in a most careful manner one of the darkest and most repulsive chapters in the history of primitive and folk-medicine. He shows that the use of all sorts of nameless remedies both current and extinct are to be traced to a religious origin. One of the best examples has to do with the Grand Lama of Tibet who is revered by his subjects as a divinity. Maltebrun asserts (13, p. 42) that "it is a certain fact that the refuse excreted from his body is collected with sacred solicitude to be employed as amulets and infallible antidotes to disease." Gilmour (p. 47) has observed that "when famous lamas die and their bodies are burnt, little white pills are reported as found among the ashes and sold for large sums to the devout as being the concentrated virtues of the man and possessing the power of insuring a happy future for him who swallows one near death."

Vambery tells of a holy Turkoman who sold as a wonder-working medicine a cup of water over which he had recited numerous sacred verses and into which he had expectorated at the end of each stanza (p. 48). In like manner we are told in the Arabian Nights (II, p. 222) how the excrement of ecclesiastical dignitaries was sold at a high price and used as a remedy for many diseases. So, too, the indestructible parts of the bodies of the Buddhas and saints are accorded curative powers by the Buddhists (p. 49). On the same principle are numerous legends (p. 57) relating to the curative properties of articles in any way connected with the body of Jesus ; *e. g.*, the water in which Mary washed the infant Jesus, the touch of his swaddling clothes, the water in which his body was washed in preparation for burial, the bandage with which he was blindfolded, his blood, the crown of thorns, cross, cup, etc., are all said to have been used in healing many diseases. In both Ireland and England the water in which the chalice used by the priest at communion is washed is given to delicate children or those having whooping cough as they are thus supposed to partake of some of the real blood of Jesus (9, p. 88).

A slightly different class of religious facts noted by Bourke (p. 97) have also much significance for the healing art. It is because of a religious conception that "the pranks and gibberish of the maniac or the idiot are solemnly treasured as outbursts of inspiration" and it was at first to the end of producing a state of religious ecstasy that the intoxicating mushroom, mistletoe, rue, ivy, mandrake, hemp, opium, and stramonium were used; but the insane talk of the mental pervert and the revelations of the religious ecstatic have often served as guides to the treatment of disease, and the natural intoxicants we have enumerated have often been used simply as medicines when their religious significance has become somewhat forgotten. For example: the mistletoe, called "all heal" by the Druids, has been used in many countries and many centuries even to the present for the cure of epilepsy, rupture, fits, sterility, for exorcism of evil spirits, and as amulets for a great number of other diseases.

It seems hardly advisable to record here the many more revolting practices connected with the healing art throughout the entire world; all the more degrading because of their close relation to religion which has always been the revealer of that which is best in man; but, deplore it as we may, the fact remains as Bourke has shown (p. 333) that "excrementitious remedies are still to be met with in the folk-medicine of various countries; indeed the problem would be to determine in what country of the world at the present day the more ignorant classes do not still use them." Indeed, in her study of the folk medicine of our own country, Mrs. Bergen finds many remedies of so offensive a character that it seems almost incredible that they "can still retain a place in even the rudest traditional pharmacopœia" except we admit that there is "in the uneducated human mind a sort of reverence for or faith in that which is in itself disagreeable or repulsive." (Pop. Sci. Mo., 33, p. 658.)

Faith in the Medicine Man. It is interesting to know what attitude the medicine man inspires in his patients. On this point some valuable data are at hand as to both ancient and modern times. Especially valuable is the testimony of those men who have made recent close studies at first hand and put themselves in a position to observe the healer at his work.[1]

[1]Such studies as those of Mooney (18, 7th and 14th), Hoffman (18, *ibid.*), Stevenson (18, 8th), Bourke (18, 9th), Mrs. Stevenson, Turner and Dorsey (18, 11th) in this country, Roth (72) in Australia and Sieroshevski (50, 4) in Siberia are mines of scientific data of the utmost psychological importance.

Berdoe (4) points out that in ancient Greece, the stories of miraculous cures, the many offerings and images left in the temple by grateful patients to commemorate their cures, and the divination, magic, and astrology used by the priests to discover the proper remedies, all tended to inspire in the patient the greatest hope and expectancy for his recovery and are strong evidence that "faith was the *sine qua non* in the patient." He is also convinced that although different means were employed the same attitude toward the healer was conserved among the Chaldeans.

Bourke in his study of the Apache Indians finds that their medicine men are credited with many wonderful powers. They cause rain, hail, tempests; they call up the shades of the dead and consult them; they handle serpents, swallow spear heads, arrows, fire; they locate lost property by crystal gazing; they kill and bring to life, and by many other means strengthen their hold on the people. In the same study (18, 9th, p. 473) Bourke quotes Dr. Fordyce Grinnell to the effect that "the Apache scouts seem to prefer their own medicine men when seriously ill, and believe the wierd singing and praying around the couch is more effective than the medicine dealt out by our camp 'sawbones.'"

Mooney (18, 7th, p. 323) finds that the Cherokee "has the same implicit confidence in the shaman that a child has in a more intelligent physician. The ceremonies and prayers are well calculated to inspire this feeling, and the effect thus produced upon the mind of the sick man undoubtedly reacts favorably upon his physical organization."

The Hudson Bay Eskimo is not so loyal to his own medicine men as some of our Indians, but his faith in the supernatural is no less for that reason. Turner (18, 11th, p. 270) tells us that magical cures are sometimes effected by harmless concoctions of ignorant white traders so great is their faith in such remedies. "Powders are rubbed over the seat of pain and liniments are swallowed with avidity. Strange as it may seem they often report good effects, and rarely fail to ask for more of the same kind." Hoffman (18, 14th) thinks there is no doubt but that the Menomini believe their medicine men possess great power, and he himself was unable in six years of careful observation of their ceremonies to detect imposition. Spencer and Gillen find that the central Australians have implicit faith in their medicine men (78, p. 530). Most observers are convinced of the honesty and sincerity of the great majority of the medicine men themselves (18, Bull. 30, medicine man), although there are, no doubt, some imposters among them.

The Place of Magic and Miracle

After all this array of facts the question naturally arises: to what extent is the total practice of primitive and ancient peoples here represented? This question we shall try to answer.

In later Chaldean times there were two schools of priest-doctors: (1) those who relied on magic entirely; (2) those who taught that disease was the result of sin and that repentance was the way to cure. In time there came also to be a class of physicians who used only simple natural means. But those who used magic, charms, prayers, and incantations were still held in higher esteem and accorded greater powers. In Egypt the profession of Medicine has always been surrounded with the greatest secrecy; its practice strictly limited to the initiated; and the mysterious and magical element preserved by forbidding the laity to practice and thus become familiar with its means.

Although many of the orthodox Buddhistic teachings of the present and more recent centuries would seem to preclude the possibility of such practices as those we have found current among other races, the sacred literature of the Hindus which reaches back several thousand years reveals a large element of magic and miracle in their early healing art. Buddhistic, Brahmin, and Jainistic monks are now forbidden to use magic arts, and efforts have been made to discredit the *Atharva-veda-Samhita* and exclude it from the sacred canon. For these reasons it is of special interest as a source of knowledge of the folk medicine which grew up before the priestly religion had developed opposition to such beliefs and practices. From the *Atharva-veda* (61, Vol. XLII) itself and from Winternitz (88) we gather that in earliest times the priests of the people were at the same time magicians and sorcerers as the name Magi, applied to the ancient Atharvans, would indicate. The ancient name for the *Atharva-veda* itself has the signification of both white and black magic and its purpose as stated by the Hindus is "to conciliate (demons), to bless (friends), and to curse (enemies)." It is made up chiefly of songs and speeches for the healing of diseases. In it we find the belief in demons as the cause of disease in many of the usual ways, the belief in incubi and succubi, and in exorcism. There are formulæ for insuring long life, for purification from sin and guilt, for restoration of harmony in households and between lovers. Winternitz (88, p. 111) finds that the *Atharva-veda* has preserved for us, in its magic songs and rites, many ideas which are still current, and that it contains "numerous verses which in their character and often also in their content are as little different from the incantations of the Indian medicine men and the Tartar shaman as from the Merseberg charms which belong to the small remnant of the oldest German poetry."

About the same may be said of the Parsees as appears from their sacred book the *Zend-Avesta*. The belief in evil spirits, demons, magic, etc., and the same sort of prayers, incantations, charms, and formal ceremonies to ward off evil or to cure disease, are to be found here as in the *Atharva-veda*. We are told (61, pp. 219, and 229) how Angra Mainyu, a helper of Ahriman, created 99,999 diseases to afflict men and how they were defeated by the 10,000 healing plants which Ahura Mazda brought down from heaven, by Airyaman's permission, for the use of Thrita a priest of the god of life and health. Here is recorded the belief in the supernatural origin of both disease and the healing art, the intimate relation of religious and magic ceremonies, and proof of the fact that originally the two were not distinguished. Whatever the later scientific developments may have introduced in the way of treatment for disease, the sacred literature of both the Hindu and the Parsee in its original purity reveals nothing that does not have in it an element of the supernatural. The healer among healers was he who healed by the holy WORD.

To Blümner (10, p. 238-9) "the healing processes to which the priests of the Æsculapian sanctuaries resorted seem to have occupied a very doubtful position between empirical therapeutics and superstitious hocus-pocus" and are to be "especially distinguished from those of the professional physicians by the veil of secrecy and miracle which surrounds them, since they rightly understood that the love of wonders among the common people would always bring them success." Every one is familiar with the popularity of these more miraculous methods of cure in Greek times, and with the great difficulty which medical reformers experienced in their attempts at the introduction of the scientific spirit in medicine.

The Romans had no systems of treating disease but by prayers, charms, prescriptions from the Sibyline books, and a crude domestic surgery and medicine, until they learned from the Greeks. The practice of the average physician was a combination of superstition and legerdemain.

While savage and primitive peoples undoubtedly know something of surgery and the use of materia medica, baths, massage, and the like, it cannot but be evident from what we have already said that their treatment of most diseases is largely or wholly by magic and supernatural means and that even real therapeutic means were given supernatural rather than the natural significance. We may safely say that in so far as diseases have, to them, no natural cause; in so far they have no strictly natural means for their treatment. However simple and natural the explanation of their methods may appear to us, and however absurd the expected results of many of their procedures, we must recognize the fact that to them these things were miraculous and no result which was conceivable to them was theoretically impossible of achievement.

Hebrew Miracles

Old Testament Times. Little has been said so far of the Hebrew miracles of healing which have always attracted, in Christian countries, more attention than any others because of the special significance that has been attached to them. We must give them the somewhat special treatment which they deserve and trace their connection with the miracles of both earlier and later times. It is undoubtedly true that, living in the midst of a civilization permeated by the influence of magic and superstition, the Hebrews as a people were less given over to such practices than any other people of the time. The reason is to be found in the superiority of their religious ideals and the necessary modifications of primitive beliefs which their strict monotheism involved. But even here we may find many traces of primitive conceptions of disease and its cure. Indeed it is inconceivable that the Chaldean father of the race should not have retained some of the ideas so prevalent in that land from which he set out as a pilgrim. Knowing what we do of the influence of surrounding nations upon the Hebrews in other particulars all through their history it would be hard to believe that they had exercised no influence upon them in this one respect. However this may have been, we are not left wholly to conjecture in the matter for whether through the influence of surrounding nations, which seems evident in many cases, or by an indigenous growth along lines normal to every human mind there is to be found abundant evidence that among the ancient Hebrews many primitive views prevailed.

To the orthodox Hebrew disease was of penal origin and might be inflicted by Satan, or by the agency of evil spirits when permitted, or by a direct visitation from God himself as a punishment for sin either personal or parental. In several instances in the Old Testament the prophets were instrumental in bringing leprosy, blindness, and even death upon the sinful by calling down the just judgment of God upon them. Under this conception God was the only physician of his people and his blessing and forgiveness were the effectual means of cure.

But not all Hebrews were orthodox, and the Bible, the Apocrypha, the Talmud, and Hebrew tradition bear witness to the fact that as idolatry lived and flourished in the very palace of the kings so the superstition and magic of Chaldea, Assyria, Babylonia, Egypt, and "the peoples of the land" flourished among the Hebrew populace. Whether the crude ideas of disease and its cure which prevailed among them were borrowed from these surrounding nations, or developed indigenously, is a matter of much less importance than is the fact that in no one of their conceptions did they differ materially from other ancient or primitive peoples which we have studied.

It is true that we have little positive evidence from the Old Testament as to the general practice of the people in the cases of ordinary sickness except that it was continually impressed upon them that the Lord would heal all their diseases. The provisions of the Levitical law for leprosy and kindred diseases were hygienic rather than medical; the consultation was with the priest and was preceded or accompanied by offerings and ceremonial observances. Most of the miraculous cures recorded in the Old Testament were performed through the power or intercession of the prophets. When David's child was at the point of death it was Nathan the Prophet whom he besought to save his life; when Jeroboam's son was sick he called for Ahijah the prophet; it was Elijah who raised the son of the widow of Zarephath to life; Elisha who restored the Shunammite's son; Isaiah who brought the answer to Hezekiah's prayer and directed the means for his recovery; it was Elisha's bones that restored the dead man to life; Moses's intercession which procured the cure of Miriam's leprosy; and it was the Man of God who restored Jeroboam's withered hand. As these comprise most of the miracles of the Old Testament, it is evident that here, as with most other peoples, the office of priest, prophet, seer, and physician are closely related.

There is considerable circumstantial evidence that the practices of the peoples of the land of Canaan, as well as those of other surrounding nations with which they had dealings, were quite prevalent among the Hebrews. The jealousy ordeal of Numbers 5:17 has its parallels in primitive medicine as well as in all Mohammedan countries, where we should call it magic. This drink, composed of holy water into which the priest put some dust from the floor of the sanctuary and into which he washed the curses against unfaithfulness which he had previously written on a parchment, the accused was forced to drink. This is "the bitter water which causeth the curse." If innocent the accused received no harm, while the guilty could not drink with impunity. The miraculous healing of those bitten by the "fiery serpents" by means of the brazen serpent which Moses lifted up in the wilderness, bears striking resemblance to the theory of sympathetic magic. It seems evident that Rachael and Leah (Gen. 30:14-16) knew the reputed magic power so universally attributed by primitive peoples to the mandrake. The beneficial effect of music was recognized when Saul was troubled by the evil spirit from the Lord. The plague is represented as a destroying angel and in one case was stayed by incense which Moses and Aaron carried among the people, in another by an angel of the Lord.

The belief in all the arts of magic, witchcraft, divination, enchantments, etc., seems to have flourished all through Old Testament times in spite of the opposition which the prophets and religious leaders of the people continually waged against it.[1] Balaam, who is spoken of as a soothsayer, was certainly believed in by the Hebrews. The Moabites came to him with the "rewards of divination in their hands," but he refused and (Num. 24:1) "went not as at other times, to seek enchantments," but "saw the vision of the Almighty, falling into a trance, but having his eyes open," and declared (Num. 23:23) "surely there is no enchantment against Jacob neither is there any divination against Israel." True, Balaam was not a Hebrew, and consultations with familiar spirits were denounced, but still there seems to have been among the people a belief in these things all the while. The Levitical law "thou shalt not suffer a witch to live" is in itself evidence of the belief in witchcraft even with the law giver. Deut. 18:10-11 says: "there shall not be found among you any one that maketh his son or his daughter to pass through the fire, or that useth divination, or an observer of times, or an enchanter, or a witch, or a charmer, or a consulter with familiar spirits, or a wizard, or a necromancer," things which they were warned not to learn from the people of the land. Saul, however, found occasion to put away those with familiar spirits and the wizards, but when he himself could get answer "neither by dreams, nor by Urim, nor by prophets," he was able to find a witch at Endor. Manasseh (2 Chron. 33:6) again introduced all these practices at the very temple itself. Josiah soon after endeavored to purge the land of all such "abominations that were spied in the land of Judah and in Jerusalem," and still Ezra much later complains that "the people of Israel, and the priests, and the Levites, have not separated themselves from the people of the lands, doing according to their abominations, even of the Canaanites, the Hittites, the Perizzites, the Jebusites, the Ammonites, the Egyptians and the Amorites." (Ezra 9:1.)

Hebrew Tradition. Aside from the rather indirect evidence as to healing practices in the Old Testament and later times, the Talmud, Cabala and other traditional writings give some insight into the ideas that are believed to have been current in those days. Tradition has it that Adam, during his last illness,

---

[1] Ezek. 8:17, Hosea 4:12, Isa. 2:6, 47:9, 12-13, Micah 5:12.

sent Eve and his son Seth to the Garden of Eden to procure some of the oil of healing which he had learned to use in the treatment of seventy-two diseases that his sin had brought upon him. Noah also figures in tradition as a man who possessed many wonderful powers among which was a knowledge of the healing art.

Coming to more distinctly Hebrew names: Abraham, the Chaldean father of the race, is credited with the discovery of astronomy, the invention of the alphabet, the knowledge of magic and other secret lore; and says Kohler, "it is related that he wore a pearl or precious stone of magic power on his neck, wherewith he healed the sick" (49, p. 87). The knowledge of this magic power, we are told in the same connection, is supposed to have been revealed to the sons of his wife Keturah. Joseph employed physicians and may be supposed to have had considerable knowledge of Egyptian medicine, as Moses, the lawgiver, undoubtedly had.

Solomon's is perhaps the greatest Hebrew name in the traditional history of medicine. Josephus (p. 593) in his account of Solomon's wisdom tells us that "God enabled him to learn that skill which expels demons . . . and he left him the mode of using exorcism by which they drive away demons so that they never return. And this method is prevalent unto this day, for I have seen a certain man of my own country, whose name was Eleasar, releasing people that were demoniacal in the presence of Vespasian . . . the manner of the cure was as follows: He put a ring that had a root, of one of those sorts mentioned by Solomon, to the nostrils of the demoniac, after which he drew the demon out through his nostrils, and when the man fell down at once, he adjured him (the demon) to return into him no more, making still mention of Solomon and reciting the incantations which he composed." The lost book "Wisdom of Solomon" is said to have been a book of magic prescriptions, formulæ, and incantations for the cure of all kinds of disease and was for centuries in great vogue among the people (17, p. 97). Another tradition states that "Solomon possessed power over demons by virtue of a talisman, which consisted of a signet ring of brass, upon which was engraved the most great name of God" (49, IV, p. 521). The Musselmans still revere him as the greatest of all physicians and no doubt many of the magical means of cure still used in the East had their place, if not their origin, in this now lost book.

The use of amulets has a history of several thousand years with the Hebrews, and it is believed that with them as with primitive peoples to-day all ornaments were originally amulets, charms or religious symbols possessing supposedly real virtues. During the Rabbinical period (first to sixth centuries) they were used very extensively, especially by women and children, but medical men are also known to have made use of them. Those inscribed with scripture texts, names and especially the mystic name of God were most highly prized. A peculiar design of a star in a circle with numerous sacred names inscribed upon it is said to have been designed by Adam. The Creator himself was believed to have made use of such means to perform his mighty works.

Although it is very difficult to know at what time such traditions as these sprang up, and although it is impossible to say just what were the facts if any which gave them credence, they are nevertheless of some value as evidence that in quite distant times many strange practices were made use of in the treatment of disease, almost all of which seem to lend credence to the view that underlying them was the disease demon theory and a belief in magic as the effectual means of cure.

New Testament Miracles. The prophets of the old dispensation looked for a Messiah who should be the healer of His people and those who had eyes to see could not fail to recognize such an one in the person of the Great Physician. In Him we find the culmination of miraculous power. When we compare the long stretch of centuries covered by the Old Testament record, its few scattering miracles centering about a few strong personalities, with the three short years of Jesus' active work crowded full of miraculous doings, we can form some estimate of the importance and frequency of these manifestations of a great personality. In short we have here the greatest epoch in the history of the miraculous. The miracles of other peoples and even those of the prophets of Israel seem on the whole a little trifling and insignificant in comparison. While many of the conceptions of disease and its cure which prevailed under the old order of things were still prevalent at the time of Jesus, there are certain marked differences that are worth noting.

In Old Testament times, as we have seen, the traces of a belief in evil spirits as a cause of disease are few, indefinite, and of a primitive type; in the New Testament we find a perfectly developed demonology clearly stated and generally accepted. The similarity between the Persian and Chaldean demonology and that of the Jews has often been noted and many critics have attributed the likeness to influences exerted on the Jews in captivity. The trend of present day criticism, however, is to make less of the influence of outside nations and to recognize that the Jews being a people of like race and temperament with the races about them, living in a similar environment, and possessing, as we have seen, the germs of such an idea, needed only the rigor of their Judaistic monotheism to complete the evolution of a demonology such as we find in New Testament and subsequent times. However that may have been, there is no mistaking the fact that a radical change of view has taken place and that Satan and his emissaries play an exceedingly important rôle in disease from this time on. How far Jesus shared the current views it is impossible to know but in His healing work He proceeded as though the prevailing explanation of the phenomena of demon possession were the true one. On the other hand there is no very strong proof that He ever considered disease a punishment for sin and on one occasion at least He is quoted as having clearly opposed this old Hebrew conception as contrary to the spirit of His religion.

Another feature which distinguishes the new from the old dispensation is the preponderance of miracles of healing. With the prophets and seers of the olden time cosmic miracles were more numerous than healing miracles. In New Testament times the reverse is true. Taking as a basis the number of occasions on which Jesus exercised miracle working power we find that more than 72 per cent. of them were of healing. If we take into account the many expressions such as: He healed "all that were sick", "all that were diseased", "them that were possessed with demons", "them that had need of healing", "many", "multitudes", "all manner of diseases", and others of like import the relative number of cosmic miracles accredited to Jesus sinks into comparative insignificance. His statement to the disciples that by faith they might remove mountains may be taken as evidencing His belief in their ability to perform cosmic miracles, but so far as we are informed only a few of them ever performed even miracles of healing.

Both Jesus and some of His disciples are said to have cured all manner of disease, and so far as the record goes Jesus never failed in any attempt to cure although the disciples did in at least one case. Among the recorded cures of Jesus where details are given the common diseases rank as to numbers cured in the following order: leprosy, demon possession, blindness with several others in about equal numbers. In the general statements the writers speak of the sick, diseased, tormented, possessed with demons, lunatic, palsied, leprous, blind, deaf, dumb, lame, maimed, halt, and those afflicted with the plague as having been cured and the dead raised to life by the Great Physician.

The methods used by all the miracle workers of the New Testament were not radically different from those with which we have already become familiar among other peoples. There was no hesitancy in making use of such traditional means as saliva, the touch of the hand of the wonder worker, baths; the Levitical regulations were observed in the healing of lepers; Peter's shadow cured any sick upon which it chanced to fall; handkerchiefs or aprons rendered potent by contact with the body of Paul cnred diseases and expelled demons (Acts 19:12); the WORD of power was used by all. Exorcism, here as with all other peoples of antiquity, was the almost universal means for the casting out of evil spirits. As in Moses's time it was admitted that the difference between the wonders of the Egyptian magicians and those performed by the Lord through Moses was only one of degree, demonstrating the greater power of the true God, so Jesus seems to have believed that others were able to use the power of exorcism. To the Pharisees He said: (Math. 12:27) "If I by Beelzebub cast out devils, by whom do your children cast them out?" At another time He rebuked the disciples for forbidding one who followed not with them to cast out evil spirits in His name. The almost universal requirement which was insisted upon as a condition of cure was the faith of the patient. Where this was weak He strengthened it and on at least one occasion He intimated that He would not, or perhaps could not, do anything without it. Unbelievers were sometimes excluded, certain traditional methods were used, in every way the patient was stimulated to do all he could for himself, and it should be noted that whether purposely or not the laws of mental medicine as we now know them were conformed to in almost every detail. For a detailed development of this point one may consult Hudson (44).

Some modern critics make much of the genetic order of Jesus' miracles, seeking thereby to show that not only did His own feeling of His power grow by exercise, but that the miracles which He performed make increasing demands upon our faith. So, too, they maintain that the publicity with which He did His work increases as His ministry advances. In His earlier miracles they find that the initiative was usually taken by the patient, whereas later from pity and a sense of duty He often relieved those who had not sought His aid. The nature of the records, their meagreness and the conditions under which they were written make it difficult to maintain this point satisfactorily, and if it were true that the miracles manifested increasing power it might be accounted for by the increasing faith of the people as they became more acquainted with His work. The genetic view is not an unreasonable one, however, and in a psychological study of the life of Jesus it yields some very helpful conceptions. It is beyond our purpose to pursue such a discussion in detail.

The fact of most significance for our study is the great power of personality manifested in Jesus. Even the extremists in criticism admit that no life has ever had crowded into three brief years such an amount of wise teaching, deep insight into life, and such manifestations of the power of personality as this life. It is no disparagement whatever to this life that the mighty works which were done in it do not differ radically from those of other great minds. It is enough to know that in it were revealed depths of insight into the great fundamental laws of life the significance of which we have scarcely glimpsed as yet. Compared with the therapeutic methods of His time, His methods were simplicity itself. They were, in short, psychic and were based on an understanding of the ultimate relation between mind and body such as that toward which medical science is now more and more tending. The miracle workers among His followers were Peter and Paul, the two strongest personalities of apostolic times and the two men who understood most fully the import of Jesus' life and teaching.

Christian Miracles of Healing

Such eminent Jewish authorities as Kohler, Broydè, Blau[1] and others are of the opinion that the Essenes a Jewish order of monks who pretended to exercise great miraculous powers through the magic spell of the Holy Name, were in a sense the predecessors of the Christian miracle workers and did much to prepare an atmosphere favorable to the acceptance of the wonder working power of the name of Jesus as used by the apostles. They also maintain that rabbinical literature made less of the miraclous than did the literature and practice of early Christianity, and that cures by exorcism and other miraculous means were most common in Judeo-Christian circles. The belief in such powers they consider a common characteristic of both pagan and Jew at the beginning of the Christian era.

During apostolic times miraculous powers were in evidence but became less prevalent at the close of this period. Beginning with the second century the rôle of the miraculous again becomes important and continues to be increasingly so till recent times. In many quarters even an extreme belief in the miraculous is still maintained as a true tenet of Christian faith historically established and not to be altered in any particular by the findings of present day science.

Brewer (16) has summed up in his recent "Dictionary of Miracles" some valuable and interesting data concerning the miracles of Roman Catholic saints. Beginning soon after apostolic times there are records of the miraculous powers of the saints in ever increasing numbers. At first these miracles

[1] For the opinion of these scholars see "The Jewish Encyclopedia," (49) vol. iv, p. 519; v, p. 230 and 306; viii, p. 607.

were wrought by the saint in person but later the same powers were credited to their relics, tombs, and shrines. The hagiography of the church teems with the miraculous all through the middle ages. There are several notable saints any one of whom, if we are to believe the record, performed miracles the briefest record of which would fill a good sized volume.

In all of these miracles there is a close resemblance to the Bible records, the example of Jesus and His early disciples being followed both as regards methods and language employed. The demands made upon our credulity are, however, so much greater than those of the Bible records that this in itself discredits many of the accounts. For example the fact that raising of the dead is one of the most common of miracles and the fact that bodies that had been completely dismembered are said to have been restored, that still born children and even roast pullets, partridges and the like could be restored to life tends to discredit the whole record of the miraculous during the middle ages. The same diseases are spoken of as those already mentioned in the preceding section.

From the fourth century on the sign of the cross became one of the most potent means of effecting cures. It was and still is regarded as a charm usually sufficient in itself to cure, but many times its use is accompanied by prayer, the name of Jesus, holy water or oil, a kiss or touch and the like. Beginning about the sixth century the crucifix was used in the same way.

A few illustrations of methods used by the saints must suffice to show how closely these were copied from Bible accounts. In raising the dead the saints either stretched themselves upon the body as did Elijah or taking the person by the hand called him by name or commanded him to arise. The use of saliva or holy water was the common means of restoring sight; pilgrimages to the tombs of dead saints were the resort of cripples; exorcism was the usual procedure in casting out demons; touch in the cure of king's evil. Some originality may be granted to the saint who exorcised a demon by drawing its picture and then burning it. The mere sight of a saint caused one demon to flee from the ear of the possessed in the shape of a mouse. Profession of faith and baptism were frequent means of cure for disease. Exorcism was long a part of the formula of baptism in the Catholic church as well as the use of saliva (16, p. 295). St. Francis (1416-1507) we are told used saliva and the sign of the cross in a remarkable miracle performed on a child born without eyes or mouth. The record says: (16, p. 232) "St. Francis marked with his spittle the place where these features ought to have been, and then making the sign of the cross the infant became possessed of two brilliant eyes and a model mouth." Several of the saints are said to have stayed the plague, and Pope Leo IX (16, p. 264) gave wine in which relics had been steeped to those who were afflicted and it is said that "all who drank in faith recovered" although we are not told how many were thus cured. As has already been stated the plague was regarded by many besides the Christians as a stroke of divine displeasure, but Procopius's description of it shows so well the immediate agency believed by people of his time to be its cause that I must quote. He says: (16, p. 438) "the manner of attack was this: visions of spirits in all sorts of human shapes were seen, and these spirits struck with a blow the victim who was forthwith taken ill. At first men tried to turn away the demons by uttering holy names and hallowing themselves as best they could; but they gained nothing by so doing . . . . . not a few saw the phantom demon in their dreams at night; it stood over them and struck them, and they were numbered among the dead."

The belief in demon posession, as we shall see, has not died out, but during the middle ages and early renaissance centuries it played a much more important rôle than now. Some things in regard to the treatment of such cases have already been noted, but the following quotation from a book entitled "A Club for Exorcising Demoniacs," written by Mengus in 1600, throws so much light on the methods of that time that we cannot afford to omit it. "If after mass has been signed with five crosses, sprinkled with holy water, and there have been invocated over her (the possessed) the name of the Father, Sonne, and Holy Ghost, the devill still shews himselfe refractarie, and will neither depart, nor tell his name,—then you must come upon him with as many nicknames as you can possibly devise, and thou shalt say: 'Heare thou sencelesse, false, and lewd spirit, maister of devills, miserable creature, defrauder of souls, captaine of heretiques, father of lyes, bestial ninnie, drunkard, infernal thiefe, wicked serpent, ravening wolfe, leane hungerbitten son, seely beast, truculent beast, cruell beast, bloody beast, beast of all beasts most bestiall, Acherontall spirit, smoakie spirit, Tartareous spirit, and so on, I command thee to tell me thy name, and to depart hence into thyne own place' " (16, p. 103). As will readily be seen, these directions combine several of the most potent means known to the Christian exorcist of the dark ages: the mass, sign of the cross, holy water, the use of holy names, knowledge of the demon's name, and abusive language. All these means, singly and together, were many times used and it seems quite probable that their effectiveness was thought to follow about the order in which they are here mentioned.

King's Evil. In this connection some mention must be made of the miracles performed by the French and English kings. The early development of the idea that by a sort of divine right the power to cure scrofula, or as it was at that time called, "King's Evil", was conferred upon the sovereign it is impossible now to trace. It seems certain (16, p. 306) that the French kings enjoyed the privilege long before those of England, and tradition says that it was first a gift to Clovis at his baptism in 496 A. D. Another persistent legend is that it was bestowed as a gift by St. Marcone in the sixth century. However this may be, we find that many of the early French kings made pilgrimages to Corbeny, where the body of this saint reposed, in order to have his skull placed reverently in their hands by the monks. On the day following this ceremony they were ready to banish with a touch that disease, "*pour la guérison desquels Dieu a accordé aux rois de France une grace singulière.*" Most of the French kings used the sign of the cross saying at the same time "*Le roi te touche, Dieu te guerit.*" This divine prerogative was exercised by the French kings from the time of Louis le Gros (1108-1137) with few exceptions until 1825 when Charles X effected many cures. Some were deprived of the right, it is said, because of evil life, but rather out of harmony with this is the fact that some of the most notoriously immoral of the kings exercised it with success. Phillippe VI (1328-1350) is said to have cured 14,000; Louis XII (1498-1515) "reconciled himself to God seven times a year by confession" and after each confession touched any one who had need of healing; Henry IV (1589-1610) touched and healed above 15,000 persons a year, according to the king's physician.

In England the first cures are accorded to Edward the Confessor who, it is thought, may have learned the practice from his French contemporaries. His method was his own. Brewer finds that "the king sent for a basin of water and dipping his fingers therein he frequently touched the parts affected every now and then forming with the tip of his finger the mark of the cross. The persons to be touched were selected by the king's surgeon and the number went on increasing every year." (p. 306). Edward I gave a gold or silver touch piece to those he cured; Elizabeth discontinued the use of the sign of the cross for fifteen years; Charles II is said to have touched 100,000 persons on his accession and more than 4,000 yearly and yet "in his reign more died of scrofula than in any other;" Anne touched some 200 in 1714 among whom was the distinguished Samuel Johnson; George I discontinued the practice in 1714 but the formula for "The office of touching" did not disappear from the "Common Prayer Book" until 1719 and pretenders continued to practise it as late as 1745. Black (9, p. 43) tells us that "In 1838, failing the royal touch, a few crowns and half-crowns bearing the effigy of Charles I were still used in the Shetland Islands as remedies for the evil."

Demonology

The subject of demonology is a study in itself, but its prominence in the miracles of healing in biblical times and since, make it necessary to note the evolution of the idea very briefly. There can be little doubt that the demonology of the early Hebrew period was a development from some simple form such as those we have found among the primitive races. From a conception of spirits devoid of moral character, good and bad spirits came to be differentiated. From the classes of good and evil spirits it was possible in time to derive the ideas of gods and devils which came much later in history, by a gradual subsumption of the powers of all the good spirits under one supreme spirit to get the idea of the God of monotheism, and by a similar process the idea of monodemonism with its arch fiend, the Devil or Satan, under whose dominion all the evil spirits of the universe live and act. Much of this evolution had been accomplished before Jewish times and it must be regarded as the necessary accompaniment of a rigorous monotheism. It is possible to note within the sacred record itself the evolution of the Satan idea in proportion as the evils of life were attributed less and less to God's judgments until finally they are laid almost wholly at the door of Satan and his emissaries.

The belief in the miraculous had not a little to do in this evolution. As we have shown, miraculous events were, as now, by no means confined to the chosen people, but in accounting for the wonders worked by others it was common to say "the gods of the heathen are devils," thus attributing their success to Satanic rather than divine power. Thus from very early times the misfortunes, diseases, and pains of the devout have been attributed to the agency of the Evil One, but at the beginning of the Christian era and from that time on a certain sort of mental disease known as "demonic possession" has been especially attributed to Satanic influence. It is this class that interests us most, not only because of its prominence in the miraculous cures of all the Christian centuries, but particularly because of the psychological phenomena it presents.

Demonic Possession in China. Perhaps no better illustration of what "demonic possession" is and always has been, is to be found than the cases of this phenomenon that may be observed in China to-day. In a recent book (65) Dr. Nevins describes in detail many cases which resemble in almost every particular those described in the New Testament, and maintains that they are inexplicable except as the actual influence of evil spirits.

He cites other modern observers in India, Japan, and Europe who have observed the same phenomena. Bishop Calloway and Brough Smith have seen many cases, in Natal and Australia respectively. (15, p. 52.)

Although the attendant phenomena vary widely there is a general similarity among all races. Some of those affected are wild and unmanageable, leap, and toss their arms about; others are more calm; some entirely unconscious while visibly possessed. The personal appearance is nearly always changed; in some there is palpitation, peculiar contractions, foaming. In nearly every case a second personality appears which personates a god, genii, the spirit of a deceased relative, or some animal. The first symptoms of the disease often occur in dreams and many times a fit of anger or grief is the first sign.

In most cases the "demon" comes in spite of the most violent opposition on the part of the person, but there are those who voluntarily invoke the spirit's presence in order through its power to make revelations by clairvoyance, clairaudience, or by a Chinese variety of planchette. Numerous other powers are claimed for the possessed such as speaking with tongues or in poetic form, singing, when otherwise they could not, and evidencing unusual physical strength. In some cases they are very destructive; they rob, steal, curse food and wells, put filth in eatables, and cause the money of the household to disappear; while if they are worshipped and propitiated all evil manifestations disappear and the family receive unusual favors instead.

The Chinese system of treating possession is in accord with their beliefs regarding it. A priest is called in who makes use of one or several of the following means: sacrifice, chants from the sacred books, prayer, offerings of money, begging or exorcism, magic spells, incantations; or by pinching, sticking with needles, by the use of peach or willow branches, charmed water and other similar means he drives out the spirit and confines it. If the priest-doctor fails they resort to spiritism and all its attendant phenomena of rappings, table tipping, planchette writing; and revelations of suitable charms or remedies to be used are as familiar to them as to our spirit mediums and our Indian medicine men. Confucianists, Taoists and Buddhists alike make use of such means. Of recent years Christian converts have been most successful in casting out these spirits by prayer and in the name of Jesus. In many cases a profession of faith is said to have been sufficient to relieve the sufferer of all further trouble. Space forbids the quoting of any of the numerous detailed descriptions of individual cases given by Nevins, interesting as they are as data for a study of comparative psychology and pathology. But one may say that almost every phase of the so-called "demon" possession of China, as it appears in the descriptions of Dr. Nevins, can be paralleled in any large institution for the insane. Any one who has had experience even in clinical demonstrations with such patients must at once admit the similarity. The periodic recurrence of attacks, the phenomena of double personality, sudden outbreaks of mania and as sudden returns to apparent normality, changes in voice, peculiar losses of control over the natural movements, or variations by habit or fixed ideas of functions naturally reflex, as breathing, etc., peculiar claims to and even real possession of unusual powers,—all these and many more of the manifestations we find repeated in every insane hospital. The differences are practically nil except those due to religious beliefs, superstitions, and race characteristics shown equally well in normal individuals. A more careful study of demonology in a comparative way by men of anthropological and psychological training will no doubt make a valuable contribution to our knowledge of the human mind and its variation under environment, education, and the influence of dominating religious and superstitious ideas, but there can be no question that it will be termed simple insanity.

Some Modern Miracles

St. Anne de Beaupré. For a modern illustration of the class of miracles performed during the middle ages one need not even cross the Atlantic. Not far from the city of Quebec there stands to-day a shrine at which we are told may be seen evidences of the miraculous almost any time. Long ago a few Breton sailors who believed themselves lost in a storm vowed to St. Anne that if she would deliver them they would build a chapel at the spot at which their boat touched land. They were saved, and true to their promise built a modest wooden chapel. In 1660 the chapel was rebuilt on another site and again in 1787. The foundation of the present structure was laid in 1872, and was completed in 1876 at a cost of nearly $200,000. About the year 1670 a relic of the saint was brought from the chapter of Carcassonne. We are told (*Cath. W. 36 p. 87*) that: "this relic is in fact a portion of the saint's finger and is vouched for by the cathedral chapter of Carcassonne, by Mgr. de Laval," etc. In 1877 another relic was brought from Rome, many valuable gifts were sent by patrons from various countries, some of whom belong to the royalty of France and Austria, and the shrine became so famous that on May 7, 1876, Pope Pius IX declared St. Anne patroness of the province of Quebec. Relics of other saints are also now to be found there and (*op. cit.*, p. 88) "the walls and sanctuary are fairly covered with crutches, hearts of gold and silver, and the like, each one telling of a belief in some cure obtained, or petition heard."

The popularity of this shrine does not seem to grow less but rather greater as the years pass. Pilgrimages are made at all times of the year and during the summer months the sick and those who have been cured come by trainloads to be cured or to render thanks for favors already received as the case may be, and the ancient prayer "*Sainte Anne, Mère de la Vierge-Marie, priez pour nous*" is on every tongue. The records for the year ending Oct., 1903, show that during the year some 168,000 pilgrims visited Beaupré, 1,250 of them coming from various parts of the United States. A study of several copies of the "*Annales de la Bonne Sainte Anne de Beaupré*" has furnished some information as to this interesting chapel as it is now. This monthly publication given up to recording the miraculous cures which take place at the shrine and elsewhere through the help of St. Anne, and to some other matters of a more general nature, offers to its subscribers participation in the merits of the prayers, masses, communions, mortifications, works, and occupations of the fathers Redemptoristes of St. Anne de Beaupré, keepers of the venerable Sanctuary.

In the "*Annales*" we find recorded each month from one hundred and fifty to two hundred or more cures besides numerous material favors such as employment, protection on journeys, conversions and other spiritual and temporal blessings. Some are recorded with some detail so that it is possible to form an idea of the general nature of the cures here effected as well as the means used to this end. Among the most common means used are prayers to St. Anne, at the shrine if possible but often at the suppliant's home; pilgrimages to the shrine; promises to publish the fact of cure in the "*Annales*"; promise to subscribe for the same; the use of medals of St. Anne as a charm; promises to break bad habits; application of an image of St. Anne or of holy oil brought from the shrine of Beaupré.

As an illustration of the nature of the cures effected and the unbounded faith of the petitioners we have selected a few of the more detailed cases from the January and February numbers of the "*Annales*" for 1904 which we present in translation from the French.

1. "Since January, 1902, I have suffered with a polypus of the nose and an ulcer of the jaw bone. I had submitted to two operations which have improved my condition without curing me. I continued to suffer. In July last I decided to put myself under the protection of the good St. Anne promising her to publish my cure. That moment there came about a sensible improvement. To-day I believe myself entirely cured and I thank my heavenly protectress for it."

2. "My little girl of two and one-half years had never walked. She was desperately weak. I commenced to pray to the good St.

Anne promising her to publish the fact in the "*Annales*" if she would make my child walk, and to subscribe for it the following year. Her grandmother made a pilgrimage to Beaupré and on her return she put a medallion of St. Anne around the neck of the child saying to her "a gift, you must walk". At the same instant the child, smiling, began to walk with a firm step. I am so happy that I do not know how to express my thanks."

3. "My husband was worn out with an affliction of mind which was about to impair his reason. The good St. Anne, invoked with confidence, has returned to him his serenity."

4. "For five years I have endured terrible suffering. Finally, I promised to make secretly the pilgrimage to Beaupré. The good St. Anne has given me enough strength to acquit myself of my promise. Since my return I have been very well. Thanks Oh Good Mother!"

5. "Last spring I had a grave sickness which brought me to the gates of death and which baffled the science of our regular physicians. I received the last rites and every one believed that I was going to die. My family commenced a "*neuvaine*" to the good St. Anne and to the blessed Gerard Majella. Heaven has yielded. I am cured of the disease which was about to take my life. It only remains that I make my acknowledgment and publish this favor according to the promise I had made."

6. "For ten years my wife suffered from a painful infirmity. I promised St. Anne that if she would cure her I would abstain from all intoxicating drinks. To-day I am happy and publicly thank the good St. Anne, first for the complete cure of my wife obtained in a few days after my promise, then for having given me grace to keep faithfully my temperance promise."

7. "I am happy to announce to you that at present I walk easily without the aid of my cane. After my return from the sanctuary of St. Anne de Beaupré during the summer, I have been able to go to mass for the first time in four years. I have made already four pilgrimages to the sanctuary of St. Anne and I propose to make a fifth as an act of gratitude and to carry my cane in order to lay it at the feet of our benevolent patroness. Praying you to join me in thanking the good St. Anne," etc.

In just how far the very primitive conceptions of disease are prevalent among the devotees who visit the shrine of St. Anne it would be difficult to say. But whether or not the primitive view of the cause is prevalent, the idea of supernatural means of cure certainly varies little from that prevalent in all ages and is evidence of the persistence of a method of procedure long after the idea which gave it origin has been more or less discredited.

Shakerism. A recent historical sketch of the society originally called the "Shaking Quakers" presents some facts which are of interest to our study. This society was formed in 1747 near Manchester, England. It's most famous member was Ann Lee (Stanley) more familiarly known as "Mother Ann." She was born at Manchester in 1736. Her biographers say that (89, p. 15) "as a child Ann was serious and thoughtful, subject to strong religious impressions and given to reverie and visions." Her child life was not that of the freedom and happiness normal to childhood, but, on the contrary, she lived a life apart and gave herself over to extreme and exaggerated religious experiences.

She early felt a strong aversion to marriage, but the customs of her time required her to marry against her wishes. She became a member of the Shaker society in 1758 when 22 years of age. She seemed to feel laid upon her the sins of the whole world, and repeatedly, through a period of nine years, there were times when she would spend her days at work and her nights at prayer until her body wasted away and perspiration as drops of blood pressed through her skin. It was at such extremities that her "visions" and "revelations" came. Some of these "revelations" have become a part of the doctrine of the Society. Those most fundamental and charasteristic are: 1. The duality of Deity, God both Father and Mother; one in essence . . . . two natures co-equal in Deity. 2. The secret of man's sin, the cause of his fall is the premature and self-indulgent use of the sexual function.

She felt that it was necessary that the Christ Spirit should come again in the form of woman to complete the revelation of God to humanity and she was the woman, anointed as she believed by the hand of Jesus himself, the second visible Head, in whom dwelt the Divine Mother. Very naturally, then, we should expect to hear that "about this humble, unlettered woman centered some of the most remarkable spiritual phenomena the world has seen . . . electric streams from Deity using her as a transmitter of spiritual force." (89, p. 15.)

According to their own claims Shakerism is the parent of modern spiritualism. At any rate spirit manifestations, prophetic utterances, phenomenal occurrences, revelations, and miraculous healings have been common throughout their history. Quoting again from "Mother Ann's" biographers (89, p. 353): "Numerous instances are certified to, by those who personally knew 'Mother Ann' and the Elders, where a word or a touch from her or from them had released from suffering, cured disease and restored health and strength . . . among them are cases of lameness, hip disease, consumption, acute dyspepsia and stomach troubles, exhaustion, obsession, dislocation of bones, glandular affections and many more."

There seems to be little or nothing here to distinguish the cures mentioned from those of other forms of faith cure. We have dwelt thus fully upon this movement only because it is somewhat less well known than many of the modern movements of this nature; because it bears some theological resemblances to Christian Science and is said to have furnished some of its doctrines; and because it furnishes one more illustration of the sort of religious expression that accompanies many of our modern mental healing cults.

Current Survivals of Old Superstitions

We have noted somewhat in detail the elements of the miraculous in primitive medicine and have seen how, as the race becomes more civilized the form in which these elements appear changes from extremely crude to more refined and intellectual types. With the beginning of the scientific age many of the superstitions of primitive life were shattered; but the human mind by no means disconcerted by this fact simply adjusted itself to the situation by the invention of new means for the exercise of its faculty of credulity, or continued to hold its former beliefs in spite of contradictions. While modern scientific thought has dislodged much of the old primitive thought, students of anthropology and folk-lore in recent years have revealed to us some startling facts regarding the persistence of such beliefs. Even in the midst of highly civilized communities, in our own country for example, are still to be found many of the superstitions of primitive peoples, not merely treasured up in memory as tales of a past generation, but used and believed in as sincerely and thoroughly as at any previous age.

The fact that medical science makes use of many plant and mineral substances, and surrounds their use with a measure of secrecy and mystery, has perhaps contributed to the retention of very much of the plant lore that has been accumulating for centuries. Such men as Folkard (36) and Dyer (27), and in America Mrs. Bergen (6), have gathered and put into accessible form the immense folk-lore of plants. The former mentions the names of no less than 1,400 plants that have been in many ages endowed with remarkable powers as charms, amulets, counter charms, means of divination, etc. As medicines the virtues of many have been extolled far beyond and often even in direct opposition to their known qualities. These same superstitions are all about us in a more or less definite form as is shown by Mrs. Bergen's (5) studies in the current folk-lore of America in which one finds such statements as the following: "In a town fifteen miles from Boston, the teacher has advised the pupils to wear nutmegs about the neck to prevent cold sores. Feb., 1892" (p. 100). The same remedy was employed for the two-year-old daughter of a New Hampshire legislator in 1893. In Maryland the belief has some currency that "biliousness may be cured by boring three holes in a carefully selected tree, and walking three times around it, saying 'Go away bilious!'" (5)

The same facts with regard to animal lore have been found by Jühling (51) in Germany and Mrs. Bergen (6) in our own country. The former mentions no less than 115 species and varieties of animals made use of in German folk-medicine alone and records the methods of employing their flesh, blood, etc., in actual practice at the present time. Many of these accounts present a picture dark indeed to one unaccustomed to noting the things upon which common humanity places its reliance in time of dire necessity. To give a single example of an animal remedy current in Labrador and in somewhat altered form in Indiana and Illinois. "Three lice taken in jelly nine days running, or put on buttered bread, will cure the yellow jaundice." (6, p. 69.) In the introduction of Jühling's work (51) it is stated that whereas in the accepted pharmacopœia of Germany at present only 13 remedies of animal origin are mentioned, even as lately as the time of the Thirty Years' War the "*Dresden Pharmacopæ*" contained 190. Newell says in the introduction to Mrs. Bergen's work (6, p. 6) "Michael Ettmüller, in his "*Opera Medica*" (1708), devoted nine folio pages to medical preparations from the human body and its excreta, of which those obtained from hair, nails, sweat and ear wax are the least filthy." When we find many absurd animal remedies used by the medical profession even as late as the middle of the eighteenth century it is not so surprising to find them used to-day among the unenlightened populace who as a rule must depend on household remedies. A comparative study of ancient and modern scientific medicine shows that as the science advances the number of efficacious remedies from both plant and animal world decreases rather than increases and that many remedies found the beginning of their use in superstition.

Perhaps as good an example as can be given of the survival in the midst of civilization of almost all features of primitive medicine is to be had in the old book "*The Long Hidden Friend*" (43) recently re-published in the *Journal of American Folk-lore* (1904). This strange book was published by John G. Hohmann in 1819 and has been ever since used among the Pennsylvania Germans of eastern Pennsylvania by what are known as "hex-doctors" (*hexe* a witch) whose business it is to overcome the spells of witches on man and beast. The book contains charms, incantations, prayers, and symbols and its extensive use throughout whole communities and almost counties is sufficient witness that the days of witchcraft are by no means gone even in our civilized communities.

Abbott finds that the evil eye is still feared in West England (1, p. 139); that in Macedonia "sorcery is expressly recognized by the Greek church as one of Satan's weapons, to be fought against by Christian means" (p. 143). As both the

Russian and Macedonian peasants considered disease the influence of evil spirits their treatment is "by purification with fire and water, and so the popular practice of physic is founded on a theory of fumigations, washings, and sprinklings attended by exorcisms of various kinds" (p. 223). Both Macedonians and Turks use as a remedy for diseases caused by "Spirits of the Air," for which they believe physicians can do nothing, verses from the Koran sewed up in leather (*Nuska*). "This prescription is either worn around the neck as a phylactery, or is burned and the patient is fumigated with the smoke thereof, or still better, it is washed in a bowl of water which is afterwards drunk by the patient" (p. 224). So, too, passages from the Bible written on the patient's neck or cheek, or water in which a leaf from the Bible has been soaked answer for exorcism.

The frequent appearance in systematic treatises on folk-medicine of prayers and spells Abbott considers strong evidence of the firm conviction that physical ailments are due to non-physical causes. Such a systematic treatise he found in MS. form among the Greeks of Macedonia. It is nameless, dateless, and incomplete, but appears to be the work of an eighteenth century scribe. It is in many ways comparable to "*The Long Hidden Friend*" of which we spoke above. Our authority (1, p. 240) says of it: "This extraordinary document —in tone and style so like parts of the Litany—affords a good illustration of the compromise by which Christianity has adopted pagan belief too firmly rooted to be swept away." What he says of this is true of all other traditional formulæ current in Christian countries. Christian names of Deity, saints, apostles, etc., are substituted for heathen deities and heroes, but otherwise the formulæ are identical. It is beyond the limits of this study to trace further the medical superstitions of this class current throughout the civilized world, and certainly what we have given is sufficiently extensive and representative to convince one of their prevalence. We have here simply an illustration of the fact that the evolution of man's ideas regarding the mysterious and unknown does not keep pace with his development along other lines. It is evidence also of the tenacity of those ideas which have to do with his physical and spiritual well being.

Present Day Cults. Christian Science, Dowieism, and the various schools of mental and "divine" healing have been so thoroughly and ably discussed by Goddard (38) and others that it seems unnecessary to add any detailed treatment of them to this already lengthy discussion. While the writer has made a careful study of these systems his aim so far as they are particularly concerned has been rather to confirm the conclusions of his predecessor in this field, and, giving him due credit, to use his conclusions in so far as they fit into a general and extensive study of the miraculous. Goddard has well shown the limits and possibilities of such systems; their relation to hypnotism; their dependence on the common principle of suggestion; and the irrelevancy of any of their distinctive features as the essential elements in their success, since all succeed about equally and in about the same fields. In other than a scientific age some of their accomplishments would be regarded as truly miraculous. They serve to attract that type of mind that does not easily give over a belief in the supernatural. They arose as a protest against a too materialistic conception of medicine and as a substitute for the cruder views supplanted by science. They mark a tendency of the day to evaluate more highly the mental element in man, a thing which psychology in a very different way is aiming to accomplish. Far from being discouraged by the fact that modern psychology has been unable as yet to make such peculiar movements impossible the psychologist should welcome their existence as furnishing a field for the study of religious, social and individual psychology unparalleled in the richness of its offers. This field is full of challenges to our science presenting as it does numerous unsolved problems in the nature of mind and its dependence upon physical, social, religious, moral, and spiritual influences.

The recent Emmanuel Movement is worthy of note in this connection because it has called renewed attention to the fact, and possibly the necessity, of a union of religious and spiritual with psychological and medical methods of treatment of disease. Our study of primitive medicine shows clearly a large religious element in all methods. The success of Christian Science, Dowieism, and kindred movements bears witness to the effectiveness of the appeal to the religious emotion made to centre about an organization and a place of meeting. The failure of some cults which make use of the religious appeal and the success of others with the same cases make plain the fact that not the same type of appeal is effective with all minds. As in primitive times, so now, while the underlying principle may be the same the outward manifestations vary greatly for different types of mind.

The Emmanuel Movement in its incipiency gave promise of being a happier and saner combination of medical, psychological, social, and spiritual methods than any that has yet appeared. In limiting its field of operation to that of functional nervous diseases it accepted the findings of science. It is to be hoped that maturer thought will rescue the movement from a too critical attitude toward medical science (91, p. 5, p. 52) to a legitimate and worthy endeavor to combine that which is best in science with that which is best in religion in an attempt to alleviate the sufferings of humanity.

The whole tendency of our study is to emphasize the view of modern psychology that the human personality is a unit—physical and mental—and that in the treatment of disease one may perhaps no more neglect the social, moral, and religious nature of the patient than he may the physical. Such a suggestion has tremendous significance for the psychologist, the physician, the social worker, and the religious leader.

To consider it from the point of view of the physician alone it seems to urge, in addition to the appeal which the profession has adready made to the psychologist and the social worker, the necessity of a closer association with the religious worker. The freakish, foolish, unnecessary views of many modern cults would gain less currency if a better understanding and a closer co-operation could be established between the workers in the fields suggested.

Psychological Interpretations

As in primitive, so in modern healing systems, the healer is the centre of much of our psychological interest. One must attempt to realize in oneself the consciousness of both healer and patient as well as to retain at the same time the critical attitude of the observer, a task which it is needless to say is not easy.

The healer has always been necessary. Even the skilled physician hesitates to attempt to heal himself. His brother physician may use the same means, but they will be more effectual than if self-administered. In this appears the social nature of man and his dependence upon his fellows as well as upon higher powers. From our study of the medicine-man it is evident that if he is not already to some degree pathological (which may in this case mean super-sensitive mentally) he makes use of various means which tend to bring about an approximation, at least, to a psychopathic condition in which, because of his training and belief, by hallucinations, dreams and the like he feels his miraculous endowment come upon him. In each case of treatment he undertakes, somewhat the same procedure must be gone through before he is ready to attempt the cure. Two reasons for such procedure seem evident. The subjective one that thereby the confidence of the medicine-man in his own abilities is increased, for in this semi-pathological condition there is an exhilarating mental activity accompanied by a lessened physical feeling of limitation which he interprets as the indwelling of divine prerogatives which he may use. In the second place his actions and appearance in the meantime become so changed that the patient believes him to be what he claims and gives his unbounded confidence—the chief element of success. The fact that his assumptions may be unfounded in no way affects his success so long as he secures the confidence of his patient and can make his suggestions effectual.

Making due allowance for changes which science, civilization, and higher types of religious thought have made, the successful healer, be he scientific physician, Christian Scientist, mental healer or what not, makes use of the same principles. Many medical men have been too slow to recognize the fact that the helpful suggestions radiating from a strong, hopeful, stimulating personality are many times a more important factor in the treatment of disease than are any of the medicines science has devised for their use. Consciously or unconsciously the successful healer of whatever school is a practical psychologist. A type of personality, with which by no means every one is endowed, is the prerequisite of success with functional nervous diseases especially.

A study of the personalities of the numerous healers who have succeeded notably under various systems reveals the following general facts. Such individuals are frequently found to have been in childhood nervous, excitable, hysterical, dreamers of dreams, seers of visions, subject to auditory and visual hallucinations. They are frequently persons who have had very precocious religious experiences; they have often little education; many are moody, self-centred, exclusive, solitary, meditative, and introspective. In adolescence and later life many have shown unusual interest in the phenomena of mesmerism, spiritualism, hypnotism, or other isms current in their day. In short they are personalities super-sensitive to mental influences both subjective and external, and for this reason they are able to influence strongly other minds possessing in the same or less degree the same characteristics. In short, all things combined to develop a personality that could make effectual suggestions favorable to mental and physical wellbeing. Barring the abnormalities and onesidedness of such persons, the physician should possess many of their mental traits.

There seems to us to be no doubt that the cures effected in primitive times by primitive means were in reality effected through the operation of the law of suggestion upon the mind of the patient. No fair-minded person acquainted with the facts can deny that cures could and must have been made by all the means enumerated in this study. Such results from such diverse and contradictory methods can only be made intelligible by an interpretation of them as the effect of mind, directed by suggestion, upon the body. Since spiritual agencies were for ages the sole discoverable cause of disease, the cures made under such a regimen could be only mind cures, barring the occasional almost accidental use of real hygenic measures. We believe we have shown that the same was essentially true in the earlier civilizations of Egypt, Chaldea, Babylon, and Greece. The healing miracles of the Bible, excepting perhaps the raising of the dead, may easily fall under the head of mental healing brought about by an intense religious faith in the healer. In so far as cures have been made by saints, their relics, shrines, and the like; by the kings of France and England; by wonder working springs and wells; by many medical cure-alls and by other non-medical means familiar to all, the credit falls chiefly if not wholly to the effect of the patient's own mind upon his body according to the law of suggestion. Modern healing cults can do no more, except as they recognize the necessity of combining the use of this age-long method with rational hygenic and medical means. If we leave out of account all hygenic and medical means, the method of the shaman in so far as its operation upon the mind of the patient is concerned is as little different from that of the "divine" healer of to-day as that of the latter is from the method of the Christian Science healer.

The decided limitations to the cure of disease by suggestion alone seems to be no longer in doubt. On the other hand the medical science that is based solely upon the physiological action of medicines, if there ever was such a medical science, seems to be almost equally limited, as has been shown by writers both within and without the profession (85) and by the history of the science itself.

The leaders of thought and practice in the medical profession have long known and acknowledged the limits of their science. They have not been ignorant of the fact of the large mental element in the cause and cure of disease. The placing of hypnotic and suggestive therapeutics upon a scientific basis has been almost if not wholly the work of practical physicians. It is not by accident that the foremost authority in psychiatry—Kraepelin—combines in one person the best of training in psychological theory with a thorough knowledge of and practice in medicine, or that the French investigators who seem to be well on the way to the discovery of the real relation of the mind to the body combine in rare proportions these same qualifications. So, too, the minister who would follow in the footsteps of Dr. Worcester must have, as he has, a thorough grounding in psychology as well as practical theological and religious training. No merely formal theology, no smattering of popularized psychology will suffice to make him successful or keep him from fatal errors.

There can be little doubt that the failure of the rank and file of the medical profession to live fully up to their opportunities in the field of psychotherapeutics is in part accountable for the rise and spread of a type of healing cult which reflects little credit upon the intelligence of our people, but which has been a natural result of a new evaluation of the mental element in man brought about largely by the popularization of psychological thought. The church has been equally negligent of her duty as a moral and spiritual force working in harmony and in co-operation with the physician for all-round health. We desire to make the strongest possible appeal for a closer and more sympathetic relation between the sciences of medicine and psychology, and the practical, social, and spiritual forces of the church, each deferring to the other in its distinctive field, but all working together with mutual understanding to the common end of physical, moral, and spiritual health. Only in some such utopian scheme can superstition be uprooted, religion purified and restored to its own, the science of medicine kept from becoming too materialistic and so powerless in some cases and to a degree crippled in all; only by some such plan can the science of psychology be rescued from the danger of too much theorizing and put to work as the practical handmaid of all the other sciences of man.

Conclusions

At the risk of apparent dogmatism it seems necessary to state in a few brief sentences some of the conclusions which we believe this study warrants. In doing so some repetition will no doubt occur and some conclusions may be stated in positive form for the first time, it having been our purpose in part to present sufficient illustrations to make clear the conclusions now stated.

We believe that there have been and are miracle workers in every age, under the most diverse conditions of civilization and savagery, among all races and under every form of religion.

Almost all miracle workers and believers in miracles have attached religious significance to the wonders they have been able to perform because, being unable to explain the phenomena, they have attributed them to the activity of such supernatural agencies as they believed to exist.

By such wonders men have always been led to a contemplation of the mysterious and through such contemplation have been led to formulate many of their religious notions.

The rapid advance of physical sciences in recent years has rendered less and less prevalent the crude and often extremely irrational explanations of miracles common to primitive and even to civilized man. The advance of scientific psychology and scientific psychotherapy has done still more to rationalize our views by demonstrating the possibility of explaining supposedly supernatural happenings on the basis of laws of physical and mental activity.

In spite of this fact, and because so much is still unknown especially in the field of mind, the belief in the supernatural still persists. This we believe to be due to the age-long tendency of the human mind to so react and to the inherent limitation and necessary finiteness of the human mind. It is possible that at least for ages yet religion without the supernatural will be for the masses an impossibility.

The field of the miraculous will always be a most hopeful and interesting field for science for it is always to be found just along the periphery of the field of science. New problems are always presented as old ones are solved.

Among the wonders of all peoples, miracles of healing have always held a pre-eminent place because of the close personal interest in the results and because problems in this field are more difficult and baffling than in any other.

It is now certain that those cures which have always seemed miraculous are the cures effected in large measure by the influence of the mind on the body. They have seemed miraculous because the laws under which they were effected were not understood.

It is now a well established fact that the cures of this nature are brought about in accordance with the law that the mind tends to translate into physical reaction any suggestion or idea which can be actively aroused and kept at the focus of attention. That this may be done the idea must seem reasonable and possible, and inhibiting and opposing ideas must be banished. In short, the mind must be made to give the idea free play.

It seems axiomatic that any system will succeed which can make this law operative for suggestions of physical, mental, and moral well-being. The ways in which this may be done vary as individuals always have and always will vary. In spite of the fact that all systems depend upon suggestion in some form as the means of making cures, it by no means follows that any one uniform or scientific system will be able to make suggestion effectual in all cases. We shall probably continue to have various mental healing cults so long as men think differently in the fields of philosophy or religion.

We may safely look for saner and more scientific views on the subject of mental healing but present tendencies indicate that we need not expect that the religious elements of faith in the efficacy of prayer, belief in the supernatural, and other deeply rooted instincts can or will be neglected as a means of making suggestions effectual. The appeal is not wholly nor chiefly to the mind acting in its most rational way but to those deeper, more fundamental activities, rooted in instincts, habits, hereditary tendencies and the like, which are more far reaching in effect than anything in the rational activity of the individual mind.

We are forcibly impressed with the necessity of further careful and accurate study of the operation of the law of suggestion. We need far more data upon the conditions favoring and hindering the operation of the law. We have had much theorizing but as yet too little scientific study and experimentation in this promising field.

We are convinced that churchmen, physicians, and psychologists have all conceived of their functions and relations too narrowly and that they need to co-operate for mutual understanding and profit.

Finally, we believe that rightly conceived the laws of mental healing have a significance as hygienic measures equal to their usefulness as means of cure and that they are therefore of moral, spiritual, and pedagogical value as a means of establishing correct habits and activities in these fields.

Bibliograhy

1. Abbott, G. F. Macedonian Folk-lore. Cambridge, Eng., 1903. pp. 366.
2. Bartels, Max. Die Medicin der Naturvölker. Leipzig, 1893. pp. 361.
3. Batchelor, John. The Ainu and their Folk-lore. London, 1901. pp. 603.
4. Berdoe, Edward. Origin and Growth of the Healing Art. London, 1893. pp. 509.
5. Bergen, Mrs. F. D. Current Superstitions. Boston, 1896. pp. 161.
6. —— ——. Animal and Plant Lore. Boston, 1899. pp. 180.
7. Bernheim, H. Suggestive Therapeutics. N. Y., 1889. pp. 416.
8. Binet, Alfred. Alterations of Personality. N. Y., 1896. pp. 356.
9. Black, W. G. Folk Medicine. London, 1883. pp. 220.
10. Blumner, H. The Home Life of the Ancient Greeks. London, 1895. pp. 548.
10a. Bogoras, M. W. Idées Religieuses des Tchouktchis. Bulletins de la Société d'Anthropologie de Paris. V. Série, 1904. Tome V. pp. 341-354.
10b. Boas, Franz. The Mind of Primitive Man. Journal of American Folk-Lore, Vol. XIV, 1901, pp. 1-11.
11. Bohme, Kate A. Mental Healing Made Plain. Washington, 1902. pp. 104.
12. Boston Medical and Surgical Journal. Vol. 146. p. 108 *et seq.*
13. Bourke, J. G. Scatologic Rites of All Nations. Washington, 1891. pp. 496.
14. Bramwell, J. Milne. Hypnotism, Its History, Practice and Theory. London, 1903. pp. 478. (Bibliography.)
15. Brinton, Daniel G. Religions of Primitive Peoples. N. Y., 1897. pp. 264.
16. Brewer, E. Cobham. A Dictionary of Miracles. London, 1901. pp. 582.
17. Bruzon, P. La Médecine et les Religions. Paris, 1904.

18. Bureau of Ethnology Reports and Bulletins. Washington.
19. CARUS, PAUL. The History of the Devil. Chicago, 1900. pp. 486.
20. CLARK, GALEN. Indians of the Yosemite. Yosemite Valley, 1904. pp. 110.
20a. CLODD, EDWARD. Animism. Chicago, 1906. pp. 100.
21. COCKAYNE, THOMAS O. Saxon Leachdoms, Wort-cunning and Starcraft. London, 1864-66. 3 vol.
22. CODRINGTON, R. H. The Melanesians. Studies in their Anthropology and Folk-lore. Oxford, 1891. pp. 419.
23. CONWAY, M. D. Demonology and Devil Lore. New York, 1879. 2 vol.
24. DEARBORN, GEORGE V. N. Psychology and the Medical School. 1901. Science, N. S. Vol. xiv, pp. 129-136.
25. DRESSER, HORATIO W. Methods and Problems of Spiritual Healing. New York, 1904. pp. 101.
26. DUPOY, EDMOND. Le Moyen Age Médical. Paris, 1895. pp. 372.
27. DYER, T. F. T. The Folk-lore of Plants. N. Y., 1889. pp. 328.
28. EDDY, MARY B. G. Science and Health. Boston, 1891. pp. 651.
29. —— ——. Miscellaneous Writings, 1883-1896. Cambridge, Mass. pp. 471.
30. EDES, ROBERT T. Mind cures from the Standpoint of the General Practitioner. Mass. Med. Soc., 1904, pp. 22.
31. —— ——. The New England Invalid. (The Shattuck Lecture.) Boston, 1895.
32. ELLIS, HAVELOCK. Man and Woman. New York, 1904. pp. 508.
33. Encyclopædia Biblica. New York, 1899.
34. ERDMAN, ADOLF. Life in Ancient Egypt. London, 1894. pp. 570.
35. FLETCHER, ALICE. Notes on Certain Beliefs Concerning Will Power among the Siouan Tribes. Science N. S., Vol. V, p. 331-334.
36. FOLKARD, RICHARD, JR. Plant Lore, Legends and Lyrics. London, 1884. pp. 610.
37. FRAZER, J. G. The Golden Bough. New York, 1901. 3 Vol., pp. 1428.
38. GODDARD, H. H. The Effect of Mind on Body as Evidenced by Faith Cures. *Am. Jour. Psy.*, Vol. X, pp. 431-502.
38a. HADDON, ALFRED C. Magic and Fetishism. Chicago, 1906. pp. 99.
39. HALL, G. STANLEY. Adolescence. N. Y., 1904. 2 Vol., pp. 589 and 784.
40. HASTINGS, JAMES. A Dictionary of the Bible.
41. HECKER, J. F. C. Epidemics of the Middle Ages. London, 1846. pp. 380.
42. HENNIG, RICHARD. Wunder und Wissenschaft. Hamburg, 1904. pp. 247.
43. HOHMANN, JOHANN G. The Long Hidden Friend. Jour. Am. Folk-lore, 1904.
44. HUDSON, THOS. J. The Law of Psychic Phenomena. Chicago, 1893. pp. 409.
45. JACOB, A. An Essay on the Influence of the Imagination and Passion in the Production and Cure of Disease. Dublin, 1823.
46. JAMES, WILLIAM. The Will to Believe and Other Essays in Popular Philosophy. New York, 1898. pp. 332.
47. —— ——. The Varieties of Religious Experience. N. Y., 1902. pp. 527.
48. JASTROW, JOSEPH. Fact and Fable in Psychology. Boston, 1900. pp. 375.

49. Jewish Encyclopædia. New York, 1901. 12 Vol.
50. Journal of the Anthropological Institute of Great Britain and Ireland. London.
50a. Journal of American Folk-lore. Boston.
51. JÜHLING, JOHANNES. Die Tiere in der deutschen Volksmedizin alter und neuer Zeit. Mittweida, 1900. pp. 355.
52. LANG, ANDREW. The Making of Religion. N. Y., 1900. pp. 355.
53. —— ——. Magic and Religion. N. Y., 1901. pp. 316.
54. LEHMANN, ALFRED. Aberglaube und Zauberei. Stuttgart, 1898. pp. 556.
55. LENORMANT, FRANCOIS. Chaldean Magic.
56. MASON, R. O. Telepathy and the Subliminal Self. N. Y., 1897. pp. 343.
57. MASPERO. The Dawn of Civilization in Egypt and Chaldea. London, 1901. pp. 800.
58. MAUDSLEY, HENRY. The Pathology of Mind. London, 1879. pp. 580.
59. MOLL, ALBERT. Hypnotism. London, 1891. pp. 410.
60. MOONEY, JAMES. The Medical Mythology of Ireland. Philadelphia, 1887. Am. Phil. Soc., pp. 135-166.
61. MÜLLER, F. MAX (Ed.). The Sacred Books of the East. Oxford.
62. MÜNSTERBERG, HUGO. Psychology and Life. Boston, 1899. pp. 282.
63. MYERS, F. W. H. Human Personality and its Survival After Death. London, 1903. pp. 660.
64. NASSAU, ROBERT H. Fetishism in West Africa. N. Y., 1904. pp. 389.
65. NEVINS, JOHN L. Demon Possession in China. N. Y., 1895. pp. 482.
66. PEABODY, FREDERICK W. Complete Exposure of Eddyism or Christian Science. Boston, 1904. pp. 53.
67. PETERS, HERMAN. Aus pharmazeutischer Vorzeit in Bild und Wort. Berlin, 1886. pp. 224.
68. PETTIGREW, THOS. J. Superstitions Connected with the History and Practice of Medicine and Surgery. London, 1844. pp. 213.
69. PODMORE, FRANK. Modern Spiritualism. 2 Vol. London, 1902. pp. 374.
70. RANDOLPH, CHARLES B. The Mandragora of the Ancients in Folk-lore and Medicine. Proc. of Am. Acc. of Arts and Sci. Jan., 1905, pp. 488-531.
71. RILEY, I. WOODBRIDGE. Personal sources of Christian Science. Psy. Rev. Vol. X, pp. 593-632.
72. ROTH, WALTER E. North Queensland Ethnography. Bulletin No. 5. Brisbane, Jan., 1903. pp. 42.
73. SAYCE, A. H. The Hibbert Lectures; Lectures on the Origin and Growth of Religion. Oxford, 1898. pp. 558.
74. SCHOFIELD, A. T. Unconscious Therapeutics. Philadelphia, 1904. pp. 317.
75. SIDIS, BORIS. The Psychology of Suggestion. N. Y., 1898. pp. 286.
76. SKEAT, WALTER W. Malay Magic. London, 1900. pp. 685.
77. SNELL, DR. OTTO. Hexenprozesse und Geistesstörung. München, 1901. pp. 130.
78. SPENCER and GILLEN. The Native Tribes of Central Australia. London, 1899. pp. 671.
79. STARBUCK, E. D. The Psychology of Religion. N. Y., 1899. pp. 422.
80. STOLL, OTTO. Suggestion und Hypnotismus in der Völkerpsychologie. Leipzig, 1904. pp. 738.

81. THOMPSON, R. CAMPBELL. Semitic Text and Translation Series. London, 1904. Vol. XIV-XV.
82. TUCKEY, C. LOYD. Psycho-Therapeutics. London, 1890. pp. 189.
83. TUKE, D. H. Illustrations of the Influence of the Mind on the Body in Health and Disease. London, 1884. 2 Vol. 2d. Edit.
84. —— ——. A Dictionary of Psychological Medicine. London, 1892. 2 Vol. pp. 1477.
85. VERESAEFF, V. The Confessions of a Physician. London, 1904. pp. 289.
86. WETTERSTRAND, OTTO G. Hypnotism and its Applications to Practical Medicine. N. Y., 1897. pp. 166.
87. WINBIGLER, C. F. Christian Science and Kindred Superstitions. N. Y., 1901. pp. 168.
88. WINTERNITZ, M. Geschichte der indischen Litteratur. Leipzig, 1904. pp. 258.
89. WHITE and TAYLOR. Shakerism. Columbus, 1904. pp. 417.
90. WHITE, ANDREW D. History of the Warfare of Science with Theology in Christendom. New York, 1900. 2 Vol. pp. 415 and 474.
91. WORCESTER, E., S. MCCOMB, and I. H. CORIAT, M. D. Religion and Medicine. New York, 1908. pp. 427.

THE ABANDONMENT OF SENSATIONALISM IN PSYCHOLOGY[1]

By MARY WHITON CALKINS, Wellesley College.

The basal purpose of this paper is to call attention to the advance made by present-day psychology on the sensationalism which persisted into the writings of the last decade. In 1893 Wundt, for example, was still designating the affection (*Gefühlston*, pleasantness or unpleasantness) as an 'attribute of sensation'.[2] To-day almost all psychologists agree in recognizing at least two classes of not further analyzable elements of consciousness—on the one hand, the sensational elements, on the other hand, the affective elements, pleasantness and unpleasantness. Stumpf's view, that sense-pleasantness and sense-unpleasantness are sensational, is the only notable exception to this agreement;[3] and recent criticism—that of Titchener,[4] Johnston,[5] and Meyer[6]—has so successfully assailed the doctrine that it need not here be considered.

But the effort to correct the crude and misleading simplicity of sensationalism has not stopped short at the admission of a new class of elements including merely pleasantness and unpleasantness. Explicitly or implicitly many psychologists now admit the occurrence of still other elemental kinds of consciousness. With the two important and distinct forms of this advance on sensationalism this paper specifically deals.

I

The first of these contemporary movements does not add to the number of classes of conscious elements, but it enlarges one of the classes already recognized. This is the theory of Wundt who includes in the class of the affections, or 'feelings', four elements (or rather classes of elements) co-ordinate with

---

[1] Read, by title, at the meeting of the American Psychological Association in Baltimore, December, 1908.

[2] "Physiologische Psychologie," 4te Aufl., 1893, Bd. I, 555.

[3] *Zeitschrift f. Phys. u. Psych.*, 1906, XLIV, 1 ff.; *Bericht über den II. Kongress f. experimentelle Psychologie*, 1907, 209 ff. It should be noted that Stumpf expressly restricts himself to the consideration of the sense-feelings (*die sinnlichen Gefühle*).

[4] "Lectures on Feeling and Attention," 1908, pp. 82 ff.

[5] Psychological Bulletin, V, 65 ff., 1908.

[6] Psychological Review, XV, 205 ff., 1908.

pleasantness and unpleasantness. These four are: tension and relaxation (Spannung-Lösung), excitement and quiescence (Erregung-Beruhigung).[1] Relaxation is opposed to tension and quiescence to excitement as pleasantness is opposed to unpleasantness, so that we have three pairs of opposites or, as Wundt calls them, 'dimensions' of feeling. The arguments for this view may be summarized as follows:

(a) The Wundtians point out that emotional states differ, according to common consent, not merely as pleasant and unpleasant, but also as exciting or quieting, straining or relaxing. Both melancholy and terror, for example, are unpleasant emotions, yet the first is quieting, or depressing, while the second is as clearly exciting.[2]

(b) This purely introspective argument is verified and supplemented by experiment. Alechsieff, whose experimental study is one of the best and most recent of those put forth by members of the Wundtian school,[3] stimulated his subjects in such wise as presumably to bring about emotional experiences, and recorded both pulse and breathing, and introspection. The introspective records first (1) clearly indicated the occurrence of straining and relaxing, exciting and depressing emotions; next (2), sometimes asserted the occurrence, in emotional experience, of elemental consciousness other than sensations, pleasantness and unpleasantness; finally (3) showed (in opposition to the results—later to be described—of Hayes) that either pleasantness or unpleasantness may occur in combination with any one of the four other 'feelings'. In other words, the records indicated that in pleasurable emotion subjects were sometimes in a state of tension, but sometimes relaxed, sometimes excited and sometimes depressed; and that in unpleasant emotion subjects were now relaxed, now strained, and now excited, again depressed. The objective results of these experiments are summarized by Alechsieff in the following scheme adapted from Wundt:[4]

---

[1] "Physiologische Psychologie", 5te Ausg., 1902, II, pp. 284 ff. (*Cf.* "*Grundriss*", 1896, 1905; "Vorlesungen über die Menschen u. Thier Seele", 1897; "Gefühl und Bewusstseinsanlage", 1903).

'Quiescence' is Royce's synonym for 'Beruhigung'. Wundt has two equivalents for the term, namely '*Depression*' and '*Hemmung*'. For telling comment on the really divergent signification of the three terms, *cf.* Titchener, *op. cit.*, pp. 145 ff.

[2] *Cf.* Wundt, already cited; Alechsieff cited in the next note; J. Royce, "Outlines of Psychology", 1903, pp. 176 ff.; O. Vogt, *Zeitschr*, f. *Hypnotismus*, VIII, p. 212, 1899 (and other writers cited by Alechsieff and Titchener, *op. cit.*).

[3] "Die Grundformen der Gefühle", *Psychologische Studien*, 1907, III, pp. 156 ff.

[4] *Cf.* "Grundriss," 1904, § 7, 104.

		Pulse			
Retarded			Quickened		
Strengthened		Weakened	Strengthened		Weakened
Pleasure	Tension	Quiescence	Excitement	Relaxation	Unpleasantness
Quickened		Retarded	Quickened		Retarded
Weakened			Strengthened		
		Breathing			

The Wundtian conclusion from both sorts of evidence is the following: Experiences which are thus shown to be, on the one hand, introspectively elemental, distinct, and independently variable and, on the other hand, accompanied by clearly differentiated yet co-ordinated circulatory and respiratory phenomena are elements of consciousness belonging in a class together. Therefore tension-relaxation and excitement-quiesence form, with pleasantness-unpleasantness, the enlarged class of the 'feelings (*Gefühle*)'.

This form of advance upon the old sensationalism has, however, found little favor outside the rather narrow group of Wundt's fellow-workers and students. No one questions the occurrence of straining and relaxing, exciting and quieting emotions; but these distinctions, it is claimed, are incorrectly referred to the presence of elemental 'feelings'—strain, relaxation and the rest. The alleged elemental experiences are analyzable, rather, into non-affective elements. Against the Wundtian arguments from experiment it is urged by the critics that the outcome of experiment is very far from conclusive in Wundt's favor. Even experiments undertaken from the same theoretical standpoint as Alechsieff's issue in results of very conflicting nature—results which he himself can explain only by a supposition which is really a criticism of the experimental method, the supposition, namely, that the stimulus was too complex to rouse any discoverably elemental experiences.[1]

The experiments (earlier than Alechsieff's) carried on in the Cornell laboratory to test Wundt's theory[2] seem also to point

[1] *Cf.* Alechsieff, *op. cit.*, p. 175[2] *et al.*, for admission of the opposing results of experimental investigations of the breathing. For Alechsieff's attempts at explanation, *cf. op. cit.* p. 207; also pp. 180-200 where Alechsieff holds that psychologists have often confused with tension, with excitement, and even with attention, what is really a complex *Tätigkeitsgefühl* which includes both tension and excitement.

[2] "A Study of the Affective Qualities, I. The Tridimensional Theory of Feeling", *Amer. Jour. Psy.*, XVII, pp. 358 ff., 1906. For other criticism on the Wundtian theory, *cf.* Orth., "Gefühl und Bewusstseinsanlage, 1903; M. Kelchner, *Archiv*, V, pp. 107 ff.

to the opposite conclusion. In these experiments Hayes presents to his subjects "series of stimuli—tones or colors or rhythms— . . . two at a time. . . . Every member of the series is paired with every other member. The observer has to decide which of the two . . . is the more pleasant, the more unpleasant, the more exciting, the more depressing, and so on." The results are the following: (1) Tension was "described throughout in kinæsthetic terms." (2) Only judgments of pleasantness, unpleasantness, and tension were easily made. (3) (In opposition to Alechsieff's later results) the decisions 'exciting' and 'relaxing' agree with the decisions 'pleasant'; whereas the decisions 'quiescent' and 'straining' agreed with the decision 'unpleasant'. In other words, the alleged elements did not vary independently, and Titchener concludes that "since the pleasant-unpleasant dimension is not in dispute, we have a strong indication that that alone is fundamental."[1]

The impartial student of these counter investigations must admit that no decisive result, on either side, has as yet been experimentally established. Alechsieff challenges the presupposition of the Cornell experimenters that "it is impossible, through one and the same stimulus, to excite two different feeling-qualities";[2] and Titchener admits that the "argument upon which the experiments rest is not demonstrably valid;"[3] but, on his side, Alechsieff by his own confession has to twist and pull the results of Lehmann, Brahn, and others in order to fit them into his tridimensional theory. The failure of experiment throws us back on introspection; and on this basis, again, in the opinion of the writer, neither the Wundtians nor their critics wholly make their point. On the one hand the critics are justified in the assertion that elemental affective elements—or feeling-elements strictly co-ordinate with pleasantness and unpleasantness—are not discovered in our emotional experience. Yet, on the other hand, the opponents of the theory, in their attempts to reduce all four of the new 'feelings' to organic sensations, ignore introspective testimony which has at least the face-value of their own. When Alechsieff's subjects protest[4] that they "feel the strain-sensations", but that they experience in addition to the strain sensations (and to the pleasantness or unpleasantness) a residuum which reduces neither to sensation nor to affection, there is no valid reason to discredit their testimony. But their 'residuum' will turn out, in the view of the writer, to be either identical with 'clearness' (the attention-element), or to belong to a third class of elemental experiences—a class co-ordinate with sensations and affections—that of relational experiences.

---

[1] *Op. cit.*, pp. 161^2-164^2 ff.
[2] *Op. cit.*, p. 208. [3] *Op. cit.*, p. 167^3. [4] *Op. cit.*, p. 202.

The radical modification of Wundt's theory embodied in the last sentence is submitted for the consideration of his critics. Stated in more detail it involves the following teaching:

(1). 'Tension' is reducible to attention, or clearness, plus the organic sensations characteristic of attention.

The significance of this assertion varies, of course, according to one's doctrine of attention. If one follow Professor Titchener in the teaching that attention, or clearness, is itself sensational—in other words, that one may attend to sensations only—then we have here no enlargement of the traditional list of elements. But, in the view of the writer, Titchener's teaching cannot be maintained. He himself is at pains to admit that it is opposed to the view of several psychologists—he names Sully, Meumann, Saxinger[1]—who hold that unsensational contents may be attended to. The doctrine seems, indeed, inconsistent with Titchener's own doctrine: that introspection consists simply in attention to phenomena. For, as Titchener unequivocally teaches, the affections are known by introspection and it follows that they must be 'clear' or 'attended to'. The denial of the sensational character of tension (attention) leaves us with the problem of the nature of it still upon our hands. The conclusion of the writer—which there is not here time to defend in detail—is that attention is an elemental consciousness co-ordinate in various ways both with pleasantness-unpleasantness (the affections) and with the elemental experience of realness, and thus belonging with these to a larger class of 'attributive elements of consciousness'.[2] This is a doctrine agreeing with Wundt's both in that it admits the unsensational and elemental character of tension (attention) and in that it co-ordinates tension with pleasantness-unpleasantness, but agreeing with the teaching of Wundt's critics in refusing to call tension 'affection' or 'feeling'.

(2). Relaxation, in the second place, probably is merely the absence of strain. Alechsieff himself seems virtually to imply this.[3] So far as relaxation is a positive experience it seems to reduce, as Titchener teaches, to organic sensations.

(3) and (4). The case is different with excitement and quiescence (*Erregung-Beruhigung*). These are complex, not elemental, experiences; and the distinguishing feature of them is neither the organic sensations—though these are present and significant—nor any new kind of feeling, but rather the vivid consciousness of doubtful future or of irrevocable past. This analysis is corroborated by a study of the introspective records

[1] *Op. cit.*, pp. 74, 76, 334.

[2] For further discussion, *cf.* the writer's "An Introduction to Psychology", chapter IX (in the second edition, 1905).

[3] *Cf. op. cit.*, p. 222[1]. Titchener has a similar criticism, p. 145[2].

of those who treat excitement and tranquillity as forms of affection. A striking confirmation of it is found in Royce's discussion of 'quiescence and restlessness': "We tend to regard with restlessness whatever tendency involves our interest in immediately future changes. The emotions of . . . fear, of hope, of suspense are accordingly especially colored by restless feelings. On the other hand, the feelings of quiescence predominate when . . . we regard the past."[1]

The analysis of the Wundtian theory has led, accordingly, to the conclusion that Wundt is unjustified in his teaching of the two new pairs of feelings co-ordinate with each other and with pleasantness-unpleasantness. Only one of the four, namely strain, is either elemental or—in any sense—parallel with pleasantness-unpleasantness. Relaxation, a second of these alleged elements, seems to reduce to bare sensation, where the name does not indicate mere absence of strain. The other two, excitement and quiescence, are, indeed, as the Wundtians insist, unsensational; but the unsensational elements which distinguish them are not affective elements (or feelings), but rather relational elements. A discussion of this third group of conscious elements, and of the theories about them, will form the final section of this paper.

II

The doctrine of elements of consciousness which are neither sensational nor in any sense co-ordinate with the affections or feelings is upheld by psychologists of the most diverse schools. Herbert Spencer was the first to name and to discuss them,[2] but his teaching attracted little notice and thirty years passed before Ebbinghaus rediscovered the *Gestaltqualitäten*,[3] and James wrote of the 'transitive feelings' of 'and', 'but', and 'if'.[4] Today two groups, or schools, and several individuals among Continental psychologists and a considerable number of English-speaking psychologists more or less unequivocally teach the occurrence of elements of consciousness neither sensational nor affective. There is, first, the school of Meinong[5] which discusses relational elements under the names '*fundirte Inhalte*' and '*Gegenstände höherer Ordnung*'.[6] The second of the Con-

[1] "Outlines of Psychology", p. 180^2.

[2] "The Principles of Psychology", first edition (1855), § 81, p. 285.

[3] *Vierteljahrschr. für wissenschaftliche Philos.*, XIV, p. 249, 1890.

[4] "Principles of Psychology", I, p. 247, Note.

[5] A. Meinong: *Zeitschrift*, II, p. 247, 1891; and XXI, pp. 182 ff.; and "Ueber Annahmen", 1902.

[6] A. Höfler ("Psychologie"), and S. Witasek ("Grundlinien der Psychologie", 1907), have incorporated Meinong's doctrine in systematic treatises.

tinental schools is that of Külpe and the students and workers in the Würzburg Institut, Watt,[1] Ach,[2] Messer,[3] Bühler,[4] and others. Individual upholders of the theory are Binet,[5] Stumpf with his doctrine of *Gebilde* and *Verhältnisse*,[6] Cornelius,[7] and, finally, in spite of great divergence in terminology, Münsterberg and Ebbinghaus.[8]

Of writers in English, Stout,[9] R. S. Woodworth,[10] and the writer of this paper[11] have most explicitly taught the occurrence of these elements of consciousness, neither sensational nor affective, which are especially characteristic of what is called thought. Judd, also, describes concept and judgment in terms of relation;[12] and Angell, in spite of his denial of literally imageless thought,[13] seems to indicate by his term 'meaning'[14] a relational experience.[15]

It thus appears that the introspection of a score of psychol-

---

[1] *Archiv f. die gesammte Psychologie*, IV, 288 ff., 1905.

[2] "Ueber die Willenstätigkeit und das Denken" (based on experiments carried on in Würzburg and in Göttingen), Göttingen, 1905.

[3] *Archiv*, VIII, 1 ff., 1906.

[4] *Archiv*, IX, 297 ff., 1907; XII, 9 ff., 1908.

[5] "L'étude experimentelle de l'intelligence," Paris, 1903.

[6] "Erscheinungen und Psychische Funktionen", Königl. Akad. d. Wissenschaften, Berlin, 1907, pp. 7 ff., 29 ff.

[7] "Psychologie als Erfahrungswissenschaft", pp. 70, 164 *et al.; cf.* also *Zeitschrift*, XXII, pp. 101 ff. (1899), where Cornelius develops a teaching of G. E. Müller.

[8] Ebbinghaus ("Grundzüge", I, pp. 410 ff.) recognizes as elements only sensations and affections, while Münsterberg ("Grundzüge", I, pp. 290 ff.) admits sensations only. Yet the first includes under the head of 'general attributes of sensation' and the second groups in the class of value-qualities the identical part-experiences which are here considered as relational elements.

[9] "Analytic Psychology", I, pp. 66; 78-96; II, p. 42.

[10] "Imageless Thought", *Journal of Philos., Psychol. and Scientific Method*, III, pp. 701 ff., 1906.

"The Cause of a Voluntary Movement" in *Studies in Philosophy and Psychology by Students of C. E. Garman*, pp. 351 ff.;

"Non-Sensorial Components of Sense-Perception" *Journal of Philosophy*, etc., IV, pp. 164 ff., 1907.

[11] "An Introduction to Psychology", 1901, chapter X (especially in the second edition, 1905); "Der doppelte Standpunkt in der Psychologie, 1905, pp. 25 ff.

[12] "Psychology, General Introduction", 1907, pp. 286 ff.; *cf.* p. 72.

[13] *Philosophical Review*, 1897, pp. 646-657.

[14] "Psychology", 1904, p. 213 *et al.; cf.* p, 267[3].

[15] It is to be regretted that enthusiastic upholders of the relational-element doctrine have remained so comparatively oblivious of each other. I find only one writer, later than James, who refers to Spencer's advocacy of the doctrine. The writers of the Meinong school seldom if ever mention any English-speaking upholders of the theory. Dr. Montague alludes, in the James *Festschrift*, to "Professor Woodworth's discovery of the . . . non-sensorial elements of many topics of thought", and Woodworth himself, in the three papers already cited, refers only to Bühler and other writers of the Würzburg school.

ogists, of different periods, prepossessions, and training, speaks unequivocally in favor of the occurrence of elements neither sensational nor affective. It is true that there is no direct physical stimulus of these relational elements and that it is difficult to make out with assurance a complete and definite list of them. Enthusiastic adherents of the doctrine have doubtless alleged as elements what are, after all, complex experiences; but when all has been said, the critics of the doctrine have nothing decisive to urge against the unambiguous introspection of psychologists so divergent in general theory as these already named.

It must be added that this testimony has been fortified, in recent years, by introspection under experimental conditions. One of the latest and most complete of such investigations is made by Bühler whose method—a modification of that of Marbe[1] and Messer—is, in brief, the following. He puts to his subjects, trained introspectors, questions answerable by 'yes' or 'no' which are intended to excite their thought. After a question has been answered the subject at once analyzes the consciousness preceding and leading to his answer. The questions are suited to the interests of the subjects. Illustrations are: "Can you reach Berlin in seven hours"? "Does monism mean the annihilation of personality"? The results of the investigation have been (1) the discovery that in most cases the observers are distinctly conscious of unsensational and non-affective experiences; (2) the apparent occurrence of some cases where no image, verbal nor concrete, can be detected; (3) the confirmation of this introspection by the discovery that a subject often remembers *not* the images, but only the relation—say, of likeness or of opposition—in an earlier experience. Wundt has very sharply criticised the method of these experiments on the ground, mainly, that it involves disturbance of the subject, and that it does not admit of repetition and variation of the experience to be studied.[2] In the opinion of the writer Bühler successfully meets this attack, appealing to the records of his subjects for evidence of their being undisturbed; and holding that repetition and variation are, in fact, obtainable in the essential sense that questions of the same or of regularly varying types may be repeated.[3]

Woodworth's method and results resemble those of the Würzburg school, except that he confines himself to the study of comparison (the discovery of equivalent relation), and that in one group of his experiments he offers concrete material—

[1] "Experimentell-Psychologische Untersuchungen über das Urteil", Leipzig, 1901.

[2] "Ueber Ausfrageexperimente", *Psychologische Studien*, 1907, III, pp. 300-360 (*Cf.* Wundt's rejoinder, *Archiv*, XI, 1908, to Bühler's reply).

[3] *Archiv*, XII, especially pp. 94, 103, 107.

colors and forms—for comparison. Earlier experimenters have found traces of relational experiences in the course of investigations concerned primarily with association. The experiments, for example, by which Professor Gamble and the writer tested Lehmann's assertion that recognition consists in associated images, disclosed a large number of cases in which the consciousness of familiarity, occurring markedly earlier than any associated images, is most readily described as relational experience.[1]

It is highly important to emphasize the fact that this doctrine of a third kind of elemental consciousness is not necessarily synonymous with the hypothesis of imageless thought. The writer of this paper frankly deprecates the tendency of certain psychologists—of Stout, Bühler, Woodworth, for example—to insist that the occurrence of imageless thought has been proved. For it is always possible to question the completeness and the accuracy of the introspect on which this conclusion is based. What is abundantly proved is that along with imagery, and often in the focus of attention, when one compares and reasons and recognizes, are elements neither sensational nor affective. It is unwise and unnecessary to advance a larger claim. Wundt's constructive suggestion that the so-called relational factor in experience analyzes into feeling and attention derives all its cogency, in the opinion of the writer, from the fact, already discussed, that Wundt's feelings include relational factors. In other words, Wundt can afford to deny relational elements because he illicitly and unwittingly holds them concealed within his heterogeneous class of 'feelings'.

From this review of the Wundtian doctrine of the 'feelings' and of the doctrines—diverse in form but alike in essentials—which affirm that there are relational of 'thought'-elements in consciousness it is clear that the domination of sensationalism in psychology has passed. This means the enfranchisement of psychology from the most hampering of the prejudices which have retarded its progress. The *a priori* assumption that all consciousness is completely analyzable into sensational factors has too long interfered with introspective observation. Students of consciousness, successful in finding what they were told to find, have resolved recognition into associated imagery, thought into verbal imagery, and will into antecedent images, with complete disregard of any further outcome of introspection. The downfall of pure sensationalism should be welcomed by psychologists in the interest—not of any other theory—but of free and unprejudiced experimental observation.

---

[1] "Die reproduzierte Vorstellung beim Wiedererkennen", *Zeitschrift*, 32, pp. 177 ff., especially p. 192. The study of Watt, already cited, is primarily an investigation of association.

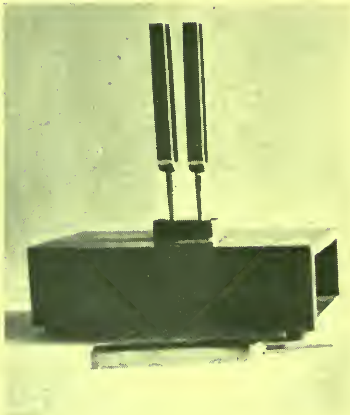
SOME NEW APPARATUS

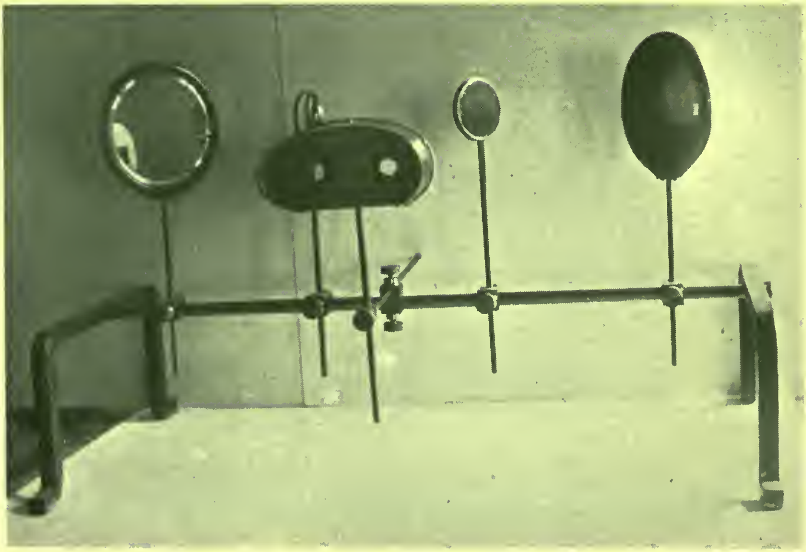
I. An Observation Table

By Madison Bentley

The table is designed for the experimental observation of protozoa and other small animal forms, *e. g.*, the smaller insects, crustaceans and worms. The top of the table is made of ⅜ in. sheet brass, 15 x 20 cm. The legs are ½ in. brass posts, furnished at the bottom with levelling screws. An opening in the top receives an ordinary microscopic slide which lies flush with the surface. The slide is illuminated from below by means of a mirror, 5 x 10 cm., supported upon a horizontal axis which pierces the posterior legs. The table is placed with its back toward a window or near an electric lamp, and the mirror is rotated by a milled head at the right until the observation-slide falls within the path of the reflected rays. The opening below the slide is beveled toward the mirror to avoid a shadow. The whole of the upper surface of the slide, then, save a supporting strip upon three sides, is illuminated by strong transmitted light. A magnifying glass, 4 x 9 cm., swings in from the left when the slide is in place. The magnifying glass is focused by means of an adjustable collar rotating upon the table, and is fixed in position by two set-screws, the one at the left end of the table and the other in the horizontal support of the glass. The slide and the observer's eyes are protected from the direct rays of light by a thin sheet-brass screen, which rises 20 cm. from the convex posterior edge of the table-top and is continued half way across the ends. The observer looks through the enlarging lens straight down upon the slide, thus avoiding the direct rays of the mirror. A black background to the field of observation is secured by a thin shield which swings eccentrically between the anterior legs of the table, just clearing the mirror. When the shield is raised at the rear, its upper surface lies in shadow, and against this shaded surface the objects under examination stand out in relief. The entire apparatus is finished in dull black.

The writer has made use of Professor Jennings' slide-cell (cover-glass or second slide supported upon capillary tubing or glass strips) for protozoan responses to chemicals, carbon dioxide, temperature, etc. He has also used a slide tank (strips of glass cemented around the edge of the slide), wax-





pits (moulded upon the slide and connected subterraneously by capillary tubing), the capillary-tank (designed for the prolonged observation of a single individual), and various simple forms of the maze or labyrinth which are fashioned out of hot capillary tubes and secured to the slide by means of glass cement. The removal of a second section of the table-top permits the insertion of a square glass floor of ampler dimensions than the standard slide affords, for the intimate study of forms of considerable size.

In the construction of the Observation Table, the writer has had in mind the needs of junior students who lack the resources and the technique of the investigator; more specifically he has tried to bring within the limits of a training course, for the study of the simpler and lower forms, experimental conditions that are already realized in the current psychology of the higher animals.

II. A Demonstrational Stereoscope

By E. B. Titchener

The instrument shown at the bottom of the accompanying Plate is a combination of Wheatstone stereoscope, Helmholtz telestereoscope, and Mach pseudoscope. Its only novel feature is its extreme simplicity.

The materials, besides rods and clamps, are two hand-mirrors, two pocket-mirrors, and an ordinary stereoscope-hood from which the lenticular prisms have been removed. If we use the faces of all four mirrors, we have a telestereoscope. If we use the faces of the smaller mirrors, and affix stereoscope diagrams to the backs of the larger mirrors, we have the Wheatstone stereoscope. Finally, if the one eye is allowed to look directly at an object, while the other eye views the same object twice reflected (one large and one small mirror), we have the mirror pseudoscope. The arrangement of parts will be readily understood from the figure.

III. Tuning-Forks for Tests of Pitch-Discrimination

By E. B. Titchener and G. M. Whipple

The arrangement of tuning-forks figured in the upper part of the accompanying Plate shows our solution of a troublesome little problem. We wished to devise a means of testing the discrimination of pitch, with especial reference to the experimental study of school-children; and the requirements were that the apparatus should be inexpensive, as compact and portable as possible, reasonably accurate and reliable, and so designed as to demand relatively little technical skill on the part of the experimenter.

Experience led us to discard at the outset strings, reeds, bottles and other wind-instruments, and to turn to tuning-forks. For discrimination work with forks, the experimenter must be able to produce readily and accurately a constant stimulus, and any one of a series of 8 to 10 variable stimuli. Such a series may be obtained from a pair of forks, the one of which carries sliding weights, etc., or from a number of forks, each one of which gives a single tone of constant pitch.

Adjustable riders of the sort used by Luft were unsuited to our purpose. They are difficult of manipulation by the inexperienced experimenter; the vibration difference must, after every determination of the difference limen, be computed anew by the laborious (and often difficult and inaccurate) method of counting beats; and the timbre of the two forks is often so different as to afford a secondary criterion of judgment. Screw-tipped forks, of the type suggested by Meyer, present similar disadvantages and are, furthermore, too limited in their range of adjustment to be available for tests of discrimination with unpractised or unmusical observers.

The problem is therefore narrowed down to that of finding an inexpensive and reasonably accurate set of forks, and of devising simple means of manipulation.

Seashore advises the employment of a single standard and a set of 10 variable forks, yielding stimulus differences of 0.5, 1, 2, 3, 5, 8, 12, 17, 23 and 30 vs. respectively[1]. Eleven forks of the standard laboratory type, mounted on resonance boxes, would be too expensive and too bulky to meet our requirements. The forks might be left unmounted, and be held after striking upon a single resonance box; but this method fails to guarantee even an approximate constancy of intensity. We have therefore made up a single resonator, upon which any pair of forks may be mounted together, and we have selected for the forks themselves the *a*-forks of 435 vs. sold by music houses at 50 cents apiece as 'extra heavy' forks for piano-tuners. The tines measure about 4.5 by 10 mm. in cross section and are about 95 mm. in length.

The ten variable forks are flatted as required by filing at the base of the U. The ball-tip with which they are supplied is filed away, and the stem is brought in the lathe to a tapered tip. The upper surface of the resonance box carries, as fork pedestal, an oval block of oak 45 mm. long, 20 mm. wide and 12 mm. thick, into which are sunk two conical holes, 20 mm.

[1]For most test work, the 0.5 stimulus-difference may be omitted.

apart and 10 mm. deep, for the reception of the stems of the two forks. The block is separated from the box by a single layer of chamois skin, and is secured to it by two light brass screws.[1]

The method of use is evident from these details of construction. The standard fork is set in the one hole, the variable in the other, with the axes of both crosswise to the box. The silent fork is damped by the finger while its mate is sounding, the finger is then transferred to the sounding fork, and the other is struck. Other variable forks are inserted as required. It is plain that any form of method may be employed. The tones themselves are rather surprisingly full and clear.

For transportation, the forks are wrapped in chamois skin and slipped into the resonance box.

---

[1]The purpose of the chamois-skin layer is to eliminate the high partials which otherwise obscure the fundamental of a light fork when it is first struck. The screws are necessary, since, if glue is employed, the leather loses its elasticity. The dimensions of the block are reduced to a minimum in order to avoid overloading the box. The holes are made as shallow as is compatible with a firm support of the forks, since a deeply drilled hole in a tall pedestal seriously impairs the tone of these forks.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XI. The Effect of Mental Type on the Interference of Motor Habits

By M. McMein and M. F. Washburn

The original purpose of this study was to investigate the influence on the amount of interference exerted by one habit on another, of the simplicity or complexity of the habits concerned. In other words, we wished to find whether two relatively complex habits interfered with each other in a less or a greater degree than two relatively simple habits. For this purpose we made use of the card-sorting habits first investigated by Bergström (this *Journal*, vol. 5, pp. 356 ff.). In the first part of each experiment a pack of cards was used, containing ten cards bearing each of four different nonsense syllables of three letters, thus forty cards in all. Upon a large sheet of paper a square was drawn containing four compartments of about 20 cm. a side, and each of these compartments was marked in large letters with one of the nonsense syllables. The pack of cards was thoroughly shuffled, and the observer was directed to sort the cards on the diagram as rapidly as possible, the time required to complete the sorting being taken with a stop-watch. After an interval of one minute, during which the experimenter shuffled the cards, a second diagram was supplied on which the syllables were differently placed, and the observer was required to sort the cards as fast as possible in this new arrangement. If the time of the second sorting was longer than that of the first, the delay was supposed to be in part at least due to the interference of the earlier acquired habit with the acquisition of the later one, in accordance with Bergström's view. The second part of each experiment consisted in repeating this procedure with a pack containing ten cards carrying each of six different syllables, sixty cards in all. In the third part, a pack with eight different syllables, ten cards for each, eighty cards in all, was used, and in the fourth part a pack with twelve different syllables, a hundred and twenty cards in all. Thus in the four parts of the experiment habits of four different degrees of complexity were involved. As the time of sorting the larger packs was of course longer than that required to sort the smaller ones, the amount of interference was stated in terms of the ratio to the time of the first sorting, of the amount of delay in the second sorting. Thus, if t represents the time of the first sorting, and t′ that of the second sorting, t′-t = the delay in the second sorting, presumably due to interference, and $\frac{t'-t}{t}$ represents the ratio of the amount of interference to the time required by the first sorting. It was this ratio that we recorded. We wished to find whether this value would increase, diminish, or remain unaffected when the complexity of the habits was increased. Three observers took part in the work, M and W, the writers, and H, a woman student in her second semester of laboratory work. Ten complete experiments of four parts each (eight sortings in an experiment) were made and averaged for each observer. The results were as follows:

Observer	Average values of $\frac{t'-t}{t}$			
	Four pack	Six pack	Eight pack	Twelve pack
M	.20	.29	.24	.17
W	.23	.16	.15	.14
H	.18	.21	.23	.13

It will be seen that the only point in which the three observers agree is that they all show the least amount of interference in the case of the most complex habit. Although in the case of all four grades of complexity, the habits were allowed an equal chance to be learned, since a given card was always placed in a given position ten times during the sorting; although each movement in each set of sorting movements was thus performed exactly the same number of times, and had the same chance to impress itself on the nervous system, the most complex habit was not so well learned as the simpler ones, indicating that the difficulty of acquiring a habit increases not in direct proportion to its complexity, but more rapidly. It is further evident that observers M and H found the simplest habits interfering less with each other than those of moderate complexity. This, it was gathered from their introspections, was due to the fact that in the very simple four-position sorting, the second diagram was learned on its own account so quickly that the disturbing effect of the first diagram wore off after a few seconds. These same introspections revealed an important cause for the individual differences in our results. M is of a decidedly visual type of mind. In sorting the cards, she rapidly formed a visual image of the diagram and guided herself almost entirely by this. She did not need, after the first seconds of a sorting process, actually to look at the marks on the compartments, because she had a picture of the whole chart in her mind. W, on the other hand, is decidedly below the average in visualizing power. She learned the diagrams almost wholly in motor terms, and at the end of a sorting was quite unable to recall the position of more than two or three syllables on the more complex diagrams, having scarcely any visual image of the diagram. M, on the contrary, could reproduce all the diagrams perfectly from her mental pictures of them. H seemed to occupy a middle position as to type; her visual images were not so marked as M's, nor by any means so defective as W's. It will be seen from the results that the degree of interference in W's case diminished steadily as the complexity of the habits increased. W did not find that the simplest diagram was less favorable to interference than those of moderate complexity. While for M and H, learning the diagrams by a visual method, the second diagram in the four-position sorting was learned so quickly that it soon effaced the visual memory of the first diagram, for W, in whose case the memory of the first position diagram was almost wholly motor, this memory persisted much longer and seriously interfered with the formation of the new habit. We have here a suggestion of the fact that visual images are acquired and effaced more rapidly than motor habits. While M and H both used visual images in their learning, these images were naturally more effective in the case of the simplest chart, where they were formed almost at the outset of the sorting. Hence the images functioned to reduce the interference effect in the case of this diagram by facilitating the learning of the new position; while in the case of W we have the law that simpler habits are acquired better than complexer ones even where each movement in all the habits is repeated an equal number of times, holding without exception.

This difference of mental type in our three observers suggested two supplementary sets of experiments. It occurred to us, first, that an observer whose card-sorting was guided by a mental picture of the diagram would be much less disturbed when the diagram was turned about through an angle of ninety degrees than would an observer whose learning was done largely in motor terms, for the former could without much difficulty turn her mental picture of the chart about to correspond with its new position, while the latter would be bewildered by the fact that all the movements involved in sorting would be entirely changed by the rotation. Accordingly this point was investigated in the following way. The four-syllable pack was first sorted on one of the diagrams used in the preceding tests, and then after the usual one-minute interval the diagram was turned through ninety degrees and the observer was required to repeat the sorting process. Five such experiments were made by each of the two observers M and W. The average delay involved in making the second sorting, in the rotated position, with observer M was —2.6 seconds; that is, M made the second sorting more rapidly than the first. She was little, if at all, disturbed by the rotation, and reported that she visualized *herself* as having moved around in front of the diagram in its new position, and guided herself by her memory picture of the chart. When a similar test was made with the six-syllable pack on observer M, she showed an average delay of one second in the rotated position, thus suggesting that the visual image was actually less effective for this more complex diagram. When observer W, on the other hand, was put through this test, the average delay in sorting the cards on the rotated diagram was, for the four-syllable pack, 6.9 seconds, and for the six-syllable pack, 11 seconds. This observer had great difficulty in sorting the cards on the rotated diagram, and constantly made false movements in accordance with the habit for the original position of the chart, while M made few or no such movements. The results were thus in accordance with what we should expect from the mental type of the observers.

In the second place, it seemed possible that a person who used much visual imagery in the card sorting would be more affected by distractions tending to suggest visual images, or indeed any mental images of whatever sense department, than one whose sorting was almost purely motor. Accordingly, four tests were made with each of the three observers, M, H, and W, in each of which the four-syllable pack was first sorted without distraction, and then, after a one-minute interval, sorted again on the same diagram while the experimenter read aloud. The observer was required to give from memory at the close of the experiment the substance of the passage read. Since the sorting under distraction was done on the same diagram as that without distraction, practice should have shortened the time occupied by it, and any lengthening would be in spite of practice. Observer W was not at all delayed by the reading aloud. The time of the sorting under distraction was on the average 2.2 seconds less than that of the sorting without distraction, and W's recollection of the substance of the reading was as good as that displayed by either of the other observers. Observer M, the most 'visual' of the trio, was delayed by the reading, on the average, 11.5 seconds, and observer H was delayed 10.5 seconds. As M sorted the cards in general more rapidly than the other two observers, her delay under distraction was, in proportion, much greater than that of H or W. The ratios of the average amount of delay under distraction to the average time of an undistracted sorting were as follows: for W, —.4; for H, .15; for M, 2.2. It will thus be seen that the most visual observer suffered most, and the most motor observer least, under distraction.

The first mentioned of these two special tests of the influence of mental type on card-sorting, that where the diagram is rotated ninety degrees, has been made use of to some extent in our laboratory as a method for determining type, and seems to promise well.

PSYCHOLOGICAL LITERATURE

An Introduction to Social Psychology, by WILLIAM McDOUGALL. Methuen & Co., London, 1908. pp. 351.

This book is one more token of the increasing leaning of psychologists to pass from the study of the individual to that of society. It is an inclination which the sociologist especially should encourage, for he needs the services of his brother scientist in order to lay bare "the springs of human action, the impulses and motives that sustain bodily and mental activity and regulate conduct." How much Ethics, Political Economy, the Philosophy of History, and Jurisprudence have suffered from an insufficient understanding of their psychological foundation, is indicated in the introduction.

The indispensable preliminary of all social psychology is, we are told, the study of the springs of human action, for "social psychology has to show how, given the native propensities and capacities of the individual human mind, all the complex mental life of societies is shaped by them and in turn reacts upon the course of their development and operation in the individual." (p. 18.)

The first section (pp. 19-264) is offered as a propædeutic of Social Science. In the second section (pp. 265-351) are "indicated some of the ways in which the principal instincts and primary tendencies of the human mind play their parts in the lives of human societies." I shall devote the space at my disposal entirely to the first section, for it not only makes up three-quarters of the volume, but it contains also most of what there is in it of originality and importance.

The author's starting point is the proposition that human nature has everywhere and at all times certain inherited tendencies which are the essential motive powers of all thought and action. These innate tendencies are either specific or general. The first are the instincts. His definition of instincts is noteworthy for the rôle ascribed in them to consciousness. They are not compound reflex actions for "every instance of instinctive behavior involves a knowing of some thing or object, a feeling in regard to it, and a striving towards or away from that object." (p. 26.) He insists upon the psychical aspect of instinct because in its absence it would be "impossible to understand the part played by instincts in the development of the human mind." (p. 30.) Of the three parts — afferent, central, efferent—of the innate disposition, the first and the third are widely modifiable, while the central part persists throughout the life history of the individual as the essential unchanging nucleus of the instinctive disposition. As the central part determines the impulse to action and the visceral changes, its constancy means the constancy of the affective aspect of the instinct (p. 42).

In the third chapter is presented a view of the relation of emotion to instinct which the author is, it seems to me, warranted in regarding as embodying an advance in the understanding of this difficult problem. McDougall regards each one of the primary emotions as the affective aspect of one of the principal instincts and the derived emotions as the outcome of the interaction of several instinctive dispositions. According to this definite relation, the emotions have no independent existence; they come and go with instincts or other innate motor tendencies. Those familiar with the theory of emotion set forth by Dewey and Paulhan will regret that McDougall has not made use of it. For it seems that a completely satisfactory view of emotion must take into account the rôle played in its production by antagonistic motor tendencies. The rest of the chapter is given up to a study of the principal instincts of man and, first, to the more definite ones. Each instinct is taken up together with its affective aspect. The principal instincts and the primary emotions are:

1. The Instinct of flight and the emotion of fear, an instinct with a double tendency in most animals, one of flight and one of concealment.
2. The Instinct of repulsion and the emotion of disgust.
3. The Instinct of curiosity and the emotion of wonder.
4. The Instinct of pugnacity and the emotion of anger.
5. The Instinct of self-abasement (or subjection) and the emotion of subjection.
6. The Instinct of self-assertion (or self-display) and the emotion of elation.
7. The parental instinct and the tender emotion.

Some have refused to place this last emotion among the primary. McDougall holds that "the most powerful of the instincts" is accompanied by a strong and definite emotion, namely the tender emotion. There is a touch of scorn in the tone with which he rejects the doctrine which would make the tender feelings as purely self-seeking as any other pleasure. He finds in this instinct-emotion the root of disinterested indignation, itself the ultimate root of justice and of public law. If we hold with Bain to hedonic calculation, instead of to instinct-emotion, "disinterested beneficence and moral indignation . . . remain a paradox and a miracle." (p. 79.)

The rôle of pleasure and pain in conduct is discussed in several places (pp. 8-10, 43, 154 ff, 190, 237, 256). Everywhere he opposes the instinctive impulse to the guidance of pleasure and pain. Yet he does not commit the mistake of denying these processes all influence on action, "they serve . . . . to modify instinctive processes, pleasure tending to sustain and prolong any mode of action, pain to cut it short; under their prompting and guidance are affected those modifications and adaptations of the instinctive bodily movements." . . . (p. 43.)

Among the less well-defined emotional tendencies he considers, are the instinct of reproduction, sexual jealousy, female coyness, gregariousness, acquisition and construction.

Sympathy, suggestion and imitation are brought together in an instructive way in Chapter IV, for these three "general or non-specific innate tendencies" are as many forms of mental interaction. Sympathy involves chiefly the affective; suggestion, the cognitive; imitation the conative aspect of mental life. We speak, for instance, of imitation when the prominent result of the interaction is the reproduction of the bodily movements of one person by another. These three innate tendencies are not to be called instincts for they do not conform to the three tests of a true instinct: there is, in imitation, for instance, nothing specific in the nature of the sense-impressions by which the movements are excited or guided, no common affective state and no common impulse seeking satisfaction in some particular change of state (p. 103). The expression "instinct of imitation" has been used so loosely of late that one is grateful for this analysis and classification.

The observation that animals in their fighting plays do not hurt each other suggests to our author a modification of the theory of Prof.

Groos. Fighting plays are not the manifestation of the fighting-instinct restrained by a strong volitional control; they are the expression of a particular modified form of the combative instinct, an instinct differentiated from and having an independent existence alongside the original instinct (pp. 110-112). I cannot persuade myself that individual adaptation of the fighting instinct does not account satisfactorily for the play. Animals learn surprisingly fast how to adapt the intensity of their efforts to particular circumstances. If, however, McDougall's opinion should be accepted, would it not have to be extended so as to include practically all plays, for every one of them is the expression of an instinct definitely modified?

In Chapter V is discussed the nature of the sentiments and the constitution of some of the complex emotions. His attempt to exhibit admiration, awe, gratitude, scorn, envy, reproach, jealousy, resentment, shame, etc., as fusions of the primary emotions considered in an earlier chapter, is illuminating and displays, in many places, an admirable penetration. McDougall accepts as valid and of great value Shaud's doctrine of sentiment. On several points, however, he separates himself from Shaud, whose views, unfortunately for the psychologist, have not yet been fully published. One such point is the nature of sorrow and of joy (pp. 80, 149 ff). They are regarded by many as primary emotions; our author looks upon them as "pleasure-pain qualifications of emotional states rather than emotions capable of standing alone." Joy is more than pleasure, "it is a complex emotional state in which one or more of the primary emotions, developed within the system of a strong sentiment, plays an essential part. We ought, then, properly to speak not of joy, but of joyous emotion."

If our emotions and our impulses were not organized, our social relations and conduct would be chaotic and unpredictable. An organized system of emotional dispositions centred about some object is what Shaud calls a sentiment. The remaining four chapters of the first section are given up to the development of the sentiments and chiefly to that of the self-regarding sentiment which is, together with the sentiment for self-control, the master sentiment of a fully developed person. Love, hate and respect form the three main classes of sentiments.

Four levels of conduct are distinguished: "(1) the stage of instinctive behavior modified only by the influence of the pains and pleasures that are incidentally experienced in the course of instinctive activities; (2) the stage in which the operation of the instinctive impulses is modified by the influence of rewards and punishments administered more or less systematically by the social environment; (3) the stage in which conduct is controlled in the main by the anticipation of social praise and blame; (4) the highest stage, in which conduct is regulated by an ideal of conduct that enables a man to act in the way that seems to him right regardless of the praise or blame of his immediate social environment," (p. 181.) The first and second levels do not involve more than the promptings of instinct; the two others are made possible by the appearance of the idea of the self. The development of the idea of the self is, of course, closely bound up with that of the self-regarding sentiment.

The influence of praise and blame on men's conduct passes all rational grounds. Whence this profound influence? The conclusion reached is that it is the outcome of "the influence of authority or power, primarily exercised in bringing rewards and punishments . . . . and [of] the impulse of active sympathy towards harmony of feeling and emotion with our fellows." (p. 201.)

There remains to be shown how from the merely egoistic levels man rises to the fourth and highest. Attention is first drawn to a connecting link. Men may be moved not so much by the approval or disapproval of some one, as by the fact that this approval or disapproval is pleasant to the one who expresses it. This motive "constitutes . . . . a very effective supplement to the egoistic motives." (p.203). But it is through original moral judgments of approval and disapproval, and not through unquestioned subserviency to praise and blame, that a man rises to the higher plane of conduct. In those primitive societies in which rigid rules govern every detail of life, original moral judgments are hardly possible, whereas the diversities of the moral codes of the various circles among which the civilized man moves stimulates reflection and leads to original moral judgments. Thus are formed an ideal of conduct and "a sentiment for a perfected or completely moral life" without which no man will be able to reach the highest plane of conduct.

A final point remains to be elucidated. How comes it that in some men—those regarded as the best—the presence of an ideal and of a sentiment for the ideal become the dominant motives of conduct. The elucidation of this dynamic problem leads to a valuable discussion of the nature of volition. McDougall agrees with James that "the essential achievement of the will is to attend to a difficult object and hold it fast before the mind;" and he maintains further that this achievement is due to an addition to the energy with which the idea of the desired end maintainsitself in consciousness. But he is a determinist; for him the chief source of this addition is in conations, desires and aversions arising within the self-regarding sentiment (p. 248), and so he defines volition "the supporting or re-enforcing of a desire or conation by the co-operation of an impulse excited within the system of the self-regarding sentiment." (p. 249.) "The essential mark of volition [when separated from the other varieties of conation] is that the personality as a whole, or the central feature or nucleus of the personality, the man himself, ... is thrown upon the side of the weaker motive. . . ." (p. 240.)

The operation of the self-regarding sentiment in him who has conceived the ideal of the perfectly moral life, gives the appearance of independence of the appeal of society, "it enables him to substitute himself, as it were, for his social environment." (p. 254.) "Moral advance and the development of volition consist, then, not in the coming into play of factors of a new order . . . . but in the development of the self-regarding sentiment and in the improvement or refinement of the 'gallery' before which we display ourselves . . . and this refinement may continue until the 'gallery' becomes an ideal spectator or group of spectators, or, in the last resort, one's own critical self, standing as the representative of such spectators." (p. 257.) This ultimate reference, in the highest type of conduct, to self-respect will give most readers a shock of surprise. One was not prepared for this apotheosis of self-esteem. The author does not, however, if I understand him right, set up the maintenance of one's self-respect as the highest aim of conduct. His view is rather that the ideal of conduct, the aim which the good man endeavors to realize, is thoroughly altruistic, namely the highest good of the greatest number, but that ideal to have power must be backed by some of the strong innate dispositions, otherwise it would remain a relatively impotent item of knowledge. The self-regarding sentiment it is which supplies the energy necessary for the realization of the altruistic ideal. But even when thus understood, his analyses of instances of self-sacrifice (p. 252) represent, in my opinion, what actually takes place in the mind of the majority of noble men but is not true of those generally regarded as the finest fruit of social development. These rare people, I will venture to say, act as they do in great moral crises without reference to the effect of their conduct on their self-esteem. Or, rather, one had better say that in them the consciousness of self has lost its original narrow individualistic meaning and has taken on a social significance. McDougall's history of the development of self-consciousness appears to me defective in that he does not recognize that the completed moralizing process involves the existence of a form of social consciousness which includes the idea of the self. This seems to me the most serious criticism one can make of a book, which because of considerable originality and definiteness of presentation, cannot fail to stir up much discussion. Considered as a whole, its most striking characteristic is the masterly firmness and thoroughness with which it traces the development of man up to his complete socialization on the basis of fundamental innate dispositions and social interaction.

Bryn Mawr College JAMES H. LEUBA.

The Psychology of Skill, with Special Reference to its Acquisition in Typewriting, by W. F. BOOK. University of Montana Publications, Bulletin No. 53. Psychological Series, No. 1, pp. 188.

The object of this study, the work of which was done in major part at Clark University, was two-fold. (1) To determine accurately the course of the practice curve for a number of individuals. (2) To analyze consciousness for the different practice stages shown by the individual curves.

Recording apparatus was attached to an Underwood typewriter and gave records on a smoked drum of the following items: (1) Stroke for each letter; (2) strokes on the word spacer; (3) movement of the carriage for a new line. Simultaneously with these, records were made on the drum of (4) the time spent by the writer (in the visual method of writing) in looking at the copy, the experimenter recording this by means of a separate key; (5) time marker beating seconds; (6) pulse rate of the writer while writing. The normal pulse rate was taken in another manner each time before beginning. Detailed introspective observation was required of the writers at all times. Eleven observers participated, including beginners who continued practice until a semi-expert stage had been reached, and professional and expert writers. Both the touch and the visual methods of writing were employed. In addition, three of the observers wrote practice sentences until an expert speed was obtained in these sentences.

The presentation of the results falls, roughly, into two parts. (1) Analysis of the learning consciousness. (2) Explanation of the characteristics of the practice curves. For both the visual and the touch methods of writing four stages of practice are made out. For the touch method they are the following: (a) The beginning stage, when the whole writing process involves a number of definite conscious steps. (b) The letter association stage, which is reached when the "sight of the letter in the copy or the first pronunciation of it calls up at once the direct movement for striking the proper key." (c) Syllable and word association stage. This is reached when incipient pronunciation of the word calls forth at once the group of movements, when the motor-tactual image for the group of strokes (general 'feel' for the group) as a means of writing is being eliminated. (d) Expert stage, writing by phrases and sentences. This is reached when the writing becomes continuous, the strokes following the incipient pronunciation of the words without breaks or further conscious processes. In learning by the visual method these several practice stages are represented by different conscious factors, but in the expert stage the visual approaches the touch method as regards the conscious factors that are involved. The details of the analysis for both methods are concerned with the way in which these different practice stages are developed, by 'short-circuiting' and elimination of conscious processes present at first, by changes in the direction and distribution of the attention, by the development of new ways of getting the copy, by the appearance, development and final disappearance of motor-tactual imagery for the individual stroke, for the small group of strokes, for syllables and words, and for larger groups.

The explanation of the characteristics of the practice curves brings into consideration a complex relationship of factors. The initial rapid rise in all curves is due to the fact that several lower order habits (ways of writing in the letter and the syllable and word association stages) are then developing together, and each more rapidly than later. Higher order habits (those involved in phrase and sentence writing) do not begin fully until the lower are fairly well developed. But the Bryan and Harter interpretation of plateaus is wrong in so far as it attributes them to a necessary and slow perfecting of the lower order habits before the higher can begin at all. These long plateaus occur towards the close of the syllable and word association stage, which may therefore be called a 'critical stage.' At this point the learner is very prone to do one of two things. Either he will (1) relax his effort and fall back to lower order habits of writing instead of pushing ahead; *i. e.*, he is 'caught' in the more or less fixed habits of this stage. Or, (2) he will assume a freedom and skill that he does not yet possess, will direct his effort to speed alone and take it from those processes that still require some conscious direction. In this lies the explanation of the long plateaus. The objective records and the pulse rate reinforce this explanation from the direct observations. High pulse rate always goes with great effort, but the effort may be wrongly directed (to speed alone), and result in a slow rate of writing. The latter was the case for the practice sentences and their plateaus at this stage of practice. For the regular writing from copy a constant high pulse rate is correlated with periods of several days or more of rapid rise in the curve, and a constant low pulse rate is correlated with the plateaus. The explanation for the smaller irregularities in the daily practice curves is more complex. In all cases, taking the attention entirely from processes that still need some conscious direction, or directing the effort to speed alone, results in many mistakes in writing and in 'education in error', which has to be unlearned again before further progress can be made. The effort is apt to be wrongly and inefficiently directed in this way on 'bad' days, at the beginning of each daily period of writing, and at other times when the learner becomes conscious that his rate of writing is low. Maximum effort counts only when the conditions, objective and subjective, for fast and correct writing are favorable. On the other hand, it is maximum effort alone under these favorable conditions that makes for progress in learning, establishes new 'short-circuits' and higher habits of writing.

The monograph is abundantly illustrated by quotations from the direct observations of the learners, by tables, curves and specimen drum records. F. KUHLMANN.

Nervous and Mental Diseases, by A. CHURCH and F. PETERSON. With 341 illustrations. Sixth edition, thoroughly revised. Philadelphia and London, W. B. Saunders Co., 1908. pp. 945. Price, $5.00 net.

The fourth edition of this admirable text-book (1903) was noticed in Vol XV, p. 452, of the *Journal*. The fact that the work has passed through six editions since its publication in 1899 is sufficient evidence that it is appreciated by the medical students and general practitioners for whom it is primarily intended.

As was pointed out in the notice of the earlier edition, a section of special interest to psychologists is the Review of Recent Problems in Psychiatry by Professor Adolf Meyer (pp. 662-700), which deals particularly with the work of Kraeplin, Ziehen and Wernicke. But apart from this, the book is a valuable addition to the working library of the psychologist.

The two sections have been written independently: that on Nervous Diseases (pp. 17-652) by Dr. Church, and that on Mental Diseases (pp. 653-916) by Dr. Peterson. "Each author has contributed to a single volume what might have been made a separate monograph." While this arrangement has its conveniences, the resulting volume is so heavy and unwieldly that it might, perhaps, be well to consider the publication of the seventh edition in two parts.

Mind and its Disorders, by W. H. B. STODDART. P. Blakiston's Son & Co., Philadelphia, 1909. 488 p.

This work seeks to give both student and practitioner a succinct account of our existing knowledge of mental diseases. The author would induce the reader to think untheoretically of mental processes, normal and morbid, his own work for twelve years being chiefly clinical research into the nature of nervous phenomena associated with mental disorders. In its first section, which deals with normal psychology, he seeks to correlate mental processes with their physical substrate in the nervous system, "the transcendental psychology of the modern school men being ignored as useless to the practical physician of today." In the second section, the psychology of the insane is treated in a similar manner. The classifications largely coincide with those of Kraepelin, although some of his nomenclature has been changed. The author has made free use of standard psychologies, and perhaps the chief feature of the book is precisely that of which the author himself evidently is most conscious, namely, the account of normal processes of sensations, perception, ideation, sleep, fatigue, sentiments, language, the ego, etc. In treating the psychology of the insane, he also follows a similar order, treating in sequence disturbances of sensation, perception, association, emotion, abnormalities of action and of judgment, or delusions. In the third part, on mental diseases, the chapters treat causation, physical stigmata, degeneration, intermittent and periodic insanities, the insistent psychoses, alcoholism, paranoia, psychæsthenia, neurasthenia, hysteria, troubles due to organic diseases, idiocy, visceral disease, combined psychosis, diseases to which the insane are peculiarly liable, case taking, feigned insanity, the relations of the insane to law, methods of studying the nervous system and cytological examinations of the cerebro-spinal fluid, etc. The author has nearly one hundred cuts, and perhaps this book might be called the most compendious and concentrated textbook now available. Personally we regret that the author does not deal more fully with the newer methods represented by Janet and Freud, Kraepelin from our point of view being intermediate between the old views and these, which seem to be the psychiatry of the future.

Les Nérvroses, by PIERRE JANET. Ernest Flammarion, Paris, 1909. 394 p. (Bibliothèque de Philosophie scientifique.)

This work gives us a rapid *résumé* of the author's many studies during the last twenty years. As to each function, he describes and compares two groups of symptoms — hysterical and psychæsthenic. Thus among mental disorders we find fixed ideas of somnambulists and the obsessions of the over-scrupulous; while in the disturbances of movements, there are hysterical paralyses which seem closely related to the phobias of action. In perceptive troubles, we have hysterical anæsthesias, despite the algies of psychæsthenic dysgnosias. These comparisons present the psychological differences which exist between the various neuropathic disturbances appearing on the neurotic bases. Everywhere functions are more or less intact in their essential and older parts, but they are decapitated by the reduction or suppression of the more recent and more perfected ones. Thus neuroses present more diverse forms of regression and involution caused by various depressive influences. Under Part I, neuropathic symptoms, the writer treats fixed ideas and obsessions. Then follow in order chapters on amnesias and doubts, disturbances of speech, chorea and tics, paralyses and phobias, perceptive troubles, instinctive and visceral disorders. The second part, which treats of neuropathic states, contains five chapters, as follows: on nervous crises, neuropathic stigmata, the mental state of hysteria, the psychæsthenic state, what are neuroses? The answer to the last question may be roughly indicated by the phrase that they are diseases of functional evolution.

Psychology, Normal and Abnormal, by WARREN E. LLOYD, and ANNIE ELIZABETH CHENEY. Baumgardt Pub. Co., Los Angeles, Cal. pp. 127.

As the authors have themselves written a critique of this book, which appears just before the table of contents, we will allow them to speak for themselves. "Now whatever the learned may say hereafter (and the learned will most surely investigate this book), they cannot undermine its foundation or destroy its structure." "No flaw can be found in the logic from start to finish." "It throws no sop to the Cerberus of superstition, it pampers no morbid dabbler in so-called 'new thought', it tickles no nerve of religious fanaticism; but straightforwardly relies upon principle, logic and facts, daringly throwing down the gauntlet to antiquated psychology, and through its up-to-dateness in all directions defies bigotry and challenges criticism." "It is a text-book for students in colleges and a volume for their professors also." "It is safe to say that a profound study of this work will have a practical outcome, and we dare to assert that he who delves into it deeply enough will find a key to the problem of life itself. *Little attempt has been made in this text-book to deal with data newly discovered, or facts gained by specialists along any of the lines of modern research that might bear on the question*, for, once having started in the trails of these sleuths of modern sciences, there would be no end to the fruit gleaned, and no possible way in the limited space of a compact work of this kind of dealing with it satisfactorily. *So the effort rests contentedly on its principle alone*."

The value of the book to the student of modern psychology is indicated by the italics, which are my own. AMY E. TANNER.

Psychological and Medical Observations among the Indians of southwestern United States and northern Mexico, by ALES HRDLICKA. Government Printing Office, Washington, D. C., 1908. 460 p.

This indefatigable observer here gives us a wealth of interesting observations on subdivisions of the tribe, personal environment, food, drink, habits of life, character, social conditions, marriage, children, on whom he makes very many interesting observations—growth, height, puberty, dentition, etc.—and then passes to the adults, having studied stature, muscle, force, the skin and its appendages, special senses, sleep and dreams, mental and nervous power, digestion, obesity, grayness, loss of hair, sterility, etc. He then takes up social abnormalities, like artificial operations, infanticide, crime, suicide, etc. Under medical observations, he treats of pathology in general, and then the special diseases—goitre, insanity, epilepsy, idiocy, spinal curvature, tuberculosis, etc. Very interesting is his chapter on Indians' notions of disease, and the medicine men and women, prevention, and folk medicine. In an interesting appendix, he treats of foods, and gives many careful anthropometric details on boys and girls, muscle tests, grayness, etc., under each rubric in more than a dozen tribes. This seems to us in many respects a model of what such studies ought to be.

Phrenology or the Doctrine of the Mental Phenomena, by J. G. SPURZHEIM. With an introduction by C. Elder. Revised edition from the second American edition of 1833. Philadelphia and London, J. B. Lippincott Co., 1908. pp. 459. Price $3.00 net.

The present volume is an exact reprint of the two-volume edition of Spurzheim's *Phrenology* published at Boston in 1833, save that the editor has omitted the author's reflections upon the moral and religious constitution of man, his volumiuous Latin notes, and a controversy with Combe. The Latin notes we might, perhaps, have spared; but it is unfortunate that the reflections and the polemic have been omitted, since they are as characteristic of Spurzheim as the phrenological doctrines themselves, and throw a good deal of light upon his attitude and methods. However, the psychologist can only be grateful for the reprint of a work which probably none but a few enthusiasts possess in any original form.

The editor contributes an introduction, in which he affirms that the spirit of the age is materialistic; that it has no science of mind and desires none; and that psychology is in no better case than it was sixty years ago. He himself, however, is still in the bonds of the psychology of faculties; and whatever the sins of current psychology, it has at least got some distance beyond Christian Wolff.

The volume is well printed and illustrated, and is pleasantly light in the hand.

Kaiser Julians philosophische Werke. Uebersetzt und erklärt, von R. ASMUS. Leipzig, Dürr'sche Buchhandlung. 1908. pp. ix, 223. Price Mk. 3.75.

This little book, forming vol. 116 of the useful Philosophical Library, contains, besides a critical introduction, German translations of six of the Emperor Julian's Orations: that on the worship of the sun, addressed to Sallustius, his old military councillor and friend, first in Gaul and afterwards in Germany; that on the mother of the gods, Cybele, whose temple at Pessinus the author visited and whose worship he restored; the two on true and false Cynicism; the one of which is addressed to the Cynic Heracleius, of whom nothing further is known; the 'letter' to the aforesaid Sallustius, in which Julian consoles himself and his friends on the recall of Sallustius, by the emperor Constantius, from Gaul to the east; and lastly the letter, or more correctly the dissertation, addressed to his former tutor, the philosopher Themistius, on the difficulty the author thinks he would experience in showing himself so perfect an emperor as Themistius expected. Each oration is preceded by a brief appreciation and analysis, and followed by a few pages of explanatory notes.

The editor and translator has performed his task with great skill and with commendable self-restraint.

The Metaphysics of Nature, by CARVETH READ. 2d ed. Adam and Charles Black, London, 1908. 372 p.

Here is a metaphysician who declares that certain shortcomings in his composition were due to the fact that he wrote "in constant dread of wearying the reader with verbiage and commonplaces." This second edition contains additions on the following subjects: truth, consciousness, transcendental being, and moral freedom. In the introduction, he treats of belief and knowledge, reality and truth. The first canonic book deals with tests of truth, historic and analytic, scepticism, and the relativity of knowledge. Book II, Cosmology, deals with substance in experience, ontology of the world, universal forms of the phenomenon. The third book is on psychology and treats of the subject in experience, and ontology and natural history. The last book is devoted to the categories, which are, first, abstract or those of relation in general, and qualitative and quantitative relations; and, second, physical, such as atoms, ether, rest, change identity, motion, force, inertia, mass, elasticity, incompressibility, mechanics, etc., and, lastly, the categories of subjective activity, parallelism, interaction, will, final causes, human ends, man and society.

The Philospher's Martyrdom, by PAUL CARUS. Open Court Publishing Co., Chicago, 1908. 67 p.

This is an interesting and stimulating book; a little more of the dramatic element put into it and it would be fit for the stage. The author's idea seems to be to bring the wise and truly philosophical agnostic of the Spencerian type in contact with a rather long series of representatives of other points of view,—priests, spiritualists, society people, etc. In the end Mr. Agnosto was shipwrecked, captured by cannibals, fattened and eaten to make them a Thanksgiving day. His memory is still kept green on Cannibal Island because he furnished them with the tenderest, daintiest Thanksgiving meal they ever enjoyed.

The Old and the New Magic, by HENRY RIDGELY EVANS. Open Court Publishing Co., Chicago, 1906. 348 p.

We have here a very interesting and copiously illustrated work which deals with the history of prestidigitation. The lives of Pinetti, Cagliostri, Houdin, and others down to Houdini, the secrets of second sight, modern thaumaturgism, magicians I have met, the riddle of the sphynx, Treweyism, are some of the chapters. The practical parlor magician is given a great many very interesting hints. The striking fact about this book is that many modern tricks of great commercial value are exposed.

The Naturalization of the Supernatural, by FRANK PODMORE. G. P. Putnam's Sons, New York, 1908. 374p.

This interesting book of a fertile author uses data largely from the unpublished journal of the Society for Physical Research. After a general introduction rehearsing the story of the foundation of the Society, the following chapters constitute the book: Experimental Thought-transference; Spontaneous Thought-transference — Mind's Eye Visions; Spontaneous Thought-transference—Coincident Dreams; On Hallucinations in General; Telepathic Hallucinations; Poltergeists; Spiritualism; On Communication with the Dead; Phantasms of the Dead; Haunted Houses; Messages Received through Trance and Automatism; The Case of Mrs. Piper; On Clairvoyance and Prevision. The marked feature of it is the progress toward an attitude of critical or suspended judgment with regard to the fundamental problem of spiritualism and even regarding Mrs. Piper, who is coming to be perhaps more than any other individual the *pièce de resistance* of the English Society.

The Theosophist, February, 1909. Edited by Anne Besant. 189 p.

The very voluminousness of this work, founded by Mme. Blavatsky and Mr. Olcott, itself bears striking witness to the size and vigor of the circle that it represents. Theosophists now have learned to make many connections with daily life. They review current magazines, have much to say about science, the ethics of competition, separateness and unity in daily life. It is evident, too, that it fares well with theosophy in other lands. The Oriental note that runs through all is very strongly accentuated.

The Psychology of Singing, by David C. Taylor. The Macmillan Co., New York, 1908. 371 p.

A peculiar gap exists between the accepted, theoretical basis of singing and the actual methods of vocal teachers. The number of scientific treatises would lead us to infer that we have a coherent science of voice culture. This, however, is not the case. To reach a basis for this, one must have insight into the operations of the voice by listening, and then must levy tribute upon anatomy and acoustics. The author has accordingly divided his work into three parts. The first describes the methods of instruction in singing, tone production, voice culture, breathing, registers, resonance, empirical material of methods; the second, a critical analysis of them, the mechanical, vocal management being the basis, describes the fallacies of the doctrine of breath control, of forward emission, chest and nasal resonance. In the third part, the author evolves the basis of a real science of the voice, based on sympathetic sensations of vocal tone, empirical knowledge, the precepts of the old Italian school and of the modern school. And in part fourth, he applies vocal science to practical voice culture, tells us the causes of throat stiffness and of incorrect vocal action, the true meaning of vocal training, intimations of the rational method and its materials; with a final outline of the practical method of vocal culture. His book is very lucidly written and printed, and cannot fail to interest and instruct all concerned. The Appendix contains an interesting bibliography.

Friedrich Schleiermachers Weihnachtsfeier. Kritische Ausgabe, by Hermann Mulert. Dürr'sche Buchhandlung, Leipzig, 1908. (Philosophische Bibliothek, Band 117.)

Einführung in die Erkenntnistheorie, von DR. AUGUST MESSER. Dürr'sche Buchhandlung, Leipzig, 1909. 199 p. (Philosophische Bibliothek, Band 118.)

René Descartes' philosophische Werke. Dritte Abteilung, die Prinzipien der Philosophie, vou DR. ARTUR BUCHENAN. Dürr'sche Buchhandlung, Leipzig, 1908. 310 p. (Philosophische Bibliothek, Band 28.)

Ueber Tierpsychologie. Zwei Vorträge, von L. EDINGER und ED. CLAPARÈDE. Johann Ambrosius Barth, Leipzig, 1909. 67 p.

Man and the Universe, by OLIVER LODGE. Methuen & Co., London, 1908. 356 p.

Psychotherapy. A course of reading in sound psychology, sound medicine and sound religion. Edited by W. B. Parker. Vol. 1, No. 2. Centre Publishing Co., N. Y., 1908. 96 p.

NOTES

The second international course for Legal Psychology and Psychiatry will be held at Giessen (Grand duchy of Hesse) Germany, April 13 to 18, 1909. The course will be under the direction of Professor Sommer with the co-operation of Professors Mittermaier and Dannemann of Giessen and Professor Aschaffenburg of Cologne. Address all communications to Dr. Sommer, Professor of Pyschiatry, University of Giessen.

COURSE ON THE FEEBLEMINDED

The Neurological Institute in Frankfurt on the Main, in connection with the *Frankfurt Special Classes* (Help-schools) will arrange a two-weeks' course on *Problems Concerning Feebleminded and Psychopathic Children*. This is to be given the latter part of June, 1909. Scientific research, clinics, psychology, education and methods, and forensic questions, will be the subjects of lectures and courses by specialists. The course is intended for those who are professionally engaged in this work, or are interested in it, or who wish to prepare themselves for it. It aims to offer a basis for extended work, a survey of the whole affair and its practical management. Accordingly, the chief emphasis will be laid on *practical presentations and demonstrations* (anatomical, pedagogical, presentation of patients and experimental demonstrations). As far as possible all sides of the subject and their bearing on other branches of knowledge will be considered.

The following courses and demonstrations are planned:

Normal and pathological anatomy of the juvenile brain. Child psychology. Psychopathology of youth. Instruction of the feebleminded. Methods of teaching. Organization. Hand-training. Institutional affairs and care for the inmates. Clinic for feebleminded children. Care and education in institutions and forensic Psychiatry. Juvenile courts. Social care. Speech therapeutics (Articulation). Hygiene. Care for the deaf and dumb, the blind, and cripples.

A series of Schools for Feebleminded, Institutions, Clinics and Scientific Institutes will be visited. The detailed programme will appear in the spring. For particulars address the committee: Privatdozent Dr. H. Vogt, Neurologisches Institut, Gartenstrasse, Frankfurt, a. M., or Rector A. Henze, Wiesenhüttenschule, Frankfurt, a. M.

THE AMERICAN

JOURNAL OF PSYCHOLOGY

Founded by G. STANLEY HALL in 1887

VOL. XX | JULY, 1909 | No. 3

SEXUAL EDUCATION AND NAKEDNESS[1]

By HAVELOCK ELLIS

What is the psychological influence of familiarity with nakedness? How far should children be made familiar with the naked body? This is a question in regard to which different opinions have been held in different ages, and during recent years a remarkable change has begun to come over the minds of practical educationalists in regard to it.

In Sparta, in Chios, and elsewhere in Greece, women at one time practised gymnastic feats and dances in nakedness, together with the men, or in their presence. Plato in his *Republic* approved of such customs and said that the ridicule of those who laughed at them was but "unripe fruit plucked from the tree of knowledge." On many questions Plato's opinions changed, but not on this. In the *Laws*, which are the last outcome of his philosophic reflection in old age, he still advocates the co-education of the sexes, and their co-operation in all the works of life, in part with a view to blunt the over-keen edge of sexual appetite; with the same object he advocated the association together of youths and girls without constraint in costumes which offered no concealment to the form.

It is noteworthy that the Romans, a coarser-grained people than the Greeks, and in our narrow modern sense more "moral," showed no perception of the moralizing and refining influence of nakedness. Nudity to them was merely a licentious indulgence, to be treated with contempt even when it was enjoyed. It was confined to the stage, and clamored for by the popu-

[1] The present article forms a chapter in the sixth and final volume of the author's *Studies in the Psychology of Sex*, "Sex in Relation to Society." (F. A. Davis & Co., Phila., Publishers.)

lace. In the Floralia, especially, the crowd seem to have claimed it as their right that the actors should play naked, probably, it has been thought, as a survival of a folk-ritual. But the Romans, though they were eager to run to the theatre, felt nothing but disdain for the performers. "Flagitii principium est, nudare inter cives corpora." So thought old Ennius, as reported by Cicero, and that remained the genuine Roman feeling to the last. "Quanta perversitas!" as Tertullian exclaimed, "Artem magnificant, artificem notant." In this matter the Romans, although they aroused the horror of the Christians, were yet in reality laying the foundation of Christian morality.

Christianity, which found so many of Plato's opinions congenial, would have nothing to do with his view of nakedness and failed to recognize its psychological correctness. The reason was simple, and indeed simple-minded. The Church was passionately eager to fight against what it called "the flesh," and thus fell into the error of confusing the subjective question of sexual desire with the objective spectacle of the naked form. "The flesh" is evil, therefore "the flesh" must be hidden. And they hid it without understanding that in so doing they had not suppressed the craving for the human form, but on the contrary had heightened it by imparting to it the additional fascination of a forbidden mystery.

Burton, in his *Anatomy of Melancholy* (Part III, Sect. II, Mem. II, Subs. IV), referring to the recommendation of Plato, adds: "But *Eusebius* and *Theodoret* worthily lash him for it; and well they might: for as one saith, the very sight of naked parts *causeth enormous, exceeding concupiscences, and stirs up both men and women to burning lust.*" Yet, as Burton himself adds further on in the same section of his work (Mem. v. Subs. iii), without protest, "Some are of opinion, that to see a woman naked is able of itself to alter his affection; and it is worthy of consideration, saith *Montaigne* the Frenchman in his Essays, that the skillfullest masters of amorous dalliance appoint for a remedy of venereous passions, a full survey of the body."

There ought to be no question regarding the fact that it is the adorned, the partially concealed body, and not the absolutely naked body which acts as a sexual excitant. I have brought together some evidence on this point in the study of "The Evolution of Modesty." It is now generally held that one of the chief primary causes of ornament and clothing was the desire to arouse sexual desire, and artists' models are well aware that when they are completely unclothed they are most safe from undesired masculine advances. "A favorite model of mine told me," remarks Dr. Shufeldt (*Medical Brief*, Oct., 1904), the distinguished author of *Studies of the Human Form*, "that it was her practice to disrobe as soon after entering the artist's studio as possible, for, as men are not always responsible for their emotions, she felt that she was far less likely to arouse or excite them when entirely nude than when only semi-draped." This fact is, indeed, quite familiar to artists' models. If the conquest of sexual desire were the first and last consideration of life it would be more reasonable to prohibit clothing than to prohibit nakedness.

When Christianity absorbed the whole of the European world this strict avoidance of even the sight of "the flesh," although nominally accepted by all as the desirable ideal, could only be carried out, thoroughly and completely, in the cloister. In the practice of the world outside, although the original Christian ideals remained influential, various pagan and primitive traditions in favor of nakedness still persisted and were to some extent allowed to manifest themselves, alike in ordinary custom and on special occasions.

How widespread is the occasional or habitual practice of nakedness in the world generally, and how entirely concordant it is with even a most sensitive modesty has been set forth in "The Evolution of Modesty," in Vol. I of these *Studies*.

Even during the Christian era the impulse to adopt nudity, often with the feeling that it was an especially sacred practice, has persisted. The Adamites of the second century, who read and prayed naked, and celebrated the sacrament naked, according to the statement quoted by St. Augustine, seem to have caused little scandal so long as they only practised nudity in their sacred ceremonies. The French Picards at a much later date insisted on public nakedness believing that God had sent their leader into the world as a new Adam to re-establish the law of nature; they were persecuted and were finally exterminated by the Hussites.

In daily life, however, a considerable degree of nakedness was tolerated during mediæval times. This was notably so in the public baths frequented by men and women together. Thus Alwin Schultz remarks (in his *Höfischs Leben zur Zeit der Minnesänger*), that the women of the aristocratic classes, though not the men, were often naked in these baths except for a hat and a necklace.

It is sometimes stated that in the mediæval religious plays Adam and Eve were absolutely naked. Chambers doubts this and thinks they wore flesh-colored tights, or were as in a later play of this kind "apparelled in white leather". (E. K. Chambers, *The Mediæval Stage*, vol. I, p. 5). It may be so, but the public exposure even of the sexual organs was permitted, and that in aristocratic houses, for John of Salisbury (in a passage quoted by Buckle, *Commonplace Book*, 541) protests against this custom.

The women of the feminist sixteenth century in France, as R. de Maulde la Claviere remarks (*Revue de l'Art*, January, 1898), had no scruple in recompensing their adorers by admitting them to their toilette or even their bath. Late in the century they became still less prudish, and many well-known ladies allowed themselves to be painted naked down to the waist, as we see in the portrait of Gabrielle d'Estrèes au Bain, at Chantilly. Many of these pictures, however, are certainly not real portraits.

Even in the middle of the seventeenth century in England nakedness was not prohibited in public for Pepys tells us that on the 29th July, 1667 a Quaker came iuto Westminster Hall crying "Repent! Repent!" being in a state of nakedness except that he was "very civilly tied about the privities to avoid scandal"

In a Chapter "De la Nudité" and in the appendices of his book *De l'Amour* (vol. I, p. 221) Senancour gives instances of the occasional practice of nudity in Europe and adds some interesting remarks of his own.

The movement of revolt against nakedness never became completely victorious until the nineteenth century. That century represented the triumph of all the forces that banned public nakedness everywhere and altogether. If, as Puda insists, nakedness is aristocratic and the slavery of clothes a plebeian characteristic imposed on the lower classes by an upper class who reserved to themselves the privilege of physical culture, we may perhaps connect this with the outburst of democratic plebeianism which, as Neitzsche pointed out, reached its climax in the nineteenth century. It is in any case certainly interesting to observe that by this time the movement had entirely changed its character. It had become general, but at the same time its foundation had been undermined. It had largely lost its religious and moral character and instead was regarded as a matter of conventiou. The nineteenth century men who encountered the spectacle of white limbs flashing in the sunlight no longer felt like the mediæval ascetic that he was risking the salvation of his immortal soul or even courting the depravation of his morals; he merely felt that it was "indecent" or, in extreme cases, "disgusting". That is to say he regarded the matter as simply a question of conventional etiquette, at the worst, of taste, of æsthetics. In thus bringing down his repugnance to nakedness to so low a plane he had indeed rendered it generally acceptable, but at the same time he had deprived it of high sanction. His profound horror of nakedness was out of relation to the frivolous grounds on which he based it.

We must not, however, underrate the tenacity with which this horror of nakedness was held. Nothing illustrates more vividly the deeply ingrained hatred which the nineteenth century felt of nakedness than the ferocity—there is no other word for it—with which Christian missionaries to savages all over the world, even in the tropics, insisted on their converts adopting the conventional clothing of Northern Europe. Travellers' narratives abound in references to the emphasis placed by missionaries on this change of custom, which was both injurious to the health of the people and degrading to their dignity. It is sufficient to quote one authoritative witness, Lord Stanmore, former Governor of Fiji, who read a long paper to the Anglican Missionary Conference in 1894 on the subject of "Undue Introduction of Western Ways." "In the centre of the village," he remarked in quoting a typical case, "is the church, a wooden barn-like building. If the day be Sunday we shall find the native minister arrayed in a greenish-black swallow-tail coat, a neckcloth, once white, and a pair of spectacles, which he probably does not need, preaching to a congregation the male portion of which is dressed in much the same manner as himself, while the women are dizened out in old battered hats or bonnets, and shapeless gowns like bathing dresses, or it may be in crinolines of an early type. Chiefs of influence and women of high birth, who in their native dress would look, and do look, the ladies and gentlemen they are, are by their Sunday finery given the appearance of attendants upon Jack-in-the-green. If a visit be paid to the houses of the town, after the morning's work of the people is over, the family will be found sitting on chairs, listless and uncomfortable, in a room full of litter. In the houses of the superior native clergy, there will be a yet greater aping of the manners of the West. There will be chairs covered with hideous antimacassars, tasteless round worsted-work mats for absent flower jars, and a lot of ugly, cheap, and vulgar china chimney ornaments, which, there being no fireplace and consequently no chimney piece, are set out in order on a rickety deal table. The whole life of these village folk is one piece of unreal acting. They are continually asking themselves whether they are incurring any of the penalties entailed by infraction of the long table of prohibitions, and whether they are living up to the foreign garments they wear. Their faces have for the most part an expression of sullen discontent, they move about silently and joylessly, rebels in heart to the restrictive code on them, but which they fear to cast off, partly from a vague apprehension of possible secular results, and partly because they suppose they will cease to be good Christians if they do so. They have good ground for their dissatisfaction. At the time when I visited the villages I have specially in my eye, it was punishable by fine and imprisonment to wear native clothing, punishable by fine and imprisonment to wear long hair or a garland of flowers; punishable by fine and imprisonment to wrestle or to play at ball; punishable by fine and imprisonment to build a native-fashioned house; punishable not to wear shirt and trousers, and in certaln localities coat and shoes also; and, in addition to laws enforcing a strictly puritanical observation of the Sabbath, it was punishable by fine and imprisonment to bathe on Sundays. In some other places bathing on Sunday was punishable by flogging; and to my knowledge women have been flogged for no other offence. Men in such circumstances are ripe for revolt, and sometimes the revolt comes."

An obvious result of reducing the feeling about nakedness to an unreasoning but imperative convention is the tendency to prudishness. This, as we know, is a form of pseudo-modesty which, being a convention, and not a natural feeling, is capable of unlimited extension. It is by no means confined to modern times or to Christian Europe. The Turks are capable of prudishness. So indeed were even the ancient Greeks. "Dion the philosopher tells us," remarks Clement of Alexandria (*Stromates*' Bk. IV, ch. 19), "that a certain woman Lysidica, through excess of modesty, bathed in her clothes, and that Philotera, when she was to enter the bath, gradually drew back her tunic as the water covered her naked parts; and then rising by degrees, put it on." Mincing prudes were found among the early Christians and their ways are graphically described by St. Jeremy in one of his Letters. Whenever a new and artificial "modesty" is imposed upon savages prudery tends to arise. Haddon describes this among the natives of Torres Straits, where even the children now suffer from exaggerated prudishness, though formerly absolutely naked and unashamed. (*Cambridge Anthropological Expedition to Torres Straits*, vol. V, p. 271.)

The nineteenth century, which witnessed the triumph of timidity and prudery in this matter, also produced the first fruitful germ of new conceptions of nakedness. To some extent these were embodied in the great romantic movement. Rousseau indeed had placed no special insistence on nakedness as an element of the return to Nature which he preached so influentially. A new feeling in this matter emerged, however, with characteristic extravagance, in some of the episodes of the Revolution, while in Germany in the pioneering *Lucinde* of Friedrich Schlegel, a characteristic figure in the Romantic movement, a still unfamiliar conception of nakedness was set forth in a serious and earnest spirit.

In England, Blake with his strange and flaming genius, proclaimed a mystical gospel which involved the spiritual glorification of the body and contempt for the civilized worship of clothes ("As to a modern man," he wrote, "stripped from his load of clothing he is like a dead corpse"); while later in America Thoreau and Whitman and Burroughs asserted, still more definitely, a not dissimilar message concerning the need of returning to Nature.

We find the importance of the sight of the body—though very narrowly, for the avoidance of fraud, in the preliminaries of marriage—set forth as early as the sixteenth century, by Sir Thomas More in his *Utopia*, which is so rich in new and fruitful ideas. In Utopia, according to Sir Thomas More, before marriage, a staid and honest matron "showeth the woman, be she maid or widow, naked to the wooer. And likewise a sage and discreet man exhibiteth the wooer naked to the woman. At this custom we laughed and disallowed it as foolish. But they on their part do greatly wonder at the folly of all other nations which in buying a colt, where a little money is in hazard, be so chary and circumspect that though he be almost all bare, yet they will not buy him unless the saddle and all the harness be taken off, lest under these coverings be hid some gall or sore. And yet in choosing a wife, which shall be either pleasure or displeasure to them all their life after, they be so reckless that all the residue of the woman's body being covered with clothes, they estimate her scarcely by one handsbreadth (for they can see no more but her face) and so join her to them not without great jeopardy of evil agreeing together, if anything in her body afterward should chance to offend or mislike them. Verily so foul deformity may be hid under these coverings that it may quite alienate and take away the man's mind from his wife, when it shall not be lawful for their bodies to be separate again. If such deformity happen by any chance after their marriage is consummate and finished, well, there is no remedy but patience. But it were well done that a law were made whereby all such deceits were eschewed and avoided beforehand."

The clear conception of what may be called the spiritual value of nakedness—by no means from More's point of view, but as a part of natural hygiene in the widest sense, and as a high and special aspect of the purifying and ennobling function of beauty—is of much later date. It is not clearly expressed until the time of the Romantic Movement at the beginning of the nineteenth century. We have it admirably set forth in Senancour's *De l'Amour* (first edition 1806; fourth and enlarged edition 1834), which still remains one of the best books on the morality of love. After remarking that nakedness by no means abolishes modesty, he proceeds to advocate occasional partial or complete nudity. "Let us suppose," he remarks, somewhat in the spirit of Plato, "a country in which at certain general festivals the women should be absolutely free to be nearly or even quite naked. Swimming, waltzing, walking, those who thought good to do so might remain unclothed in the presence of men. No doubt the illusions of love would be little known, and passion would see a diminution of its transports. But is it passion that in general ennobles human affairs? We need honest attachments and delicate delights, and all these we may obtain while still preserving our common sense. . . . Such nakedness would demand corresponding institutions, strong and simple, and a great respect for those conventions which belong to all times." (Senancour, *De l'Amour*, vol. I, p. 314.)

From that time onwards references to the value and desirability of nakedness become more and more frequent in all civilized countries, sometimes mingled with sarcastic allusions to the false conventions we have inherited in this matter. Thus Thoreau writes in his Journal on 12th June, 1852, as he looks at boys bathing in the river: "The color of their bodies in the sun at a distance is pleasing. I hear the sound of their sport borne over the water. As yet we have not man in Nature. What a singular fact for an angel visitant to this earth to carry back in his note-book, that men were forbidden to expose their bodies under the severest penalties!" Yevan Bloch, in Chapter VII of his *Sexual Life of our Times* discusses this question of nakedness from the modern point of view, and concludes: "A natural conception of nakedness: that is the watchword of the future. All the hygienic, æsthetic and moral efforts of our time are pointing in that direction."

Stratz, as befits one who has worked so strenuously in the cause of human health and beauty, admirably sets forth the stage which we have now attained in this matter. After pointing out (*Die Frauenkleidung*, 3rd ed. 1904, p. 30) that, in opposition to the pagan world which worshipped naked gods, Christianity developed the idea that nakedness was merely sexual and therefore immoral, he proceeds: "But over all glimmered on the heavenly heights of the cross, the naked body of the Saviour. Under that protection there has gradually disengaged itself from the confusion of ideas a new transfigured form of nakedness made free after long struggle. I would call this *artistic nakedness*, for as it was immortalized by the old Greeks through art, so also among us it has been awakened to new life by art. Artistic nakedness is in its nature much higher than either the natural or the sensual conception of nakedness. The simple child of nature sees in nakedness nothing at all; the clothed man sees in the uncovered body only a sensual irritation. But at the highest standpoint man consciously returns to nature, and recognizes that under the manifold coverings of human fabrication there is hidden the most splendid creature that God has created. One may stand in silent worshipping wonder before the sight; another may be impelled to imitate and show to his fellowman what in that holy moment he has seen. But both enjoy the spectacle of human beauty with full consciousness and enlightened purity of thought."

It was not, however, so much on these more spiritual sides, but on the side of hygiene, that the nineteenth century furnished its chief practical contribution to the new attitude towards nakedness.

Lord Monboddo, the Scotch judge, who was a pioneer in regard to many modern ideas, had already in the eighteenth century realized the hygienic value of "air baths" and he invented that now familiar name. "Lord Monboddo," says Boswell in 1777 (*Life of Johnson*, edited by Hill, vol. III, p. 168) "told me that he awaked every morning at four, and then for his health got up and walked in his room naked, with the window open, which he called taking *an air bath*." It is said also, I know not on what authority, that he made his beautiful daughters take an air-bath naked on the terrace every morning. Another distinguished man of the same century, Benjamin Franklin, used sometimes to work naked in his study on hygienic grounds, and, it is recorded, once affrighted a servant girl by opening the door, in an absent-minded moment, thus unattired.

Rikli seems to have been the apostle of air baths and sun baths regarded as a systematic method. He established light and air-baths, over half a century ago, at Trieste and elsewhere in Austria. His motto was: "Light, Truth and Freedom are the motive forces towards the highest development of physical and moral health." Man is not a fish, he declared, light and air are the first conditions of a highly organized life. Solaria for the treatment of a number of different disordered conditions are now commonly established and most systems of natural therapeutics attach prime importance to light and air, while in medicine generally it is beginning to be recognized that such influences can by no means be neglected. Dr. Fernand Sandoz, in his *Introdnction à la Thérapeutique Naturiste par les agents Physiques et Dietétiques* (1907) sets forth such methods comprehensively. In Germany sun-baths have become widely common; thus Lenkei (in a paper summarized in British Medical Journal, 31. Oct. 1908) prescribes them with much benefit in tuberculosis, rheumatic conditions, obesity, anæmia, neurasthenia, etc. He considers that their peculiar value lies in the action of light. Professor J. N. Hyde, of Chicago, even believes "Light-Hunger in the Production of Psoriasis," *British Medical Journal*, 6. Oct. 1906) that psoriasis is caused by deficiency of sunlight and is best cured by the application of light. This belief, which has not, however, been generally accepted in its unqualified form, he ingeniously supports by the fact that psoriasis tends to appear on the most exposed parts of the body, which may be held to naturally receive and require the maximum of light, and by the absence of the disease in hot countries and among negroes.

The hygienic value of nakedness is indicated by the robust health of the savage throughout the world who go naked. The vigor of the Irish, also, has been connected with the fact that (as Fynes Moryson's Itinerary shows) both sexes, even among persons of high social class, were accustomed to go naked except for a mantle, especially in more remote parts of the country, until the seventeenth century. Wherever primitive races abandon nakedness for clothing, at once the tendency to disease, mortality, and degeneracy notably increases, though it must be remembered that the use of clothing is commonly accompanied by the introduction of other bad habits. "Nakedness is the only condition universal among vigorous and healthy savages; at every other point perhaps they differ", remarks Frederick Boyle in a paper ("Savages and Clothes," *Monthly Review*, Sept., 1905) in which he brings together much evidence concerning the hygienic advantages of the natural human state in which man is "all face."

It is in Germany that a return towards nakedness has been most ably and thoroughly advocated, notably by Dr. H. Pudor in his *Nackt Cultur*, and by R. Ungewitter in *Die Nacktheit* (first published in 1905), a book which has had a very large circulation in many editions. These writers enthusiastically advocate nakedness not only on hygienic but on moral and artistic grounds. Pudor insists more especially that "nakedness, both in gymnastics and in sport, is a method of cure and a method of regeneration;" he advocates co-education in this culture of nakedness. Although he makes large claims for nakedness—believing that all the nations which have disregarded these claims have speedily become decadent—Pudor is less hopeful than Ungewitter of any speedy victory over the prejudices opposed to the culture of nakedness. He considers that the immediate task is education and that a practical commencement may best be made with the foot which is specially in need of hygiene and exercise; a large part of the first volume of his book is devoted to the foot.

As the matter is to-day viewed by those educationalists who are equally alive to sanitary and sexual considerations, the claims of nakedness, so far as concerns the young, are regarded as part alike of physical and moral hygiene. The free contact of the naked body with air and water and light makes for the health of the body; familiarity with the sight of the body abolishes petty pruriencies, trains the sense of beauty and makes for the health of the soul. This double aspect of the matter has undoubtedly weighed greatly with those teachers who now approve of customs which a few years ago would have been hastily dismissed as "indecent." There is still a wide difference of opinion as to the limits to which the practice of nakedness may be carried, and also as to the age when it should begin to be restricted. The fact that the adult generation of to-day grew up under the influence of the old horror of nakedness is an inevitable check on any revolutionary changes in these matters.

Maria Lischnewska, one of the ablest advocates of the methodical enlightenment of children in matters of sex (*op. cit.*), clearly realizes that a sane attitude towards the body lies at the root of a sound education for life. She finds that the chief objection encountered in such education as applied in the higher classes of schools is "the horror of the civilized man at his own body." She shows that there can be no doubt that those who are engaged in the difficult task of working towards the abolition of that superstitious horror have taken up a moral task of the first importance.

Walter Gerhard, in a thoughtful and sensible paper on the educational question ("Ein Kapitel zur Erziehungsfrage, *Geschlecht und Gesellschaft*, Vol. I, Heft. II), points out that it is the adult who needs education in this matter—as in so many other matters of sexual enlightenment—considerably more than the child. Parents educate their children from the earliest years in prudery and vainly flatter themselves that they have thereby promoted their modesty and morality. He records his own early life in a tropical land and accustomed to nakedness from the first. "It was not till I came to Germany when nearly twenty, that I learnt that the human body is indecent, and that it must not be shown because that would 'arouse bad impulses.' It was not till the human body was entirely withdrawn from my sight and after I was constantly told that there was something improper behind clothes, that I was able to understand this. . . . Until then I had not known that a naked body, by the mere fact of being naked, could arouse erotic feelings. I had known erotic feelings, but they had not arisen from the sight of the naked body, but gradually blossomed from the union of our souls." And he draws the final moral: that, if only for the sake of our children, we must learn to educate ourselves.

Forel (*Die Sexuelle Frage*, p. 140), speaking in entirely the same sense as Gerhard, remarks that prudery may be either caused or cured in children. It may be caused by undue anxiety in covering their bodies and hiding from them the bodies of others. It may be cured by making them realize that there is nothing in the body that is unnatural and that we need be ashamed of, and by encouraging bathing of the sexes in common. He points out (p. 512) the advantages of allowing children to be acquainted with the adult forms which they will themselves some day assume, and condemns the conduct of those foolish persons who assume that children already possess the adult's erotic feelings about the body. That is so far from being the case that children are frequently unable to distinguish the sex of other children apart from their clothes.

At the Mannheim Congress of the German Society for Combating Venereal Diseases specially devoted to sexual hygiene the speakers constantly referred to the necessity of promoting familiarity with the naked body. Thus Eulenburg and Julian Marcus (*Sexualpedagogik*, p. 264), emphasized the importance of air-baths not only for the sake of the physical health of the young but in the interest of rational sexual training. Höller, a teacher, speaking at the same congress (*op. cit.* p. 85), after insisting on familiarity with the nude in art and literature, and protesting against the bowdlerising of poems for the young, continues: "By bathing-drawers ordinances no soul was ever yet saved from moral ruin. One who has learnt to enjoy peacefully the naked in art is only stirred by the naked in nature as by a work of art." Enderlin, another teacher, speaking in the same sense (p. 58), points out that nakedness cannot act sexually or immorally on the child since the sexual impulse has not yet become pronounced, and the earlier he is introduced to the naked in nature and in art, as a matter of course, the less likely are the sexual feelings to be developed precociously. The child thus, indeed, becomes immune to impure influences, so that, later, when representations of the nude are brought before him for the object of provoking his wantonness they are powerless to injure him. It is important, Enderlin adds, for familiarity with the nude in art to be learnt at school; for most of us, as Siebert remarks, have to learn purity through art.

Nakedness in bathing, remarks Bëlsche in his *Libeslehen in der Natur* (vol. III, pp. 139 *et seq.*), we already in some measure possess; we need it in physical exercises, at first for the sexes separately; then, when we have grown accustomed to the idea, occasionally for both sexes together. We need to acquire the capacity to see the bodies of individuals of the other sex with such self-control and such natural instinct that they become non-erotic to us and can be gazed at without erotic feeling. Art, he says, shows that this is possible in civilization. Science, he adds, comes to the aid of the same view.

Ungewitter (*Die Nachtheit*, p. 57) also advocates boys and girls engaging in play and gymnastics together, entirely naked in air-baths. "In this way", he believes, "the gymnasium would become a school of morality, in which young growing things would be able to retain their purity as long as possible through becoming naturally accustomed to each other. At the same time their bodies would be hardened and devoloped, and the perception of beautiful and natural forms awakened." To those who have any "moral" doubts on the matter he mentions the custom in remote country districts of boys and girls bathing together quite naked and without any sexual consciousness. Rudolf Sommer, similarly, in an excellent article entitled Mädchenerziehung oder Menschenbildung?" (*Gesehlecht und Gesellschaft*, Bd. I, Heft 3) advises that children should be made accustomed to each other's nakedness from an early age in the family life of the house or the garden, in games and especially in bathing: he remarks that parents having children of only one sex should cultivate for their children's sake intimate relations with a family having children of like age of the opposite sex, so that they may grow up together.

It is scarcely necessary to add that the cultivation of nakedness must always be conciliated with respect for the natural instincts of modesty. If the practice of nakedness led the young to experience a diminished reverence for their own or others' personalities the advantages of it would be too dearly bought. This is in part a matter of wholesome instinct, in part of wise training. We now know that the absence of clothes has little relation with the absence of modesty, such relation as there is being of the inverse order, for the savage races which go naked are usually more modest than those which wear clothes. The saying quoted by Herodotus in the early Greek world that "A woman takes off her modesty with her shift" was a favorite text of the Christain Fathers. But Plutarch, who was also a moralist, had already protested against it at the close of the Greek world: "By no means," he declared; "she who is modest clothes herself with modesty when she lays aside her tunic." "A woman may be naked," as Mrs. Bishop, the traveller, remarked to Dr. Baelz in Japan, "and yet behave like a lady."

The question is complicated among ourselves because established traditions of rigid concealment have fostered a pruriency which is an offensive insult to naked modesty. In many lands the women who are accustomed to be almost or quite naked in the presence of their own people cover themselves as soon as they become conscious of the lustful inquisitive eyes of Europeans. Stratz refers to the prevalence of this impulse of offended modesty in Japan, and mentions that he himself failed to arouse it because he was a physician, and moreover. had long lived in another land (Java) where also the custom of nakedness prevails.[1] So long as this unnatural prurience exists a free unqualified nakedness is rendered difficult or impossible.

Modesty, is not, however, the only natural impulse which has to be considered in relation to the custom of nakedness. It seems probable that in cultivating the practice of nakedness we are not merely carrying out a moral and hygienic prescription but allowing legitimate scope to an instinct which at some periods of life, especially in adolescence, is spontaneous and natural, even, it may be, wholesomely based in the traditions of the race in sexual selection. Our rigid conventions make it impossible for us to discuss the laws of nature in this matter by stifling them at the outset. It may well be that there is a rhythmic harmony and concordance between impulses of modesty and impulses of nakedness, though we have done our best to disguise the natural law by our stupid and perverse by-laws.

Stanley Hall, who emphasizes the importance of nakedness, remarks that at puberty we have much reason to assume that in a state of nature there is a certain instinctive pride and ostentation that accompanies the new local development, and quotes the observation of Dr. Seerley that the impulse to conceal the sexual organs is especially marked in young men who are under-developed but not evident to those who are developed beyond the average. Stanley Hall (*Adolescence*, Vol. II, p. 97) also refers to the frequency with which not only "virtuous young men, but even women, rather glory in occasions when they can display the beauty of their forms without reserve, not only to themselves and to loved ones, but even to others with proper pretexts."

Many have doubtless noted this tendency, especially in women, and chiefly in those who are conscious of beautiful physical development. Madame Celine Renooz believes that the tendency corresponds to a really deep-rooted instinct in women little or not at all manifested in men who have consequently sought to impose artificially on women their own masculine conceptions of modesty. "In the actual life of the young girl to-day there is a moment when by a secret atavism she feels the pride of her sex, the intuition of her moral superiority and cannot understand why she must hide its cause. At this mo-

---

[1]C. H. Stratz: *Die Körperformen en Kunst und Leben der Japoner*, 2nd ed. ch. III; *id. Frauenkleidung*, *3rd* ed., pp. 22, 30.

ment, wavering between the laws of nature and social conventions, she scarcely knows if nakedness should or should not affright her. A sort of confused atavistic memory recalls to her a period before clothing was known, and reveals to her as a paradisiacal ideal the customs of that human epoch." (Celine Renooz, *Psychologie Comparée de l'Homme et de la Femme*, pp. 85-87.)

From the point of view with which we are here essentially concerned there are three ways in which the cultivation of nakedness—so far as it is permitted by the slow education of public opinion — tends to exert an influence. (1) It is an important element in the sexual hygiene of the young, introducing a wholesome knowledge and incuriosity into a sphere once given up to prudery and pruriency; (2) The effect of nakedness is beneficial on those of more mature age, also, in so far as it tends to cultivate the sense of beauty and to furnish the tonic and consoling influences of natural vigor and grace; (3) The custom of nakedness, in its inception at all events, has a dynamic psychological influence also on morals, an influence exerted in the substitution of a strenuous and positive morality for the merely negative and timid morality which has ruled in this sphere.

Perhaps there are not many adults who realize the intense and secret absorption of thought in the minds of many boys and some girls concerning the problem of the physical conformation of the other sex, and the time, patience and intellectual energy which they are willing to expend on the solution of this problem. This is mostly effected in secret, but not seldom the secret impulse manifests itself with a sudden violence which in the blind eyes of the law is reckoned as crime. A German lawyer, Dr. Werthauer, has lately stated that if there were a due degree of familiarity with the natural organs and functions of the opposite sex ninety per cent. of the indecent acts of youths with girl children would disappear for in most cases these are not assaults but merely the innocent though uncontrollable outcome of a repressed natural curiosity. It is quite true that not a few children boldly enlist each other's co-operation in the settlement of the question and resolve it to their mutual satisfaction. But even this is not altogether satisfactory, for the end is not attained openly and wholesomely with a due subordination of the specifically sexual, but with a consciousness of wrong doing and an exclusive attentiveness to the merely physical fact which tend directly to develop sexual excitement. When familiarity with the naked body of the other sex is gained openly and with no consciousness of indecorum in the course of work and of play, in exercise or gymnastics, in running or in bathing, from a child's earliest years, no un-wholesome results accompanying the familiarity with the essential facts of physical conformation thus naturally acquired. The prurience and prudery, which have poisoned sexual life in the past, are alike rendered impossible.

Nakedness has, however, a hygienic value, as well as a spiritual significance, far beyond its influences in allaying the natural inquisitiveness of the young or acting as a preventive of morbid emotion. It is an inspiration to adults who have long outgrown any youthful curiosities. The vision of the essential and eternal human form, the nearest thing to us in all the world, with its vigor and its beauty and its grace, is one of the prime tonics of life. "The power of a woman's body," said James Hinton, "is no more bodily than the power of music is a power of atmospheric vibrations." It is more than all the beautiful and stimulating things of the world, than flowers, or stars or the sea. History and legend and myth reveal to us the sacred and awful influence of nakedness, for, as Stanley Hall says, nakedness has always been "a talisman of wondrous power with gods and men." How sorely men crave for the spectacle of the human body—even to-day after generations have inculcated the notion that it is an indecorous and even disgusting spectacle—is witnessed by the eagerness with which they seek after the spectacle of even its imperfect and meretricious forms, although these certainly possess a heady and stimulating quality which can never be found in the pathetic simplicity of naked beauty. It was another spectacle when the queens of ancient Madagascar at the annual Fandroom, or feast of the bath, laid aside their royal robes and in the presence of their subjects who crowded the palace courtyard, descended the marble steps to the bath in complete nakedness. When we make our conventions of clothing rigid we at once spread a feast for lust and deny ourselves one of the prime tonics of life. "I was feeling in despair and walking despondently along a Melbourne street," writes the Australian author of a yet unpublished autobiography, "when three children came running out of a lane and crossed the road in full daylight. The beauty and texture of their legs in the open air filled me with joy, so that I forgot all my troubles whilst looking at them. It was a bright revelation, an unexpected glimpse of paradise, and I have never ceased to thank the happy combination of shape, pure blood and fine skin of these poverty-stricken children, for the wind seemed to quicken their golden beauty and I retained the rosy vision of their natural young limbs, so much more divine than those always under cover. Another occasion when naked young limbs made me forget all my gloom and despondency was on my first visit to Adelaide. I came on a naked boy leaning on the railing near the Baths, and the beauty of his face, torso, fair young limbs and exquisite feet filled me with joy and renewed hope. The tears came to my eyes and I said to myself 'While there is beauty in the world I will continue to struggle.' "

We must, as Bölsche declares (*loc. cit.*), accustom ourselves to gaze on the naked human body exactly as we gaze at a beautiful flower, not merely with the pity with which the doctor looks at the body, but with joy in its strength and health and beauty. For a flower, as Bölsche truly adds, is not merely "naked body", it is the most sacred region of the body, the sexual organs of the plant.

"For girls to dance naked", said Hinton, "is the only truly pure form of dancing, and in due time it must therefore come about. This is certain: girls will dance naked and men will be pure enough to gaze on them." It has already been so in Greece, he elsewhere remarks, as it is to-day in Japan (as more recently described by Stratz). It is nearly forty years since these prophetic words were written, but Hinton himself would probably have been surprised at the progress which has already been slowly (for all true progress must be slow) towards this goal. Even on the stage new and more natural traditions are beginning to prevail in Europe. It is not many years since an English actress regarded as a calumny the statement that she appeared on the stage barefoot, and brought an action for libel, winning substantial damages. Such a result would scarcely be possible to-day. The movement in which Isadora Duncan was a pioneer has led to a partial disuse among dancers of the offensive device of tights, and it is no longer considered indecorous to show many parts of the body which it was formerly usual to cover.

Attempts have here and there been quietly made to cultivate a certain amount of mutual nakedness as between the sexes on remote country excursions. It is significant to find a record of such an experiment in Ungewitter's *Die Nacktheit*. In this case a party of people, men and women, would regularly every Sunday seek remote spots in woods or meadows where they would settle down, picnic, and enjoy games. "They made themselves as comfortable as possible, the men laying aside their coats, waistcoats, boots and socks, the women their blouses, skirts, shoes and stockings. Gradually as the moral conception of nakedness developed in their minds, more and more clothing fell away, until the men wore nothing but bathing drawers and the women only their chemises. In this 'costume' games were carried out in common, and a regular camp life led. The ladies (some of whom were unmarried) would then lie in hammocks and we men on the grass, and the intercourse was delightful. We felt as members of one family, and behaved accordingly. In an entirely natural and unembarrassed way we gave ourselves up entirely to the liberating feelings aroused by this light and air bath, and passed these splendid hours in joyous singing and dancing, in wantonly childish fashion, freed from the burden of a false civilization. It was of course necessary to seek spots as remote as possible from high-roads, for fear of being disturbed. At the same time we by no means failed in natural modesty and consideration towards one another. Children, who can be entirely naked, may be allowed to take part in such meetings of adults and will thus be brought up free from morbid prudery.'' (R. Ungewitter, *Die Nacktheit*, p. 58.)

No doubt it may be said that the ideal in this matter is the possibility of permitting complete nakedness. This may be admitted, and it is undoubtedly true that our rigid police regulations do much to artificially foster a concealment in this matter which is not based on any natural instinct. Dr. Shufeldt narrates in his delightful *Studies of the Human Form* that once in the course of a photographic expedition in the woods he came upon two boys, naked except for bathing drawers, engaged in getting water lilies from a pond. He found them a good subject for his camera, but they could not be induced to remove their drawers, by no means out of either modesty or mock-modesty, but simply because they feared they might possibly be caught and arrested. We have to recognize that at the present day the general popular sentiment is not yet sufficiently educated to allow of public disregard for the convention of covering the sexual centres, and all attempts to extend the bounds of nakedness must show a due regard for this requirement. As concerns women Valentin Lehr, of Freiburg in Breisgau has invented a costume (figured in Ungewitter's *Die Nacktheit*) which is suitable for either public water-baths or air-baths, because it meets the demand of those whose minimum requirement is that the chief sexual centres of the body should be covered in public, while it is otherwise fairly unobjectionable. It consists of two pieces, made of porous material, one covering the breasts with a band over the shoulders, and the other covering the abdomen below the navel and drawn between the legs. This minimal costume while neither ideal nor æsthetic, adequately covers the sexual regions of the body, while leaving the arms, waist, hips and legs entirely free.

There finally remains the moral aspect of nakedness. Although this has been emphasized by many during the past half century it is still unfamiliar to the majority. The human body can never be a little thing. The wise educator may see to it that boys and girls are brought up in a natural and wholesome familiarity with each other, but a certain terror and beauty must always attach to the spectacle of the body, a mixed attraction and repulsion. Because it has this force it naturally calls out the virtue of those who take part in the spectacle, and makes impossible any soft compliance to emotion. Even if we admit that the spectacle of nakedness is a challenge to passion it is still a challenge that calls out the ennobling qualities of self-control. It is but a poor sort of virtue that lies in fleeing into the desert from things that we fear may have in them a temptation. We cannot dispense with passions if we would: reason, as Holbach said, is the art of choosing the right passions, and education the art of sowing and cultivating them in human hearts. The spectacle of nakedness has its moral value in teaching us to learn to enjoy what we do not possess, a lesson which is an essential part of the training for any kind of fine social life. The child has to learn to look at flowers and not pluck them; the man has to learn to look at a woman's beauty and not desire to possess it. The joyous conquest over that "erotic kleptomania," as Ellen Key has well said, reveals the blossoming of a fine civilization. We fancy the conquest is difficult, even impossibly difficult. But it is not so. This impulse, like other human impulses, tends under natural conditions to develop temperately and wholesomely. We artificially press a stupid and brutal hand on it, and it is driven into the two unnatural extremes of repression and license, one extreme as foul as the other.

To those who have been bred under bad conditions, it may indeed seem hopeless to attempt to rise to the level of the Greeks and the other finer tempered peoples of antiquity in realizing the moral, as well as the pedagogic, hygienic and æsthetic advantages[1] of admitting into life the spectacle of the naked human body. But unless we do we hopelessly fetter ourselves in our march along the road of civilization, we deprive ourselves at once of a source of moral strength and of

[1] I have not considered it in place here to emphasize the æsthetic influence of familiarity with nakedness. The most æsthetic nations (notably the Greeks and the Japanese) have been those that preserved a certain degree of familiarity with the naked body. "In all arts," Maeterlinck remarks, "civilized peoples have approached or departed from pure beauty according as they approached or departed from the habit of nakedness." Ungewitter insists on the advantage to the artist of being able to study the naked body in movement, and it may be worth mentioning that Fidus (Hugo Höppener), the German artist of to-day who has exerted great influence by his fresh, powerful and yet reverent delineation of the naked human form in all its varying aspects, attributes his inspiration and vision to the fact that as a pupil of Diefenbach he was accustomed with his companions to work naked in the solitudes outside Munich which they frequented (F. Enzensberger, "Fidus," *Deutsche Kultur*, Aug., 1906).

joyous inspiration. Just as Wesley once asked why the devil should have all the best tunes, so to-day men are beginning to ask why the human body, the most divine melody at its finest moments that creation has yielded, should be allowed to become the perquisite of those who lust for the obscene. And they are further convinced that by enlisting it on the side of purity and strength they are raising the most powerful of all bulwarks against the invasion of a vicious conception of life and the consequent degradation of sex. These are considerations which we cannot longer afford to neglect, however great the opposition they arouse among the unthinking.

"Folk are afraid of such things rousing the passions," remarks Edward Carpenter. "No doubt the things may act that way. But why, we may ask, should people be afraid of rousing passions which, after all, are the great driving forces of human life?" It is true, the same writer continues, our conventional moral formulæ are no longer strong enough to control passion adequately, and that we are generating steam in a boiler that is cankered with rust. "The cure is not to cut off the passions, or to be weakly afraid of them, but to find a new, sound, healthy engine of general morality and common sense within which they will work." (Edward Carpenter, *Albany Review*, Sept., 1907.)

So far as I am aware, however, it was James Hinton who chiefly sought to make clear the possibility of a positive morality on the basis of nakedness, beauty, and sexual influence, regarded as dynamic forces which when suppressed make for corruption and when wisely used serve to inspire and ennoble life. He worked out his thoughts on this matter in MSS., written from about 1870 to his death two years later, which, never having been prepared for publication, remain in a fragmentary state and have not been published. I quote a few brief characteristic passages: "Is not," he wrote, "the Hindu refusal to see a woman eating strangely like ours to see one naked? The real sensuality of the thought is visibly identical. . . . Suppose, because they are delicious to eat, pineapples were forbidden to be seen, except in pictures, and about that there was something dubious. Suppose no one might have sight of a pineapple unless he were rich enough to purchase one for his particular eating, the sight and the eating being so indissolubly joined. What lustfulness would surround them, what constant pruriency, what stealing! . . . Miss —— told us of her Syrian adventure and how she went into a wood-carver's shop and he would not look at her; and how she took up a tool and worked, till at last he looked, and they both burst out laughing. Will it not be even so with our looking at women altogether? There will come a *work*—and at last we shall look up and both burst out laughing. . . . When men see truly what is amiss, and act with reason and forethought, in respect to the sexual relations, will they not insist on the enjoyment of women's beauty by youths, and from the earliest age that the first feeling may be of beauty? Will they not say, 'We must not allow the false purity, we must have the true? The false has been tried, and it is not good enough; the power purely to enjoy beauty must be gained, attempting to do with less is fatal! Every instructor of youth shall say: 'This beauty of woman, God's chief work of beauty, it is good for you to see it; it is a pleasure that serves good; all beauty serves it, and above all this, for its office is to make you pure. Come to it as you come to daily bread, or pure air, or the cleansing bath: this is pure to you if you be pure, it will aid you in your effort to be so. But if any of you are impure, and make of it the feeder of impurity, then you should be ashamed and pray; it is not for you our life can be ordered; it is for men and not for beasts.' This must come when men open their eyes, and act coolly and with reason and forethought, and not in mere panic, in respect to the sexual passion in its moral relations."

RETENTIVENESS IN CHILD AND ADULT[1]

By Cephas Guillet

CONTENTS

---

[1]I wish heartily to thank Mr. Hikozo Kakise for conducting the experiments with the Japanese names; also Dr. Sanford for helpful criticism.

I. Birds Memorized by Child

"Here's another book for you from Santa Claus," I said to my two-year-old boy the day after Christmas. "Aw-w-w," he said, and chuckled all the way up stairs. It was a bird book, containing fifty-eight excellent, highly colored pictures of forty-eight different kinds of birds. I showed it to him and he said over all the names after me. He made me go over it all again, and this time he knew three of them, the rooster, blackbird and chickadee. The next day these proved to be the only ones retained. But the third day and nearly every succeeding day he had added new birds to his store, till the twenty-fourth, when he named all correctly. During this time he was actually at work (or rather at play, for we did not allow it to degenerate into lessons) for five and a half hours. This means that on the average each bird required 5.7 minutes, and each name 6.9 minutes to be learned. The canary, parrot, robin and thrush occurred twice in different sizes and positions, and there were two ducks, two grosbeaks, two wrens and three chickadees of different species.

The periods averaged nearly fourteen minutes, being a little shorter at the beginning and at the end than at the middle of the experiment; at the beginning, because, knowing but few birds, he simply named most of them after me; toward the end, because he had become familiar enough with most of them to say them readily. We urged him very little and generally accepted his "I cawt say't," whether he meant I can't remember it or I can't repeat it. The longest periods were the 9th and 10th which each lasted twenty minutes. The length of time taken, of course, depended somewhat on his willingness, which in turn depended on his physical condition. On the 7th day, for example, at 10.45 A. M., after a run on the meadow, he felt good and showed great interest, saying all the words in about ten minutes; whereas the 9th day at 8.30 A. M., just after breakfast, he got tired and unwilling toward the end, refused to pronounce a half dozen of the last names and was at it twenty minutes. On the 19th day at 6 P. M., after supper, when he felt in particularly good spirits, he did not say "I can't" once and his reactions were generally very prompt. He knew most of the birds well and took but ten minutes to say them. The 24th day he required only six minutes to name all the birds without prompting and the day after he went through the book correctly in five minutes. He was never allowed to see the book except during these periods and we never suggested the birds to him, though naturally he sometimes recalled them to mind and spoke of them.

A week later he was found still to know all except one (redbird), but required ten minutes to repeat them, showing hesitation in recalling a few, and particularly redbird and yellowbird. Five days later, he still knew all but one, the same one, and said them in seven minutes. It is strange that the name forgotten had been learned on the sixth day and always thereafter said correctly. But he had been learning the colors lately and evidently was applying this imperfect knowledge to some of the birds instead of trusting his memory simply; for he called the redbird as well as the yellowbird "whitebird", while yellow warbler and redstart were recalled readily. Yellowbird was finally recalled but not redbird. He seems to have begun to analyze these two names but not the two former. I was perhaps responsible for this, for on the 21st day, the day after he had first said yellowbird correctly, when he hesitated at this bird, I asked "what color is that?" He replied "whitebird." (Thereafter he always said whitebird for yellowbird before he gave it correctly.) So now when I asked him what color the redbird was, pointing it out, he said first white, then blue, and finally red, but even then would not say redbird. On the last occasion above mentioned he had the same difficulty with the redbird as on the previous occasion, calling it whitebird again, and he called the redstart simply "start," something he had never done before, though he had always had difficulty with it, not having learned it until the 15th day, and thereafter failing to say it again until the last day. It was indeed the only bird he missed on the day before the last. When asked what color the redbird was, he said red correctly this time, but then when asked what the bird was he said "redstart", though with some uncertainty. When now shown the redstart, he said redstart at once; but I could not get him to name redbird correctly. Three days later he called the redbird "whitebird" but told me the color correctly when asked. Yet when I said "What bird is it?" he replied "I don't know", although he named the redstart correctly at sight. Does this not illustrate the difficulty of trying to teach a child anything formally? Conscious effort would seem to be confusing. I have noticed in general that while the boy now uses the words red and blue correctly, yet when asked "What color is this?" he is quite likely to say blue for red and red for blue, white for black and black for white. The question seems to make him self-conscious, disconcerting him and affecting the correctness of his response. Of course later on when this association has been well established, so simple a question will not confuse him, but will not one more difficult, but relatively to his stage of development equally simple, be just as liable to disconcert?

The following table shows on each day the number of bird-names thus far learned, the number of birds that had been already learned, the new birds added that day, the birds forgotten that day (denoted by minus sign), and the total number of birds named that day:

1st day, three species (no birds previously known) blackbird, chickadee, rooster	3
2nd day, 3 species, (3 birds previously known)	3
3rd day, 8 species (3 birds previously known), cuckoo, duck (2) chickadee (2nd), kingfisher, crow, umbrella-bird	10
4th day, 10 species (10 birds previously known), parrot (2), owl, chickadee (3rd), —duck (2nd)	13
5th day, 15 species (14 birds previously known), albatross, grosbeak, doves, swallows, flamingoes,—rooster, parrot	17
6th day, 17 species (19 birds previously known), grosbeak (1st), macaw, redbird, —doves	21
7th day, 20 species (22 birds previously known), canaries, jackdaw, quail	25
8th day, 22 species (25 birds previously known), lark, nightingale, —canaries, doves	25
9th day, 25 species (27 birds previously known), hawk, lovebirds, martin, swallow, —canaries, doves, lark, nightingale	27

10th day, 25 species (31 birds previously known), —canaries, doves, hawk, lark, nightingale 26
11th day, 27 species (31 birds previously known), canary, ibis, pigeon, —canaries, doves, hawk, jackdaw, nightingale 29
12th day, 30 species (34 birds previously known), goldfinch, hummingbird, thrush (2), hawk 37
13th day, 36 species (38 birds previously known), jay, nuthatch, ostrich, robin (1st), vireo, vulture, —doves, hummingbirds, goldfinch 41
14th day, 38 species (44 birds previously known), stork, wren (2nd), —doves, vulture 44
15th day, 40 species (46 birds previously known), redstart, heron, —vulture 47
16th day, 40 species (48 birds previously known), hawk, hummingbirds, redstart, wren (2nd) 44
17th day, 41 species (48 birds previously known), yellow warbler, —doves, hawk, redstart, thrush (2nd), vulture 44
18th day, 42 species (49 birds previously known), cassowary, robin (2nd), —hawk, redstart, yellow warbler 48
19th day, 46 species (51 birds previously known), eagle, wren, magpie, partridge, yellowbird, —hawk, redstart, yellow warbler 52
20th day, 46 species (56 birds previously known), —hawk, redstart, yellow warbler, robin (2nd) 52
21st day, 47 species (56 birds previously known), secretary-bird, —redstart, robin (2nd) 55
22nd day, 48 species (57 birds previously known), stormy petrel, —redstart, yellowbird 56
23rd day, 48 species (58 birds previously known), —redstart 57
24th day, 48 species (58 birds previously known) 58

Between the 17th and 18th days a day intervened upon which no trial was made. He was on a visit to a neighboring city that day and the two preceding, and it was found that the presence of strangers and strange surroundings somewhat excited him and interfered with the naïveté of his behavior. This was plainly mirrored in the fact that on each of these days he named only 44 birds, although the previous day he had named 47 at home and the day after he returned he named 48. There was only one other instance of his losing ground, namely the 10th day, when he named one less than the previous day.

It will be seen that the child added on an average about 2½ birds per day to his store, and that the number of *new birds* added each day fluctuated between none (3 times) and seven (once). It cannot be said that there was a slowing down of the speed of acquisition in the latter part of the time. There were 3 apexes of acquisition, namely on the 3rd, 12th, and 19th days (7, 8, and 5 net *additional* birds as compared with the net number recalled on the previous day). After each of these the curve fell with considerable regularity until the next almost abrupt rise. It reached its lowest points, in fact, on the 8th and 10th, the 16th and 17th and the 20th, 22nd, 23rd, 24th. The curve of acquisition of new bird-*names* was much the same, reaching its highest point, however (6), one day later than the other curve. The acquisition was slow at the end, probably because there were few new birds left to learn and these very difficult (secretary-bird and stormy petrel).

Some names were particularly easy of acquirement, others particularly hard. Of the easy ones rooster was a word already known and the bird was known; the word blackbird had been heard, but had not been associated with a bird; chickadee proved an attractive sound, as it was learned immediately, though never heard before. Cuckoo was very nearly his way of saying cookie (coocoo), so he possessed the word and only needed to get a new association for it, which proved easy; umbrella-bird was easily learned for a similar reason, the oddly-shaped crest of this big bird doubtless helping. Duck, crow, and kingfisher had all been heard and used of the live birds, the last two the previous summer, and the first frequently since then. None of these birds, once learned, ever failed to be recalled, except duck and rooster once, and this last was probably due to willfulness or playfulness, a factor that must be reckoned with in studying children. Other birds that proved easy to learn and recall are owl, parrot, albatross, grosbeak, swallows, flamingoes, macaw, and redbird, all of which are in the first half of the book except the last two.

On the ninth day when he was still unable to recall lark, I substituted the name sparrow because the English lark as pictured looks much like a sparrow, a more useful word for him. He would not pronounce it, however, saying "I cawt". The next day he again refused, but the following day he repeated "payo". Yet the day after, when asked to name it, he recalled its original name, saying "lock". This is an interesting case of the persistence of an association. He failed, however, to recall it the two following days, but never thereafter.

Words particularly hard to learn were redstart, stormy petrel, secretary-bird, magpie, hawk, partridge, yellowbird, eagle, wren (1st), robin (2nd), cassowary, doves and yellow warbler, all of which were in the second half of the book except eagle, hawk, doves and wren. I called the second wren "Jenny Wren" and the first robin "Robin Redbreast", while the others were named simply wren and robin. The longer name proved considerably easier to learn and remember in both cases. I did not call the second wren, which was our common house wren, "Jenny Wren" until the seventh day. He always kept these two names in their first association, but, though he did this at first in the case of the two robins, he finally applied "Rob Rebest" to both. The fact of the hawk and eagle occurring together on the same page and resembling each other retarded their acquisition. The same was true of the English robin and redstart and the martin and magpie. In the last case the names also are somewhat similar, especially as he called the martin "mockin". When, of a word that he could not recall, the first letter or syllable was sounded, he could usually finish it, but such cases were not counted. We frequently found that a little encouragement was helpful. When he would say "I can't", our "O yes you can, that's a—" would often bring the name. A coaxing request also several times induced successful effort. The "I can'ts" were much more numerous in the second half of the book.

Adding together the number of days each word took to be learned and dividing by the number of birds, I find that the first 29 birds in the book took on an average 7.6 days to learn, and the last 29 birds 12.2 days, or, taking the bird-names only, the first 24 bird-names required an average of 7.8 days, the last of 12.1 days each, showing clearly the effects of fatigue in retarding the acquisition of the associations occurring latest. We always began at the beginning of the book except on one day, the 12th, when we began at the end. On this day the child was remarkably successful, making the largest gain of any day during the experiment. The previous day he could recall 29 names, this day he recalled 37.

He is very fond of seeing me draw cats and mice and frequently says "Mate a kitty, dado". On the ninth day of our experiment he began to ask me to "mate a umbella-bi(r)d", "Mate a tush-bird", "Mate a nightingale singin away", "Mate a gosbeat-a pawt". It is curious that though he had never yet succeeded in recalling the name "thrush" during the regular lessons, yet he here used the word and in an original form (tush bird). He had kept up his interest in this exercise for 23 minutes, when we were interrupted. On six other occasions during the course of the experiment he called for the drawing of certain birds and this no doubt helped to fix some of names in his mind. Those asked for besides the ones above mentioned were in order kingfisher (12th day), chickadee and owl (the day of no trial), vulture, hawk, macaw, nuthatch (18th day), duck (21st), blackbird, ostrich (22nd). All these were learned early and retained well except "hawk". The umbrella bird and grosbeak were called for on five of the seven occasions, none of the others more than three times. During the week that he was not allowed to see the book, he asked once for me to draw the ibis, eagle, and hawk. I, of course, drew only those that were asked for. One day he commanded "Mate a caw, mate a caw, mate a *me*-caw, a *me*-caw". When I drew the macaw, he said "At 's a hawk". So I determined to take a few more lessons from the book before I should be called on again by this critic of two years and three months!

He would sometimes use bird names in his prattle and his sing-song. This I observed six times during the period when he was acquiring them, the names so used being umbrella-bird, flamingo, chickadee, and grosbeak twice, and macaw, warbler, rooster and jenny wren once. He once said "I 'm a grosbeak, I 'm a macaw, I 'm a chickadee," and one night he half awoke from sleep and asked dreamily, "Wha's the hummin-bi(r)ds?" Three days after he had learned all, as he lay in bed after waking, he said over to himself fourteen of the names. The same day, pointing first to me and then to a little friend, he said, "At a pawt and dis a gosbeak." Then putting his hand on my nose, he said, "At 's a beak," then he added "a hawk." The next day, turning his back towards me, he said, "Look at my tail." These are rather remarkable applications of his bird-lore. He added on the last occasion, "I 'm a jay; I 'm two love-birds." There are two love-birds in his book—"kissin togedda," as he expressed it in his original way the day before. We had told him they were kissing each other. Two days later he asked his mother, "What does the redstart say?" His mother passed the question on to me, but, before I could answer, he said, "see me, see me, see me;" I had told him that fully a week before.

The following proved to be difficult where not impossible sounds throughout the experiment: the simple consonants r, v, y, th; the combinations represented by kw, sw,-ing, st, sp, -tsh, j,-dg,-dsh, wh; and many compounds of l and r with other consonants (lb, bl, fl, ld, sl, kl, tr, gr, str, kr, thr, br, rk, rd, rt, rbl). The following were fairly mastered during the experiment: lb, kw, j, x,-dg,-tsh, st, th, -vs, ch. The child prefers to accent the first syllable, as fam'ingoes, me'caw, no doubt by analogy with most English words. The child used b for v, f for sw, d for j, p for sp, t for st, sh for tsh, w for r, f or t or later th for thr, and initial y was omitted.

On Feb. 12th, for nearly half an hour, beginning at 1.45 P. M., I had the child name the birds whose heads I showed him, covering all the rest of the bird, and indeed of the book, with papers in which I had cut holes of different sizes to show the heads. I did not show him two birds consecutively on the same page but skipped about through the book. He would run away after each and come at my call with great glee thinking it a fine game. After I had tested him on 23 birds, fearing fatigue, I desisted and finished the remaining 32 in the evening in 50 minutes. He named all correctly but 10, namely vulture (called swallow), jackdaw (magpie), thrush (dove), redbird (yellowbird), nightingale (canary), yellow warbler (yellow bird), lark (thrush), swallow (martin), ostrich (partridge), cuckoo. I tested his mother in the same way and she missed the following fifteen: jackdaw (called blackbird and stormy petrel), magpie (blackbird, crow), blackbird (crow), redstart, yellow warbler (thrush), lark (nightingale), dove (cuckoo), swallow 1st and 2nd, chickadee, duck, martin (swallow), cuckoo, partridge (pigeon), chickadee, 2nd. The duck was an English grebe. The child's responses were almost invariably prompt, in fact instantaneous. For the robin he first said "canady-bird," but corrected himself.

On Feb. 14th and 15th I tested his ability to name the birds minus their heads and all surroundings. Of the 58 birds he was able to name correctly all but two, the ostrich and the first grosbeak (called pigeon). The ostrich was called partridge, just as when only the head was shown, but I think that in both cases this was purely a confusion of the names, since he named it correctly next day when a similar trial was made. However, he also named the grosbeak correctly next day. This bird and the pigeon are much the same size and form in the book, but the pigeon is pink while the grosbeak is gray. The head evidently helped him most with the grosbeak, on account of the large red beak, for, when I excluded everything but the beak, he said "grosbeak," after exclaiming, "Any (*i. e.*, no) head on it!" which he sometimes did of birds without heads. It is interesting to note that when I showed him the pigeon without its head the day after he had called the grosbeak pigeon, he called the pigeon grosbeak at first and then pigeon. From this it would appear that form is more noted by this child than color. The other headless birds that were only named correctly at the second guess were redbird (called yellowbird), lark (humming-bird), hawk (grosbeak), vireo (wren), and a flying canary (sparrow). These mistakes also suggest that color is of minor importance to this child at present as a distinguishing mark. While he named the headless jackdaw correctly the first day, yet the next day, when I showed it to him again, he first called it magpie and then jackdaw. Now the magpie and jackdaw resemble each other very closely except in color. Again, the heads of these birds are of exactly the same color (a blue black), yet he only confused them once out of four times, being shown both heads on two successive days. The jackdaw's head is a little larger and is turned in the opposite direction from the magpie's, which are the only clues he could have, seeing that the bills are alike as well as the shape of the head. The eyes are a little different. I carefully covered the spoon the magpie carried in his bill. That action is of marked interest to this child is shown by his calling the heads of the first robin and the nightingale canary. All these birds are represented as singing with their heads turned up and their beaks open. It would seem as though action played a more important part than color in his perceptions. Similarly the singing lark's head was called thrush, which is also represented as singing; his mother called it nightingale. Again the headless lark was called hummingbird. Here there is no resemblance except in the wings being outspread in flight in both. The color is quite different (mauve and brown). Moreover the headless flying canary was called "sparrow," by which he meant lark. One questions whether children note distinctions of color consciously before their attention is called to them and the names given. We know how many things escape ourselves until we have been trained to observe them, for instance, the many curious and brilliantly-colored mushrooms in the woods. Even as I write, this child, now two years and a half, though he uses correctly black, red, white, yellow and generally blue, yet calls all his light-colored marbles (yellow, light brown, etc.) white, and those of dark color (blue, dark red, purple, etc.) black. The child needs to know the form of things more than their color, and color is given to him through only one sense while form is conveyed to him through not only sight but particularly the very emphatic sense of touch. Color is only one factor in the building up of the perception of form and would seem not to be noted as such and for its own sake at first. His confusion of redbird and yellowbird, both when the head alone of the redbird was shown and when it was shown headless, may be only a confusion of color names. The head of the yellow warbler was called yellowbird, but no other mistake was made with these two birds. The vulture is evidently known by the great size of its body (it is the largest bird but one in the book), for its head was named swallow one day and thrush the next. The second thrush's head, which is not in the attitude of singing, he called first dove with uncertainty and then said "eye," "beak" (pointing to them), then "I don't know." But, being urged to say, he guessed "swallow." These are the only bad mistakes he made. It is not remarkable that he should have called the head of the second swallow martin. The surprising thing is that he should have named the head of the first swallow and of the martin correctly, and that he should not have confused the heads of the eagle and the hawk (a falcon). In the case of the martin and swallow I think color must have played a part, as I showed him three different swallows' heads, one of which is turned in the same direction as the martin's and is in every respect similar except in color (being brown, while the martin's is a blue-black), and in the martin's head being slightly larger. The head of one of the other swallows is of a different color again, a dark purple with reddish throat, while the martin and the other two swallows have a white throat.

His calling the canary sparrow is a point of much interest for a different reason. When I showed him the headless flying canary, he hesitated a little while and then said "spayo." I said, "No that 's not a sparrow, and then he said "canady-bird." He had not heard the word sparrow since the three days upon which I had given him that word for the flying lark, and he had only repeated it once after me, namely on the last of the three days when he said "payo." Now forty-five days after, he recalled this word and applied it to a bird flying like the lark. The sound "sparrow," therefore, had remained reproducible, associated with the visual image of a bird flying (during all this time), and was now for the first time called forth in connection with the visual stimulus of a flying bird. With the head the bird would have been called canary as usual but the headless canary did not at once suggest the name canary, and hence there was a chance for the other association to appear. That he should have called it sparrow rather than lark is possibly due to the fact that "lark" also had through frequent repetition got connected more closely with the visual image of the whole bird.

In order to see how much further the child had carried his unconscious analysis (if I may be allowed the expression), I tested him during the next few days in recognizing the birds when seeing only the bill or, if the bill was not then recognized, extending to and then beyond the eye till the whole head, or more if necessary, was uncovered. I also showed crests alone where present, and, in the case of the owl, the eyes alone. I found that the quail, rooster, and umbrella-bird were recognized by the crest, and the owl by its eyes. The rooster was not known by the bill alone, but the quail was; when shown the umbrella-bird's bill, he said first eagle, then at once umbrella-bird. The following birds were recognized when only the bill was shown: both grosbeaks (one with red, the other with brown bill), flamingo, ibis, macaw, first parrot (large red bill), first canary (not singing), both ducks, pigeon. The following were recognized when a little bit of the head was seen with the bill but not including the eye: first thrush (singing), first and third chickadees, stormy petrel, lovebird, martin, nuthatch, first swallow, first robin (not singing), goldfinch, dove, yellow warbler, vireo. Not recognizing them at once he would say, "Lill bit more, dado." The following were recognized as soon as the eye appeared: stork, blackbird, albatross, second canary (singing), hawk, second robin (singing), kingfisher, hummingbird, yellowbird, jay, partridge (first said pigeon), eagle, second parrot, second chickadee, crow, second thrush (said "I can't see it, make his eye"), nightingale (said "thrush" for bill only), redstart, secretary-bird. The second "chickadee," a long-tailed tit, has a small black bill much like the martin's but more hooked; before he saw the eye he called it hawk and then martin. He said magpie for jackdaw; but when shown the latter another day, he said magpie when only the bill was shown, but jackdaw as soon as the eye appeared. Of the following the whole head had to be shown before they were recognized: cassowary, heron (fish excluded), lark, magpie (spoon excluded), first wren, ostrich, redbird (but called yellowbird as usual), vulture. A little of the body had to be shown with the head for the recognition of the cuckoo and the second wren; and on two different days the second swallow was called martin until the whole bird was seen. There is evidence of considerable redintegration here, or such an association of the whole with the various parts of which it is composed, that the part, and often a very inconsiderable part, at once suggests the whole, doubtless in a very mechanical and unconscious way. Three birds were known by their crests, one by its eyes, and a dozen by their bills; in thirteen others a small part of the head only needed to be added to the bill; twenty others were known when the eye was added. This leaves eleven birds, eight of which were known when the whole head was seen and two more when the wing-shoulders were added. This leaves only one bird that had to be seen entire to be known, and this bird was confused with one which resembled it closely in every way but in color, in which they differed markedly. Both were flying, though in opposite directions.

While he was learning the birds, attention was scarcely ever called to any part, but he occasionally pointed to and named the tail and eye, and later the beak and legs, as "Look at a eye!" "Little tail stickin' up", "Long beak!" "Look at a big legs!" He would sometimes refer to their actions as "singin' away, he's flyin', sittin' on fence, he's climbin' a pole", the last two original. He noted surrounding objects as the duck's and dove's eggs, the snake in the secretary-bird's bill, and the fishes held by kingfisher and heron.

He was tested with the first twenty birds in the book by showing him the tail or feet in the same manner that the head had been shown. The following fourteen birds were known from the tail alone: wren and ibis (tip only), rooster and jackdaw (half of tail), albatross, blackbird, chickadee, swallow, dove, cuckoo, both ducks, goldfinch, jay. He knew the flamingo by its feet, the cassowary by its feet and legs, the eagle, hawk, and canary by the feet and tail, the grosbeak by the tail and wing, and the hummingbird by its wing, feet and tail.

I also had him guess the birds from their association with the others on the page. On two pages there was but a single bird, but on each of the other thirteen pages there were on the average about four birds. I tested him on only one bird a day on each page for four days, covering the bird up completely along with its immediate surroundings and leaving the rest of the page uncovered. The only way he could guess the name of the bird was by its association in his mind with the other birds on that page and by its position on the page. During the four days he was tested with fifty-one birds, of which he guessed twenty-six correctly and twenty-five wrong. The exact numbers each day were 6 right and 7 wrong, 6 right and 7 wrong, 8 right and 5 wrong, 6 right and 6 wrong.

II. Birds Memorized by Adult

To compare my memory for new oral verbal associations with the child's, I had myself taught similarly the Japanese names for the same birds. Mr. Hikozo Kakise kindly consented to conduct this experiment with me. It came out that I required thirteen days to learn the names, or about half as many as the child. But as my method of learning the names differed from the child's, in that I consciously strove to recall the names and to add as many as possible each time, I took much more time each day than did the child. In fact the total amount of time spent by me in the experiment was 4 hr. 50 min., or only 40 min. less than the child used. It should be mentioned, however, that some time was consumed each day in note-taking. I used an average of 22 min. a day, and one day occupied 40 min. On the 13th day, when I finally succeeded in saying all the names correctly, I required nine minutes to do it; while the child on his successful day required but six minutes. The day after, however ("14th day"), I named the birds in exactly the some time as the child did the day after his first success, namely in five minutes. A week later ("15th day"), I required a little over nine minutes to name all the birds, finding three particularly hard to recall, namely, hikuidori, yamasuzume, and umisuzume. Six days thereafter ("16th day") I required eight minutes to name the birds, finding difficulty only with oniwatori and uzura. After a further interval of six weeks

("17th day"), however, I was found to have lost nine of the words, two absolutely (hikuidori and kawarahiba), four less completely (akazuru, hibari, hebikuidori, and kasadori), and three I got nearly right (hototogis, uzura, saezuridori). Five others were hard to recall (bakadori, kuro-tsugumi, aka-garas, kasasagi, wase). Moreover, I used thirty-five minutes, or more than four times as much time as six weeks previous. It will be seen that my experience tallied herein very closely with the child's. The child required five minutes more to recall the words after a week's interval, and I a little over four minutes. He did it three minutes better five days later and I a little over one minute better six days later.

It must be taken into consideration that the birds were all familiar to me and so stood out distinct from one another, ready to have a new name attached to them, while they were nearly all quite new to the child. Also my power of articulation is better than the child's. On the other hand, I had no knowledge of Japanese except what every one picks up in his reading, while the child already possessed a large vocabulary of English words, which doubtless had some suggestive value, especially as it comprised some of the words occurring in the bird names; namely, bird, red, black, crow, umbrella, kingfisher, "coocoo", duck, rooster, Jack, love, caw, night, nut. My knowledge of several foreign languages, however, doubtless offset this. Again the child, as we have seen, rather frequently recalled bird names to mind and used them, while only very rarely did any Japanese name recur to my mind and it was never dwelt upon.

In examining Mr. Kakise's notes of my method of recalling, so far as he could gather it, and supplementing his records from my memory, I find that the matter may be resolved into the following kinds of reaction:

a. The mixing of the name wanted with some other.

b. The substitution of another name for the one wanted.

c. Some of the letters and possibly the general character of the word recalled, but generally not visualized; if visualized, in my handwriting.

d. An impression of the general character of the word,—the number of syllables or the length of the word, the difficulty or lack of familiarity, the heaviness or lightness of sound. Waving the hand to indicate the rhythm sometimes helped me recall a word.

e. Making a "bluff" at it and succeeding or nearly without any visual or auditory image to guide me, the vocal organs my sole guide; no consciousness till the word is struck.

f. Method of trial and error: using the vocal organs directed by my ear till, by a process of elimination of what did not sound right, I finally struck the right word, or what seemed to me right or nearly so. Sometimes I began by a gutteral trilling.

g. A feeling of familiarity, an inkling, a foreshadowing, as if the word or sound were approaching and projecting a vague image of itself before it.

h. (1) A feeling that I am nearly right, but not satisfied.
(2) A certainty that I am wrong.

i. Recalling one of the parts of a compound word.

j. Association with previous knowledge.
1. Of other languages.
2. Of Japanese.
3. Association of character of word with character of animal.

k. Recognition of word when prompted.

l. Ability to finish word begun for me.

m. Recalling the first part or the last part more or less correctly, and then repeating it till it possibly called forth the rest by association.

n. Elimination of the wrong word by being assured it was wrong; and then the line of least resistance to the effort was along the path made by the true word.

o. Uncertainty whether right after pronouncing the word correctly; failure to recognize the word completely after I say it.

p. Mind a perfect blank, incapable even of making a "bluff" at the word.

q. Persistence of an error once made.

The method of trial and error (f) was the most characteristic, and there were several cases of blind attack or "bluff" (e), when I succeeded by the sheer effort of speech without any conscious guidance. There were also many cases of the strange feeling of familiarity without further content or possibility of recalling anything (g), as well as of an impression as it were of the ghost of the word, its general character, without any image, visual, auditory, or motor, of the letters or sounds, but only a vague feeling of the rhythm of the word or some other quality (d). Some use was made of associations as memory hooks (j); on the other hand, there were many cases of interference (a and b).

A slightly wrong pronunciation was sometimes accepted as correct, the error not being detected. Had I been able to see the word or hear it spelled, this could hardly have occurred. I only discovered it when shown the words after the experiment was over.

Beginning with the second day the number of bird-names recalled by me up to and including the day in question, and the net number of bird-names actually recalled that day were as follows :

2, 7, 11, 14, 22, 26, 34, 41, 43, 45, 47, 48;
2, 7, 11, 14, 22, 25, 32, 40, 41, 42, 46, 48,

It is perhaps worthy of remark how few words were lost once they had been recalled without aid. I made 10 misses, my little boy 47, with the birds. The curve of acquisition both for new names and for net number of additional names each day has three distinct apexes at the 3rd, 6th, and 8-9th days (5, 8 and 8 names respectively), with strong dips at the beginning, and end and between; but there is another for net additional names at the 12th day (4 names). This makes a very regular curve with intervals of 2 days between the apexes.

Fatigue induced by the experiment must have played a very small part in my case, for the first 24 names in order as they occurred in the book required on an average 6.7 days each, the last 24 each 7.1 days to learn or a difference of less than half a day in favor of the first half of the book. In my experiment with the mammals, to be described later, I began on alternate days at the beginning and the end of the book. In this case the first 26 required each on an average 7.2 days and the last 26 required 6.1 days each to memorize.

III. Mammals Memorized by Child

On the 26th of February, in his 30th month, I began to show my little boy a mammal book in the same way that I had shown him his bird book. This was Schubert's *Naturgeschichte der Säugetiere*". From it I selected 52 animals, all different, of which he was to be given the names, half English and the remainder foreign, equally divided between French and German. I took care that as nearly as possible the words in each language should be of the same length. Of one syllabled words there were 4 English, 3 French and 2 German; of two syllabled words there were 14 English, 6 French and 9 German; of three syllabled words 7 English, 4 French and 2 German; and there was 1 English word of 4 syllables. Thus the 26 English words contained 57 syllables and the 13 French words 27 syllables, and the 13 German words 26 or 110 syllables in all. I also so distributed the names that 12 English, 7 French and 7 German words occurred in the first half of the book and 14 English, 6 French and 6 German in the second half.

Mindful of the influence of fatigue in retarding the learning of the names of the birds in the last half of the book, I began at the beginning and at the end of the mammal book on alternate days. The result showed very clearly how essential this change of method was to the experiment, and at the same time furnished remarkable evidence of the immense importance to pedagogy of the question of fatigue. During the first seven days, the child learned to name correctly 15 animals, and although they were equally distributed between the first and second halves of the book (8 in the first half and 7 in the last), yet every one was correctly named for the first time in the first half shown him. Again, the total number of correct recollections during the seven days was 38, of which 20 were names of mammals in the first half of the book and 18 of mammals in the second half. Yet the number that occurred in the first half for each day respectively was nearly twice as great as that of the names occurring in the second half for the day, or 24 and 14 respectively. Not only did the child not learn any new words when fatigued but he forgot some he had already learned. For example, the skunk which is the first animal on the list, was named correctly on the 5th day, while on the following day when he began at the end of the list, he could not recall it clearly, saying "hunks" instead of skunk. This word "hunks", whose inception was evidently due to fatigue, persisted through nearly the whole of the experiment, although corrected each day. Sea-lion and narwhale which occur in the second half of the book and were named correctly for the first time on the 6th day, when we began at the end of the book, could not be recalled on the next or 7th day, when they occurred in the second half as taken up on that day. To attempt therefore to teach a child anything when fatigued is not only futile as regards new acquirement but is even destructive of progress already attained. Moreover the effects of fatigue are subtle and not always easily or readily discernible in the behavior or appearance of the child. It was not until the 7th day that I noticed signs of fatigue in the child towards the close of the experiment, namely holding his head with his hands, and tripping and falling as he ran away. However, I divided the words into two equal sections and gave him them thereafter at different times of the day, while still continuing the alternation in order. The result was a marked change. The number of words repeated in the first half of the experiment and the second half was about the same, even a few more in the second (255 and 262). The number of new words that came in in the second half for each day was twice as great as in the first half (25 to 12). Two periods in the day, therefore, of 10 minutes each proved not to be too much for the child. Let us hope that the suffering that this child, healthy as he is, must have undergone through fatigue in the earlier part of the experiment will prove to have been vicarious, and by its suggestive value help to lift the burden that weighs so heavily upon childhood throughout our land in school and shop and factory.

The child required 21 days to learn the mammals, less by 3 days than he took to learn the birds. But as the time spent each day was longer, being on an average 19 minutes, and about six hours in all, there was about an hour more time given to the mammals. It should be remembered that there were four more *names* of mammals than of birds, but six fewer mammals. At times his attention was very flitting. Once he ran to his mother after each animal to tell her the name. On that and one other day he consumed 30 minutes which was the longest time spent over the book on any one day. The shortest time was 12 minutes and occurred four times, the 2nd, 5th, 19th and 21st days. It should also be mentioned that this experiment was only conducted during five days of the week, Saturdays and Sundays being omitted, except the 21st day which was a Saturday. The experiment thus lasted 4 weeks. Each mammal required on an average 7.6 minutes to be learned, as compared with 6.5 for the birds. It should be remembered that half the mammal-names were foreign, and hence, as will be seen, harder to learn.

It seemed to us throughout that he took less interest in the mammals than in the birds. This might be accounted for by the brilliant colors of the birds and especially their greater naturalness and activity. The mammals are represented in quite wooden postures. In books prepared for children it would be better to have a few good illustrations, representing the animals at work and play, than a great number of reproductions of museum specimens. My boy was constantly on the lookout for activity, and in its almost total absence, he would try to supply it, making use of every hint. For example, many of the pictures in the mammal book contained representations of water; so the child would frequently say "He 's drinkin' ", though no animal was represented as in the act of drinking. "He 's goin' to catch it", he said, seeing a duck in the water with mammals on the shore, but no mammal was represented as trying to catch anything. Also he said that the raccoon was going to jump on the bear though the illustrator was innocent of any such intention. Perfect decorum was observed by all the animals, who evidently had sat for their pictures. He was particularly interested in the tails and eyes of the mammals and in their open mouths. "He goin' to bite me," (putting his hand on the open jaws) "he 's got some eye" (hedgehog), "where his eye? there 't is" (porcupine) "there 's his eye" (whale), "he has some tail" "look at a horns." He noticed the mole had no eye visible "any eye!" (no eye). The keenest observation, perhaps, was his remark on seeing the forlorn looking donkey: "he 's cwyin'".

The foreign words were introduced in order to test the child's relative ability to learn foreign and English words. The child's playful prattle contains an exceeding variety of sounds, many of which are thought to be lost through the growing specialization imposed by the necessity of learning and using exclusively a single definite set of sounds, known as English or French, etc. It seemed to me of interest to discover how far such specialization had already proceeded in a child so young.

The following table shows for each day the number of mammals so far learned, the new ones added that day, those forgotten that day, and the total number named that day. Those forgotten are preceded by a minus sign.

2nd day, 1 mammal, camel	1
3d day, 3 mammals, spitzmaus, anteater	3
4th day, 4 mammals, goat	4
5th day, 7 mammals, skunk, zobel, porcupine	7
6th day, 12 mammals, lama, gazelle, giraffe, sea-lion, narwhale, —skunk	11
7th day, 15 mammals, wiesel, jackal, schnabeltier,—skunk sea-lion, narwhale	12
8th day, 18 mammals, ours, dachs, zébu, —skunk, zobel	16
9th day, 26 mammals, hedgehog, guinea-pig, armadillo, wild boar, tapir, chamois, seal, wal, —skunk	25
10th day, 32 mammals, maulwurf, opossum, kangaroo, muskrat, biber, taureau, —skunk	31
11th day, 33 mammals, chevrotain, —skunk, guinea pig, kangaroo, wild boar, taureau, narwhale	27
12th day, 37 mammals, manis, rhinoceros, zebra, morse, —skunk, wild boar, chevrotain, taureau	33
13th day, 38 mammals, ferret, —skunk, rhinoceros, wild boar, chevrotain, zebra, morse	32
14th day, 39 mammals, musk-ox, —skunk, rhinoceros, zebra, sea-lion	35
15th day, 43 mammals, flusspferdt, esel, seebär, manatee, —skunk, musk-ox, zebra	40
16th day, 44 mammals, dauphin, —skunk, flusspferdt, chevrotain, esel	40
17 day, 46 mammals, loutre, moose, —skunk, flusspferdt, esel, seebär	42
18th day, 48 mammals, raton, renntier,—moose	47
19th day, 49 mammals, edelhirsch	49
20th day, 52 mammals, renard, volverenne, paresseux, —edelhirsch	51
21st day, 52 mammals	52

The aggregate number of days the child said correctly the 26 English words was 318, the 13 French words 94 days, and the 13 German words 138 days. French words are, therefore, if we can draw any inference from this test, nearly twice as hard as English, and German words not one quarter harder than English; or to be more exact, French is 59.1 per cent. as easy as English, and German 83 per cent. as easy. Thus German is 23-9 per cent. easier for this child than French. As he is partly of French descent and not at all of German, it would appear that heredity must play but a small part in the acquirement of language, and the onus of proof rests with those who hold that it has any direct influence. To a child whose mother tongue is English, it is not surprising that German, a Teutonic language like our own, should have proved easier than French, as that language much resembles our own in its pronunciation and particularly in its accentuation. The German way of pronouncing *sp* was no harder for this child than the English and the *z* and *schn* proved easy; the *pf*, however, he never mastered. *Flusspferdt* and *Edelhirsch* proved very difficult, and *Maulwurf* rather difficult. On the 8th day, by watching my lips, he was able to pronounce *Wiesel* without substituting a b for the w, but he did not pronounce it correctly of his own motion till the last two days. He had the same difficulty with *Wal* and *volverenne*. One of his chief sources of trouble with French, besides the accentuation or lack of strong stress, was the frequent occurrence of the letter *r*. It happened that every French word but three contained this letter. The French words in order of ease beginning with the easiest were *ours*, *zébu*, *chamois*, *armadillo*, *taureau*, *morse*, *dauphin*, *loutre*, *raton*, *renard*, *volverenne*, and *chevrotain*. The last two he never pronounced correctly. I had to be satisfied with chemotain and volvenne at the best. *Volverenne* was bolwenne and bolvenne until the 20th day when he recalled it for the first time and called it volvenne. For *paresseux* he said cacasseux on the ninth day, and thereafter when he recalled this word he always so called it until the last day when he corrected himself and added "paresseux." His pronunciation of the so-called nasals is interesting. These sounds evidently puzzled him. At first he said daupha and wato; but he seemed to feel there was something lacking in his pronunciation and to supply the lack he added the syllable nna and said dauphanna and dauphinna (8th day to 20th) and ratonna (13th day to 18th inclusive) and sometimes temotan and chemotanna. It is remarkable that he should have supplied an n, as I did not suggest such a letter except in so far as a proper pronunciation of the nasal *in* and *on* would do so. He had no difficulty with any other vowels, however, in either language, even the

French *u*. For chamois he persisted in saying "chambwa," like wha in wharf. This was perhaps his way of mimicking the foreign accent. English words that proved hard to pronounce were ferret, kangaroo, giraffe, porcupine, zebra, and especially rhinoceros, which he never could say better than wynoceos and frequently the *n* was left out as well as the r's. Porcupine was always portupine, giraffe dewaff. Altogether I am inclined to think that those who speak of the infinite variety of the child's instinctive speech in the babbling stage are possibly correct as regards vowels but probably mistaken as regards consonants. In any case it seems as though if one wished to take advantage of the "nascent period" for speech in the acquirement of a foreign language, one would have to begin it at birth, where many other nascent periods lie. The first sound my own child made (namely, in crying) was kazthá, the second consonant lying somewhere between a *z* and a *th*, in fact just like the Japanese z in *azaras* to my ear.

The curve of acquisition of the names of the mammals is of the same general character as that for the birds. The highest point was reached in the second experiment three days earlier, namely on the 9th and 10th days, and was slightly higher, the greatest rise in the net number of mammals on any one day being nine, and the greatest number of new names added being eight, both on the 9th day. There were 3 other marked apexes, namely on the 6th, 12th, and 15th, and, for number of birds only, also 18th day. This makes a strikingly regular curve, the apexes being all 3 days apart with a strongly marked dip between. In fact it is precisely similar to my curve for the birds. After an interval of a week he took the same amount of time to name the mammals, namely 12 minutes. He still remembered all except *Edelhirsch*, the last learned, and skunk had degenerated again into hunks. He also mispronounced zebra (deba), *volverenne* (bolbenne), and *dauphin* (dauphinna), and said hockena at first for hedgehog and cacasseux at first for *paresseux*, though in both instances he corrected himself.

In the learning of English words the length of the word had little if any influence upon the time required; for the one-syllabled, two-syllabled, and three and four-syllabled words respectively, required on an average 8.7, 8.6, and 8.7 days to learn. Indeed if one omits the one four-syllabled word rhinoceros, which was especially difficult, the average for the three-syllabled words is 8.3, showing a slight improvement upon the shorter words. It was different, however, with the foreign words. Here the corresponding figures were for French words 12.3, 13.5, and 15 days, and for the German 8.5, 10.9, and 13 days. The longer the foreign word, therefore, the harder it was for this child to master.

From time to time the child would make allusions to the animals in his play. The second day he said to me, "Djou portupine" and called his mama a biesel the 8th day. Three days later he said, "I'm a biber, I'm a muskrat, mama." These occur on the same page. Also, placing his fingers together at the tips, he said, "At's a schnabeltier." On the 16th day, building blocks he said, "cacasseux, at's a gazelle"; the next day, pointing to his boot which he was lacing, he said, "At's a renntier", and of two pencils he had placed in a line, "At's a narwhale, dauphinna, I eat it." Two days later one of his blocks was "a ours"; two days thereafter his boot "a jackal", also his mother (You's a jackal). He used the word opossum the next day and zébu two days later. Some days later he said, "The alleguatehs will eat my schnabeltier all up," and indeed the live alligators threatened to eat the whole dead animal book up in interest. I had taken him to see the alligators at the University. He was frightened by their hissing and kept a considerable distance away; but being alarmed for my safety as I stood quite near them, he came and pushed me away determinedly, saying, "They scare you; those alligatehs are naughty." This was rather brave for a frightened child. When he got safely down stairs he told a professor, "Those alligators are afraid of me!" Thereafter, the alligators were much more real, much more a part of his psychic life, than all the pictured mammals. For example, of his blocks he said, "Look out! You're stepping on my little alleguatehs." And when he sees me getting ready to go out, he says, "Where you goin'? To see the alliguatehs?" or, "to see the 'coons?" (live 'coons at the University) whereas the "raton" is little more than a word. He also shows increased interest in the parrot now that he has seen a pair at the University, and wants me to draw a parrot more than formerly.

On the 11th day of the experiment, while I was drawing cats and mice, he said "Make a biber, a muskrat, a zobel." On the 13th day he drew two loops with a tail at one end and a dot for an eye at the other and called them "Spitzmaus." The tail was made with the same stroke as the loop and was from a half to three quarters of an inch long; the loops were about 2 inches by a half and two inches and a half by one and a half. This is rather early for any definite form to appear in a child's drawings. He had made several drawings before this, the first being an "owl" when he was 2 years 4 months 18 days old. This also was an oblong, about 4 inches long, considerably wider at the head end where he placed three dots as eyes; there was a tail at the other end. It has been said that the child in its drawings is first and chiefly interested in the human form. This was not the case with my boy. He did make a man after making the owl, but he has never made one since, though he has made many mice and cats. This is no doubt due to the fact that I have constantly drawn for him cats and mice and birds, especially owls and parrots, and scarcely a single human figure. His man, above mentioned, was rather more complicated than his owl, and the eyes instead of being dots were two round scribbles, imitating no doubt the big owl's eyes I made for him. At one end he drew a narrow oblong continuation and called it a "poke." Then he drew a third figure on the same page, big and round, with a tail at one end and three dots for eyes at the other, and called it a "bin." These words are probably original. His mice and cats are all simply loops or oblong figures with a tail at one end and eyes one, two, or three, at the other. Once when he showed me a "little mouse" with an eye but no tail, I said, "where's his tail?" "Right here", he replied, drawing a line from one end of the figure as a tail. As I write this part of the article, the child is two years and a half old.

While being tested from day to day the child made much less effort to recall than I; as a rule his responses were pretty immediate, and when he did think hard, he generally thought silently. Hence he did not give occasion for so many observations of his method. But, from the very immobility of his vocal organs, so far as I could see them, I should judge that he was chiefly working in the auditory realm, striving to hear the sound that had come to him with the visual image. In such a case he sometimes looked at the animal and at other times straight before him. Of course the words were frequently imperfect when first recalled and several times only one part of a compound word was given; as kingfish (kingfisher), queel (quail), ogle (eagle), wobble (yellow wabbler), tart (redstart), tomy or petel (stormy petrel), chem (*chevrotain*), and hilsch (*Edelhirsch*). Of the mammals I counted no incorrect pronunciations as correct unless I felt it was the best the child could do with his present powers of articulation. There was very little mixing of names, as mawkten for martin (confused with magpie on the same page) and lockin for lark confused with mockin his usual word for martin; but there were many cases of substitution with the birds, though very few with the mammals; examples are swallows for doves, lark (and another time "lock, lockin") for redstart; stork, macaw, eagle, grosbeak and parrot for hawk at various times, macaw for manatee, "wild" (boar) and do-bär for See-bär. With the child this was probably oftener a confusion of the animal than of the name, with me it was a confusion of the name. I noticed very few instances of his audibly correcting himself. One was "fish, fush", and later "fush, thush" said two successive days, another "lockin, no seh, mama, lark"; in two other cases he got the last part of the word first and it suggested the whole word, namely, "finch, goldfinch" and "bu, zébi." Very often the first syllable or the first letter or even the placing of my vocal organs in position to begin the word was sufficient to suggest it to him; as for example, h- hawk, hum- humming birds, ma and the next day m- magpie, ra- *raton*, r- *renard*, v- *volvenne*, n- narwhale, chev- *chevotain*, e- *Edelhirsch*. Examples of the persistence of a wrong association are "lockin" and "moss" frequently for lark and moose, and the naming of the yellowbird "white-bird" always before saying yellowbird, once he had made this mistake. By analogy with yellowbird and umbrella-bird, he would sometimes say canady bird and heron bird, and tickabirdie (chickadee). He called the secretary bird snake-bird from the snake in its bill before he could name it by its proper name; and he called his birdbook parrotbook from the large and gaudy picture of a green parrot on the cover; while of his mammal book he once said "I want to see the book daddy-lion and mama-lion" from the picture of the two lions' heads on the cover which he had been told were the daddy lion and mama lion.

IV. Mammals Memorized by Adult

With Mr. Kakise's aid I learned the Japanese names for the 52 mammals in the same way I had learned the 48 Japanese bird-names. The birds had required 12 days to be memorized, the mammals required 13 days, or an exactly proportionate time. I did not allow myself to be granted so much time to strive to recall the names as in the previous experiment, using an average of only 19 minutes per day instead of 22, or 4 hours 26 minutes in all. It would, therefore, appear that there is nothing to be gained by an excessive expenditure of time and effort in recalling. I missed only two words on the 12th day and also on the 13th. On the 14th day I said all correctly in 4.5 minutes, and did the same the next day. It is rather odd that in repeating the names of mammals, the child and I took on an average the same amount of time per day, namely 19 minutes. But he required 20 days to learn them to my 13. His maximum time per day was 30 minutes, mine 22; his minimum 12 minutes, mine 4.5. A week later I still remembered all except that I confused the words for sea-lion and manatee (Kai-hyo and Kai-giu). The child after an interval of a week missed only *Edelhirsch*, which, however, he pronounced correctly when prompted. He mispronounced several, however, saying *hunks* (skunk), *portupine*, *dewaff*, *bolbenne*, *deba* (zebra), *dauphinna*, *cacasseux* at first for *paresseux* and then, when I said "no," corrected himself, and in the same way first called hedgehog *hockena*, no doubt being influenced by the French words ending in a nasal. I required six minutes, and the child twice as long. Then, after an interval of six more weeks, we both tried again. I had forgotten 15 and required an hour for the effort. The child had forgotten 35, but he used only 25 minutes in trying to recall the names, not being desirous or capable of making so much effort as I.

Beginning with the second day, the number of mammal-names recalled by me up to and including each several day, and the number of mammal-names actually recalled that day were as follows:

1, 2, 5, 11, 16, 28, 34, 39, 44, 46, 51, 52, 52
1, 2, 5, 11, 14, 28, 31, 37, 43, 43, 50, 50, 52

In 14 cases I missed words previously known; my child missed 41 times. This does not greatly differ from our record with the birds. My curve of acquisition rises and falls with much the same regularity as before, except that the intervals are one day instead of two, the apexes being on the 5th, 7th, 9th, 10th and 12th days.

There is a remarkable rise on the 7th day of 12 new names and 14 net additional names. That day was a Tuesday and the rise was apparently due to the fact that I omitted Saturday and Sunday repetitions during the course of this experiment, but not of the former one. Through not repeating the words on Saturday and Sunday, my score was little increased on Monday but more than proportionally increased on Tuesday. On the following Tuesday there is another sharp rise of 5 new and 7 net additional names. In the number of names recalled there was a greater rise on these two Tuesdays than on any other day; while on one of the Tuesdays (the second) was only one word missed that had been previously learned, and on the other Tuesday none. On Monday I had to expend much energy in recalling words that the two days' rest had partly obliterated or sunk deeper below consciousness. On Tuesday I got the full benefit of the general Sunday rest in the ability to turn my energy to the acquirement of new words without the embarrassment of having to hold on to half-learned words. The relative loss of Monday is more than made up on Tuesday. It is possible that the long interval had contributed to the fixing of the words in the mind, though not in a way to be readily recalled at first.

A careful examination of these notes shows that my method of recalling these words varied little from that used with the others. Association (j) was more used this time, however, and was of great help. The first and third words recalled were recalled by its means, and altogether this method, without any systematic effort to exploit it, proved possible and helpful in 17 cases, or one-third the number of words. The mind naturally seizes any suggestion of this kind, however remote. In ten cases the association was with English, French or German words, in four cases with Japanese words; in three other cases a connection was seen or imagined between the sound of the word and the appearance of the animal. In only three cases were words so memorized forgotten, and then only partly forgotten. The method of trial and error (f) is the usual one when the word does not at once recur or when I have no association in mind by means of which to recall it. The word is usually built up gradually from day to day, beginning often with a mere feeling that it is *there*, or a curious sense of the character of the word. And just as the word becomes more and more clearly conscious, so it gradually fades out of consciousness, until but a syllable or a letter (not necessarily the first or the last), or an indistinct perception of its vowel sounds or its length, or its rhythm, or even only a vague feeling of familiarity remains. In only three cases was the word completely lost after an interval of six weeks.

Such an experience makes one rather incredulous of Dr. Titchener's somewhat clear cut division of consciousness into two states. If, applying his theory to the memory of a word, he calls the clear consciousness of the whole word one state, and all states of consciousness below this another, he will, of course, count two. But these other states are indefinitely many and diverse, from a vague feeling of familiarity, the ghost of the word that may or may not take on flesh, to the feeling that the word is not quite right, though only one letter or sound is astray.

V. Third List of Animal-names Memorized by Adult with Addition of Visual Word-sign

To see what difference the addition of the visual image of the word would make, a third list of 52 animals was selected, and the animals were shown me day after day, including Sunday, by Mr. Kakise, while he both repeated to me and showed me their Japanese names. Six of the words proved to be ill-chosen, as being too much like words I had already learned, and were rejected and others substituted for them the fourth day (one) and the seventh day (five). Leaving these six out of account, the other 46 names were mastered in 10 days, or in three days less time than the 48 bird-names and in four days less time than the 52 mammal-names. The additional six names, though introduced so late, were all recalled with the rest on the 11th day, except one, and this long word (nokogirizame) was finally recalled with the rest on the 13th day, which was the 7th from its introduction. It may be fairly inferred, therefore, that the addition of the visual image of the word will hasten by three days my memorization of about 50 Japanese names of animals. The total amount of time given to this third series of words, however, was greater than that given to the others, as the average time each day was longer, being 26.6 minutes per day for the ten days and 24.5 per day for the 13 days. The maximum time per day was 33 and the minimum (12th and 13th days) 14 minutes. The six words introduced late got, of course, less time than the others, and, what is of far greater importance, fewer repetitions.

Beginning with the second day (Wednesday), the number of names recalled up to and including each several day, and the number of names actually recalled each day were respectively:

1, 5, 9, 15, 22, 30, 40, 44, 46.
1, 5, 8, 14, 22, 29, 40, 43, 46.

It will be seen that names once recalled were dropped but four times during the experiment, a much better record than when no visual image of the word was allowed. A curve plotted from these figures shows an almost steady rise to the 8th day, in this differing from the previous curves. A certain periodicity is shown as before by the apexes at the 3rd, 6th, and 8th days, but the first two are not succeeded by so marked falls as in the former experiments.

After an interval of six weeks I found I could only recall 14 of the 52 names, though I tried for an hour. Among the 14 was only one of the six names that had been introduced late. This leaves 13 words recalled out of the 46, or 33 forgotten. This is a worse record than the child's, for the list of mammal-names, half English and half foreign. And it is two and a half times as bad as my own record with the list of mammals learned without the visual image of the word. This is probably sufficiently accounted for by the fact that in being memorized the second list of names was repeated on 15 days, while the third was only gone over on 13 days, and still more by the fact that I was tested on the second list after a week's interval, but not on the third list. The child also was tested on his list after a week's interval. But even when due weight is given to these considerations, it would appear that the addition of the visual image, while it makes the foreign word easier to learn, makes little difference to its retention, partly because one makes one's own visual image of the word in time, if not given one and the lack of the visual image makes for a deeper motor and auditory impression. It may also be inferred from this study that a given amount of time accorded to the learning of language will produce better results if distributed over many days than if lavished on a few. In the 16 repetitions of the second list (the 16th being after a week's interval), I occupied 4 hours 36.5 minutes; in the 13 successive repetitions of the third list I occupied 5 hours 18 minutes with the additional advantage of the visual image; and yet the former list was very much better retained. And the slight expenditure of time (six minutes) devoted to the repetition of the list, with effort to recall, after the interval of a week, which made the 16th repetition of the second list, undoubtedly had much to do with my comparative success in recalling these words after an additional interval of six weeks. All this suggests to the educationist the great value of periodic repetition and reviewing.

I next set about ascertaining how many repetitions I should require in order to relearn the words so badly retained. I accordingly had myself prompted in those I did not know and tried the list again the following day, when I succeeded in half an hour in recalling 44 out of the 52, again recalling only one of the extra six words. The next day I recalled 49 in 11 minutes, two of those missed being still of the aforesaid six. The following day all were recalled in 7 minutes. Three repetitions, therefore, proved necessary for the retaining of the words. But a week later five were again missed, one of them belonging to the extra six.

I put my little boy through the same process with his English and foreign mammal-names. After the six weeks' interval he remembered 17 of the 52 words. Half of the others he could give me when I uttered the first letter or syllable. The 17 words recalled were *Wiesel*, fegwet (ferret), *Sobel*, kangaoo, guinea-pig, oposs, *Dachs*, muskrat. *taureau*, *zébu*, tapir, wild boar, cambels, dewaff, gazelle, goats, seals. He said aw for Wal, loute (*loutre*) for *ours* and gazelle for *Esel*. In relearning the words he, like myself, made very great progress the first day and then rapidly slowed down. He required three times as many repetitions as I to relearn 52 words. Sometimes he said them all at one sitting, at other times he took but a part of them at one period. I let this depend on his own willingness. The numbers recalled at each repetition and the time required were as follows:

			English	German	French	Names
1.	One period;	25 minutes:	12	3	2	=17
2.	Two periods;	28 "	18	8	6	=32
3.	Three "	25 "	23	9	8	=40
4.	Two "	20 "	24	12	8	=44
5.	Two "	25 "	24	10	9	=43
6.	One "	10 "	26	11	9	=46
7.	One "	12 "	26	10	10	=46
8.	One "	12 "	26	13	10	=49
9.	One "	12 "	26	12	12	=50
10.	One "	15 "	26	13	13	=52

Those missed at the 8th repetition were *renard*, *urs* and *volverenne*, at the 9th *raton* and *Edelhirsch;* on the last day he hesitated a long time at *Renntier* and *Edelhirsch*, first saying *renard* for the former. Instead of the 21 repetitions, therefore, which had been necessary to learn the 52 words in the first place, he now required 9, while I had required 3 repetitions instead of the original 9 to learn 46 words. It is evident that the French words were still harder than the German, and the German words much harder than the English. All the English words were known on the 6th day.

Some of the more interesting examples of the play of memory are missmaus-krismaus-*spitzmaus*, also spitzmousies, phinphin-*dauphin*, also phana for dauphin, muskox-muskrabbit-*muskrat*, stunk for skunk, ants for *ours*, dit, hed, ketchog, and hedgekok for hedgehog, bolepol for *volverenne*, caresseux and caresseute for *paresseux*, mayma and ibis for manis, fanton for *raton*, and wynocewofewos for rhinoceros. The child gave evidence of the use of methods a, b, c, d, f, i, l, n, q as detailed on pages 331 f.

Some of his remarks during this experiment were: he 's cwawlin along, I guess (of the duckbill); he 's goin to lie down, I guess (of the porcupine); look at the black feet (of the muskox); they fightin togeddeh, aren't they? (of the reindeer and stag standing quietly facing each other). He likes the monkeys far better than any other animals in the book, and frequently asks to see them. He is doubtless attracted by their grotesque resemblance to human beings. At the page containing anthropoid apes I asked him "What do the monkeys look like?" and he replied at once, "They look Jack, I guess." Being asked which looked like Jack, he pointed to the chimpanzee.

I now tried to get him to learn the English words for the animals whose French and German names he had learned, but it was in vain. I tried for a week and decided it was no use to try longer. The first day it amused him and he pronounced the words after me well and easily; but throughout the week it was impossible to get him to say the English words of his own motion. He would insist on the foreign word and would always say when I prompted him, "No, not dolphin, *dauphin*, not sable, *Zobel*, not otter, *loute*," etc. The only exception was beaver, which he would sometimes say for *Biber*. He even retained the v sound in *Wiesel* and *Wal*, and the French pronunciation of *zébu*. Two weeks later I showed him the book again and we went through it. He was particularly interested in the red squirrel through having seen several (gray) lately in a park. Again he substituted no English word for the foreign except beaver. He had seemingly forgotten six of the words again, namely, *loutre*, *renard*, hedgehog, *volverenne*, *raton* and *Edelhirsch*, and said caresseux for *paresseux* and veva for zebra. He was then, at the close of the whole experiment (15th June), in his thirty-third month, a healthy, active, naïve boy of possibly more than average alertness, intelligence, and energy of will.

Toward the end of May the boy was taken twice to the Agassiz Museum and once to Barnum and Bailey's menagerie. The first day at the museum he named correctly the following animals: kingfisher, owl, fish, monkey, *renard*, *Dachs*, 'coon, *dauphin*, cats, goats. It is interesting that the real raccoon which he had seen before alive as "coon" and far more frequently pictured as "*raton*", was now called 'coon. (In this connection it might be mentioned here that several days later when we came to the raccoon in the animal book and I called it so, he said "Did you see some the University?" and added "Where are de alligatehs?") The following were named incorrectly, the name he gave being in brackets: another raccoon (opossum, rabbit), caracal and lynx and leopard (tiger), seal (muskrat), roebuck and chamois (narwhale and giraffe), a seal with head raised high (sea-lion). Two birds which were quite unknown to him, the curasoa and turnstone, he called respectively stork and dove. I did not correct him in any instance and the next day he was taken again. This time he named correctly monkeys, bats, deer, narwhale, *dauphin*, owls, ibis, rabbits, giraffe, moose, guinea-pigs, tiger, kingfisher, crow, blackbird, camels. For the following he said the names in brackets: beaver (muskrat), leopard (tiger), gazelle (goat), opossum ('coon), zebra (horsie, tiger), sloth (monkey), flamingo (ibis). Again he was not corrected and the same day after lunch and his usual midday sleep I took him to the menagerie. There he named correctly camels, giraffe, elephant, monkeys, lion, deer, goats, zebra (first called *Esel* which occurs on same page as zebra in his book. The leopard was called tiger and the rhinoceros *Flusspferdt*. The following, of which he knew the pictures, were not named: tiger, kangaroo, tapir, lama, bear, wild boar, zebu. I think he knew the tiger, but would not name it. Three days before, seeing a big gray domestic cat, strikingly barred on legs, head and tail, he had remarked spontaneously first "he 's gray", and then "he 's a tiger." The great number of animals rather bewildered him and he took little interest in naming them, and indeed a less demonstrative interest of any kind than I had expected. But animals in cages are little better than stuffed animals in a museum, of which he had seen a great number the same day and the day before.

My method in my third experiment in the learning of Japanese names of animals differed from that used in the others by my greater use of visualization. The relative infrequency of methods *e*, *f* and *m*, compared with the former experiments, shows that I trusted less than before to the memory of the vocal organs and more to the visual image. This may be one reason for my failure long to retain the memories: the motor memory is probably more tenacious than the others. On the other hand I found myself using and profiting by the visual word image less than I had expected I should. My mind seems to have got into auditory and motor channels for this purpose more than is usual with me. Is it not possible that a man's visual or auditory or motor-mindedness may be a result of habit, environment and training more than of a native condition of the mind? Visualization of the word did occur, but I felt it was a much less prominent factor than it would have been had I used it also in my earlier experiments.

The decreased reliance upon the vocal organs to retain words probably accounts in part for the numerous cases of complete disappearance of a word. There were 16 such cases as compared with but two in the former experiment. A visual image if retained at all would be more likely to be complete than a motor impression, as the former is one act of the eye and taken in by one effort, all parts being equally clear, whereas the motor image is a series, if the word is of more than one syllable or even letter. One sound, on account of emphasis or some peculiarity that made it of especial interest, would be likely to make a deeper impression upon the vocal organs than the others, as, for example, the sound *k* in *kairi*, which alone was retained in the former experiment, *n* in *kamonohas*, *a* in *roba*, *aras* in *yamaras*, *zangko* in *zenzangko*, its repetition then drawing out the rest of the word, etc. Of the words *lost* 87% (13 out of 15) left some conscious trace behind in the auditory-motor experiment, and but 58% (22 out of 38) in the experiment in which the visual image of the word was added. On the other hand the visual image seems to make for clearness, though not for permanence, of impression, for in repeating the series learned without the visual image I six times failed to recognize words after I had said them correctly; while in the series with the visual image of the word this happened only once.

Association was used rather more in the third experiment than in the second, namely, in some 24 words as compared with 17. Eight of the 15 words retained six weeks had been learned at least partly by its aid; in the former experiment this was true of 14 out of the 37 retained. Six of the associated words were completely lost in the last experiment, none in the previous one. Sometimes the association alone was recalled, not bringing with it the desired word.

It might be supposed that the retention of a word would depend upon its length; and, indeed, I found that a word's length not only materially affected the facility with which the word was learned, but also had considerable effect upon ease of retention, when it had not been learned with the aid of the eye. The number of Japanese words of the several lengths in the three experiments and the average number of repetitions required to learn them are as follows:

5	one-syllabled	words	required	3.4	repetitions
45	two-	"	"	4.7	"
32	three-	"	"	6.2	"
40	four-	"	"	6.6	"
24	five-	"	"	6.9	"
5	six-	"	"	6.2	"
1	seven-	" word	"	9	"

The longer the foreign word, therefore, the more difficult it is to memorize. This also was the child's experience.

The 48 words of the first series contained 170 syllables.

The 52 words of the second series contained 154 syllables.

The 46 words of the third series contained 154 syllables.

The 6 extra words of the third series contained 26 syllables.

There were more long words in the third series than in the second; yet the third series was mastered more quickly, another evidence that the addition of the visual image makes for the rapid memorization of foreign nouns. The 14 words of the third list that were retained, however, contained on an average 3.6 syllables apiece, while those lost contained 3.4 syllables, a slight difference in favor of the retention of the longer word. In the case of the second series, however, the difference of 0.7 was in favor of the shorter word, and in the case of the first series it was very marked, being 2.2 in favor of the shorter word.

In the case of the child the difference was 0.3 in favor of the retention of the shorter foreign word, and 0.2 of the native word. It would appear, therefore, if what I have found for my boy and myself proves true generally, that not only are shorter foreign words much more easily learned by both child and adult, but they are also more easily retained. But in the case of the adult, the shorter foreign word was not retained better than the longer, when the words, in being learned, were seen as well as heard.

VI. Tentative Conclusions, Psychological and Pedagogical

1. In learning lists of some fifty names of animals this child ot about two and a half years added 2.33 words each repetition to his store of memorized words; I added 4.35 (4 without the

visual word-sign): so that I learned the words nearly twice as fast as the child.

2. Of a list of 52 names of mammals, the child's list being half English and half French and German, and my list Japanese, the child had retained 33% and I 71%, or more than twice as many, after a six weeks' interval.

3. After a six weeks' interval this child required a little less than half as many repetitions to relearn 52 names (one-third of which had been retained) as to learn them in the first place; I required one-third as many repetitions as at first, when I had retained about the same proportion as the child. I relearned them in one-third the number of repetitions that the child required.

4. An adult will learn foreign names faster than a child and remember them better. A child would therefore appear to have no advantage over an adult but rather the contrary in any method of learning a foreign language. It is probably chiefly the adult's trained and developed capacity of attention that gives him the advantage. His wider linguistic experience is also of importance, giving him many helpful associations to aid his memory. Meumann and his assistants found a similar advantage on the part of adults when compared with older children.

5. At two and one-half years the child's linguistic habits are already sufficiently formed by the acquirement of his mother tongue to make the acquirement of a foreign language considerably more difficult than that of his own. To an English-speaking child French is more difficult of acquirement than German.

6. The longer the *foreign* word, the harder it is both to learn and to retain, for both child and adult. With the visual image, however, I found long words at least as easy to retain (though not to learn) as short ones. The length of *native* words had little perceptible effect upon facility of acquisition in the case of the child, and but a slight effect upon ease of retention.

7. Words interesting as sounds whether from agreeableness or some other striking peculiarity are easy to retain, as *chickadee* and *macaw* for the child and *nemurinezumi* and *tokaké* for myself.

8. Words hard to pronounce are hard to learn and retain, as doves, secretary-bird, stormy petrel, musk-ox, hedgehog, zebra, *renard*, *volverenne*, *raton*, *Edelhirsch*, *Flusspferdt Renntier* for the child, and *nokogirizame*, *hikuidori* and *saezoridori* for me.

9. With the child the auditory image of a word seemed to be very closely associated with the visual image of the animal, the latter generally calling up the former directly and promptly. With me the motor impression seemed to be more important,

the ear serving as a more or less necessary critic of the efforts of the vocal organs to form the word. The visual image of the word was generally of least importance.

10. The addition of the visual word-sign made fewer repetitions necessary for my learning of words, but was of doubtful advantage for their retention. The lack of the visual image seems to make for a deeper motor and auditory impression and a solider attainment. The visual word-image makes for completeness rather than permanence of impression; the motor impression is more lasting though not so clearly and completely conscious. I ought to add that I have good visual as well as auditory imagination.

11. A foreign language may be learned more rapidly with the aid of the eye, but will be a more permanent possession if the attention is mainly focussed on the ear with consequent increased and *correct* use of the vocal organs. If a modern language teacher will read a great deal to his pupils in the foreign tongue and will use it himself a great deal in class, articulating with great care and accuracy, the pupil's ear will be trained to correctness of pronunciation and accent and his vocal organs will also unconsciously be exercised in the utterance of the correct speech which alone the pupil has heard and for which his ear is attuned. Thus the vocal and auditory organs will be constantly and more or less mechanically trained together, without the pupil's ever uttering a word; for we naturally repeat with unconscious movements of the vocal organs that which we hear. After some months of such unconscious learning, the pupil may be allowed to speak and read the foreign language and he will be found able from the start to do this with unwonted correctness and facility as regards pronunciation.

12. A given amount of time devoted to memorization will be more permanently effective if expended in frequent repetitions with considerable intervals, than if concentrated in fewer study periods, as discovered by Ebbinghaus with meaningless syllables and later demonstrated by Jost.

13. The actions of an animal attract a child's interest most, its form next and its color least. For this reason the child greatly prefers seeing the animal itself, especially when free, to seeing its picture, however good. Pictures of animals for children should represent them as doing things, fighting, running, chasing, climbing, swimming, drinking, etc. It is evidently not action, form and color as such, *i. e.*, as abstractions, that this child was interested in, but it is these as expressions of meaning, as interpretations of animal life. The action of the animals was fullest of meaning to this child, and consequently of interest, the form came next, and color had the least meaning.

Mere exercises in sense-training divorced from living interests are unpsychological.

14. The power of observation and redintegration, the ability intuitively to associate the various parts of which an object is composed with the object as a whole, is remarkably developed in the little child of two and a half years. There would seem to be no hard and fast line between intution and judgment.

15. Formal, direct teaching with little children is confusing and futile.

16. The curves of acquisition of various lists of some fifty names per list all showed from three to five marked periodical rises and falls, the recovery being generally slower in the case of the child than in that of the adult.

17. Work done under conditions of fatigue not only is harmful physically, but results in mental loss and deterioration. Time spent in study by a fatigued brain is worse than wasted.

18. A child of two and a half years has little power of continuous attention and he may suffer for a considerable time without giving any perceptible sign of fatigue other than that discovered indirectly by a careful examination of his work. A little child, like a "willing horse", can easily be overworked by appeals to his affection or interests. This is probably true of children all the way up. Our school boards and exploiters of child labor are oblivious of this as of much else that vitally concerns the child.

19. The child's earliest drawings showing perception of form are determined by his interest and experience. In this child's case they were of animals, the first at the age of 2 years 4 months 18 days. If the child's drawings are of any value as a test, reproduction or the recall of free memory images must be much later and more slowly developed in him than recognition. This would indicate that his wonderfully keen perception of form and action involves but a vaguely conscious analysis, is in fact very intuitive. Children would seem to know more than we give them credit for, but are deficient in power to reproduce and to express it. My child of three does not recognize the cruder drawings of men, horses, clocks, etc., made by children of his own age, and very similar to those he makes himself.

20. The phenomena of memory throw light upon the question of degrees of consciousness, showing them to be indefinitely many. A word may be built up in the mind by gradual stages of clearness and completeness and as gradually disappear.

SEX DIFFERENCES IN THE TAPPING TEST: AN INTERPRETATION

By FREDERIC LYMAN WELLS, Ph. D., Assistant in Pathological Psychology in the McLean Hospital, Waverley, Mass.

While it is doubtful if we ought to attach to the speed of repeated voluntary movements the fundamental significance accorded to it by some of its earlier investigators, certain sex differences have appeared in the writer's experience with the test that would seem to justify a brief presentation. The subjects whose results form the basis of the present discussion are ten men and ten women, with two exceptions nurses in the McLean Hospital, and of corresponding age and environmental level. It would be very difficult under other conditions than the present to equal these groups of subjects in homogeneity, co-operativeness, and unaccustomedness to the conditions of psychological experiment. For this reason, a significance may perhaps be attached to the present results which the limited number of subjects might otherwise largely vitiate.

The experiments with the ten men have already been described in detail,[1] and those with the ten women are essentially similar. Two experiments are performed with each subject, each consisting of five 30″ series of tapping with each hand, the right hand preceding in the first experiment and the left hand preceding in the second experiment. From the results of these experiments as performed, we derive various functions, the principal ones with which we are here concerned being as follows:

1. The relation to each other, in respect to gross rate, of five successive 30″ series of tapping with the same hand, the series being separated by rest intervals of 2′ 30″.

2. The gross fatigue effect, as given in the "index of fatigue," (*f*) which index is derived by dividing the average number of taps in the last five 5″ intervals (25″ of tapping) by the number in the first 5″ interval.

3. The fatigue curve, or curve of the decrease in the number of taps executed during the six successive 5″ intervals of a 30″ series.

4. The relation of the right hand to the left hand, as given in the "index of right-handedness" which is the average num-

[1] *American Journal of Psychology*, XIX, 1908, pp. 437 ff.

ber of taps executed in five successive 30″ series by the left hand, divided by the corresponding average for the right. The higher this index, the better the left hand in proportion to the right; above 1.00 the left is absolutely superior to the right.

5. The mean variation of successive experiments in the same individual, *i. e.*, the constancy of his results to each other.

We shall first examine those aspects of the results concerned with differences in the average of performance, and later those functions which deal with the relative variability of the sexes about these averages.

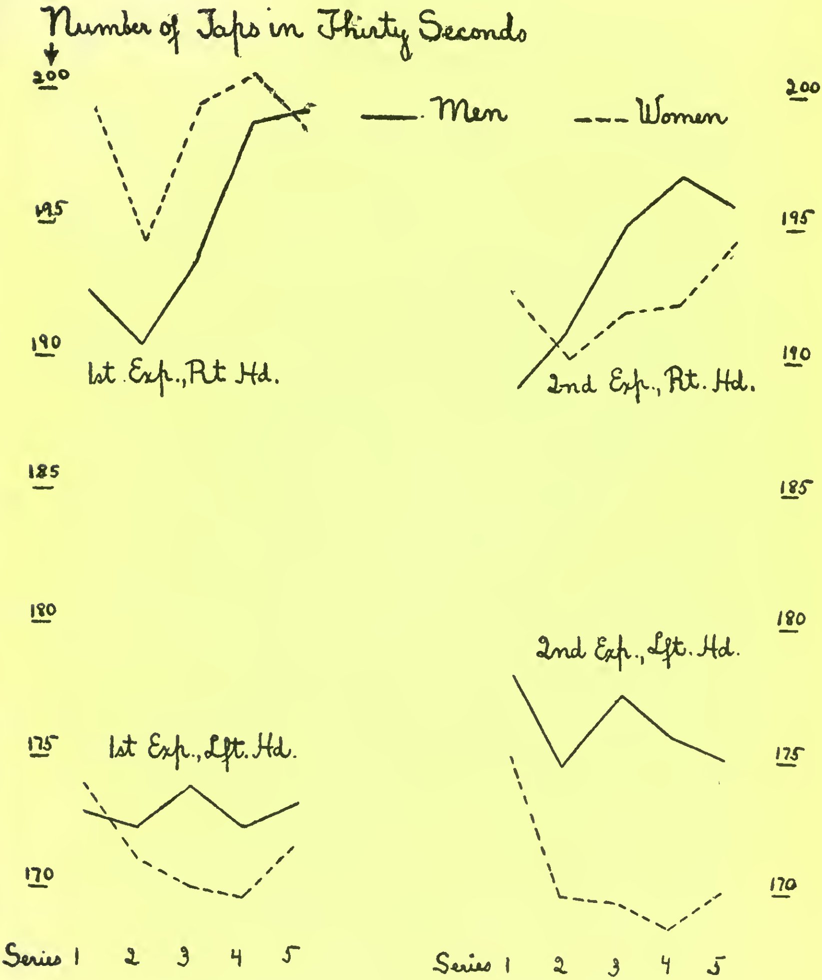
The previous researches which afford material for the study of sex differences in the tapping rate are concerned mainly with children and adolescents. While they do not maintain absolute correspondence, their general indication is that the boys are faster than the girls, and that the sex difference increases with age, so that we should expect to find here a consistent superiority of the men over the women.

The actual results are best presented in the form of curves, which give the total number of taps executed in each of five successive 30″ series of tapping. The continuous lines represent the tapping rates of the men, the dotted lines those of the women; the right and left hands, and first and second experiments being as indicated on the curves themselves. Thus in the first series of the first experiment with the right hand, the men execute 192.4 taps, the women 199.4; in the fifth (and last) series of the second experiment with the left hand, the men have 175.1 taps, the women 170. The curves give the fluctuations in rate from series to series. (Plate I.)

Briefly, the women surpass the men in the performance of the right hand in the first experiment. Elsewhere they are inferior, and they are much more inferior in the second experiment than in the first. The women are more favored in the first experiment, by virtue of its being the first, than the men, and they lose more in the second experiment, as such, than the men. The first experiment represented the first contact of any of the subjects with the test. There is thus of course a primary *Neuigkeitsantrieb* which is absent in the second experiment. This scarcely affects the men at all; indeed, their second experiment is as a whole somewhat better than the first. On the other hand, the women are in the second experiment markedly inferior to their first performance, in the absence of this *Neuigkeitsantrieb.*

There is a corresponding sex difference in the fluctuation in rate of the successive series, as illustrated in the curves. Just as the first experiment has a special *Neuigkeitsantrieb*, so is there a secondary *Anfangsantrieb* in the first series of each record of either hand. If the curves are examined, it will be seen that this first series is, relatively to the rest of the curve, very much higher in the women than in the men; *i. e.*, the women also indicate more strongly the presence of this secon-

PLATE I



dary *Anfangsantrieb*. The indication of *Schlussantrieb* at the conclusion of each experiment (the last series with the left hand in the first experiment, and that with the right hand in the second) is also more marked in the women.

As regards susceptibility to fatigue, previous studies, while differing somewhat from the present in manner of calculation, leave a slight balance of probability in favor of the men as the less susceptible to fatigue. As given in terms of the *f*, the present experiments average as follows :

Comparative *f* 's of the two groups.

	R1	R2	L1	L2
Men	.90	.91	.88	.90
Women	.90	.91	.87	.89

(In the tables, R1, R2, L1, L2 will be employed to indicate the first and second experiments with the right and left hands respectively.)

That is, the last 25″ tapping averages from 87% to 91% as fast as the first 5″. There is no immediate sex difference worth mentioning, especially when we find that the mean variations average about .03. Such infinitesimal differences as exist are in favor of greater fatigue immunity for the men. It is noticeable that in both men and women the left hand is more susceptible to fatigue than the right, and the first experiment more so than the second, this latter presumably a manifestation of less *Antrieb* in the second experiment.

The writer has previously mentioned that in the tapping test the fatigue phenomena seemed remarkably independent of the fatigue sensation accompanying the work. This generalization was based on experiments with men only, and the results with the women do not bear it out so well.[1] Each of the women was asked whether sensations of fatigue were present, and if so, in which hand they were the more prominent; it being then noted whether greater fatigue sensations on one side corresponded with greater fatigue phenomena on that side. The results were as follows.

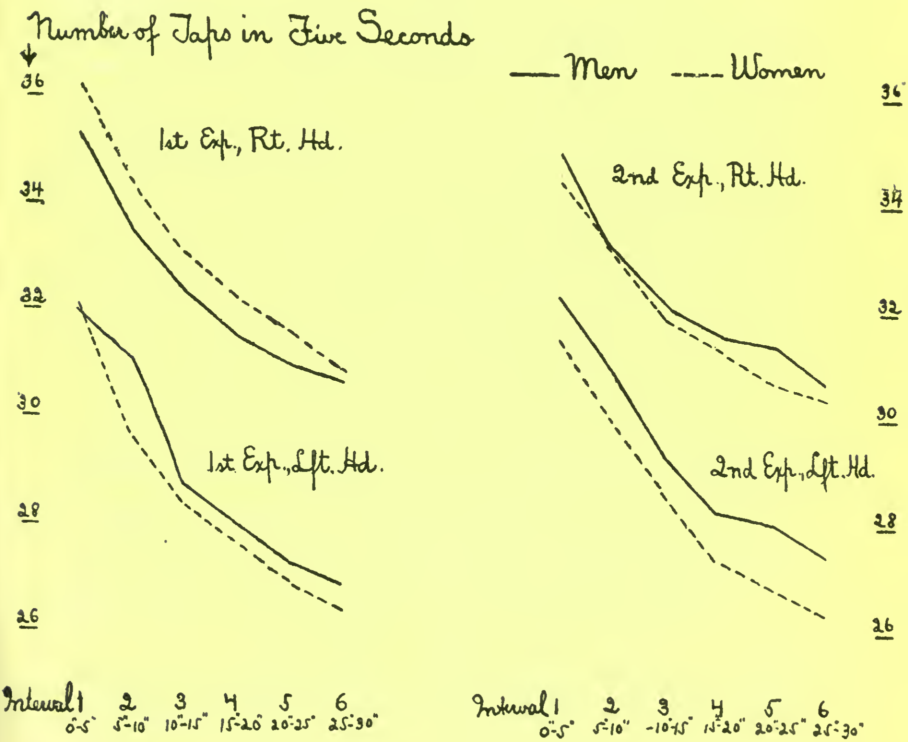
Sensations of Fatigue	*Phenomena of Fatigue*
None	No significant difference
Equal	Equal
Only in left	More in left
More in left	More in left
More in left	No significant difference
More in left	No significant difference
More in left	More in left
More "tension" in right	More in left
More in right	Equal
Probably more in right	More in left

We obtain a positive relation in five cases, four are equivo-

[1]The experiments with the men antedate those with the women by about a year.

cal, and one is negative. This correspondence, while by no means good, is rather better than seems to obtain among the men, though the inquiries with them were not so systematic. If there is a sex difference here, *i. e.*, in that the women's introspective account of fatigue sensation shows a better correspondence with the actual fatigue phenomena than that of the men, its most reasonable interpretation would seem to be that the women's performance is more influenced by fatigue sensation. It should be mentioned that subsequent features in the results on fatigability bear out this interpretation.

PLATE II



The accompanying cut shows the fatigue curves of the right and left hands in each experiment, the curves of the two groups of subjects being superposed. As previously, the continuous lines are the records of the men, the dotted lines those of the women. The curves are of the ordinary shape and show no characteristic sex difference in form. The relative inferiority of the women in the second experiment, already mentioned, is again evident. Another sex difference may be mentioned here, namely that the women are more inferior to the men with the left hand than with the right, *i. e.*, the right and left hands are farther apart in the women than the men. In the gross, this difference is best expressed in the index of righthandedness, which averages as follows in the two experiments.

	1st Experiment	2nd Experiment
Men	.89	.91
Women	.87	.89

In both groups the index is higher in the second experiment than in the first; *i. e.*, the preceding hand is more favored as such. The writer has previously mentioned this as a normal property of the test; but it is more probably related to *Anfangsantrieb* than to any generalized fatigue effect upon the following hand. It is seen also that the index is throughout lower in the women, the difference being just at the limits of probable error. This shows again that for this function there is more bilateral asymmetry, the right and left hands are farther apart, in the women than in the men.

Up to the present we have been dealing with averages only, reserving the mean variations for separate discussion, since these have a special bearing upon the question of sex differences. It seems to be very generally accepted that at least in most aspects, individual differences tend to be greater in men than in women, and it may be worth while to examine the data from this point of view. Only the mean variations need be presented, since it would be hardly possible to consider the more minute features of the problem of variability from a basis of ten cases of each group.

In initial rate, *i. e.*, rate for the first 5″, as well as in gross rate, *i. e.*, rate for the whole series of 30″, the mean variations of the different groups are as follows.

	R1		R2		L1		L2	
	5″	30″	5″	30″	5″	30″	5″	30″
Men	3.3	14.0	3.9	14.3	3.0	17.6	3.5	15.9
Women	2.8	14.3	2.1	13.8	2.3	13.2	2.8	14.6

The women are practically throughout less variable than the men. In the m. v. of the gross rate (30″) this difference is not so well marked, and for a reason that is worth analysis. Attention has already been called to the fact that there was in the men a marked tendency for fatigue to *decrease* the individual differences. This tendency is also present in the women, but very much less so than in the men; indeed, at the end of the 30″ the variability of the two sexes is practically equal. Under fatigue, the individual differences of the women become relatively greater than those of the men.

As it previously appeared that there was no characteristic sex difference in the average of the f's, so here we fail to find it in their variability. The mean variations of the f's are practically the same in men and women throughout. As may however be inferred from what has just preceded, this equality is purely superficial; the f itself shows no sex difference, but both of its factors (the first and the remaining five intervals) do. Inasmuch as the women are to a certain extent less variable in the initial interval, if they continued to be equally so throughout the rest of the fatigue curve, the m. v. of the f's would show equally the smaller variability of the women; but, inasmuch as we find the variability of the f's equal, or nearly so, it follows that the fatigue curves of the women have under fatigue gradually increased in variability relatively to those of the men.

Absolutely, the fatigue curves of the women (whose average is given in the cuts on p. 357) resemble each other in form somewhat the more closely, as is indicated in the fact that in the women the mean variations of the successive 5″ intervals are consistently smaller than those of the men. It is in this, an important aspect of the results, that smaller variability of the women is the most unequivocally illustrated. The average m. v.'s of the six successive intervals in the two experiments for the men and women are as follows:

	R1	R2	L1	L2
Men	2.4	3.0	2.4	2.7
Women	2.1	1.9	2.3	2.4

In view of what has been said, however, it is more than probable that at the end of 60″ of tapping the variability of the women would exceed that of the men.

It was noted previously that the right and left hands are farther apart in the women than in men. It is also observable that the difference between the right and left hands is more variable in the women than in the men, the figures being as follows.

	1st Experiment	2nd Experiment
Men	.035	.033
Women	.047	.041

The relationship of the two hands in these experiments is affected mainly by the matter of precedence of either hand in the test. We saw that this factor affected the tapping rates of the women somewhat more than those of the men, and we see here that it also affects them more variably than those of the men.

As an aspect of the same general result, it may be noted that the day to day variability in gross rate was in the women strikingly greater than in the men. Taking the mean variation of the two experiments performed for each hand in each subject, we obtain for the two experiments the following results in the men and the women.

	Right	Left
Men	1.6	3.5
Women	3.4	3.9

That is, the first and second experiments differed much more from each other in the women than in the men. (*Cf.* p. 354.)

Considered individually, one could attach no special significance to these data as isolated facts; and it is much more reasonable to regard them as secondary to other characters of sex difference having a more fundamental value. The results are best discussed in their relation to two such characters, which may be cited according to Havelock Ellis, as the lesser variability of women, and the greater affectability of women. The latter is perhaps the most fundamental sex difference that exists on the psychical side, and indeed, were it not so apparent to everyday observation in general, it could be asserted positively for the group of subjects immediately concerned. This being the case, we must expect that the affective factor in every feature of the experimental conditions will influence the performance of the women to a significantly greater extent than it will those of the men. And in review of the points of sex difference previously mentioned, it seems that, as the sex differences appear most markedly in those features of the experiments in which the affective element is the most prominent, so are these differences most reasonably interpreted as the expression of this fundamental sex difference in affectability. It is, of course, possible to construct other hypotheses which will cover the various points of difference observed; but it is a precarious logical system which interprets with a variety of assumptions a series of observations referable to a known fact.

To this we must add, that besides being more affectable, the women were much more variable in their affectability than the men. This was clearly observable in the differences of experimental attitude. The women went at the test in much less of a routine manner; they more frequently evinced desire to examine and understand the apparatus, and were more likely to express concern as to the object and outcome of the tests, and regarding future experiments. The men showed a more uniform behavior in these respects than did the women, and their attitude toward the test was in general a more "objective" one. Among the women a varying amount of persuasion was occasionally demanded; but it was always apparent that the subjects were really co-operative, and that the seeming inadequacy was wholly the product of an exaggerated affective reaction to the unaccustomed conditions of the experiment.

There are operative in these experiments four special affective influences: the precedence of the hands in the experiment, the sensations of fatigue, the use of the preferred or unpreferred hand, and the relative novelty of the test. The sex differences are marked principally according to the amount which these factors influence the special aspect of the results under consideration.

To particularize, the women are much more susceptible to the influences of *Antrieb* than are the men. The *Anfangsantrieb* of the inception of the experiment was illustrated in the record of the preceding right hand in the first experiment and perhaps even more strikingly so in the second experiment with the preceding left hand. As between the successive 30″ series, the influence of a secondary *Anfangsantrieb* is apparent in that the first series of each hand is in the women relatively superior to the first series in the men. The apparent failure of the women to bear up so well under fatigue sensations indicates a heightened responsiveness to an affect of the opposite character.

If the greater difference between the right and left hands in the women is significant, an analogous interpretation is possible in that we perhaps try harder in doing what we expect to do better. The subjects naturally expect to do better with the right hand, and so try harder with it; the women, on an average, harder than the men. In a more affectable group, the hands might thus appear, in general, farther apart.

There are certain features of the test which we should hardly expect to be influenced by these differences in affectability, and which in fact do not seem to be so. The individual differences in tapping rate are slightly greater in the men than in the women, and the form of the fatigue curve is also more constant in the men, though it tends to become less so. Here we seem to have a sex difference referable rather to the generally lesser variability of women, already mentioned as a fundamental sex difference. In the remaining results, however, we find this general tendency toward lesser variability in women in conflict with the special factor of the greater affective variability of the women, and we shall find that in these cases, this latter factor is sufficiently potent to more than offset the other, so that the final figures show from almost every viewpoint, a lesser variability of the men.

Mention has been made of the fact that under fatigue the individual differences of the women do not show the same tendency to decrease as those of the men. As we previously judged the women to be more responsive to sensations of fatigue than the men, we now find them to vary more in their responsiveness to them than the men, the former being an aspect of the women's greater affectability, the latter of their greater affective variability. And as we found the right and left hands to be father apart in women than in men, so now we find the index of righthandedness to be more variable in women than in men; the greater difference a product of greater affectability, the greater variability of the difference a product of the greater affective variability.

The greatest affective difference that is objectively given in these tests is perhaps that between the first and second experiments; certainly the sex difference is very well marked here, the women being relatively much better in the first experiment than in the second. The women respond readily to the *Neuigkeitsantrieb* of the first experiment, and miss more strongly the interest of novelty in the second; a more even experimental attitude is indicated in the men.

To briefly recapitulate, no immediate sex difference was indicated in the absolute rate of tapping, in the gross amount of fatigue loss during the 30″ period, or in the form of the curve of this fatigue loss. More detailed examination of the results, however, indicated sex difference as follows:

1. The women relatively surpass the men in those periods of work most likely to be subject to special *Antriebe*.

2. The introspective accounts of the fatigue sensations given by the women agree better with the objective fatigue phenomena than those of the men seem to do; hence the women are probably more influenced by fatigue sensation than the men.

3. The right and left hands are farther apart in women than in men.

4. In initial rate (5″) the women are considerably less variable, in the gross rate (30″) slightly less variable than the men.

5. In the 30″ work period employed, the shape of the work curve is considerably less variable in the women than in the men, but the women show a tendency to relatively increasing variability under fatigue. (*Cf.* 2.)

6. The relationship of the right and left hands is more variable in the women than in the men.

7. The first and second experiments differ more from each other in the women than in the men.

The above sex differences are found mainly in those features of the experiment which especially involve the affective factor in the subject's attitude; and they are manifestations of the greater responsiveness of the women to this affective element.

When this factor is not especially involved, the individual differences of the women tend to be less than those of the men; but when it is involved they are greater, illustrating not only the presence of greater affect among the women, but also the greater variability of this affect. Thus the differences found are not fundamental sex differences, but are secondary to certain differences in temperament; and in a group of subjects in which these temperamental differences were by any chance reversed, we should expect to see the experimental differences also reversed.

It is very possible that a practiced experimenter can interpret these temperamental differences in the subjective attitude from observations of the general behavior under experimental conditions more reliably than through the crude introspection of a naïve subject. And though introspective accounts of the subjective attitude often seems to add little enough to the significance of the experiment, we might still be exposed to grave error in concluding from this that the subjective attitude does not still influence the results of such observations to a marked degree. In the foregoing, it has been endeavored to indicate the extent to which certain rather elementary motor functions might be influenced by what is perhaps the most important single factor in the subjective attitude, its susceptibility to affective influences.

THE RELATION OF ACCURACY IN SENSORY DISCRIMINATION TO GENERAL INTELLIGENCE

By Professor Edward L. Thorndike, Dr. Wilfrid Lay and Mr. P. R. Dean.[1]

The purpose of this paper is to present certain new data concerning the relationship mentioned in the title and to show the bearing of these data upon the conclusions set forth by Spearman in his *General Intelligence Objectively Determined and Measured* which appeared in this *Journal* (Vol. XV, No. 2) in April, 1904.

The Original Measures

The measurements from which conclusions will be drawn were made by Dr. Lay upon 37 young women students in a normal school and by Mr. Dean upon 25 high school boys (all in the 3rd year of the high school course). The 37 young women drew each 90 lines, 30 as nearly as possible equal to a 100 mm. standard, 30 as nearly as possible equal to a 75 mm. standard and 30 as nearly as possible equal to a 50 mm. standard. They also each filled 16 boxes with shot, 8 as nearly as possible equal to a 100 g. standard and 8 as nearly as possible equal to a 200 g. standard. Each one rated all the rest in order of merit for general intellect, using each her own conception of what general intellect was as the basis of grading. Eight of the professors in the normal school also graded each of them (with a few exceptions in the case of three of the teachers) in the same way. Their scholastic records in the normal school were also used as measures. I use the average deviation from the standard as the measure of inaccuracy in the case of the tests with lines and weights. Some reasons might be adduced for choosing the variability around the individual's constant error instead, but there are far weightier reasons against doing so.

The 25 boys drew lines similarly except that some drew fewer than the 90 while others drew more; made up weights

[1] The shares of the authors in this research were as follows: The original measurements were taken by Dr. Lay and Mr. Dean, who also calculated some of the deviation measures and correlations. The remaining calculations were made by Professor Thorndike who was also responsible for the research and for the account of it here given.

similarly; were estimated similarly by 6 of their own members and 4 of their teachers.

The lines were drawn and the weights made up under the same conditions for all students within each group. The tests covered several days for each individual, so that spurious correlation from fatigue, temporary illness, etc., was reduced to a small amount. Within each group differences of age and maturity are for our purpose so slight as to be negligible. The median deviation of the young women in age was only 10 months and that of the boys only about one year.

We have then for the women fairly accurate measures of accuracy of discrimination of these lengths, accuracy of discrimination of these weights, intellect as judged by one's fellow students and intellect as judged by one's teachers. Such measures from two random halves of the scores correlate to .665, .504, .915, .72 and .62 respectively, which means that the measures used from the entire scores for each individual would correlate with other similar sets to about .8, .7, .9½, .9 and .8 respectively.

In the case of the boys the opinions of intellect of fellow pupils and teachers were combined. We have then for the boys fairly accurate measures of discrimination of these lengths, discrimination of these weights, intellect as judged by one's teachers and fellow students, and scholarship. Such measures from two random halves of the scores correlate to .691, .722, .869 and .873 respectively, which means that the measures used would correlate with other similar sets to about .8, .8, .9½ and .9½ respectively. The "raw" correlations from which we have to argue are consequently subject to only very moderate "attenuation" from chance variations in the obtained measures from the true measures for which they stand. The number of cases is sufficient to determine close correlations with a very small margin of probable deviation from the true result. When the relationship is only slight, the reliability of the result is, of course, much less, but is still sufficient to prevent insecurity in any of the general conclusions which are of interest.

To these conclusions I proceed at once, referring the specialist in mental relationships to the detailed table at the close of this paper.

The Measures as Samples of 'Intellect' and of 'Sensory Discrimination'

Intellect as judged by teachers and intellect as judged by fellow students are much the same thing. The raw correlation in the case of the woman students is .85. This becomes about .95 when allowance is made for the inadequacy of the original measures. The raw correlation in the case of the high school boys is .76. This becomes nearly .9 when the Spearman correction is applied. The women preparing to become teachers naturally weight aspects of intellect more in the fashion of the teachers than do the high school boys. But the congruence of school-boy's and school-teacher's opinion is remarkable.

In the case of the normal school women scholarship is an almost perfect symptom of intellect as they and their teachers judge the latter. The correlation between the combined judgment of fellow pupils and teachers and the scholarship record is .85, which becomes about .95 when allowance is made for the inaccuracies of the latter. And much the same would of necessity hold of the judgment of fellow-pupils alone.

With the high school boys scholarship is by no means a perfect symptom of intellect either as judged by the boys or as judged by their teachers. The correlation is naturally somewhat higher in the latter case, but it is by no means high. These relations are approximately

.6 for scholarship and intellect by the combined judgment,
.6 " " " " " " teachers' judgment, and
.4 " " " " " " pupils' judgment.

This difference is, of course, what should be found from accurate measures, since the students in the professional school do devote their intellects to scholarship, and do, so to speak, measure their intellects by it, more than is the case with the high school boys. Scholarship is, with the latter, in large measure a product of interest rather than ability.

From these facts it is evident that in the case of the high school boys the three measures,—teachers' opinions, fellow-students' opinions, and school marks,—are something like a fair sampling of measures of general intellect. In the case of the women students the sampling is much weighted in favor of the scholarly sort of intellect.

The discrimination of lengths and the discrimination of weights are known to be random samples of sensory discriminations for the very good reason that they were picked at random.

The Relation of 'Intellect' to 'Sensory Discrimination'

From his measurements Spearman calculates that the factor common to school marks and ratings as to 'common sense' by fellow-students and teachers, correlates perfectly with, and hence is identical with, the factor common to discrimination of pitch, discrimination of light intensities and discrimination of weights, and concludes that there exists "a correspondence between what may provisionally be called 'General Discrimination' and 'General Intelligence' which works out with great approximation to *one* or *absoluteness*.''

The measurements obtained in the present investigation do not in the least support this hypothesis. The correlation between whatever is common to (A) drawing 50, 75 and 100 mm. lengths accurately and (B) making weights equal to 100 g. and 200 g. standards and whatever is common to (C) intellect as judged by fellow-students and (D) intellect as judged by teachers does not come out as 1.00 but as .26 or .15 according as we apply the first or second of Spearman's correction-formulæ. When, in the case of the high school pupils, the two measures of general intellect taken are (C) combined student's and teacher's estimates and (D) school marks, the correlation comes out .29 and .22 by the two methods.

That is, the most probable relation between the factor common to all sensory discriminations and the factor common to intellect judged in these three ways is, from our data, not 1.00, but .23.

It is perhaps best to wait for further and fuller measurements of the relation in question before attempting to explain the difference between this result and Spearman's. But one fact may be noted now. With young children a test designed to measure sensory discrimination may easily become, to a considerable degree, a measure of ability to understand instructions, that is, of one feature of general intellect.

The variety of measures taken and the elaborate corrections made by Spearman make a detailed comparison step by step of his and the present research difficult and in the end unproductive. The essential differences are (1) that Spearman does not give measures of the reliability of his measures of any species of sense discrimination or of any but a few of his measures of intellect and (2) that his material is complicated by age and sex.

The theoretical importance of Spearman's conclusion lies in the support which it would give, if verified, to the hypothesis that the efficiency of what may be called the general mammalian foundation of the central nervous system is closely correlated with what may be called the specifically human neurone-connections. The present results support the contrary hypothesis, that the efficiency of a man's equipment for the specifically human task of managing ideas is only loosely correlated with the efficiency of the simpler sensori-motor apparatus which he possesses in common with other species.

Spearman's other main conclusion is ''*that all branches of intellectual activity have in common one fundamental function* (*or group of functions*), *whereas the remaining or specific elements of the activity seem in every case to be wholly different from that* *in all the others*." This is, of course, contradicted by the correlation of .23 instead of 1.00, and also by the fact that we obtain a much higher correlation between discrimination of lengths and discrimination of weights than between either one of them and general intelligence. From our figures the correlations, if perfect original measures were at hand, would be about .15, .25 and .50 respectively for accuracy in drawing lines with general intelligence, accuracy in making up weights with general intelligence and accuracy in drawing lines with accuracy in making up weights.

I may add that other studies of correlation made by my students and myself are unanimous in contradicting Spearman's ingenious hypothesis of one sole common element as the cause of all positive correlations. We find, for example, that efficiency in marking A's on a sheet of printed capitals, efficiency in finding circles or hexagons or isosceles triangles on a sheet of printed geometrical forms and efficiency in finding misspelled words are in adults all very closely intercorrelated (to .8 or more), but are by no means so closely correlated to general intellect. In general there is evidence of a complex set of bonds between the psychological equivalents of both what we call the formal side of thought and what we call its content, so that one is almost tempted to replace Spearman's statement by the equally extravagant one that there is *nothing whatever* common to all mental functions, or to any half of them.

I regret that it is out of question to print the original measures. But as the record of each individual of the 62 measured comprises 90 numbers representing errors in drawing lines, 16 numbers representing errors in estimating weights, 10 or 42 rankings in estimating weights, 10 or 42 rankings in intellect and from 12 to 30 marks for scholarship, the total table would require nearly 10,000 entries. The table of correlations follows. Column I gives the results from the women students; Column II gives the results from the high school boys.

TABLE I

ACTUALLY OBTAINED ("raw") CORRELATIONS: PEARSON COEFFICIENTS

	Correlation	I	II
1.	Scores from half of the 100, 75 and 50 mm. lines with scores from the other half	.665	.69
2.	" " " " 100 and 200 gram weights " " " "	.505	.72
3.	" " " " pupils' impressions of intellect " " " "	.915	
4.	" " " " teachers' impressions of intellect " " " "	.72	
5.	" " " " academic records " " " "		.87
6.	" " " " combined pupils' and teachers' impressions of intellect with scores from the other half " " " "		.87

TABLE I—*Continued*

		I	II
7.	Scores from all the lines with scores from all the weights	.52	.25
8.	" " " " " " " " pupils' impressions of intellect	.25	
9.	" " " " " " " " teachers' " " "	.12	
10.	" " " " " " " " pupils' and teachers' impressions of intellect combined		.055
11.	" " " " " " " " academic records		−.01
12.	Scores from all the weights, with scores from all the lines	.52	.25
13.	" " " " " " " " pupils' impressions of intellect	.235	
14.	" " " " " " " " teachers' " " "	.08	
15.	" " " " " " " " pupils' and teachers' impressions of intellect combined		.205
16.	" " " " " " " " academic records		.21
17.	Scores from all the pupils' impressions of intellect with scores from all the teachers' impressions of intellect	.85	
18.	Scores from all the pupils' and teachers' impressions of intellect with scores from entire academic records		.54
19.	Scores from all the lines and weights with scores from all the teachers' and pupils' impressions of intellect combined	.165	
20.	Scores from all the lines and weights with scores from combination of teachers' impressions, pupils' impressions and academic records		.145

COEFFICIENTS CORRECTED FOR CHANCE VARIATIONS IN THE ORIGINAL MEASURES BY THE SPEARMAN METHODS OF CORRECTION

I. (A) The factor common to accuracy in lines and accuracy in weights with (B) the factor common to pupils' impressions of intellect and teachers' impressions of intellect .20

(A) As above, with the factor common to the combination of teachers' and pupils' impressions and academic scholarship. .255

The most probable correlation between "general discrimination" and "general intelligence" is thus .23.

AN APPARATUS FOR THE STUDY OF KINÆSTHETIC SPACE PERCEPTION.

By JAMES H. LEUBA, Bryn Mawr College, Pa.

The accompanying drawing reproduces the apparatus as set up for forearm movements.

The subject sits or stands with the arm over the lever (*L.*). The hand-holder (*H. H.*) is put at such a distance from the shaft (*S.*) that the centre of the elbow joint is opposite the end of the shaft. The forearm does not rest on the lever, but is entirely supported at one end by the hand-holder, and at the other by the upper arm. Thus, only the hand of the subject is in contact with the instrument, unless, instead of making use of the hand-holder, the forearm be tied to the lever by a soft bandage. (See below, No. 5.) When the point of the lever indicates zero on the scale, the upper arm hangs vertically from the shoulder and the forearm is horizontal.

The two essential dispositions of the apparatus are the following:

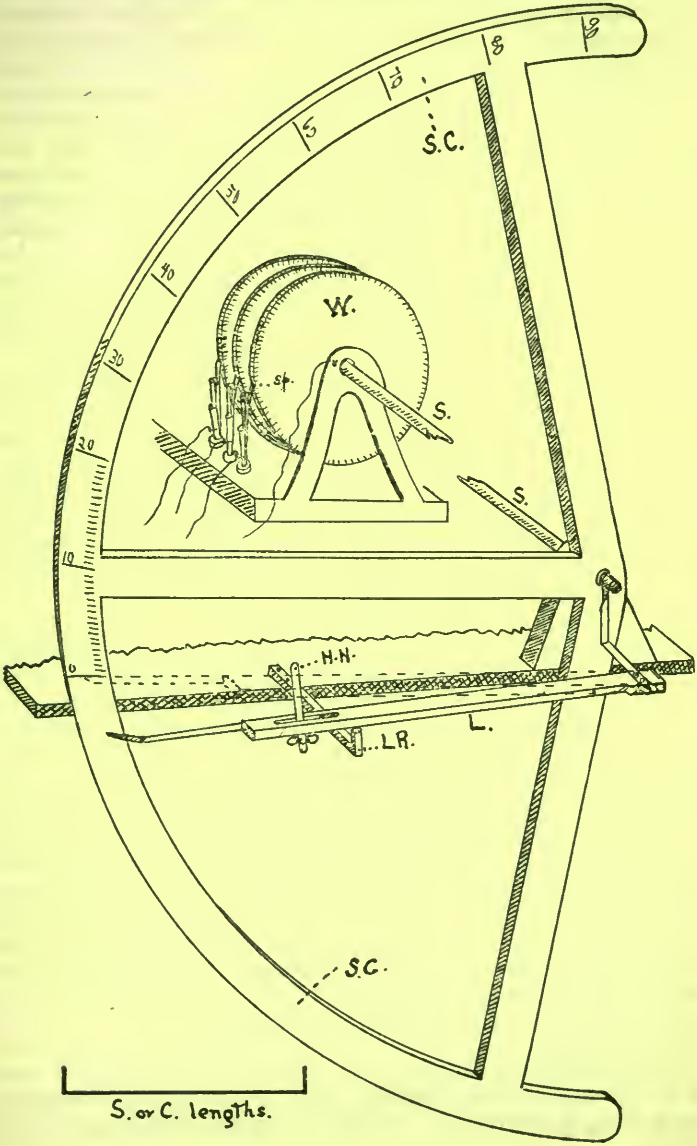
1. The arm movements produce the rotation of a shaft placed at right angles to the plane described by the moving arm and directed towards the centre of rotation of the arm, *i. e.*, the elbow joint.

2. On this shaft a wheel (*W.*) of non-conducting material (fibre) is fixed, bearing across its edge metallic blades at equal distance from each other. These blades are connected with the axis of the wheel, and the axis with an ordinary recording magnet with which is also connected a spring (*sp*) pressing against the edge of the wheel. Thus an electric circuit is completed whenever one of the blades passes under the spring and recorded on a kymograph drum.

As an ordinary recording magnet cannot be made to move at a greater speed than 100 movements a second, several wheels may be used. If three wheels, each connected with a separate recording magnet, be used and the distance between the blades on the rims of each wheel be 2 degrees, movements of any velocity, up to 450° a second (that is, ⅔ × 300), can be recorded.

The wheels are to be adjusted on the axis so that they produce contacts successively and not simultaneously.

The distance between each two successive downward movements made by each one of the recording magnets corresponds to an arm movement of 2°. But by combining the lines traced



by the three markers, the distance of two successive movements corresponds to ⅔ of a degree. Thus the length of the arm movements, wherever made on the semi-circle, can be computed on the kymograph records. If a time line be added, the total duration of the movement as well as its velocity at any point can be read.

This method of determining the length and the duration of a movement is evidently subject to an error, an underestimation: the movements begin to be recorded only when the first contact is made, and cease to be registered when the last contact takes place, although the movement usually begins a little before and continues a little after. But the average amount of this underestimation, depending as it does upon the distance of the blades from each other, is easily calculated. Moreover, this error affects equally, on the average, the standard and the comparison movements. It can, therefore, be looked upon as immaterial, at least when the movements have a certain length and when the number of tests is sufficiently large.

It should not be overlooked, however, that one is not dependent upon the drum records for a knowledge of the length of the movements. Except when the movement is a free-ending one, the lengths of both the standard and the comparison are determined by the operator himself. And as to the free-ending comparisons, their lengths can be read directly on the semi-circle (*S. C.*).

The duration error mentioned above can easily enough be done away with by discarding the wheels, connecting the end of the lever (*L.*) with the recording apparatus, and using metallic standard and comparison lengths (*S.* and *C.* lengths) also connected with the recording magnets. These lengths may be made of sheet metal bent at both ends and placed wherever wanted on the semi-circle. In the initial position the point of the lever is in contact with one of the bent ends and in the final position, with the other. Thus an electric contact is broken at the beginning of the Standard and of the Comparison and one is made as they are completed. This method, used by us in several series, has its advantages and its drawbacks. It cannot, of course, be used when the comparison is to be a free-ending movement. And the click produced by the end of the lever striking the ends of the metallic lengths is an undesirable complication.

This apparatus meets, to some extent at least, a need long felt in the study of kinæsthetic space perception by means of arm and leg movements. Its chief uses and advantages are as follows:

1. The movements are arc movements, *i. e.*, movements involving but one joint and the muscles operating it.

2. It is usable for movements of the forearm only, or of the whole arm, of any length and at any point of the arc the arm can describe. The accompanying drawing represents the apparatus set up for the study of vertical movements; but it can be set up for the investigation of movements in the horizontal plane.

3. It can be used for active and for passive movements, and also for the study of the influence of resistance. For this last purpose, a weight is attached to a wire running in a groove made in the rim of one of the wheels.

4. In the case of movements of the fore-arm in the vertical plane, the upper arm hangs freely from the shoulder, and thus disturbing strain elements, present in most of the other methods, are eliminated.

5. It does way with the sensation-complications produced by holding a pencil and drawing a line, or by marking the beginning and the end of the lengths by a pressure. For it was found easy for the observer to maintain practically the same pressure on the hand-holder (*H. H.*) from the warning signal to the return to the rest position. It may be preferable, however, to tie the arm to the lever, and thus do away with the possibility of changes in the hand pressure synchronous with the movements compared.

6. Used as we have used it, it is soundless. The only parts of the apparatus which make any noise are the wheels (*W.*) in rubbing against the springs (*sp*) and the recording instruments. In our experiments these were placed in a room adjacent to the one occupied by the observer.

7. The elimination of the sensations arising from contact of the air with the skin of the arm and hand, is secured by the use of a loose glove which reaches nearly to the elbow where it is covered by a loose sleeve. This, together with the elimination of sound and of the other disturbing sensations already referred to, leaves as factors to be considered practically only the joint, the tendon, and the muscle sensations. The direction of the attention to one or the other of these is thus made considerably easier.

8. It provides for the recording of the duration of movements of any length, performed at any point of the arc described by the arm, in both directions. It may thus be used for an exact study of the duration and of the velocity factors in the estimation of spatial arm movements. Free movements are recorded and timed as well as those that are objectively limited.

9. The apparatus can easily be adapted to the study of leg movements.

THE INFLUENCE OF THE DURATION AND OF THE RATE OF ARM MOVEMENTS UPON THE JUDGMENT OF THEIR LENGTH

By PROF. J. H. LEUBA, assisted by MISS ETHEL CHAMBERLAIN, Fellow in Psychology, Bryn Mawr College

Description of the Experiments. During the past year I undertook, with the help of the apparatus described in the preceding pages under the name of "An Apparatus for the Study of Kinæsthetic Space Perception", a three-part investigation of the duration and of the velocity factors in the estimation of the length of arm movements made by the forearm. Two of these parts are still unfinished. They deal, one with the source of the constant error arising when a length, objectively limited, is reproduced by a free, *i. e.*, unlimited, movement; the other with the effect of voluntary changes in the velocity with which the reproducing movement is made.

The part here described consists of four series: 1. a Normal Series (N. Ser.)[1]; 2. a Weight Resistance Series (W. R. Ser.)[1]; 3. a Muscle Resistance Series (M. R. Ser.)[1]; 4. a Control Series, or an Increasing Weight Resistance Series (I. W. R. Ser.)[1].

The method used throughout was the modification of the Method of Average Error called the Method of Constant Stimuli. The observer's task in this method is to compare a standard movement with a number of fixed comparison-lengths so chosen that the smallest seems distinctly shorter and the largest clearly larger than the standard (St.)[1], and to express his judgment by one of the words, "smaller", "equal", "greater". I have used only three comparison lengths (C.)[1]. The results show this small number to have been sufficient for our purpose.

In each of the four series, the St. Movement (Mt. in the singular and Mts. in the plural)[1] was a length of 8° gone through in the upward direction and beginning each time at a different point between 0° and 5°. The zero point is the one indicated by the pointer when the arm is held horizontally. The reason for this latitude in the starting point of the St., and for a similar latitude in the starting point of the C. was

[1] These abbreviations will be used throughout the article.

the wish to make judgments by means of the sense of position impossible.

In the N. and in the W. R. Ser., the C. Mts. began between 25° and 30°, *i. e.*, approximately midway between the position of the St. in all Ser. and that of the C. in the M. R. Ser.

In the N. Ser. the Mt. was unimpeded; in the W. R. Ser. a resistance of 1½ kilogr. was provided in the form of a weight attached to a wire running in a groove made in the rim of one of the wheels, the whole being so disposed that the weight was put on only after the St. and before the C. Mt.

In the M. R. Ser., the C. lengths were so placed on the semi-circle as to end at different points within 3° of a point 1° from the extreme position the arm was able to reach. A rest was provided in this series for the back and the head of the subject so that his position with regard to the apparatus would remain constant. A very considerable resistance, due to the pressure of the forearm against the biceps, was thus introduced. It is to be noted that this resistance was not constant from the beginning to the end of the C. Mt., as it was the case in the W. R. Ser., but that it increased steadily until its conclusion.

Two subjects performed the four Ser., one of them (R.), a graduate student in psychology, the other (L.), the writer who was the only subject informed regarding the purpose of the experiment and the detail of procedure. A third subject (H.), a senior student, who had already had a good deal of experience in the comparison of arm Mts., served for the W. R. and the M. R. Ser.

The observers worked with their eyes bandaged, except L. who kept them closed without that help. Practice series were given before each new Ser. and a few tests at the beginning of every session. These sessions were limited to about half an hour's duration, broken by several interruptions for rest.

Introspection was asked for only towards the end of each series.

The directions given to the subjects were simply that they concentrate their attention upon the length of the Mts. and express by the words, "shorter", "equal", and "longer", the relation of the C. to the St. The words "duration" and "velocity" were not mentioned to them, at least not before the introspective account was asked for. They were thus left to move at the speed they liked best.

Results. I. I give first, as a sample, a complete record of the N. Ser. of L. (Table I). The figures indicate, in terms of fiftieths of a second, the duration of the St. and of the C. Mts. when the C. were respectively 7°, 8°.5, and 10° and also the judgment passed by the subject in each case. Observe the

TABLE I

Normal Series

St. = 8°

Subject L
Equality judgment 8°.4

	C. = 7°						C = 8°.5						C. = 10°.					
	−		=		+		−		=		+		−		=		+	
	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.
	67	85	66	88			73	80	75	84	82	87			83	101	79	93
	84	72	83	72			74	58	81	88	80	91			71	97	97	95
	63	82	119	68			78	62	81	90	67	88			111	88	73	106
	85	57	77	42			63	68	77	76					86	88	88	102
	60	65	63	48			79	56	94	77					58	66	112	101
	83	67	86	81			73	58	86	93							69	90
	60	49					79	77	75	69							82	113
	75	54					69	83	75	82							86	109
	58	52							95	65							76	94
	87	69							85	75							53	94
	83	59							76	69							73	102
	79	62							47	64							73	101
	92	59							55	55							70	64
	74	68							63	52							69	74
	74	61							60	79							70	80
	67	66							106	92							70	85
	90	50															74	81
	71	43															77	56
	88	—															59	78
	87	55															71	72
	74	46															79	83
																	64	94
Sums	1601	1221					588	542	1231	1210							1664	1967
Aver.	76	61					73.5	68	77	76							76	89

Average of all — judgments, St = 75, C = 61, Differ. = — 14.
" " " = " St = 79, C = 76, Differ. = — 3.
" " " + " St = 76, C = 90, Differ. = + 14.

considerable variable error in the duration both of the St. and of the C. In column 7°—, for instance, the St. times vary from 58 to 92. The differences between the compared St. and C. are also considerable. They range in the same column from +19 to —40. Similar variations were found in series of experiments in which the observer was comparing not the lengths of Mts., but the *duration* of pressures upon his forefinger. I shall use this similarity in a subsequent article to reinforce the thesis that in the estimation of the length of Mts. here dealt with, it is the duration and the velocity, not the length, which are directly perceived. Nevertheless, the average duration of the St., leaving out the columns containing too few tests, is strikingly uniform: 76 for column 7°—; 77 for column 8°.5=, and 76 for column 10°+. We look upon this uniformity as an indication of the sufficiency of the number of tests taken.

The duration of the individual Mts. varies for L. in this series from a little below one second to a little over two seconds. He moved more rapidly in the M. R. and the I. W. R. Ser.

The most interesting figures of the table are those expressing the duration of the C. When the C. was 8°.5 in length and felt equal to the St., St. and C. were made practically in identical times (77 to 76); when the same C. was felt shorter, its duration was materially less than that of the St. (73.5 to 68). If we pass to the comparison of the St. with a C. length of 10° (felt markedly longer), we find that it took a much longer time to make the C (76 to 89). The proportion existing in this case between the duration of the compared movements is practically equal to that existing between their lengths: $\frac{76+0.5}{90}=\frac{8.5}{10}$ When the C. Mt. was 7°, it was made in considerably less time than the St. (76 and 61). The other columns contain too few tests to yield an average free from the variable time error to which I have drawn attention.

These figures lead, it seems, to the conclusion—a conclusion to be reinforced by our other results—that in comparing, as he thinks, two lengths, L. compares in reality, the duration of two movements made at approximately the same rate. I may add that during the experiment he did not have duration in mind. He strove to reproduce in the C. the sensations of movement experienced in the St. The sensations that drew his attention seemed to be localized in and around the elbow joint.

II. In order to get the duration figures given in Table II, the sums (not the averages) of the —, the =, and the + columns in each series were added, and the total divided by the number of tests included in each column. The differences between the St. and the C. are indicated in parenthesis. The W. R. Ser. of H. includes but 50 tests; of these only ten fall in the + column, hardly a sufficient number for a reliable average. The figure in the equality C. (marked with an asterisk) is not strictly comparable with the others. It is derived from a series not reported in this paper in which the C. Mts. were made considerably higher up on the semi-circle than in the N. Ser. The figure may serve, however, as an approximation.

If the judgments recorded in this table are based, as it appeared probable from the series already examined, upon time

TABLE II

Average duration of all the tests arranged by series and classified according as the C. was judged larger, equal, or smaller than the St.

Subject	Series	— St.	— Diff.	— C.	= St.	= Diff.	= C.	+ St.	+ Diff.	+ C.	Equality C.
L	N	75	(−14)	61	79	(− 3)	76	76	(+14)	90	8°.4
	W R	76	(−10)	66	78	(+ 1)	79	80	(+11)	91	8°.3
	M R	Not enough cases.		(6)	48	(− 3)	45	54	(+ 2)	56	7°.3
	I W R	50	(− 1)	49	51	(0)	51	50	(+ 9)	59	7°.9
R	N	39	(+ 2)	41	37	(+ 5)	42	37	(+ 9)	46	8.7
	W R	41	(+ 8)	49	40	(+11)	51	41	(+19)	60	8.5
	M R	32	(+ 6)	38	31	(+ 8)	39	31	(+14)	45	7.5
	I W R	43	(+ 5)	48	40	(+ 7)	47	40	(+13)	53	7.6
H	N										10*
	W R	30	(+ 6)	36	29	(+11)	40	31	(+10) 10 cases.	40	9.8
	M R	29	(+11)	40	29	(+15)	44	31	(+16)	47	7.2

comparisons, we should expect the duration of the compared Mts., in every case in which they are judged of equal length, to bear to each other a fixed relation, whatever be the length of the C. That fixed relation would be one of equality if the subject moved through the C. with the same velocity as through the St. But if he should move systematically slower through one of them, the constant relation could not be one of equality. I assume, of course, in saying this, that the subject is influenced, not only by the duration but also by the speed of his Mts., that, in fact, a quasi-automatic compensation takes place between duration and speed when he is judging of spatial lengths.

The data before us bear out these suppositions. Subject L., moving through the St. and the C. in the N. and in the W. R. Ser. at an approximately equal velocity, finds the C. equal to the St. when both take the same time, shorter than the St. when the C. takes less time, and longer than the St. when it is the reverse. In the M. R. and the I. W. R. Ser., done several days after the others, he again maintains practically the same rate of speed through the St. and the C. Mts., although the absolute speed is considerably increased when compared with that of the first two Ser. Subject R. differs from L. in that she takes the C. more deliberately than the St., and, allowing for the lesser speed of the C. Mts. calls, in the first series, the C. equal to the St. when the former takes her 5 units longer. When it takes her only 2 units longer, she calls it shorter, and when it lasts 9 units more, she calls it longer. In the W. R. Ser. she proceeds through the C. still more slowly than in the preceding Ser., and the figures show that, even though she is not clearly conscious of it, this decrease is taken into account. Her Equality C. takes her 11 units longer than the St. When the C. takes her only 8 units longer, she calls it shorter, and when it lasts 19 units more than the St., she calls it longer. Her last two Ser. are, in this respect, in full agreement with her first two Ser. The averages from H., as far as they proceed from a sufficiently large number of tests, match those of the two other subjects.

We find, thus, conclusive evidence that these subjects, set to the task of comparing the length of two arc movements, were guided in their judgments by the duration and the rate of the Mts., although their introspection does not indicate any awareness of the fact. The degree of precision with which changes of absolute speed and of relative speed between the St. and C. are taken into account, came to me as a surprise. And the further fact that, making use of duration, the subjects did not try to keep the velocity equal (L. excepted) was, I confess, somewhat disconcerting. I intend to find out at some future time whether persons required to compare the length of two Mts. by means of their duration would not endeavor to move with an equal speed through both.

Explanation. Let us, now, compare in the different series the average lengths of the C. when it was felt equal to the St. of 8°. These lengths are written down in the last vertical column under the head "Equality C." With regard to the influence of the weight resistance, they confirm, in every subject, the results obtained by other experimenters, *i. e.*, the equality C. in the N. and in the W. R. Ser. do not differ materially one from the other. It should not be overlooked that it is the addition of a *constant* resistance which does not alter the length of the Mt. The *increasing* resistance used in the M. R. and in the I. W. R. Ser. produced a substantial overestimation of the C. Mts. But why this overestimation of the C. in the M. R. Ser.? We know from the figures of Table II that it has not its origin in an error of duration: a shorter time is not mistakenly judged equal to a longer one; the lengths 7°.3 and 8° felt equal by L. are made in equal times. If not in the duration, the error must be in the estimation of the rate of Mt. There is as a matter of fact in the M. R. Ser. a gradual reduction in the velocity with which the arm moves through the C. This falling off is correlated with the increasing resistance offered to the Mt. by the forearm pressing against the upper arm. We shall submit figures on this point in a future paper. But the falling off of the rate is not in itself sufficient to cause an error since, as we have seen, changes in velocity are, under certain conditions, exactly taken into account. It must be that in this Ser. the conditions do not allow of the normal compensatory effect of the speed upon the duration.

At this point we must inquire into the sensory basis for the apprehension of the rate of Mt. The skin sensations,—air pressure and temperature—at best secondary criteria, had been eliminated in the cases of R. and H., by means of a loose fitting glove, reaching almost to the elbow, and overlapped by the sleeve. These sensations being out of the question, there remained those arising from the muscles, from the tendons, and from the joint. When the arm is moved through a fixed number of degrees, the effort made is greater when the velocity is high than when it is low, because a part of the innervation finds its way into the antagonistic muscles, and an increased innervation of the muscles that do the work is required. But I do not see any reason for assuming that, as far as tendinous and muscle sensations are concerned, the untrammelled arm moving upward at a considerable speed yields a sensation-experience different from that produced by a slower movement of the same duration made against a properly chosen resistance. The muscle activity would, it seems, be the same in both cases. The muscle and tendinous sensations cannot, therefore, provide an unequivocal basis for the rate of Mt. One is, thus, driven to the hypothesis that unequivocal speed information comes from the joint surfaces.

What is it, then, that takes place in the joint surfaces when speed changes? Two things: (1), an increase in the pressure of the joint surfaces against each other, corresponding to the greater innervation of the muscles. This pressure-increase produces, presumably, a rise in the intensity of the joint sensations. But an intensity series correlated with an increase in muscular tension cannot, as we have seen, serve as a basis for an apprehension of speed. (2.) Speed-increase means also an increment in the rapidity with which the joint surfaces pass over each other, *i. e.*, an increase, shall I say, in the quantity of the stimulus applied to each one of the joint sense-organs in a unit of time? It is credible enough that a qualitative or a quantitative sensation-difference corresponds to this quantitative increase of the stimulus. We may think of it as being the same sort of difference as the one experienced when the velocity of a wheel, the edge of which rubs against the skin, is altered. Whatever be the psychical effect produced, in the case of the joint organs, by changes in the rate with which the stimuli succeed each other, that seems clearly the only possible cause of a sensory change corresponding unequivocally to the rate of the Mt.

I shall therefore hold, until further information comes to hand, that our comparative judgments of the length of arm Mts., when the "sense of position" is excluded, is really a comparison of duration and of a peculiar sensory value depending upon the rapidity with which successive joint organs are stimulated. This sensory value may be called the *rate-value* of the joint sensation. That introspection does not clearly reveal the existence of this rate-value is no argument against its effective presence.

We are now prepared to account for the over-estimation of the rate of Mt. in the M. R. Ser, an over-estimation leading to the length errors recorded in Table II. I have said already that as the C. movement proceeds the resistance increases somewhat irregularly and the velocity falls off. It decreases, not for the physiological reason offered by Loeb, but because the effort made at any particular moment to overcome the increased resistance, so as to keep the speed constant, falls short of its purpose, since by the time the effort becomes effective the resistance has again increased. The decrease in speed would not of itself cause the observed error. It is because of complicating circumstances making the correct estimation of the rate impossible that the Mt. is made shorter. What happens is that the rate-value of the joint sensation is obscured by a gradual and somewhat irregular increase in the intensity of the joint sensations,—an increase arising from the increment in muscular tension made necessary by the growing resistance. Furthermore, and chiefly, the necessity of repeated readjustments of the speed to the resistance tends to draw the attention away from the rate-value.

The error is an *overestimation* of the rate because the intended speed is greater than the realized one.

In the W. R. Ser. the disturbing circumstances just mentioned did not exist. There was, indeed, an increase of the muscle tension in the C. Mts., and, presumably, a slight corresponding increment in the intensity of the joint sensations. But this increase was, in this series, constant from the beginning to the end of the C. movement. It did not, therefore, divert the attention from the rate-value and thus no velocity error took place.

If the duration of the Mt. remains undisturbed in the M. R. Ser., it is, I think, because the duration of the Mt. experience as a whole, or of the joint sensations in their entirety, is estimated by means of the duration of other sensations themselves unaffected by the arm movement. It is, therefore, an indifferent matter whether or not any particular qualitative or intensive alteration takes place in any one of the sensory components of the whole Mt. experience. The important points for a correct duration-estimate are that the Mt. should be present in some way in consciousness and that the sensations used to measure its duration—and we know that they are not necessarily the same for every one—remain undisturbed by extraneous factors as long as the Mt. lasts.

In order to verify the preceding explanation of the space error present in the M. R. Ser., a control Ser. was devised. In it the C. Mts. took place over the same portion of the semicircle as in the W. R. and the N. series. A resistance was introduced in such a way that, instead of being constant as in the W. R. Ser., it increased as the C. Mt. proceeded. Thus the pressure of the forearm against the upper arm, and the high degree of contraction of the biceps were eliminated, but the increasing resistance remained. Under these circumstances one would expect, if our explanation is valid, a rate error just as in the M. R. Ser. The resistance provided was a long weight of 2 Kgr. dipping in mercury. During the C. Mt. the weight was gradually lifted out of the mercury. As, according to our practice, the starting point of the C. as well as of the St. varied by a few degrees in this series also, the initial resistance in the C. Mt. varied from 0 to about 300 grams. The amount of resistance at the end of the Mt. was not at all as great as in the M. R. Ser.; nevertheless, the results show, in the case of R., an overestimation of the equality C. practically equal to the one present in the M. R. Ser. (7°.6 against 7°.5 in the M. R. Ser.). The error is not so great in the case of L. (7°.9 against 7°.3 in the M. R. Ser.), yet an unmistakable error in the same direction as in the M. R. Ser. is apparent in his results. It is in fact the size of R.'s error and not that of L. which is surprising in view of the considerably smaller resistance applied in this Control Ser. Lack of time prevented H. from doing this series.

Our account of the over-estimation of the equality C. is thus verified, as far as it was in the power of the Control Series to do.

Historical and Critical. In 1890 J. Loeb published in Vol. 46 of *Pflüger's Archiv*, pp. 1-46, under the title, "Untersuchungen über die Orientirung im Fühlraum der Hand und in Blickraum", certain experiments with arm movements from which he concluded that the more contracted are the active muscles at the beginning of a movement, the greater is the overestimation (p. 41). In order to account for this fact he makes use of two hypotheses, the first of which is superfluous, and the second (dependent upon the innervation theory) is now quite discredited. They are, (1.) the excitability of a muscle decreases as its state of contraction increases; (2.) the length of a Mt. is judged by the amount of energy sent to the muscles performing it. If, then, we try to make two equal Mts., the second of which follows in the direction of the first and is therefore performed with the muscles in a greater state of contraction, we shall send to the muscles for the execution of the second movement an amount of energy equal to the amount by which the first was produced. But as the more contracted muscles are less excitable than the less contracted ones, an equal amount of energy will produce a shorter length in the second than in the first Mt.

F. Kramer and G. Moskiewicz in "Beiträge zur Lehre von den Lage- und Bewegungsempfindungen" in the *Zeits. f. Psy.*, Vol. XXV (1901), pp. 101-125, repeated Loeb's experiments and, generalizing his conclusions, stated that of two movements intended to be of equal length, everything else remaining equal, the more uncomfortable (*unbequem*) falls short of the other. They rejected Loeb's explanation, and suggested, without putting their supposition to an experimental test, that the ovestimation of the "*unbequem*" Mts. is the outcome of a natural tendency to move more slowly through a difficult Mt. than through an easy one (pp. 121-123). This statement we know to be inexact, unless the effect of an added resistance, as in our W. R. Ser. does not fall within the intended meaning of the word "unbequem".

The assumption of Kramer and Moskiewicz with regard to Loeb's illusion was recently put to a test by Erich Jaensch in "Ueber die Beziehungen von Zeitschätzung und Bewegungsempfindungen", *Zeits. f. Psy.*, Vol. 41 (1906), pp. 257-279. He used for measuring the duration of the Mts. a pencil so constructed that when its point is pressed down upon the paper it recedes into the handle. Air is thus pressed back through a rubber tube into a Marey's tambour. In this way a record is made of the beginning and of the end of the Mts. In one of these two series of experiments the observer, starting with the hand near the chest, moved it away from the body for a certain distance, stopped an instant, and then proceeded in the same direction until he had made a line seemingly equal to the first. In the other set, the direction of the Mts. was reversed, the observer starting away from the body and coming toward it.

His duration records led him to this conclusion: "One may take it as proven that the lengths appear equal because the times used in making them are equal" (pp. 269). He discusses the causes of the lower velocity of the uncomfortable Mts. and comes to the opinion, different from ours, that the chief one is physiological, namely, the decreasing excitability of the contracting muscle.

Loeb's explanation of the decrease in length of the Mt. is thus taken up by Jaensch as the cause of the decrease in its rapidity. This decreasing excitability hypothesis is shown by our Increasing Weight R. Ser. to be superfluous. Moreover, if the relation it supposes between the degree of contraction and excitability really existed, we would in all likelihood have learned to increase automatically the innervation as the length of the muscle decreases and thus have overcome the tendency to error. Jaensch does not seem to have clearly realized, any more than his predecessors, what our experiments prove, namely, that it is an *increasing resistance* and not an added, constant resistance which causes the velocity error in Loeb's experiments. Neither did he grapple with the real problem, which was to make clear why, while we are usually able to take into account, for the sake of the length of the Mt., minute changes of velocity, under special conditions, a constant error in the estimation of speed takes place. To this problem an answer has been given in the preceding pages.

Introspection revealed that our three subjects made no use of visual imagery. In the case of L. there was at times a vague image of the space covered, but it followed the production of the Mt. and never had the clearness necessary in order to serve as a guide. One of the two subjects we used in other experimental series, and only one, had clear visual images of the length of the Mt. She saw the St. and tried to reproduce an equal visual length. Of all our subjects she was the least accurate. I hope to have the opportunity of finding out whether she also made use of duration and rate in judging of the length of Mts.

In the light of the preceding facts and discussions the complete inadequacy of Külpe's statement is evident: "our judgment of the extent of arm movement is not based (in the author's observation) upon the temporal relations of the movement, but upon the reproduced visual image of the space passed through, and more especially of the extreme positions of the moved arm." (Outlines, p. 348.)

Before closing I must refer to the constant positive error in the Equality C. of the N. and of the W. R. Ser. in the case of the three subjects. (See the last column of Table II.) I have so far neglected it. My reason for doing so is that I have at present nothing decisive to say about it, I do not know how to account for the overestimation of the velocity, that, in my view, it implies. There are several possibilities. I shall probably return to the point in another paper.

Summary of Conclusions. When the sense of position is excluded, the comparison of the length of arc movements is made through the comparison of the duration of one or several of the sensations arising from the Mts. (preferably the joint sensations) and of a particular value of the joint sensation, called here the rate value.

A quasi-automatic compensatory relation exists between the duration and the rate value.

A gradual increase in the resistance offered to a Mt., whether caused by a weight or by the pressure of the forearm against the upper arm, produces a decrease in the rapidity of the Mt. This decrease, for reasons mentioned above, is underestimated, and thus an overestimation of the length of the movement takes place.

Local signatures cannot be connected with the rate value of joint sensations, for one and the same joint organ is susceptible of a whole range of rate values. It is the joint sensation as a whole, not the rate value, which possesses, or may possess, a local sign.

The comparison of the length of Mts. under the conditions present in our experiments does not necessitate the existence of local signs in the joint sensations. An apprehension of duration and of rate is sufficient.

ORGANIC SENSATION[1]

By Elsie Murray

TABLE OF CONTENTS

[1] The investigation of which this article forms a part was first taken up in the Cornell Laboratory in the year 1905-6. A related article appeared in this *Journal* in July, 1908.

It was our original intention to limit this study to the sensations originating in the vegetative processes of the visceral and circulatory systems, exclusive of all cutaneous or kinæsthetic components. In the course of investigation, however, the impracticability of keeping up this line of division became apparent, and the discussion is frequently broadened to cover *internally or indirectly initiated* sensation in general, a category practically identical with what Sherrington has termed the 'proprio-ceptive', as over against the 'extero-ceptive', sphere of sensation. The reasons for this will be more apparent in a discussion of the subject which is to be published later.

Preliminary Survey

The following study represents an attempt to bring a subject commonly assumed to be of physiological import only under the discipline and into relation with the data of experimental psychology. In view of the scope and novelty of the undertaking and the inherent difficulties of the subject, apology need hardly be offered for the fact that in the following pages project rather than accomplishment is presented to the reader.

To the rôle of somatic sensation in our daily consciousness various set descriptive phrases, such as the 'thrill of pleasure', the 'shock of surprise', the 'pang of dismay', the 'wave of pity', the 'shudder of disgust', bear witness. That these phrases are not merely metaphorical in significance, but have a direct physiological reference and derivation, that the 'thrill', the 'pang', the 'wave', has each its own more or less definite bodily seat, characteristic timbre, and limited emotional context is sufficiently patent to all. From the literature and speech of every day many phrases referring directly to heart or circulatory sensations may also be culled, *e. g.*, the heart 'aches', 'sinks', is 'ready to burst', the blood 'boils', 'curdles', the gorge 'rises', and the like. To this list other less highly colored yet distinctive and persistent bodily reactions, fatigue, hunger, tension, excitement, satisfaction, and many more or less shadowy modes of cœnæsthesia may be added. Of the group thus constituted it may be safely said that in some form or other, in the focus or margin of consciousness, one or another of them is always with us, even in the most intellectual of our preoccupations. That these, the most intimate, unescapable, and superficially accessible of our sensory experiences should stand to-day on the frontier as it were of the psychology of the senses is an interesting if hardly creditable phenomenon.[1] Speculation as to the significance and inner mechanism of certain of the bodily accompaniments of our mental states or emotions has indeed been rife, witness the James-Lange theory and the present discussion pro and con. But of curiosity as to the primary sense stuff of these bodily reactions, of query whether the diversity in the varying web of mood is one of 'color' or texture merely, of inquiry as to the possible existence of an extended gamut of organic as of visual sensation, of exploration of the peculiar cohesions, conformations, and behavior of such sensations in experience, there is in the litera-

[1] See E. B. Titchener: The Problems of Experimental Psychology (St. Louis Congress address), this *Journal*, xvi, 1905, 212. "Of all problems in the psychology of sense that are now before us, the problem of the number, nature and laws of connection of the organic sensations appears to me to be the most pressing."

ture singularly little evidence. Yet if psychology is to be raised above the level of the merely subjective, any explanatory use of organic sensation must of necessity be preceded by some sifting of the facts and agreement as to the states in question. Here, if anywhere, are surely in order the old-fashioned, plodding methods which seek by the correlation of fixed terms with certain of the simpler, commoner, and more readily identifiable phases of consciousness to pave the way for the description and exploitation of the subtler and more significant phases within which the simpler function as partials, or prime factors.

The following research was prompted in part by a desire to bridge the gap in our knowledge of the psychology of the sensory components of experience. This motive was, however, strongly reinforced by the conviction that (all question of their specific function in the vegetative economy of the organism aside) these so-called 'by-products' of the mental life must play some considerable rôle not only as the matrix or basis of continuity of our experience, but further as conditioning in some fashion both the affective processes, and attention, action, and the interplay of ideas and meanings. Inquiry was further stimulated as the result of certain observations incidental to a laboratory investigation of the affective qualities, in which the writer, serving as observer, had occasion to note that even the simple pleasantness (or unpleasantness) occasioned by various sensory stimuli was attended in consciousness by certain more or less definite and uniform if hardly describable bodily reverberations or feelings. In view of the difficulties in the way of unequivocal correlation of affective qualities with objectively demonstrable organic changes,[1] the possibility of correlation with *felt* organic changes (such as the bodily set or mood of acceptance or rejection) is not to be let slip unchallenged. Without prejudice to the question whether or not the organic sensations form the indispensable basis and vehicle of affective experience, as d'Allonnes would have us believe, the demonstration of certain constant correlations between affection and bodily sensation would at least afford a tangible basis for the handling of affective experiences, for their release from the disconcertingly detached and disenfranchised position which they now hold in psychology.

However this may be, it is not unreasonable to surmise that the shirking of the analysis of the bulky if nebulous residue of our marginal consciousness constituted by organic sensation

---

[1] See, *e. g.*, R. Lagerborg's résumé of the discussion as to the symptomatic or causal relation of organic changes and feeling, in Das Gefühlsproblem, 1905, 42-68.

plays into the hands of an uncritical use of the 'subconscious' in explanation, and of a more or less reckless speculation as to the number and traits of the affective elements, the ultimate constitution of mind, and the functions and attitudes of the empirical (and non-empirical) ego. In view of the present unsettled state of the science, and the growing demand that psychological treatment should render itself more adequate to concrete reality, scientific economy forbids the leaving at loose ends of so large a body of conscious fact. In the attempt to establish a descriptive basis for the psychological treatment of functions and attitudes this study must inevitably contribute by bringing into the light of day many supposedly intangible and shadowy modes of experience.[1]

Again, in view of the more or less prevalent lack of recognition of any simple affective consciousness other than pleasantness or unpleasantness attaching to our ordinary perceptual experiences, and the consequent reproach of over-intellectualism, the possibility that the organic coloring of conscious events may serve not only to enrich the affective consciousness constituted by the above mentioned elements, but may itself represent an independent feeling moment fairly on a par with pleasantness and unpleasantness, deserves examination. Whether or not this speculation prove profitable, introspective examination of bodily sensation under experimental conditions should be employed to establish the present debatable boundary line between organic sensations or complexes and unitary affective processes, pleasantness, and unpleasantness, or tension relaxation, quiescence, and the like, in the systems of Ladd, Wundt, and Royce.

Lastly, whatever may be the relation which subsists between feeling and organic sensation, in the opinion of the writer discussion of the James-Lange theory, and experimentation with animals and abnormal subjects in support and in disproof of it, are in a sense both premature and futile so long as the method of introspection has not been stretched to the uttermost and the possible organic contributions to emotion, and their behavior in simpler and more complex contexts critically examined.

---

[1] With an organic terminology at our command we shall be able to do fuller justice to the reactive as well as the presentative aspects of perception and thought. It is indeed the hope of the writer, as doubtless of many others, that a study of the organic margin or lower stratum of everyday consciousness will not only ultimately contribute to the elucidation of such refractory psychoses as the judgment of certainty, of likeness, the feeling of reality, of contrast, of recognition, but may even furnish a basis of distinction for peripherally and centrally excited elements, for recollection and imagination, as well as for many of the so-called 'feelings of relation'.

PRIMARY PROBLEMS OF ORGANIC SENSATION

Certain fundamental questions as to the nature and conditions of appearance of organic sensation in contradistinction to recognized sensory qualities loom large at the very outset of our undertaking. Is, for instance, the customary dichotomy of sensation into external and internal psychologically valid? Does it have its roots in a fundamental disparity between the underlying psychophysical conditions or is it an epistemological distinction merely? Does it have an ultimate justification if not in elemental qualitative differences, at least in the diverse structural or textural patterns and cohesions to which the two classes of sensation are liable, or in the extent to which they are amenable to analysis and reproduction? The sources to which we may apply for aid in the solution of these problems are various. Casual references to organic sensation in literature in general; in monographs dealing with the problem of feeling in especial; the relevant facts of histology, physiology, and pathology; and, lastly, direct experimentation and introspection, must all be drawn upon to contribute what light they may to the subject. Preliminary to more detailed discussion, the general trend of opinion of psychologists and physiologists on three or four elementary phases of the problems involved will be briefly sketched. The neurological, physiological, and pathological evidence bearing on these same problems will then be more minutely examined.

1. *Possibility of observation in the sphere of organic sensation.* In explanation of the dearth of systematic observations in this field many difficulties have been cited; the inaccessibility alike to experimentation and attention of the sensations concerned, their alternately feeble and violent character, high affective tone, unanalyzability, and vague sensory coloring.[1] With re-

[1] The problem is somewhat obscured by the custom deriving from Weber, Wundt, and Goldscheider of lumping organic or visceral sensation with the *Gemeinempfindungen* ('common sensation'), a class which by definition consists in whatever residue of sense material proves resistant to elaboration, analysis, and localization. The significance attributed to this classification varies considerably in the different writers, and may be quoted here in view of its bearing on their several attitudes toward organic sensation proper. The term or its equivalent is used in turn to cover sensation derivable from any portion of the body or any nerve, sensation referred to the body (as a whole), or totally unreferred, subjective. In the usuage of J. Mueller the significance of the adjective 'common' is anatomical or topographical merely: common sensation (*Gemeinempfindungen*) includes the pain-pressure-temperature equipment common in his belief to the exterior and interior of the body alike. Weber, using the term *Gemeingefühl* or *Gemeingefühlsempfindung*, gives it an epistemological turn: common sensation is unexternalized sensation, the raw sense material in the formal elaboration of which and the application of spatial, in-

gard to the impracticability of experimentation, a word only is necessary. This difficulty first finds expression with Weber, who in listing the reasons why our inner sensations are customarily outside the range of apperception, gives prominence to the fact that our inner organs possess no *Ortsinn*, and are withdrawn from the possibility of the local application to their surfaces of moving or changing stimuli, hence can never take their place in the world of outwardly referred, spatially determined perceptions.[1] For this formulation of the difficulty the Kantian principle adopted by Weber that only the *spatially* determined and outwardly projected[2] attains to any degree of

---

tensive, or qualitative categories to which perception is balked, either by the violence of the sensation itself or the conditions of its origin (internal stimulation). This rubric, which includes tickle, itch, organic sensation and pain, is stretched to cover muscle sensation also, but obviously not without violence to the original meaning. Practically the same subdivision is set up by Wundt on the basis of the internal nature of the stimulus (functional changes, central irradiation, etc.) as a condition unfavorable to projection. Wundt, however, sees the impossibility of retaining kinæsthetic sensations in the category, though still including pain, which certainly may appear in as objective a light as strain. Goldscheider, using the term *Gemeingefühl*, shows a tendency to apply it to organic and external sensation only in so far as these are actually unanalyzed and unprojected, *i. e.*, felt rather than perceived. Thus a faint tactual impression may consist either in a touch perception, a tickle feeling, or both, according to the conditions under which it appears. Kröner, on the other hand, insists upon the limitation of the term *Gemeingefühl* to its strict logical signification, *i. e.*, a sense consciousness absolutely unanalyzable (owing to the central or general nature of the stimulus), and referred to the body as a whole. Hunger, thirst, drowsiness, nausea, general bodily comfort or discomfort, and various other organic states, belong under this heading. Lagerborg makes a somewhat similar use of the category of *Gefühl* as representing undifferentiated or fused organic sensation and inclusive of unitary bodily states (Lust, Unlust and the like), and exclusive of organic sensations in so far as these are localized or analyzed.

[1]E. H. Weber: Tastsinn und Gemeingefühl, in R. Wagner's Handwörterbuch d. Physiologie, 489-91.

[2]See Weber's peculiar distinction between *Empfindung* and *Vorstellung der Empfindung*, *op. cit.*, 485-7, and 489-91, in which he maintains that only that which changes, or, more precisely, which can be made to pass through a minute and varied series of changes through the introduction of movements, is susceptible of separation from the self, of spatial reference and perceptive clearness. The subjective, on the other hand, represents not so much the *self-referred* as that portion of the raw material of sense which because it is relatively constant and unchanging remains unreferred, unlocalized, hence chaotic, vague, unclear. For a somewhat similar emphasis on the correlation of clearness and minute spatial delimitation and reference, see E. Meumann: Zur Frage der Sensibilität der inneren Organe, *Archiv für die gesamte Psychologie*, IX, 1907, 57, 58. Where Weber neglected the possibility of differentiation and analysis through any other means than movement and localization, Meumann overestimates

apperceptive clearness is obviously responsible. At any rate, it is obvious that the difficulty cited applies mainly to our knowledge of the general contours and positions of our internal organs rather than to the observation of the varieties of sensation to which, functioning as wholes, the lungs, heart, diaphragm, stomach or internal circulatory system may give rise.

This experimental difficulty finds a later version in the statement of Külpe[1] that the customary experimental appliances for the facilitation of the isolation of constituents are here useless. This objection likewise rests on a false parallel between external and internal conditions. In the first place there is no reason for assuming that even if appropriate methods were applicable any great differentiation of sensation in different portions of the same organ would be discovered. In the second place, psychology is interested less in the sensory mapping of the viscera than in the peculiarities of organic sensations themselves, and the impossibility of detailed surface exploration by no means precludes the isolation of individual qualities or masses of sensation due to the functioning of an organ or tissue as a whole, through indirect stimulation by drugs, chemicals, or by taking account of various known correlations of external stimuli and internal *Mitempfindungen*.

There are, however, according to many writers, more serious obstacles in the way of the analysis and localization of internal sensation. Certain inherent peculiarities in the normal anatomical and physiological conditions of production of organic sensation manifested on the conscious side in its intrinsic vagueness, diffuseness, feebleness and violence, effectually debar it from the realm of clear perception. Among the unfavorable conditions specifically mentioned we find the scanty sensory innervation of the viscera[2], the central or diffuse and

---

the clarifying value of the spatial reference and definition contributed by the visual image. Both positions, in our opinion, exaggerate the importance of one among several possible contributory factors in attentional clearness.

[1]O. Külpe: Outlines of Psychology, 140.

[2]According to Müller, who perhaps set the tradition on this point, the feebleness and vagueness of internal sensation is the necessary correlate of the fact that the interior of the body is innervated chiefly by the sympathetic system, the primary office of which is the mediation of reflexes, not of sensation, and the ganglia of which were in Müller's day believed by some actually to function as the suppressors of sensation. Müller himself seems to have assumed, as Mackenzie, Head and others have more recently maintained, that the sympathetic possesses in its own right little or no entrée to the sensorium, depending, in cases of high intensity, on a forced passage over the fibres of the cerebro-spinal system correlated with pain. See J. Müller: Elements of Physiology (tr. by Baly, 1837), Vol. I, 723, 795-9.

general character of the adequate stimulus, and the possession of a peculiar secondary conduction path functioning in the case of pain.

Among those maintaining the essentially feeble and liminal or subconscious character of the internal sensory processes in their non-painful phases are Lotze,[1] Höffding,[2] Wundt,[3] and Ladd.[4] According to all, the affective tone correlated with the consciousness of bodily state (cœnæsthesia) is far more prominent than the semi-chaotic mass of sensation to which it is attached, and which may even be totally submerged. This state of affairs is indeed susceptible of explanation as a case of affective summation. On the face of it, however, so extreme a statement gives rise to the suspicion that introspection is here adding to the credit of feeling at the expense of sensation,[5] and that the construction of Kröner and Lagerborg, who maintain that in such cases the sensation mass itself constitutes the feeling or affective tone,[6] stands closer to the truth.

Some peculiar mechanism of transmission for higher intensities lies presumably at the root of the sudden jump which Weber, Wundt, Külpe and others assert to take place when organic sensation abruptly rises from this vagueness and indistinctness into pain of such violence and high affective value that accurate observation of it is impossible.[7] The requisite conditions of such a mechanism might indeed be in part fulfilled on the hypothesis of a secondary conduction path offering considerable resistance to its more peripheral synapses, in its more central portions a low limen and considerable capacity for explosive and intensive functioning. Feebleness and violence, at any rate, appear alternately as the basis of explanation of the ease with which organic sensation customarily

---

[1] R. H. Lotze: Medicinische Psychologie, 281.

[2] H. Höffding: Outlines of Psychology, 1891, 225-6.

[3] W. Wundt: Physiol. Psychologie, 1902, II, 42-4.

[4] G. T. Ladd: Psychology, Descriptive and Explantaory, 1894, 175, 184.

[5] It is noteworthy in this connection that the four authorities quoted are all exceedingly liberal in their estimates of the variety and richness to be ascribed to the affective elements in general. They are also all more or less inclined to identify the distinctive feature of pain not as a sense quality, but as feeling moment superadded to or extinguishing the latter.

[6] E. Kröner: Das körperliche Gefühl, 1887; R. Lagerborg, *op. cit.* See also A. Horwicz: Zur Lehre von den körperlichen Gemeingefühlen, *Vierteljahrsschrift für wiss. Philosophie*, 1880, IV, 273-310, for an excellent discussion of organic sensation as feeling, disposition or mood.

[7] Wundt: *op. cit.*; Külpe: *op. cit.*, 140, 146; Weber: *op. cit.*, 22. According to Weber, this state of affairs is due, at least in part, to the lack of developed *Tastorgane* to protect the terminations of the internal nerves from over-stimulation.

eludes the attention. The possible neurological obstacles in the way of organic sensation as well as the query as to its essentially painful character will come up for discussion later. In the meantime, however, it is worth noting that the authorities quoted cite the high affective value and commotion produced by organic sensation in its supraliminal phases as irremediable obstacles to observation, without apparently pausing to consider whether the gap between liminal and violent organic sensation is actual and absolute, or merely the effect of the normal absence of any incentive to attend to intermediate intensities; all organic sensation possibly appearing liminal in its marginal vagueness until of a strength to secure its automatic admission to the focus of consciousness.

While the trait of unanalyzability has been in part ascribed to the defective and undeveloped functioning or lack of elaboration of the nervous mechanism, that of unlocalizability especially has been referred[1] to the central or general character of the adequate stimuli. If for instance the immediate effect of an alteration in the composition of the blood on the brain centres or on the peripheral nervous system as a whole constitutes the adequate stimulus, the operation of the ordinary mechanism of localization or reference becomes improbable. A word of caution is, however, here in order. While the tonic action in reflex centres of the condition of the blood is an accredited physiological fact, the direct effect on consciousness of such action is problematical. Therefore, even though the appeal to conjectural central processes is, in the absence of reliable introspective data and in the interests of special theories and classifications, admissible, such theorization can hardly be permitted to throttle investigation of the facts. The authority of this position is, moreover, weakened by the lack of any detailed working out of the hypothesis, or attempt at adjustment with current doctrines or facts, as *e. g.*, in Kröner's chemical theory, where, in defiance of the generally accepted doctrine of specialization or adaptation in the afferent nervous system and its central stations, absolute indifference of function is tacitly assumed.

In view of the absence of any definite experimental evidence against the possibility of observation and analysis the more hopeful attitude of certain psychologists is worth noting. Beaunis in especial is firm in the conviction of the possibility of training the attention upon the observation and singling out of internal sensations, while Meumann reduces their customary vageness to indefiniteness of localization and the latter primarily to the absence of correlated visual images,[2] leaving

---

[1] Wundt: *op. cit.*, p. 43. Kröner: *op. cit.*, 28, 31. [2] *Loc. cit.*

open the possibility of the application of other methods to the breaking up and clarifying of organic sensation.

Against the objection that organic sensations are not reproducible, give rise to no free images,[1] it may be also urged that the question remains open whether this defect is intrinsic to the neural mechanism of internal sensation, or transcendable through experimental methods and training. The whole problem of the possibility or impossibility of the investigation on this or other grounds is, of course, intimately bound up with the question whether visceral afferent impulses in general represent rudimentary, atrophied and vestigial sensation, or sense material the further development of which has been arrested, not inevitably impeded by functional or practical causes. We shall, therefore, in a succeeding section, have recourse to the natural history of organic sensation, in its biological, physiological, and histological phases, as a guide to the probable limits of profitable investigation.

2. *Existence of any organic sensation other than pain.* Common sense sanctions the recognition of a wealth of either actively pleasant or only mildly unpleasant organic experiences, hunger, satisfaction, satiety, and the like, yet clinicians, physiologists, and even a few psychologists, notably Wundt, have stubbornly asserted the essentially painful character of all visceral sensation. When, however, the basis for this assurance is examined, its negative and inferential rather than introspective character is quickly evident. In the case of the clinician an over-emphasis on pain is but the normal outcome of his preoccupation with surgical and pathological, *i. e.*, *painful* cases. This prepossession is strengthened by the insensitivity manifested by the viscera under surgical manipulation; the customary stimuli (mechanical, thermal, and chemical) of external cutaneous sensations (pressure and temperature) are here inoperative. Arguing by analogy with the skin, he therefore quickly arrives at belief in the non-existence of any painless phase of organic sensation. We need only pause here to point out the logical conclusion of argument upon these premisses, *i. e.*, the negation of all visceral sensation, painless and painful alike. The Swedish surgeon Lennander has indeed recognized the logic of this position, and achieved the *tour de force* of transferring all the responsibility for the so-called

[1] *Cf.* E. B. Titchener: Organic Images, *Journal Phil.*, *Psych. and Sci. Meth.*, 1904, I, 36 ff. The objection is here raised as a protest against dogmatic assertions that any sensation, just because it is sensation, must be reproducible in image. The author finds evidence that at any rate in minds of certain types, certain organic sensations are not thus reproducible, and enters a plea for further systematic work on the subject.

"visceral" distress of disease and inflammation to the body wall, a doctrine which we shall revert to later. Others, while less extreme, are nevertheless sensible that on the line of reasoning above adopted the emergence of any organic sensation whatever, even pain, is an anomaly to wrestle with. Given the law of parsimony and the fact that the afferent impulses of the viscera do not normally reach consciousness, what explanation is possible of the neural mechanism by which in cases of exaggerated intensity or disease these processes in the guise of pain assail the attention? Would cortical connections functioning perhaps once, perhaps never in the lifetime of the individual persist in the race?

The answer to this problem furnished by Foster[1] is physiologically simple enough, though involving a dubious recourse to the subconscious, and will be touched on later. According to the more radical, however, the pain of disease represents in all probability a later development in the history of the race for the protection of the regions supplied by the older subconscious sympathetic system,[2] and is mediated by some makeshift of a mechanism, such as leakage from the sympathetic or vagus to the pain centres of the external system (Ross, Mackenzie, and Head), or the stimulation through excessive visceral activity or inflammation of nerve trunks of the external system which happen to pass near the organs or tissues affected (Lennander).[3] The paradox of the apparent painful response of the viscera to external pressure in case of disease or inflammation is, moreover, generally reduced to a mere matter of the hyperalgesia of the body wall or skin (Mackenzie and Head).

With regard to the insistence of the surgeon on the non-existence of painless visceral sensation, it will be sufficient here to indicate not only the fallibility of observations conducted under such unfavorable conditions, but further, the inconclusiveness of argumentation on the basis of a doubtful analogy between skin and viscera. The fallacy involved becomes the more apparent when a similar line of reasoning is applied to the external muscle system. The insensitivity of muscle, tendon, and joint, many times demonstrated, probably long retarded the recognition of the 'muscle sense'. Yet to-

---

[1] M. Foster: Physiology, IV, 284, according to whom pain is mediated by the ordinary afferent mechanism, and represents merely an exaggeration of afferent impulses which are continuously present but unnoticed. Another solution is presented by L. Oppenheimer who has pressed the vaso-motor nerves into service in explanation of internal as of external pain; see his Physiologie des Gefühls, 1899.

[2] J. Mackenzie: Pain, *Brain*, 1902, XXV, 384.

[3] For further discussion and references, see below, Section B.

day the absurdity of a denial of the kinæsthetic sensations bound up with the normal functioning of those tissues would hardly be tolerated. There is every reason for assuming that the adequate stimuli of internal tissues are radically different from those of the external (the cutaneous in particular), being in all probability predominantly chemical in character, as the functions of the internal organs would imply. The doctrine of the insensitivity of the viscera, while in its limited clinical application a fact of prime importance, need therefore in cases of normal functioning carry little weight for the psychologist prior to more exact and extended observations.

Among the physiologists[1] the parallel doctrine of the essentially abnormal and painful character of any sensation in the viscera represents a tradition handed down from Weber and based primarily upon a somewhat hasty inference from the absence of protective 'touch' organs similar to those of the skin on the mucosa or endodermal surfaces and of the lack of temperature or pressure sensations in the more peripheral portions of the alimentary canal,[2] secondarily perhaps on the physiologist's tendency toward preoccupation with the purely vegetative and nonconscious functions of the visceral nervous system. In view of the counter evidence available on the introspective side in the writings of J. Müller, Bain, Sherrington, Helmholtz, Lehmann, Ebbinghaus, James, Beaunis, Titchener, and Meumann, we need not at this point enter into further discussion on the question.

3. *Plurality of Qualities.* For those who grant the possibility of painless visceral sensation, the terminological question of the variety of sense qualities, the number of tones, so to speak, in the organic gamut, next arises. In view of the general conviction that analytical methods are unavailing in this sphere, the distance covered by individual observers is worth

---

[1] *Cf.* Gruenhagen's Lehrbuch der Physiologie; W. Nagel's Handbuch der Physiologie des Menschen: M. Foster's Physiology; and W. H. Howell's Text-book of Physiology, 1906, all of which virtually ignore any quality other than pain. This is frequently, however, little more than an assertion of the essential difference between internal and external surfaces, and the lack of response of the former to environmental stimuli. When the physiologist happens to think of a true functional visceral complex, as hunger or nausea, he is usually moved to make an exception to his 'no non-painful visceral sensation' rule. Further, his concept of pain is largely physiological; pain is simply the harmful to the organism. Inasmuch as he is usually uncertain, on the psychological side, whether the painfulness of pain is a sensory or affective attribute, and intrinsic or adventitious, his applications of the term may frequently be misleading: his physiological pain may connote, on the psychological side, a quite indifferent sensation.

[2] *Op. cit.*, 562, 489, 497. See also Meumann: *op. cit.*, 43-47.

noting. A brief survey of the literature reveals the case as follows. In spite of the alleged inaccessibility of the organic factors to detailed examination, the general trend of opinion guarantees their uniqueness and variety—uniqueness as over against the qualities of the muscle-skin continuum, variety as among themselves, in virtue of their origin in different organs and tissues. The significance of this apparent liberality must, however, not be overrated; the above statement must be hedged in with certain reservations. In the first place, it must be admitted that, in individual cases, notably in the psychologies of Ladd and Wundt,[1] this class of sensations fares badly: the richness and variety of coloring ascribed to organic content are invariably inversely proportionate to the importance and variety attributed to the affective elements proper. In the second place, the significance of the unanimity above cited is greatly lessened when the varying rigidity with which introspective criteria and the concept of quality are applied by the different writers is taken into account. The varying importance and significance attached to the doctrine of specific energies here plays an important rôle. Laxity in regard to this principle is usually manifested in a tendency to accept offhand any superficial difference, topographical or practical in origin, as qualitatively valid. In general, exact introspective evaluation and analysis is absent and classifications are based upon purely extrinsic data. Müller, Bain, Sherrington, Helmholtz, Ebbinghaus, James, Beaunis, Titchener, and Meumann admit plurality of organic qualities in some form or other, but with what discrepancy in the details! Müller,[2] *e. g.*, while conceding a variety of shading in visceral sensation, nevertheless unhesitatingly ranks it in the same sub-class with the external cutaneous and muscle sense elements, pressure, temperature and pain, themselves susceptible of many qualitative fluctuations and gradations. The remainder of the authors listed hold apparently to the uniqueness of the organic contribution as over against the muscle-skin continuum, but this can only be predicated with certainty of Titchener, Ebbinghaus and Meumann. In general, the 'divisibility of organic experience into two great categories, the pleasurable and painful, is admitted, but in the detail of further classification great diversity exists.

The prodigality with which Bain[3] lavishes his sensory rubrics is especially conspicuous. Various forms of muscular, nervous, circulatory, respiratory, and digestive 'feelings' are

---

[1] G. T. Ladd: *op. cit.*, 168; Wundt: *loc. cit.*
[2] Müller: *op. cit.*, 1324 and 1087.
[3] A. Bain: Senses and Intellect, 1864, 118ff.

cited, and the peculiar attributes of fatigue, exhilaration, depression, suffocation, hunger, satiety, nausea, and various other bodily comforts and discomforts described in general terms. Comparison or detailed analysis is, however, not attempted, with the result that ramifications overlap and the classification remains of dubious value except as a testimonial of the author's absorbing interest in the subject.

Beaunis'[1] voluminous schema is likewise unreliable, based as it evidently is on a half dozen different principles of classification. Differences in the way in which sensations come to consciousness, variations in their mode of origin or occasion of appearance (*i. e.*, in the normal exercise of function, in premonition of organic needs, or in surgical manipulation), their pleasant or painful character, rather than observed qualitative distinctions form the *raison d'être* of his eightfold division. The casual character of the classification stands revealed in the headings, 'organic sensibility', 'organic needs or cravings (*besoins*)', 'functional sensations', 'cœnæsthesia', 'emotional sensations', 'specific sensations', 'pain', and 'pleasure', which obviously indicate groupings or points of view significant to the physician or biologist, but hardly to the psychologist. While Beaunis is usually quoted as having testified to the plurality of organic qualities, he really never comes to close quarters with the question, but concerns himself, as a matter of fact, almost exclusively, with the determination of the number of the internal tissues sensitive, and the extent of the rôle played by organic, more especially, *muscle* sensation, either in its conscious or subconscious phases, in all adjustments of the organism.

Horwicz seems to occupy a position midway between those of Bain and Sherrington. While apparently admitting specific sense qualities, he maintains that many of them are obscure and indescribable.[2]

Sherrington,[3] though primarily interested in the problem of pain, admits the existence of many organic sensations both pleasurable and painful which are far from obscure. Both functional activity and the call for repetition of this activity are plainly conscious, although they must remain forever indescribable in virtue of the fact that the physical offers no adequate and unequivocal ultra-individual symbols for these

[1] H. Beaunis: Les sensations internes, 1889, 3.

[2] A. Horwicz, *op. cit.* See also J. Sully's review, *Mind*, 1882, VII, 302-3.

[3] C. S. Sherrington: in E. A. Schäfer's Text-book of Physiology, II, 971-3. There is also in "The Integrative Action of the Nervous System," 1906, 130, an implication that the 'proprio-ceptive' qualities of the deeper tissues are specific.

sensations. The variety of organic sense qualities is, however, in this discussion an implication merely. In view of the tendency occasionally appearing among physiologists to identify pleasantness and unpleasantness with pure sensory qualities (*i. e.*, the physical sensations of pleasure and pain), it is interesting to note that Sherrington, along with the three preceding writers quoted,[1] may perhaps best be described as regarding organic sensations as modes or shades of the two basic qualities of bodily comfort and discomfort.

For Ebbinghaus,[2] the sensory contribution from the internal tissues is as varied and extensive as for Bain. Hunger, thirst, satiety, discomfort, nausea, fatigue, energy, oppression, suffocation, buoyancy and the like are listed as arising from the digestive, muscle, and respiratory systems respectively, along with certain more diffuse organic states, such as excitement, unrest, relaxation, depression, dullness and drowsiness, the probable origin of which is assigned to changes in the circulatory system. The question of the possible complexity, similarity or relationships of these states is not raised. The implication is that each represents a specific sensation peculiar to the system from which it arises, each organ possessing '*eigenartige Empfindungen*'. Further, sensitivity to pain is declared to be lacking only in connection with the substance of the brain, and the mucosa of the intestines and stomach.

Titchener (see J. M. Baldwin's Dictionary of Philosophy and Psychology, II, 218) distinguishes, apart from skin and kinæsthetic sensations, a number of organic qualities for which the alimentary canal, circulatory, and respiratory systems are held responsible.[3]

Meumann,[4] in a recent article, has expressed himself in favor of a great multiplicity of organic sensations (*innere Tastempfindungen*), on the basis not only of the *a priori* argument of origin in different organs and tissues—unstriped muscle, serosa, glands, capillaries, the alveoli of the lungs, etc., and of usefulness to the organism, but also as the result of his own carefully recorded introspections and of certain observations of d'Allonnes. This qualitative diversity is obscured, he maintains, only by the indefiniteness of localization of the

---

[1] Weber and Kröner might be included in this list.

[2] H. Ebbinghaus: Grundzüge der Psychologie, 1902, I, 426-32. The 'Abriss der Psychologie', 1908, p. 49, contains perhaps the most positive statement in the whole literature. "Die durch sie vermittelten Empfindungen stehen ebenso selbständig und eigenartig nebeneinander und neben den übrigen Empfindungen wie Farben neben Tönen und Geschmäcken."

[3] See the same author's Primer of Psychology, § 21; Text-book of Psychology, §§ 56 ff.

[4] *Op. cit.*, 56-8.

sensations, and their deficiency in correlated visual images by which qualitative isolation might be facilitated.

James, while assigning tremendous importance to organic sensation and expressly stating that at least for pain the 'internal organs have their explicit qualia of sensation just as different areas of the skin have their respective local signs',[1] contents himself characteristically with the assertion that 'our whole cubic capacity is sensibly alive',[2] and with casual references to 'precordial anxiety', the 'euphoria arising from the respiratory organs', to the 'pang in the breast', a 'fullness in the breathing', etc., without at all committing himself to the composite or elementary character and specific differences of these bodily symptoms. It is further to be noted that a large proportion of the physiological 'reverberations' reported by him as constituting emotion are obviously cutaneous and muscular rather than visceral.

Finally, there is a small group of writers, who, while attributing a certain diversity to organic sensation (*i. e.*, not limiting it strictly to the category of pain), recognize its close kinship if not identity with the external (cutaneous and muscular) continuum. In this class belong Müller, Külpe, Goldscheider, and Calkins,[3] all of whom are inclined to find in the quality of pressure a sufficient explanation for all visceral complexes. A similar tendency to posit a single muscle quality common to both skeletal and visceral tissues may also be noted. Weber, Bain, and Beaunis may here be mentioned, although the most explicit statement occurs perhaps in Lagerborg.[4] Lagerborg himself, however, is unwilling to commit himself unconditionally to the identity of this quality in the external and internal tissues, owing largely to the differences in the minute structure of the latter. As a matter of fact this phase of the question seems to have rested so far on anatomical rather than psychological observations and comparisons.

In conclusion, it must be admitted that there is nowhere in the literature any consistent attempt to indicate similarities

---

[1] W. James: Principles of Psychology, II, 156.

[2] *Op. cit.*, 451.

[3] Müller: *loc. cit.;* Külpe: *loc. cit.;* Goldscheider, Gesammelte Abhandlungen, I, 1898, 47-8; M. W. Calkins: Introduction to Psychology, 1901, 84-6. Mention may here also be made of E. Becher who has recently on the basis of certain experimental observations on the stomach and œsophagus (see *Zeitschrift f. Psych. und Phys. d. Sinnesorg.*, 1908, XLIX, 341 ff.) arrived at the conclusion that feeble sensations of warmth, cold and pressure may be obtained from the œsophagus, and that all the other viscera are probably insensitive. The sensations usually ascribed to the viscera are either centrally excited or arise from the peritoneum, pleuræ, diaphragm, or body wall (hence are presumably closely related to the external continuum).

[4] *Op. cit.*, 16 and 17.

and relationships in the states under discussion, nor is there even any substantial agreement in the use of single terms. Hunger, *e. g.*, is identified by Kröner with a diffuse, unlocalizable feeling (preferably, faintness), by Sherrington and others with a localized sensation mass in the epigastrium. Neither, moreover, is there any light shed upon the subject of specific liver, kidney, lung or heart sensations in the discussion of psychopathic or emotional cases by Störring, Kraepelin, Janet or Lange, as we had hoped. It is plain that the solution of the problem of a scientific terminology and descriptive basis for organic sensations must be preceded by a more minute examination than has yet been attempted (in any of the authors quoted) of the sense material concerned; and especially by a detailed comparison of visceral with external (kinæsthetic and cutaneous) sense qualities. Such a procedure should, at the very least, establish some large likenesses or unlikenesses (other than pleasantness or unpleasantness) which may serve as the basis of a stable classification.

Summary. While, on the testimony above cited, the existence of a large body of sensations not usually externally referred is incontestable, the precise significance of this fact remains to be determined. The ground of distinction between organic and special sense experience stands greatly in need of definition.

The fusability, absence of memory images, unanalyzability, lack of cohesiveness with other sensations, unlocalizability, capacity for eluding the attention, and other features ascribed guardedly or confidently in various quarters to our organic experience, demand critical verification. Finally, whatever may be the actual status of these sensations, the investigation of what represents apparently the least developed and systematized sphere of our consciousness may reasonably be expected to throw a new light upon the processes of localization and attention. Even though the results of this study should fail to assist in the solution of any psychological problem, a systematic account of these processes and their localizability in the normal subject may be not without value to the pathologist and clinician.

Histological, Pathological and Physiological Evidence

An investigation of the psychology of internally initiated sensation can hardly be undertaken without first attempting to stake out the possibilities in keeping with neurological conditions. As already indicated, such problems as the differentiation of qualities and the range of sensitivity in the internal tissues, of the possibility of localization and analysis, and the significance of the former, are clearly related to the histological problems of the variety and distribution of afferent nerve endings and the possibilities of diffusion inherent in the peripheral or central neural mechanism. A brief survey of the literature of the sympathetic system and the sensory innervation of the internal tissues has accordingly been undertaken and the consensus of expert opinion upon the number of tissues sensitive, kinds of endings, relative fewness or multiplicity of nerve fibres, conduction paths, brain centres, and the like collated. For information upon the immediate and remoter origin of internal sensation, according as determined directly by chemical or mechanical changes in the circulatory or muscular tissue, or indirectly through the medium of reflexes or irradiation in the central system, recourse has been had to physiology.

The result has proved somewhat disappointing. Authenticated knowledge of the sensory endings in any given tissue and their conduction paths from periphery to cortex is meagre. Direct evidence based on the results of degeneration or experimental stimulation of the nerve elements figures to a surprisingly small extent and is eked out largely by inference from more or less ambiguous introspective data. *E. g.*, the existence even of afferent endings in plain muscle tissue is argued from the fact of locally referred pain in renary and biliary colic. The insecurity of any conclusion based on such premisses becomes apparent in view of the doubt cast upon the trustworthiness of localization by certain clinicians and pathologists, *viz.*, Mackenzie and Head, in their doctrine of referred pain. The need not only of a more thorough-going exploration of the centripetal paths through the posterior root ganglia and cord but also of a more cautious use of the facts of apparent localization, will become apparent as we proceed. On the whole we can extract from histology in its present status very little evidence bearing directly on organic sensation. So far as sensory in distinction from afferent innervation of the tissues is concerned, psychology is largely thrown back upon her own resources.

With regard to the evidence of pathology, the question raised by Mackenzie whether internally initiated sensations have central pathways and end-stations in their own right, or are dependent for access to the higher (*i. e.* conscious) centres upon pathways borrowed from the sensory system of the body wall, is an important one. Its bearing upon the existence of unique and specific sensory qualities in the viscera is, in view of the current assumption of the all-importance of the end-station and the indifference of the process in the nerve fibre, obvious, and will be examined in some detail.

A. Anatomical and Histological

1. *End-organs in internal tissues.* Only two kinds of receptive tissue[1] have so far been described in the internal organs, the free sensory endings and the Vater-Pacini corpuscles. Of these, the latter (which occur also in the dermis of the hand and foot, near the joints, in the periosteum of certain bones, in tendons, intermuscular septa, and even in muscle) are found in the epineural sheaths of certain nerve trunks and near large vessels, and are numerous in the peritoneum, mesentery, pleura and pericardium.[2] The free sensory endings, which are usually branching and nodulose, are found in epithelial connective and plain muscle tissues, and are distinguished from motor endings mainly by terminating on, rather than in, the muscle fibre.[3]

The bearing of this limited differentiation of sensory endings on the question of the number of specific differences in organic sensation is of course debatable. If we accept the theory of specific energies, which maintains that the nerve process excited in any peripheral fibre by a physical stimulus is *per se* indifferent, *i. e.*, of one kind only, and that even if the nerve fibre itself is excitable by a wide range of stimuli known as common stimuli (thermal chemical, mechanical, electrical, etc.), mere difference in character of the stimuli is not sufficient to secure differentiation of sensory response, but that the latter presupposes an elaborate neural evolution in which appropriate end-organs to select one form of stimulus, and protect the nerve ending from all other forms are developed along with a central mechanism for securing differentiation in consciousness of the corresponding sensation,—if we accept this hypothesis in all its strictness, the implication for organic sensation is of course pretty straightforward: evidence for a plurality of organic sense qualities is lacking. Two objections to such a conclusion may, however, be raised. First, it is not improbable that further histological research may discover slight differentiations in the chemical composition if not in the structure of the

[1]A possible third is represented by the occurrence in the mucosa of the ileum of fibrils terminating on or near nerve cells similar to tactile corpuscles. Poirier et Charpy: Traité d'anatomie humaine, 1901, IV, 92.

[2]A. A. Böhm, M. von Davidoff, and G. C. Huber: Textbook of Histology, 1904, 174. It is unfortunately not always clear whether human or mammalian tissues in general form the basis of such deductions.

[3]There is, however, a difference of opinion here. G. C. Huber maintains that the motor fibre itself ends *on*, not in, the muscle fibre, leaving the sensory-motor distinction the more in doubt. *Cf.* The Sympathetic Nervous System, *Journal of Comparative Neurology*, 1897, VII, 192.

tissues in which the nerve fibres end or in the terminal nodules themselves, such as to render them specifically adapted to the chemical reactions or substances peculiar or significant to the tissues in which they occur. The fact that a relatively small variety of end-organs has been discovered in the mucous membrane of the mouth which nevertheless possesses a sensitivity to temperature, pressure, and pain (not to mention taste) as acute or more so than that of the external skin, lends color to this hypothesis. Secondly, the tendency is now showing itself to abandon the doctrine of the identity of the nerve process in all fibres and the more rigid conception of specific energies, and to suggest, what indeed was the older view, that the range of excitability of a single nerve fibre may include stimuli slightly different in quality, each of which is capable of giving rise to its own peculiar nerve impulse, which when transmitted to the centre may be sensed as an individualized sensation quality.[1] Apply this doctrine to the free sensory endings of the viscera, and the impossibility of an extended gamut of related organic qualities vanishes.[2] In the absence of any deterring histological or physiological evidence, we may at least admit that the field remains open to psychological exploration and experiment.

2. *Distribution of afferent innervation.* The occurrence of 'sensory' nerve fibrils in the epithelium of the bladder, œsophagus, tongue, lung, liver, pancreas, stomach, intestine and the like; in the connective tissue of the heart, lung, and eyes, of the dura mater, and of certain mucosa; and probably in the plain muscle tissue of the hollow viscera (heart, intestine, ducts, glands, etc.) is maintained by Barker[3], and more or less substantially sustained by Böhm, by the anatomists Poirier and Charpy, and by various others with certain reservations. It is to be noted, however, that the term 'sensory' is to a certain extent used interchangeably with 'afferent', and that the classification of a nerve as sensory is in many cases based merely on the supposedly non-motor character of the endings, unsupplemented either by the tracing of the fibre back to the posterior root ganglion, or the testing of the stump for pain. Further caution is suggested by the possibility that many of the fibrils listed, even if not motor or secretory, represent nerves concerned only with the mediation of reflexes in the cord or lower centres, and incapable of projecting a sensory

---

[1] W. Nagel: *op. cit.*, 13, 15.

[2] As a third possibility we might suggest the structural degeneration of the end-organ, after a functional and central differentiation had become established.

[3] L. F. Barker: The Nervous System and Its Constituent Neurones.

impulse to consciousness.[1] The evidence and line of argument by which the neurologist or physiologist arrives at his conclusions must therefore be scrutinized in detail.

The general method of procedure by which the afferent or sensory innervation of the various tissues is established is as follows. Gross anatomical and experimental methods come first in point of time. The stumps of the general nerve supply of any region were tested by the early anatomists, either for outward evidences of pain, or for simple reflex discharge in related motor channels, and inferences on the sensitivity or insensitivity of the tissues innervated made accordingly. Thus the insensitivity to pain of the sympathetic nerve as tested in the experiments of Majendie was assumed to establish the insensitivity of the innervated tissues, notably the intestine and its outlying glandular systems,[2] while the sensitivity of the stump of the vagus was, on the contrary, assumed to indicate the sensitivity of the region of its peripheral distribution, inclusive of heart, lungs, and stomach in particular. The ambiguity of tests of this type is readily apparent. The test based upon the stimulation of the stump for pain symptoms is valid only for pain nerves and overlooks the possibility of the presence of non-painful sensory fibres, at the same time that it ignores the probability that the adequate stimulation of visceral fibres is serial, intermittent, and summating. Even the success of the test in eliciting evidences of reflex action indicates at best the existence of a reflex arc through the cord or lower centres.

As a matter of fact, later application of these very tests has resulted in the reversal of Magendie's findings. Evidences of pain following stimulation of the sympathetic (through the cœliac or solar ganglia, or the splanchnic nerve) have been demonstrated by Flourens, Brachelt, Mayer, and Müller.[3] Indeed, to-day the responsibility for pain in organs below the diaphragm (the stomach, liver, and pancreas in particular) is usually shifted to fibres reaching these organs via the sympathetic rather than the vagus,[4] although the innervation by the two systems is to a large extent overlapping. In general, however, with refinement of methods and a growing discrimi-

[1]The possibility that even purely afferent or reflex fibres may under conditions of excessive stress borrow from other systems a path to the higher centres will be examined later. The *normal* sensory functioning of the fibres is alone here in question.

[2]The innervation of the intestine by both sympathetic and vagus (*cf.* Langley) was evidently not then recognized.

[3]Müller: *op. cit.*, 646, 705, 711 ff. These tests were usually, of course, performed upon animals.

[4] *Cf.* J. N. Langley: The Autonomic Nervous System, *Brain*, 1903, XXVI.

nation in the use of the terms affective and sensory, the tendency has sprung up among neurologists to distrust the sensory function of any fibre travelling with the sympathetic unless its passage direct from the posterior root ganglion *through* the sympathetic ganglia without termination can be demonstrated or inferred. That is, the only clear title of any fibre to sensation lies, histologically speaking, in its claim to enrollment as a cerebro-spinal fibre. On this basis the bias for the sensory innervation of the tissues supplied by a cranial nerve such as the vagus (the thoracic viscera and stomach especially), or by a spinal nerve, as the phrenic (arising from the third to fifth cervical roots and supplying the pleuræ, pericardium, and diaphragm),[1] is strong. Further, the demonstration of individual cerebro-spinal fibres (*i. e.*, of fibres passing without interruption from the posterior root ganglion cells to peripheral terminations in visceral tissue) in the sympathetic plexuses,[2] and the observation that certain histological peculiarities characterize such fibres, have suggested a practical basis for the distinction of afferent from efferent fibres (more particularly of sensory from

---

[1] K. Hasse: Handatlas d. sensiblen und motorischen Gebiete der Hirn- und Rückenmarksnerven.

[2] The fact that practically no afferent fibres occur in the gray rami by which efferent fibres of sympathetic origin return to the spinal system for distribution to the body wall, which must therefore draw its afferent or sensory supply directly from spinal ganglia (P. J. Cunningham, Textbook of Anatomy, 704), has led to the hypothesis that a large number of the afferent fibres of the viscera, in homology with those of the body wall, do not reproduce the course of the efferent autonomic system and terminate in sympathetic ganglia, to resume their course in the form of dentritic or axone fibres from sympathetic cells, but behave in every way like ordinary sensory nerves. Kölliker (*cf.* Quain, Elements of Anatomy; 350,), Gaskell (*Journal of Physiology*, 1886), Huber (*Journal of Comparative Neurology*, 1897, VII, 131), and Langley (Schäfer, *op. cit.*, 687), all recognize the presence in the sympathetic strands of a number of these (strictly speaking) *non*-sympathetic or *cerebro-spinal* fibres, having their trophic centres in spinal ganglia.

From this point of view the sympathetic ganglia would, of course, possess no automatic or reflex functions but represent merely a provision for the wider distribution of the sphere of influence of all preganglionic efferent elements, most of which end in relation with several different ganglion cells. To be sure, certain histologists, notably Dogiel, maintain the presence in the sympathetic ganglia of sensory or afferent cells, the axones of which end in association with efferent cells in the sympathetic system or cord to form reflex arcs. Schulze, moreover, finds among smooth muscle fibres sensory ganglion cells with one long central process, which, he suggests, is ordinarily concerned with the mediation of reflexes (presumably in sympathetic ganglia), but in cases of muscular spasm may give rise to pain. Lenhossek is also of the opinion that the sympathetic ganglia represent either relay or terminal stations for certain of the sensory fibres (Huber, *op. cit.*). The evidence on these points is, however, insufficient, and even if such a condition of things exists it obviously holds good only for a small proportion of the afferent fibres.

merely afferent), and for a more accurate discrimination of sensitive from non-sensitive tissues.

The marks by which the character of any nerve as cerebrospinal and afferent, hence presumably sensory in function, are established, are three. First, the degeneration test, by which continuity of the fibre in its complete course from posterior ganglion to endings in any tissue is traced. Of evidence collected in this fashion there is only too little. Such as does exist points to the sensory innervation of the mesentery and the peritoneum (where the fibres end in Pacinian bodies), and of the bladder (in the frog).[1] Secondly, medullation of the fibres,[2] and thirdly, their large size or diameter. Neither of these last criteria is held to be absolutely valid in itself. They are, however, accepted as indicating the sensory character of the nerve endings in blood-vessels, heart, dura and pia matter,[3] bladder, gall-bladder and mucosa of the intestines.[4]

With regard to the more minute histological indications of differentiation, the presence of free nerve fibrils in non-motor or non-secretory tissue (connective or epithelial) is assumed to represent afferent or sensory innervation. This applies to the vascular tissues, to the bladder, intestines, and endodermal or epithelial tissues in general.[5] On the other hand, such free fibrils as occur in the walls of hollow muscular organs—the alimentary canal, biliary and renal ducts, blood vessels, heart, etc.—are distinguishable from motor endings by minor characteristics only,[6] and their sensory function is largely inferred from the apparent sensitivity of these tissues in colic, renal and biliary inflammation, angina pectoris, migraine, etc.[7]

---

[1] Huber: *loc. cit.*

[2] It is a generally accepted fact that only post-ganglionic fibres—*i. e.* the fibres taking origin in ganglionic cells and representing the post-ganglionic continuation of the paths of fibres which have terminated around these cells—are unmedullated. Since this is known to be the rule with efferent sympathetic nerves and since many medullated fibres have been traced back to the dorsal ganglia, the *a priori* assumption is usually made that a medullated fibre in the peripheral twigs of the sympathetic represents an afferent or sensory neurone of the spinal system.

[3] Böhm, Davidoff and Huber: *op. cit.* Sensory endings in the fibro-elastic tissue of many vessels, their endings beset with varicosities, have been recently demonstrated by Dogiel, Schemetkin, and Huber (p. 223). For endings in the inner lining of the larger vessels and in the endo and pericardium, see p. 215; in the dura mater, p. 437.

[4] Stöhr: Lehrbuch d. Histologie.

[5] Van Gehuchten: Anatomie du systême nerveux, 1901, II, 531. See also Stöhr: *op. cit.*

[6] *Cf.* Schultz: Die glatte Musculatur der Wirbelthiere, *Archiv. f. Anat. u. Physiol.*, 1895.

[7] Barker: *op. cit.* See also Huber (*op. cit.*) for the suggestion that this line of reasoning is misleading, especially in view of the fact that sensory fibres have been traced into the *epithelium* of certain of the hollow organs.

The first line of argument overlooks the possibility that the fibres may be trophic in function, the second, the probability that other (sensitive) tissues than those immediately concerned in contraction or spasm may be involved. In itself, it is obvious, either line of reasoning goes only a short way unless supported by the system of experimentation and inference outlined above.

In summary, it must be granted that there is hardly a tissue in the interior of the body (mucosa, serosa, or plain muscle fibre) which may not be regarded as possessing an afferent, and thus potentially, a sensory neural equipment. Further, a number of minor lines of evidence converge to indicate the sensory function of this equipment in a large number of these tissues. That for the plain muscle fibres is perhaps the weakest.

According to the calculations of Langley, the proportion of afferent to efferent fibres in the sympathetic system in general is about one to ten. While this seems to corroborate Weber's reference of the feebleness of internal sensation to the fewness of the innervating fibres,[1] it is perhaps more accurately represented as a device for the intensification of the internal sensory impulses through summation. The branches of a single fibre are distributed over a wide area, and the neural effect due to the stimulation of a large number of terminal fibres is thus presumably concentrated in a single centripetal impulse. Further, a large number of the viscera (the thoracic organs, stomach, liver, intestines, and probably liver and pancreas in particular) receive a double afferent innervation, *i. e.*, from both vagus and sympathetic.[2]

3. *Pathways of visceral impulses in the spinal cord.* While the peripheral equipment of the internal tissues appears thus to extend almost indefinitely the possibility of sensation, a further question arises as to the provision for the transmission in isolation and in their full strength, of the sensory impulses to the cortex. First, does the afferent neurone possess a clear pathway in its own right to the cortex (and hence to consciousness) as does for example the sensory neurone from the exterior; or does its axone terminate in association with motor cells in the cord or lower centres, if not actually in a sympathetic ganglion, thereby indicating the function of the neurone to be merely reflex?[3] Secondly, even if a clear centripetal

---

[1] *Op. cit.*, 491.

[2] *Op. cit.*, 29.

[3] As would apparently be demanded by J. Müller's conception of the afferent sympathetic. See *op. cit.*, I, p. 723 f., where he suggests that visceral impulses are unconscious not because suppressed by sympathetic ganglia (Reil's suggestion), but because they function in the mediation of reflexes, and are accustomed to 'sich ausgleichen' in the cord.

pathway may be assumed, are there no peculiarities intrinsic to the afferent sympathetic neurone (such, *e. g.*, as the absence of medullation, the multipolar character of the cell bodies and the complexity of their terminal fibre connections) which make for diffusion, poor localization, or feeble intensity?

With regard to the first question, the fact that the afferent 'sympathetic' supply to the unstriated muscle fibres, glands, and blood vessels of the skin and body wall demonstrably does not even pass through a sympathetic ganglion but emerges directly from the posterior ganglion to join the postganglionic efferent fibres supplying the same general region, lends color to the probability that the homologous system of internal afferent fibres is quite like ordinary cerebro-spinal nerves, and that an unbroken and medullated course from the posterior ganglion to their final peripheral division into terminal fibrils may be predicted for them. As a matter of fact, so far as an actual examination of the state of affairs has been made, by means of the degeneration method or the tracing of medullated fibres to the very periphery, the evidence goes to show that the majority of the afferent fibres of the white rami communicantes (the visceral branches of the spinal nerves), and their peripheral continuations have their trophic centres in the posterior root ganglia just as do the cutaneous sensory fibres, and enter into no actual connections with sympathetic ganglion cells.[1] Further, experimentation has thus far failed definitely to demonstrate reflex action in any sympathetic ganglion,[2] hence the probability that the afferent fibres neither end themselves in sympathetic ganglia, nor give off any branches for that purpose, but afford a direct pathway to the cord with no opportunity for the shunting off of the sensory impulse into peripheral reflex paths, is materially strengthened.

The question whether the route through the posterior sensory ganglia is uninvolved and in all ways similar to that of an ordinary sensory neurone, *i. e.*, interrupted by one cell body only, is answered in the affirmative by Langley, on the basis both of histological demonstration and the fact that the number of fibres on either side of the ganglia is the same. The feebleness of sensations from the interior is due probably to high liminal values and fewness of nerves rather than to friction in peripheral paths.[3] Barker, however, thinks it probable that the sensory impulses collected by means of sym-

[1] Langley: *loc. cit.;* Cunningham: *op. cit.*, 704; Kölliker in Van Gehuchten: *loc. cit.;* Huber: *op. cit.*, 131.

[2] Exception must possibly be made here of such peculiar peripheral structures as the plexuses of Meissner and Auerbach in the alimentary canal.

[3] Brain, 1903, XXVI.

pathetic neurones pass through a chain or series of individual neurones before arriving at a primary end-station in the cerebrospinal system.[1] Their fate after joining the dorsal roots of the spinal nerves he admits is unknown, but suggests that some of the sympathetic axones end within the spinal ganglia around the bodies of spinal ganglion cells, and that others pass directly through into the cord. As a matter of fact, Cajal has reported the ending of sympathetic fibres in branches about spinal ganglion cells, and Dogiel asserts that certain sympathetic fibres (which he assumes arise from sensory sympathetic ganglion cells) end about posterior root cells of a new type—small, and with a branched axone, the branches ending in association with a number of ordinary unipolar cells.[2] The evidence on these points is, however, casual and unsupported: Langley's position represents the larger body of facts.

With regard to the further pathway of the afferent impulse through the cord, Langley suggests two possibilities.[3] A certain number of the visceral afferent fibres in all probability serve only to mediate reflexes to autonomous tissues, and are incapable of giving rise directly to sensation,[4] while all the rest are sensory. The distinguishing histological characteristics of the two are yet to be determined, but presumably the upward paths in the cord are poorly developed in the case of the former. It is elsewhere suggested by Langley[5] that the large medullated fibres ending in serous membranes are probably the ones which give rise to (direct) local pain; while the small fibres (ending in connection with unstriated muscle and glandular tissue) give rise to referred pain, that is, acquire an upward pathway and a sensory function only under very unusual conditions.

With regard to the provision in the gray matter of the cord for the transfer of irradiation from visceral to cutaneous fibres (in case of referred pain), there is at present no satisfactory evidence, as Langley admits. Even the irradiation from fibre to fibre of the sympathetic to which Müller[6] had recourse in

---

[1] *Op. cit.*, 553.

[2] *Cf.* Van Gehuchten: *op. cit.;* also Schäfer: *op. cit.*, 686.

[3] *Op. cit.* M. Meyer's suggestion for *pain* paths of the external and internal systems, *i. e.*, their convergence into fewer and fewer neurones as they pass up the cord, thus eliminating the possibility of exact localization of sensation, seems to find no direct support in the histological literature. See Der Schmerz, 1906, 74.

[4] Mackenzie's hypothesis (see above) would seem to demand the ending of *all* visceral afferent axones in the lower motor centres, and the total absence of centripetal pathways past these centres to the cortex, sensation being effected only through the leakage in the cord to sensory neurones of the *somatic system.*

[5] Schäfer: *op. cit.*, 686.

[6] *Op. cit.*, I, 723 and 680.

explanation of the vagueness and indefiniteness of localization of organic sensation has found so far no basis in the anatomical peculiarities of the afferent sympathetic neurones. Neither absence of medullation in the peripheral fibres, nor the multipolar type of cell peculiar to the efferent sympathetic neurone, is characteristic of the afferent.

Lastly, with regard to the representation of visceral sensation in the cerebral cortex. In general, no reference of visceral sensation to cortical centres has been attempted. The projection of visceral fibres above the medulla seems to be unknown. Even for cutaneous pain no end-station higher than the optic thalami has been demonstrated. Sollier,[1] however, maintains on the evidence of experimental and pathological cases the existence in the parietal lobe (generally given over to the 'stereognostic' sense) of centres for the stomach, intestines, heart, respiratory apparatus, genital organs, and bladder. Ferrier, on the other hand, names the occipital lobe as the recipient of excitations from the viscera. Further research is obviously in order before the question can be satisfactorily settled. Even if, however, the projection of visceral paths beyond the lower centres of the brain should not be demonstrated, and the inaccessibility of visceral impulses to the cortex (except through borrowed paths, as pain) were admitted, the possibility would still remain that consciousness is not limited to any rigid correlation with the cerebral cortex, but accompanies also the activity of the lower centres, and that even the feebler visceral impulses may thus attain a dim sort of consciousness.

Our survey of the histological and anatomical evidence for organic sensation brings us, therefore, to the following conclusion. Although the number of afferent fibres to the viscera is relatively small, their distribution is wide and the corresponding possibility of sensitivity considerable. While on the whole the probabilities are against any extensive differentiation of organic sensation, the possibility that non-painful sensations of some sort or other may reach consciousness from the viscera remains practically uncontroverted. Further, no insuperable obstacle to successful observation and analysis has been demonstrated in the histological conditions of the centripetal pathway. It remains, of course, to examine the limitations on the physiological side which may possibly be established by a study of the experimental, pathological, and clinical evidence.

B. Physiological. The Conditions and Functions in the Organism of Organic Sensation

1. *Adequate stimuli of afferent nerves in internal tissues.* Among the possible stimuli, mechanical and chemical, the

[1] P. Sollier: Le mécanisme des émotions, 1905, 215-7.

following first suggest themselves. The contact or friction on mucous surfaces of food substances, secretions, excreta, etc., on serous surfaces of heavy or distended viscera; the intercellular pressure from dilated blood vessels, or distended gland cells in certain tissues; the distension of ento- or meso-dermal linings (in the stomach, bladder, ducts, blood vessels, alveoli, etc.) by gaseous or fluid contents (operating probably through the stretching of the afferent nerve fibres of these tissues); and the compression through muscle contraction of the endings in the walls of hollow muscle organs (heart, stomach, certain blood vessels, ducts, etc.). Further, the chemical properties of entering substances (food, air, etc.), the drying of the epithelium of stomach and pharynx, the presence of fatigue or toxic products in the tissues, and the varying consistency and chemical attributes of the blood may be mentioned.[1] Of these the physiologists specifically recognize (on the ground of their efficiency in the production of visceral reflexes) the following: distension of lung tissue in inspiration,[2] mechanical stimulation of food in the œsophagus, stomach, etc.,[3] of foreign or pathological substances in the respiratory entrances and secretory ducts, compression through muscular contraction (effective in vaso-dilator reflexes), the chemical properties of the blood in the cavity of the heart,[4] or distension of the latter, noxious gases at the entrance of the lungs,[5] and chemical properties of food or the acidity of the contents of the stomach (effective in the production of secretion, nausea, peristalsis, and the like).

The question whether these stimuli act upon nerve endings specifically adapted to receive them, or whether they belong merely to the class known as common nerve excitants, *i. e.*, are capable *per se* of acting directly upon any nerve fibre they may encounter, is a difficult one. In the former case the possibility of reducing all the excitants listed to a single closely related class, *i. e.*, mechanical (in which case chemical irritants would act only through direct attack of the nerve substance) or chemical (distension would then represent *inadequate* stimulation through stretching of the axones) may be mentioned, and will be referred to later.

2. *Adequate stimuli of internal sensation.* There is general agreement that the mechanical sensibility of the viscera differs radically from that of the outer integument. There is in the

---

[1] The fact that the viscera are customarily rated *insensitive* to many of the above has no immediate bearing on the point here under discussion, the excitability of afferent, rather than merely sensory, visceral endings.

[2] W. H. Howell: Text-book of Physiology, 1906, 617.

[3] *Op. cit.*, 645-6, 648.

[4] *Op. cit.*, 501.

[5] *Op. cit.*, 625.

deeper tissues no 'contact' sense homologous to that of the skin. The adequate stimuli of internal sensory nerves are physiological rather than physical; they are pre-eminently functionally or reflexly initiated excitations, whether ultimately represented by chemical or mechanical agencies. The impinging of the outer environment in the form of food, air, etc., is as a usual thing indirectly if at all sensible, *i. e.*, in the form of reflexly excited activities which represent not the objective properties of the stimulus, but the effect of the latter upon the organism. Contact or pressure on the inner mucous surfaces is, for instance, effective only in the reflex excitation through autonomic ganglia[1] (plexuses of Meissner and Auerbach) of secretion of digestive juices, peristalsis of the digestive tract, etc., which in turn are probably conscious only when excessive.[2] An exception to this rule must possibly be made in favor of substances producing distension of hollow organs, and thus directly affecting their intrinsic nerve fibres through stretching, as in cases of too rapid swallowing and the lodging of a morsel of food in the œsophagus, or in over-distension of the bladder, stomach, or arteries.[3] It is to be noted, however, that even here not mere presence or pressure of the disturbing substance, but actual distension of the tubes is apparently necessary, and, further, it may be only the resulting muscular reaction of the walls which comes to consciousness. Similarly, the effects of certain exogenous chemical substances, such as the feeling of suffocation produced by certain noxious gases, or the nausea attending the stimulation of the digestive tract by certain poisonous substances, are probably to be interpreted as representing the beginning of protective activity (contraction of the bronchial musculature, or of portions of the digestive tube preparatory to vomiting). They are further noteworthy as associated primarily with the entrances of the great receptive

---

[1] These reactions may of course represent in part merely the direct effect of a mechanical stimulus upon plain muscle tissue (see American Text-book of Physiology, 1896, 308), or upon its resident motor endings.

[2] The question whether the afferent neurones of autonomic ganglia have any connection with the higher centres is, as we have already shown, a difficult one. Neurological study has revealed so far merely the passage of a number of medullated afferent fibres through these ganglia on their centripetal course from the epithelium. Since, however, the ganglia are accessible to influences from higher centres, *i. e.*, have efferent connections with the cord, the supposition that they have also some sort of afferent connection does not seem extravagant.

[3] Here, as in the case of satiety, certain possible parietal peritoneum sensation, attending increased pressure of the organs and their contents upon the linings of the body wall, may be responsible for the conscious effect.

systems of the body, and as representative of abnormal or injurious conditions.

This brings us to the real reason for the prepossession in favor of correlating sensation with functional stimuli, which is partly a matter of argument that the consciousness of excessive or harmful activity can alone be of value, partly a matter of observation that in such familiar complexes as fatigue, hunger, colic, cardiac oppression, stuffiness, the nausea of indigestion, it is pre-eminently excessive functional activity or the need for such activity which comes to consciousness.

The fact that the functional activity of the organism is closely associated with metabolic change makes a strong case for those who regard the adequate internal sensory stimulus as pre-eminently chemical. The aptness of this doctrine in many of the above mentioned experiences, and especially in such generalized bodily states as faintness, exhilaration, etc., is manifest. At the same time, the probabilities are strong that a certain amount of mechanical stimulation (either muscle contraction, distension, stretching, or pressure upon parietal peritoneum surfaces) enters into the production of such psychoses as cardiac oppression, stuffiness, nausea, satiety, and colic, as well as into the majority of functional activities. Even such apparently refractory cases as the peripheral sensations characteristic of extreme nervous exhaustion and low blood pressure, and usually referred to hypothetical chemical changes affecting circulatory endings or the nervous system generally, may be reduced to mechanical terms, as Sherrington has demonstrated by referring them to the possible relaxation of certain tissues usually kept turgid by an abundant supply of lymph, *e. g.*, the Pacinian corpuscles. Therefore. although Meumann has proposed the chemical processes or products as the appropriate stimuli of internal sensation, and Kröner has posited an indefinite number of organic products as the immediate agents of felt internal changes, we may side if we like with the physiologist Sherrington in the less fanciful belief that the internal sensory stimulation is effected largely by known mechanical agencies,[1] and that such chemical sensations as do occur are probably cases of 'inadequate' stimulation, analogous to the production of visual sensations through the galvanization of the optic nerve.

However this may be, the limits of sensibility may be defined about as follows. The activity of the internal glands, and of the long middle stretches of the digestive tract, are presumably normally non-sensory. Even in the deep tissues for the sensory character of which physiology or neurology offers

[1] Schaefer: *op. cit.*, 971-2.

some warrant, *i. e.*, certain muscle layers (in the stomach, diaphragm, and possibly the heart), certain serous membranes (dura mater, pericardium, pleuræ, peritoneum parietale), certain epithelial surfaces (bladder, stomach, etc.), the appropriate conditions of excitation are probably rare. In both painful and non-painful stimulation spatial summation is presumably important; for both there is undoubtedly a high areal limen. Further, repetition, prolongation, temporal summation, must play a considerable part, especially in pains, whether the stimulus be mechanical or chemical. Whether because of the absence of the differentiated endings and heightened sensitivity peculiar to the cutaneous and higher senses, or to the lack of development of a comparatively frictionless neurone chain (with few synapses) to the higher centres,[1] the liminal values of the sensory endings, and the sensitivity of the viscera to isolated stimulation, are undoubtedly higher and less acute than those of the skin.

3. *Specific energies in internal sensory nerves; the teleological argument.* The question whether the adequate stimuli of the internal organs differ for different endings, *e. g.*, whether there are specific hunger, fatigue or exhilaration stimuli and specialized sets of nerve endings to receive them, whether there is adaptation throughout to a single stimulus mode (mechanical) or whether the effective stimuli are all what is known as 'inadequate' or 'common' in the external pain system, is intimately associated with the problem of specific organic qualities. In the absence of agreement among physiologists, or of definite psychological evidence, we are thrown back upon the teleological argument, which has lately been advanced in favor of specialization. Meumann, for instance, maintains the probability of a great variety of qualities on the basis both of a chemical stimulation hypothesis and the wide variation in the chemical processes and functions of the different tissues,[2] and of the

---

[1] The speculations of certain writers who suggest that the neural equipment of the viscera represents (at least in its more central portions) an atrophying system, that we have here certain organic functions the stimuli of which were originally conscious, but with the development of higher sensory and functional interests, and the relegation of vegetative functions to subordinate centres, have dropped to the subliminal, while their neural mechanism has undergone a corresponding simplification, seem hardly convincing. Even if these stimuli or functions were originally conscious, they were probably never in relation with a central mechanism or supreme conscious centre, but represent rather originally autonomic and independent functions which are gradually pressing in and at once acquiring a certain influence among conscious processes, and submitting themselves to a certain influence from higher centres.

[2] Kröner's propaganda for a multitude of chemical stimuli adequate to excite the nervous system as a whole (centrally or peripher-

importance of differentiated qualities if discriminated consciousness of internal conditions is to arise and serve as a warning to the organism. While this sounds not unreasonable, the counter view, that the *sensory* stimuli in the internal organs are essentially alike in character, has a certain cogency of its own. If activity and the need for activity are the primary conditions sensed, this appropriate stimulation might conceivably be mechanical and homologous in all organs, *e. g.*, compression through muscle contraction, or tension and intercellular pressure, varying with vascular dilatation, or with the turgidity of the gland cells in a resting tissue. Further, in so far as Meumann's argument rests upon the need of differentiated qualities as the basis for the sensing and localizing of organic wants, we may suggest that the 'local sign' or individualizing factor of any visceral experience is not of necessity qualitative *sensu stricto*. It may take the form either of an intrinsic peculiarity in the grouping or sequence of the sensations, of an acquired local reference, or of the external associated adjustments in which the internal need finds partial, and reflex or instinctive expression (*e. g.*, the muscle contractions preceding vomiting, evacuation and the like).

The whole problem of specific nerve excitants and qualities comes out especially in the speculations as to the real nature of hunger.[1] Opinion differs as to whether the adequate excitation is diffused and general, operating mainly through the altered composition of the blood, or local and relative to the condition either of the mucosa or muscle coat of the stomach, or of both. Those who are unable to reconcile the conditions of its appearance with their own theories of internal excitation regard it as centrally excited;[2] the majority can explain its peculiarities only by reference to both peripheral and central sources.[3] Those who derive it from contraction of the muscle fibres in the walls of the stomach (Müller, Weber,

---

ally, and in its external or internal branches), and represented each by its individual conscious quality, stands in defiance not only of teleological argument and scientific 'parsimony', but also of all accepted views of nerve excitability and specialization.

[1] For the facts and theories cited see: J. Müller: *op. cit.*, I, 530; E. H. Weber: *op. cit.*, 145; A. Bain: Senses and Intellect, 129; H. Helmholtz: *op. cit.*, 48; A. Goldscheider: Gesammelte Abhandlungen, 1896, I, 46; Kröner: *op. cit.*, 59; W. Nikolai: Ueber die Entstehung d. Hungergefühls, 1892 (reviewed in Zeit. f. Psych. u. Phys., 1893, V. 358); H. Beaunis: Sensations internes, 24-35; Schaefer's Text-book of Physiology, II; C. S. Sherrington: 991-3 and E. H. Starling, 321; W. H. Howell: Text-book of Physiology, 1906, 267-8.

[2] Z. Oppenheimer: Physiologie des Gefühls, 1899, 36 ff.; E. Becher: Ueber Sensibilität d. inneren Organe, *Zeit. f. Psych. u. Phys.*, 1908, XLIX, 358.

[3] See Beaunis' discussion.

Bain) are inclined to identify it with the muscle (or pressure) quality proper; those who trace it to the mucosa (Külpe, Goldscheider, Sherrington) recognize the possibility of specific endings, specific stimulus, and sensation quality, though inclined to relate it closely to cutaneous sensation.

The physiological facts, drawn from observations in normal and abnormal cases (fistula, fever, etc.), from X-ray examinations and experiments upon animals (cutting of vagi or sympathetic, introduction of nutriment into the duodenum, or of non-nutritious material into the stomach, œsophagus, etc.), are briefly as follows. The stomach in a fasting or hungry animal is empty, anæmic, and at rest in a state of slight tonic contraction, the mucosa being thrown thereby into folds. The sensation which arises under these conditions is temporarily banished by the introduction of any bulky substance into the stomach, or by pressure from the outside; if normally satisfied, it passes over into the feeling of satiety. Further, if not appeased, hunger may after a time disappear of itself, usually to reappear later in intenser form; or its appearance may be considerably advanced or deferred by alteration in the meal hour and habits of the individual. Lastly, dogs evince signs of appetite with both vagi cut, and with the sympathetic connections intact or severed.[1]

Beaunis after a critical consideration of these facts is inclined to suspend judgment and attribute hunger to both central and peripheral causes, the latter to include the condition of the stomach, œsophagus, pharynx and masticatory muscles. Sherrington regards the tension of the mucous lining resulting from local alterations in the blood supply, or the intercellular pressure arising from the engorgement of the cells of the mucosa with digestive granules as all important. The explanation of Goldscheider seems, however, to do most justice to the gnawing, uneasy, fluctuating character of the sensation as usually described. Goldscheider explains it as an irregular pressure sensation analogous to that produced in a finger from which the blood supply has been partially cut off, and in which, owing to the local anæmia, the separate pulsations of the blood are vaguely and unpleasantly sensible. This or some similar use of the fact of anæmia (such as the chemical action on the nerve endings of a diminished or impoverished nerve supply) seems to the writer the most satisfactory; especially since the relief of hunger is always associated with conditions (active peristalsis, introduction of bulky substances, of water, or of

[1] Doubt may, however, be expressed as to whether in the latter case the feeling of hunger itself is present, or merely a co-ordinated eating reflex and its accompaniments.

certain irritants, or predigested substances), the immediate effect of which would probably be either local hyperæmia or a general alteration of the consistency of the blood.

While some writers suggest hypothetical products of oxidation in the muscle tissues or blood as the peculiar stimulus of hunger, no organic substance is known which when injected will produce the sensation. It would hardly seem necessary, therefore, after a survey of these facts, to assume a specific hunger apparatus (and quality) beyond the presence of sensory endings in the stomach so situated as to be especially exposed to mechanical or 'common' and 'inadequate' stimulation through drying or intercellular conditions in general. The consideration of hunger, therefore, adds little weight to the doctrine which would make the internal sensory nerves responsible only to differentiated (chemical?) stimuli. Their accessibility to 'inadequate' or 'common' stimulation or their uniform adaptation throughout the viscera to some form of mechanical excitation (compression?) would, for all we can see, be equally satisfactory.[1]

As to the biological sanction for the existence of visceral sensations (apart from the question of specific qualities), we must agree with Meumann that bodily feeling undoubtedly performs a function in securing concentration on the pressing organic needs of the moment,[2] even if, with Becher, we must admit that this function may be in large measure performed by sensations not 'organic' or 'visceral' in the strictest sense.

4. *Origin and differentiation of the organic contribution to emotion.* One of the principal objects in examining the physiological evidence is to determine the possible scientific basis for the conscious activity of some specific organ (heart, liver, kid-

---

[1] The writer was at first inclined (on the basis of personal observations) to identify the sense quality of hunger as muscular. Laboratory investigations have, however, shown that the quality usually characterized as 'muscular' is by no means peculiar to muscular tissue or contraction but may be obtained by the application of pressure over almost any surface or structure. In view of this fact and of certain others relating the appearance of hunger to the condition of the mucosa, and its absence in disease to pathological changes in the latter, it seems unnecessary to hold to a muscle theory pure and simple, based, as such a one must be, either upon some hypothetical chemical substance, or very doubtfully upon the quite moderate tonic contraction of the muscle coat, since the absence of active peristalsis antecedent to the beginning of digestion is established by modern physiology.

[2] Even the internal distress of slightly abnormal conditions (such as insufficient or injurious diet, unfavorable climate, etc.), usually cited as a positive disadvantage, may possibly, in the opinion of the writer, operate not unbeneficially in primitive conditions, by inducing a restlessness which may lead to change of habit or habitat, and thus possibly to amelioration of conditions.

ney, or the like) in association with some particular emotion—grief, anger, fear, etc.[1] With the data of physiology and of immediate experience at our command, the direct effect of mental processes upon the great reflexly or semi-reflexly governed organic mechanisms, along with the subsidiary effects upon the glands, capillaries, and unstriped muscle fibres of the skin may be roughly sketched.[2]

Of these no doubt the larger number appear primarily as changes in the external muscle or cutaneous system, as alterations in the activity or tonic contraction due to increase or decrease in innervation of the large body or respiratory muscles, or of the unstriped muscle layer of the skin (gooseflesh, etc.). Others, representing primarily internal changes (vaso-motor?), may nevertheless come to consciousness only through their effect upon the sensory nerves of the cutaneous or body wall system (as tingling, temperature, shudder, etc.). On the other hand, changes in the great body muscles may have a secondary sensory effect upon the serous linings or viscera, while a certain fraction of the bodily response undoubtedly finds expression in the direct inhibition or reinforcing of the activity of the latter.

In general, mental events, acting now as stimulants, now as depressants, upon this centre and that, may give rise to the following bodily changes and probable sources of sensation. In the respiratory system, to violent expulsion or suspension of the breath, and frequently prolonged contraction of certain of the expiratory muscle groups, thoracic and abdominal, involving compression of the viscera, or pressure upon the serous linings of the two cavities, along with altered conditions of cardiac activity; or to increased capacity or depth of the excursions of the diaphragm, and a resultant quickening of the activity of the affected viscera (thoracic especially), along with various remote effects from the increased rate of oxidation in the tissues, heightening of innervation, etc. In the heart, to inhibitory action, followed by increased dilatation in diastole and pressure on the pericardium; or to violent contraction, increased carotid pressure, choking sensation in the throat, etc. In the circulatory system at large, to constriction (possibly even painful) of the cutaneous arterioles, attended by dilatation in the cortical system and a corresponding rise of pressure on the sensory endings of the dura mater, possibly also by sensory effects due to the rise of pressure in the great arteries; to dila-

[1] An ancient doctrine, the modern version of which is perhaps to be ascribed to J. Müller.

[2] See C. Lange: Ueber Gemüthsbewegungen, 1887; A. Mosso: Fear; W. James: *op. cit.*, II, 478-9; Th. Ribot: Psychology of the Emotions, 1898.

tation, flushing, and changes of temperature in the cutaneous system; or to secondary vaso-motor effects (probably due also to direct secretory reflexes) upon the lachrymal, salivary, urinary, and sweat secretions, along with the sensations not improbably resulting from disturbance of each of these systems.

Further into these hypothetical changes, physical and conscious (and their possible classification as purposive or merely irradiatory phenomena, resulting from casual overflow of stimulation in sensory or motor centres), we need not here go. It is noteworthy, however, that physiological data so far fail to suggest any basis for strongly differentiated reactions, involving entirely different organs in isolation, in the case of different emotions or feelings. As Féré and others have pointed out, bodily effects are classifiable mainly as sthenic and asthenic, as furthering or retarding the vital functions *en masse*. It is, of course, quite possible that though differentiated merely as stimulant, depressant, and possibly suspensory in action, the various affective states in virtue purely of a difference in the intensity of their inhibitory or excitatory powers, may call into activity a different range of organs according to the susceptibility or exposure of the latter, and thus give rise to a conscious differentiation. In general, however, while heart sensations (cardiac oppression) may doubtfully be classed as characteristic of depressing emotions; respiratory (or general?) sensations, of exhilarating; and general innervation sensation, of the intermediate exciting and soothing emotions, the basis for any finer differentiation of the internal response is indecipherable in the present state of our knowledge.

C. Clinical and Pathological Evidence.

From the surgeons and experimenters who antedated the general use of anæsthetics (Haller, Bichat, etc.) has come down the doctrine that the internal tissues are insensitive except in case of disease or inflammation.[1] On the limited significance attaching to an insensitivity based on data so restricted we have already commented. The question might indeed be dismissed for the present on the ground that men such as Helmholtz, Müller, Beaunis, Sherrington and d'Allonnes freely admit the normal presence in consciousness of a variety of sensations from the internal organs. Since, however, the dividing line between internal and external sensation has never been accurately established, the possibility of error or illusion in the identification or observation of the former must be admitted. As a matter of fact, the interpretations which certain

[1] *Cf.* E. A. Schaefer's Textbook of Physiology, Vol. II, pp. 970-71; and W. Nagel, Handbuch der Physiologie des Menschen, III, p. 699.

surgeons and clinicians have placed upon the insensitivity of the internal organs in operation contain important implications not only as to the plausibility of a plurality of organic qualities but further as to the trustworthiness and feasibility of observations on organic sensations in general. A closer scrutiny of the facts and hypotheses of this group of workers seems therefore desirable.

The work of the three surgeons (Lennander, Head, and Mackenzie) which we shall here examine deals exclusively with pain; and, further, with the pain of abnormal or diseased conditions. Indeed, the main problem grappled is just the reconciliation of the occurrence of pain in disease with the (assumed) insensitivity of the viscera in health. If the normal afferent impulses are merely reflex in function, possessing no means of access to the higher centres, if in health the internal tissues are proof against all ordinary mechanical, thermal, electrical or chemical stimuli, what can be the conditions of stimulation, what the nervous mechanism by which in case of disease or inflammation a pain excitation is propagated to the cortex? How explain the development or persistence of a pain mechanism or pathway which functions rarely, perhaps never, in the lifetime of the individual?

Certain physiologists and psychologists[1] seem inclined to find the solution of the riddle in the doctrine of semi- or sub-conscious organic sensation always with us but pushing into the focus of consciousness only when greatly intensified, as pain.[2] The Swedish surgeon Lennander,[3] however, scorns any such subterfuge and cuts the Gordian knot by practically repudiating the possibility of any visceral sensation whatever, in health or disease. The data on which this bold solution of the dilemma is based are the result of an extensive and varied experience with surgical cases in which a local anæsthetic only was used. The observations cited cover the testing of a large number of the abdominal viscera, in a state both of health and disease, of rest and of violent contraction. No sensitivity was in any case encountered, *unless* the stimuli employed affected the parietal peritoneum either directly or indirectly. The

---

[1]M. Foster: Textbook of Physiology, IV, 284; and G. T. Ladd: Psychology Descriptive and Explanatory, pp. 175-6. Ladd, however, is not immediately concerned with the problem of pain, but rather with the 'feeling' function of the sympathetic. See also R. Lagerborg: Das Gefühlsproblem, for the theory that only change is sensed.

[2]Such an hypothesis assumes, of course, that conditions requisite for the production of pain are inherent in any sensory mechanism, ignores the need of a specific set of pain nerves, or a special pain pathway.

[3]K. G. Lennander: Beobachtungen über die Sensibiltät in der Bauchhöhle, *Mitteilungen aus den Grenzgebieten der Medizin und Chirurgie*, 1902, X, p. 38. See also references in Nagel, *op. cit.*, p. 700.

latter responds with pain to light contact or palpation as well as to stretching, and one case at least is noted in which the patient was able roughly to locate the sensation thus produced as upon the right. Neither pain, touch, nor temperature sensations, Lennander therefore concludes, are ever initiated in the nervous tissue of the viscera proper: the sympathetic fibres which supply the internal organs are vegetative or reflex in function merely.[1] Direct stimulation of the cerebro-spinal fibres in non-visceral tissues is requisite to the production of sensation. The pain sensations of operation or inflammation which are usually attributed to the internal organs arise really from the accidental stretching or infection of the parietal peritoneum, pleura, or diaphragm (through the excessive peristaltic action in the adherent viscera, or the spread of noxious chemical products from inflamed and closely associated tissues), and the resulting stimulation of nerve fibres (intercostal, lumbar or sacral, etc.) running in or immediately beneath the structures mentioned. The findings of other authorities[2] are believed by Lennander to be in general reconcilable with this hypothesis. Many surgeons have noted the sensitivity of the parietal peritoneum, while most assertions of the sensitivity of the viscera in disease are matters of inference merely. The classic citation of the insensitivity of the parietal peritoneum rests merely upon a discredited eighteenth century experiment of Haller's upon animals, in which the structureless and nerveless character of the pertioneum is assumed from the start and no attempt made to keep the nerve connections intact in preparing for the experiment.

In criticism of Lennander's sweeping rejection of the possibility of any pain, pressure or temperature sensation from the viscera proper, Meumann in a recent article has raised the following objections: the limitation of the observations to experimentation with artificial and inadequate stimuli, and the unfavorability of the conditions to detailed and accurate observations, *i. e*,, the improbability that under the excitement of operation any sensation short of severe pain could have made its way to the perception of the subject.[3] With regard to the inadequacy of the stimulation we have already raised a

---

[1] *Op. cit.*, p. 104.

[2] *Op. cit.*, pp. 50 ff. For corroboratory evidence, see also J. F. Mitchell, Local Anæsthesia and General Surgery, *Journal of the American Medical Association*, XLIX, 3, 1907, p. 198.

[3] E. Meumann: Zur Frage der Sensibilität der inneren Organe, *Archiv für d. gesam. Psychologie*, IX, 1907, p. 26.

He also places the weight of certain *a priori* arguments, and of his own observations and introspections, in favor of the probability of a varied continuum of sensations from the internal organs, as already noted.

further point. The rôle of summation in internal excitation, recognized by the physiologists but totally ignored in the inferences of Lennander, may in part explain why such stimuli as stretching, or intensive contraction of the muscular wall of the alimentary canal, which we have every reason to think represents a normal source of sensation or stimulation, should be as ineffective as cutting, contact, and thermal applications.

In attempting to evaluate Lennander's conclusions, it may be useful to point out the two sources of his bias in favor of the total insensitivity of the viscera: the insensitivity in surgical operations to pain of all tissues (so far as tested) except those immediately associated with the body wall and diaphragm; and a prepossession in favor of the co-ordination of sensory function with cerebro-spinal nerves. With regard to the latter, it is noteworthy that in acknowledging its influence on his choice of the subserosa (which is innervated from spinal nerves) as the sensory tissue of the abdomen, he apparently ignores the cerebro-spinal fibres which Langley and others have traced into the viscera along sympathetic pathways. As to his results in general, regret must be expressed that more careful records of the apparent localization of the pain following the stimulation of different parts of the peritoneum was not kept. Finally, we may again suggest that even though much of the pain of disease, and probably the greater part of that incident to surgical operation, arises in the fashion prescribed by Lennander, this admission leaves the problem of the actual seat of origin of the various internally referred experiences of normal life practically untouched.

The position of Mackenzie is only in small measure less extreme than that of Lennander. According to the former, all internal reference of sensation is an illusion, an error of judgment; the body wall (muscle, skin, and subserosa) alone is sensitive. The afferent pathways of the viscera do not extend to the cortex. In so-called visceral pain, the nerve impulses may indeed be initiated in afferent fibrils of the unstriated muscles of the internal organs (never in mucosa or serosa even in case of disease or inflammation), but they attain to consciousness only by transference to cerebro-spinal pathways and centres belonging properly to the body wall. This conclusion is based in part upon the static character of the pain referred to certain organs known continually to undergo changes of position, such as the heart and stomach, in part upon the discrepancy found to exist between the limits of the spontaneous pain of disease (as defined by palpation in diagnosis) and the limits of the diseased organ or tissue itself as revealed in operation. In view of these facts and the marked sensitivity or hyperalgesia (as tested by pinching between the fingers) of the body wall over the affected organ or thereabouts, he assumes that 'visceral' pain is mediated by irradiation from afferent neurones of the viscera to sensory neurones supplying fibres to the skin, muscles or subserosa[1] from the same segment of the cord, and is accordingly referred to the same general region of distribution as the latter.[2] The superficial tenderness of disease is merely another phase of this irradiatory action and is indicative of the hyperexcitability to which the neurones of the external system have been raised by continual irritation from visceral fibres.[3] That is, Mackenzie reconciles the insensitivity of the viscera during surgical manipulation with the apparent sensitivity of diseased organs in diagnostic handling by declaring that in the latter instance only somatic (body wall) nerves are affected. Afferent impulses capable of terminating in pain can apparently be excited in the viscera by no artificial means unless it be stretching. Their adequate stimulus is prolonged or excessive contraction.[4] The sensitivity of the serous membrane is denied on the basis both of clinical cases and of the poverty of nerve endings in that tissue, a dictum apparently widely at variance with that of Lennander. Since, however, in the second article quoted, Mackenzie admits the sensitivity of the subserosa along with the skin and muscle of the body wall, his earlier statement is not necessarily unreconcilable with Lennander's insistence on the pain function of the parietal peritoneum.

On the purely afferent and non-sensory function of the sympathetic fibres Mackenzie is in his way as dogmatic as Lennander. Pain is a function of cerebro-spinal nerves only. It is always 'somatic', never 'splanchnic', as its reference in any given case to the general area of distribution of the cerebro-spinal nerves which would conceivably be affected through irradiation bears witness. Its internal reference, when this

[1] Mackenzie: Meaning and Mechanism of Visceral Pain, *Brit. Med. Jour.*, I, 1906, pp. 1449-54, 1523-28.

[2] This explanation of the nature and mechanism of 'referred' or transposed pain is really a development of the irradiation hypothesis for 'sympathetic' pain put forward in the rough by J. Müller (Elements of Physiology, tr. by Baly, 1837, Vol. I, p. 723), and first definitely applied by J. Ross (The Segmental Distribution of Sensory Disorders, *Brain*, X, 1887, p. 333).

[3] It is interesting to note in this connection that the fact of hyperalgesia is apparently reconcilable only with the theory which accords a common peripheral apparatus to touch (or temperature) and pain, accounting for the latter by the hypothesis of a secondary high resistance conduction path in the gray matter of the cord. The doctrine of specific pain endings with high limen of stimulation would apparently offer no basis for this lowered sensitivity.

[4] *Brain*, XXV, 1902, p. 368.

takes place, is essentially illusory.[1] The adaptive or purposive nature of this device (*i. e.*, the transfer to body neurones and the substitution of body wall hyperalgesia or pain for visceral pain) he argues from the fact that the hardening of a portion of the body wall, reflexly effected in consequence, undoubtedly serves as a protection to the affected tissues situated actually or approximately beneath.[2]

Such a theory, denying as it does the existence of any private pathways from viscera to cortex, with central stations of their own, would, if accepted, cut away the ground from the hypothesis of specific organic qualities, hunger, nausea, suffocation and the like. Even though the transfer of non-painful as well as painful impulses were conceded these must, according to accepted theories, appear in the qualitative guise peculiar to the borrowed paths or neurones.[3] Several points may, however, be raised against the unconditional acceptance of Mackenzie's line of argument. First, in denying sensory functions to the sympathetic on the score of the fewness of its fibres in proportion to the bulk of the viscera, and the other and varied functions which these must subserve in the vegetative economy of the organism, he ignores both the possibilities of intensification through spatial summation provided by the sympathetic, and the presence throughout the system of cerebro-spinal fibres. Secondly, the peculiarities of local reference adduced in support of his conclusions are in the main handled on the basis of a psychology of space too naïve and ambiguous to be convincing. For example, in proceeding

[1] A curious instance of an illusion which represents the objective if not the immediate subjective truth. The seat of the disturbance is pretty correctly sensed, but the significance of the route by which it reaches the cortex is ignored. It may, moreover, be suggested that the secondary pain path in the cord which Mackenzie evidently assumes may just as well be conceived of as belonging to the internal as the external system, and it may be regarded as only more or less of an accident that the local signs of the external system customarily adhere to its stimulation. In this case the insistence on the illusion of internal reference loses much of its force.

[2] The displacement of the viscera from their original segmental position in the course of mammalian development is called upon to account for the occasional discrepancy between the position of the diseased organ and the localization of the pain or the section of the wall contracted.

[3] The query might be raised in this connection as to why leakage to temperature and pressure as well as pain neurones does not take place in the cord, *i. e.*, why visceral disturbance is not sometimes sensed as heat, cold, or temperature. Mackenzie would, however, probably answer that every sensory neurone has a high resistance gray matter conduction path correlated with pain, and that impulses intensive enough to escape from visceral to body wall neurones are also intensive enough to effect the passage of this path, to the prejudice or swamping of the other possible sensations.

from the fact that the abnormal position of an organ is not correctly sensed to the conclusion that the seat of all sensation must therefore be in the stationary body wall, he ignores the customary mechanism of localization, *via* associations, visual, motor or the like, based upon the accumulated data of experience, and holding good for normal or typical cases only. The localization of pain in a constant position with reference to the body wall, though the actual situs of the organ excited may have altered, is a phenomenon resting conceivably upon the same basis as the reference to an amputated limb of sensations arising from the excitation of the nerves of the stump. Further, in the case either of stationary or moving organs a certain amount of vagueness or inaccuracy must always be allowed for in view of the peculiar conditions and the lack of incentive to the establishment of exact processes of localization in the viscera.[1] Mackenzie, however, habitually speaks of space perception in terms of immediate intuition. Again, the reference of internal sensation toward the anterior body wall, which is indeed commonly noted,[2] and which might appear to support Mackenzie's thesis, may be often merely a matter of the attraction of the sensation to be localized outward, toward visualizable or familiar boundaries, rather than inward toward an unexplored third dimension. Moreover, the fact of body wall hyperalgesia or pain is in itself no argument against the simultaneous presence of true visceral sensation, as indeed Head himself virtually admits. Lastly, no positive evidence against the direct conduction of sensory impulses to the cortex from the viscera by visceral neurones is adduced by Mackenzie. This point is merely inferred from the assumed incapacity of the sympathetic for pain.

Head,[3] in his article on visceral disease, occupies a less radical position than either Lennander or Mackenzie, admitting as he does that the viscera are veritably sensitive, mucous, muscular, and serous surfaces alike. Pain both originates and may be felt therein without involving an 'error of judgment'.

---

[1] This same criticism does not, of course, apply to Mackenzie's citation of the decided regional separation of reference in the pain from the large and small intestines (*op. cit.*). If his statement that pain from the large intestine is confined to the region below the umbilical area, while pain from the small intestine is customarily referred higher, is sustained by the observations of others, some extra-visceral explanation of the severe pain of visceral disease must evidently be sought, whether in terms of his own system or of that of Lennander or Head.

[2] This peculiarity has, as a matter of fact, been noted by both Helmholtz (Tatsachen in der Wahrnehmung, p. 48) and Meumann (*op. cit.*, p. 52).

[3] H. Head: On Disturbances of Sensation with Especial Reference to the Pain of Visceral Disease, *Brain*, 1893, XVI, 1-333.

Such pain is, however, of the dull, diffuse, vaguely localized variety, akin in every way to the 'protopathic' type of cutaneous sensation which Head in his article on the injury of the nerves of the arm has separated out from the sharply defined and located 'epicritic' sensitivity of the same region. For this dull internally referred sensation of the viscera either sympathetic endings or cerebro-spinal fibres terminating in Pacini corpuscles are responsible,[1] and in its mediation presumably only visceral neurones are concerned. The great bulk of visceral pain is, however, of the sharp, stabbing, clearly defined variety, and is felt in the cutaneous region innervated from that segment of the central system from which the nerve supply of the organ affected is also derived, owing to the transfer of the afferent impulse from visceral to cutaneous neurones in the cord. In support of this conclusion, which in itself, of course, represents little new, he cites (and here lies his importance) a tremendous system of evidence drawn from detailed mapping of the areas both of tenderness (hyperalgesia) and of severe pain attending affections of the various viscera, together with similar observations upon the skin areas affected in herpes zoster at different levels of the cord. The observations for both herpes zoster and hyperalgesia show irradiation in non-overlapping bands around the body; peritoneal and serous affections alone constitute an exception to the rule. Further, the points or areas of severe pain lie usually in the corresponding hyperalgesic regions, and frequently at some distance from the tissue diseased.[2]

In this theory, as in the doctrine of Mackenzie, the irradiation of nerve impulses posited offers no especial difficulties. Its operation would indeed be essentially similar to that of the mechanism assumed in the ordinary theory of a gray matter conduction path for the pain of the external system. The difference is mainly that where the latter assumes for any peripheral neurone a direct route for non-painful impulses, an indirect one for pain, the hypothesis of Head practically maintains two pain paths to the cortex, a primary private path, and a secondary one borrowed from the external or cutaneous sys-

---

[1] The Afferent System from a New Aspect, *Brain*, 1905, XXVIII, 99-113.

[2] The facts that the areas mapped are not appreciably overlapping, and that gaps in cutaneous reference correspond precisely to the regions supplied by the brachial and lumbar sections of the cord, which Langley has shown send no fibres to the viscera, are cited in favor of the hypothesis that the irradiation takes place in the *cord* and not in the spinal roots, each of which may be derived from several adjacent segments of the cord and which represent irregular and overlapping peripheral fields.

tem.[1] The external secondary path theory assumes either that the centripetal impulse expends itself in forcing the high resistance path and in the production of pain, or that the excitation travelling along the primary pathway reaches consciousness also, and is sensed in its appropriate terms, although frequently swamped by the pain component approaching along the secondary path. This second alternative is evidently that adopted by Head for visceral sensation. He does not actually deny the existence of a true visceral pain simultaneously present and parallel with the sharp 'referred' skin pain, but this if present blends with the latter, is *felt* in the skin. That is, there is not only a physical transference of visceral impulses, and a corresponding projection, but also a psychical misreference of true visceral pain.[2] This peculiar state of affairs is, however, quite adequately explained by Head as analogous to the allocheiria which arises in the external system under certain abnormal conditions entailing reduced sensitivity of certain regions, and which is definable as the reference of a sensation to a more sensitive asscciated area (adjacent or symmetrical). The terms in which Head phrases the application of this principle to visceral pain are somewhat obscure, but the process itself seems readily explicable on the ground that the cutaneous nerve impulse or corresponding cortical end process commands a more definitely organized body of assimilating and localizing associates, and, further, possesses a more efficient directive power over these associates (in virtue perhaps of its intrinsic energy or dynamic intensity), than the feebler nerve processes in the organic channels proper. Hence in cases of relatively intense visceral excitation with irradiation into cerebro-spinal channels, the perceptive associates controlled by cutaneous neurone processes swamp those of the less vigorous organic components which gave them origin. In the words of Head, the 'diffusion area' is accepted by consciousness by an 'error of judgment'.

If we grant, as we must, the essential reasonableness of Head's hypothesis, the desirability of harmonizing his observations with those of Lennander at once arises. Unfortunately, however, the two sets of data are taken from too widely different points of view to be readily comparable. In the absence among Lennander's observations of any detailed correlation of

[1] This peculiarity apparently arises from the fact that Head favors specific pain nerves rather than a secondary path theory for both the external and the internal system (*op. cit.*).

[2] That is, Head's statement seems to indicate that it is the corresponding localizing associations or cortical processes rather than the visceral pain itself which fail to rise above the limen of consciousness when accompanied by referred pain. *Cf.* Disturbances of Sensation, *Brain*, 1893, XVI, 124-6.

the objective and subjective sites of disturbance, *i. e.*, of the precise location of the portion of the peritoneum affected and the localization of the accompanying pain (by the patient), the weight of authority must rest with the elaborately systematized and coherent results of Head. The likelihood that if Lennander's denial of the sensory functions (direct and indirect) of the sympathetic system were accepted, the subserous parietal neurones could be substituted for visceral proper as the source of irradiation to cutaneous neurones, without doing violence to the system of facts accumulated by Head seems slight. If Head's referred pain of the hyper-sensitive bands were to be ascribed to stimulation of the intercostal or lumbar fibres running in or beneath the parietal peritoneum (to supply the body wall) and external reference of the corresponding excitation through either irradiation, or peripheral projection, it would be difficult to explain such facts as the reference of visceral pain higher or lower according as the cardiac or pyloric end of the stomach is affected, since presumably in either case the entire gastric connection would be concerned. This difference in reference might of course find a basis (in terms of the theory of Lennander) in the probability that affections of the cardiac end of the stomach involve the peritoneal connections with the diaphragm, affections of the pyloric end, the lower and dorsal mesenteric attachments of the duodenum. Similarly, Mackenzie's instance of the higher and lower reference of pain according as it originates in the small or large intestine might be explained by recourse to sites of the corresponding peritoneal connections—mesenteric in the former case, lower caecal or sigmoid flexure attachments in the latter. So far as the bandlike hyperalgesic areas are concerned, however, these according to Head could never be assigned to peritoneal stimulation since the pain arising from the parietal peritoneum (*e. g.*, in cases of inflammation) is never referred (*i. e.*, never transferred through irradiation to cutaneous neurones), and is irregular, not bandlike, in its manifestation; that is, such diffusion of sensation as does occur is a matter not of irradiation but of peripheral projection of sensations resulting from the stimulation of fibres somewhere on their centripetal course. It seems on the whole, then, probable that while Lennander's explanation is valid for certain active and griping pains such as those of colic, Head's holds for certain other semi-pathological hyperalgesic or referred pains. At the same time the possibility of genuine resident pain or normal sensation in the viscera is not disposed of.

This brings us at once to the question of the bearing of the hypotheses of the pathologists upon the problem of the localization and sensing of normal sensation (if this may be granted to exist). What explanation, for instance, does each system make possible of the off-hand local reference of unpainful as well as painful organic sensation, what possibilities of illusion in regional localization as well as in inward reference does each reveal?

The popular notion of localization is of course that the seat of hunger, nausea, or internal pain is either immediately and intuitively sensed, or may be ascertained by means of exploratory pressure from without. The important rôle, however, which such suggestion,and an atomical or physical knowledge may here play (see for instance the wide divergences in the localization of hunger in the case of the educated and the uneducated reported by Beaunis) and the uncertainty and instability in the mechanism of localization thereby indicated would seem too great to warrant the application of any nativistic doctrine. With regard to the second alternative, localization by means of palpation is indeed abstractly conceivable, if external pressure be admitted as effective in either setting up, diminishing, enhancing or putting an end to the particular sensations concerned, as immediate experience would seem to affirm, and as indeed Meumann has assumed, without any apparent sense of the contradiction involved with his principle that functional changes alone are the adequate stimuli of sensation. Or, localization might be empirically effected by evoking certain definitely localized muscle or skin sensations which in previous experience have become so closely associated with or assimilated to the internal sensations in question as to represent their local sign, as Helmholtz suggests.[1] In either case, however, we find ourselves immediately involved in the necessity of reconciling the setting up of said alterations in internal sensation and the formation of associative local signs out of external sense material, with the pathologists' doctrine of the insensitivity of the viscera to all mechanical stimulation.

Irrespective of any theoretical limitation of the sensitivity of the viscera, the following possibilities may be suggested. First, while there are indications that pressure does not represent the adequate *pain* stimulus of the viscera, it may nevertheless excite certain of their nerve endings to a sensible if feeble and non-painful action,[2] as it undoubtedly does those of the parietal peritoneum. Apart, however, from this possibility of direct action, it seems reasonable to assume, whatever theory of the extent of the sensory and non-sensory tissues we

---

[1] *Op. cit.*, pp., 47-9.

[2] If as Sherrington maintains (see preceding section) the adequate internal stimuli are largely mechanical (in the form of compression, not contact) there is a bare possibility that in certain cases pressure from without may be feebly if not painfully effective.

may entertain, that such mechanical stimulation affects certain of the *afferent* endings of the viscera, and may thus reflexly, under favorable conditions, through the resulting changes in visceral muscles, evoke sensations. Even more probable is it that through the displacement of gaseous or fluid contents external pressure may produce alterations in visceral distension, and thus give rise to sensation either directly through the stretching of afferent visceral fibres or increased parietal pressure, or indirectly through the compression of the resident endings, or the stretching of subserosal parietal nerves, attendant upon reflexly increased vsceral activity. Secondly, with regard to localization through associated sensations or movements. It is, for example, possible that the discharge of the respiratory function may in certain cases be effective in altering sensation, either directly and mechanically through displacement and pressure (in some such way as that suggested above), or indirectly and remotely through the metabolic changes which it induces in lung and other tissues. The external or kinæsthetic sensations thus associated to certain of the internal sensations may come to act as the 'local sign' of the latter and exert a certain directive power over their localization even when arising from different causes. Thus intestinal pain, nausea, and hunger may conceivably get their general reference from the external sensations representative of diaphragm contraction or abdominal wall distension or contraction; the feelings of stuffiness or exhilaration their thoracic or cephalic reference through association with chest muscle contractions, or with the constrictions or relaxations of the nasal passages and respiratory entrances. The possibility that the sensations thus associated may represent conditions standing not at all in causal relations, *e. g.*, in the association of sensations due ultimately to general circulatory changes with respiratory movements, of course arises. Further, even where a direct causal connection exists there may be a considerable discrepancy between the actual seat or origin of the sensation and the locality indicated by the external localizing movement. These points come out more strikingly when the localization of pain is examined from the point of view of each of the theories under discussion.

Take, for instance, the theory of Lennander that the peritoneum parietale, pleuræ, etc., are alone sensitive. What becomes of the apparent spatiality of organic sensation, of the possibility of determining its seat in the organism by external palpation? In the first place, the so-called massiveness, or voluminousness, the triple dimensionality of organic sensation, is reduced to a hollow mockery. Further, though the parietal sheet of subserous tissue may be directly sensible to pressure and though every portion of this sheet accessible to such pressure from the outside might have its own local sign[1], yet the portion accessible, also, to stimulation from the viscera must be exceedingly small. Indeed, in the abdomen if not in the thorax it must be limited to precisely those tracts of the posterior and upper walls which can have no local sign so associated to them. Localization toward the front wall of the abdomen must then represent an illusory reference, to be explained either as representative of some such central irradiation and segmental reference as Head has figured in his theory, as the result of the peripheral projection of sensations resulting from stimulation (in the rear wall) of the trunks of intercostal and lumbar nerves, the endings of which, distributed mainly to the front of the body wall, lend their corresponding local sign to the resulting sensation, or in one of ways suggested in the preceding paragraph. In the latter case, the throwing forward may be merely a matter of association with the sensible activity of abdominal and thoracic muscles or diaphragm in respiration, with the pull of diaphragm contraction upon the anterior wall, or the increased pressure of the body against the clothing during inspiration. A basis for the formation of such associations is further afforded by the fact that the pain from the drag of the stomach on its peritoneal connections would conceivably be lightened by lowering the diaphragm, renewed again when it lifted, while in the case of painful pressures upon the peritoneum the opposite would hold. In any case, however, within the theory of Lennander only the most general reference of internal sensation would be possible. Correlation with the precise organ active in its origin would be little short of a miracle.[2]

For Mackenzie, the localization of internal sensation is simple enough. It is felt always in the body wall, in accordance with the principle of irradiation from splanchnic neurones and peripheral projection, each internal tissue having a definite correlation with some patch or portion of the external body wall. For him, also, exploratory pressure produces a sensory

---

[1] As a matter of fact Lennander leaves us very much in doubt as to whether he regards pain as the result of the stretching (or inadequate stimulation) of parietal nerves running to supply the muscles and skin, or whether he recognizes the presence of *bona fide* sensory endings sensitive to compression or chemical excitation under the epithelial plates of the peritoneal linings. Unfortunately, neurologists can as yet give us no precise information on such endings.

[2] In many of the possibilities suggested, however, to call the misreference in which the localization consists pure illusion would be the merest quibble, since the seat of the main physiological disturbance, if not of the immediate sensory excitation, would roughly coincide with the region of reference.

excitation in the somatic endings only, thereby reinforcing the local sign of the centripetal neurones already excited by internal irradiatory processes. In no case is localization trustworthy in any detail. Further, for Mackenzie as for Lennander, the third dimension is an illusion: the distinction between internally and externally initiated sensation is fairly blotted out.

For Head, the above likewise holds true, with, however, certain qualifications, since, according to him, the duller, weaker sensations of pain are actually localized in the third dimension, though somewhat vaguely. With regard to the possibility of applying his principle of referred pain to the localization of faint or unpainful sensations, an expedient suggested by Meumann, it must be noted here that Head himself does not claim that the localization, such as it is, of dull visceral sensation (for him, always pain) is determined by the same law as that of the sharp referred pain arising from the same endings. In dull pain, the excitation is by hypothesis too weak to effect irradiation; and further, a stronger excitation accompanied by sharp referred pain may conceivably never have been initiated in that particular tissue, thus precluding the formation of association with any somatic region. It is indeed difficult, if not impossible (without abandoning the principle of the insensitivity to mechanical stimulation of the internal organs, or modifying it in some such way as we have suggested), to conceive of any mechanism by which even vague localization of the duller phases of visceral sensation could be set up in experience. Head's own position in the matter seems to resolve itself into an intuitive or nativistic theory of organic (and tactual) space. Although in one passage he expressly states that localization is dependent upon association, his treatment of the concept of localization (in another connection) belies his words. He uses, for instance, the fact that at a certain stage in the regeneration of the fibres in an injured nerve trunk diffuse, indefinite reference of sensation alone occurs, as evidence for the classification of sensory nerves into a protopathic and epicritic set, representing different grades of localizability and possessing different rates of regeneration. This would seem to argue a failure to recognize the complex nature of localization, and the necessity for the gradual reinstatement of the processes disturbed by abnormal conditions before accurate reference can again arise. Much the same naïve attitude toward the psychology of localization and reference is apparent in the writings of Mackenzie, as we have already pointed out.

In summary, we may say that so far as the accuracy of regional reference of *pain* is concerned, it is for Head and

Mackenzie, and possibly also for Lennander, only a happy accident that the region of reference and the underlying seat of origin are in some measure and in certain cases coincident or adjacent. Further, with regard to the possibilities of accurate localization of unpainful sensation (if, indeed, this may be granted to exist) the above theories taken strictly offer little warrant. Even upon a more liberal sensory doctrine either of the tissues sensitive or of the adequacy of artificial mechanical stimuli, the probability is strong that localization (especially in the thoracic regions, where the application of direct mechanical stimuli is even more dubious than in the abdominal) means merely association with the local signs furnished by movements which, while often concomitant, are dubiously causal or relevant. It may indeed be that while almost every organic feeling is capable of an apparently definite local reference, such local reference most frequently is a matter of suggestion or inference merely, and is pure illusion so far as the actual position of the nerve endings stimulated is concerned. Again, with regard to internal reference and the third dimension, the insistence of Head and Mackenzie upon the actual external status of many pains usually regarded as internal suggests the probability that among unpainful as painful sensations many commonly rated as internal or visceral may actually originate somewhere in the external system; that no universally valid criterion of external and internal exists; and, lastly, that the separation of organic and external sensation in investigation is hardly practicable.

While the facts and theories of the pathologists, therefore, help us only a very little way in the study of the possible sources and kinds of organic sensation, they are valuable in demonstrating the need of a critical examination of all rough and ready modes of localization or identification of organic qualities, of the clues by which such localization is effected, and their relative validity. It is, moreover, to be hoped that with the extension of the use of local anæsthetics a growing body of available data bearing upon the localization of pain will supplement the introspective and experimental results obtainable under normal conditions.

Experimental

Our object here, as already stated, was to investigate the richness or poverty of the organic gamut; to determine the traits by which organic is differentiated from ordinary sensation; the possibility of singling out and localizing the organic qualities under the play of attention; and, lastly, to devise a fitting terminology for whatever qualitative strains might be found to characterize the different feeling attitudes, emotions, etc. The classic difficulties in the way of such a scheme have been already cited; the impossibility of isolated stimulation, shortcomings in the way of memory images and other aids to attentive examination and description, and the inaccessibility in laboratory research of the major emotional and vegetative complexes in which organic sensations presumably play their most striking part. Yet the resources of the laboratory are worth more careful examination. Certain comparatively simple or contracted feeling aspects are there at our command: surprise, expectation, recognition or familiarity, alarm, doubt, amusement, certainty. Further, various *Mitempfindungen* or minor organic reverberations are attendant upon simple sensory stimulations accessible, *e. g.*, in the bodily effects of certain tastes, odors, noises, and the like. In such compounds the occasional emergence of an individual organic quality through accidental changes, differences of intensity, etc., is not impossible. Further, by fractionating the observations so as to fix the attention now upon one part of the body, now upon another, and by facilitating description through furnishing the observer with an extended and standardized list of adjectives, it seemed probable that certain verbal clues and associations could be established by means of which various casual experiences such as hunger, fright, drowsiness, nausea, embarrassment, disappointment, regret, and other semi-emotional complexes might later be laid open to analysis and description.

Organic Attitudes. First of all, we devised a set of simple experiments with the Jastrow drop apparatus and a number of series of inkblot figures, names of well-known men, colors, and numerals (with a certain number of blanks interspersed in the series), so arranged as to provide the appropriate conditions for such mental reactions or attitudes as the imaginative, the recollective, expectancy, surprise, and recognition.

These preliminary experiments were pretty uniformly unsuccessful. The four observers tested failed to report any organic factors, beyond various externalized kinæsthetic strains and relaxations, and alterations in respiration, varying little for the different types of experiences. It was evident either that the bodily attitude of active attention and release therefrom is in laboratory experiments of this type so prominent as to swamp any possible specific organic characteristics of the several attitudes, or that some special development of the technique of observation upon physiological sensations was necessary. We, therefore, abandoned for the time the use of reactions to these comparatively complex stimulus situations, and turned to the study of the simpler, more striking and varied, and hence more readily describable physical reactions to simple sense stimuli of decided affective value.

Organic Reverberations. A number of tastes and odors were tested for their physiological effectiveness, and the following selected: quinine and sugar solutions, vinegar, lemon, sarsaparilla, laudanum, camphor, mace, ammonia, carbon bi-sulphide, listerine, castor oil, gasoline, asafœtida, ginger. These were submitted to the observer for periods of from five to twenty seconds (at considerable intervals), with the instruction to give himself up to the experience of the moment (*not* to the cognition of the stimulus), to get all the pleasantness or unpleasantness possible out of it, and then to report upon any concomitant organic reverberations, diffused or specific, superficial or apparently internal and deep-seated.

In general, all four observers reported with unpleasant stimuli a feeling of revulsion, traceable to the automatic contraction of the muscles of the mouth, throat, nasal or respiratory passages, and sometimes grading into a feeling of nausea. With pleasant stimulation, either a feeling of relaxation, general or localized in the muscles of the face, chest, or limbs; or a 'glow' felt in the head or referred vaguely to the respiratory apparatus, the activity of which seemed always appreciably involved. Sensations of nausea were referred usually four to twelve inches below the larynx, sometimes to the base of the tongue, and described by all observers as wavelike, muscular, uneasy. Pricking sensations on the tongue, in the nasal passages, or proceeding apparently from the salivary glands, were recorded following stimulation from vinegar, lemon, or ginger, or the whiffing of ammonia. In spite, however, of considerable variety and difference in reference (the stomach, œsophagus, respiratory passages, pharynx, salivary glands, etc., were all apparently at one time or another affected) there seemed to be no necessity for referring in description to any specific quality other than the familiar external categories, unless the mention of 'pricking', which seemed neither pain nor pressure precisely, be excepted.

Organic Mitempfindungen. A third set of experiments had for its object the study of certain borderline phenomena (not visceral, but presumably subcutaneous and distinguishable from pressure, contact, or ordinary muscle sensation), such as tingling, the 'creeping' or gooseflesh sensations, etc. Rasping and harsh auditory stimuli and rough textures were used here. For the sake of contrast a number of pleasing tones or chords were introduced into the series, and their organic accompaniments duly noted. A vocabulary of descriptive terms (mostly, it must be admitted, characterizations of merely temporal or spatial peculiarities), such as tingling, pricking, stinging, thrilly, aching, glowing, dull, sharp, massive, delicate, vague, creeping, and the like, were, after a few observations, supplied to the observer, and their significance later agreed upon. A very shrill whistle, a bowed Mi_5 fork, cymbals, discords, and a major triad on the König tuning forks were used.

While a considerable variety of organic reactions was thus secured, the introspections again failed to show the presence of any peculiar organic qualities. Among the mass of muscle contractions noted and localized in body wall, limbs, ear, or head region, visceral sensations proper were mentioned only twice, and then doubtfully, as the apparent concomitant of contraction of the abdominal wall muscles. Unpleasantness was sensed mainly as a checking or obstruction of respiration, a general muscular recoil or revulsion, or a positive (localized) pain, *e. g.*, in the ear. Pleasantness was again referred vaguely to the chest or respiratory region. Tingling or waves of shivering, 'creepy' sensations were frequently noted, but not further analyzed or qualified, and hardly internally referred or differentiated from cutaneous sensations.

By this time, then, it was pretty evident either that visceral sensations are lacking in other than strongly toned emotional complexes, or that they are far less easily discriminable from the ordinary external qualities than we had so far assumed. A series of casual introspections on a wide range of spontaneous organic complexes seemed to favor the latter conclusion. Accordingly, in a fourth set of experiments (still more or less crude and exploratory) we set out to work systematically from the known to the unknown, running the observer through a typical series of external sensations—pressure, pain, muscle, etc., and their simple variations—which should then serve as standards of comparison in dealing with the sensations following similar stimulation of the accessible mucosa (*i. e.*, of the tongue, and throat, œsophagus and stomach). Special attention was to be paid to the possible appearance of diverse kinds of cutaneous or subcutaneous and muscle pain; to the comparison of the effects of heat and cold upon mucosa and epidermis; to the analysis of itch, ache, tingle, smart, burn, and other of the simpler external complexes, with a view to the discovery of some new specific element (prick or tingle?) other than the certified pain and pressure qualities, representative, perhaps, of a subcutaneous or circulatory contribution, hence likely to afford a clue to deeper visceral sensations.

Comparison of the Direct Effects of External and Internal Stimulation. Experiments on heat-pain were carried through as follows. Several small areas on the forehead and arm were tested with heated temperature cylinders, the pointed and blunted ends of which were alternately used in order that in the latter case the appropriate conditions for the stimulation of both warm and cold spots, *i. e.*, for Alrutz' 'heat' fusion, might be present. The stimulus was applied from three to five seconds, allowing considerable intervals for recovery from fatigue. The following is a typical set of observations on the arm with the blunt end of the cylinder.

39° C. *a.* Warm; a little flash of heat toward end.
b. Heat at once, a prickly sharp point, not areal as when merely warm.
41° Warm, then suddenly hot, then warm again.
43° Heat at once, flickers, warmth between.
45° More like heat; never constant.
47° Hot, then pain. Heat flickered, warmth between, pain only with first flash of heat.
51° Heat, changes to pain, but not piercing.

From such observations as these, and further questioning, the fact came out to our surprise that the experience known as 'heat' to our observers is very slightly suggestive of a fusion of warmth and cold. The description of heat as a *point* without the *areal* feeling common to warm and cold spot stimulation, its delayed appearance, and flickering, fluctuating character suggest that the major feature is a weak pain, akin to prick, to 'pins and needles', to the sensations resulting from the application of a mustard plaster, or a weak interrupted current to the skin; of salt, pepper or ginger to the tongue.

For the direct comparison of heat and cold pain one finger was dipped into water heated to 65 or 75° C, another into ice water or the surface chilled by spraying with ether. Under these conditions heat and cold pain were found to differ, though hardly qualitatively. The former is more superficial, sharp, piercing, unsteady, throbbing out particularly strong just after the finger is removed from the water, and dying away into tingling, or soreness. The latter appears less abruptly, is deep, steady, massive, and spreading, especially to joints—wrist, elbow and shoulder—where it persists as a dull ache or strain sensation some time after the removal of the stimulus.

Our experiments were of course too roughly carried out to indicate whether the pain in either case represented over-stimulation of temperature spots (Bader, Wundt), direct action upon pain fibres proper (possibly at different depths for heat and cold pain respectively) or a secondary effect related to vaso-motor changes. In favor of the first hypothesis stands the fact that pain adaptation seems to follow temperature adaptations, *i. e.*, a cooled finger responds painfully to lower temperatures than it would under ordinary conditions. In favor of the second, especially in so far as it concerns the origin of heat pain in superficial, cold pain in deeper strata of pain fibres, stand the difference in depth of reference of the corresponding sensations, the customary sharpness of heat pain, the aching character of cold, and the frequent adapting out of the former, characteristics which tend to identify it with the superficial pain sensitivity to mechanical stimuli. Whether, however, the painful temperatures lie within the thermal limits of direct excitability of nerve fibres is a point which is apparently yet to be determined.

Experimentation with the swallowing of hot and cold water (55°C and ice water) revealed again the ambiguity of the term heat. The observer was at first inclined to locate a heat sensation (two or three seconds after swallowing) at the lower end of the œsophagus. After further study and observation of external temperature sensations, however, he usually reported merely a glow of warmth, appearing late and due probably to diffusion to the skin, or a certain momentary, radiating sensation, internally referred (at about the level of the stomach), bright and hardly distinguishable in the two cases of warm and cold stimulus, sometimes described merely as 'contact', sometimes as similar to his cutaneous experience of 'heat' or weak heat-pain.[1] This sensation was usually absent on repetition and may of course have represented a peritoneal or diaphragm, rather than an alimentary mucosa response.

For the comparison of different kinds of pain—itch, smart, burn, ache, etc.—the application of a mustard plaster to a small area on the arm, a sharp rap on the palm of the hand with a ruler, and heat and cold stimulation were employed. Typical introspections for the first refer to itching as made up of intermittent pain sensations, fusing now into a sharp sting referred to a single point on the skin,[2] now spreading and irregular as if hundreds of little bubbles were breaking through to the surface; followed by a suffused warmth coming in waves along with pain of increasing intensity until a fairly unanalyzable mass of heat and pain results. For the second: "Smart made up of prickly points; gets more steady, a general ache or throb; ache is below surface, large, of one piece, rounder edges than smart." Similar observations on heat and cold pain, and the use of ginger, pepper, vinegar, lemon, etc., on the tongue, bring out the fact that the observers feel no need of different

[1] It seems to us highly probable that in the infinitely more painstaking experiments of Becher (*loc. cit.*), the 'temperature' sensations ascribed to the œsophagus were really of the nature of the sensations described above, due to the direct effect of comparatively high and low temperatures respectively upon internal fibres.

[2] This punctiform reference was not tested, but judging from other observations probably signifies only that pain was steady and sharp, not intermittent or dull.

qualitative terms in distinguishing any of these sensation experiences, even when for the sake of more exact comparison they followed closely in succession. Temporal qualifications, such as are implied in the terms intermittent, pricking, tingling, or intensive and spatial differences, seemed still to afford sufficient distinction. It is, however, to be noted that in connection with stimuli affecting either the mucosa, epidermis, or salivary glands the isolated sensation described as 'prick' or 'sharp' caused the observer some hesitation. While it is in itself neither painful, nor yet apparently possessing anything in common with the dull, massive quality usnally identified with pressure, it stands in a sense between pain and pressure, since when intensified it passes gradually and insensibly into pain, and since also when an area of pricking sensations is allowed to die gradually away it merges into a massive dull sensation not dissimilar to pressure.

To sum up, we were left in considerable doubt as to the respective limits of the terms pain and pressure, the proper ranking of the pricking sensations, and their probable origin, whether in the skin or subcutaneous strata (in specific circulatory endings), in contractile or other changes in the walls of the smaller blood vessels. Further, we had so far failed to decipher any fixed qualitative variety among the pains of external origin which might serve as a guide or a clue in the analysis of internal pain. Lastly, a parallel series of casual introspections on visceral complexes indicated strongly that a thorough study of the external qualities and their different guises under the influence of intensive changes and reduplication, especially some investigation of the qualitative validity of the terms 'sharp' and 'dull' (which so far seemed to cover the most salient and distinguishing features of internally referred sensation), was a necessary preliminary to the successful analysis and description of the latter.

Significance of the terms 'sharp' and 'dull'; qualitative distinctions among cutaneous, subcutaneous and kinaesthetic sensations. A thorough study of certain variations of cutaneous qualities was accordingly undertaken, the results of which have already appeared in these pages, and may be summarized as follows. The typical simple cutaneous pressure (better *contact*) sensation, obtainable with moderate isolated stimulation of either pressure bulbs or 'pain' spots, is sharp or 'bright' in quality, and closely akin to the 'prick' which we had heretofore associated with stimulation by electric current and various irritants.[1] The granular pressure of Goldscheider is a

[1] XIX, 1908, 289 ff. A Qualitative Analysis of Tickling: Its Relation to Cutaneous and Organic Sensation.

complex, not a simple sensation, though qualitatively homogeneous. The ordinary dull pressure referred usually to lower strata, subcutaneous or muscular, appears under certain conditions of purely superficial stimulation of moderate intensity, as in the dying away of tickle. Tickle itself is not a peculiar organic quality, but represents a semi-analyzable pressure or contact complex and may be obtained directly from the isolated stimulation of a pressure bulb. The general bodily reaction conditioned by its irregular, flickering character, and strong dynamogenic power as an incompletely analyzed semi-cohesion of sensation probably accounts for the tendency to relate it to the organic or internal. Lastly, the dull pressure and the sharp prick above noted seemed with increase of intensity to pass into pain—dull ache in the former case, sting or sharp pain in the latter; the difference being probably a difference in massing and intensity merely. In a word, the results pointed to the possibility of reducing all the apparent multiplicity of sensation above cited, sharp and dull, to an astonishing qualitative simplicity.

Certain supplementary introspections combined with these experimental results, led to the formulation (in the above mentioned article) of the hypothesis that the whole muscle-skin continuum (barring pain, for the present) is qualitatively simple, and that its great apparent diversity is entirely explicable as a matter of 'form-color', or the peculiar temporal, spatial and intensive massing of similar sensations and the various resulting degrees of fusion, blurring, or flickering of the component parts.

In these introspections, of which unfortunately no considerable number was obtainable except from the writer, description was based as far as possible upon comparison with other more familiar or more easily designated sensations. The following descriptive summaries represent each from ten to twenty introspective studies made at odd moments for a period of about two years.

Gooseflesh. Arising from cutaneous stimulation—cold, the friction of clothing, nausea, rasping noises, and certain musical clangs. First, an apparently wave-like migratory sweep of sensation (after many repetitions analyzable into a multitude of vibratory or intermittent points), described at different times as pricking, seething, 'cork-screwy', 'whirring', bright, glowing, shadowy, 'shivery', or flickering, localized just under the skin, likened to a feeble faradization of the skin, to pins and needles, or to a light breeze over the hairs; later losing their sharpness and merging often into a drawn, tense feeling. Temperature sensations rare or feeble. After repeated analysis it seemed probable that the 'migratory' character at first noted was merely a matter of intermittence; careful introspection revealed no onward sweep of the sensation, though successive patches might appear. The curious, uncanny feeling-tone usually present seems to arise largely from the somewhat ghostly suggestion of movement in the absence of any visualizable stimulus. A similar complex of sensations, but much fainter, hardly sharp at all, fine, shadowy, arises when a current of steam or compressed air is allowed to strike the face: presumably from the same cause, the movement of the fine hairs.

Pins and needles; numbness; circulatory sensations. The first (from falling asleep of the arm) consists, as in the case of faradization of the skin, of tingling or pricking sensations, sometimes so rapid as to merge into a mere whir or even a dull, pressure-like sensation, usually described as 'numb'; when sharper, frequently shading into a muscle ache, or a feeling of iron tension, which seems to split up again into sharp, needle-like sensations, if the limb is moved or the muscles contracted. The application of turpentine to the skin gives rise at first to something very like a low-toned ache, but giving away rapidly to boiling, seething sensations which seem to get faster and sharper, but never painful so long as attended to; or to a decided pulling sensation (or tautness) as in gooseflesh, which, however, always vanishes with analytic attention, hence apparently represents an interpretation of a certain degree of fusion and not a peculiar quality to be ascribed to the plain muscle fibres of the skin, as some have supposed. The sensations attending excitation of the salivary glands through certain odors or tastes show the same peculiarities,—lively, vibratory sensations grading into 'strain'.

Muscle, strain and joint sensations. The former we found very difficult to identify under experimental conditions (anæsthetization of skin and galvanization of muscles). What we obtained was either a continuous dull ache, outlasting stimulation (very possibly related to the use of the ether on the skin), or a shooting, jerking, quivering line of sharp sensation associated apparently with the contraction of a smaller muscle. We, therefore, began to look among casual instances of sensations referred (rightly or wrongly) to muscle regions—to fatigue, drowsiness, relaxation, numbness, the shock of fright, of sudden waking, etc.,—for a clue to the peculiar quality usually ascribed to muscle sensations by experimentalists.

It was first necessary to throw out the strain sensations, which we found readily identifiable, easily excited by voluntary muscle contraction, and accurately localizable, either directly or by pinching. They grade imperceptibly into pain, and are easily matched by pressing firmly over any bone or muscle. The remainder of the sensations referred to the deeper strata (apart from the pricking sensations already ascribed to pilomotor or circulatory changes) comprise a certain dull, massive, ill-defined 'tridimensional' feeling indistinguishable from pressure and present sometimes as a sense of the hugeness of the limbs in fatigue, in drowsiness or after a heavy sleep (when the peripheral capillaries are dilated). This sensation, the voluminousness of which is perhaps a suggestion from its mere aggregate character as representative of the stimulation of innumerable endings, may be in part cutaneous, but is present characteristically with prolonged massage, long after the superficial contact sensations have adapted out. It seems sometimes to be peculiarly unsteady (though this may be merely the effect of a shifting attention), and grades imperceptibly into a dull ache.

Certain other sensations are referred to the muscles, though possibly arising from circulatory changes, contraction of sweat duct fibres, etc. These are bright rather than dull, and arise *en masse* when we receive a sudden physical or mental shock—are angry, hurt, moved to pity, suddenly awakened from a nightmare, and the like. They are frequently sharp, 'corkscrewy', and fade quickly; the feeling as if 'boiled to shreds' described by James is apparently identical.

While it is, of course, probable that many of the sensations above described may originate in peculiar nerve-endings—muscle, tendon, plain muscle, circulatory and the like—as it is certain that many of them originate in changes in the above-mentioned tissues, there is little warrant for assigning peculiar, individual qualities to such endings, since most of the characteristics of muscle, joint, skin, or tendon sensations may, under appropriate conditions, appear in the other tissues.

Varieties of Pain: dumpf and stechend, stabbing and cutting. With regard to this two-fold division, which is apparently accepted as ultimate and referred to different sensory systems (deep and superficial) by Nagel, Thunberg, Ebbinghaus, and probably Head, one or two points may be added. Pricking pain sensations may die away into a diffuse ache. A characteristic ache (deep, massive, and more or less dull) may, through changes in intensity or attention, swell out into a 'bright', almost sharp pain and die away again without otherwise altering in character, as embers may glow and dull again. It seems reasonable, therofore, to assume that here also the basis of division is merely a matter of spatial and intensive massing. Superficial nerve endings are, of course, more exposed to isolated and violent stimulation; deeper lying ones to massive and distributed attack.

Pain: The Concept of Indifferent Pain

Experimental. The immediate purpose here was to examine the nature of liminal and full-fledged pain sensations, their relation to the pressure (or contact) sense and its endings, and to determine whether this relation rises gradually along a graded series of stimulus intensities or springs suddenly out from painless sensation, as many assert. The results of these experiments, which were comparatively few and limited to the testing of pain spots with mechanical thermal and electric stimuli, have been already outlined in the article quoted. Two important facts disclosed were the existence of a graduated series of sensations ranging from vague indifferent punctiform pricks to pain (along a scale of graded stimulus intensities), and the uncertainty or instability of the pain judgment. Whether the same would hold for all pain endings or all stimulus forms we can hardly conjecture. With a fine pointed stimulus (needle) and a light momentary touch the response of a pain spot is frequently a tiny, *itchy*, irregular, apparently irradiating and delayed sensation, as under similar conditions the characteristic response of a pressure organ is tickle. In general, secondary or delayed pain (which at first we thought might represent the effect.of vaso-motor reflexes in deeper endings) was obtainable from any pain spot if lightly enough stimulated; these delayed sensations are, however, strictly speaking, not always painful. Lastly, with intenser stimulation pain was obtainable from any pressure bulb tested.

Introspections and conclusions. Observations on the stinging or prickling sensations felt in the skin with light twisting massage, in the nostrils just preceding a sneeze, in pins and needles, and many other cases already quoted have revealed sensations closely akin to the sensory aspect of pain, and yet unpainful, indifferent or even pleasant. Hence our inclination, already admitted, to credit the pain sense proper with a stretch of indifferent sensations in the direction of its lower limen, to regard the 'pain' quality as not alien to the pressure-contact-muscle-strain continuum, but simply representative of its upper stretches. It will, of course, be objected that in ordinary pain (*e. g.*, a needle prick) there is something peculiarly penetrating, peculiarly explosive, vivid, or stinging. Careful introspection, however, seems to resolve these peculiarities into a matter of abruptness of outswelling, of intermittence and apparent irradiation (in the case of itch), of complexity (in the case of tearing, wrenching pain), or into a matter of attentive vividness and general bodily reaction.

In cases of pricking or piercing sensations which seem to lie upon the border line, the observer, when doubtful, customarily basis his judgment upon his rebelliousness against the sensation. If now the natural reflex revulsion can be inhibited, or the sensory core singled out and dispassionately regarded by the attention, it temporarily loses its painful character and becomes merely an exceedingly vivid, live, sharply defined sense perception. Further, many sensations which, it momentary only are hardly painful, become so if continued, even when the sensation itself, if carefully noted, is neither intensified (by summation) nor otherwise altered. Neither is this a matter of an added diffuse unpleasantness, but rather of a complex dynamogenic effect. When the assault of any pain sensation which, if come upon unawares, would automatically grapple the attention and excite the impulse of avoidance or escape, is, as it were, headed off, the reaction inhibited, or some other substituted (as is possible only momentarily or not at all if the sensation is of considerable violence), the unbearableness, and with it the painfulness is lost.

It therefore seems to us that the pain consciousness represents neither a simple feeling or sensation,[1] nor a peculiar sense quality plus a simple and invariable unpleasant affective element, nor yet merely a peculiar sensation plus sometimes indifference, sometimes unpleasantness, but rather a peculiar miscellany or complex, an assimilation or welding of the immediate sense content (brilliantly illumined by attention) with a great bodily and mental reaction, the shorthand expression for which is 'intolerability'.

In taking this position we have, as above indicated, no intention of reverting to the earlier and cruder interpretation which,

---

[1]The *Gemeingefühlsempfindung* of the physiologists of the early nineteenth century; the *Gefühlsempfindung* of Stumpf.

confusing painfulness and disagreeableness, identified the pain consciousness as a peculiar 'feeling' and lightly posited the transmutation into pain of any specific sense quality or any specific nerve excitation—visual, auditory, temperature, or tactual. A concrete, specifically localizable aspect of pain is, we maintain, always present, and is sensory in the ordinary significance of the word.[1] Whether or not it is in many or all cases mediated by a specific end station and spinal pathway (*via* collateral connections in the cord), we are inclined to regard it as qualitatively alike in all senses and closely related to the indifferent prick.

In support of this concept of indifferent 'pain', certain theoretical considerations may be added. First, the facts of hyperalgesia and summation are hardly explicable upon the assumption of a high peripheral limen. Further, the concept enables us to account for the various sensations received from the viscera and inner tissues (painful, indifferent and pleasant) without a needless or difficultly intelligible duplication of nerve apparatus.

Summary. The net results of the above experimentation, so far as it bears upon the problems of organic sensation, may be summarized as follows:

1. The differentiation of external and internal sensation is less obvious, and the conditions of external or internal reference more complex than we at the outset imagined. A large number of internally referred sensations quite probably arise from the excitation of cutaneous nerves.

2. The texture or massing of sensation is as important in creating apparent qualitative differences (sharp and dull) as is the original sensory element itself.

3. Internal sensations may differ from external texturally rather than qualitatively, just as tickle differs from pressure merely from being so put together that it constitutes a 'feeling' rather than a sensation compact, is affectively vivid while perceptually vague.

4. The possibility of an indifferent beginning of the pain continuum offers a valuable suggestion for the solution of some of the problems of internal sensation.

---

[1]That is, we assume that intense visual or auditory stimuli can evoke the pain reaction only when attended by veritable 'pain' sensations, by whatever mechanism aroused.

NOTES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

I. Some Statistics on Synæsthesia

Collected by K. B. Rose

Two hundred and fifty-four women students, mostly from the Junior and Senior college classes, were asked to report upon any associations they possessed between colors or forms and letters, numbers, days of the week, months, and so on. It was found that 23, or a little over 9%, had color associations. Of these, 6 showed the phenomenon in a very striking degree; 7 in a moderate degree, and 10 in a slight degree. The order of colors arranged according to the frequency with which they entered into associations of this sort was, beginning with the most frequent: brown, yellow, gray, red, blue, green, pink, white, orange, violet, lavender. The colors were associated oftenest with letters, next oftenest with names of persons; then came names of cities, and lastly musical tones. Of the letters associated with colors 40% were vowels: since the number of consonants in the alphabet is about four times the number of vowels, this means a decided preponderance of vowels in color associations. The letter *o* was most frequently found in such associations: *a* was a close second, then came *e*, while *i* and *u* stood together as the vowels least often occurring in association with colors.

The number of persons having form-associations was 32, about 12% of the number questioned. In 27 cases the year was associated with a form, and in 22 of these the figure was that of an ellipse or circle, an obvious suggestion from diagrams of the earth's orbit such as are often found in geographies. In 21 cases the numbers from 1 to 10 suggested a form; in 16 cases the days of the week had this sort of association, and in two instances centuries had a figure associated to them.

II. An Instance of the Effect of Verbal Suggestion on Tactual Space Perception

Reported by M. F. Washburn

The observer in the experiments to be described was a young woman student of psychology, a good visualizer, and, according to her own statement, decidedly suggestible. The experimenters were the writer and Dr. Elsie Murray; probably the fact that they both, as members of the instructing staff, possessed prestige in the observer's mind, added to the effectiveness of their suggestions.

In the first set of experiments, the method was as follows. Rubber-tipped compass points, separated by a distance of 15 mm., were set down on the volar side of the observer's wrist, parallel to the long axis, and, after an interval of two seconds, set down again in the same region, being shifted only enough to avoid fatiguing the skin.

This procedure was repeated for ten experiments. The observer had been told beforehand that the points in the second of the two impressions would be either further apart or nearer together than in the first, and was asked to make the judgment 'larger' or 'smaller' with regard to the second distance. As a matter of fact, the same separation of the points was used throughout. Ten experiments made after this plan were followed by ten in which the observer was told that the second distance would be either larger than smaller than or equal to the first; the actual distances were kept constant as before. In a third series of ten, the conditions being otherwise the same, the observer was told that some of the second impressions would be of one point only. Eight complete sets, of thirty experiments each, were made. It was found that the judgments were distributed in the following way.

Suggestion	Judgment 'larger'	J. 'smaller'	J. 'same'	J. 'one'
Larger or smaller	28	42	5	5
L., s., or same	25	24	20	11
L., s., same or one,	14	16	26	24

The effect of suggestion is very striking.

In a second part of the investigation, the same suggestions were given, but instead of keeping the compass points at a constant distance apart, one point only was used in all the experiments. Ten complete sets of thirty experiments each were made. The following results were obtained:

Suggestion	J. 'larger'	J. 'smaller'	J. 'same'	J. 'one'
L. or s.	38	61	0	1
L., s., or same	23	42	33	2
L., s., same or one	22	34	28	16

Here the suggestion was still more efficacious. It is remarkable that the judgment 'one point' should have been given less often throughout this part of the investigation, where two points were never used, than in the first part, where one point was never used. After-images very likely played a part in producing the illusion of duality, although the skin was lightly rubbed between experiments to get rid of them. Twice, while the series where the suggestion 'Larger, smaller, same, or one' was being given, the observer expressed wonder that so few one-point impressions were given, although she was actually, of course, being given nothing else.

Finally, a single set of thirty experiments, ten in each series, was made where the impressions were given in accordance with the suggestions. That is, in the first set the second impression was sometimes larger, sometimes smaller than the first; in the second set, the two distances were sometimes equal, and in the third set both impressions were sometimes of one point only. There were five correct judgments in the first ten, six in the second, and six in the third, showing, as far as so small a number of experiments could, that the separation of the points was somewhat below the limen.

So marked an example of the influence of suggestion in this field, an influence which was first demonstrated by Tawney, seemed worth reporting.

SOME OBSERVATIONS UPON PRACTICE AND FATIGUE AS THEY AFFECT THE RATE OF TAPPING[1]

By ALICE M. BATTY

These observations were taken to determine how different periods of rest might affect the rate of movement in successive trials at tapping and what might be the rate of practice gain in a series of observations taken for a number of successive days. The observations were made upon three regular reagents and the students who came into the laboratory by chance and for the regular laboratory work in the practice course. A single trial at tapping lasted for five seconds and five periods of tapping separated by different periods of rest were made to follow one another. The rest periods were five, ten or twenty seconds. Five trials at any given rest period are called an observation and six observations, three with either hand at the three different rest periods, constitute a record. Usually four observations were taken at a sitting, two with either hand, and only one sitting a day was allowed. The hands were alternated in beginning the successive observations and the tapping was begun each day at a rest period different from that at which it was begun the day before. In this way the after effects of previous tapping were as nearly equalized as possible. Upon reagent B_1 thirteen records were taken, upon B_2 sixteen and upon Ph twelve. The students made only a single record. Of the students there were two classes. The first class took their observations in irregular order, some beginning with one rest period and some with another. About an equal number began with each rest period. In the second class all began wth the twenty-second rest period, then followed the ten and finally the five. The mental after-effects of tapping with five second rest periods are felt for some time—several hours perhaps. While after-effects from the other periods are not observable for more than a few moments introspectively, they may still exist and affect the subsequent rates of tapping. If, however, records are taken with only one rest period, the practice effects, which are much more easily demonstrable than the temporary after-effects just referred to, will affect any subsequent work with the tapping test for many weeks. Consequently the plan of equalizing these various results by alternating the different rest periods was chosen.

The tapping was done upon a lever attached to the escapement of a clock. A hand was rigged to one of the wheels and this passed over the face of a dial. The time was taken with a stop watch. In the following table are given as general results, the average number of taps for either hand in the five successive trials and for all three rest periods. In one or two cases a figure was copied incorrectly so that the record was thrown out. Three observations from the records of Ph and one from the record of B_1 are wanting.

---

[1] This work was done under the direction of Professor T. L. Bolton in the psycho logical laboratory at the University of Nebraska.

TABLE I.

Reagent B1.

	Right Hand					Left Hand				
	5 sec. rest. 12 records.									
Trials.	1	2	3	4	5	1	2	3	4	5
Av. Taps	36.5	36.08	37.08	36.08	36.16	34.33	36.75	35.0	34.25	34.25
	10 sec. rest. 13 records.									
Av. Taps	35.69	38.53	36.3	36.07	35.61	32.84	35.3	35.07	34.61	34.0
	20 sec. rest. 13 records.									
Av. Taps	36.61	36.61	36.38	35.84	36.61	35.15	38.38	35.0	35.35	35.76
	Reagent B2.									
	5 sec. rest. 13 records.									
Av. Taps	38.15	37.0	37.0	36.61	37.76	37.46	36.84	36.15	36.07	36.53
	10 sec. rest. 18 records.									
Av. Taps	37.83	38.5	39.0	38.5	38.72	37.94	39.16	37.94	38.61	38.11
	20 sec. rest. 16 records.									
Av. Taps	38.0	38.31	39.25	39.06	39.31	39.56	39.0	39.37	39.87	40.81
	Reagent Ph.									
	5 sec. rest. 12 records.									
Av. Taps	43.75	43.08	43.41	43.08	43.16	44.41	43.25	42.83	48.75	41.33
	10 sec. rest. 12 records.									
Av. Taps	46.00	44.16	44.25	44.41	44.00	46.91	44.50	44.41	43.33	44.00
	20 sec. rest. 9 records.									
Av. Taps	42.88	42.11	42.22	43.22	43.88	45.55	44.44	44.44	44.33	44.22
	Class Records. First Division.									
	5 sec. rest. 38 records.									
Av. Taps	39.10	39.07	38.28	37.75	37.78	35.10	34.71	34.71	35.25	33.92
	10 sec. rest. 24 records.									
Av. Taps	38.25	39.25	38.95	39.66	39.33	36.41	36.20	35.87	35.37	35.58
	20 sec. rest. 26 records.									
Av. Taps	39.26	39.61	38.61	38.30	38.03	35.76	36.53	36.42	36.15	35.84
	Class Records. Second Division.									
	5 sec. rest. 31 records.									
Av. Taps	39.61	40.54	39.32	38.64	37.29	37.22	36.54	35.58	36.74	36.00
	10 sec. rest. 32 records.									
Av. Taps	40.40	40.62	39.81	40.06	39.03	37.03	37.18	36.62	36.40	36.78
	20 sec. rest. 32 records.									
Av. Taps	38.58	38.78	38.40	40.06	39.71	36.22	37.03	35.15	36.39	36.65

From these figures the average gain or loss in the five successive trials for all rest periods and both hands was computed. The average gain or loss for the five successive trials is given in Table II following:

TABLE II.

Reagent B1.

	Right Hand. Gain	Right Hand. Loss	Left Hand. Gain	Left Hand. Loss
5 sec. rest	——	.175 taps	——	.145 taps
10 sec. rest	——	.123 "	.161 taps	——
20 sec. rest	——	.067 "	——	.084 taps

Reagent B2.

	R. H. Gain	R. H. Loss	L. H. Gain	L. H. Loss
5 sec. rest	——	.114 taps	——	.307 taps
10 sec. rest	.177 taps	——	——	.022 "
20 sec. rest	.377 "	——	.337 taps	——

Reagent Ph.

	R. H. Gain	R. H. Loss	L. H. Gain	L. H. Loss
5 sec. rest	——	.110 taps	——	.900 taps
10 sec. rest	——	.375 "	——	.700 "
20 sec. rest	.311 taps	——	——	.277 "

Class Record. First Division.

	R. H. Gain	R. H. Loss	L. H. Gain	L. H. Loss
5 sec. rest	——	.470 taps	——	.182 taps
10 sec. rest	.258 taps	——	——	.266 "
20 sec. rest	——	.376 taps	——	.231 "

Class Record. Second Division.

5 sec. rest	——	.687 taps	——	.035 taps
10 sec. rest	——	.331 "	——	.153 "
20 sec. rest	.334 taps	——	.120 taps	——

The figures in this table show that when a five second rest period is allowed between the successive trials at tapping there is an average loss for all reagents. When the ten second rest is allowed, there is generally a less average loss than with a five second rest or there is an average gain. Two cases of the latter are found, the left hand of B_1 and the right hand of B_2. With a twenty second rest period the average loss is generally less than with either the five or ten second rest period, or there is a decided average gain. In the possible ten cases three times a greater loss is made with the ten and twenty second rest periods than with the five, and in two cases the ten second rest period shows a gain where a small loss is shown in the twenty second rest period. The evidence, however, that the twenty second rest is the most favorable pause between five second periods of tapping is fairly conclusive.

If the algebraic sum of the losses and gains for the three reagents and the two classes of students for each rest period is taken, the following figures are obtained:

	R. H.	L. H.
5 sec. rest	1556 loss	1559 loss
10 sec. rest	604 loss	1302 loss
20 sec. rest	739 gain	135 loss

The right hands show for the ten second rest less loss than for the five second rest and for the twenty second rest a considerable gain. The left hands show a loss decreasing inversely with the length of the rest period. The left hand thus responds less clearly to the favorable condition of a longer pause than the right. This is about what we should expect, although when the total amounts of work done by either hand and for all three reagents were computed, it was found that they are all practically ambidextrous. B_1 and B_2 are perhaps slightly right-handed and Ph is with equally small amount left-handed. While then the two hands have done about equal amounts of work, they have arrived at this result by different roads. Being less accustomed to voluntary direction, the left hand would not profit so readily by favorable conditions.

Another method of treating these results is here offered which shows well what has actually been transpiring. If the average rates of tapping for the successive trials for the three rest periods and both hands are taken separately and arranged by the ordinal numbers in the order from the highest to the lowest and the ordinal numbers are then added up, an expression for the place where the slowest rate of tapping is found will be obtained. Let this illustrate the method which we propose here to follow: For the right hand of reagent B_1 at the five second period of rest, the highest rate of tapping was found in the third trial and the next highest in the first trial, the fifth trial was third, the second was fourth and the fourth trial the lowest or fifth. The five trials for each reagent and the two classes are then arranged in the same way for both hands and for each rest period and the sums of the five columns are then taken. The result is given in the following tabular statement:

TABLE III

Five Second Rest

	R. H.					L. H.				
B_1	3	1	5	2	4	2	3	1	4	5
B^2	1	5	2	3	4	1	2	5	3	4
Ph	1	3	5	2	4	4	1	2	3	5
Cl I	1	2	3	4	5	4	1	2	3	5
Cl II	2	1	3	4	5	1	4	2	5	3
Total	8	12	18	15	**22**	12	11	12	18	**23**

Ten Second Rest

Total	12	**18**	14	14	14	7	14	17	**20**	18

Twenty Second Rest

Total	**17**	15	16	14	13	12	**19**	16	14	13

We have omitted the individual records and given only the sums of the five columns under the ten and twenty second rest periods. For the five second period the highest figures are found under the fifth column which means that the lowest rate of tapping was found here in the fifth trial. For the ten second period the slowest rate of tapping was at the second trial with the right hand and at the fourth trial with the left hand. For the twenty second period the slowest rate was at the first trial with the right hand and at the second trial with the left hand. In other words the slowest rate has moved from the fifth place with the five sec. rest to the first place with the twenty sec. rest. This process of shifting has been slower with the left hand than with the right hand; it has lagged behind two places for the left hand with the ten seconds rest and it has reached only the fourth place with the left hand with the twenty sec. rest. Two main influences that act together to determine the amount of work that can be done within a given time and under given circumstances are fatigue and practice. Other influences pointed out by Kraepelin are beginning incitement, concluding incitement (finishing spurt) and warming up. If Wells's usage of the term "warming up" as an increase in rate of work during a period of continuous work rather than an increase which takes place between successive periods of work[1] is to be accepted and adopted, then none of the last three mentioned influences need to be considered here. Only interserial rather than intraserial changes have been studied here. While this distinction is well taken in the main, a further word for its broader usage will be offered. With the five sec. rest there is a continued loss in amount of work in successive trials. With the twenty sec. rest for the right hand there is a continued gain and for the left hand the gain begins only after the second trial. The five second rest is clearly inadequate to dissipate the effects of the fatigue of the preceding trial. Fatigue here outweighs practice or other accelerating factors. Twenty seconds seem fully sufficient except after the first trial with the left hand. Here there seems to be another phenomenon. The reagents complained of losing interest during the twenty second rest; they spoke of finding it difficult to keep themselves 'keyed up' for that length of time. The term "warming up" might then be made to cover this phenomenon of being keyed up or of not losing interest or excitement. Fatigue would still cover interserial gain, but it does not seem quite applicable for the gain showing after such short rest periods. With the ten second rest there is first a loss in work power and then a gain. This process of getting keyed up, excited or "warmed up" here takes some time so that at first fatigue overbalances any gain through practice, but later when the excitement has arisen, the speed of work increases with successive trials. When the average trial gain is taken, both hands show a loss, the right showing less loss than the left. The results in this last table show that the left "caught its pace" later than the right which harmonizes with the greater loss for the left than for the right.

The following table contains the average daily gains for the sums of the five trials.

TABLE IV.

Reagent	Hand	5 sec.	10 sec.	20 sec.
B1	R.	3.234	2.027	1.494
	L.	2.770	2.410	2.500
B2	R.	1.247	.566	.836
	L.	3.129	1.983	2.011
Ph.	R.	4.120	4.270	2.800
	L.	1.741	.923	1.360
	Total	16.241	12.179	11.001

The result is that the average daily gain is inversely as the length of the rest. The practice gain seems to be great in proportion as the

[1] F. L. Wells: Normal Performance in Tapping Test, *Amer. Jour. Psych.*, Vol. XIX, p. 446.

work is fatiguing. In view of this fact it may be said that, if the series were to continue long enough, the five second rest might prove the most favorable to the work capacity of the reagent. In Table III we might expect that for all rest periods the highest numbers would shift to the first places as the highest number is now found in the first place for the twenty second rest with the right hands. It is evident that in instituting comparisons between different grades of intellect, between classes of persons doing different kinds of work and between the normal and abnormal subjects, account must be taken of the practice efficiency in which the various subjects may find themselves at the time of the tests. Kraeplin's suggestion that we must study in such comparisons the manner in which the rates of practice gain are affected is in point here. Further work must be done to show how the curve of practice behaves and to determine the permanence of any practice effects that may be made and the likelihood with which they will appear in short and long series. Results gained from applying tests to different classes of subjects whose differences are alleged only upon the basis of common observation must be weighed with extreme care and conclusions drawn only with deliberate precaution.

In order to determine in what part of the series the greatest gain was made the observations were divided in the middle and the average daily gains were computed for each division. In the following table are given the algebraic differences between the gains or losses for the first and second halves. Where the second half has shown an advantage over the first half the figures are marked 'g' and where the result has turned out in favor of the first half 'l' is placed after the figures.

Right hand

Reagent	5 sec.	10 sec.	20 sec.
B_1	.233 g	.178 g	.025 l
B_2	.866 g	.358 g	.043 l
Ph	.566 g	.617 g	1.200 g
	1.865 g	1.153 g	1.132 g

Left hand

	5 sec.	10 sec.	20 sec.
B_1	.300 g	.133 l	.250 g
B_2	.530 g	.293 l	.214 g
Ph	.400 g	.100 l	.025 g
	1.230 g	.526 l	.948 g

All these records, except those for the left hands with the ten sec. rest, show a decided advantage in favor of the second half. In carrying out a piece of work fatigue makes itself felt first and practice gain disappears in counteracting the effects of fatigue. Practice consists in part in overcoming fatigue. The greatest advantage of the second half over the first is shown for the five sec. rest where the greatest average daily gain was made. It might be pointed out further that reagent B_1 who showed least fatigue loss gains least during the second half and reagent Ph who was most affected by fatigue shows here a decided advantage as gaining most during the second half.

The general conviction that practice gain is in proportion to the amount of fatigue engendered by the work is supported by the results. One must work up to his maximum, where he suffers most fatigue, that he may profit most by his work in the way of practice gain. The first object of practicing an effort is to overcome the depressing and inhibiting effects of fatigue and even throughout any series of efforts a certain amount of the practice effect is lost through habituation of the reagent to the newer conditions imposed by the gains already made by practice. After a certain number of trials, however, the reagent is affected less and less by the mental symptoms of the work and the practice shows itself by the increased amounts of work done within a given time interval.

In conclusion it may be said that with five sec. trials at tapping the rest periods, five, ten and twenty sec. are favorable to work in an order the reverse of that in which they stand here and that these rest periods are favorable to practice gain in the order here given. With these rates of daily gain it is not improbable in a long series that the shortest rest period might prove the most favorable. It is suggested that before this test can be used as a basis of comparison for different classes of subjects tests must be made upon more subjects with only one rest period at a time, that they be continued for a longer time, and that other periods of tapping, ten and twenty seconds, be tried with the same and longer periods of rest. The standardization of the various psychological tests is imperatively demanded before further work in using them is done.

PSYCHOLOGICAL LITERATURE

Report of the Royal Commission on the care and control of the Feeble Minded, Vols. I-VIII. Wyman & Sons, London, 1908.

The most important contribution to the literature of mental defectives that has appeared for many years is the Report of the Royal Commission in England. This Commission, consisting of eleven men and one woman, was appointed by King Edward in 1904, "to consider the existing methods of dealing with idiots and epileptics, and with imbecile, feeble-minded, or defective persons."

This Commission has had a remarkable opportunity to study the problem and apparently they have made good use of the four years that they have devoted to it. They had full power to summon witnesses, visit institutions, aud carry on investigations. They have utilized all of these methods, and have presented their findings in eight large "blue books" of which the eighth alone contains 512 pages. Five of these volumes contain the evidence relating to conditions in England, Scotland, Wales, Ireland, and the United States; one contains the Reports of Medical Investigations, one Appendices, and the last or eighth volume contains the recommendations of the Commission.

One of the most interesting chapters is that which discusses the cause of mental defect. The Commission did not consider an exhaustive investigation into this subject within their province, but they took much testimony from which it appears that the great weight of authority in England is for heredity as against environment. We had thought this point beyond question. It is the only view in accordance with the prevailing biological views and, besides that, it is practically certain that from 60 to 80% of all defectives have defective ancestry. There are, of course, a few cases of defect due to accidental injury to the brain, but these are comparatively rare and do not affect the main question.

Professor T. Clifford Allbutt says "I regard feeble-mindedness (if not accidental) as always hereditary." Dr. Bevan Lewis says "I look upon feeble-mindedness as a germinal variation as all variations are." Dr. Tredgold states that over 80% of mental defectives are connected with neuropathic inheritance. He has never seen a normal child born of two feeble-minded persons. Twenty-five physicians among the most prominent in England, and all dealing with large numbers of mental defectives, express similar opinions.

It must then be stated as the accepted opinion among those of largest experience, that feeble-mindedness is in the first instance a spontaneous variation, like a sixth finger or any other "sport", and as such is likely to be transmitted. The Commission sum up their conclusions on this point as follows.

(1) "That both on the grounds of fact and of theory there is the highest degree of probability that feeble-mindedness is usually spontaneous in origin—that is, not due to influences acting on the parents—and tends strongly to be inherited."

(2) "That especially in view of the evidence concerning fertility the prevention of mentally defective persons from becoming parents would tend largely to diminish the number of such persons in the population."

(3) "That the evidence for these conclusions strongly supports measures, which on other grounds are of pressing importance, for placing mentally defective persons, men and women, who are living at large and uncontrolled, in institutions where they will be employed and detained, and in this and other ways, kept under effectual supervision so long as may be necessary."

The "evidence concerning fertility" referred to is to the effect that mental defectives are about twice as fertile as normal individuals. Sixteen feeble-minded women in one workhouse together produced one hundred and sixteen children! The Commission concludes that the people would rightly condemn surgical and other artificial interference to prevent hereditary transmission of mental defect. Of twenty-one witnesses who mentioned the subject only three expressed opinions in favor of the practicability of such a course. As to preventing the marriage of defectives, they are equally conservative, and think it would be unwise to modify the existing law.

As to the number of mental defectives, their returns are rather startling. It has been generally held that two to every thousand of the population was a conservative estimate. The Commission find in Scotland 2.6 per thousand, in England 4.6, and in Ireland 5.7. If these figures are reliable, they furnish much food for thought and suggest many things in sociology.

The practical side of the work of the Commission is embodied in ninety-six recommendations for England and Wales, forty-three for Scotland and seventy-four for Ireland. H. H. GODDARD.

Das Gedächtnis; die Ergebnisse der experimentellen Psychologie und ihre Anwendung in Unterricht und Erziehung, von DR. MAX OFFNER. Berlin, Reuther & Reichard, 1909. pp. X : 238.

The chapter of Memory is one of the oldest in Psychology and one of the most voluminous in its literature. Even the experimental literature which began only twenty-five years ago with the work of Ebbinghaus has reached serious proportions. A work like this of Offner's which gathers up and co-ordinates with judicial fairness the results obtained by the large body of different workers is, therefore, particularly welcome. Though the book is intended in the first instance for teachers and for those who are entering psychology through their interest in memory, and for this reason has been kept close to essentials and purged of unnecessary technicalities, it will be found not to come amiss to the working psychologist whose labors have kept him in other fields and who wishes to put himself rapidly in touch with recent work on memory. The material is systematically presented and made further accessible by an analytical table of contents and full indexes. A bibliography of 181 titles, though not all upon the experimental studies of memory, is also included. The work as a whole is well calculated to fulfil the author's hope that it may promote a "comprehension of the methods of the newer psychology and confidence in them." E. C. S.

Ce que l'Armée peut être pour la Nation. Par le Lieutenant adjoint d'État-Major A. FASTREZ. Paris, Misch et Thron, 1907. pp. xiii, 294.

This work was called forth by Mlle. Joteyko's book on *Entrainement et fatigue au point de vue militaire*, 1905, and seeks to refute, on behalf of the Belgian army, the conclusion arrived at by its author and accepted by Prof. Richet, that the limit of military training is satisfactorily attained at the end of six to twelve months. M. Fastrez believes, on the contrary, that it is unwise to reduce the present term of twenty months' effective service; he argues that Mlle. Joteyko has unduly narrowed the concept of training, and has overlooked the economic position and importance of the army.

The book opens with a chapter upon physical training, and its distinction from psychological. M. Fastrez then proceeds to outline the course of physical (including professional) training imparted to recruits and to appraise its results. He passes to the question of the psychological training of the soldier, and makes out a strong case for his side, mainly by reference to the events of the Russo-Japanese war. The psychological training which the recruit receives during his twenty months with the colors must be supplemented by similar training in the family, in the schools, and from the social environment at large.

M. Fastrez is thus led to consider the economic rôle of the army in the modern state. He points out, with especial reference to the history of Rome, the part played in the economic development of the ancient world by the peculiar circumstances of the military class, and declares that there is a close agreement, to-day, between the qualities of the trained combatant and the qualities of the economic worker. He shows that the maintenance of an army, as organ of defence, is implied in the struggle that pervades all nature, and particularly in the economic war that has come with the lessening of world-distances and the spread of imperialism. He ends with a paragraph on the resulting danger to small nations, which cannot afford to neglect any means for obtaining full returns, social, economic and 'energetic,' from the armed group of their populations. An appendix criticises, unfavorably, the outcome of the experiment recently made in England by Colonel Pollock and his 'Spectator' company. M. W. WISEMAN.

The Methods and Scope of Genetics, by W. BATESON. University Press, Cambridge, 1908. 49 p.

This address is a simplified statement of the theory of heredity as it has evolved in recent years under the influence of Mendelism, which has shed new light on the general thesis that everything that lives is not one thing but two, double throughout in every part of its composition. It teaches that we are assemblages or medleys of our parental characteristics, and establishes a law as to the prepotence of each of the moieties of heredity that come to us from the two parents. "If both parent-gametes brought a certain quality in, then all the daughter-gametes have it; if neither brought it in, then none of the daughter-gametes have it; if it came from one side and not from the other, then on an average in half the resulting gametes it will be present, and from half it will be absent." This last phenomenon, which is called "segregation," constitutes the essence of Mendel's discovery. The rest of the lecture gives a rapid, popular sketch of many special studies upon plants, animals and men that illustrate these general laws, which for the first time have given to variation and reversion a concrete and palpable meaning. "The time for discussing evolution as a problem at large is closed. We face that problem now as one soluble by minute, critical analysis." Variation is a definite, physiological event, viz.: the addition or omission of one or more definite elements; and reversion is that particular addition or subtraction which brings the total of the elements back to something it had been before in the history of the race.

Heredity, Variation, and Evolution in Protozoa, II, by H. S. JENNINGS. Reprint from Proceedings of the American Philosophical Society, Vol. XLVII, No. 190, 1908, pp. 393-546.

The author, who has made valuable contributions to the psychology of simple forms of life, here presents us with a very painstaking series of studies showing growth of paramecium through seven stages, the effects of environment, inheritance of size, results of selection within pure lines, etc. It was found that large and small specimens of a single pure line produce progeny of the same mean size. The causes and nature of the variations in sizes even in a pure line must be many. Environment, especially nutrition, is very significant. A given wild culture generally contains many different races which maintain their relative sizes throughout all sorts of conditions. After many experiments it was found that selection within a pure line was quite without effect. Large individuals of the line produce progeny of the same mean size, as do small individuals. The variations found in many different individuals of the same pure line are not inheritable. The fact covered by this last sentence seems to be the most important of the author's results. Some elements of the environment increase breadth and decrease length, but most such elements change the breadth most. Any agent that causes rapid multiplication decreases the correlation between length and breadth. In general it would seem that selection is not effective within pure lines. In a mixed population, selection operates upon the various different lines already existing. Selection often will not carry a character beyond a certain point, because the line that has this original character strongly marked has been isolated, and selection of the fluctuations has no effect within the pure line. As to regression, the product of extremes stands nearer the mean than did the parents, although they diverge in the same direction. In such cases we have the largest individuals of the largest line, and the smallest of the smallest line producing the mean of the lines. Thus the largest and the smallest approach the mean of the original collection as a whole. As to the main question, how the different pure lines arise. we need further study.

Heredity and Prenatal Culture, by NEWTON N. RIDDELL. Child of Light Publishing Co., Chicago, 1900. 351 p.

This indefatigable author here pursues with great enthusiasm his own independent studies of heredity. His chapters are entitled: Some Objections Considered, Psychology, Brain Building and Soul Growth, The Reproduction of Life, the Factors of Heredity, Parental Adaptation, Sex Potency, Dual Parentage, Atavism, Prenatal Influences, Physical Preparation, Mental Preparation, Initial Impressions, Maternal Impressions, Maternal Impressions (continued), Abnormal Impressions; Heredity, Insanity and Imbecility; Heredity, Homicide and Suicide; Heredity and Commercialism, Heredity and Intemperance, Heredity and the Double Standard.

Mann und Weib. Edited by R. KOSSMANN and JUL. WEISS. Union Deutsche Verlagsgesellschaft, Stuttgart, Berlin and Leipzig, n. d. 3 v.

These three stately volumes are by more than a score of different authors, and are copiously illustrated with 421 cuts in the text and 22 illustrated inserts. In the first part man is described—his form, as child, youth, husband, father, widower, bachelor, his sexual and intellectual life—with eleven chapters by as many authors. Then comes woman in fourteen chapters. The work contains very little that is new for the scholar. Its chief merit consists in its voluminous illustrations and in its moderate price, 36 Mks.

Anti-Pragmatisme, par ALBERT SCHINZ. Félix Alcan, Paris, 1909. 309 p. (Bibliothèque de Philosophie Contemporaine.)

Although Dr. Schinz is a professor of modern languages, he has long been known as a man of thorough philosophical training, wide knowledge, and very active mind. Those who read English will deeply regret that the author saw fit to write his work in French. It is the most masterly analysis and criticism that modern pragmatism has yet had. We regret that we are unable to devote more space to it and to give a more adequate review of it here. In the first part, the author takes up pragmatism and its relations to intellectualism, discussing its principles in general. A special chapter is given to Dewey. In the second part, entitled "Pragmatism and Modernism", the author shows the social phenomena that explain the principles of such a philosophy, and then considers the pragmatism of the Middle Ages and modern scholasticism. Scholastic metaphysics was the pragmatism of the Middle Age, and pragmatism is modern scholasticism. Not only scholasticism, but the pragmatism of Kant, the author deems indefinitely superior to that of James and Schiller. In one chapter, Dr. Schinz discusses the question whether James is a pragmatist or not; and leaves the reader to infer that neither James nor he himself is able to determine. In appendices, the author discusses the common sensations and philosophy, and the relations between literature and the moral code.

What is Pragmatism? by JAMES BISSETT PRATT. The Macmillan Co., New York, 1909. 256 p.

We have here six lectures given last summer at the Glenmore Summer School before an audience, if we understand the writer, of hardly more than half a dozen people, and the rest of the world is here compensated for its loss by being given these lectures in due form. No two writers have the same conception of pragmatism, and this makes it an admirable topic for those in our day who have a strong scholastic bent. The author tells at the outset of a law professor who discussed the question, whether the individual really owned his land or whether the state, which could exercise its right of eminent domain or could condemn it, was the party in whom ownership really vested. Pragmatism would say this was no problem at all, because ownership consists in enumerating the things the so-called owner can do. Pragmatism asks about everything what it means for me, for a thing is what it does. Meaning is influencing practice. Truth is what is useful or works well. Idea is a synonym for a plan of action. This is what Schiller calls humanism. It is the pet child of epistemology and gives speculators of this ilk a new and fascinating ambiguity to charge up against truth. Just as no pragmatists agree, so no two critics of it agree, and it is rather curious to see two books that have simultaneously appeared both disputing its claims, viz., Pratt and Schinz, taking almost diametrically opposite views of it. For himself, the writer confesses, that after having read much and, alas! written several papers concerning pragmatism, he is obliged to confess that there is such an incommensurability between it and the writer's mind that neither finds anything in the other.

Psychotherapy, by HUGO MÜNSTERBERG. Moffat, Yard & Co., New York, 1909. 398 p.

This is second in the series of books the author is writing to discuss for the wider public the practical applications of modern psychology. He promises others on sex, social problems, commerce, industry, etc. He tells us he has chosen "the form of loose popular essays;" and yet in the next sentence tells us there is too much loose talk afloat about psychotherapy. We are told that he has a personal right to deal with these questions because he studied medicine and holds the degree of M. D., and also gave the first university course on hypnotism in the world, and since then "has never ceased to work psychotherapeutically." He has helped "many hundreds" and "no one ever had to pay anything." At the outset, we are given the psychological basis of psychotherapy, including the aim of psychology, mind and brain psychology in medicine, suggestion and hypnotism, psychology of the subconscious. The author develops his philosophical notion of a radical difference between the causal series of events which doctors chiefly concern themselves with, and the purposive series. In part II, he discusses the practical work of psychotherapy: its fields, general and special methods, mental and bodily symptoms. In the third, the place of psychotherapy, viz.: its relations to the church, the physician and the community. Barring the commingling of philosophical and scientific psychology, so characteristic of Harvard, especially protesting against the ever intrusive distinctions between causal and purposive, and the reiterated foible of the author that there is no subconscious or no unconscious, the book is on the whole a broad-minded and very sensible popular presentation of the subject. We confess, however, to some disappointment, after the many allusions in the past to this author's work, that the clinical cases he publishes are not richer, more numerous, and that some of them are so very fragmentary. Despite its defects and perhaps especially its diffusion, which it is very tempting to enlarge upon, its merits far preponderate; and it is on the whole the best popular presentation the subject has ever had in English up to date.

The Emmanuel Movement in a New England Town, by LYMAN P. POWELL. G. P. Putnam's Sons, New York, 1909. 188 p.

This is the third in a trilogy of books by this author within two years, the first entitled, "Christian Science;" the second, "The Art of Natural Sleep." The present shows the possibilities of the Emmanuel Movement as applied to a wide range of nervous functional disorders in the town of Northampton, and indicates the wider range to which the movement is destined to re-energize the whole church. The chapters are: What the Emmanuel Movement is; the Clinic in a College Town; a Year's Results; the Treatment of the Nerves; the Queer One in the House; the Cure of the Alcoholic; the Miscellaneous Cases; the Movement and the Church.

Letters on Psychotherapeutics, by PROF. H. OPPENHEIM. Translated by Alexander Bruce. Otto Schulze & Co., Edinburgh, and G. E. Stechert, New York, 1907. 60 p.

Here we have a number of interesting letters to various patients, some of whom have been eminent. There is certainly great sagacity shown.

The *Proceedings of the Society for Psychical Research* for Feb., 1909, contain an article by Sage on the Alleged Miraculous Hailstones of Remiremont, and two on D. D. Home by Count Petrovo Solovovo and Miss Alice Johnson, the former arguing against his employment of suggestion to produce illusions and hallucinations in his sitters, and the latter for this hypothesis. Miss Johnson's thesis is, in brief, that Home put most of his sitters through an educative process by which he made them highly suggestible, and she cites various instances, showing especially how he gradually worked up to his famous levitation feat of floating out of one window and in at another, eight feet above the ground. Count Petrovo Solovovo cites cases in which he thinks the proof is plain that the phenomena were objective at least, though they might have been produced by trickery, and he argues that since there was no hallucination in these cases there could have been none in others. But Miss Johnson believes that Home employed whatever means were best suited to the particular time and place—sometimes suggestion, sometimes other methods of fraud. Mrs. Sidgwick also reviews fully Morselli's new book on Eusapia Paladino and Count Solovovo reviews Podmore's recent book entitled the Naturaliaztion of the Supernatural.

Text-book of Experimental Psychology, by CHARLES S. MYERS. Edward Arnold, London, 1909. 432 p.

At last we have a text-book of experimental psychology written in England by an Englishman. In this country we have had plenty. But this work is written on different lines. It presupposes some acquaintance with the elements of general psychology, such as Stout's work; but is less advanced than that of Titchener. It assumes, too, some acquaintance with the general structure and functions of the nervous system. Although the author lays stress upon the physiological and physical conditions, his ultimate object is to describe the methods, principles and results of psychological experimental research. He has not included the topics of animal behavior, or of children and primitive races, nor subconscious, abnormal states. The book is based upon experience in teaching. It begins with sensations—cutaneous, visceral, auditory, labyrinthian, visual, gustatory, olfactory; and then considers the specific energy of sensations, statistical methods, reaction times, memory, muscular and mental work, psychophysic methods, weight, local signature, sensibility and sensory acuity, identity and difference, and binocular and binaural experience, the visual perception of size and direction, and time, rhythm, attention, and feelings. There is an appendix of nearly one hundred pages in finer type at the end on laboratory exercises. There are in all sixty-six cuts, and a judicious bibliography at the end of each chapter.

Modern Educators and their Ideals, by TADASU MISAWA. D. Appleton & Co., New York, 1909. 304 p.

No text-book exists in English or in any other language save German (in which thirty years ago Vögel printed a volume on the educational views of the great thinkers in the history of philosophy who had treated the subject) on the history of the philosophy of education. The student in this field has hitherto been entirely dependent upon special monographs or upon gleanings such as he could make from the history of philosophy. This book attempts to fill this gap from Bacon and Comenius down to William T. Harris and Stanley Hall, both inclusive. The author is a special student of philosophy and psychology and his training and interest in both seem to be equally good and his sympathies well balanced between theory and speculation on the one hand, and empiricism and experimentation on the other. It is singular that it should have been left to a native of Japan first to attempt this work in the English language, and yet more singular that he should have succeeded in giving us in such lucid and idiomatic English the often abstruse topics of which the book treats. It cannot fail, in the opinion of the present writer, to find a speedy entrance into normal schools and educational departments of college grade. The references, in which the author has had the expert help of Dr. Theodate Smith, are well chosen. In our opinion the chief criticism of the book would be that it is not a little more detailed in some chapters. The writers treated, in addition to the above names, are Locke, Rousseau, Basedow, Kant, Pestalozzi, Fichte, Froebel, Herbart, Spencer, and Hegel.

The Life of the Spirit, an Introduction to Philosophy, by RUDOLPH EUCKEN. Translated by F. L. Pogson. Williams & Norgate, London, 1909. 68 p.

This is rather a charming group of five lectures with a conclusion, on unity and multiplicity, change and persistence, time and eternity, the outer and the inner world, problems of truth and of happiness. It is an easy and soothing work. Much of it is taken up with comprehensive, backward glances over the history of philosophy, in order to give the writer momentum of mind enough to come down into close and hard contact with contemporary topics of interest. The lecturer's chief anxiety is lest in the multiplicity of interests and movements to-day, the human mind shall lose something of its integrity, and unity take its departure from the world, discountenanced by many strident specialties. The author gives pragmatism a rather shady characterization through a number of pages, but does not go into it in detail. For those who are not familiar with the author's voluminous philosophical writings in the past, this will serve as an admirable introduction to them.

Elements of Ethics, by NOAH K. DAVIS. Silver, Burdett & Co., New York, 1907. pp. 294.

This is designed to give a simple, direct and comprehensive theory of morals, and to be a handbook for those pursuing a liberal education. It is the ripened fruit of a long life of teaching in this field, and the author, with due modesty, makes this the contribution of his experience. It is certainly lucid and an admirable introduction to the subject. The author has done a great deal of conscientious thinking and reading, being stronger in his knowledge of French and Latin than of German writers, which is, after all, perhaps no very great loss. The prolegomena has two parts, psychological and philosophical. His ethics also has two: the first, obligation, treats rights, liberty, trespass, law, sanctions, right and wrong, justice, beauty and virtue, selfishness, service, charity, welfare, and deity, each under a number of sub-divisions. The second part, organization, treats of man, the family, community, state and church. The last book in such a field ought to be the best available for college work, and we are inclined to think this book has met that high requirement.

Travail et Folie; Influences Professionelles sur l'Etiologie Psychopathique, par les DRS. A. MARIE et R. MARTIAL. Paris, Bloud. 110 p.

After an historical criticism, with various statistics as to the liabilities of insanity in the different walks of life, the authors discuss the development of medicine toward sociology in general and toward mental medicine in particular. In a chapter on inherent difficulties they discuss especially labor and then other causes of mental maladies among laboring people and how they manifest themselves. They then pass to a discussion of the types of mental disease most common to laborers in general, with a few conclusions concerning the contributions which certain of the great classes of laborers make to the specific disorders, with a number of tables.

Mental Fatigue and Its Measurement by the Æsthesiometer, by A. R. ABELSON. Engelmann, Leipzig, 1908. 147 p.

This appears to be a careful experimental work, but it is very hard to get into. There is no index of either topics or chapters, no general summary of the work, and the sixth chapter, entitled "Résumé", and generally a conclusion, does not tell us definitely just what the author himself claims to have found out. He is sure, however, that the loss of sensibility runs parallel with the conditions of fatigue. He thinks its curve rises quickly at first, then more slowly, until it hardly increases at all. Those who are nervous and weak show a more rapid rise of the curve; those who are strong and healthy will keep up for a longer time. When fatigue supervenes upon a generally exhausted condition and resistance is less, its curve rises higher in an irregular way. Still, weak persons often possess great resisting power until there may be a breakdown without any warning.

Human Physiology, an Elementary Text-book of Anatomy, Physiology and Hygiene, by JOHN W. RITCHIE. World Book Co., Yonkers-on-Hudson, N. Y., 1909. 362 p.

This little text-book is written after years of experience in elementary and more advanced classes, with the chief object of making physiology tributary to health. The author has tried to balance his book between the mere teaching of facts and the rules of health. Physiology, he thinks, is the teacher's science and it should be brought into relation to nature study and agriculture and to preventive medicine, treatment of emergencies, etc., so that he gives special chapters to tuberculosis, preventing the spread of disease germs, accidents, etc.

Psycho-Biologie et Énergétique, par CHARLES HENRY. A. Hermann et Fils, Paris, 1909. 216 p.

This vigorous writer here brings together many of his papers, which have appeared elsewhere under various headings, and gives them a certain unity. In Part I, under theory, he discusses principles, representative elements, multiple and complex representation, specialization of higher kinds. In part II, which treats of the applications to certain problems, he gives particular sections to balistics, the complete formulæ of the simple pendulum, the problem of a jet of water sustaining a weight, the dynamics of electrons, the law of Dulong and Petit, curves of growth, chemistry of respiration, sensations and energy, ending with conceptions of energy as applied to sociological problems.

Un Problème de l'Évolution. La Théorie de la Récapitulation des Formes Ancestrales au Cours du Développement embryonnaire, par L. VIALLETON. Coulet et Fils, Montpellier, 1908. pp. 244.

This work attempts to characterize the theory of the recapitulation of ancestral forms, particularly in the course of embryonic development, and especially to test the fundamental law of biogenesis according to Haeckel. To our mind, the author takes too many chapters in recapitulating what is rather well known of the history of this doctrine, and is naturally most interesting when in the later chapters he comes to his own work.

L'Evolution Psychique de l'Enfant, par le DR. HENRI BOUQUET. Librarie Bloud et Cie, Paris, 1909. 100 p.

The writer is a psychologist and specialist in infant maladies and here tries to sum up the results of his own long experience concerning the development of human mentality in the early years, following the earliest sensorial activities, touch, taste, hearing, etc., the development of language, the least and then more complex psychic activities, habit, memory, affective life, fear and imagination, fetichism, etc.

Rassentheorie und Geschichtsforschung, von FRANZ SCHOLZ. G. Scriba, Metz, 1908. 24 p.

The scientific and social idea of race is of recent origin but is now making very impressive headway among learned men. This perhaps would hardly be seen in comprehensive scientific works, and so the writer attempts to develop it in these few pages, on the old and new method of writing history, the idea of race, the pre-historic man, the nature of kinds or varieties, the views of Gobineau, Chamberlain, and Lienhardt. The writer is a disciple of Gobineau in holding that the only real cause of racial decline is the mixture of bloods with alien and less valuable stocks. Christianity is said not to have helped mankind from a stirpicultural point of view and so is "not a civilizing agent" in this sense. The idea of race is based on natural history. Lienhardt thinks that the characteristic marks of a race are found only in the spiritual domain, and that physical traits are inadequate.

Erinnerung, Aussage und Lüge in der ersten Kindheit, von CLARA und WILLIAM STERN. J. A. Barth, Leipzig, 1909. 160 p. (Monographien über die seelische Entwicklung des Kindes.)

This very interesting and condensed presentation of many studies, in the first part treats of individual development and power of reproduction, telling how recognition is the forestadium of memory, and describes the sources of false statements. The second part treats the comparative psychology of the expressions of early childhood. This is treated from the standpoint of general observation and also from controlled experiments with single and prolonged exposures. Apparent and real lies are treated in perhaps the most interesting section of all. The practical applications bear upon observation and the power of reproduction, honesty, and the testamentary ability of children.

The Bawenda of the Spelonken, by R. WESSMAN. Trans. from the German by Leo Weinthal. "The African World," London, 1908. 154 p.

This is a contribution toward the psychology and folk-lore of the African people and describes life in the chief's kraal, family life, arts and industries, the salute, social laws and life, justice, mothers-in-law and sons-in-law, beer, various superstitions, customs, hospitality, proverbs, religion, rights, demonology, medicine men, witchcraft, African philosophy, the African war, ethnology, etc. The work is illustrated by perhaps two dozen photographs. The type is clear, and the author, from his long experience as a missionary, writes in an entertaining and very instructive way.

L'Adolescence, par GABRIEL COMPAYRÉ. Félix Alcan, Paris, 1909. 195 p.

The author has here written a work of fifteen chapters, based essentially on the work of Stanley Hall, whose name appears on nearly every page. The chapters are as follows: the duration of adolescence, physical growth, development of the organs, mental evolution, adolescence in literature, genetic psychology, feelings in adolescence, friendship and love, juvenile pathology and criminology, pedagogy of adolescence, the psychology and pedagogy of girls, co-education, the education of the young girl, the education of the young man. Thus essentially it follows Hall's order of topics and chapters.

Die sexuelle Not, von DR. FRITZ WITTELS. C. W. Stern, Wien und Leipzig, 1909. 206 p.

This work, which is dedicated to the author's teacher, Freud, discusses the following topics: Prohibition of abortions, Lust, Family, the Child—its ego, its rawness of *Gemüth*, its problems and cares, its voyages of discovery, and its sexual illumination. A chapter is given to the masculine woman, with two subdivisions: one on female crimes and the other on female artists; and the final chapter is on the child-woman or the precociously beautiful and developed girl. The author writes with the knowledge of a physician, but in rather a literary style, so that his book has a good deal of attractiveness from this point of view.

The Psychology of Thinking, by IRVING E. MILLER. The Macmillan Co., New York, 1909. 303 p.

The writer once taught mathematics in a New England academy. Here he was forced into close touch with the actual mental processes involved in thought. Thus his interest in its processes became clinical, and it was this that turned him to the study of psychology. Although his work is colored with this dominant idea, it is also largely biological. Man's control over nature depends upon the higher, psychic processes we call thought. The author discusses the biological point of view, the sensori-motor circuit, the significance, function, differentiation and organization of consciousness, organic unity of mental and motor life, typical modes of adjustment, the condition and function of thinking, unity and diversity in the process, training in thought and the use of subject matter, the activity of the imagination in thinking, the image as an element of technique, the concept in its various aspects, induction and deduction, judgment, reason.

Fifty Years of Darwinism. Modern Aspects of Evolution. Centennial Addresses in honor of Charles Darwin, before the American Association for the Advancement of Science, Baltimore, Jan. 1, 1909. Henry Holt & Co., New York, 1909. 267 p.

This work contains an introductory lecture by Professor Chamberlain, Fifty Years of Darwinism, by E. Poulton; Natural Selection and Botany, by J. M. Coulter; Isolation and Evolution, by D. S. Jordan; the Cell, E. B. Wilson; the Influence of the Environment, by E. C. MacDougall; the Behavior of Unity Characters in Heredity, by W. E. Castle; Mutation, C. B. Davenport; Adaptation, Carl H. Eigenmann; Darwin and Paleontology, H. F. Osborn; Evolution and Psychology, G. Stanley Hall.

Analyse der Phobie eines 5-jährigen Knaben, compiled by Sigmund Freud. Franz Deuticke, Leipzig and Wien, 1909. 109 p.

Acquaintances of Freud working under his direction have studied a remarkable case of sexual precocity of a boy who at the age of three began to be interested above all things else in the Wiwimacher, and was eagerly concerned as to whether chairs, animals, men and women, etc., possessed this part, and could not be withheld from incessant interest and conversation on the topic. The child was plainly hereditarily *belastet*, but was cured by hypnotic treatment modified in form to be applicable to children.

Die Psychologie des Verbrechers, Kriminalpsychologie, von PAUL POLLITZ. B. G. Teubner, Leipzig, 1909. 148 p. (Aus Natur und Geisteswelt, 248. Bäudchen.)

This is the most condensed treatise on criminal psychology known to the reviewer. Nevertheless, it is extremely comprehensive, touching most of the very many problems in the field. First comes all that can be taught from statistics; and then, in the second part, a special criminal psychology, such as the meaning of alcohol, juvenile crime, crimes of sex, tattooing, industrial crime, beggary, etc.

Life's Day, by WILLIAM SEAMAN BAINBRIDGE. Frederick A. Stokes Co., New York, 1909. 308 p.

This is a volume of Chautauqua lectures upon heredity, physiological and psychical, environment, education, infancy, nursing and modified milk, childhood, the irresponsible age, defectives, the waif, adolescence, mid-day with notes on psychotherapeutics, twilight or old age, and night or death. The work is, on the whole, a wholesome combination of good sentiment and practical applications of modern science to the care of childhood.

The Psychology of Dementia Præcox, by DR. C. C. JUNG. Translated by Drs. Peterson and Brill. The Journal of Nervous and Mental Disease Publishing Co., New York, 1909. 153 p. (Nervous and Mental Disease Monograph Series, No. 3.)

Doctors Peterson and Brill have done a good service in translating these five chapters, which are entitled: a Critical Presentation of Theoretic Views on the Psychology of Dementia Præcox, the Emotional Complex and its General Action on the Psyche, its Influence on Association, its relations to Hysteria, and an Analysis of a Case of Paranoia Dementia as a parody.

Studies in Paranoia, by N. GIERLICH and M. FRIEDMANN. Journal of Nervous and Mental Disease Publishing Co., New York, 1908. 78 p. (Nervous and Mental Disease Monograph Series, No. 2.)

To this work Dr. Gierlich contributes an essay on periodic paranoia and the origin of paranoiac delusions; and Friedmann makes a contribution to the treatment of paranoia.

The Peasantry of Palestine, by ELIHU GRANT. The Pilgrim Press, Boston, 1907. 255 p.

The author lived nearly three years in Ram Allah, a village ten miles north of Jerusalem. The aim of the author has been to write only of matters that came under his personal observation, and to give a systematic description of village peasants, and also to contribute to the subject of Palestinian research and prepare the way for further study in the folk-life of the country. The author's chief object was to know his neighbors thoroughly; and he paid attention to everything, because everything interested him. To turn his journal into a book was an after-thought, but in doing so he has added scriptural references.

Ein Beitrag zur grammatischen Entwicklung der Kindersprache, von I. A. GHEORGOV. Wilhelm Engelmann, Leipzig, 1908. 295 p. (Sammlung von Abhandlungen zur psychologischen Pädagogik, herausgegeben von E. Meumann, II. Band, 3 Heft.)

The author has here brought together his new and interesting but very special studies upon this subject, which it is impossible adequately to describe without great detail.

The Epileptic Voice Sign, by L. PIERCE CLARK and E. W. SCRIPTURE. William Wood & Co., New York, n. d. 10 p.

Wilhelm von Humboldts Sprachphilosophie, von MORITZ SCHEINERT. Wilhelm Engelmann, Leipzig, 1908. 55 p.

Grundriss der Psychologie für Padagogen, von O. LIPMANN. Leipzig, J. A. Barth, 1909. pp. vi, 100. Price Mk. 2.

This little book, by the co-editor of the *Zeitschrift für angewandte Psychologie*, is intended as a practical aid to teachers, who have not the time to apply themselves to the special text-books of psychology. It thus forms the counterpart ofthe same author's *Grundriss der Psychologie für Juristen*, which appeared in 1908. Certain theoretical discussions are, indeed, identical in the two works.

The Preface contains a brief selected bibliography of general psychology, child psychology, and experimental pedagogy. The Introduction (1-8) then outlines the relation of pedagogy to psychology and ethics, defines the subject-matter and the method of psychology (laying especial emphasis upon experiment), and indicates the place and character of child-psychology within psychology at large.

The following sections treat of the various departments of psychology, always in the light of educational application. Section I (9-17) deals with certain laws of sensation, contrast, habit, adaptation, color-blindness, etc.; and then passes to ideas, their vividness, and their differences from sensation. Section II (18-35) takes up the intellectual functions: memory-span, association of ideas, illusion and suggestion, imagination, *Aussage* and children's lies, thought and the formation of concepts. Section III (36-48) continues these topics, treating of types of ideation, the laws of memory and of economical learning, the methods of recitation (question, dictation, etc.). Section IV (49-65) brings us to the affective life: sense-feelings, the higher ethical, intellectual and esthetic feelings, the emotions and the passions. Section V (66-76) turns, naturally, to volition, which is regarded as being, like emotion, a complex of intellectual and affective elements. The author here discusses the reflex, impulsive action, voluntary action, choice, character, inhibition; and ends with an essay on the pedagogy of conduct. Section VI (77-92) treats of attention, and the laws of practice and fatigue, bodily and mental. Section VII (93-98) touches briefly on the child's development in intellect, attention and interest, and on differences of native ability, and applies what is known of these matters to the problems of school-organization and of coeducation.

It is astonishing that Dr. Lipmann has been able to cover so wide a range of subjects in so brief a space, and has found room to say the many sensible things he has, on the side of application. The only positive criticism that occurs to the reviewer is that the treatment of attention should have come earlier in the book. It may be added that the work is far too abstract in its psychology for use by the American teacher.

TH. WALTERS.

NOTES

Preliminary announcement has been made of the celebration to be held in the coming September (6th-11th) of the completion of the second decade of work at Clark University. The celebration takes the form of a series of lectures by distinguished men of science and a series of conferences on the pedagogy of science in various aspects. The lecturers announced in the Departments of Psychology and Pedagogy are as follows:

Professor Sigmund Freud, University of Vienna.
Professor L. William Stern, University of Breslau.
Dr. C. G. Jung, University of Zürich.
Dr. Leo Burgerstein, University of Vienna.
Professor Franz Boas, Columbia University.
Professor H. S. Jennings, Johns Hopkins University.
Professor Adolph Meyer, Johns Hopkins Medical School.
Professor E. B. Titchener, Cornell University.

Conferences will be held upon the teaching of psychology in normal schools, under the chairmanship of Professor Guy M. Whipple of Cornell University, and on the teaching of psychology in higher schools for the training of teachers (teachers' colleges), under the chairmanship of Professor Carl E. Seashore, of the University of Iowa. A more informal conference on Laboratory Equipment and Management with demonstration of apparatus from various sources will also be held. Conferences are planned by the Department of Pedagogy upon Education as a Collegiate Subject and on School Hygiene in various aspects. Detailed programmes will be issued during the summer.

THE SIXTH INTERNATIONAL CONGRESS OF PSYCHOLOGY, GENEVA, AUGUST 3-7, 1909

From the second circular of announcement we take the following items of interest.

The regular session of the Congress will begin, nearly a month earlier than usual, on Tuesday, Aug. 3d, and continue until Saturday, Aug. 7th, inclusive. On Monday evening, Aug. 2d, there will be an informal gathering of the members of the Congress already present in Geneva.

The modifications in the plan of work of the Congress outlined in the first circular (*vide* this *Journal*, *XIX*, 1908, p. 286) have met with general approval and will be put into effect as indicated in the general scheme below, the detailed programme following later.

I. Themes for Discussion

A certain number of widely interesting questions—several of outside suggestion — have been placed upon the programme for special discussion. The papers of those who have agreed to bring these questions before the Congress will be printed and sent to the members of the Congress beforehand in order that they may be read at leisure and that members may prepare for their discussion. They will at the time of the discussion be presented in abstract only. The questions and leaders are as follows:

A. General Questions.

1. The Feelings, presented by Prof. O. Külpe (Würzburg) and Dr. P. Sollier (Paris).

2. The Subconscious, presented by Profs. Dessoir (Berlin), P. Janet (Paris) and Morton Prince (Boston).

3. The Measurement of Attention, presented by Profs. Patrizi (Modena) and Ziehen (Berlin).

4. The Psychology of Religion, presented by Profs. Höffding (Copenhagen) and Leuba (Bryn Mawr).

B. Special Questions — to be discussed probably in sectional sittings.

Psycho-pedagogy

5. The Psycho-pedagogical Classification of Dullards, presented by Dr. Decroly (Brussels) and Profs. Ferrari (Imola-Bologna), Heller (Vienna) and Witmer (Philadelphia).

6. The Methodology of Pedagogical Psychology, presented by Mlle. la Dr. Ioteyko (Brussels).

Psycho-zoölogy

7. Tropisms, presented by Dr. Bohn (Paris) and Profs. F. Darwin (Cambridge), Jennings (Baltimore) and Loeb (Berkeley).

8. Orientation at a Distance, presented by Prof. Thauziès (Président de la Fédération des Sociétés Colombophiles de l'Ouest-Sud-Ouest (Périguex).

Psycho-physiology

9. Perception of the Position and Movements of the Body and of the Members, presented by Prof. Bourdon (Rennes).

II. Questions of Unification and Standardization

1. Terminology. By way of introduction to this subject and to elicit suggestions, the Committee will soon publish and distribute a preliminary list of terms proposed, covering those most frequently used in experimental psychology.

2. Standard colors. The Committee here invites the assistance of psychologists and others expert in the matters involved. It is hoped that Prof. Nagel (Rostock) may bring the question before the Congress.

3. *Method of Enumerating Errors in Experiments on Testimony.* This matter will be brought before the Congress by Dr. Lipmann (Berlin).

4. *Notation for the Age of Children.* The Committee recommends the adoption of the method used by Stern.

5. *Mathematical Treatment of the Numerical Results of Experiments.* If it is desired, an opportunity will be given for the presentation of papers on this subject; and other questions can be placed upon the programme by prompt communication with the Committee.

III. Exposition of Instruments and Similar Material

Those who propose to exhibit instruments, apparatus, books, pamphlets, collections or other material, and those who will require facilities for demonstrations should notify the Committee at as early a date as possible.

IV. Individual Communications

The purpose of the Committee has been to restrict these, *pro bono publico*, but they do not wish to exclude them absolutely. In response to demand already made, a section for Animal Psychology has been organized and communications are expected from Prof. Yerkes (Cambridge, Mass.) on Scientific Methods in Animal Psychology with a demonstration of apparatus, and from M. Hachet-Souflet (Paris) on the Theory and Psychological Applications of Training.

Those desiring to be enrolled as members of the Congress are invited to register at once with the Treasurer. The registration fee is 20 fr. covering, besides the opportunities of the Congress, the subscription for the Proceedings and all printed matter given out. Members of the families of those enrolling may obtain the privileges of the Congress, except the Proceedings and printed matter, at half-rates.

Committee of Organization

Th. Flournoy, President. P. Ladame, Vice-President.
Ed. Claparède, General Secretary. L. Cellérier, Treasurer.
E. Yung.

All communications, except those with reference to membership, should be addressed to the General Secretary, No. 11, avenue de Champel, Geneva. Communications with reference to membership and fees should be addressed to the Treasurer, M. Lucien Cellérier, Montchoisy, Geneva.

THE SIXTH ANNUAL MEETING OF EXPERIMENTAL PSYCHOLOGISTS.

At the invitation of Professor Howard C. Warren, the sixth annual meeting of Experimental Psychologists took place this year in the psychological laboratory of Princeton University, on April 8th, 9th, and 10th. Twenty-three psychologists were in attendance. The opening session was devoted to reports of work in progress in the chief laboratories. Of a somewhat more formal character was the demonstration by Prof. Leuba of his apparatus for the study of judgments of the extent of arm movements (described in an earlier portion of this number of the *American Journal*), the presentation by Mr. Geissler, of Cornell, of the results of his studies on the degrees of clearness in attention, the report of Prof. Hayes, of Mt. Holyoke, upon certain cases of color blindness (including one monocular case), and the summaries of Dr. Ferree, of Bryn Mawr, of his studies on spatial vision in the region of the blind-spot and on after-images and contrast due to subliminal stimuli. Mr. Clark, of Cornell, Mr. Woodrow, of Columbia, and Dr. Goddard, of the New Jersey State School for the Feeble Minded, reported respectively on the effect of distraction of attention on the apparent intensity of tones, the effect of variations in intensity and duration in the sounds in auditory rhythms, and the correlation between physical and mental development. Dr. Goddard also offered to the experimentalists opportunity for investigation in the institution to which he is attached.

Drs. Dunlap and Vaughan secured from the psychologists present an expression of opinion with reference to the suggested limitation of the scope of the *Psychological Index*, the prevailing sentiment proving to be against any limitation. In compliance with the invitation of Prof. Watson it was voted to hold the seventh meeting (1910) in the laboratory of Johns Hopkins University.

An agreeable social feature of the meeting was the Smoker given by Professor Warren at the Nassau Club on the evening of the 8th, which was followed by an informal gathering at the same place on the next evening. The personal hospitality of Professor Warren and his colleagues was also extended to many of the visiting psychologists.

We regret to record the loss to psychological science of the personality and trained productive capacity of Professor Hermann Ebbinghaus, of the University of Halle, who died on the 26th of February of pneumonia.

Prof. Ebbinghaus was born in Barmen, Jan. 24, 1850, and studied in the gymnasium of his native town and later in the Universities of Bonn, Halle, and Berlin. As his studies progressed he turned by degrees from history and philology to philosophy, and chose for the topic of his doctor's dissertation that of "Hartman's Philosophy of the Unconscious." Still later he applied himself to natural science and mathematics. The years from 1880 to 1894 he spent at Berlin as *Privatdozent* and extraordinary professor; he was called to Breslau as ordinary professor in 1894 and to Halle in the same capacity in 1905. While at Berlin he opened a psychological laboratory for research and for practice courses, and it was also while there that his classical experimental work on Memory was published (*Das Gedächtnis*, 1885). This work was a monument of patient and successful experimentation, and it was besides a demonstration that experimental methods could be applied with success to more complex fields than those of sensations and responsive movements.

In 1890 Prof. Ebbinghaus founded with Prof. Arthur König the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, now grown to publication in two series and flourishing in its 51st volume. In 1902 appeared the first volume of his *Grundzüge der Psychologie* (2nd ed. 1904 and a 3rd in prospect), and recently the first *Heft* of the second volume. The contents of this second volume is foreshadowed in his *Abriss der Psychologie*, a concise and less technical sketch of the whole field of psychology, proposed originally for Hinneberg's "*Kultur der Gegenwart*", but used there only in abbreviated form and later brought out complete as an independent work (1908: 2nd ed. 1909, English version from the abbreviated form by Max Meyer, 1908). Beside these major works, he had published a considerable, but not a large, number of briefer contributions of which the best known are, perhaps, his experimental study of brightness contrast, his theory of color vision and his new method of testing mental ability in school children and others.

His number of papers was not large because in all his work he added to the qualities of the skilful man of science much also of the artist's reverence for his work—a characteristic which Jaensch's sketch in the *Zeitschrift* (Bd. 51, Heft 1-2, from which the above historical items are taken) shows to have been fundamentally characteristic of the man, and which causes us to regret his loss the more.

THE AMERICAN
JOURNAL OF PSYCHOLOGY

Founded by G. STANLEY HALL in 1887

VOL. XX | OCTOBER, 1909 | No. 4

THE MEASUREMENT OF ATTENTION[1]

By L. R. GEISSLER

CONTENTS

PART I

A CRITICAL STUDY OF PREVIOUS VIEWS AND METHODS

As soon as the experimental method was carried into psychology and applied to mental phenomena, the fundamental importance of attention, which until then had not been generally recognized,[2] became convincingly manifest. There is, indeed, no field of psychological experimentation in which attention does not play a determining part, or, in Ribot's words, "attention is, in fact, the fundamental psychical condi-

[1] From the Psychological Laboratory of Cornell University.

[2] W. James: Princ. of Psychol. I, 1890, 402; Ebbinghaus: Grundz. d. Psychol., 2d ed., 1905, 610 f.; Titchener: Lect. on Feeling and Attention, 1908, 172-174.

tion of almost all psychometrical researches."[1] Results are widely different, according as they are or are not obtained under maximal attention. Hence the precise distinction between various degrees of attention becomes a matter of practical necessity. As such it has made itself felt in two ways. On the one hand, it is often desirable to know how certain results are influenced by the degree of attention, and the experimenter has consequently to use some means of inducing variations of attention, though he need not make any particular effort to measure them accurately. For such purposes two general methods have been mainly employed, the one consisting in distracting the observer from a certain task to which his whole attention is required, and the other demanding that he perform simultaneously two different mental or motor operations. On the other hand, certain investigations have been undertaken with the express purpose of discovering reliable methods and standards for the measurement of all possible degrees from maximal to minimal attention.

Our study of previous views and methods regarding the measurement of attention will naturally divide into three parts. First we shall take up the theoretical discussions concerning the nature of degrees of attention. In the second place, we shall review the general experimental methods usually employed for inducing different degrees of attention without attempt at their accurate measurement. Finally, we shall have to study the special methods and standards proposed or actually used for measuring attention. This, in general, is the programme which we have laid out for ourselves in Part I of the present study. In the second Part we shall submit experimental evidence for the possibility of an adequate measurement of attention in terms of clearness values.

Among the German psychologists, probably the first systematic reference to attention was made by Leibniz[2] in his distinction between perception and apperception. The difference between these two states of consciousness, as Wundt[3] has pointed out, was simply a matter of degrees of clearness. Wundt in accepting this important distinction seems almost surprised to find "how correctly Leibniz had already observed that there were no sudden or abrupt changes but rather gradual transitions between the different clearness degrees of the ideas."[4] Such a view implies, on the one hand, that there can be no absolute limit separating the low degrees of clearness included

[1] Ribot: The Psychology of Attention, Transl., 1896, 67.

[2] The Philosophical Works of Leibnitz. Tr. by G. M. Duncan. The Monadology, paragraphs 19-25, esp. 24.

[3] Wundt: Vorlesungen, 2d ed., 1892, 262.

[4] *Op. cit.*, 262.

in perception from the higher degrees of apperception, and, on the other hand, that there must exist an indefinitely large number of such degrees. A somewhat later and more explicit emphasis upon clearness as an essential factor in attention is found in Wolff's writings. He gives, for example, the following definition: "facultas efficiendi, ut in perceptione composita partialis una majorem claritatem ceteris habeat, dicitur Attentio." As an illustration he mentions the case of a man listening to a conversation so attentively that he entirely forgets his visual surroundings and does not notice the temperature of the room until his attention is called to either the one or the other, when he also loses the thread of the conversation. Many other passages of a similar character could be quoted from Wolff.[1] However, the significant fact for us is that, as Wundt says, "since Leibniz the distinction between clear and obscure ideas has remained an almost uncontroverted possession of psychology."[2] Certainly it was quite familiar to the German psychologists of the 18th century, as a few references from Dessoir will suffice to show.[3] Sulzer (1720-1779), for example, speaks of attention as "das Licht der Seele" and points out "dass durch vermehrte Aufmerksamkeit ein blosses Bild der Phantasie das Leben eines wirklichen Gegenstandes bekommen und hingegen der wirkliche Eindruck durch den Mangel der Aufmerksamkeit zu einer sehr schwachen Vorstellung werden kann." Hentsch (1723-1764) says: "diese Bemühung, auf einen Teil einer vorkommenden Sache mehr Acht zu haben, als auf die übrigen, nennen wir die Aufmerksamkeit"; while von Irwing (1728-1801) thinks that "die Klarheit und Lebhaftigkeit aber allein ist nicht hinlänglich, um eine Idee bis zur Apperception zu bringen."

The psychological writings of a later period, especially at the beginning of the 19th century, were too seriously influenced by the philosophical interests and theories of their authors to give unbiassed consideration to our topic. At the same time, we have already arrived at the "turning-point of modern psychology,"[4] for in Herbart's system the problem of apperception and attention begins to assume fundamental importance. His discussion of the limen of consciousness and of the rise and fall of competing ideas can easily be interpreted as referring to the increase and decrease of the clearness of ideas. This is particularly true of his *Psychologische Untersuchung über die Stärke einer gegebenen Vorstellung als Function ihrer*

[1] Psychologia empirica, 1732, par. 237; *cf.* par. 235-238, and 592; Psychologia rationalis, 1734, par. 357, 358, 360, 367, 369, 372.

[2] Wundt: Vorlesungen, 2d ed., 262.

[3] Dessoir: Gesch. d. neueren deutschen Psychologie, 1894, 235-237.

[4] Titchener: Exper. Psych., Vol. I, Part II, 186.

Dauer betrachtet.[1] While Herbart was the first to apply mathematical formulas to mental phenomena, he nevertheless thought it impossible to perform experiments upon human beings, and he thus implicitly denied that psychology could become an exact science. Lotze, on the other hand, with his expert medical knowledge and his deep psychological insight, helped indirectly to affiliate psychology to the other natural sciences. He seems to have been the first to introduce into modern psychological literature the analogy of attention and inattention to the Blickpunkt and Blickfeld of vision. He says: "the mind is not so constituted as to experience all its (simultaneous) contents with equal clearness and attention. It is rather to be compared to the retina of the eye with its singularly sensitive fovea around which a large area of symmetrically decreasing sensitivity is centred. Just as in the eye every peripheral point, in spite of its indistinctness, has nevertheless its definite position with regard to the more distinct centre, so in the course of ideas in the mind the more suppressed excitations centre around the clear focus of attention and, without disturbing the focal content, contribute to the greater richness of consciousness and to its peculiar moods or states of illumination."[2] Lotze also speaks of a "Steigerung der Aufmerksamkeit" which manifests itself in peculiar changes of the "Helligkeit gewisser Sinneseindrücke,"[3] a phrase which evidently refers to the clearness of the sensation, since the intensity of the stimulus is assumed to be constant.

With the beginnings of experimental psychology in the middle of the 19th century, a new emphasis was laid upon attention. Many years before Wundt had started his investigations with the complication pendulum and the tachistoscope, Fechner had been led by his quantitative study of the psychophysical relation to distinguish sharply the limen of sensible intensity from the limen as influenced by the degree of attention. As early as 1860 he wrote, for instance: "We can think or have ideas, weak as to their content, but strong as to their intensity. . . . Hence the intensity of the idea and the strength with which we think or perceive it must be in some way distinguishable from each other".[4] In 1877 he was still more explicit on the same point: "If we perceive a sensory phenomenon or represent it to ourselves in the form of an idea, the intensity of our conscious activity is then determined on the one hand by the degree of attention with which we perceive

---

[1] Herbart's Sämmtliche Werke, ed. by Karl Kehrbach, 1888, Vol. III, 121-145, esp. 123. *Cf.* Lehrbuch z. Psych., Werke VI, 140 ff.

[2] Lotze: Medicinische Psychologie 1852, 505.

[3] *Op cit.*, 510.

[4] Fechner: El. d. Psychophysik, 2d ed., Vol. II, 453.

the sensory or the memory image, and on the other hand by the vividness or intensity which pertains to the phenomenon itself. . . . In such cases we can quite well distinguish how much is due to the degree of attention and how much intensity belongs to the phenomenon as such''.[1] A very similar passage occurs in the *Revision:* "If I look at a more or less bright surface or listen to a more or less loud sound, then I can very well distinguish the degree of attention with which I perceive it from the degree of brightness or of intensity of the sound which I experience''.[2] He even goes further and adds: "the strength of the attention may decrease while the intensity of the phenomenon, as, *e. g.*, the brightness or sound sensation, may increase, and conversely''.[3] Now what else could such repeated emphasis upon the distinction between degrees of psychophysical intensity and degrees of attention indicate, if Fechner did not wish to differentiate as sharply as possible between intensity and clearness? Although it is true that he himself nowhere uses the term clearness in connection with degrees of attention, yet it is equally certain that there is no other sensational attribute than that of clearness which could be thus fundamentally opposed to the attribute of intensity. It is perhaps due to Fechner's avoidance of the term clearness that he did not state more explicitly the inevitable conclusion implied in these passages, that the degree or strength of the attention given to a certain mental process is equivalent to the degree of clearness which this process attains as compared with the other mental processes of the moment.

In this respect Stumpf seems to go beyond Fechner, for in the second volume of the *Tonpsychologie* there is a passage which indicates that for Stumpf clearness is not only essential to attention, but also that it increases as attention itself increases. He says: "the effect of attention in analysing is capable of an almost unlimited increase. An analysed clang and a particular tone in it may in all inner characteristics and external relations, in all respects and in all directions become more and more distinct''.[4] Here the terms "Steigerung'' of attention and "immer deutlicher werden'' are plainly synonymous. With regard to the psychophysical basis of attention Stumpf accepts and defends[5] Fechner's analogy of the wave form against G. E. Müller's criticism,[6] and quotes Exner[7] in his support.

---

[1] In Sachen d. Psychophysik, 1877, 85 f.
[2] Revision d. Hauptpunkte d. Psychoph., 1882, 270.
[3] *Op. cit.*, 270.
[5] Tonpsych. I, 1883, 70, footnote.
[4] Stumpf: Tonpsychologie II, 1890, 288.
[6] Müller: Grundl. d. Psychophysik, 1878, 359.
[7] Pflüger's Archiv XI, 1875, 429.

In Lipps' discussion of attention, its relation to clearness is made still more explicit. The function of attention is to "increase that energy of the excitations with which they are to gain independent consciousness",[1] and Lipps sharply distinguishes this increased mental energy from the stimulus intensity. In another passage he gives an admirable description or definition of clearness: "it is indeed possible for ideas to break their way into consciousness with greater or less strength, to remain there a longer or shorter time, and accordingly to make a more or less energetic impression, to enter into more or less close relations with other mental contents, to be more or less appropriated by the total conscious situation and thus to occupy in it a more or less dominating position".[2] The same thought occurs twenty years later in a more concise form in his *Leitfaden*, where he says, for example: "the real psychical fact which lies at the basis of attention may be defined as a more or less strong psychical effectiveness of the processes upon which the attention is directed",[3] or: "the clearness degree of a sensation or idea would then be only another name for its degree of effectiveness in the mental life".[4] "Instead of psychical effectiveness", Lipps writes elsewhere, "we may just as well say: psychical force. We speak of force when we refer to some effect or work done. And we measure the magnitude of the force by its effects. Thus the amount of attention directed to an ideated object is nothing but the psychical force of the idea of this object".[5] This means in other words that, in order to measure the attention given to a certain mental process, we have to consider its "psychical effectiveness", or better, the degree of clearness with which it appears in consciousness.

Külpe agrees with Fechner, Stumpf, and Lipps, when he writes: "It is a matter of familiar experience that the vividness or degree of attention is itself capable of quantitative gradation, and that the differences may be distinguished from differences in the intensity of sensations."[6] However, he is very sceptical as to the fruitfulness of the experimental methods employed in securing and measuring degrees of attention, and closes his discussion by saying: "The discovery of a reliable measure of the attention would appear to be one of the most important problems that await solution by the experimental psychology of the future".[7] Ten years later he prac-

[1] Lipps: Grundtatsachen d. Seelenlebens, 1883, 134.
[2] *Op. cit.*, 42 f.
[3] Leitfaden: 1903, 34.
[4] Leitf: 38.
[5] Leitfaden: 34.
[6] Külpe: Outl. of Psych., 1895, 441.
[7] Külpe: Outl., 429.

tically repeats this statement in the following passage: "It is clear that, if we are to induce a determinate degree of consciousness, we must know, first, the mode of distribution of the total energy of attention over the separate conscious contents included within its range, and, secondly, the distractive value of the process which we have selected for our purpose. Now no general rule can be laid down under either heading; the results that have been obtained hold only in particular cases; so that, from this point of view also, the investigation of attention is peculiarly difficult. So much we may say, and no more: that the distribution of attention brings with it a reduction, and concentration of attention an enhancement, of the degree of consciousness for a determinate content. Psychologists have not yet attained to any quantitative formulation of the changes".[1]

A similar complaint was made in the same year, as well as two years later, by Ebbinghaus: "A desirable aim for the study of the distraction phenomena would be the securing of exact and more or less generally valid figures. . . . However, it is clear that the prospects for accomplishing this aim under the described complication of things are not very bright".[2]

It is difficult to state correctly Wundt's attitude toward the problem of measurement of attention, since he makes no explicit references to it. As was said above, Wundt accepts Leibniz' distinction between clear and obscure ideas, between apperception and perception,[3] and he also seems to agree with Leibniz as to the possibly endless number of intermediate degrees between these two extremes. Since they are always accompanied by feelings of strain, the intensity of the feelings may, according to Wundt's earlier writings, serve as an indication of the probable degree of apperception or clearness. There seems here to be an ambiguity in Wundt's terminology, in as far as he uses synonymously *Spannungsgefühl* and *Spannungsempfindung*. But since strain, according to him, constitutes the main element of the *Thätigkeitsgefühl*, since in his later pluralistic theory of the affections it is one of the six dimensions of feeling, and finally, since he himself objects very strongly to Münsterberg's wide-reaching use of the muscular and other bodily strain sensations, it seems probable that even in his earlier writings he intended to classify strain among the feelings. It is then not surprising that in other psychological treatises, referring to Wundt's earlier writings, the terms sensation and feeling of strain are likewise confused. Mentz, for

[1] The Problem of Attention, Monist XIII, 45.
[2] Ebbinghaus: Grundz. d. Psych., 2d ed., 1905, 623 f.
[3] Wundt: Vorlesungen, 2d ed., 262.

example, uses as a basis for a number of experiments the following passage in Wundt: "Those strain sensations which, with the same attention, accompany the external volitional act as well as the direction of the will to the particular sense departments, form a complex of qualitatively related sensations which is characterized by a constancy attained by no other conscious content. These sensations are always present, —which cannot be said of any other sensory process; and through their various degrees of intensity they offer at the same time a direct measure of the energy of any given volitional activity".[1] Mentz, accordingly, gives his observers the following instruction: "The reactor is required voluntarily to increase his attention in equal degrees, as far as this is possible, and to report afterwards what particular degree he has given", and goes on to say: "Wundt has already remarked that the strength of attention may be measured by the intensity of the strain sensations".[2] Again, Wundt says, after speaking of the relation of association to active and passive apperception, that "in both cases the apperception, as a process which differs from the mere course of ideas, comes to consciousness partly by means of the accompanying feelings and partly by means of the accompanying strain sensations whose intensity increases with the degree of attention".[3] This leads Pillsbury to make the statement that, according to Wundt, "the feeling of activity and the sensations of strain are most intensive when the ideas are clearest. Increase in the clearness of ideas and in intensity of strain sensations, etc., always go on side by side".[4] Hence Mentz was justified in his interpretation of Wundt, and on this basis his observers distinguished, according to his tables, in the one case four and in the other case six degrees of voluntary attention. Unfortunately, Mentz does not give any further description of these degrees and adds only incidentally the remark "that just this increase (probably of strain) with the higher degrees was rather difficult to accomplish".[5] Wundt himself, in the 5th edition of the *Physiologische Psychologie*, has made a sharp distinction between the feelings and the sensations of strain. He now connects the feelings of strain with the intensity of the sensory processes attended to, because they inform us of our correct bodily accommodation to the external stimulus and thus indirectly help us to bring it to its full intensity. He says: "Furthermore we notice that the degree of the feelings of strain

[1] System d. Philos., 1889, 100.
[2] Mentz: Phil. Stud., XI, 1895, 574.
[3] Grundz. d. physiol. Psych., II, 4th ed., 1893, 279.
[4] Pillsbury: *Amer. Jour. Psych.*, VIII, 1896, 279.
[5] Mentz: *op. cit.*, 576.

keeps step with the intensity (*Stärke*) of the impressions. Upon the exactness of the occurring accommodation depends what is called the keenness (*Schärfe*) of attention".[1] By this keenness he means simply the accuracy of the sense organ involved. After all, then, we are still left in the dark as to Wundt's attitude toward the problem of measuring attention. It seems hardly possible that he would deny the existence of such a problem, although his refusal to make any direct reference to it or to the large literature in question might perhaps be interpreted in this way. However that may be, this much is certain: Wundt at least admits a large number of degrees of attention and makes them equivalent to degrees of clearness ranging all the way from the highest level of apperception to the lowest level of perception. On this point he agrees with all the other German authors quoted thus far; in fact, his insistence upon clearness as the essential characteristic of attention has become the central aspect of his whole system of psychology.

In Münsterberg's treatment of attention great stress is laid upon kinæsthetic strain sensations. They are said to arise not only in the muscles involved in the adjustment of the particular sense-organ, but also in the muscles of the head, the neck, and the chest, especially in the muscles involved in respiration. Münsterberg holds that "the greater the demands that are made upon the efficiency of the sense organ, the more intensive will all these strains have to be, for the adaptation (*Einstellung*) takes place the more exactly, the stronger the tension in the antagonistic muscles. . . . The intensity of the strain sensations is therefore generally accepted as a measure of the strength of attention; thus the latter may be increased considerably while the impression itself remains constant".[2] Külpe, however, makes the criticism, which is based upon introspective evidence, that "there is no simple or necessary proportionality between the intensity of strain sensations and the degree of attention".[3] This objection cannot be satisfactorily answered, and therefore Münsterberg's criterion for degrees of attention is psychologically impossible.

The earliest and perhaps the only systematic French treatise on attention is written by Ribot. He also admits the existence of many degrees of attention, and even attempts their graphical representation. He says: "graphically we might represent the totality of its normal and morbid manifestations by a straight line. . . . At the centre, let us put ordinary

[1] Grundz. d. physiol. Psychol., 5th ed., III, 1903, 337.
[2] Münsterberg: article on Aufmerksamkeit in the Real-lexikon der Medic. Propädeutik by Joh. Gad, Vol. I, 1893, col. 532-539.
[3] Külpe: Outlines, 428.

spontaneous attention. Following our imaginary line to the right, in the direction of increasing attention, we find strong spontaneous attention, then preoccupation, then the weak fixed idea. . . . Reverting to our starting point, we now turn to the left, in the direction of decreasing intensity. Here we have voluntary attention, at first in the form of an organized habit, then in its general ordinary form, then vacillating. . . . Between each form and its adjacent ones, there occur shades which I omit to notice''.[1] As to the nature of these "shades", Ribot is thus not very explicit; but we can easily infer from the passage quoted as well as from many other statements that he must have had in mind differences in clearness. This is, for instance, evident from the following statement: "I am, accordingly, fully inclined to hold, with Buccola, that the fixed idea is attention at its highest degree—the extreme limit of its power of inhibition".[2]

Much less unanimity of opinion both as to the number and the nature of degrees of attention exists among the English and American psychologists. Ward, for instance, seems to agree with Ribot and the German school, although he, too, is not very explicit. He distinguishes between "the intensity or concentration of attention and its diffusion or the extent of the field of consciousness".[3] Still, he makes "loss of intensity" correspond to loss of "distinctness" in the following passage: "if, then, attention be distributed over too wide a field, there is a corresponding loss of intensity, and so of distinctness". He also seems to imply a scale of gradual changes "between the two zeros of complete indifference and complete absorption". Practically at the same time Sully published his first works on psychology. As he was one of the first English systematic writers to give attention an independent place, it is interesting to find him also in agreement with the Germans so far as degree of attention is concerned. He says, for instance: "the field of consciousness, however, is wider than that of attention. Consciousness admits of many degrees of distinctness. . . . To attend is to intensify consciousness by concentrating or narrowing it on some definite and restricted area. It is to force the mind or consciousness in a particular direction so as to make the objects as distinct as possible".[4] However, since he thinks it best "to reserve the term attention for the more palpable exertions of mental activity in definite directions",[5] his terminology is different from that of previously

[1] Ribot: The Psychology of Attention, 104-105.
[2] *Op. cit.*, 86. Other evidences are quoted in Titchener's Lectures, [184.
[3] Ward: art. on Psychology in the Encycl. Brit., 1886, 70.
[4] Sully: Outlines of Psychology, 2d ed., 1885, 73.
[5] *Op. cit.*, 75, footnote.

mentioned writers. Especially the terms activity and energy play an important part in his later treatments of attention, as may be seen from the following passage: "the amount of attention exerted at any time depends on two chief circumstances, (*a*) the quantity of active energy disposable at the time; (*b*) the strength of the stimulus or force which excites the attention or rouses it to action. If there is great active energy a feeble stimulus will suffice to bring about attention. The healthy, vigorous child in the early part of the day has a superabundance of energy which shows itself in attention to small and comparatively uninteresting matters. On the other hand, a tired or weakly child requires a proportionately powerful stimulus".[1] Here the reference to clearness is replaced by a strong emphasis upon the hypothetical mental energy.

Practically the same thing holds of Ladd's discussion of the nature of attentive degrees. He begins by following Wundt, for example when he writes: "that changes in the clearness of perception take place in dependence on the changes in the degree of attention is a matter of the most ordinary experience,"[2] or: "by gradual increase in the intensity of attention (changing the casual glance into a steady look) these objects become apperceived more and more clearly."[3] However, he adds to this plain statement of a psychological fact the physiological hypothesis that "under the influence of attention the cerebrum has become more susceptible for certain impressions, less so for certain others. Stored energy of the nerve cells is being rapidly called forth. Concentrated voluntary attention implies a large amount of work being done in the cerebral hemisphere."[4] Hence he concludes in a somewhat puzzling way that "by the amount and speed expended in attention we measure in a large degree the extent and intensity of consciousness."[5] According to Ladd "primary attention, essentially considered, is the variously related degrees of psychic energy expended upon the different aspects, elements, and objects, in the one field of consciousness."[6] The admission "of an indefinite number of degrees of (conscious) energy" he thinks leads one to ask, for instance, "how much can I feel without losing consciousness?" or "how vivid can my memory-image or picture of imagination become as measuring the utmost capacity for vividness which my conscious mental life can display in this way?"[7] These questions again refer to at-

[1] *Op. cit.*, 79.
[2] Ladd: El. of Physiol. Psych., 1889, 541.
[3] *Op. cit.*, 541.
[4] *Op. cit.*, 543.
[5] *Op. cit.*, 544.
[6] Psych. Descr. and Explan., 1894, 74-75.
[7] *Op. cit.*, 42.

tention as essentially characterized by clearness, and thus we find Ladd's discussion of attention wavering between two different standpoints.

According to Baldwin, the whole area of consciousness may be subdivided into five levels or degrees. The lowest is beyond consciousness; it is the physiological region of the Unconscious. Then follows the Subconscious, next Passive or Diffused Consciousness, then Active Consciousness or Attention, and finally the level of Apperception.[1] In another passage he likens attention to "the line of mental vision," or to "the visual field in which objects are scattered, those being most clearly seen which are in the line of direct vision or centre of the field. Between these limits," he immediately adds, "there are all degrees of distinctness."[2] This last sentence would seem to imply that each of the four or five levels must itself include a large number of lesser differences, although perhaps no hard and fast distinction could be drawn between them. As to the nature of these degrees we are left to infer that they are differences of clearness. Such an implication is for instance made in his discussion of the relation of attention to the intellect, where he says: "in general, it may be said that attention increases the vividness of representative states,"[3] and lower down on the same page: "the capacity to retain mental pictures depends upon the intensity of the original presentation, and the clearness of its relations; and this intensity and clearness are enhanced by the attention." However, in Baldwin's later writings the clearness factor is entirely disregarded, a fact due probably to the dynamogenetic point of view. In this respect he is not followed by J. R. Angell, who otherwise seems to accept Baldwin's view as expressed in his earlier discussion of attention, even reprinting the diagram of the five concentric circles which represent the different levels of consciousness.[4]

The latest systematic treatise on attention, that of Pillsbury, again emphasizes the clearness factor. He admits various degrees of attention, and identifies them with degrees of clearness: "increase in the degree to which an impression is conscious and increase in attention to that impression are synonymous."[5] In his sixth chapter he discusses in some detail "the Methods of Measuring Attention" and makes special reference to the experimental work on this problem, which we, too, shall have to consider later on. For the sake of com-

---

[1] Baldwin: Senses and Intellect, 1889, 68.
[2] *Op. cit.*, 64.
[3] Baldwin: *op. cit.*, 75.
[4] Angell: Psychology, 1904, 65-67.
[5] Pillsbury: Attention, 1908, 2.

pleteness we should mention in this place Titchener's latest discussion of our topic, but since it also is intimately connected with the experimental work upon attention we reserve it likewise for later treatment.

To sum up our review of the theoretical discussions concerning the nature and number of degrees of attention, we find that most of the writers, as far as they are explicit on this point, agree that there are an indefinitely large number of such degrees, and that the degrees consist in differences of clearness of the conscious processes attended to or distracted from.

We are now ready to pass to the experimental work upon different degrees of attention. Here we have, first of all, to distinguish between the general means of inducing variations of attention on the one hand, and the special methods proposed or used for the sake of standardising or accurately measuring these variations on the other hand. Of course, all the investigations here in question had to employ some means of varying the attention; but some of them did not go beyond this, simply because their main problem lay elsewhere. Nevertheless, in our present discussion of the various means of inducing different degrees of attention, we shall not confine ourselves to this class, but shall include also those of the other class, because this procedure will enable us to reduce unavoidable repetition of references to a minimum. Later on, when we come to discuss the special methods of standardising degrees of attention, the investigations of the first kind will naturally have to be omitted.

The main question for any one who attempts to vary attention is this: How can an observer be induced (or forced against his will) to give less than maximal attention to a prescribed task? The choice actually made among the numerous possibilities that offer themselves has depended partly upon the nature of the given task, and partly upon the main purpose of the particular investigation. Some of the authors have found it necessary to use various means. Nevertheless, in general, all procedures may be divided, as has been said in the introduction, into two great classes, which we may briefly characterise as the "single-task" method and the "double-task" method, sometimes called the method of distraction and the method of simultaneous activities. These two methods are intimately connected with the basal fact of consciousness that at any given moment only a limited number of mental processes may be attended to, that is, may rise to focal clearness, while other simultaneously present processes occupy the relatively obscure background. In the single-task method, as the name implies, either the processes constituting the prescribed task are to be attended to, while other processes, constituting what is usually called the distraction, are artificially introduced in the background in order to reduce indirectly by their strength and number the attention given to the focal processes of the task; or, more frequently (and especially in the later experiments), the mental processes of the distraction are made focal while the original task is relegated to the background. In the double-task method, on the other hand, two sets of mental processes are introduced which are intended to occupy simultaneously the focus of consciousness, while the background processes are not taken into account. Külpe was the first to suggest this distinction, when he said: "experience shows that there are but two ways, in the normal waking state, by which this end may be accomplished. We may either distract the attention, or we may divide it".[1] In both cases, the variation of attention is inferred from the change in quality or quantity (or both) of the task performed.[2]

The single-task method is, so far as our knowledge goes, chronologically the older of the two. It seems to have been first employed by Wundt[3] and Obersteiner[4], as early as 1874. Obersteiner, who attempted to measure attention by means of the reaction time, used auditory, cutaneous, and visual distractors. He says: "I placed a musical box which played softly in his vicinity", or "I applied a tolerably strong induction current to the left arm", while in other series the reactor "looked into a kaleidoscope with changing figures".[5] Obersteiner assures us that his observer "always endeavored to abstract from the disturbing ifluences and to concentrate his attention on the reaction".[6] A few years later, Boas, in connection with his work on the difference limen, reported experiments on the determination of the difference limen of brightnesses in which "the attentive listening to a piece of music" was used as a distraction. Here, then, we notice for the first time

[1] Külpe: The Problem of Attention; The Monist, XIII, 1903, 44 f.

[2] Sometimes the term distraction has been applied to one of the two tasks which had to be attended to simultaneously. Or, the double-task method has been called the method of simultaneous activities. This has resulted in a good deal of confusion, since the single-task method likewise involves simultaneous activities, the difference consisting mainly in the direction of the attention. Therefore we have thought it better to employ the new terms "single-task" and "double-task" method as indicating the essential difference between the two, instead of following Külpe and calling them the methods of distracted and distributed attention.

[3] Wundt: Grundz. d. physiol. Psychol., 1st ed., 1874, 745-749.

[4] Obersteiner: Virchow's Arch. f. pathol. Anat. and Physiol., LIX (N. S. IX), 1874, 427-458.

[5] Obersteiner: Brain I, 1879, 447 f.

[6] *Op cit.*, 449.

that the observer did not attend to the original task of brightness discrimination, but to the distraction, so that the former was relegated to the periphery or background of consciousness.[1] Boas' results were known to Stumpf, to whom they perhaps suggested a new method of standardizing the different degrees of attention, which we shall later discuss in detail under the heading: method of graded distractors. The first criticism of sensory distractors, as unable to reduce indirectly the attention given to a prescribed task, was made by Cattell in 1886, in his investigation of the influence of degrees of attention upon reaction-time. He "let three metronomes beat and ring rapidly",[2] and he found that "the attention can be more thoroughly distracted if the brain is busied with some other operation while the reactions are being made. A good way to accomplish this is to let the subject beginning with any number add as rapidly as possible 17 after 17 to it".[3] It remains doubtful even here whether in the few moments preceding and including the reactions the subject's attention was not quickly withdrawn from the addition-task. Münsterberg has used both the single-task[4] and the double-task[5] method, employing as a rule addition as a distractor. In some of Bliss' experiments it was desirable to note the influence of attention upon the task of tapping, and therefore "an effort was made to distract the attention of the person tapping" by weak sounds, but without effect, while on the other hand "the blowing of a loud whistle was followed by a great irregularity", as happened also with "the mental addition of 214 and 23" and the "mental multiplication of 14 by 5".[6] Swift reports that muscular reaction-times were lengthened "while a metronome was ticking one hundred and twenty times each minute".[7] He also made some experiments "to find how the simple 'muscular' and the 'choice' reactions would vary while the reactor's attention was directed to certain kinds of work. Three tasks were given: 1, repeating a poem already committed to memory; 2, reading an English book; and 3, reading Kant's *Kritik der reinen Vernunft*. . . . The instructions were to fix the attention as closely as possible on the work assigned".[8]

In the years from 1895 to 1900 there appeared a relatively large number of experimental articles bearing upon our problem. The single-task method was mainly used in the in-

---

[1] Boas: Pflüger's Archiv, XXVI, 1881, 496.
[2] Cattell: Mind, XI, 1886, 237.
[3] *Op. cit.*, 238.
[4] Münsterberg: Zeits. f. Psychol., I, 1890, 104.
[5] Münsterberg: Beiträge z. exp. Psychol., IV, 1892, 200.
[6] Bliss: Stud. fr. the Yale Psychol., Lab., I, 1893, 50.
[7] Swift: *Amer. Jour. Psychol.*, V, 1893, 5. [8] *Op. cit.*, 17.

vestigations of Hamlin, Moyer, Birch, and Darlington and Talbot, done under Titchener's direction in the Cornell laboratory. Hamlin,[1] repeating some of Münsterberg's[2] experiments for the purpose of measuring the effect of attention upon sensible intensity, found that addition as the distracting task could not be used effectively together with judging small differences of intensity, brightness, and pressure, because "the subjects usually found that it acted as a spur rather than as a check to the attention. Their attention was wholly absorbed in the addition for a part of the interval, but as a rule the distraction was not continuous, and there came free instants in which the stimulus flashed into consciousness with great clearness and distinctness. Although the subjects were instructed to give their attention to the addition, the fact that they must give some judgment of the stimuli made them instinctively attentive to the stimulus, whenever there was any break in the continuity of the distraction."[3] In Moyer's experiments the distractions to be attended to were, besides the mental addition of three-place figures, "writing the words of a sentence in reverse order, writing the letters of a word backward, translation of simple sentences into a foreign language and writing the words and letters in a reverse order, and discrimination of odors."[4] Birch[5] continued and extended the study of odors as distractors, while Darlington and Talbot used musical "phrases of nine notes each, played upon the piano. Al were written in $\frac{2}{4}$ time and in the key of C. Five different octaves were used, the notes ranging from the c^{-1} to the c^{4}". Important work on attention has also been done by de Sanctis, who in some of his experiments aimed at a measurement of the intensity of concentrated and distributed attention. He required the observer "to occupy himself with a (tapping) movement which had to be made simultaneously with the beats of a metronome and under total concentration of attention. . . . While the observer was thus busy, certain distractions were induced in him, being made progressively stronger and more numerous; at the same time he was required to close an electric current whenever the distraction intruded into the focal consciousness."[7] In order to study the variations of attention under the influence of different conditions, Binet and Henri[8] have used as a distraction two metro-

---

[1] Hamlin: *Am. Jour. Psychol.*, VIII, 1896, 1-66.
[2] Münsterberg: Psychol. Rev., I, 1894, 39-44.
[3] Hamlin: *op. cit.*, 49.
[4] Moyer: *Amer. Jour. Psychol.*, VIII, 1896, 411.
[5] Birch: *Amer. Jour. Psychol.*, IX, 1897, 45-55.
[6] Darlington and Talbot: *Amer. Jour. Psychol.*, IX, 333.
[7] Sante de Sanctis: Zeits. f. Psychol., XVII, 1898, 209.
[8] Binet et Henri: L'année psychol., III, 1897, 237.

nomes of slighly different rates, "par exemple 50 et 60 à la minute," with the attention on the original task, while Toulouse and Vaschide[1] employed odors for the same purpose.

After 1900 the interest in the problem of measuring the attention seems to have somewhat relaxed. The experimental work was, however, continued by Wirth and Peters. The latter attempted to find a measure of attention in the rise and fall of the stimulus limen, and used the single-task method in order to eliminate the *Einstellung* of the attention to a liminal stimulus. "*Einstellung* to the liminal stimulus," he says, "is lacking in the experiments in which the observer had to concentrate upon one of the following four contents: reading, mental arithmetic, reading Hungarian, pulling the spring in a spring balance. . . . In these experiments not only is the *Einstellung* lacking, but we have also a new phase, the concentration upon a different content from the expected liminal impression."[2] There remains only to be mentioned Ebbinghaus' recommendation of using "geeignete Hauptleistungen und geeignete Störungen"[3] in order to obtain a more exact measurement of the attention.

The double-task method seems to have been much less frequently employed in psychological experiments. Perhaps the first use of it was made by Loeb[4] in 1886. His aim was to find out whether muscular activity could be used as a measure of mental activity. One of his two tasks was to give "the maximum pressure which the flexor muscles of the hand can exercise upon a dynamometer," while the observer was reading or making multiplications of different degrees of difficulty. His work was continued and extended several years later by J. C. Welch, who correlated the muscular strength of the hand with many different mental activities,[5] for instance, visual and auditory perception and the registration of rhythmical pendulum movements, mental adding and multiplication, reading, writing, and repeating poetry. Münsterberg has frequently and for various purposes used adding simultaneously with other kinds of observations, but he has also employed, especially together with large arm-movements,[6] such tasks as reading, counting letters, and fixating dots. Smith investigated "the relation of attention to our power of associating and recollecting objects presented to our consciousness", and

[1] Toulouse et Vaschide: Compt. rend. de soc. de biol., XI, 1899, 965.
[2] W. Peters: Arch. f. d. ges. Psychol., VIII, 1906, 406.
[3] Ebbinghaus: Grundz. d. Psychol., I, 2d ed., 1905, 623.
[4] Loeb: Arch. f. d. ges. Phys., XXXIX, 1886, 592.
[5] Welch: Amer. Jour. of Phys., I, 1898, 283-306.
[6] Münsterberg: Beitr. z. exper. Psychol., IV, 1892, 200.

found[1] among other results that a "distinct but not too complicated device for securing what one may call mental distraction (and what he elsewhere better terms "variation of attention") consisted in requiring the subject, while learning the letters on the card (for the sake of memorizing them) to perform a simple sum in addition, viz., that involved in repeating the sum 2, 4, 6, 8, . . . or rarely, when the former series tended to become too mechanically easy, the series 3, 6, 9, . . . In order that an effective control over the behavior of the subject might be secured, he was required farther to repeat the numbers aloud and to make each step in the addition coincide with the stroke of a metronome beating at the rate of 60-70 strokes per minute."[2] In Drew's experiments the observer had to make reactions "on five and ten-fingered keyboards" together with reciting the "multiplication table up to 12×12, . . . reading text (1) silently, (2) aloud, naming letters (1) in direct order in text, (2) in reverse order, (3) on drum, seen one at a time. . . . Reaction made by finger key and with mouth key; to be made in shortest possible time."[3] As to the purpose of this series of experiments, it is interesting to note that Drew says: "its object was to arrange a series of tasks of increasing degrees of complexity which should make ever greater demands on the mind, until the attention should pass from a fully concentrated to a completely distracted state."[4]

Perhaps the most frequent use of the double-task method has been made by Binet and Henri. For instance, they required an observer to press rhythmically a rubber bulb connected with a Marey-tambour tracing on a kymograph. At the same time he was asked to perform some difficult mental task such as adding, reciting, or reading aloud.[5] In another case, Henri[6] required his observers to read and write simultaneously two different pieces of prose. In this way Binet and Henri tried to find an answer to the question "how can we know whether a person gives much or little attention to a certain task"[7] Quite recently the double-task method has received a very strong impulse from McDougall, who has designed a new apparatus for this special purpose. In his description of its possibilities he says: "in studying the concurrent or intercurrent performance of two different mental operations, the

[1] W. G. Smith: Mind, N. S. IV, 1895, 50.
[2] *Op. cit.*, 50.
[3] Drew: *Amer. Jour. Psychol.*, VII, 1895, 533.
[4] *Op. cit.*, 533.
[5] A. Binet: Rev. philos., XXIX, 1890, 138-155.
[6] Henri: L'année psychol., III, 1897, 232-278.
[7] Henri: *op. cit.*, VI, 1900, 250.

subject is first set to mark the spots while the cylinder rotates at such a rate that he easily puts his black mark on each red spot. The rate of rotation is then increased step by step until a maximal effort of attention alone enables the subject to mark each spot. He may then be set any other task that does not involve the use of his eyes or of his right hand, *e.g.*, the addition of figures dictated to him, the counting of irregular taps, the discrimination of two points touched on the skin, the performance with the fingers of the left hand of simple or compound reactions to auditory signals."[1] Unfortunately, McDougall himself has not as yet published any results obtained with this instrument, although his "Preliminary Communication" appeared in 1905. Finally, the double-task method reached the climax of its development in some of Wirth's experiments which we have discussed at length in a previous paper.[2] The observer had, for instance, to fixate a given point, to distribute his attention in various ways over the whole field of vision or over parts of it, and at the same time to expect and react upon a brightness change liable to appear in any part of the visual field. Thus the double-task method has, after all, been applied to a sufficiently large number of mental and physical activities to allow a fair comparison with the more frequent single-task method.

However, a true estimate of the relative value of the two methods based solely upon the results obtained with them is hardly possible, because in both cases the results depend very largely upon the effectiveness of the distraction material employed. Aside from this, the two methods are probably about equally well suited to serve the general purpose of varying the attention to a given task. If either, the single-task method has the advantage, because it is more like our everyday experience, where as a rule one set of mental processes occupies for some time the focus of consciousness, while others are forced into the background. Some authors, it is true, think that the power of distributing attention is a sign of greater intelligence,[3] but to take up this question would lead us too far from our problem.

We return, then, to a criticism of the effectiveness of the distraction-material. One reason for the ineffectiveness of a given task, whether attended to alone or together with another task,

---

[1] McDougall: Brit. Jour. Psychol., I, 1904, 438.

[2] Geissler: A Critique of Professor Wirth's Methods of Measurement of Attention. *Amer. Jour. Psychol.*, XX, 1909, 120-130.

[3] S. de Sanctis, *op. cit.*, 213; "dass das Vermögen, die Aufmerksamkeit zu verteilen, in der Psychologie eine höhere Bedeutung hat als das, sie zu fixieren."

is a lack of perfect continuity. It allows brief moments of interruption, during which the other task may rise to the focus, and thus be performed about as well as if it were done by itself. This holds especially in the case of sense-discriminations and reactions, which can be accomplished almost instantaneously. Another defect is that frequent repetition of the same task leads to habituation, that is, it involves less and less consciousness, and of course less and less attention; thus more attention can be given to the other task. Easy mental arithmetic and easy prose reading as well as many muscular activities are especially subject to this criticism. Finally, the complaint has sometimes been made that the distractions do not affect all normal subjects in exactly the same way. This is perhaps the main fault with odorous stimuli, although otherwise they appear to have more distracting power than any other source of distraction examined. Thus far it seems to have been impossible to find a task which is not open to one or other of these three criticisms. As Külpe says: "it is clear that, if we are to induce a determinate degree of consciousness, we must know, first, the mode of distribution of the total energy of attention over the separate conscious contents included within its range, and secondly the distractive value of the process which we have selected for our purpose. Now no general rule can be laid down under either heading; the results that have been obtained hold only in particular cases".[1]

The fact then that the means of varying the attention are themselves but little reliable is one of the reasons why several of the attempts at standardising the degrees of attention have thus far failed to give satisfactory results. However, there are many other reasons, which it will be best to discuss in connection with the various methods that have been proposed for, or employed in, the measurement of the attention. For the sake of systematic treatment we have divided all the methods into six groups. The first five may be classed together as methods of expression, analogous to the physiological methods employed in the investigation of the affective processes. According to these five methods the degrees of attention may express themselves in changes of 1, peripheral vision; 2, muscular strength; 3, liminal and differential sensitivity; 4, reaction-time; 5, accuracy of work. The sixth method may be likened to the method of impression. By a series of graded distractors different degrees of attention are to be induced in the observer, and he is afterwards to report which degree he experienced. We shall now discuss these methods in the order thus presented.

[1] Külpe: The Monist, XIII, 1903, 45.

I. The fact that *Peripheral Vision* is changed and narrowed under concentrated attention seems to have first been mentioned by Stanley Hall, in 1883. He says: "if we concentrate attention upon an image at the centre of the field of vision, its peripheral tracts seem to grow dark, as indeed does the centre itself with some observers, when the attention is fixed on a point in indirect vision"[1] Experimental evidence for this fact was submitted by Janet, who "concluded from it that the nature of the point fixated by the subject and the degree of attention she paid to it played a part in the extent of the visual field".[2] To quote him in full: "it is for this that we undertook a series of researches conducted as follows: At the centre of the apparatus, on the central point, we fasten a piece of paper on which, according to the case, some sentences or numbers are written very small. We place the subject in the proper position to measure her visual field; we close one of her eyes and request her not only to look at the centre but also to read the paper or mentally to make up a sum with the numbers written on the paper. When her attention is well fixed on the work, . . . we draw the stick on which the small white object is fastened over the perimeter from the external side of the eye that is being examined, moving from the periphery to the centre. We stop a few moments at the point which we know to be within the subject's field, 40°, for example. . . . We succeed thus in determining the subject's visual field during the fixedness of her attention. . . . With many normal men these conditions do not modify the visual field; with some they diminish it from 5° to even 10°; but with hystericals, and in general with patients whose attention is modified, this process brings with it surprising contraction".[3] Although Janet did not find a uniform contraction of the visual field with normal observers, he nevertheless thinks "that the phenomena of attention do not depend solely on movements (he probably has Ribot and perhaps also Münsterberg in mind), as has been said, for the modifications of the visual field cannot be connected with movements".[4] Instead, he would rather relate these contractions to "the power of personal perception", *i. e.*, the limited range of attention. "In short", he continues, "it would perhaps be possible to establish here the effort of attention in order to synthetize phenomena".[5] His facts were confirmed, as he himself points out, by Séglas, who had "established them independently, not aware of this first communication".[6]

[1] S. Hall: Mind, VIII, 1883, 180.
[2] P. Janet: The Mental State of Hystericals. Tr. by Corson, 1901, 71.
[3] *Op. cit.*, 71-73.
[4] *Op. cit.*, 73.
[5] *Op. cit.*, 73.
[6] *Op. cit.*, 71.

However, Wirth's recent experiments[1] do not seem to corroborate them. Wirth, it is true, did not investigate the extent of the visual field under various degrees of attention, but tried to establish changes in the difference limen in the peripheral regions of the field; nevertheless, it seems reasonable to expect a close relation between a higher limen and a narrower field. Unless, then, Janet's results are verified by other experiments repeating his conditions, there is no reason for assuming that degrees of attention can be expressed by the range of peripheral vision. Even if such a correlation could be established, it would still, although valuable in itself, be limited entirely to visual attention, and new methods would have to be found for other departments of attention.

II. *Muscular Strength* exerted on a dynamometer is supposed to express degrees of attention according to J. Loeb and his pupil J. C. Welch. They assumed, in the first place, that, "if P be the maximum pressure with simultaneous mental work, the more attention the mental work requires the larger P-p becomes", and in the second place, that "if . . . P is the expression of the attention in the case that the whole available attention is given to muscular work, the expression $\frac{P-p}{p}$ represents the constant of attention for one kind of mental work".[2] In other words, "the maximum static innervation of the hand is arbitrarily selected here as a measure of other voluntary activities simultaneously attempted", so that "the determination of the constant of attention of various kinds of mental work affords a method of comparing the degree of concentration of attention required in different kinds of activity".[3] Thus the whole investigation depends upon the correctness of the assumption that "P is the expression of the attention". Loeb must certainly have known that static muscular pressure or "the maximum static innervation" is not simply a function of the attention, not even *mainly* a function of the attention. Does he, then, assume that the other factors involved, especially the physiological factors, remain constant and are, therefore, negligible? At least he fails to take them into consideration, while he makes no analysis of the whole situation which, from the nature of the task, must have been very complicated. His argument seems to rest upon the fallacy that attention is analogous or even equivalent to physical energy and may, therefore, be added to or subtracted from bodily strength and thus calculated in terms of kilogram-seconds. Another objection to Loeb's earlier work was raised by Mün-

---

[1] Wirth: Psychol. Stud., II, 1906, 30-88.
[2] Welch: Amer. Jour. of Physiol., I, 1898, 283.
[3] *Op. cit.*, 288.

sterberg[1] in 1888, but Welch makes no reply to it, as she systematically disregards the psychological aspect and literature of her problem.

III. *Sensitivity*, *Liminal and Differential*, has been proposed and used as a measure of attention by several psychologists. The original suggestion probably came from Fechner's work upon liminal intensities. The first experiments were made, upon Kraepelin's proposal, by A. Bertels in 1889 with the purpose of measuring not so much attention as distraction. Bertels used liminal brightnesses. However, his stimuli were not given simultaneously, but at determinate intervals of variable length, lasting from several seconds to fractions of a second. Besides, his assumption that "a stimulus, in order to reach a liminal value, must be the greater the less concentrated is the attention to it",[2] was not at all based upon experimental evidence, but simply taken for granted. Kraepelin himself did not seem satisfied with the results, for in 1896 he wrote: "the degree of concentration of the attention can be measured by determining the just noticeable stimulus intensity. However, such measurements, except where quite rough disturbances are employed, are so difficult to make and depend upon so many other presuppositions that, in spite of their great theoretical interest, for practical purposes they would hardly come into consideration".[3] In recent years the liminal intensity has been used by Wirth and Peters. The latter employed as stimuli (1) a liminal noise made by a shot falling through a variable distance upon a hard surface; (2) a liminal change in the illumination of a white cardboard produced by increasing or decreasing, in discontinuous steps, the resistance in an electric current feeding a light of 35 c. p.; and (3) a liminal pressure upon the nail of the middle finger. "The judgments of the observer were: noticed (+), perhaps noticed (+?), doubtful (?), not noticed (—). The observer had also to indicate after every experiment whether, in his subjective estimate, he had followed the instructions well, fairly well, or badly".[4] In some of the experiments, "the attention had to be concentrated as keenly as possible upon the expected impression", in order to "obtain normal liminal values". In another series the attention was concentrated upon a different activity, as reading, arithmetic, or dynamometer tensions. Finally, in a third test, the observer had to attend to whatever liminal stimulus might be given, without expecting any particular one.

[1] Münsterberg: Die Willenshandlung, 1888, 160 f.

[2] A. Bertels: Versuche über die Ablenkung der Aufmerksamkeit, Dorpat Dissertation, 1889, 9.

[3] Kraepelin: Psychol. Arb., I, 1896, 58.

[4] Peters: Arch. f. d. ges. Psych., VIII, 403.

Thus the *Einstellung* to any one of the three possible stimuli was practically eliminated. According to Peters these three series represent three different degrees of attention to the liminal stimulus, concentration with *Einstellung*, concentration without *Einstellung*, and distraction. He finds a uniform rise of the limen above the normal value in the experiments without *Einstellung*, while in the distraction series the results show great individual differences (*cf.* his Table 8, p. 416), though as far as the distractions were effective, there is a tendency of the limen to rise even higher than in the experiments without *Einstellung*. Of great interest to us is also the correlation of the results with the subjective estimate of the observer's attention to the distracting task. Peters, however, did not place great confidence in these statements, because he had found "long before that the criterion for the judgment of what is usually called attention is not (as, for instance, Münsterberg seems to believe) uniform at all, but is variable with different individuals, that one person judges the 'intensity' of his concentration by the strain sensations or by the intensity of the impulses of his will, another by the clearness and distinctness with which he has apprehended a conscious content, a third one finally by the affective components, his interest, etc."[1] Besides, Peters thinks that the estimates on different days cannot be compared. This would be true, of course, if one and the same observer varied on different days between the three possible ways of estimation; otherwise Peters' objection is not necessarily valid. If it were true, then the same criticism might be brought against judgments of intensity made on different days. Since Peters knew of the variety of means of estimating the attention, he might, if it had become necessary for his purpose, have overcome the difficulty by requiring his observers to use only one prescribed criterion. As it is, the most important addition which his investigation makes to the solution of our particular problem consists in the furnishing of experimental evidence for the fact that attention without *Einstellung* is not maximally intense.

IV. *Reaction-Time* was first used to express degrees of attention by Obersteiner. He assumed that the "retardation of the reaction . . . stands in inverse proportion to the intensity of attention".[2] The results show that with different people the same distractions have different effects. In this they agree with Wundt's and Buccola's results,[3] who also found that under distraction the time of reaction lengthened.

[1] *Op. cit.*, 428.
[2] Obersteiner: Brain I, 1879, 444.
[3] Buccola: Rivista di filos. scientif. I, 19.

While, on the one hand, Buccola, according to Stanley Hall,[1] thinks this to be "due to distraction or defective powers of voluntary attention", Wundt, on the other hand, is very cautious in the interpretation of the retardation. In the first edition of the *Grundz. d. physiol. Psychologie* he says, indeed: "as long as a disturbing secondary impression is present, so long it is impossible to raise the concentration of attention to its highest degree".[2] In later editions, however, this passage is omitted. Instead we read, for instance, in the third edition: "the innervation, which exists in every concentration of attention, probably varies with different sense-impressions, perhaps it arises in different localities of the centre of apperception".[3] Even this is entirely rewritten in the fifth edition, and the whole fact is simply referred to as "eine Verlängerung der Apperceptionsdauer des Reizes".[4] Nothing is said here about an intensified concentration of attention or apperception. Similarly careful in his interpretation of lengthened reaction under distraction is Swift, who found a small but uniform increase when the reaction was made while the attention was concentrated on some other task. He wrote: "I willingly admit that it is impossible to determine how far the instructions are observed in such a case".[5] He even believed that "there are no means by which the amount of attention given to the reaction or to the other task can be measured". Cattell,[6] who distinguished three introspective degrees of attention, concentrated, normal, and distracted attention, found that there was no corresponding lengthening of the reaction-time with less attention. Sharp, on the other hand, thinks it possible that "an individual who gave a quick reaction-time, owing to great power of attention, might, by having his attention artificially distracted, lengthen his reaction time until it corresponded to that of other individuals who normally gave long reaction times from small power of attention. Given a constant correspondence between length of reaction-time and degree of concentration of the attention, and the rapidity of reaction of an individual might be taken as an index of his power of attention".[7] Nevertheless she doubts the range of its practical application, while to us the basal fault seems to lie in the fact that "the constant correspondence" of which she speaks as given is not given, but is a very dubious matter,

---

[1] Hall: Mind, VIII, 1883, 177.
[2] Wundt: *op. cit.*, 1st ed., 1874, 749.
[3] *Op cit.*, 3rd ed., II, 1887, 293.
[4] *Op. cit.*, 5th ed., III, 1903.
[5] Swift: *Amer. Jour. of Psychol.*, V, 1895, 17.
[6] Cattell: Mind, XI, 1886, 242.
[7] Sharp: *Amer. Jour. of Psychol.*, X, 1899, 16.

which has not yet been established by experimental methods. Binet and Henri also made use of the reaction experiment, not, however, for the sake of its retarded character, but for its irregularities as expressed by the mean variation. Quite recently Wirth and Kästner [1] have combined their perimetrical brightness-experiments with the reaction to the recognition of the brightness change, but their results are much too irregular to give any indication as to the different degrees of attention under which the task was done. Thus the final outcome of the reaction experiments, as used for the measurement of attention, has been on the whole negative; it has been impossible to establish a positive correlation between high degrees of attention and short reactions, and between lower degrees and correspondingly lengthened reactions.

V. The *Degree of Precision*, as the last method to be mentioned in this connection, seeks to find an expression of the degree of attention in the quality and quantity of work performed in a given time. At first sight this method seems to be the most natural and promising of all, but even here we find disagreement as to its efficiency. Some psychologists have assumed that, in general, changes in the quality or quantity of the work done are due directly to changes in the amount of the attention given to the task. Peters [2] calls this Bourdon's method. It has been recommended, for instance, by Jodl: "it is obvious that attention cannot be measured directly. However, its degrees may be determined indirectly by reference to its effects. These effects consist, in every task which does not have the character of an involuntary movement, in the avoidance of errors and mistakes and in the quickness of the execution".[3] Lipps, as we have pointed out, likens attention to a "psychische Kraft" which, like any kind of power, may be measured by the "Höhe der Leistungen".[4] In a similar way Stern holds that "the quality and quantity of a task performed under normal conditions could be used as a measure of the attentional energy".[5] This method has been used, for example, by Sharp: "the *degree* of attention habitually exercised by an individual was measured by the quickness and accuracy with which a certain given task was performed".[6] Hence her seven observers were classified accordingly, the fastest and most accurate as giving the best attention, the slowest and least accurate as giving the least attention to the same task. Vari-

[1] Wirth and Kästner: Psychol. Stud., IV, 1908, 139-200.
[2] Peters: *op. cit.*, 390.
[3] F. Jodl: Lehrbuch d. Psychologie II, 1903, 84-85.
[4] Th. Lipps: Leitfaden d. Psychologie, 1903, 34.
[5] W. Stern: Über Psychol. d. individ. Differenzen, 1900, 84.
[6] Sharp: *Amer. Jour. of Psychol.* X, 1899, 28.

ations of the attention of a single individual at different times could, of course, be detected and treated in a similar way. A modification of this method was first suggested by Friedrich in 1883. He said: "it is tempting to fix definitely the somewhat unsettled concept of attention by making it proportional to the measure of precision, *i. e.*, to the reciprocal value of the mean error, so that a small mean error corresponds to a high degree of attention, and conversely, a large mean error to a low attention, . . . since the characteristic phase of inattention consists rather in the large variations of the physiological times within the same experimental series". Although his results showed "a very good agreement between this theoretical consideration and experience", nevertheless he thought that "the identification of the attention with the measure of precision is in most cases open to objections. Only in cases of the simplest mental processes, which besides should be as homogeneous as possible and little subject to practice, may one assume that the mean error is mainly dependent upon the degree of attention. With more complex mental processes, however, not only does practice play a considerable part in the fluctuations of the individual experiments, but in certain cases the cause of great variations may lie in the mental processes themselves".[1] The experiments of Friedrich were made under Wundt's direction just before the publication of the second edition of the *Grundz. d. physiol. Psychologie*, where Wundt himself made the first change in his interpretation of the retarded reaction-time, so that it is probably safe to assume that Wundt agreed with Friedrich's contention. Nevertheless, the mean variation has been frequently proposed and employed as a measure of attention. H. Griffing, for example, says: "although we cannot assume that the average number of letters seen by an observer measures his powers of attention, the mean variation from the average of the numbers seen in the different experiments is presumably due, principally at least, to variations in the attention".[2] A wide application of the mean variation as a "dynamometer of the attention" is found in Oehrn's experiments. He gives,[3] for instance, the following *m. v.* values: for writing 2.6%, reading 3.4%, reading a foreign language (Latin, Livy) 3.3%, counting letters 4.2%, arithmetic 4.6%, counting letters in groups of three 5%, memorizing 12-place numerals 14.7%, and learning series of nonsense-syllables 27.4%, of the average value in the particular task. However, this work is criticised by Henri thus: "we do not

[1] M. Friedrich: Philos. Stud., I, 1883, 73 f.
[2] Griffing: *Amer. Jour. of Psychol.*, VII, 1895, 235.
[3] A. Oehrn: Psychol. Arb., I, 1896, 121 f.

believe that these figures express exactly the part due to attention in any one of the above cases, since other factors, like fatigue and practice, are not eliminated and certainly influence also the mean variation''.[1] On the other hand, van Biervliet[2] maintained that "the attention can be measured by the consistency of the results, in fact, by the mean variation'', and he applied this method in his experiments on visual acuity. Binet, reviewing and criticising this article, makes the following peculiar comment: "il y a dans l'article de M. Biervliet une remarque très ingenieuse, qui est a conserver. Il dit que l'attention se marque dans la variation moyenne d'une expérience; c'est tout a fait juste, dans beaucoup de cas. On le savait, mais personne jusqu'ici ne l'avait dit avec cette netteté''.[3] Peters, whose work we have already discussed, found a close correlation between the magnitude of the *m. v.* in determinations of the liminal sensitivity and the degrees of attention under which he had established the limen. Nevertheless, even so he had to assume that practice had "a slight but distinct''[4] influence upon the results. Thus the mean variation can at best afford only a very rough measure of attention. Perhaps its greatest usefulness would consist in furnishing a convenient check upon other methods and results, especially since it is not bound up with any particular apparatus or procedure of experimenting, as is the case with all the other methods of expression.[5]

This review of experimental work brings to light the important fact that the expression methods have almost entirely neglected the factor of introspection. The main reason for this has probably been correctly stated by Peters; the investigators have not been able to agree upon a common characteristic feature of the state of attention. Some of them have emphasized the motor factor, some the affective components, some the strain sensations, but with the exception of Wirth none have used the clearness factor. This oversight is the more surprising as most of the theoretical discussions of the degree of

[1] Henri: L'année psychol., III, 1896, 244.

[2] Van Biervliet: Journal de psychol., I, 1904, 231.

[3] A. Binet: L'année psychol. XI, 1905, 71.

[4] Peters: *op. cit.*, 426.

[5] In addition to the five methods of expression thus far indicated we briefly mention the following method reported by Bliss, *op. cit.*, pp. 53 f. "During the course of my experiments Dr. Scripture suggested that the accuracy with which a person could steadily point to a given spot would be a measure of the amount of attention he could direct toward the work. In accordance with that suggestion the apparatus shown in Fig. 15 was arranged to measure this accuracy. . . . (The result was that) within certain limits the movements of the point increased or decreased inversely with the amount of attention given to the work".

attention have explicitly or implicitly referred to clearness and, as a matter of fact, have agreed upon it as one of the main characteristics of attention, while as to the other characteristics they heartily disagree.

VI. The only systematic attempt to employ introspection as an aid toward measuring the attention has been made by the method of *Graded Distractors*, as we may briefly call it, which is therefore the only representative of the impression-method in attention. By means of a series of stimuli which should make greater and greater impression upon the observer's mind, he would be induced to pay less and less attention to another simultaneous task. Stumpf, who considers it one of the two problems of a quantitative study of attention "to find means by which degrees of attention may be differentiated",[1] thinks that such means can be found, and gives as an example "the simultaneous perception of other simple phenomena which are capable of a progressive multiplication. The amount of this multiplication furnishes, of course, not a direct but an indirect measure of attention. Attention itself is not, but the means of varying it are, measurable".[2] Stumpf refers to Wundt and Boas as having already used this principle in their experimental work. Later it was applied by Drew in certain experiments. Their object was "to arrange a series of tasks of increasing degrees of complexity which should make ever greater demands on the mind, until the attention should pass from a fully concentrated to a completely distracted state".[3]

Although he accumulated bulky introspective records, Drew does not state in how far he succeeded with his series of distractions. After Drew, Stumpf's principle was made the leading problem of a series of minor studies in the Cornell laboratory, carried out during the years 1895-97. To quote Titchener: "Suppose, again, that we have arranged a series of distracting stimuli, homogeneous in kind but graded in complexity, such that we are able to reduce the observer's percentage of right cases from 100 to 95, 90, 85 . . . according to the distraction employed. It is necessary that the action of the distractors be constant; and it is necessary that they be of the same kind, and therefore exert an influence which differs only, and differs measurably, in degree. Having secured these conditions, we should let the observer decide whether the clearness of conscious contents was distinguishably different under a 5 per cent. and a 10 per cent. distraction, or under a 5 per cent. and a 15 per cent. distraction, or again under an 80 per cent. and an 85 per cent. distraction, and so on, all through the

---

[1] Stumpf: Tonpsychologie, I, 1883, 73.
[2] *Op. cit.*, 75. [3] Drew: *Amer. Jour. of Psychol.*, VII, 1895, 533.

series. We should thus finally obtain a scale of noticeably different clearnesses paralled by a scale of measured amounts of distracting stimulus''.[1] However, the experiments have not yet led to the discovery of an entirely satisfactory series of graded distractors. It was found ''that odors are admirably constant distracting material,—if only they could be measured''![2] Thus the impression method has had little more success than the expression methods, although psychologically it seems to be the sounder,[3] because it attempts to parallel experimental results with introspective data. The reason for its failure thus far is not inherent in its nature, but lies rather in the difficulty of finding satisfactory material, namely, effective distractions which are at the same time objective, homogeneous, and quantitatively measurable.

We have thus completed our review of the experimental work upon the problem of measuring the attention and may conclude, on the one hand, that, although much effort has been spent upon it, we are still far from a satisfactory solution. On the other hand, we are now better able to judge which paths would seem most promising for future experiments and which should be abandoned. We are also convinced that Pillsbury's suggestions on ''*a priori* grounds''[4] are not at all hopeful. We miss in his chapter on ''The Methods of Measuring Attention'' the reference to the clearness factor, whose importance he emphasized in his introductory chapters.

PART II

An Attempt at a New Measurement of Attention in Terms of Clearness Values

The general aim of the following investigation was to renew the search for an exact method of measuring the attention by advancing simultaneously along old lines which the previous review of the literature had shown to be most promising, and along new paths which suggested themselves in the course of the experiments. Previous investigations had simply assumed that the variations of attention are directly paralleled by certain changes, whether in reaction-times, or in muscular pressure, or in quickness and accuracy of work, etc., etc., but they had not attempted to verify this assumption by reference to experimental introspection, and thus their psychological task was only half completed.

The purpose of our own work was to determine whether

---

[1] Titchener: Lectures on the El. Psychol. of Feel. and Att., 1908, 277.
[2] *Op. cit.*, 278.
[3] *Cf.* Ebbinghaus: Grundz. d. Psych., I, 1905, 621 f.
[4] Pillsbury: Attention, 1908, 92.

and in how far the assumed parallelism of the expression-methods rested on an experiential basis. This test might be applied in several different ways. For instance, one might argue that, if the assumption made by the expression-methods holds true, then it must be possible to find a direct parallel between any two ways of expressing degrees of attention, or rather, conversely: if two kinds of operations, which seem to depend mostly upon a strong concentration, give a positive correlation between different degrees of perfection, then the assumption holds true that their different degrees of perfection express different degrees of the attention with which they were executed. Or, one might argue thus: it would seem unreasonable to hold that objective variations in the work accomplished should indicate subjective variations in the observer's attention, if his careful introspection could not reveal such variations.

The former principle was applied in a first group of experiments, in which we compared the accuracy and quantity of work performed with the muscular rigidity or motor inhibition which is said to become the more pronounced the more attention is concentrated. Ebbinghaus, for example, has pointed this out in the following passage: "the same inhibitory relation which exists between purely mental states and processes, occurs also between them and bodily movements. In medium degrees of intensity both may quite well take place simultaneously. As soon, however, as on the one side the energy of activity is considerably increased, so soon will the other side suffer a decrease."[1] This fact had never been tested experimentally, at least for the purpose of measuring attention by it, unless one should include here Loeb's and Welch's dynamometer experiments. In order to get a graphic record of muscular rigidity the following arrangement was made. The observer's left arm was screened from his sight, and he was required to grasp with the hidden hand a handle fixed in an arm rest. The observer had no knowledge of the existence and purpose of the further details of the apparatus. The handle was in reality a plethysmograph, which was connected by glass and rubber tubing with a Marey tambour writing on a kymograph. The slightest movement of any of the five fingers, or any tightening or loosening of the grip, caused an appreciable excursion of the pointer, which increased with the extent of the movement.

The work which, after several trials with various materials, seemed best suited for an objective, quantitative gradation as to quickness and accuracy, was mental arithmetic in the form

---

[1] Ebbinghaus: Grundz. d. Psychol., I, 1905, 597.

of continuous adding. We arranged 4 different series of 30 figures each, selected so as to constitute 4 different degrees of difficulty. The easiest series contained all figures from 3 to 13, except 10, each figure occurring 3 times. The second degree of difficulty included the following figures: 23, 24, 25, 26, 27, 33-37, 43-47, and 53-57, each figure occurring twice. In the next series the following figures were used: 63-67 once, 73-77 and 83-87 twice, and 93-97 once. Finally the most difficult series contained: 128, 129, 131-135, 221-227, 314-319, 408, 409, 411-413, and 503-507. By using different starting points from day to day, practice or memory of the results was entirely eliminated, while the actual series did not need to be changed. The results are given in Table I. The following distractions were used: an electrically driven tuning fork of 512 vibrations, an electric bell, a metronome beating

TABLE I

Difficulty 1.	Difficulty 2.	Difficulty 3.	Difficulty 4.
To: 25, 36, 47, 58, or 64	To: 65, 66, 68, 72, 74, or 77	To: 29, 31, 34, 35, or 38	To: 18, 22, 29, 32, or 38
Add:	Add:	Add:	Add:
9	27	63	314
11	34	74	409
6	45	85	135
7	53	97	315
12	37	75	503
13	55	86	411
8	46	77	226
5	36	96	224
3	24	87	128
9	57	73	504
4	26	67	412
11	47	76	316
7	35	64	227
12	25	83	222
8	43	75	129
3	37	84	505
13	54	73	221
4	35	83	413
5	47	95	319
11	33	74	225
6	45	87	131
9	56	86	506
12	34	65	134
7	23	93	317
3	44	76	133
6	43	94	507
13	36	85	318
5	46	77	223
8	33	66	408
4	44	84	132

at 3 different rates, 60, 100, and 150 strokes per minute, and a flicker moving across the whole field of vision at 4 different rates of speed.

All experiments were done in a large dark room. Directly in front of the observer, at a distance of 2 m, stood a large screen, 1.20 x 2.40 m, covering practically the whole field of vision when the eyes fixated the central point. It was illuminated by an electric light above and somewhat behind the observer's chair. In front of this light a rotating spoke, made of a strip of cardboard, cast its flickering shadow over the visual field. A rectangular hole of 3 x 8 cm was cut in the centre of the screen, and through it the figures to be added were exposed. The figures of a series were pasted on a long strip of cardboard sliding in a groove directly behind the screen. To the upper end of the strip a string could be attached, which ran over a pulley and carried a counter weight. Another string, tied to the lower end of the strip, led to a spool with a handle, in front of the observer's seat. By turning the handle he wound the string around the spool, thus pulling down the strip and exposing the figures one after another at any desired speed.

As soon as the experimenter had announced the initial figure the observer began to add to it the figures exposed, calling out each individual sum until all 30 figures of a series had been added. The observer was instructed to concentrate all his attention upon the adding, and to neglect as far as possible any distraction, purposive or accidental. At the same time the experimenter behind the screen checked up each individual sum, marking every mistake, and also obtaining a graphic record of the time of each individual addition. This was done in the following way: one of the two ends of a pair of compasses carried a pencil with which the accuracy was recorded, while around the other end a copper wire was wound which was connected with an electromagnetic pointer writing on the kymograph. Next to the strip of paper upon which the accuracy of the additions was recorded, a long strip of sheet copper was fastened, connected by a wire with the other pole of the magnet. As soon as the observer pronounced a sum the experimenter touched the copper strip with the wired compass end; thus the electric circuit was closed, and the pointer raised by the magnet made a mark on the kymograph. A Jacquet clock furnished the time line by writing seconds. Thus for every experiment three graphic records were obtained; the movements of the observer's left hand, the time line, and the duration of each single addition as well as that of the whole experiment. The kymograph with the writing pointers was enclosed in a soundless box, a lever being so placed that the experimenter, by raising or lowering it with his left hand, could easily and quickly start and stop the movement of the kymograph. After this was done, his left hand was free to manipulate any of the distractions to be employed during the experiment, while his right hand held the compass ready to check up the results.

After every experiment the observer was required to write out or to dictate answers to the following introspective questionary:

(1) What was your general affective mood during the experiment?
(2) Did it undergo any change, and if so of what kind and during which part of the experiment?
(3) Where and how intensive was your attention?
(4) Did it undergo any noticeable changes? If so, of what kind were they and when did they occur?
(5) Did you notice any internal sensations during the experiment? If so, of what kind were they and when did they occur?
(6) Did they distract you and to what extent?
(7) Did you notice any external stimuli? Of what kind were they?
(8) Can you remember in which order they occurred?
(9) Did they distract you and to what extent?
10) Can you add any other introspective observations not included in the previous questions?

It should be noted that questions 3 and 4 did not suggest any criterion for distinguishing different degrees of attention. The observers judged the intensity of their concentration of attention in terms chosen entirely by themselves. After some experience and practice in estimating their attention they developed, independently of one another and without suggestions from the experimenter, a scale of 4 or 5 different degrees of concentration. This fact was utilized in the second half of the experiments belonging to this group, by the adoption of the following arbitrary scale of degrees: very good or excellent, good, fair, bad, very bad; without, however, any definition of the nature of these degrees.

The experiments of the first group, including all preliminary and practice series, lasted from November, 1907, till March, 1908, when they were discontinued because they promised nothing new or of further significance. The observers in this as well as in the next group of experiments were Dr. W. H. Pyle (P), Dr. T. Nakashima (N), and Mr. R. A. Tsanoff (T). All three were graduate students in the psychological department, and had had considerable practice and experience in introspection. They observed regularly three times a week. Four experiments were made in the hour, and a rest of from 5 to 10 minutes was allowed between the second and the third, during which the observer could leave the dark room. In every hour, each one of the four degrees of difficulty occurred once. Their temporal order was so arranged that every difficulty occurred an equal number of times in the first, second, third or fourth place. A long preliminary practice was required from the observer, and also from the experimenter. The addition tasks used in these experiments were slightly different in arrangement, but not in difficulty, from those employed in the later series. The regular series began in January, 1908, each observer making at first 24 experiments without any distractions. In the following 300 experiments, about 100 for each observer, the distractions occurred with the following frequency: the tuning fork, the electric bell, and each one of the four rates of flicker occurred three times, while each one of the three metronome rates occurred twice with every one of the four degrees of difficulty. The distractions were so distributed as to let each kind occur only once in an hour, and as often in the first place as in the other places. However, they did not persist during the whole experiment, but were used only during the first twenty additions, while the last ten additions were always performed in the normal way.

The results showed that, in spite of all precautions, a slow and gradual effect of practice and habituation made itself felt, especially during the first half of this group of experiments. The practice both reduced the length of time for the different degrees of difficulty and decreased the number of errors. The habituation manifested itself in a more and more pronounced disregard of the distractions, so that the first twenty additions were done as well as the last ten, and in some cases better, because the sudden interruption of the tone or flicker might confuse the observer for a few moments.

Considering the results as regards the degree of difficulty, we notice a uniform increase in the average length of time from the easiest to the most difficult series. Table II gives the results in seconds for the first 24 normal experiments, for all 96 experiments with distractions, and for the last 48 experiments with distractions. The item Limits shows the quickest and the slowest experiment that occurred with each degree of difficulty.

The overlapping between neighboring degrees of difficulty is due to practice, which brought it about that toward the end a more difficult task could be done as fast as an easier task had been done at the beginning. It is interesting to notice that, without exception, the more difficult additions reached the maximum of practice sooner than the easier additions. The shortest times for the former occurred after about six weeks of regular tri-weekly experimenting, while for the latter the shortest times occurred much later. The only difference of effectiveness between the different distractions was that the experiments with the flicker gave as a rule much shorter times and fewer errors than those with auditory distractions. Among

TABLE II

	Difficulty 1.		Difficulty 2.		Difficulty 3.		Difficulty 4.	
Obs.	Av.	Limits.	Av.	Limits.	Av.	Limits.	Av.	Limits.
P	90	75–96	110	105–116	146	134–160	163	136–214
N	108	90–132	137	121–159	185	149–233	209	189–221
T	75	66–84	117	100–135	154	136–160	180	166–201
P	73	57–98	92	72–111	109	89–146	128	100–167
N	85	71–117	110	87–143	145	116–188	171	139–237
T	57	41–96	103	81–131	116	84–145	152	114–190
P	71	62–84	85	72–94	100	89–115	116	105–137
N	80	69–98	102	87–119	141	116–153	165	139–191
T	49	41–61	100	81–128	103	84–123	138	114–173

the latter themselves no differentiation as to effectiveness could be observed.

In comparing the observers' estimates of attentive concentration with the calculated quickness and accuracy of their work, we shall refer only to the last 48 experiments, because here a uniform scale of degrees was used and the influence of practice was minimal as to improvement both in rate and in accuracy. In fact the number of errors was so small as to be entirely negligible. In order to discover whether there was any correlation between the observer's estimate of attention and the calculated time-values of his work, the latter were divided into 5 groups: quickest rate, fairly quick, average rate, fairly slow, and slowest rate. Because the number of experiments available from each observer was small, the results were combined for the purpose of calculation, after we had assured ourselves that the individual results were sufficiently similar to allow such a procedure. The 5 introspective degrees of attention were then paired with the 5 rates of work to give the following Correlation-Table:

TABLE III

Degrees of Attention

		Best	Good	Av.	Bad	Least	Total No.	Means
Rate of work	Quickest	24	1	—	—	—	25	1.04
	Quick	13	17	6	1	—	37	1.86
	Average	22	17	16	1	—	56	1.93
	Slow	8	6	3	4	—	21	2.14
	Slowest	—	4	1	—	—	5	2.20
	Total No.	67	45	26	6	0	144	
	Means	2.21	2.91	2.96	3.5	0		$r = .60$

The correlation coefficient was calculated according to the familiar Bravais-Pearson formula $r = \frac{\Sigma xy}{\sqrt{\Sigma x^2 \Sigma y^2}}$, which resulted in the equation $\frac{13.09}{24.29 \times 17.29} = .60$. This is a surprisingly high value, considering the fact that the observers were not told in what terms to define degrees of attention. They were, however, frequently warned not to judge these degrees on the basis of an estimation of the accuracy or quickness of the work, and as a rule they faithfully followed this instruction. The introspections do not, however, throw much light upon the methods of estimating attention used by the various observers. Occasional hints indicate a few general facts: in the case of P, the estimation was mostly based upon the ease with which the different sums occurred in consciousness. N usually relied upon his memory of the force with which the distraction impressed him; while T used as a basis the presence or absence of motor concomitants or "general nervousness". However, these criteria do not seem to have been used consistently or uniformly, nor were the observers always aware of their own methods. This fact tends to make the close correlation between the subjectively estimated degrees of attention and the objectively determined quantity of work the more remarkable, and should lead to greater confidence among psychologists in the observer's ability to make reliable estimates of quantitative aspects of his own consciousness. Our results are entirely different from those obtained by Peters, and our later experiments show that the trustworthiness of introspective estimation of degrees of attention can be still further increased by the adoption of a uniform principle of differentiating such degrees.

On the other hand, the plethysmographic curves, that were to have shown the muscular rigidity of the left hand, gave absolutely negative results. In most cases they were perfectly smooth, while with one observer they gave sometimes distinct pulse records. Out of 128 experiments with P, only 6 showed brief irregularities, with N this number was 17, with T 12; and most of them occurred in the very earliest experiments. Hence it was impossible to discover any kind of correlation between the state of bodily rigidity and mental work. In this respect, then, the application of our principle of comparing two expression-methods failed, mainly because of an unfortunate choice of the methods to be compared. The principle itself is not affected by these negative results, and awaits further test.

Since the results of the first group of experiments had incidentally shown how well even unsystematic introspection is able to reveal subjective variations of attention, they offered a sufficient inducement for continuing these experiments as an explicit application of the second principle mentioned above. In other words, our problem shifted to this: How closely are the introspective estimates of attention correlated with quantitative changes of an objectively measurable character? In order to obtain systematic uniformity in the results, it was advisable to prescribe to the observer the terms in which he was to estimate the degrees of his attention. We followed Titchener's suggestion in asking the observers to base their subjective estimates of attention upon differences in the clearness of the mental processes attended to. In general, the procedure in this second group of experiments remained the same as in the first group. A few slight changes in method and material will be mentioned later, but the plethysmographic handle was not removed, chiefly because we wished to keep the external conditions of the experiments as constant as possible.

In order that the observers might become perfectly familiar with the introspective differences of clearness, they completed a large number of preliminary experiments, which were later repeated with those who observed in the third group of experiments. Besides, several undergraduate students, who at the time were members of the introductory laboratory course, were asked to take part in these preliminary experiments, mainly in order to see how easily they learned to distinguish between differences of clearness and differences of intensity.

The method in the preliminaries was in general this: two metronomes were set going at different rates, about 100 and 120 strokes per minute, and the observer, who sat with his back toward them, was asked to count the number of sounds between coincident strokes. Then the same stimuli were repeated, but the observer was required to perform some mental operation, for example, spelling or reciting in a foreign language, adding or subtracting successively the same number, naming rapidly the states with their capitals along the Atlantic coast, etc. Each experiment should not last longer than a minute or two. After several paired experiences of this kind, the observer is not only able to compare the differences between the clearness of the metronome strokes in the first and in the second case, but is also ready to assert that the strokes were not always equally clear or obscure. As soon as this stage is reached, the experiments are repeated with the same or similar material and the observers are encouraged to estimate, first the larger, later the finer, differences in clearness. Some observers will easily construct for themselves a scale of five or more steps from very clear to very obscure, while others will prefer to use rough differences in terms of per cent. Nearly all of our observers, both experienced and inexperienced, found it relatively easy to apply some such gradation of clearnesses. In all these preliminaries the differences between the focal and the peripheral processes were rather large. The efforts to induce clearnesses which lay nearer the middle of the arbitrary scale were most successful when the observer was requested to hum to himself a more or less familiar melody and gently beat the time to it with hand or foot, while either one or both of the two metronomes were striking their own rhythms. Another effective method was to spell long phrases, or to add or subtract series of figures, to the rhythm of a metronome. After several days of practice the more experienced observers were able to give a fairly complete analysis of their consciousness during an experiment, and to estimate without difficulty the clearness or obscurity of every mental content experienced by them. It seemed, at least according to some of the observers, that the comparison of simultaneous processes as to their degree of clearness facilitated especially the task of analyzing a complete consciousness even in entirely different experiments.

After eight or ten hours of practice the observers P, N, and T had worked out for themselves, with suggestions as to a uniform terminology from the experimenter, the following scale of nine degrees of clearness:

1.	About	90-100% =	maximally clear	or	excellent	attention
2.	"	80- 90% =	very clear	"	very good	"
3.	"	70- 80% =	clear	"	good	"
4.	"	60- 70% =	fairly clear	"	fairly good	"
5.	"	50- 60% =	fair	"	fair	"
6.	"	40- 50% =	fairly vague	"	fairly poor	"
7.	"	30- 40% =	vague	"	poor	"
8.	"	20- 30% =	very vague	"	very poor	"
9.	"	0- 20% =	absolutely obscure	"	no	"

Later, during the experiments in the dark room, all the observers added, of their own accord and independently of one another, an intermediate step between the first and the second degree, while T made one also between the second and the third degree.

The experiments of the second group were resumed in the dark room about the middle of April. The distractions which had been used in the first group were again employed, but now in various combinations, in order to find out whether this would make them more effective for inducing different degrees of attention. In the four simplest cases two auditory distractors were combined in the following way:

1. two metronomes of different rates, 100 and 120 per minute,

2. one metronome and the electric bell,
3. one metronome and the electric tuning fork,
4. the bell and the fork.

In the next four combinations the visual distraction caused by a medium rate of flicker was added to these pairs. Finally, three simultaneous distractors were given in this way:

1. two metronomes of different rates, the bell, and the fork,
2. two metronomes of different rates, the bell, and the flicker,
3. two metronomes of different rates, the fork, and the flicker.

The addition tasks were continued with one degree of difficulty only, but the figures 43-47, 53-57, 63-67, and 73-77 were given in the four different orders of Table IV. The number of single additions in one experiment was reduced from 30 to 20, so that the last ten (without any distractions) were omitted. Instead of this, during every hour one of the four experiments was completed normally, that is without any distractors, in order to be used as a standard of comparison. The normal series, as well as the series with the various combinations of distractions, were so distributed over the whole group of experiments as to occur with equal frequency in the

TABLE IV

To: 113, 115, or 117 Add:	To: 221, 225, or 229 Add:	To: 332, 335, or 338 Add:	To: 444, 445, or 446 Add:
56	47	46	73
45	63	64	55
67	56	73	64
74	44	57	47
54	73	43	74
73	55	66	54
63	76	55	65
44	66	77	56
55	77	65	75
77	46	54	46
64	65	47	63
46	54	74	45
53	45	63	53
65	67	53	67
76	53	48	76
57	75	75	57
47	64	67	77
43	74	56	44
75	57	45	43
66	43	76	66

first, second, third, or fourth place of an hour's work. After 3 days' preliminary work with normal series only, each observer completed 60 experiments, of which 44 were done with distractions, so that each distractor combination occurred four times.

The questions for introspection were also somewhat modified, in accordance with the uniform standard of differentiating degrees of attention, and now ran as follows:

(1) Making the relative clearness of the adding process the criterion for degrees of attention, and calling the absolute maximal clearness which you can experience under most favorable conditions 100% or excellent attention, what percentage of attention or what degree of clearness prevailed, during the experiment, for the adding process?
(2) Was it constant, or intermittent, or changing to more or less?
(3) On the same basis of distinction, what degree of clearness or attention would you ascribe to other mental processes that occurred during the experiment?
(4) Were they uniform, or variable, and how?
(5) Were you distracted by anything else? If so, by what and how much?
(6) Was the distraction continuous or intermittent?
(7) What affective processes or moods dominated your consciousness during the experiment?
(8) Can you add any other appropriate remarks?

Besides, the observers were again expressly cautioned *not* to estimate their attention by the apparent temporal rate or the seeming correctness of their calculations. The general instruction was to concentrate attention upon the adding-process and to neglect as much as possible any intentional or accidental distractions. The large number of introspective data obtained from these, as well as from the other observers in later experiments, furnish interesting facts concerning the steadiness of attention and the problem of the two levels, which will be discussed later.

The results of this second group of experiments showed plainly that even the most complex combinations of distractors, after a few days' work, were unable to induce great variations of attention. Instead, toward the end of the whole group, most of the normal series were actually performed at a slower rate than the distraction series. Throughout the whole course of this group of experiments a slight and gradual practice effect was noticeable which, in the case of P and N, manifested itself in both a shortening of the time and in smaller deviations from the general average. In the case of T, however, a rather sudden improvement occurred about the middle of the whole course, while his variations from the average remained as great as in the beginning.

Although the distractions were not as effective as expected, yet the attention to the adding-process was not the same in all experiments, but varied between the first and the fifth degree; most frequently it was judged "very good", in a few cases it sank to the fourth degree, once it was estimated by N at "fair", and once at "fairly poor"; but all these lower degrees occurred during the first few days, before the distractions had lost much of their effectiveness. In the case of P attention was most frequently judged at the highest degree, and only once or twice below "very good". This agrees perfectly with the quality of his work, the rate of which is much faster and the quality more uniform than that of the other two observers. Table V allows a detailed comparison of the subjective estimates of attention in terms of clearness with the average time (in seconds) of the experiments at these levels. With all three observers there is a perfect correlation between their best attention and their shortest time, and between correspondingly lesser degrees and longer times. The fractions given under the item Error have for their numerator the number of experiments belonging to each rubric, and for their denominator the number of errors occurring in these experiments. Wherever the m. v. is given as 0, only one experiment occurred under that rubric.

TABLE V

Degrees of Attention

		First			Betw. 1–2			Second			Betw. 2–3			Third			4 and 5	
O	Expts.	Av.	MV	E	Av.	MV	E	Av.	MV	E	Av.	MV	E	Av.	MV	E	Av.	E
P	Total	54	4.2	27/21	57	3.7	15/15	63	5.5	15/15	66	0	1/1	—	—	—	—	—
N	No.	67	7.7	4/4	69	3.7	18/11	71	7.8	32/28	—	—	—	78	8.6	7/7	95	4/6
T	Expts.	—	—	—	60	9.6	16/20	64	9.5	29/23	79	9.5	10/9	87	5.0	4/3		
P	Normal	59	3.2	8/6	54	3.3	4/3	63	2.0	3/3	66	0	1/1	—	—	—	—	—
N		67	7.7	4/4	71	2.6	8/5	78	5.7	3/0	—	—	—	69	0	1/0	—	—
T	Expts.	—	—	—	63	13.	6/6	69	6.8	6/4	76	4.2	4/6	—	—	—	—	—
P	Distr'n	53	4.4	19/15	58	3.4	11/12	62	5.0	12/7	—	—	—	—	—	—	—	—
N		—	—	—	66	3.4	5/6	69	7.7	29/28	—	—	—	79	8.5	6/7	95	4/6
T	Expts.	—	—	—	55	9.4	10/14	64	9.5	28/19	83	11	6/3	87	5.0	4/3	—	—

In the whole Table there is but one case in which a lower degree of clearness shows a shorter average time than the next higher degree. This is due to the fact that all 4 experiments of the lower degree occurred within the last two weeks of work, while 6 of the 8 experiments in the higher degree came in the first three weeks, so that in this particular instance a cumulative practice effect obscures the correlation. On the whole, however, the shortening of the times due to practice was so gradual and constant that the correlation still holds, with but few exceptions in the case of N, if the whole period of experimenting is divided into 3 equal parts of 5 days' work each. We omit the table, because it is very like Table V. In a similar way, we also fractionated the results with reference to the number of distractions used, and found that the correlation in this case holds with practically the same uniformity. The fact that the difference in the averages of two successive columns is only a few seconds is not surprising, if one considers that the difference in clearness itself is at best only very slight. On the other hand, the great regularity of these results and the general uniformity of the correlation, in spite of the very small differences in degrees of attention, are the best evidence for the general validity of our conclusion that the degrees of attention may be very accurately estimated by introspection, if they are distinguished in terms of the clearness of the mental process attended to.

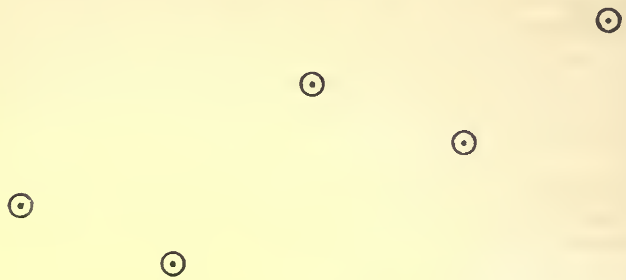
Because the experiments of the second group involved only the higher degrees of attention, and since they were not numerous enough for certain degrees, our next problem was to find a method that should induce all possible variations of attention to a given task. At the same time certain other requirements had to be fulfilled. In the first place, the efficiency of the work performed must depend as exclusively as possible upon the degree of attention given to it and as little as possible upon such factors as practice, fatigue, mood, etc. In the second place, the performance should never become entirely automatic or habitual, no matter how frequently it was repeated. Thirdly, it must require absolutely continuous attention, so that a momentary lapse should at once manifest itself in a momentary reduction of the quality of work. Lastly, the execution of the work must easily submit itself to a scale of quantitative gradations.

The kind of work which seemed to fulfill these conditions best of all was a modification of W. McDougall's method of marking irregularly arranged and step-fashion exposed colored dots or circles.[1] The exposure was made by means of the memory apparatus furnished by Spindler and Hoyer. The largest of the 5 aluminum cylinders belonging to it has a circumference of 48 cm, is 7.7 cm high, and gives 24 successive exposures. We fitted a large neutral-gray cardboard around the black metal exposure frame, in order to screen from the observer the whole apparatus and the experimenter. An electric light of 16 c.p. was fixed just above the exposure slit in the metal frame, so as to give a constant illumination and to do away with the shadow which by daylight the observer's hand and pencil cast upon the visual field. Another small screen protected the observer's eye from the direct light of the bulb. An

[1] McDougall: Brit. Journ. of Psychol., I, 1904, 438.

armrest was adjusted to the table in order to support the right arm from elbow to wrist, and to bring the hand exactly in front of the exposure slit.

The rings or circles to be marked were the common mathematical sign for the circle, approximately 3 mm in diameter (or 10-point of printers' size), as the sample shows. They



were printed in red ink upon strips of white paper which could be pasted tightly around the cylinder. The circles were arranged in 5 different irregular patterns which could be used in either direction so as to give really 10 different series. The lateral or horizontal deviations were also much larger than in McDougall's case, allowing for 5 different positions of the circles within a space of 32 mm, thus making the distance between two neighboring positions 8 mm. The greatest lateral deviation between two successive circles was 24 mm, or the length of three positions; however, this occurred only once or twice on each strip. The series thus far described will be referred to as "normal patterns", in distinction from the following "complication patterns" which were also employed. In one kind of series, which may be called the "letter pattern",

A M T D V ⊙ L U I

Y B L ⊙ A G P F Z

L S C U ⊙ V K H M

the circles appeared in rows of capital letters (10-point printers' size), each of the 24 rows on a strip containing 8 letters, their lateral distance from each other being 8 mm. The whole strip thus forms a sort of printers' pi, with the red circles mixed in. In another kind of series, the "digit pattern", the numerals 2 to 9 were substituted for the letters, so that each numeral occurred only once in a row. The figures show some

5 7 4 2 ⊙ 6 9 3 8

8 3 ⊙ 5 7 9 2 6 4

2 7 9 ⊙ 5 3 6 4 8

samples of each of the three patterns (with the exception that the circles are not painted in red). Finally another series was the "color pattern", in which little colored paper disks, of the size of the circles, were pasted on the strips of paper, in place of the letters or numerals. The colors used were blue, yellow, red, green, black, neutral gray, blue-green, purple, reddish orange, and yellowish orange. They occurred in irregular order, the last four also with irregular frequency, but, like the letters and numerals, about equally often in each of the nine possible lateral positions.

The marking of the red circles, which was done with a pointed, medium soft pencil, consisted in drawing a straight vertical line through the centre of the circle so that its length was about twice that of the diameter and extended the length of a radius above and below the periphery. Through the slit, 2 x 6.5 cm, in the black metal screen in front of the cylinder, one circle, or one whole row of letters, numerals or colors, with a circle among them, was exposed at a time for a fraction of a second, coming to a momentary rest and then disappearing quickly while the next row came into sight. The speed of the successive exposures could be regulated by means of adjustable fans, and it was so chosen for different observers as to be the fastest rate at which they could do the best possible work. The two observers who took part in the final series marked the 24 circles in 12 seconds; the three observers in the preliminaries, who had less practice, varied between 15 and 22 seconds. The reason for drawing a line, instead of making a dot, as in McDougall's experiments, was that the former way of marking secured greater continuity of attention by filling out the entire interval of exposure, thus preventing momentary wanderings of attention, or at least revealing them by a bad mark.

In determining the quality of the task, the first four marks in each experiment were disregarded, in order to eliminate the effects of the initial imperfect motor preparedness. The 20 remaining marks were graded in the following way. On the basis of 100% for an absolutely perfect record, each individual perfect mark received 5%. A single error, which reduced its value to 4%, consisted either in a slight variation of the length or the direction of the line, or in the failure to pass through the centre. If two such errors occurred together, or if the line touched the periphery of the circle, the mark was graded 3%. If three errors came together, or if peripheral displacement was combined with a change in either length or direction, the value of the mark was 2%. Finally, if the line did not touch the circle at all, or if it could not be classified under any of the above headings its value was 1%, while an entirely omitted mark was graded zero. The sum of the 20 individual values thus represents an exact measure of the accuracy of marking during any given experiment.

For the purpose of inducing a greater range of degrees of attention to the marking test, our experimental arrangement offered numerous possibilities, because it involved three kinds of variable factors. In the first place, the stimulus itself could be either the normal pattern or one of the three combination patterns. In the second place, the task could be either a single task, namely marking the circles only, or a double task, namely marking the circles while performing simultaneously some mental operation like spelling, calculating, reciting, memorizing, etc. (other kinds are mentioned also by McDougall). Finally, the direction of the attention was variable; it could be shifted from the marking to the mental operation or even to the accompanying letters, numerals or colors.

In the autumn of 1908 a number of preliminary experiments were done, mostly for the sake of a general orientation. The observers were Prof. I. M. Bentley (B), Dr. W. H. Pyle (P), and Mr. T. Okabe (O), the latter a graduate student in the department of psychology, with considerable experience in introspection. B and O took part in 30 and 40 experiments respectively, while P finished as many as 60. These preliminaries showed that the method was adequate to our purpose and that it also fulfilled the other requirements. Hence the experiments were continued in February and March, 1909, with

Miss H. M. Clarke (C) and Miss A. de Vries (V) as observers. Both were pursuing graduate work in psychology and had several years' training in experimental work. They each completed 130 of the final experiments, in which the following combinations of the three variables occurred:

	Stimulus	Task	Direction of Attention
1.	Normal pattern	Marking circles only	To the Marking task only
2.	Letter pattern	" " "	" " " " "
3.	Digit pattern	" " "	" " " " "
4.	Color pattern	" " "	" " " " "
		and	
5.	Normal pattern	" " Spelling	" " Spelling " "
6.	Letter pattern	" " "	" " " " "
7.	Digit pattern	" " "	" " " " "
8.	Color pattern	" " "	" " " " "
		and	
9.	Normal pattern	" " Reciting	" " Reciting " "
10.	Letter pattern	" " "	" " " " "
11.	Digit pattern	" " "	" " " " "
12.	Color pattern	" " "	" " " " "
13.	Letter pattern	Marking circles only	To the Letters only
14.	Digit pattern	" " "	" " Digits "
15.	Color pattern	" " "	" " Colors "

Other combinations had also been tried in the preliminaries, but with less success; for example, interpreting as letters certain tactual impressions upon the left hand. In the combinations 13 to 15 the direction of the attention to the letters, digits, or colors, was aided by requiring the observer to form a judgment, say about the most frequent or least frequent letter, numeral, or color, or about some combination of them. In the case of the combinations 5 to 12 the marking of the circle was done on a lower level of consciousness; it had, so to speak, to take care of itself.

From 5 to 8 experiments could be finished in one hour. At the end of each experiment the following questionary was answered:

1. How much attention (in terms of clearness) was given:
   (a) to the marking of the circles?
   (b) to the other required task?
   (c) to other sensory or ideational processes?
2. How steady was this clearness (or: within which limits did it vary), and which degree of clearness was the most frequent with regard to:
   (a) the marking of the circles?
   (b) the other required task?
   (c) the other mental processes?
3. What affective mood prevailed during the experiment?
4. At how much is the quality of marking the circles estimated in terms of per cent.?
5. What other relevant comments can you make upon this experiment?

There was at first some difficulty in the estimation of the clearness of the marking. This task involves the visual perception of the circles and their positions as well as kinæsthetic sensations from the quick motor adjustments of the hand and the movements of the pencil. After some practice, however, these factors fused into a conscious complex of homogeneous clearness which was easily estimated. Rare exceptions to this were always revealed in the introspections, and if in such cases the differences in clearness between the three factors were only slight, their average was accepted as a final estimate, while if the differences were more than two degrees of clearness, the experiment was disregarded and repeated later on. In a few cases, observer V felt unable to make a judgment about the clearness of the marking, because her attention was too intensely directed to some other task. These doubtful cases were classed in the ninth degree, which represents "least or no attention."

The results of the marking tests agree perfectly with those of the adding tests, and extend the correlation between degrees of attention and quality of work to the lower levels of clearness. Table VI shows the average value and the mean variation of the experiments made at the various clearness levels.

TABLE VI

	Degrees of Attention							
	First	Second	Third	Fourth	Fifth	Sixth	Seventh	8th and 9th
Obs.	Av. MV	Av. MV	Av. MV	Av. MV	Av. MV	Av. MV	Av. MV	Av. MV
C	76 4.9	71 3.0	67 5.3	61 4.5	55 5.0	49 6.0	40 6.9	29 7.6
V	77 3.4	70 3.5	63 5.3	51 5.3	47 5.0	43 4.0	41 5.6	40 5.6
P	85 2.1	77±2.9		71±6.3		55±8.5		34 11.3

Only in the case of P the values obtained at two levels were combined, because they were not very numerous, while the results of B and O, although they show a similar tendency to correlation, were too few to justify their tabulation. The results of C and V were treated also by the Bravais-Pearson formula. For this purpose, the whole range of the calculated values of work, from the lowest to the highest per cent. was divided into nine equal parts, which were then compared with the nine degrees of clearness. The correlation-coefficients obtained under this arrangement were .83 and .73 from C and V respectively. These two figures show beyond doubt that introspectively distinguishable variations of attention are very closely paralleled by corresponding differences in the accuracy of work performed at these different levels of attention, provided that the estimation of degrees of attention is done in terms of clearness values, and that the work itself is as little as possible influenced by anything else but changes in attention.

The objection might be raised that our observers based their judgment of degrees of attention upon an implicit estimation of the value of their work ; this estimation is easier, since daily life offers many opportunities for practice in judging one's own work and ability. If this were the case, however, the correlation between the estimated and the calculated value of the work should at least be just as great as, if not greater than, the correlation between estimated attention and calculated work. But the correlation coefficients for estimated and calculated work were .76 and .28 for C and V respectively, as compared with .83 and .73 in the previous correlation. This difference shows plainly that our observers used a criterion for the judgment of degrees of attention different from that employed in the estimation of the quality of the work. What their criteria were can be best shown by quoting from a summary introspective description given by C and V at the close of the entire work with the marking tests.

Observer C who, like V, preferred to estimate clearness differences in terms of per cent., employs "a sort of schematic visual image of 100, rather complex," but somewhat different from her customary visual image of the whole number-scale. She described the latter as a line going out to the right in a horizontal plane, while the visual clearness scale is a line that goes out straight in front of her, with its farther end slightly raised. She goes on to say :

"Clear out at the other end I can see visually the 100, and that is the clearest part for me of the whole thing, which is becoming very shadowy toward me. The clearness degrees represent themselves to me by the approach they make to the 100 mark. It is a matter of clearness in memory; if the circles stand out plainly in memory (after the experiment) I put them high on the scale, if not, I put them low. There seems to be a cumulative effect of the twenty-four circles in any experiment. Sometimes, however, a single circle stands out strongly, but this does not affect the whole estimate. . . . I find a great difficulty in noticing strain, because something else is so much more interesting that the strains are too much in the background. Sometimes the strains are noticed later (after the experiment), as *e. g.* in a fatigued hand, but ordinarily they are too much in the background to be remembered even later." As to her judgment of the quality of work, C says: "the ease with which I mark enters sometimes into my estimates. I also had often some kind of a general impression as to how many good and bad marks there were. If I could not remember this I took it for granted that my work was poor. It was always pleasant when the work seemed easy, and unpleasant when it seemed hard. The cumulative effect of these individual feelings influenced my judgment of the quality of the work."

These introspections are in close agreement with her results. She has fairly definite, reliable, and constant criteria both for her clearness estimates and for her judgments of work, and hence her correlation coefficients are remarkably high, namely .83 and .76 respectively. She was never in serious doubt about either of her two estimates, and hesitated only rarely in cases of the very lowest degrees. In this respect she differs widely from observer V, who in 20 out of the 130 experiments was unable to tell what kind of work she had done, especially if her attention had not been directed to the marking. V was also much more uncertain about the lowest degrees of clearness, and the averages of work (given in Table VI) for these lower levels are very little different from one another. Her final introspections indicate that she was aware of this weakness, and they also show plainly that her clearness estimate could not have been influenced by an evaluation of her work. She says:

"I valued the circles in terms of clearness. Sometimes I could distinguish between their visual clearness and the motor clearness of marking. Sometimes I was not conscious of seeing the circles at all. The thing attended to was always very much clearer than the others, which receded the more into the background, the higher the other process was. My standard is either maximal or minimal, that is, I estimate from "no attention" upward, or from "maximal attention" downward. With my strongest attention there is no strain sensation, but after the experiment is over I feel relaxed, from which I infer that I must have been strained before, although I did not notice it, because my attention was too good on the task. The scale was not visualized; it is simply a matter of clearness of the object attended to. The quality of work had never influenced the estimate of the attention. In fact the estimate of the work was mostly a guess. I never remember the marking of the circles collectively. If I saw myself making a bad mark I judged my work usually very poor. When my attention is on some other task, I certainly do not focus my eyes upon the circles, and the marking seems to do itself. It must be analogous to the automatic writing of answers to questions without hearing the questions."

There can be no doubt, therefore, that the objection mentioned above does not hold.

The main problem of our investigation has thus received a satisfactory solution. We have shown that the assumed parallelism of the expression-methods, especially as far as the measure of precision is concerned, rests upon solid introspective evidence, if clearness is accepted as the essential characteristic of attention. At the same time we believe that our results have brought us within reach of a new and fairly definite method of measuring attention, for the results have shown that degrees of clearness are just as accessible to introspective determination as variations of the intensity of sensations. This was somewhat surprising to ourselves, because we doubted in the beginning the adequacy of introspection, and expected at best only a rough parallelism between estimated degrees of attention and calculated degrees of precision in work. There is sufficient warrant in our results for the assumption that continued practice will lead at least to a differential clearness limen which may be just as definite and precise as any other psychophysical difference limen, while on the other hand it may be perhaps more difficult to establish the least possible or the highest possible clearness degree for any given mental process. However, the determination of a difference limen for clearness would be the most important step toward an exact measurement of the concentration of attention. It would enable one, by starting with a certain clearness degree of a given mental process under fixed experimental conditions, either to increase or decrease that clearness by just noticeable differences, until the maximum or the minimum of attention to the particular process is reached.

The results of our experiments also give some indication that the increase or decrease in clearness depends not so much upon the nature and number of distracting stimuli as upon certain other factors. The chief among these seem to be the kind and complexity of the tasks involved and the instructions or directions for subsequent introspection. Peters had already shown that attention with *Einstellung* is more concentrated than without it. Now the *Einstellung* itself depends largely upon the *Aufgabe* or instruction. If only one task is to be performed, as, for instance, in our case, the marking of the circles in the normal and the combination patterns, it is necessary before every experiment to state at least whether maximal or normal attention is required, otherwise the *Einstellung* is determined by chance and will result in a larger range of degrees of attention. If two tasks are to be performed simultaneously, for example, the marking together with the reciting, the spelling, or the counting of certain combinations, then it must be indicated which task is to be attended to, and to what extent the other may be neglected. Such an instruction is easier to follow than the requirement to give to both tasks an equal amount of attention, especially if they are very different. How far the nature and complexity of the tasks to be performed determine the degree of attention given to them is shown in Table VII. The vertical columns of figures headed by the 9 degrees of attention indicate the frequency with which the marking in different experiments was done at the various levels. The individual means for each observer as well as the combined means are shown in the last two columns, while the column before gives the total number of experiments in each case. In the first four rows of the Table the clearness of the marking test when done alone is compared with the clearness

TABLE VII

No.	Nature of Expts.	Obs.	Degrees of Attention 1	2	3	4	5	6	7	8	9	T.	Means Ind.	Comb.
1	Norm. and Comb'n-P	C	9	20	9	5		1				44	2.32	2.3
	Marking Circles	V	13	13	10	7	1					44	2.31	
2	Norm. and Comb'n-P	C		3	2	8	14	1				28	4.28	5.0
	Marking and Reciting	V			1	4	6	7	8	2		28	5.81	
3	Norm. and Comb'n-P	C			2	4	9	7	4	2		28	5.46	5.8
	Marking and Spelling	V				3	5	8	7	5		28	6.21	
4	Combination-P	C			3	2	6	6	7	3	3	30	6.1	6.2
	Attention to Comb'n	V			3		4	8	11	2	2	30	6.3	
5	Normal Pattern	C	4	9	9	8	6	3	1			40	3.4	3.54
	All possible Tasks	V	10	8	3	4	5	3	3	4		40	3.67	
6	Letter-Pattern	C	1	6	3	3	9	2	6			30	4.43	4.9
	All possible Tasks	V	1	2	4	3	3	6	8	2	1	30	5.37	
7	Digit-Pattern	C	2	5	1	4	9	4	1	2	1	29	4.55	4.8
	All possible Tasks	V	2	2	4	2	3	8	7	1		29	5.0	
8	Color-Pattern	C	2	3	3	3	5	6	3	3	2	30	5.0	5.3
	All possible Tasks	V		1	3	5	5	6	8	2	1	30	5.58	

of the same test when most of the attention is given to reciting, to spelling, or to some combination of letters, digits, or colors. The latter kinds of task require very high degrees of clearness for themselves and permit only very little attention to be given to the marking, namely about the 6th or 7th degree as a rule, the two means being 6.1 and 6.3 for C and V respectively. Spelling allows more attention to be given to the marking, and reciting still more, while of course the marking alone may involve the very highest degree of clearness. The influence of the complexity of the patterns is shown in the last four divisions of the Table. Work done with the normal pattern permits as a rule the highest attention to the marking, the letter and the digit patterns allow very much less, while the color pattern allows the least attention to the marking of the four kinds of patterns. However, we do not wish to place too much emphasis upon these results, because the number of experiments is not large enough for general conclusions of this sort to be based confidently upon them.

Finally, another point in regard to which the large introspective material accumulated in the course of our investigation reveals some interesting evidence, is the number of simultaneous clearness levels and their relative distance from each other. It is frequently maintained that extreme concentration to one group of mental processes is accompanied by an equally extreme obscurity of other mental processes forming a general background, and that less concentration is simultaneous with correspondingly less obscure background processes. Pillsbury expresses this by saying: "the amount of attention is practically constant and cannot be applied to one object without affecting the clearness of others".[1] So far as our knowledge goes, such statements have been based on casual observations only, and we have, therefore, thought it advisable to present the following Table VIII as giving a numerical expression to the facts in question. The 9 large horizontal divisions indicate the 9 clearness degrees which by introspective estimation were assigned to the marking test. The figures in the third vertical column tell how frequently with each one of the three observers O, P, and V, the marking was estimated at any particular level. The next 9 vertical columns of figures show in how many cases other mental processes simultaneous with the marking at a certain level were mentioned under a certain degree of attention. Thus, for instance, in 24 experiments O estimated his marking at the highest level and in the same experiments assigned to other simultaneous processes in 2 cases the 6th degree of clearness, in 10 the 7th degree, in 12 the 8th degree, and in 6 cases the lowest or 9th degree. In some of these cases an observer was able to mention 2 or 3 individual contents and to assign to them either the same or slightly different degrees, in other cases he would give merely their average degree, while in a few cases he remained doubtful. The last mentioned were omitted from the Table, likewise those experiments in which the attention to the marking varied between more than two neighboring degrees. In the case of observers O and P several practice series were included in order to get a sufficient number of results for tabulation. The Table shows as definitely as can be expected under our experimental conditions that the frequency-modes of distribution for the simultaneous processes at first gradually approach, starting with 1 and 7, then 2 and 6, then 3 and 5, then they fuse in the 4th and 5th horizontal division, and finally they change places with each other, occurring now at 6 and 3, at 7 and 1, at 8 and 1, and lastly at 9 and 1. At the 4th and 5th degree of marking, there is no mode distinguishable for the other mental processes, because here we have a more or less uniformly distributed attention without much of a focus or a background. At the 6th level, however, the dual division becomes again apparent and remains so for the rest of the Table. This

[1] Pillsbury: Attention, 1908, 9.

TABLE VIII

No.	Obs.	T.	Degrees of attention									Total
			1	2	3	4	5	6	7	8	9	
	O	24						2	10	12	6	30
1	P	13		2	1		1	5	6	2	4	21
	V	33							4	1	5	10
	3 O's	70	0+70	2	1		1	7	*20*	15	15	71
	O	8				1	1	5	1			8
2	P	22				2	3	9	8	5	1	28
	V	13				1	4	1	2		3	11
	3 O's	43		0+43		4	8	*15*	11	5	4	47
	O	0										0
3	P	13				2	7	6	4	2	1	22
	V	14		1	2	4	1				7	15
	3 O's	27		1	2+27	6	*8*	6	4	2	8	37
	O	1		1								1
4	P	5			1	1	3	2	1			8
	V	14	1	2	7	3	1	1			3	16
	3 O's	20	1	3	*8*	4+20	4	3	1		3	25
	O	3	1	3				1				5
5	P	5			4		1		3	1		9
	V	16	5	4	3	3	1	1			5	22
	3 O's	24	6	7	*7*	3	2+24	2	3	1	5	36
	O	6		4	1				2		2	9
6	P	6			3	1	1	2	3	2		12
	V	23	6	5	7	1	2	2			8	31
	3 O's	35	6	9	*11*	2	3	4+35	5	2	10	52
	O	6	2	2	2	1			2	3	1	13
7	P	0										0
	V	26	19	2	4	3	2	1			12	42
	3 O's	32	*21*	4	6	4	2	1	2+32	3	13	55
	O	8	3	6						3	3	15
8	P	3		2	2				1	2		7
	V	6	6								2	8
	3 O's	17	*9*	8	2				1	5+17	5	30
	O	3		3	1	1						5
9	P	0										0
	V	5	4	1		1					6	12
	3 O's	8	4	4	1	2					6+8	17

mutual relation of the two clearness levels in an attentive consciousness may perhaps be compared to the two levels of a quicksilver column occupying about half the space of a U-shaped glass tube. The one end of the column cannot rise to its maximal height without depressing the other to a minimal height, nor can the one end move toward a medium high point without causing the other end to approach approximately the same level, but of course from the opposite direction. This analogy can apply, however, only to the two-level formation of attentive consciousness, which since Leibniz has been known under the terms apperception and perception.

This distinction has not been unanimously accepted by modern psychologists as a universally valid description for all possible states of attention. Some of the authors[1] discover in their own consciousness processes of more than two levels of clearness, while Wirth even seems to hold that at any moment of attention all possible degrees of apperception may be represented by simultaneous processes and that only "under certain conditions, which favor a kind of dual division, it is possible that a region, to which a fairly uniform attention is given, may be opposed to a 'background' or 'periphery' of consciousness, to which attention is as uniformly denied".[2] To this statement we may reply that according to our results the occurrence of the two-level formation in the attentive consciousness does not depend upon "certain conditions", but upon the individual observer. We believe we have sufficient evidence to justify the assumption that there are two more or less distinct types of observers, those for whom the two-level division is the most natural and most common, and those who experience as a rule several levels of clearness.

Of our 5 observers B, C, O, P, and V, who worked under identical experimental conditions, the last three reported without exception the dual division of clear focus and vague background. There is, of course, the possibility that during the experiment, which lasted at least 12 seconds, processes of an intermediate degree occurred but were too fleeting to be recalled later. The first two observers, B and C, on the other hand, frequently mentioned such processes on intermediate levels of clearness. Here again it might be objected that our experiments did not deal with a simultaneous consciousness, but with successive states of attention, which might have exhibited a variable maximum of clearness and thus, if viewed as a whole, would present simultaneously what really was experienced in succession. We can only offer in reply the introspective material of our observers. We therefore quote a few passages from their introspections:

Observer B

(1) Clearness of digits varied from 3-7, some attention was given to the whole row, a few individual digits were as high as 3 in clearness. Circles pretty dark, never higher than 5, and usually 8, while the

---

[1] *Cf.* Titchener's discussion of this topic, Lectures, 220-228.

[2] Wirth: Phil. Stud. XX, 493; *cf. Amer. Jour. of Psychol.* XX, I.

marking was almost automatic, about 7-8. Other processes, *e. g.*, the noise of apparatus darkest, 8-9.

(2) Clearness of colors as whole rows 4-5, marking visually 5, as motor adjustments perhaps 6. In the background mostly verbal comments, 6-7. Sometimes flashes of colors, especially red and yellow, rose occasionally as high as 3.

(3) Focal clearness was distributed among the circles which got 3. Letters were quite varying, those next to the circles between 3 and 6, others 8. The general conscious background was very dark and dead, about 8 or 9, mostly verbal images, saying "I must avoid the letters", or noise of apparatus, and the general experimental setting.

(4) Focus on average 3, mostly visual and kinæsthetic. The background as a rule 8, made up as usual. A few distractions, chiefly verbal ideas, rose as high as 5 at times.

Observer C

(1) Marking of circles 90%. Individual colors not noticed, perhaps 5%. But the rows as such were confusing, the difficulty of finding circles about 50%, being mostly strain in hand and eye accompanied by verbal comment.

(2) Marking the circles about 80%, the digits not more than 10%, they were scarcely seen. But the noise of the apparatus sometimes rose as high as 40% and was markedly unpleasant.

(3) Marking 70%, colors about 30%, noise of apparatus 50%, rose so high especially when it suggested ideas of railroad, which were mostly visual and about the same in clearness.

It must be added that even with these two observers the dual division was experienced at times; but it is significant that B, previous to his participation in our experiments, had firmly believed in the universality of the two-level formation. Among the undergraduate students who took part in the preliminary experiments on clearness, we also found some insisting that in their experiences during these experiments as well as in their daily life the dual division was only rarely realized. We must, of course, admit that in daily life we do not usually analyze our consciousness, nor do we then sharply distinguish between psychological clearness or prominence in consciousness, logical clearness of the meanings and relations, visual clearness or distinctness, and differences in intensity of simultaneous processes. Nevertheless our observers were aware of these distinctions at least during the hours of experimentation. Adding to this the fact of the difference of opinion among expert psychologists upon this very point, we are led to make the assumption of two types of attentive consciousnesses, the dual division type and the multi-level type.[1] Whether these two types are mainly a matter of individual difference of constitution or of training and habit, or whether they depend upon certain psychophysical conditions within the same individual, must be left for experiments to decide. The distinction is not

[1] Such a possibility is, if not admitted, at least implied in Titchener's Lectures, 228.

intended to be a substitute for Titchener's hypothesis that in the two-level formation there may occur slight differences of clearness both in the focus and in the periphery, but is offered rather as a supplement to serve its purpose until a better explanation is proposed to account for the differences of introspection regarding the number of simultaneous clearness levels in the attentive consciousness. Our results furnish, as a matter of fact, many instances for the coexistence of small clearness differences on both the upper and the lower level of attention.

In conclusion we may briefly restate the general results of our investigation :

(1) A very close parallelism was found to exist between introspectively distinguishable variations of attention and corresponding differences in the precision of work performed at these levels, under the condition that the estimation of degrees of attention was made in terms of clearness and that the work itself was not influenced by anything else but change in attention.

(2) Under the same conditions the introspective estimation of the quality of the work was not as reliable as the evaluation of the degrees of attention.

(3) It seems possible that by continued practice a differential clearness limen may be established which would be of great assistance in the measurement of attention.

(4) The degree of concentration does not depend so much upon the nature and number of distractions, as upon the nature and complexity of two simultaneous tasks and the preliminary instructions regulating *Einstellung* and direction of attention.

(5) There seem to be two types of the attentive consciousness, the dual division and the multi-level formation.

(6) In the dual division type of attention a reciprocal relation exists between the two levels ; that is, the higher the apperceptive level rises, the lower the perceptive level falls, and conversely.

AN EXPERIMENTAL STUDY OF EXPECTATION[1]

By William Henry Pyle, Ph. D.

CONTENTS

Psychology uses many terms designating states of consciousness, which are much in need of accurate definition. Such terms as surprise, belief and expectation are often used, but no one has ever carefully analyzed the states which these terms designate. The particular patterns or types of consciousness for which they stand have not been adequately described. The present study attempts to perform this service in the case of expectation. If expectation is a state of consciousness having characteristics which differentiate it from other states, characteristics which constitute it a distinct type, careful investigation ought to disclose these characteristics. On the other hand, if expectation has no constant characteristics which differentiate it from other states, investigation ought to reveal that fact. The experiments reported in this paper were begun in the hope that they would enable us either to give an accurate description of the expectant consciousness, or to prove that expectation has no distinguishing characteristic.

There are plenty of psychological studies which describe the effects of expectation on other mental and physical states and processes. We find described the effect of expectation on reaction time, on perception, on apperception, on feeling, on illusions, on disease and the curing of disease, but we seldom find any attempt to describe the state itself. It is with this

[1] From the Psychological Laboratory of Cornell Uuiversity.

description that the present paper is solely concerned, and no reference will be made to the effects of expectation, or to its relation to other states and processes.

An account of the treatment that expectation has received from psychologists in the past is difficult. It has been treated from very different points of view, so that it is not always easy to make out an author's meaning. And such treatment as the subject has received has usually been quite incidental, often a mere mention in the consideration of some other subject. We find it considered under emotion, and also under cognition; it is sometimes called a feeling, sometimes a process or attitude. The reason for this state of affairs is plain. In all the literature, we can find no account of a single experimental study of expectation. Systematic psychologists give it either casual treatment or none at all; and when they have anything to say on the subject, their account seems to be based on general principles, or on observation incidental to the reaction experiment or some other experiment into which expectation enters as a factor. Sometimes, indeed, they may rely on mere psychological tradition; and one may remark that a good deal of psychology is still mere tradition. Psychological interest takes certain directions, and certain fields are more or less carefully explored, until their psychology comes to be written with considerable accuracy. But when the systematist comes to a subject that has not been carefully worked out from the experimental standpoint, all that he can do is to depend on his everyday experience and on what he finds in the literature. This is precisely the case with expectation. There has been only one psychological monograph written on the subject, that of C. Hitchcock; and this was written, so far as one can tell by reading the paper, without the performance of a single experiment.

There are, perhaps, several reasons why no one has yet worked out the nature of the expectant consciousness. In the first place, every one knows what is meant by the term, every one knows an expectant consciousness when he meets it. Why, then, go to any further trouble about it? In the next place, when one goes beyond a mere general account, and undertakes to give an accurate description of the expectant consciousness, one finds this special task as difficult as the general task is easy. One has to deal with the relation of images and kinæsthetic sensations to perceptions and to one another. One has to operate not so much with qualities as with the attributes of clearness, time and intensity. The study of the relative clearness-values and temporal relations of images and kinæsthetic (and other organic) sensations presents unusual difficulty to an analytical introspection. Only a person of long training and practice in introspection is competent as an observer. The average observer in a psychological laboratory misses the very things that one must know to be able to describe the state of expectation. The kinæsthetic and other organic sensations may even escape his notice entirely. He may speak of images when the actual content of his consciousness is kinæsthetic sensation. It has been the writer's good fortune to have as observers graduate students of several years' experience in introspection. Finally, there has been no great incentive to an analysis of the expectant consciousness. The effects of the state in shortening reaction time, in producing illusion, its effect on discrimination, on apperception, and on association are fairly well known. And since every one knows in a general way what the expectant consciousness is, only one interested in the analysis and description of all conscious states and processes for their own sake could offer an apology for further excursion in this field. Such an interest the author confesses to have, and such an apology he begs to offer.

Although the psychology of expectation has fared badly in systematic psychology, the literature of the subject cannot be neglected. For more than one writer has clearly conceived the nature of the problem, and some have come near to the correct solution. Our treatment of this literature, however, need not be exhaustive. We shall mention only those authors who have had something pretty definite to say. Most recent writers define expectation in terms of attention, and, in particular, as a preparatory attention.

Külpe[1] treats expectation as a form of attention in which one is preparing for a coming state, process or content. This form of attention owes its specific characteristic to certain complexes of organic sensation, and may be either pleasant or unpleasant. The effect of expecting pleasantness or unpleasantness is the same as the effect of attending to these affections, *i. e.*, it reduces their intensity or entirely prevents their arrival. The feeling-tone of an expectant state is complicated by the fact that the idea of the expected impression may have its own specific affection, and the strains and other sensations of the state have also their affective tone. For instance, the idea may be pleasant and the sensations unpleasant. The feeling tone of the state as a whole is then determined by the stronger of these affections. For Külpe, accordingly, expectation is consciousness when one is preparing for an impression that is about to come. This preparation consists in such things as a favorable attitude of the body, an adjustment of the sensory

[1] O. Külpe: Outlines, 1909, 39, 261, 320, 323.

apparatus, the arousing of ideas of the coming impression, verbal ideas of the coming task or judgment, and so on.

Lehmann[1] describes expectation mainly in biological terms, but in so far as he is psychological, in terms of attention. He says that, in expectation, the attention is directed upon an image and also upon an action or movement to be performed. He means, of course, that one attends to the idea of a certain stimulus that is to come and also to the image of a certain movement that is to follow the stimulus. This idea of movement brings about an innervation of muscles, particularly of the muscles which regulate the internal organs, and it is normally these organs that are excited during expectation. However, Lehmann differs from Külpe in holding that expectation is always unpleasant.

Sully[2] agrees in the main with Külpe. He points out the fact, however, that a succession of impressions must previously have taken place if one member of a series of impressions is to call up the idea of the next, and thus cause the next to be expected. And this expecting of the next impression consists merely in the having an idea of it, and in having the proper bodily attitude for its reception, together with a readiness to act in conformity with the occurrence. The conscious aspect of this readiness is strain sensation. Sully speaks of this as a *strenuous activity*, a *stretching forward of the mind*, and differentiates the state from memory, which he calls a passive state of mind, by the characteristic of "tension, effort or strain". Sully also points out that a certain interval of time must elapse after a perception, or the idea of a coming impression will not arise.

Titchener[3] also treats expectation as anticipatory attention, and says that he means attention to the idea of something that is to happen in the future. He points out that the term expectation should be applied to a certain *state* of consciousness and not to an emotion accompanying that state. It is well to have had this distinction made, for it is in this connection that we find most confusion in the literature. Many writers speak of the emotion or feeling of expectation when they really mean the feeling accompanying the strain sensations prominent in the expectant state.

Wundt[4] also considers expectation a state of consciousness in which the attention is directed not upon a present but on a

[1] A. Lehmann: Die Hauptgesetze des menschlichen Gefühlslebens, 1892, 394.

[2] J. Sully: Outlines, 253-258.

[3] E. B. Titchener: Primer of Psychology, 153.

[4] W. Wundt: Grundzüge der physiologischen Psychologie, 5th ed., iii, 346.

future impression. He means, apparently, on the idea of the future impression; for one cannot attend to something not yet in the mind. Wundt finds in expectation three constituents: strains, memory images, and a characteristic feeling of unrest. He considers the feeling the essentially important thing in the expectant consciousness, and says that it becomes painful if the strain sensations are much prolonged. The memory elements, on the other hand, may sink to a lower level of consciousness, from which they *strive toward apperception*. It is, however, difficult to understand how expectation can be a state in which one is attending to memory images, while these images still remain outside the focus of attention.

This striving toward apperception introduces us to Lipps[1] who, in the peculiar terminology of his system, speaks of expectation as a *striving*. According to Lipps, in expectation, an idea is striving toward sensation or perception. In another connection, he calls expectation an associative striving. Lipps seems to mean, interpreted in more familiar terms, that a consciousness whose main content is an image *striving to become* a perception is an expectant consciousness. This striving, which he attributes to the idea, represents merely the strain and other organic sensations which are given in expectation.

Dewey[2] may be mentioned as a representative of those psychologists who have called expectation an emotion. For him, expectation is the feeling that accompanies the stretching forward of the mind. Lipps and Wundt say that expectation is just this stretching forward, this striving; Dewey, that it is the feeling that accompanies the striving, not the striving itself.

Hitchcock[3] who, as mentioned above, has written the only psychological monograph on this subject, considers expectation a state of consciousness in which images or ideas are regarded as substitutes for sensational contents which are to be later experienced. Her article is considered at length on a later page.

In addition to the views given above, there are formal and logical treatments, of which Ward's may be taken as typical. We take no account of those in this paper, since they are theoretical constructions that must be examined and tested by appeal to the facts.

It is evident from this brief outline, that the tendency among psychologists is to regard expectation as a form of attention,

---

[1] Th. Lipps: Grundtatsachen des Seelenlebens, 669; also, Leitfaden der Psychologie, 217.

[2] J. Dewey: Psychology, 273.

[3] C. Hitchcock: The Psychology of Expectation, Psych. Rev. Mon. Sup., v, No. 3.

namely, anticipatory attention. Some authors think that there is a characteristic feeling tone, others that there is no common feeling element.

Expectation of Visual Stimuli

Experiments with the exposure apparatus. We now turn to the experimental work which was done in the Cornell laboratory during the years 1907-1908 and 1908-1909. At the beginning of the work we made the general assumption that expectation is a state of consciousness beginning with a sensation or perception and followed, after a short interval, by another sensation or perception. The first sensation or perception gives rise to the expectation of the second sensation or perception. We conceive the problem to be, chiefly, the determination of the type or pattern of consciousness which lies between these two sensations or perceptions. What is the mind like when it is waiting? What is it to await a thing with the assurance that it will happen? What is the difference between the relation that exists between two perceptions that are connected by the tie of expectation, and that existing between two perceptions otherwise connected? These were our problems. The direction which the experiments should take was suggested by everyday experience. To give an instance: I stand by my window looking out over the town and see the steam rising from the mill-whistle. I *expect* that in a moment I shall hear the sound of the whistle. Put into general terms, this experience of the whistle is somewhat as follows: An invariable succession of stimuli after a time causes the second to be expected when the first of the series is perceived. We therefore decided that what was needed to solve the problem was to bring the mill-whistle into the laboratory. We planned means of giving the observers an invariable succession of stimuli, not only in the field of audition but also in vision, touch and temperature. We constructed an apparatus that would serve for both vision and audition. This apparatus will be designated the exposure apparatus. It consisted of a pendulum, 130 cm. long, having a large bob of lead. The pendulum swung from a cross-bar of wood mounted with sharp iron points resting on metal plates. The mounting gave practically no friction. By rack and pinion attachments the wooden bar supporting the pendulum was made to move a carriage in the horizontal direction. This carriage carried the stimulus to be exposed, and in front of the carriage was a large black cardboard in which were the exposure holes and slit. From the cardboard hung a black cloth, entirely concealing the apparatus behind. The two exposure holes were 1½ cm. in diameter, and the exposure slit was 2 cm. wide and 25 cm.

long. The pendulum was held at the top by a clamp and set-screw, so that its length could easily be changed, and therewith the rate of speed of the carriage.

(1) *Expectation of colored stimuli after an auditory signal*

This, the first set of experiments, was performed essentially as follows. The observer was seated about one meter in front of the exposure apparatus, and instructed to look at the hole; behind this exposure-hole the carriage carried a small colored disc, and as a color passed the hole it could be seen by the observer. The carriage, at the beginning of an experiment, was pushed as far as it would go to the observer's right, and the pendulum was fastened to one side, ready to swing, and held in position by a clamp. To release the pendulum the experimenter struck the clamp with a piece of wood. The noise of this stroke was the signal for the experiment to begin. At first, the observer was told nothing about what would happen; he was only to look at the exposure hole. In a very short time, the signal stroke threw the observer into the expectant condition. In order to get the best possible analysis of this expectant state, we varied the experiment in all possible ways. The colors could be known, and the time between the signal and the appearance of the color could be known; the color could be known and the time unknown; the color could be unknown and the time known; or the color and time could both be unknown.

The observers for this experiment were Dr. L. R. Geissler (G), assistant in psychology; Dr. T. Nakashima (N), fellow in pyschology; Miss Cheves West (W), fellow in psychology; Miss Helen Clark (C), scholar in psychology; Mrs. Helen Piotrowska (Pi), and Mr. W. S. Foster (F), graduate students in psychology. By the help of an assistant, the writer (Py) was himself able to make several hundred observations.

We give first the results obtained from G.

Time and color known. The color used was green. The green was followed in about three seconds by blue. This experiment was repeated 100 times in the course of a month. G's consciousness may be characterized by saying that it consisted almost wholly of verbal ideas; there was an almost total lack of memory ideas, there were practically no visual images of the coming color, and there was no feeling. Only twice was there any hint of an image of the coming stimulus; and, in these cases, there was no color in the image; it had the form of the stimulus but was gray. It may be said, however, that this observer was not visually minded. At the beginning of the experiments, he hardly knew, from his own experience, what a visual image was like. Later on in the work, he frequently got visual images.

Before quoting from G's introspective reports, it is necessary to add a statement of the directions given the observers in regard to their reports. They were asked to report the contents of consciousness that followed the signal given at the beginning of the experiment. They were also told to describe the contents with reference to the distribution of clearness and intensity and the temporal relation existing among the different parts of consciousness. They were further instructed to report any affective tone that might accompany the ideational content. G, now, gave such reports as these: "I kept saying 'when is the blue coming?' expecting it with every shift of the green. I heard myself saying 'now, this time' and so on, until the color was there."

"I noticed at first that the green seemed to shift faster than usual, but even then thought this an illusion. I noticed quite distinct quivers in my tongue, as if I wished to say something. I interpreted these as tendencies to verbal expression. I began to think of the color blue, this was done in verbal terms, though quite unintentionally. My emotional mood was quite uneven and variable."

"I kept saying 'blue, what is blue like?' After that, I *felt like a conscious blank*. I wondered, finally, why no ideas came to me; there was the staring at the moving color; but noises and organic sensations, which I notice while giving this report, I did not notice while observing. I cannot tell what was in the focus of attention."

"I feel that I am staring blankly at the hole," he reported in one experiment, and added this comment: "From my experience in everyday life, I remember that this has always been so. Before the beginning of a play or concert at the theatre, I am the dullest blankest mind imaginable, often ashamed of it but helpless."

Stimulus unknown, time known. In this experiment G. was told what the time of waiting would be, but did not know, in any given case, what the color of the stimulus would be.

G's experience was practically the same as in the preceding experiments, as a few quotations will show. "I was definitely expecting another color of the same size and kind, this in verbal terms. I had no visual images. I asked myself how a red stimulus would look, but no red visual image came. When the stimulus came and was actually red, I had an intense feeling of pleasant relief, but do not remember any organic sensations. The mental processes forming the conscious background were very vague."

"Color came before it was expected. The time interval was vaguely estimated; and after a certain lapse of time, the ideas came 'the next swing of the pendulum will bring the color'; this was muscular or organic."

"This time, the idea of time was most prominent. I was trying to see what the idea was made up of, and this caused the interval to seem somewhat shorter. I said to myself 'it ought to come now' long before it came. I was greatly interested in the idea of time and had no images of color."

Time unknown, stimulus known and constant. Nothing new came out in this form of the experiment. The verbal ideas reported usually had reference to the time of appearance of the stimulus.

Stimulus and time both unknown. The type of consciousness here was, in general, the same although organic sensations were sometimes reported, especially in the longer waits. G. would say, "For the first time I was conscious of my eyes, and of an effort to keep them fixed on the hole; I felt a little excited organically."

"I had expected, at the very beginning, a blue; this expected blue existed in the form of a verbal idea."

The results obtained from N. were quite different so far as the contents of consciousness were concerned. N. had visual images of the color where G. had chiefly verbal ideas. N. also had more organic sensations than G. N. seldom reports any verbal ideas. With this observer we performed some 250 experiments, distributed among the four variations of the experiment as described above. His introspections were often more general than were G's. We give typical sentences. When the time and color were known, N. would say: "I had the idea of blue while looking at green." Again, "Strong muscular sensations." "Too much absorbed in green, so that there was not much idea of blue." Again, "At the latter part of green, the idea of blue came in and gave a feeling of satisfaction." Again, "From the early part of green the idea of blue came in, and the attention fluctuated between the perception of green and the idea of blue." N. gave this report many times. He had very vivid visual images. At other times he would say, "From the beginning of the green I had idea of blue while looking at the green." Again, he said, "Vivid but fluctuating idea of blue while looking at the green." In nearly all the above cases, N. reported strain sensations in the eyes. Several times he reported that the memory image of the blue actually mixed with the perceived green and gave him a blue-green. For example, he said, "While expecting the next color there was a fused or mixed color sensation of the present sensation with the expected color, the sensation green with the idea blue." With the stimulus varied and unknown to the observer, while the time was kept constant, N's introspections were much the same. He reported ideas, strain sensations from the eyes and organic sensations. The ideas, however, since he did not know what color was coming, were usually memory images of the color given in the preceding experiment, sometimes ideas of several colors which had been given him in recent experiments. He would, for example, say: "The idea of yellow was present till the appearance of red, whose appearance was very striking because I had never expected that color." Again, he says: "I had memory of what had happened before in the last experiment; it was perfect memory of the order and arrangement of expected ideas, the actual appearance of color and the affection at the time. The appearance of dark brown was strikingly unexpected." N's introspections in the other two types of experiment were not different in any essential. Many of his observations, especially when the nature of the stimulus was unknown, appear not to fall at all into the category of expectation. The signal color would appear and give rise in N's mind to various colors previously seen. Since he seldom speaks of verbal ideas, it is often difficult to say whether he expected any particular color or, indeed, expected anything.

We did not give C. all the variations that were given to the first two observers, but instead passed before the exposure hole a series of four small colored discs on a white cardboard. This series was repeated many times. C. combines, in a measure, the characteristics of both G. and N. After one perception, she would have usually visual images of the coming color and also verbal ideas of it. She seldom reports strain or organic sensation. The colors of the discs were red, yellow, green and blue, and appeared in that order C. would report: "After the experiment began, and before the first color appeared in the hole, I had visual image of all four of the colored discs; when the first one appeared I had image of the remaining three colors, and so on to the end, and I said the names of the coming colors each time." Again, "I had image of red alone, then of yellow alone, then of green and blue together."

W. was given the same succession of colors as C. The experiment was not carried very far with her, however, since her experience was much the same as that of N. During the waiting period of the experiment she would have visual images of what was coming; but she reports few organic sensations and also few strain sensations. She would usually give some such report as follows: "I expected the colors in the correct order; had image of them all in a row before the first color appeared."

(2) *The successive appearance of the same visual stimulus*

The purpose of this experiment was to repeat under laboratory conditions a type of experience rather common in everyday life. A piece of colored paper was fastened to the carriage, and passed first by the right and then by the left hole. The interval between the two appearances could be varied by changing the rate of the pendulum. The appearance of the color in the one hole was the signal for its future appearance in the other hole. The difference between this experiment and the preceding will readily be seen. A color appears, then disappears, and in a moment appears again at another place. To see the color the second time, the direction of gaze must be transferred to the second hole. We found this form of experiment one of the most fruitful for a study of the subject, and repeated it many times. There were, however, only two observers, C. and Py.

The usual report of C. was as follows. She would perceive the color in the first hole, and this would be focal until the color moved by and passed out of sight. Then her eyes would move to the next hole and fixate that; frequently there were strain sensations from the eyes, and sometimes more generally from the body. There would arise a visual image of the color coming into view behind the second exposure hole. This visual image appeared in nearly half (41%) of the observations; when it did not occur, kinæsthetic and organic sensations, and also verbal ideas, were always prominent.

In the writer's case (Py), kinæsthetic sensations were practically always present (in 90% of the cases). Organic sensations were present in 70%, verbal ideas in 57%, while images were reported in less than 8% of the observations. Images played a much less important, and kinæsthetic and organic sensations a much more important part, than in the case of C. In the earlier experiments, verbal ideas were more common than in the later. Quotations from the introspections will illustrate this type of experience.

(C.) "Saw the holes, then the red square as it came by the first hole; it seemed to stop there, and I had the verbal ideas 'it is slow'. Then it passed. While waiting for the color to appear in the second hole, I had visual images of the red square crossing the hole. Unpleasant."

(C.) "Saw the holes; saw the color pass the first hole; had image of the color in the second hole moving a little, not clear; verbal ideas 'it is coming now'; organic sensations from breathing; then color came to the second hole. No feeling."

(C.) "Saw holes and shadows, then color coming; verbal ideas 'it is very slow'; then, before the color came to the second hole, had the verbal ideas 'the holes seem yellow'; no image; rather pleasant."

(C.) "Saw holes; had strain sensations in eyes, organic sensations from breathing; then noticed the second hole; vacant stare till color came; unpleasant."

(C.) "Saw the first hole, second hole in the background; motion of the carriage attracted my attention to the second hole; strain sensations from the eyes; then my attention went to the first hole; saw the color pass; then moved my eyes to the second hole; strain sensations from my eyes again; no image; no feeling."

(C.) "Saw shadows, then color pass the first hole, then visual image of the color passing the second hole, slightly pleasant." And so the reports run. In general, it may be said that the observer moved her eyes to the second hole as soon as the color had left the first hole; then she would often have an image of the color passing by the second hole, unless organic sensations, kinæsthetic sensations, or verbal ideas were in the focus of attention. These four constituents of the waiting consciousness are so vague that when one or two rise to the focus of attention, that is to say, to a fair degree of clearness, the others are very low in clearness-value and often below the limen of consciousness.

The writer's introspections run as follows. (Py) "Saw the first hole; then as the color appeared, I said 'red'; then the left hole was in the focus of attention; there were organic sensations from breathing, and these were slightly unpleasant."

(Py.) "Saw the first hole, heard the noise of the pendulum, fixated the left hole and had strain sensations from the eyes and neck; then saw the red paper pass the second hole."

(Py.) "Said 'now it will come'; then saw the red paper in the first hole; had bodily and eye movements corresponding to the movements of the pendulum; these were felt consciously as organic sensations and kinæsthetic sensations. I began to say 'now it will come'; perception of the left hole and strain from the eyes in the background, organic sensations rather high in the background."

(Py.) "Said 'here it is, now it comes', then my eyes fixated successive portions of the cardboard between the two holes; these fixated portions of the screen were in the focus of attention; next came the verbal ideas 'now it will come', and I fixated the second hole, which then rose to the focus of attention with definite strain sensations from the eyes and neck; then the color appeared in the second hole."

(Py.) "A general strain coming from tense muscles holding the head, neck and eyes still; eyes steadily fixated the right hole; the perception of the right hole was in the focus of attention, then the color, and then the cardboard between the two holes, then the left hole; increasing strain sensations, a quiver in the throat; a suppressed, nascent 'now'; the color appears in the left hole."

(Py.) "Color in the right hole; then verbal ideas 'here it is here it is' as the eyes moved with the pendulum noise, fixating successive points between the two holes; in a short time, quite involuntarily, my eyes fixated the left hole and it sprang into clearness; strain and organic sensations; then came the color." These illustrations are sufficient to show the character of the experience.

Auditory Stimuli

Two classes of experiments were performed with auditory stimuli, (1) with noises and (2) with tones. The first class of experiments was performed with the exposure apparatus. Attachments on the carriage were so arranged that they pushed off steel balls and caused them to fall into a light wooden box, thus producing a sharp noise. The height of fall, and therefore the loudness of the noise, and also the interval between the first and second noises, could be varied at will. The second class of experiments was performed with tuning forks.

(1) *Experiments with Noises*

The general method of this experiment was the same as that first described with colors. The observer was seated before the apparatus and told to listen for sounds. The pendulum was released as before, the blow that released the pendulum making a sharp noise that served for the starting signal. The moving carriage pushed off successively two and, in some cases, three balls. To change the intensity of the sound, we raised or lowered the boxes into which the balls fell. The observers were given the same directions with regard to their introspections as in the case of visual stimuli.

Observer F.

While waiting for the sounds, F., after the experiment had progressed for some time, had auditory images (memory images) of the sounds of the falling balls, sensations of strain from the throat as if trying to produce the sounds, and visual images of falling marbles which he imagined the balls to be. He also reported strains about the ears. His introspections run like the following: "After the first noise I had auditory image of what the next sound was to be like, also had visual image of marble falling through hole and dropping into a box." Again: "I looked attentively at the screen, at a place which I thought was directly in front of the marbles, and not at the place whence the sound is to come. It seems as if I wished my eyes to aid my ears. I kept imaging the marbles dropping. Had visual image of the box into which the marbles were to drop, although I have never seen it, and have never seen the mechanism of the apparatus behind the screen." Again: "My sound images were not very distinct; my mind was filled with visual images of what was happening behind the screen; was under great strain." And again: "Had visual images of balls falling." At another time he says: "After the first noise, had very distinct memory image of it; strains in the muscles below and in front of the ears; the image of the sound to come was very indefinite; my *attention was not on it and seldom is.* Had a tendency to make a sound like the coming one, *i. e.*, I had kinæsthetic speech images."

N., while waiting for the second noise, had auditory images of the noise, sensations from the throat in trying to produce the noise, and strain sensations about the eyes and face. He says in his reports: "There were subjectively reproduced sound-sensations from my vocal organs, as if I was myself producing the expected sound." At another time he says: "In all these expectations there are kinæsthetic sensations from my eyes, mouth and throat, which correspond to those in actually hearing and producing the sounds."

Pi., while waiting for the second sound, had no verbal ideas, but had auditory images of the sound as remembered, visual images of falling and rebounding balls, and strains about the face. The following are typical introspective reports: "Had in memory the succession of sounds of rebounding balls, as in preceding trials." "Attention was on image of the expected sound; there were strain sensations from the teeth which were tightly pressed together, and from lips which trembled a little." "Time seemed longer; there was strain sensation from a slight tension of the limbs, and also sensations from compressed teeth and trembling lips. The sound of the falling balls was clear and pleasant; a faint picture of a silvery ball falling appeared." "Pronounced muscular tension in the upper part of the body, organic sensations from the holding of the breath, strain sensations from twitching of the eyelids; said verbally 'it is coming, coming, coming'."

(2) *Experiments with Tones*

Three tuning forks, c', e', and g', mounted on resonance boxes, were used for this experiment. The method of the experiment was to strike the forks in the order c', e', g', allowing a short interval between the tones. *O* sat with his back toward the forks. To introduce uncertainty into the experiment, the observer was sometimes told that the order of the tones would be varied. The time interval between tones, and the intensity of the tones were also varied.

In this experiment, G. always had auditory images of the coming tone, when he knew the order in which the tones would be given, and had sensations from the throat's setting for the next note. He also had visual images of the forks in their arrangement on the table. When he did not know the nature of the coming stimulus, his mind was chiefly filled with verbal ideas, referring to the nature of the coming stimulus.

In this particular experiment, most work was done with C., who made in all about 100 experiments. C. was more variable than was G. Her auditory image of the coming tone was sometimes clear and sometimes obscure. She nearly always had sensations from her throat, as it adjusted itself for the next note. In addition, she had strain sensations more or less general over the body as she waited for the coming note, and she frequently reported organic sensations connected with breathing. In general, after the first tone was given her, she had an auditory image of the coming tone; in subdued manner, she sang the tone herself; and she had strain and organic sensations due to tension of the muscles of the body and to a controlled breathing. This experience occurred when the nature of the coming stimulus was known. The following are typical introspections:

"After the first signal, had organic sensations from breathing, auditory image of coming tone, kinæsthetic sensations from the throat" (5 seconds). "Sensations from throat quite pronounced, organic sensations from breathing, strain sensations from the eyes." [C. often reported a sense of direction toward the forks, and this apparently came from strain sensations in the eyes: time, 5 sec.] "Kinæsthetic sensations from the lower part of the chest, sensations from breathing, auditory image of the coming tone, and strain sensations from the adjusting of the muscles of the throat" (5 seconds). "Kinæsthetic sensations from breathing, general strain sensations from the whole body, strains in the face and eyes and lips." This general strain in the whole body is often reported, and once, when the wait was 15 seconds, C. says: "Organic sensations from breathing almost painful (so long a wait), verbal ideas about its 'being a long time.' Sensations from the throat, and visual image of the forks in their arrangement on the table."

Knowing that he almost never has an auditory image, the writer was curious to know what would characterize his own consciousness while waiting for a tone. He secured an assistant to strike the forks, and acted as an observer in a number of experiments. Not once did he have an anticipatory auditory image of the coming tone. He practically always had organic sensations from suspended or controlled breathing, and usually strain sensations more or less general over the body, and particularly strain sensations in the eyes. He had an image of the forks, in their arrangement on the table, and when one fork was sounded his eyes turned to the next fork—as he saw it in imagery; this turning of the eyes gave slight strain sensations, doubtless the source of the sense of direction.

These experiments, done under laboratory conditions, are in agreement with his general observation of similar experiences for the last three years. Almost every day he has turned his attention to the analysis of an expected sound, usually the library clock striking the hour, or the whistle signalling the weather prediction for the coming day. At such times, the experience begins with the first stroke of the clock or the first blast of the whistle. The body is at once thrown into a characteristic attitude of waiting. This attitude gives rise to strain sensations, more or less general; nearly always there are organic sensations, and sensations from the eyes as they turn toward the source of the expected sound, or as they roll upward as if to cut out all actual visual sensation. If the time of waiting is rather long, there are verbal ideas, sometimes vague, sometimes quite clear. The verbal ideas seem to occur most often when the experience is one whose sequence is not entirely certain, or when the time of wait is several seconds in length. The following are typical reports of introspections in experiments with the forks. (Py.) "Verbal ideas before the first tone; I was not sure that I had heard the signal, and the verbal ideas were on this point. The first tone came, and then my eyes turned to the next fork; there were strain sensations from my eyes and body generally; on a lower conscious level were organic sensations.'' Once, when the coming tone was unknown, ''I had verbal ideas and organic sensations; these were in the focus of attention alternately. I said, 'now which will it be?' then the body tightened up and gave a general strain, and immediately there came organic sensations.'' Again, when the wait was long, ''I said, 'What next?' '' But the turning of the eyes and a general set of the body which followed gave sensations which formed the focus of consciousness. Again, ''almost the whole body took part in the wait. My eyes turned toward the forks; there were organic sensations from breathing; general strain sensations; and verbal ideas, such as 'now, there is that tone, the next one is coming'.''.

Expectation of Touch and Temperature Stimuli

Only a few experiments of this kind were performed. The results obtained were quite definite, and extended experiments seemed unnecessary. Several methods were used, with four observers. The methods were as follows. The observer sat by a table, with bared arm lying on the table, and eyes closed. For known stimuli, the experimenter would touch the palm of the observer's hand at definite intervals with a cold metal rod. Compass points were also used. First one point would be placed upon the palm of the hand, and then two points; this procedure would be repeated over and over. To introduce uncertainty, the interval was varied and, in the case of the points, the place and order of application. In temperature stimulation, a cold and a warm metal were used, and applied to different places on the observer's arm. In definite expectation, some observers had chiefly visual images and organic sensations, others had strain and organic sensations. When uncertainty was introduced, verbal ideas played a part.

To illustrate this form of expectation, the following quotations may be made from the introspective reports. Pi., when a cold metal was applied to the palm of the hand after constant intervals of waiting, reports, ''There was an intense strain before the first application of the stimulus; there was a tingling of the muscles of the palm, they seemed to come upward with a slight tension. After the stimuli began to be applied, there was a feeling of pleasure because of the coolness. [The time of experimentation was July.] The sensation of coolness persisted after the removal of the metal; during the wait between stimulations there was a strain sensation from the slight tension of the muscles of the palm; there was a desire to feel the coolness again." The observer also reported vague images of the metal which was applied to her hand, but there were no verbal ideas. When the same form of stimulus was applied, with variations of the time and place of application, the observer reports, "Vivid expectation, strains from the trembling palm, pressure image on the palm; there was a startling effect when the stimulns was applied to a different part of the arm; there were strains from different muscles of the body, which twitched. Coolness gave pleasure. When the experimenter's motion was heard, as he moved his arm to apply the stimulus, there were strain sensations from the swelling of the muscles of the arm. There was a vague shadowy image of the experimenter's arm holding the metal, but never any image of the metal touching the arm. Muscular (anticipatory) sensations were always most prominent on the spot last touched. When the arm was actually touched, the cold sensation filled consciousness." This is sufficient to show that tactual expectation, for this observer, consists essentially in strain and pressure sensations or images, organic sensations, and sometimes very vague visual images. In observer W. visual images were more prominent, and in the writer's case (Py.) verbal ideas were prominent, although he had the same strain and organic sensations that were reported by Pi. The experience of observer C. was about the same as that of observer W.

The Reaction Experiments

Many, perhaps most, of the things which we expect bring about some reaction on our own part. Doubtless, the preparation of the body for the coming stimulus and the consequent reaction have given rise to the peculiar strain and organic sensations prominent in the expectant state. With this belief in mind, we planned a series of experiments in which the observer should receive a signal and then, after an interval, be given a stimulus to which he was to respond as soon as possible by movement. After trying many different forms, we chose the reaction experiment as the most suitable for the purpose. We employed a Sanford pendulum chronoscope with three keys. One of these keys was used by the experimenter, and the other two by the observer. Both auditory and visual stimuli were employed, with four variations: (1) time of wait and stimulus known; (2) time of wait and stimulus both unknown; (3) time of wait known and stimulus unknown; (4) stimulus known and time of wait unknown.

For visual stimulation, small red and green discs were used. They were pasted on a cardboard strip, and concealed from the observer by another cardboard strip in front. When the experimenter pressed his key, the cardboard in front fell, and the disc was revealed to the observer.

The auditory stimuli were two noises. The one of these noises was produced by striking the reaction key with a piece of metal. The other noise was produced by having fastened to the experimenter's key a clapper which struck a bell when the key was pushed down. Choice reactions were introduced in all those cases in which the observer did not know beforehand the kind of stimulus that was coming; in these the right forefinger was to respond to the one and the left forefinger to the other stimulus.

The method in the four variations was simple. (1) The observer was told that he would be given a certain stimulus after a given time, *e. g.*, that after five seconds from the signal he would be shown the red stimulus, to which he was to react with his right hand. (2) The observer was told that, after an indefinite time, he would be shown either the red or the green stimulus, and that he was to react with the right hand to the red and with the left hand to the green. (3) The observer was told that after a given time, *e. g.*, five seconds, he would be shown either the red or the green stimulus, and was to react with the right hand to red and with the left hand to green. (4) The observer was told that, after an indefinite time, he would be shown a certain stimulus, *e. g.*, red, and that he was to react to this red stimulus with his right hand.

After the reaction, the observer reported his introspections to the experimenter, giving the contents of consciousness in terms of images, sense-perceptions and verbal ideas, together with the affective tone if any were present. The observer was instructed to trace the course of his attention during the experiment, that is, to describe the relative clearness-values of the constituent parts of his consciousness, and also to give, as well as he could, the temporal relations of the processes. Three observers took part in this experiment: G., C., and W. After finishing with these observers, the writer (Py.) acted as observer in a short series of experiments.

The following are summaries of the introspections.

Observer G. *Time and stimulus unknown.* G. got no images when the stimulus was visual, but when the stimulus was auditory he nearly always reported both visual and auditory images; it is to be remembered that, during the experiments with auditory stimuli, the eyes were closed. The auditory images were of the stimulus, and the visual images of the sound-producing instrument. He had images of his own hand with finger on the key, also of the bell, and of the experimenter's hand holding the metal striker. The image of the bell was associated to the image of the left hand on the key (the left hand was to respond to the bell stimulus). The image of the hand holding the metal striker was associated to the image of his own right hand with the finger on the key. There were practically always verbal ideas, relating to the nature of the stimulus, and also kinæsthetic and organic sensations. The verbal ideas were such as, "Right hand to red and left hand to green," or in the case of auditory stimulus, "Right hand to metal and left hand to bell." The kinæsthetic sensations were strains in the reacting fingers. When the verbal ideas were "Right hand to red", the strain would be in the right fingers and hand, perhaps extending up the arm. And when the verbal ideas were "Left hand to green," the strains would be in the left hand and arm. The organic sensations were in the chest, and were connected with breathing. There was no observable uniformity in the affective tone reported; it was sometimes pleasant, sometimes unpleasant. and often the observation was indifferent. If organic sensations were prominent, the experience was usually reported unpleasant.

Time known and stimulus unknown. The results were much the same as in the first type of experiments. There were no visual images when the stimulus was visual, and very seldom even when it was auditory. Organic sensations were always prominent, as were also verbal ideas, while kinæsthetic ideas were only a little less frequent and less clear.

Time unknown and stimulus known. There was very little imagery of any kind in this type of experiment, even when the stimulus was auditory. The sensory contents of consciousness were perception of the cardboard shield, verbal ideas and kinæsthetic and organic sensations when the stimulus was visual, while organic sensations and verbal ideas were the prominent contents when the stimulus was auditory. The stimulus as image played very little part in consciousness, which was chiefly kinæsthetic and organic. The stimulus was known, so that there was little concern about it. The body was set for the proper reaction, the breathing was under control, "consciousness was all body".

Time and stimulus known. In these experiments there were rarely visual images when the stimulus was visual, but there were both visual and auditory images when it was auditory. The visual images in the latter case were of the key, the apparatus, and the experimenter's hand ready to strike the key. The auditory images were *not anticipatory images*, but memory images of the signal key just after it sounded. That is to say, they were images not of the next constituent of the experience that was being repeated, but of the constituent that had just been experienced in perception. The chief constituents of consciousness were organic and kinæsthetic sensations. The experience was almost uniformly unpleasant if the organic sensations were prominent. This type of experiment was thus different from the others, except that it resembled somewhat the second type described above. There was on the part of the observer, a set, an attitude. After a certain known time, a known stimulus was to come, and a definite reaction was to follow. Therefore, the observer's consciousness was poor in content, except for the organic and kinæsthetic sensations of the set. The organic sensations were always present, and were connected with a controlled breathing. There were often verbal ideas relating to the task, which served evidently to keep the body tense and ready for the reaction. The observer was inclined to call his mind a blank during these experiments.

Observer C. *Time and stimulus unknown.* In the visual experiments, C. usually had visual images of the colors, and also images of the movements of the experimenter and images of the cardboard shield. The images accompanied such verbal ideas as "Right hand to red, left hand to green." There were strain sensations in the hands and fingers, and there were verbal ideas. The experience was usually indifferent.

Time known and stimulus unknown. No images were reported in the case of visual stimuli; in the case of auditory stimuli, they were reported in about 50 per cent. of the observations. These images were *not auditory images of the coming stimulus*, however, but were visual patterns of the apparatus, and lines joining the source of a stimulus with its corresponding reaction key. In the case of visual stimuli, the sensory contents of consciousness were verbal ideas and the perception of the cardboard. There were usually noticed strains in the hand whose reaction corresponded verbally with the stimulus. In the case of auditory stimuli, the chief constituents were kinæsthetic sensations. Occasionally these sensations were low in clearness-value, and verbal ideas were prominent.

Time unknown and stimulus known. Images occurred in one-third of the visual experiments and in two-thirds of the auditory experiments. The prevailing sensory contents of consciousness were strains in the reacting finger. There were occasionally organic sensations connected with breathing, and some verbal ideas. The experiences were uniformly indifferent.

Time and stimulus known. Images were reported. There were kinæsthetic sensations in the reacting finger, but no organic sensations were noticed. The kinæsthetic sensations coincided temporally with images of the hand and finger on the key. Except for the images, C. agrees with G. in this experiment; her consciousness was poor in contents. The experience was uniformly indifferent.

Observer W. With this observer only about half as many experiments were performed as with the other two observers. When stimulus and time were unknown, she had images in about one-third of the observations. The sensory contents of consciousness were cardboard shield, in the visual experiments, with verbal ideas, and kinæsthetic and organic sensations. The organic sensations were from the chest and were connected with breathing. The ideas related to the stimulus, and the strains, were in the appropriate hand and finger. There was usually no affective coloring. When the time was known and the stimulus was unknown, no images were reported. There were verbal ideas and strains in the fingers, all indifferent. When the stimulus was known and the time unknown, there were verbal ideas and strains in the fingers, but only occasional images. All were indifferent. When time and stimulus were both known, images were reported in about half of the observations. The sensory contents of consciousness, in the visual experiments, were perception of the cardboard shield, strains in the reacting finger, and verbal ideas. The experience was sometimes unpleasant, but was usually indifferent.

The writer need give only a brief account of the experiments in which he himself acted as observer. His report is in substantial agreement with the others. His experience in general was as follows. When the stimulus was visual, the cover-card and strains in the reacting finger and in the eyes were alternately focal. As time passed, the reacting finger rose in conciousness to a higher clearness-level, visually and in strain sensations, as well as in pressure from the key. Low in the background of consciousness were organic sensations. Verbal ideas were not prominent, except that they would often occur at the beginning of an experimental series. When the stimulus was auditory, the eyes were closed; then arose a vague visual image of the place whence the ready-signal came, the eyes were turned in that direction; after the signal was sounded, this image passed at once, and there appeared a visual image of the table at which the observer was sitting, and of the apparatus on it. There was also a vague visual image of the experimenter holding the striker, or with finger above the key. This would rise to the focus and then sink to a lower level, and then an image of the observer's own finger on the reacting key would rise, and with strains from the finger become focal. While *O* was in this stage of the experience, the reacting stimulus usually came; or, in the case of the variable times, attention would shift back and forth between these two sets of images and sensations, and the time of reaction would depend on what was focal when the stimulus came.

It is interesting to note that in the reaction experiments there was little difference in the introspective accounts whether the stimulus was known or unknown. This is in direct opposition to what should occur if the traditional view of expectation were correct. According to that view, expectation is a state of consciousness in which the subject is attending to an image of a coming impression. But we come back to this point later. We must now quote from the introspective records, in order to show the typical expectant consciousness of the reaction experiment.

To quote from some of the writer's introspective reports; when the time and stimulus were unknown, he gave such reports as: "Organic sensations most prominent and unvarying constituent. The breath was entirely under control, the breathing being very slow. I was ready at every instant to react; this in conscious terms was constant strain sensation. There were vague visual images of my hands, with finger on the key. The experimenter made a little noise which served to bring him visually into the background of consciousness, in image of course." There were sometimes verbal ideas, as in the following: "Verbal, 'this will be a long one', 'Now I am ready, I shall always be ready.' There was visual image of hand and pressure of the finger. The experimenter came vaguely into consciousness visually. There was strain in the eyes [the eyes were directed toward the hands]. Organic sensations, strains pretty general over the body, indifferent." And the following: "Verbal ideas 'now, when will it be?' Noise of the drum, visual image of the finger, pressure and strain sensations at the same time. Every time my finger rose to consciousness in visual image, there was a tightening up of the muscles of the finger, in consciousness strain sensations. The noise of the drum came in at times. There was vague visual image of the experimenter. The time was long and the above experience repeated itself."

In the experiments when all the conditions were known, he would report: "Heard signal, repeated it in throat in some way, noise of drum, then my attention went over to my own table and the whole thing and the apparatus on it came into visual imagery. Then came vague verbal ideas 'it is about time', then the chronoscope came into visual imagery, and my key and finger, then strain in the hand and finger, pressure of the finger, organic sensations from chest." At another time he said, "Caught a bit unaware. I started as usual, I moved over to the reacting experiment and had verbal ideas, 'it will be a little while yet.' I kept my finger tense, however, and had some strain sensations from it, but the stimulus came and found me in the midst of verbal ideas."

Observer C. made such reports as follow. Time and stimulus unknown, time sixty seconds; "After the signal I had feeling of direction toward the instrument; this was essentially sensations from the eyes, that is from eye movement; and the strains in the eyes lasted for some time; then there were strain sensations in the face. At intervals, I heard slight sounds made by the experimenter; there were kinæsthetic sensations in my fingers. It occurred to me in verbal terms that the experimenter was adjusting the bell, so I thought the stimulus would be the bell. In the background of consciousness were organic sensations from breathing."

Time variable and stimulus noise known: "I heard the signal; then my attention went to image of my reaction key, then to the experimenter's key; I then had image of the experimenter striking his key; I then had kinæsthetic sensations in my own reacting finger, and these lasted till the stimulus came and the reaction took place. The experience was rather pleasant." When all the conditions were known, she would give such reports as these: "Noise of signal key, verbal ideas of its long continued sound; then my attention went to image of my own key, but there were no other images. Indifferent." Again: "Noise of signal, sensations from my reacting finger (it trembled), visual image of finger; then came the reacting stimulus." And again: "Ideas of 'time being short,' strain sensations from the body, more or less general, due to the adjustment of the body, visual image of my key and then of the experimenter and then of my key again."

Observer G. Time (ten seconds) and stimulus known. "Kinæsthetic and organic sensations almost entirely. The noise of signal key set up a strong tendency to react, manifesting itself in strain sensations; these were strong and extended up through the trunk. There were no images." Again: "Mostly sensory, auditory-verbal ideas and pressure of the key on right finger; then as definite visual image as I ever had of the two pendulums starting; then came stimulus." And still again, he says: "Mostly an absolute blank; once there was pressure sensation, and at about the middle of the time, the verbal ideas 'now it will come pretty soon', then there may have been some vague strain sensations in the reacting hand." It is especially to be noted that this observer, although auditory in his general ideational type, had not once an auditory image of the coming noise-impression in the whole series of experiments with auditory stimuli where all the conditions were known. Nor were there any auditory images when the stimuli were varied and unknown to the observer. He reports in such cases: "Funny; definite expectation of the bell consisting in kinæsthetic correlation of the bell and left hand; strong and clear pressure of the left finger on the key strengthened by the verbal ideas, 'this time it will be the left finger.' Indifferent." Again: "Indefinite verbal expectation of either stimulus, 'this time I will not expect either, nor emphasize either'; verbal ideas concerning the time; breathing clear in the background; no images." When all the conditions were unknown, he reports: "The processes are becoming more and more evasive. The kinæsthetic sensations are stronger, being very strong this time in the chest; faint strain from holding self erect; had in verbal ideas expectation of the bell; the cutaneous sensations in the left finger were plainly clearer; the right hand came in verbally once but made no special impression." Again: "Definitely expected the right-hand stimulus. The right hand was emphasized verbally; in spite of this there were strong cutaneous sensations from the left finger. I purposely verbally emphasized the right finger to counterbalance this pressure in the left finger. There was only a vague visual image of the position of the hands." These observations are sufficient to show the general character of the experience.

Expectation of Words, Numbers and Geometrical Figures

When one listens to a person who is speaking, one often anticipates the words that are coming; and similarly, when one reads, there is often an anticipation of coming words, phrases and even sentences. The purpose of this series of experiments was to discover the form and nature of this anticipation. Is it the same thing as the expectation found in the other experiments?

The method used for these experiments was much the same as that employed for the other experiments with visual stimuli, described above. By means of the exposure apparatus, typewritten sentences were slowly moved into view. For this purpose the exposure slit was used instead of the exposure holes, which were closed during these experiments. The slit was on the left side of the large cardboard of the apparatus, and the sentences were moved in from the right. Different rates of movement were employed, and many different kinds of material were taken, such as quotations from standard literature, definitions of psychological and other scientific terms, sentences in German, arithmetical problems such as $6+9=20-5$, miscellaneous material, and also drawings and geometrical figures.

G. had, during these experiments, the characteristic expectant attitude of waiting. This attitude, in terms of body, was tension and set of muscles, not only of the eyes, but more or less of the whole body. The attitude was, however, very vaguely expressed in consciousness. In fact it was only by making all sorts of variations in the experiments that accounts of it are brought out in the introspective reports. These variations consisted in increasing or decreasing the rate of speed of the apparatus, or even of stopping it altogether. If the time of wait was very short, few if any of the strain and organic sensations common in the other experiments came to consciousness. However, the repetition of the same sentence, figure or drawing brought about the same sort of experience as in the simple visual experiments with the colors.

There was, further, what might be called a *mental attitude*. The personality of the experimenter served to put the observer into an attitude that was different from what it would otherwise have been. Of the nature of this mental attitude, we shall have something to say later.

When sentences were shown to G., he would, after the appearance of one or more words, verbally complete words and phrases; he would often see the rest of a word, and even more than one word, on the card behind the screen. In some cases, after the appearance of two or three words, G. would complete the sentence verbally, and often see whole words and phrases on the card behind the screen. This was the simplest method by which G. proceeded. From this method, of plainly seeing and plainly saying the words, there were all degrees of clearness down to vague and unanalyzable processes, apparent short-cuts, appearances of meaning in consciousness in other than verbal terms. Often G. was entirely unable to analyze his experience, and instead gave his report in the most vague and general terms. It was the same in the case of all the other kinds of material used. When an arithmetical statement in type-written form started across the field, G. would *say* and in some cases *see* the rest of it. When a drawing was gradually exposed, G. would verbally comment on what he thought it would be, and in some cases visually complete the drawing in image.

The following quotations illustrate G.'s procedure.

Sentence given: Art is long and time is fleeting. G. reports: "After 'Art is long', had notion that the sense was not very elevating. Can not analyze this notion. When 'and' came, I said verbally, 'life is short', projected 'is' on the cardboard. 'Is must follow', I thought verbally. The tendency to say 'is' was strong. When 'f' came, I said 'fleeting'."

Given: $2+3+5=5+5$. G. verbally repeated the first three figures as they were exposed, expected verbally the sign of equality, and after the first '5' appeared, plainly saw in imagery "$\times 2$".

Given: $6+9=20-5$. G. reports: "I visualized the + sign before it was in sight and said verbally 'equals 15', after the first two numbers appeared. After the 2 of 20 appeared, I said 'equals two plus what?'. When the 20 appeared, I was dead sure visually and verbally that it would be '—5'."

Given: two similar triangles, the one following the other on the paper. G. reports: "I saw, in image, the second triangle completed behind the screen. I was very uncomfortable when the apparatus stopped moving. [The apparatus was stopped in the midst of the experiment.] The stopping of the pendulum entirely changed my waiting attitude; as soon as the pendulum started moving again, back came the old attitude."

Given: Let us then be up and doing. In the experiment preceding, the experimenter had allowed two balls to fall and make a noise. In this experiment G. reports: "Had auditory images of balls falling. Said verbally, 'they must be coming pretty soon [referring to the falling of the balls]. Now, why don't they come?' Then I saw on the cardboard 'Let us' and said 'be happy', but if I really expected it, it was vague and not at all like expecting the balls. There was no idea like 'that will be the next thing on the screen'. I just verbally filled out the sentence from the hint given."

The above difference noticed by the observer is, it seems to us, very important. The observer speaks of the fact that verbal ideas of expectation were absent, and organic and strain sensations are also either weak or lacking. Organic and kinæsthetic sensations and verbal ideas play little part in this experience. And it was the lack of these constituents that made all the observers hesitate to say that they expected such and such a word or phrase or drawing to appear. In the experiments above described, one or two words would appear, and then the observer would, doubtless more or less mechanically, say certain other words, or perhaps visual images might appear. But there seems to have been no expectation that the word said or imaged would appear, in the same sense that the impressions were expected in the other experiments. In some of the cases with the sentences and similar material, there were present those elements that characterized expectation in the other experiments. For example, if the same sentence was repeated several times over, the experience of the observer was about the same as in the case when colors were shown. The meaning of the thing disappeared, and it was expected in the same sense as were the colored discs.

In anticipating words, then, under the conditions of these experiments, the experience is essentially like that of memory. Suppose, for example, that I see or hear the word "mother". There may rise immediately an image of my mother, and perhaps verbal ideas concerning her; but I should not say that I expected immediately to see her. In such cases there is association without expectation. There is first a sensation or perception, and then at once follows an image or idea of some sort; but this association alone is not expectation; there must be other elements present. These elements, as is already evident, are strain and organic sensations. Suppose that I had a letter saying that my mother is coming to visit me, and that she is likely to come at any moment. I should at once be put into the state of expectation; my body would be thrown into an attitude of strain, and in consciousness the kinæsthetic sensations would be so strong that there would either be no visual image or at any rate the image would not be clear as it is in the case of association.

However, there are, no doubt, all possible degrees of expectancy between this direct association and the cases in which the sensory constitutents above mentioned are quite clear. In the experiments with sentences, which we are here describing, there are cases in which there is no expectant element, and other cases in which that element is present. Especially is this true in the case of observer C. Thus C was given the sentence: Man is mortal, God is an immortal spirit. C. says that she had "an image of the general appearance of a type-written sentence. Recalled seeing a German sentence on the experimenter's desk, so when 'man' appeared, I gave the German pronunciation for it. I was surprised when 'is' came, for it did not go with the German. Later, when the word 'God' appeared, I said 'immortal'. When I saw 'an', I had a thwarted feeling and simply waited." It seems that in this experience there was the expectant element, although it was not at all prominent. The experience is analogous to what we have been calling expectation, but is not identical with it. The observer has just seen a slip containing a German sentence. She may or may not have had the verbal ideas "Now I shall be given a German sentence." The word 'man' appears; the observer has been put into the 'German attitude', which means physiologically that certain brain centres or tracts have been called into function; and when the word 'man' appears, the German pronunciation is given without conscious intention, automatically. When *is*, instead of *ist* or some other German word, followed 'man', the observer was 'surprised'. The physical organism was evidently in a different condition from that in which it would have been had the observer not seen the German sentence on the table. But unless this physical condition makes itself manifest in consciousness, unless the muscles are not only *set* but their set *given in consciousness* as strain or organic sensation, it seems best not to call this experience expectation. If expectation is to be the name given to some well-defined pattern of consciousness, then there must be something *in consciousness* to characterize it, something characteristic of the pattern. And as it turned out in this particular case, the physical set or condition of the body served only to bring about one particular association instead of another.

Another somewhat similar experience from the same observer is the following. The sentence given was: Shakespeare was a man and a dramatist. C. thought the first word was 'Shakespeare's' and says: "Expecting something to come that would make sense, said 'plays, Shakespeare's plays'. When 'was' came, I read it right, and after the word 'man' appeared I supplied visually and verbally 'and a dramatist' ."

At another time the experimenter had been talking about Longfellow, and quoting the lines beginning "This is the forest primeval". The words actually given to the observer, were: Virgil, Milton, Byron,—all great poets. C. says: "I could hardly get away from Longfellow. I had image of the 'forest primeval'. After 'Virgil,' there was no expectation. After 'Milton,' I thought it would be poets, and supplied verbally 'Shakespeare'. With the word 'Byron', I had the feeling of unfitness, but it is hard to describe this feeling."

After the above, the observer was given the sentence: It is no use to be dogmatically inclined in psychology. C. reports: "At the beginning, I had image of typewritten line, although it was not definite. I said verbally, 'may be it is a line of poetry', and had visual image of the word 'Milton', but I was not surprised when it did not come. After the words 'It is no use' appeared, I said 'to try', and when 'be dog' appeared, I said 'dogmatic' and supplied, after 'in' the word 'philosophy'. I was only slightly surprised when 'psychology' came instead of 'philosophy' ."

Combination Experiments

Finally we performed a number of experiments, which, for the want of a better name, may be called 'combination experiments.' They consisted in the performance, before the observer, of a series of continuous and connected acts. The observer was told to sit quietly, and to say nothing till the end, and then to give as good a report of the proceedings as possible. The results of these experiments were essentially alike, so that only a few of them need be quoted. One was as follows: *E* took cocoa, red pepper and sugar, laid them out on separate papers as a physician does when preparing medicines, mixed the three materials, and put them into a glass of water. C. gives the following report. "At first, when the materials were uncovered, I had verbal and visual ideas of the room from which I thought they had come. When the experimenter took out his knife and cut the paper into squares, I had the verbal idea 'powder', and visual image of our old family physician we had when I was a child. While the experimenter was still cutting the paper I had visual image of him putting the powders on the paper and folding it up as a physician does, and had verbal ideas, 'Dr. Pyle,' with tendency to laugh. I really expected the papers to be folded up. At times the perceptions of what the experimenter was doing became low in consciousness and the image of the old family doctor was the clearest thing in consciousness. When the experimenter took the glass of water and put the mixtures into it, I merely watched and wondered if he would try to make me drink it. I said the words 'I wonder if he will try to make me drink it'. I knew he could not make me drink it."

Another experiment performed with C. several times was the following. A tin coffee-pot with narrow top was placed on the floor and above it was tied an iron weight. The weight was tied by a cotton string. A piece of paper several inches long was tied to the string. The paper was lighted by placing a lighted match at the end. C. says that she "had peculiar strain sensations in every muscle of the body, as I always have in important situations. I imagined the weight would sound louder than it did when it struck the metal below. I had kinæsthetic image of jumping and eyes winking. I had sensations all over the body as I do in apprehension. I do not remember any organic sensation. The strain sensations seem to push everything else out of consciousness. I should call the experience that of weariness all over the body. I thought the paper would burn longer than it did before it burned the string. I had my eye fixed on a point to which I thought it would burn before it would fall, but the weight fell before the blaze had reached that point.''

Two experiments of a similar nature performed with G. were as follows. G. was placed in a chair and told to keep his eyes closed. *E* went out and brought in a large knife and a long cylindrical piece of metal. He rubbed the knife on the metal as if sharpening it, and then slowly drew the back of the knife across the observer's neck. He reports as follows: ''I followed the experimenter in visual imagination. When the whetting was going on I followed the scene and waited. I tried to interpret verbally what the experimenter was doing. I would say, for example, 'now he is sharpening something'. There was no thought of what was coming. The mere putting me in a chair, and telling me to wait, puts me into the general attitude of waiting and ready to take whatever comes, a general attitude with no particular image of what is going to come.'' When this experiment was performed the second time, G. says that he ''had organic reverberations of the metal touching my neck before it really touched it.'' In this latter case, there was expectation. In another experiment with G., *E* deliberately folded and burnt a piece of paper, holding it in his hands till it had burned up. G. reports: ''When the experimenter began to fold the paper, I thought he was going to make a small roll for lighting purposes. When the roll was made, I visualized the experimenter taking a match and lighting the paper roll. When the experimenter took his knife and opened it, I said, 'what is all that for? He is trying to scare me.' Then I saw in image the burning of the paper before it was lighted.''

It is unnecessary to give more quotations from the results of these experiments. An examination of them shows that there may or may not be expectation in the case of continued performances like the examples mentioned above. The perception of one part of the performance may arouse an association, an image of some other act, but not in all cases is this other act expected. To constitute it expectation, there must be in consciousness something that represents the holding of the body ready for the future impression that a coming act will produce, that is, there must be organic or strain sensations or verbal ideas, either or all of these. Any one of these constituents can serve to keep the organism in readiness for the coming impression, and any one of them in proper temporal and intensive setting gives to consciousness a characteristic pattern.

This pattern, this peculiar state, should be called *expectation*. But before giving a final description and explanation of the state, it will be well to look back over the results of the various experiments, to see what common elements they show, and to determine what conclusions they warrant.

Interpretation and Criticism

We have now described our experiments and given a summary of their results. This has been done very briefly. The observers recorded observations sufficient in quantity to fill a volume. But examples have been taken in sufficient numbers to give the reader a fair idea of the whole. We now have to answer the question, What comes out of the results? Does an examination of the observers' reports of experience under all the different conditions reveal any definite pattern of consciousness? And if such a definite pattern is found, does it resemble the experience which people have been accustomed to call expectation? There is no doubt that a definite, unvarying type of consciousness is found from an examination of the results, and this type is always found when we duplicate experimentally those experiences which people agree in calling expectation. There are, however, other experiences much like this type which show no common characteristics. Many of these are also commonly called expectation. Putting it in another way, we may say that if all the types of experience which people are accustomed to call expectation are examined, they fall into two classes. The one type is definite, with constant characteristics arranged in a more or less definite pattern. This type, then, best represents what everybody is accustomed to speak of as expectation. The other class has no definite pattern, no definite characteristic, nothing to distinguish it from many other forms of experience. It is merely a mode of association. These two classes of experience begin in the same way; they begin with a perception. In the second class above mentioned, the perception is followed temporally by one of two different conditions. Either a memory image comes up, forming the focal constituent in consciousness, with no other elements, or at least no prominent organic or kinæsthetic sensations. Or the perception results in the body assuming automatically a definite attitude, without there being any conscious concomitant of this attitude. In some cases, it is hardly proper to speak of this condition as an attitude; the perception brings about a definite response automatically and immediately. In reading, for example, one may *say* a word after seeing a certain other word. The perception of the one word brings about immediately the speaking of the other word, or perhaps the seeing of the other word in image, when one would not care to say that one *expected* the other word. That is to say, the perception of the first word is followed immediately by the speaking of the second word in an entirely automatic way without any intervening consciousness. Many such experiences are called expectation, but there is nothing whatever in consciousness to distinguish them from experiences that no one would think of calling expectation. We may dismiss these experiences, then, with the statement that they have nothing to characterize them as expectation. Unless an experience has definite attributes, that enables us to set it off from other experiences, it should not be designated as belonging to any special type of consciousness. We shall reserve the term Expectation for a particular type of consciousness, a type that can be described and defined with sufficient accuracy to enable us to distinguish it from all other types or patterns of consciousness. Any classification, of course, must admit some doubtful examples, but the examples mentioned above are certainly not of this doubtful kind.

The nature of this specific type of consciousness, for which we are reserving the term expectation, must already have become clear to the reader. We have several times given at least a partial description of it. We shall now try to define it more specifically and to state the conditions under which it arises. The experimental results as well as general observations show that there are two situations in which the state arises. In the first of these situations, the experience begins with a perception; the subject, after the perception, awaits another perception that in the past has followed the first. This awaiting of the second perception constitutes the expectant state. In consciousness, this state is characterized by strain and organic sensations and verbal ideas. The sensations are ordinarily the main constituents, but the verbal ideas may also be present as a characteristic element, may even be present to the exclusion of the kinæsthetic and organic sensations. Especially when the time of wait is long do the verbal ideas appear and proponderate. Functionally, they serve the same purpose as the initial perception; they serve as stimulus to keep the organism tense and ready for the coming impression, and perhaps for the appropriate response. The verbal ideas take the focus otherwise occupied by the sensations mentioned. The latter often drop only to the background, to a lower level of clearness, where careful introspection usually finds them; or they may drop entirely below the limen of consciousness. In this case, the set of the organism has no definite conscious concomitant, but nevertheless the organism is ready for the coming impression and necessary response. As was pointed out in the discussion of the experiments with sentences, the organism may be given a 'set' at the beginning of an experiment, a set that serves to bring up a certain group of associations and accordingly a definite expectant response, while this 'set' itself has no conscious concomitant or, at least, lies very low in the background. If the time of wait is short, such cases as these are likely to sink to the level of mere reflexes; not only are the kinæsthetic and organic sensations wanting, but the verbal ideas as well; there is no consciousness intervening between the perception and the response of the organism; such a consciousness is not of the type which we call expectant. In these cases, the 'set' of the organism corresponds to inherited nervous conditions that serve to bring about definite responses upon the appearance of definite stimuli.

An image of the coming impression may or may not be present. It is not essential, and usually it is not present. The focus of consciousness is occupied by sensational elements, and images sink to a low level of clearness-value or are altogether wanting. Some forms of expectation are more favorable than others to the appearance of the image. In the reaction experiment, the image of the coming impression is an unimportant factor, because of the sensational elements. But when one sits waiting for a tone, and no action is to follow the tone, then an image is likely to arise, especially from observers whose ideational type is chiefly auditory; but even with such observers, the strain sensations from the throat may be as prominent as the auditory image, and with the great majority of observers would be the main constituent of consciousness.

If we wished to speculate, we could easily find a biological explanation of this type of the expectant state. There comes a perception which is the first of a series formerly given in our experience. The organism at once assumes an attitude favorable for the reception of the next member or members of the series of impressions. Movements of the body are more or less arrested, the breathing stops or proceeds under control, the eyes are directed toward the source of the expected object of vision, or are averted in order that light sensations may not interfere with perception by means of other sense organs, and in either case add their mite to the general sense of strain coming from the muscles of the whole body. The type of expectation which arises under the conditions of this situation may properly be called Definite Expectation, in distinction from Indefinite Expectation which arises under the conditions of the second situation.

In the second situation, there comes into consciousness a perception which has not been wrought by experience into a definite series. In common parlance one would say, in such cases, that one expected something but did not know what.

Experiment shows the same inhibition of breathing and of other muscular activity, the adjustment of sense organs, as in definite expectation; and the constituents of consciousness are, as in the other case, chiefly strain and organic sensations. For example, in the indefinite type of reaction experiment, the stimulus may not have been known and the action that was to take place may not have been known, but the organism was in the same tense state, ready to receive *some* impression and ready to make *some* response. The conscious constituents were essentially the same as in the conditions in which the character of the impression and response was known.

The ordinary use of the term expectation, as well as our experimental analysis of consciousness, warrant the extension of the term to include both of these cases. In fact, the structural analysis of the conscious types gives, in strictness, no warrant for making even the distinction that we have made. The type of consciousness is the same, whatever the situation. There is no psychological reason for making two classes of expectation; there is only the difference in the two situations, and to this difference we attach little importance. There is, however, another reason why we have mentioned this difference in situations which give rise to the state under discussion. It is evident that psychologists have not taken properly into account what we have called indefinite expectation; for, if they had, they would never have emphasized the importance of the image of the coming impression. In indefinite expectation, one cannot know what the image of the coming impression is, since one is ignorant of the nature of the coming impression. But our experiments show that the structural type of consciousness is essentially the same, whether the conditions of expectation are definite or indefinite. Therefore the image, as a distinctive characteristic of expectation, falls to the ground. The only structural difference is in the localization of strain sensations, in cases in which action follows the impression that comes after the initial perception. In definite expectation, the strain may be from the reacting finger, whereas in indefinite expectation the strain is more or less general. There is in both cases a general strain but, in the definite type of expectation, there may arise very definite sensations from the particular muscles that are tense and ready to contract when set off by the coming definite impression. This coming definite impression need not, however, be in consciousness in the form of an image. It may sometimes be there; but our experiments certainly show that it need not be there. The definiteness consists not in conscious clearness but in the definiteness of the preparation for the impression. It will serve to set off only the particular reaction. It is not the image of the coming impression but the initial perception that throws the body into the attitude, which gives rise to the characteristic consciousness which we call expectation. In very few of the experiments with any of the observers was the image a prominent factor. Even when images were present, they were seldom images of the coming impression.

In ordinary memory, that is, in cases in which a perception is followed by an image, organic and strain sensations may also be present; but such a state is quite different from the expectant state. In the case of memory it is the image, not the perception, that introduces the organic and strain sensations. The name of a certain man, for example, may call up his image and an occasion when he insulted me, and I may then have organic and strain sensations; but the pattern of this latter state is entirely different from that of the expectant state. This state runs: perception, image, organic and kinæsthetic sensations. And not only do the image, and the ideas clustering around it, form the nucleus of this state, but it is largely a different set of organic and kinæsthetic sensations that surround the nucleus. They are not from the muscles adjusting sense-organs and from a controlled breathing, but from the actual and perhaps violent movement of limbs, and from a violent disturbance of breathing and other organic processes; whereas in expectation, as has already been pointed out, the sensations are from inhibited movements and from partially controlled organic processes. And apart from this difference in contents there is the important difference of arrangement in the pattern of consciousness.

The fact that all the observers often reported sensations from breathing suggested the idea of taking breathing curves from them. We accordingly did so, from nearly all the observers. Without any exception, the curves show that in the expectant state voluntary breathing is arrested and that breathing proceeds under control; it becomes shallow and more or less irregular. We also took a large number of curves from Mr. C. R. Hugins, an undergraduate student who had not been an observer in the experiments and who did not know that his breathing curve was being taken. The curves were taken while the observer was performing the reaction experiments in all their variations. In all cases, these curves show a marked inhibition of breathing. We have, then, at least partial proof of the correctness of our distinction between expectation and memory. There is, surely, no doubt that in memory the image is an important factor and gives rise to the organic and kinæsthetic sensations when they are present, while in expectation the image is not an important or necessary factor and the kinæsthetic and organic sensations are occasioned by the perception. There may still be cases in which a memory-form would approach expectation, and others in which expectation would much resemble pure memory. But any classification must take account of such intermediate forms.

While there is a type of consciousness which is properly called expectation, the word has another and more general use about which it may be well to inquire. We refer to such uses of the word as are illustrated in the statements: I expect to die; I expect to marry; I expect to graduate; I expect hard times, etc. When one makes such statements, does one have a specific type of consciousness? The answer to this question does not much concern us in this paper, and we have taken no account of it in the experiments. Analysis of such experiences in the writer's own case convince him, however, that whether or not they form a distinct type of consciousness, they certainly do not belong to the same class with such experiences as he has been experimentally studying. Any one can satisfy himself, by a continued introspection, that when one has such thoughts one has not the definite, simple type of consciousness which our experiments show. The thoughts do not begin necessarily with some definite perception, nor is the state essentially made up of organic and kinæsthetic sensations. For the writer, the contents of such states are almost entirely verbal ideas and visual imagery. We have seen, it is true, that the verbal idea may also be present in expectation; but its structural relation is different in the two cases. In real expectation, the verbal idea, as we have shown above, has a close relation to kinæsthetic sensations that come from the adjusting of sense organs, and often from other muscular adjustment. In the case of such thoughts as we have just mentioned consciousness is more like pure memory. The image plays an important part, and the kinæsthetic and organic sensations that may arise follow the image and are merely a reverberation of past experience. Functionally they serve no present purpose; they are, it appears, essentially emotional in nature; whereas in real expectation they have functional purpose, and are emotionally unimportant. These thought experiences, of which we have been giving examples, are probably much more closely allied to Belief than they are to such experiences as we have been studying. They stand structurally on a much higher plane than does expectation.

What, now, are we to say about such treatments of expectation as those described in the first part of this paper? In the main, we found the authors there mentioned defining expectation as preparatory attention. This definition is not only inadequate, but not in agreement with the facts as we have found them.

In the first place, expectation is not an attentive as distinguished from an inattentive consciousness. If the absolute clearness value of the focal idea is to serve as criterion of degree of attention, then expectation is rather an inattentive than an attentive consciousness; for the focal ideas are so low in clearness value that they can hardly be remembered long enough to be described by the observer. If we are to consider not the absolute but the relative clearness of the focal constituents, there is still no reason for calling expectation a form of attention; for there is very little difference in the clearness of the constituents in an expectant consciousness. One might, with some exaggeration, say that an expectant consciousness *has no focus* but *is all background.* An expectant consciousness is like a picture whose central figure has been left out. Often in the experimental work, especially in the early part of it, the observers would report that their minds were "like a conscious blank." Later, more practised introspection revealed a mass of organic and kinæsthetic sensations low in clearness value.

Especially those views which consider expectation to be attention to the image of the coming impression are not in accordance with the facts revealed by experiment. For, if there is any constituent of expectation that is usually inconspicuous and unclear, that constituent is the image or idea of the coming impression. Whatever one's view as to the nature of attention, one can hardly hold it possible to attend to that which is not at the time in consciousness.

There is a sufficient functional reason for the fact that an expectant consciousness is, so to say, all background, not only with no definite image, but also with no very clear sensational elements of any kind in the focus. For, from the standpoint of function, expectation is a preparation for a coming impression and for the reaction of the organism to the impression. Now, upon the appearance of some initial perception the organism 'sets' for the reception of the impression signalized by the perception and for the reaction to that impression, and very clear or intense processes or strong feelings would delay the assimilation of the impression and the co-ordinated reaction. Here, then, is an *a priori* argument which adds its weight to the facts as found by experiment.

There is, however, another reason that might be advanced for calling expectation a form of attention, a reason that has doubtless had an influence in the past. This is the fact that the bodily attitude is the same in expectation and in active attention. There is, in both cases, the same tenseness of the muscles of the body in general and of those connected with the sense organs in particular. But the resemblance is only external; the conscious patterns are different. In active attention, perceptions (or ideas which functionally take their place) form the focus of consciousness, and the kinæsthetic and organic sensations are secondary. In expectation, the perception or idea is absent, and there is only a mass of kinæsthetic and organic sensations.

In active attention, the mind is all *now;* it has no future reference. In the focus are perceptions and ideas now in process. In expectation, a perception[1] realizes an *Aufgabe.*[2] The organism is thrown into a 'set,' an attiude, whose conscious parallel is to be found in the organic and kinæsthetic sensations. The 'set' gives the consciousness of the moment its future reference. There is nothing in attention as such to give it this reference; and when the reference is introduced, attention therewith changes to expectation; consciousness loses its focus, which constituted the essential characterstic of attention, and becomes all background, the processes aroused by the *Aufgabe.* On the other hand, when the *Aufgabe* sinks below the conscious level, consciousness loses its expectant character. Illustrations of this fact were found above in the experiments with sentences. Such a degeneration of the *Aufgabe* results from long association and habituation.

Whether, then, we look at the question from the structural or from the functional standpoint, it does not seem adequate or in accordance with the facts to define expectation in terms of attention.

Apart from the above criticism, however, Külpe gives us a description that is not very wide of the facts, so far as the constituents of the expectant consciousness are concerned, when he says that the specific characteristics of the expectant consciousness are certain complexes of organic sensation. If one gives up the attentional reference, one may very well consider expectation, from the purely functional point of view, as a preparation for a coming impression, for that is certainly the function performed for the organism. But the question of pattern or type of consciousness, as such, has nothing to do with the question of function, although this latter question

[1]We have not raised the question whether a central process, an idea, may initiate expectation. There is no *a priori* reason against such initiation, but we doubt whether an idea really does, as a matter of fact, often serve to initiate an expectant consciousness. The author has completely failed to find, in his own case, an example of such central initiation.

[2]For this specific use of the word *Aufgabe*, the reader may be referred to H. J. Watt, Exper. Beiträge zu einer Theorie des Denkens, *Arch. f. d. ges. Psych.*, iv, 1905, 289 ff.

is important in itself and we have given it consideration above.

Sully, it will be remembered, says that a succession of impressions must previously have taken place, in order that one member of the series may call up the idea of the next, and cause the next to be expected. Yes, if expectation means attention to the *idea* of the next; but if we take account only of the structural type, indefinite expectation is equally expectation, and we may expect without expecting anything in particular. Although there may be logical objections to such a condition, there is no psychological objection. Besides, this 'idea' is in fact not important, either structurally or functionally.

Our contention that the idea is not an essential part of expectation is in direct opposition to the view of Wundt and Lipps that expectation is a state in which an idea is striving toward apperception. Since we do not find the idea to be an essential constituent of the expectant consciousness, we need not stop to discuss the question what such an idea would be like, as distinguished from other ideas not so striving. In reality, the activity-aspect of the expectant consciousness should not be over-emphasized. The state, while one of strain, is rather a strain that comes from inhibition than from activity.

There is no evidence whatever for classing expectation as an emotion, as Dewey and others have done.

It will now perhaps be well to consider Hitchcock's monograph somewhat at length. She says[1] that the purpose of her study is "to investigate the nature of the process [of expectation], the part it takes in the development of conscious life, its true relation to other mental activities and its consequent value in helping to determine our knowledge of the world and of ourselves." All these problems have been approached, however, without the performance of a single experiment. It will be seen that the author makes the assumption that expectation is a process. Her provisional definition is: "Expectation may be spoken of as a mental process or attitude in which certain ideas or images are regarded as substitutes for definite sensational contents which are to be experienced later" (9). Now a process and an attitude are different things; and if expectation is the one, it is either more or less than the other. And is it not a good deal to assume *a priori* that ideas or images can be regarded as substitutes for definite sensational contents which are to be experienced later? In what sense can images be regarded as 'substitutes' for sensations?

---

[1] *Psych. Rev. Mon. Sup.* v, No. 3, 1903, 3.

The organism certainly does not react to images as it does to sensations.

Two ways are pointed out in which expectation varies: in vividness, and in definiteness. "It may be intense and vivid, accompanied by strong emotion making the whole organism tense and alive with nervous activity; or it may arouse no feeling, remaining in consciousness only as a passive and almost ignored factor. Again, expectation may vary in definiteness from the clearest, most distint ideas to vague, formless premonitions of something other than the present experience, a bare consciousness that there is a beyond." This passage evidently means that expectation is a process, a factor among the factors that make up consciousness. It must then, in the mind of our author, be a process of definite quality which can vary in vividness, intensity and clearness. She proceeds to distinguish expectation from memory and constructive imagination. All alike are processes, and at the same time *references!* Memory "is usually defined as the reference of present ideas or images to past presentations within one's individual experience; so expectation may be defined as the reference of present ideas or images to future presentations within one's individual experience. We see that both alike deal with ideas or images. In the case of memory these ideas represent the sensations which, with their escort of images, at one time made up the significance of some immediate presentation or passing event. In the case of expectation the ideas present some sensational effect which shall at a later period become a present experience. We are awaiting, looking forward to a juxtaposition of circumstances that shall afford the stimulus for arousing the sensation for which we at present substitute the image. The same image may then serve in the case of memory to recall an experience and in the case of expectation to foreshadow it. If this be true, how is it that we do not confuse the two states? Whether we are to refer the image to the past or to the future is largely determined in each case by the supplementary contents of the total psychosis" (9). But this discussion of the reference of the image to the future or to the past lapses when we learn that the image is not the important thing in expectation and indeed is usually absent.

Continuing her analysis (16), the author calls expectation an ideational process in which certain constituents are always found to be present. These constituents are "central idea, ideational and sensational factors with their attending feelings, and the definite time relation to the presentative element." This we understand to be her definition from the standpoint of structure, and we interpret it to mean that expectation is a name to be given to a central idea that has as supplements ideational and sensational factors, feelings, and a relational element. This relational element relates the central idea to the presentative element *not yet in consciousness*. The sensational element is made up of kinæsthetic and organic sensations. On the latter point the author is correct, except that these sensational elements usually constitute the whole of the expectant consciousness.

At the beginning of chap. iii, we find a new definition. The author has already called expectation a process, a reference, and an attitude. Here we find that expectation is "the incipient response of the organism to the demands of some new situation." This, of course, cannot be a psychological definition; it is rather biological. But in psychological terms it would mean that expectation is kinæsthetic sensation. The author's 'supplementary' part becomes primary. From this biological point of view, she says that expectation begins in motor relations to a subjective stimulus which is in turn aroused by some perceptual form of consciousness. She explains that: "the central idea is identical with the subjective stimulus. The essential concomitants are supplied by the motor reactions, while the present situation, embodied in some perception, affords the external excitation of the process as a whole." The central image, she says, is always sensory, follows immediately after the sensation in immediate expectation, and introduces a perception. The ideational concomitants are of two kinds, sensory and motor. "The predominance of lively motor images, passing over into sensations, constitutes a large part of the difference between expectation and memory." We have already pointed out that this difference does not hold, that expectation is not essentially an active consciousness, opposed to memory as passive. Memory may have even lively kinæsthetic and organic sensations, whereas in expectation the strain sensation is from inhibition of movement rather than from movement; expectation is not essentially an 'active' consciousness. The difference is not so much that the one is passive and the other active; it is rather a difference in the kind of organic and kinæsthetic sensations, and their relation to the rest of consciousness. What these are, we need not repeat.

In order to get a better understanding of the author's idea, let us examine one of her illustrations. She says: "Suppose I see a flash of lightning, I expect to hear the peal of thunder immediately afterward, The image of the sound arises at once and forms the centre of the anticipation." Now the fact is that in the case of many—perhaps most—people there is no image of the thunder at all. In the writer's case, for example, there would be no auditory image of thunder. If the time intervening between the flash and thunder were long enough, we should have kinæsthetic and organic sensations and perhaps verbal ideas.

Chap. iv contains a discussion of the relational factor. Such a chapter could hardly have been written by one who had done experimental work in this field. We find, in experiment, no evidence whatever for a 'relational element.' The formal discussion of *abcD* and *Dcef*, etc., is entirely speculative, and has slight relation to what one actually finds by an experimental analysis of the expectant state. The author speaks of the image as "thrown forward" in expectation, and of its being compared with the percept when the percept comes. Our experimental results show that nothing of the kind occurs.

We need not further notice Hitchcock's work. Her discussion of affective tone has little meaning, in the light of our present knowledge of feeling, and the same thing may be said concerning her discussion of the relation of expectation to other processes, and particularly of its relation to attention. Painstaking and comprehensive as her treatment is, it fails of its purpose by reason of the lack of any experimental groundwork. The last part of the monograph treats of functional and logical aspects of the subject, and has less psychological interest.

To summarize, Hitchcock's idea is, briefly stated, that there is in expectation, first, a perception; this is followed by an image of an impression that had been, in the past, associated to the perception. The image has, as attendant elements, kinæsthetic and organic sensations, a relational element, and a feeling. The image gets its future reference from lively motor images and from the shifting of attention between perception and image. Our chief criticism of this analysis is that the image is not an essential element of the expectant consciousness, that consequently there is no play of attention between image and perception, and that there is no relational element to be found.

We add a few words regarding some recent experimental work. Watt,[1] in his *Experimentelle Beiträge zu einer Theorie des Denkens*, gives an analysis of the *Aufgabe*, the first period of the reaction experiment, corresponding roughly to what we have called expectation. Watt, however, speaks of expectation as if it were only a constituent of the *Aufgabe*, accompanied by strain sensations. And when the *Aufgabe* has degenerated, on account of repetition of the experiment, he describes it as consisting of bodily accommodation *and* a weak expectation. Our experiments seem to show that expectation is the conscious aspect of the bodily accommodation, not something in addition to it.

[1] Archiv für die gesamte Psychologie, 1905, iv, 298 ff.

The introspections of Ach's[1] three observers for the fore-period of the reaction experiment correspond very closely with our own results; his observers, however, speak of expectation as if it were something in addition to strain and organic sensations and verbal ideas.

Messer's[2] observers report for the waiting period (*Vorbereitung*) of the reaction experiment chiefly verbal ideas and strain sensations. In addition, some of his observers report that they had a *weak expectation; an expectation that so and so would happen; expectation of the stimulus word; strained expectation; very strong expectation of what would come*, etc. The form or nature of this expectation is not stated. There is no analysis of it by any observer. With this exception, the reports of Messer's observers are in substantial agreement with our own.

In conclusion, let us bring together the results of our study. The experiments show that expectation is initiated by a perception, and that the perception is followed by kinæsthetic and organic sensations, and in some cases by verbal ideas. These sensational elements, following the initial perception (or possibly an initial idea), are the conscious aspect of an *Aufgabe* set up by the perception as the result of habit. Expectation is an habituated consciousness. The psychophysical organism 'sets' to meet an imminent situation; and, on the conscious side, this 'set' is expectation. On the physical side are: bodily attitude, strained muscles, inhibited breathing, fixed sense organs. The image of the coming impression may sometimes be present, but is not an essential factor, not a characteristic element. Although the bodily attitude is that of attention, the pattern of consciousness is not attentional, chiefly because it lacks a definite focus, and has the mark of futurity given by the *Aufgabe*. Functionally, the expectant consciousness exists not for itself but for a consciousness about to be. It therefore lacks definiteness and clearness. It is a preparatory, a transitional consciousness.

[1] Über die Willenstätigkeit und das Denken, 38 ff.
[2] Archiv für die gesamte Psychologie, 1906, viii, 7-11.

A BIBLIOGRAPHY OF THE SCIENTIFIC WRITINGS OF WILHELM WUNDT

By E. B. TITCHENER and L. R. GEISSLER

(*First supplementary list*)

In accordance with the intention expressed in this *Journal*, xix., 1908, 541, we here print the first supplement to our Wundtian bibliography. We shall be grateful for any corrections and additional items that the readers of the *Journal* may be able to supply. It is probable that many occasional papers have escaped our notice.

1908

(4) *Völkerpsychologie*. Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. ii., pt. i. Die Kunst. Zweite neu bearbeitete Auflage. With 59 illustrations. Large 8vo. Leipzig, W. Engelmann. pp. x., 564. (In the new arrangement of the *Völkerpsychologie*, vol. i. contains Die Sprache, erster Teil; vol. ii., Die Sprache, zweiter Teil; and vol. iii., Die Kunst.)

(5) *Logik*. Eine Untersuchung der Prinzipien der Erkenntnis und der Methoden wissenschaftlicher Forschung. Vol. iii. Logik der Geisteswissenschaften. Dritte umgearbeitete Anflage. Lex. 8vo. Stuttgart, F. Enke. pp. xii., 692.

1909

(1) *Ueber reine und angewandte Psychologie*. Psychologische Studien, v., Heft 1 und 2, 18. Juni, 1-47.

(2) *Grundriss der Psychologie*. Neunte verbesserte Auflage. With 23 illustrations. Large 8vo. Leipzig, W. Engelmann. pp. xvi., 414.

(3) *Völkerpsychologie*. Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. ii., pt. iii. Mythus und Religion. Large 8vo. Leipzig, W. Engelmann. pp. xii., 792.

(4) *Die Anfänge der Philosophie und die Philosophie der primitiven Völker*. Pp. 1-31 of Die Kultur der Gegenwart, ihre Entwicklung und ihre Ziele. Vol. I, Abt. 5. Algemeine Geschichte der Philosophie. Herausgegeben von P. Hinneberg. Large 8vo. Berlin, B. G. Teubner, pp. viii., 572.

(5) *Festrede zur fünfhundertjährigen Jubelfeier der Universität Leipzig*. [Mit einem Anhang: Die Leipziger Immatrikulationen und die Organization der alten Hochschule.] Leipzig, W. Engelmann. pp. 1-53: [Anhang, 54-83.] Also in Offizielle Festzeitung zum fünfhundertjährigen Jubiläum der Universität Leipzig, 4. Nummer, 31. Juli 1909. Fol. Leipzig. Press-Ausschuss der Jubiläumskommission. pp. 3-9.

(6) *Das Institut für experimentelle Psychologie*. Pp. 118-133 of vol. iv., pt. i. of Festschrift zur Feier des fünfhundertjährigen Bestehens der Universität Leipzig. Leipzig, S. Hirzel, pp. vii., 246. Also as Sonderabdruck. Lex. 8vo. pp. 1-16.

THE INTERNATIONAL CONGRESS OF PSYCHOLOGY

By Edmund B. Huey.

The Sixth International Congress of Psychology convened at Geneva, August 3, 1909, and closed at noon, August 7. The committee of organization, consisting of Messrs. Flournoy, Ladame, Claparède, Cellérier, and Yng, had well arranged for the comfort and entertainment of the visitors, and worked out, as well, certain improvements in the internal procedure of the Congress. An informal reunion on the eve of the Congress was followed on successive evenings by receptions, a tour of Lake Geneva, a banquet given by the Council of State and the city of Geneva, and a farewell luncheon. The psychologists and their accompanying friends owe much to Geneva for this generous welcome, and the work of the Congress was much facilitated by these opportunities for personal acquaintance and discussion. A list of members, with their addresses and positions, was early placed in the hands of each member, and was a constant convenience.

There had been chosen, in advance, ten principal themes for discussion, and two or three principal reporters for each theme. The reports were obtained in advance of the Congress, and were printed and put in the hands of members on or before their arrival in Geneva. By this means the inconvenience suffered by previous Congresses, of having a very large number of "individual contributions" on very many disparate subjects, was reduced to a minimum. There were still some fifty individual contributions, as against 282 at Rome in 1905, but at Geneva their presentation and discussion occurred for the most part in secondary halls, and did not interrupt the consecutive treatment of the chosen themes. The individual contributions themselves were grouped, as much as possible, by subjects, and later by languages. There were also adjourned meetings in the secondary halls to continue discussions of the main themes. Apart from the main meetings occurred, as well, a few demonstrations of special apparatus and methods, an exposition of apparatus by various European dealers and mechanicians, and an exposition of certain methods of instruction in schools for the feeble-minded.

The central themes, in the order in which they were discussed, were as follows: The Psychology of Religion, reported by Professors Höffding and Leuba. The Subconscious, Professors Dessoir, Pierre Janet (absent), Prince. Mediumistic Phenomena, Professor Alrutz of Upsala. Backward Children, Drs. Decroly of Brussels, Ferrari of Bologna, Heller of Vienna, and Witmer (absent). Tropisms, Professors Bohn (Paris), Fr. Darwin (absent), Jennings (absent), Loeb. Orientation at a Distance, Professor A. Thauziès of Périgueux, France. The Feelings, Professor Külpe, Dr. Sollier. Perception of the Positions and Movements of our Body and Limbs, Professor Bourdon (Rennes). The Methodology of Pedagogical Psychology, Mlle. Dr. Ioteyko of Brussels.

Reporting on the psychology of religion, Professor Höffding argued that each of our feelings depends on some need which demands satisfaction, and thus on certain *values*,—physical, æsthetic, intellectual, moral. The religious character of the psychic life consists in our feelings about the *fate* of these values, in a need for security and calm in the soul in regard to them. The amount and character of our religious experiences will depend on which of these values touches most the heart of the individual. He may feel himself master of his fate and of that of his chosen values, and thus may dispense with religion. Or he may have any of a variety of feelings,—fear, hope, resignation, joy, chagrin, admiration and veneration, indignation, peace or its opposite. Religion having taken many forms, must be studied historically as well as by psychological observation and analysis,—in this not differing from a host of other psychological questions. The gods were divine because they were supposed to maintain men's supreme values, and the character of the gods depended on what values were felt as supreme. Man first sustained his values by his magic; then, resigning himself to his own insufficience here, he sought to realize their safety through appeal to beings more powerful than man. Of religious documents the most precious for the psychologist are those which show how eminent persons, plunged in the religious *milieu* of their time and people, react to and interpret the religious value of the prevailing dogmas and rites. The sudden appearance of great religious personalties may be compared to de Vries's mutations. Here there are certainly some enigmas and problems not yet solved. But this does not make it necessary to appeal to transcendent causes. And just as the biologists, while admitting the birth of new organic types in mutations, are asking if there were not preparatory, *pre*mutation periods of insensible gradations, so back of these sudden psychic transformations it may be our task to find neglected forces that have been active.

Professor Leuba believes that man first felt himself surrounded by beings of the psychic order, and later conceived the force material. Man became religious when he felt himself in relation with certain of the psychic forces. At first, religion was amorphous, larval, not yet organized to a system of beliefs and social practices. This religious condition is a stage found still in the decay of religions, and in certain individuals of all times. Religion has generally been a relation with *personal* powers; but there are exceptions in early Buddhism, in Compte, in certain modern pantheists and others who find in the service of Humanity what others seek in God. Religions, then, in their exterior aspect, are the systems of visible relations with these personal or impersonal powers, and in their subjective aspect are the states of consciousness which correspond to these. Generally the divine is conceived simply by the effects attributed to it, as electricity is conceived in terms of lightning, of movements of a machine, or other manifestation. Each pulsation of the religious life, just like the rest of conscious life, consists in will, feeling, and thought, indissolubly bound together in pursuit of an end. One or other of these three predominates in the consciousness, according to circumstances, but predominance is not essence. The God of the ordinary man is a power sufficient for his needs, who can free the sinner, cure the sick, etc. His religion can get on well without the "perfect" or "absolute," for which the religion of certain philosophers have such a special taste.

Differing from Professor Höffding, Professor Leuba considers the pursuit of values, itself, to be religion, when done with the help of a superhuman psychic power. As to the distinction between philosophy and religion, philosophy seeks God to know what he is, religion seeks in him the satisfaction of an insufficience.

The discussion, taken up again in several secondary meetings, often went far afield; and among the closing resolutions of the congress it was recommended that the psychology of religion should not be one of the themes of the next congress.

On the Subconscious, Dr. Morton Prince reviewed the six main meanings of this term, and found the term *co-conscious* preferable for the active dissociated states which are not really phenomena of "unconscious cerebration." The physiological brain dispositions which subserve memory are simply to be termed unconscious. Dr. Prince emphasized the unreliability of judgment that is not based on personal familiarity with the phenomena in this field of study, and also the fact that "introspection" fails as a technical method, and gives but a poor, inadequate, and partial glimpse into the world of consciousness. The psycho-analytical method, as well, can only give questionable results. Subconscious ideas, he believes, play a large part in normal and in pathological life, but their presence is difficult to prove, as the technical methods *create* dissociations and, therefore, artifacts.

In the absence of Professor Janet, his paper was supported especially by Dr. Bernard Le Roy of Paris. He recalled Janet's original application of the term subconscious to certain phenomena of hysteria, and urged that confusion would still be avoided if the term were restricted to the practical needs of the psychiatrical clinic.

Professor Dessoir endeavored to treat the subject from a "purely psychological" standpoint, but based his treatment on certain presuppositions regarding the nature of consciousness as "Formprinzip." The discussion which followed did not indicate that this method of approach was likely to be very fruitful.

On the subject of Backward Children, Dr. Decroly proposed and explained a complete system of classification, and pointed out, as de Sanctis and others have done, the insufficience and inaccuracy of the usual classification into idiots, imbeciles, and feeble-minded or "debiles." He emphasized the importance of taking into account extrinsic causes, the abnormalities of environment, in the production of irregularities, and also the fact that the causes, whether extrinsic or intrinsic, were always multiple.

Dr. Schuyten reported experiments which indicated that certain tests of memory, and even tests with the æsthesiometer gave very trustworthy means of distinguishing degrees of intelligence. While this work seems to have been done with much care, the other investigators were not ready to accept these tests as sufficient. Professors de Sanctis of Rome and Ferrari of Bologna were prominent in the discussion, the former urging, among other things, that too much attention was being given to questions of classification.

Professor Loeb, the central figure in the discussion of Tropisms, briefly and clearly reviewed the historical development of the theory of tropisms and the stages of his own study in attempting to explain mental phenomena in terms of physico-chemical laws. Many objections, he thinks, are due to ignorance of these laws, or to the mistaken belief that all the organism's tendencies are necessarily to the organism's advantage; or account is not taken of the fact that tropisms form only one of several classes of physical-chemical processes which are basal to mental functioning. Ideas, for instance, are mechanisms which can heighten the sensitivity for certain stimuli and so lead to tropism-like movements which are directed to a purpose, just as heliotropism may be dependent on the presence of acid, and just as certain other reactions depend upon the presence of sex-secretions.

Discussing the orientation reactions to gravity, Professor Loeb reported experiments proving that these reactions are not regulated by pressure of the otoliths upon the nerve-endings, but that the regulation is mediated by changes occurring within the nerve-endings themselves. If the otoliths are washed out without injuring the nerve-endings, the reactions to gravity occur as before.

Professor Bohn, of Paris, taking the position of Loeb as against Jennings, showed that tropisms must necessarily have a great variability, following all the variations of chemical composition of living matter. He argued that the tropism was not learned but that the apparent "trials and errors" of tropisms were only perturbations caused by the new activities of "differential sensibility," and "associative memory." The tropisms and their variations are very badly adapted, and the world of lower animals is "made of imperfections."

Professor Jennings, in his paper, adopts heartily the aim of Loeb "to analyze the behavior of animals from a chemico-physical point of view and substitute the methods of modern science for the anthropomorphism of the metaphysician." He admits the fact of tropisms in Loeb's sense, and makes much the same definition and limitations of the concept. But even in the reactions in which orientation is a prominent feature, in the tropisms in other words, the organism uses "whatever means it has at its command." So these reactions, like food reactions, are simple in the simplest organisms and complex in the highest ones. Professor Jennings argues essentially for "the complexity, modifiability, and regulatory character of behavior, and its refusal to fit any simple and uniform schemata."

Professor Thauziès gave a demonstration of loosing carrier pigeons, which took up their flight for Versailles and other home points, the time of their arrival being telegraphed to the Congress. Professor Thauziès reviewed the theories of orietentation at a distance, adopting none; but he called attention to the perturbations of flight found to occur under atmospheric conditions of "magnetic storm" and "special electrical situation."

Professor Bourdon reviewed the theories and experiments on the function of the labyrinth in mediating the perception of the position of our body and head with reference to the vertical. He reported his own varied experiments with the rotation table, and other apparatus, showing that the "excitations, mechanical, galvanic, etc., of the tonic labyrinth which are produced either normally or experimentally do not affect the consciousness,"—the perception of an inclination of the body "being furnished by sensations of pressure, of distension of skin, and of effort." He admits, of course, the control of movements through stimulation of the labyrinth.

Professor Külpe, in his report on The Feelings, first distinguished between true and false criteria of feeling. The feelings, pleasantness and unpleasantness, may accompany single consciousness contents or may color the whole consciousness; may be active or passive; may be feelings of shock or of mood. There are no qualitatively different varieties of pleasantness or unpleasantness. Professor Külpe classified the methods of investigating the feelings, and made a critical résumé of the results of investigation thus far, and of the theories of feeling.

Dr. Paul Sollier reviewed the theories of cœnesthesia and its relation with the consciousness of personality, as observed in various pathological conditions. He suggests that the feeling of our personality may base upon the association between our present and our past states, and that the cœnesthesia is the feeling of this association. Or, the cœnesthesia may be the sensibility peculiar to the brain as such, giving us data concerning the brain's functioning, accompanied by a special feeling, that of the "me," something beyond the affective tone inherent in every sensation. The centres involved would seem to be in the frontal lobes.

Reporting on Pedagogical Psychology, Mlle. Ioteyko of Brussels, directress of the new *Revue Psychologique*, proposed an interesting application of mathematical methods to psycho-pedagogical problems. Analyzing the fatigue curve, for example, she determines certain parameters, or constant factors, due to diminution of reserve carbo-hydrates, to the using up of albuminoids, to the action of resultant toxins on the muscle, etc. The value of these constants is modified in definite ways by introducing the factor of alcohol, sugar, anæmia of the arm tested, etc. Similarly, she believes, from the complex curve of growth may be analyzed out the values of constituent factors such as muscular force, weight, attention, memory, etc. In a given individual, certain psychic constants, as timidity, weakness, indecision,—certain moral ideas, certain feelings, tend to exercise a certain force in conduct, the total curve being a resultant of these with other factors. *Observation* of the operation of these "parameter" factors in the child's conduct gives results superior to those from *tests*, the latter always presenting an artificial side, though needed to complete the study. The Congress recommended that a committee be appointed to further the interests of psycho-pedagogy. This committee will be appointed by the committee for the next congress.

Reporting on Questions of Unification, Dr. J. Courtier, of Paris, proposed an elaborate system of symbols and signs for use in psychology. Professor Claparède showed the advantages that would come from the adoption and use of a precise nomenclature, and from a determination of equivalent psychological terms in the various languages used in psychology. His report made certain admirable recommendations in these directions.

M. R. de Saussure argued for the use of esperanto as a "base of unification and of comparison of technical terms." As a demonstration three short addresses were delivered in esperanto. The proposition did not meet with any special favor in the Congress. Professor Baldwin made certain recommendations looking toward unification of terminology, and the Congress appointed a committee consisting of Messrs. Baldwin, Claparède, Lippmann (Berlin), and Ferrari (Bologna), to study the subject, receive suggestions, and make recommendations to the next Congress.

The discussion on standardization of colors seemed to indicate that an approximate system of standards, all that seems possible in the present state of science, might best be found on the side of the industries; and M. Th. Valette, of the laboratory of the Gobelins Manufactory in Paris, briefly reported the classification of Chevreul, used in their manufacture of tapestries. A committee on standardization of colors was appointed, consisting of Messrs. Nagel, Asher, Thiéry, Yerkes, Larguier, with a chemist yet to be named.

The next congress will be held in the United States in 1913, preferably in New York or Boston. The Committee of Organization consists of Professor William James, President d'honneur; Professor Baldwin, President effectif; Professors Titchener and Cattell, vice-presidents; Professor Watson, general secretary. The American representatives at the Geneva congress, so far as noted by the writer, were Professors Armstrong, Baldwin, Fullerton, Haines, Hollingworth, Huey, Jones (Toronto), Mrs. Ladd-Franklin, Leuba, Loeb, Max Meyer, Will S. Monroe, Ogden, Prince, Riley, Sanford, Strong, Vibbert, and Miss Williams.

PSYCHOLOGICAL LITERATURE

Memories of my Life. By FRANCIS GALTON. With eight illustrations. New York, E. P. Dutton and Company, 1909. pp. viii., 339. Price $3.50.

In this handsome volume, clearly printed and light in the hand, Sir Francis Galton—the recent list of birthday honors has given him the title long ago conferred by a public careless to discriminate between cousins—sketches, briefly and interestingly, the main events of his long and varied career. There are three portraits, and an Appendix contains a full bibliography of the author's books and memoirs.

"It has been a difficulty throughout," we are told in the Preface, "to determine how much to insert and how much to omit. I have done my best, but fear I have failed through over-omission." The fault is, no doubt, upon the right side. But the psychological reader would have been grateful for a more detailed account of the author's psychophysical work. Sir Francis Galton was, after all, the pioneer of experimental psychology in Great Britain; he originated much that was either independently discovered or further elaborated on the Continent; and his memoirs are widely scattered in technical magazines and in the proceedings of scientific societies. However, we may perhaps look forward to a complete collection of these papers at some future date.

We are here not concerned with the author's explorations in southwest Africa, with his work upon the council and in the secretaryship of the British Association, or with his activities as member of the Meteorological Committee and Council. The chapters in which these matters are discussed are delightful reading; but that bare statement must suffice. We come nearer to psychology with the chapter on anthropometric laboratories, which ends with an amusing anecdote of Herbert Spencer. The succeeding chapter deals with composite portraits and stereoscopic maps. The writer does not seem to be aware of the objection, raised in France some years ago and mentioned by Binet in his *Étude expérimentale de l'intelligence*, that the first portrait exposed exerts an undue influence upon the photographic plate, so that the camera is not impartial. An attempt at 'analytical photography,' in which a negative composite was photographed together with a positive portrait of one of its elements, is referred to as a failure. The conventional representation of a galloping horse, with all four legs simultaneously extended, was found to be reproducible by compounding a series of momentary attitudes. "When all of the twenty attitudes [of Muybridge's photographs] are combined in a single picture, the result is certainly suggestive of the conventional representation, though in a very confused way . . . Seeing that according to the photographs . . . the two fore legs were extended during one quarter of the complete motion, and that during another quarter the two hind legs were similarly extended, I made composites of these groups separately." Combination of the halves of the composites gave "a very fair equivalent to the conventional attitude. I inferred that the brain ignored one-half of all it saw in the gallop, as too confused to be noticed; that it divided the other half in two parts, each alike in one particular, and combined the two halves into a monstrous whole."

Two experiments, hitherto unpublished, may be quoted from the chapter on Human Faculty. The one was planned "to gain some idea of the commoner feelings in Insanity. The method tried was to invest everything I met, whether human, animal, or inanimate, with the imaginary attributes of a spy. Having arranged plans, I started on my morning's walk, . . . and found the experiment only too successful. By the time I had walked one and a half miles, . . . every horse on the stand seemed watching me, either with pricked ears or disguising its espionage. Hours passed before this uncanny sensation wore off, and I feel that I could only too easily re-establish it." The other was designed "to gain an insight into the abject feelings of barbarians and others concerning the power of images which they know to be of human handiwork. I had visited a large collection of idols gathered by missionaries from many lands, and wondered how each of these absurd and ill-made monstrosities could have obtained the hold it had over the imaginations of its worshippers. I wished, if possible, to enter into those feelings. It was difficult to find a suitable object for trial, because it ought to be in itself quite unfitted to arouse devout feelings. I fixed on a comic picture, it was that of Punch, and made believe in its possession of divine attributes. I addressed it with much quasi-reverence as possessing a mighty power to reward or punish the behavior of men towards it, and found little difficulty in ignoring the impossibilities of what I professed. The experiment gradually succeeded; I began to feel and long retained for the picture a large share of the feelings that a barbarian entertains towards his idol, and learnt to appreciate the enormous potency they might have over him." Experiments of this sort, empathic experiments, might well be introduced into the laboratory, and should yield valuable analytical results. The writer himself does not go further into description of the uncanny sensation of being watched, or of the reverential feeling towards the figure. The chapter contains other suggestions of an experimental sort, among them this: "the human senses, when rhythmically stimulated in certain exact cadences, are capable of eliciting overwhelming emotions not yet sufficiently investigated."

A controversy with Max Müller (who had emphasized the importance of language as a means of thought, while the author believed that he himself thought hardest when making no mental use of words) led to the well-known experiments upon arithmetic by smell. After certain associations had been practised, the mental cue to 'add' or 'subtract' was sufficient to start the arithmetical operations in terms of smell alone; "there was not the slightest difficulty in banishing all visual and auditory images from the mind, leaving nothing in the consciousness besides real or imaginary scents." These experiments should be repeated: first, in order to determine the specific mode of representation of the 'imaginary' scents; and secondly in order to determine whether the odors are at once 'sign' and 'thing signified,' as may be the case with words, or whether the meaning which has been read into the odor by association is carried by some concomitant process which, in the original experiments, escaped observation.

The final chapters treat of Heredity and Race Improvement. After stating the Ancestral Law, the author writes: "my data were not as numerous as is desirable, still the results were closely congruous, and seem to be a near approximation to the truth. The conclusions have been much discussed and criticised, and have been modified by

Professor Karl Pearson; but they have not been seriously shaken, so far as I know." Here is a point where a more detailed consideration would have been in place. We do not know whether Mendelism has received careful study, and has been rejected as unreliable, or whether the writer has failed to follow recent developments in the theory of hereditary transmission. On the subject of Eugenics we read: "its first object is to check the birth-rate of the Unfit, instead of allowing them to come into being, though doomed in large numbers to perish prematurely. The second object is the improvement of the race by furthering the productivity of the Fit by early marriages and healthful rearing of their children. Natural Selection rests upon excessive production and wholesale destruction; Eugenics on bringing no more individuals into the world than can properly be cared for, and those only of the best stock."——

Sir Francis Galton belongs to a vanishing type of workers in science,—men of high native ability and independent fortune, who devote themselves to the advancement of knowledge and its application in the public service from an intrinsic interest and a keen sense of public duty. Charles Darwin, his near kinsman, is perhaps the most conspicuous instance of the type, which indeed has always found its principal representatives in Great Britain. The vast accumulation of scientific observations in recent years, and the necessity of a technical training to cope with it, have now brought the specialist to the front in all intellectual concerns; and the amateur, however gifted, must in the future be content to take a lower place. It is not likely that our author will have successors. All the more should we be grateful for this outline of his life and labors; all the more should we pay our free homage to one who, without the responsibilities of an official position, did yeoman's work on behalf of the struggling science of experimental psychology. TH. WALTERS.

Fifty Years of Darwinism: Modern Aspects of Evolution. Centennial Addresses in Honor of CHARLES DARWIN before the American Association for the Advancement of Science, Baltimore, Friday, Jan. 1, 1909. New York, Henry Holt & Co., 1909. pp. v., 274.

This volume, the nature and object of which are sufficiently indicated by its title, contains ten addresses and a brief introduction. The Introduction, written by Prof. T. C. Chamberlin of the University of Chicago, the president of the American Association for the Advancement of Science, points out the influence that the thought of Darwin has exerted and still exerts upon the work of the Association. "In the first decades of the great Darwinian movement in biology, the tribute of our members may not have been wanting in demonstrations of the force of old adhesions, but even then, whether by resistance or by co-operation, we gave our testimony to the new power that made itself felt in the scientific world. A little later, we paid the tribute of conviction—the general tribute of willing conviction, on the part of some of us, and the even more significant tribute of reluctant conviction, on the part of others; but, in one way or another, we paid a universal tribute." The continuance of this influence is attested by the following addresses.

Professor E. B. Poulton, Hope professor of entomology at Oxford, opens the series of special papers with a review of Fifty Years of Darwinism. We cannot mention all the points, biographical, appreciative, critical, controversial, made by Professor Poulton in the course of his address. We notice only his strong insistence on the influence of Sir Charles Lyell, the geologist, upon Darwin's mind; his hardly qualified rejection (p. 40), as against Francis Darwin, of the doctrine of the hereditary transmission of acquired characters; his emphatic statement of Darwin's conviction, after prolonged study of mutations, that "it is by the accumulation of extremely slight variations that new species arise," that evolution is continuous and not discontinuous; and his defence of natural selection against the charge that it "descended like a numbing spell" upon studies of hybridism. It is interesting to read (footnote, p. 22) that Adam Sedgwick attacked the *Origin* on the ground that it "utterly repudiates final causes." Huxley, too, thought that it had banished teleology from biological science. The event has proved that, on the contrary, it threw the doors only too wide open for facile explanations in terms of end.

Professor J. M. Coulter, of the University of Chicago, next discusses The Theory of Natural Selection from the Standpoint of Botany. Two points are given special emphasis: the prevalence of non-adaptive characters, and the inadequacy of natural selection to account for the great changes in the phylogenetic series. Botanists are forced to see in a very large number of structures "inevitable responses to conditions that have nothing to do with adaptation. . . Natural Selection does not select individual plants on the basis of some small and better adapted variation, and so build up a character, which with its associates will gradually result in a closely allied new species; but . . . its selection of individuals seems to hold no relation to their useful characters. On the other hand, . . . Natural Selection determines what species shall survive, simply by eliminating those that do not." The principle still, therefore, has an important place, since "the species that survive determine, within limits, the species to be produced." But it is inadequate to account for "the establishment of the assemblages of different characters that distinguish great groups"; for the study of phylogeny shows that these "have been wrought out by steady and progressive change through all imaginable changes of environment"; the evolution of Gymnosperms is cited in evidence. On the whole, then, Professor Coulter's paper gives a negative impression, though he grants that Darwin's personal position in plant physiology and ecology is one of the first rank, and that the principle of natural selection came into plant morphology at the psychological moment and served greatly to stimulate investigation.

Dr. D. S. Jordan, president of Stanford University, discourses on Isolation as a Factor in Organic Evolution. "So far as species in nature are concerned, we can account for the origin of none of them, except on the ground of the presence of some forms of isolation. . . . The minor differences which separate species and sub-species among animals and plants, in so far as these are not traits of adaptation (and most of them are clearly not such), owe their existence to some form of isolation or segregation. . . . From difference of parentage, or difference in selection, or from difference in the trend of development, whatever its cause, local peculiarities arise. . . . It may be that change of environment sometimes excites germinal variation. . . . The obvious factor in the splitting apart of races or species is, therefore, in all groups, that of isolation. Behind this lies the primal factor of variation, continuous or discontinuous. . . With these come the factor of heredity and the factor of selection. . . . Selection alone does not prodnce new species, although it may continuously modify old ones." These quotations sufficiently indicate the line of reasoning pursued in the paper. "In Darwin's view, isolation or segregation was doubtless a feature of Natural Selection." Since Darwin, Moritz Wagner has convincingly shown the fundamental relation of this factor to the problem of the origin of species.

Professor E. B. Wilson, of Columbia University, treats of The Cell in Relation to Heredity and Evolution. Darwin's doctrine of pangenesis involved two principal postulates: the particulate assumption that particular hereditary traits are represented in the germ-cell by discrete and specifically organized particles, capable of self-perpetuation without loss of specific character; and the assumption that these gemmules or pangens are cell-germs originally produced by the somatic cells. The first of these postulates has been adopted and worked out, notably by Weismann; but it is so far unverified, and probably unverifiable. The second is unsupported by experimental or cytological evidence; we must accept, in its place, the law of genetic cellular continuity. Four questions then arise. (1) What is the physical basis of heredity? It is probable that the nucleus (the chromatin-substance) embodies the specific primordia of determination. True, there are formative protoplasmic materials; but some of the most important are known to be of nuclear origin, and it is possible that all have that origin. (2) How is it transmitted from cell to cell? By division; and, in most cases, by karyokinetic division. The chromosomes are composed of specifically different materials, the ensemble of which is essential to normal development; and karyokinesis ensures the meristic division and equal distribution of these materials. (3) In what way does it play its part in the determination of the hereditary characters? Chemically: the chromosomes may be regarded as self-perpetuating magazines of specific substances, similar in nature to enzymes or their chemical antecedents, that play an essential part in the determination of the cell-activities, including those involved in development. The fertilization of the egg may almost be compared to an intracellular injection of enzymes. This view squares admirably with the Mendelian view of heredity. (4) How may it be so modified as to give rise to new heritable characters? Both body-cells and germ-cells react to physico-chemical changes of the environment by definite physiological and morphological changes; such modifications of the germ-plasm produce blastogenic variations or mutations that are inherited, permanently or temporarily. In this way we can account for direct influence by invironment, fluctuating variations, mutational variations, particulate mutation, orthogenetic transformation,—for everything, in fact, but the transmission of acquired characters.

Dr. D. T. MacDougal, of the Carnegie Institution of Washington, writes upon The Direct Influence of Environment. We know that secondary effects of the action of external factors, or of morphogenic or embryonic procedure, may be freely communicated in the plant from part to part of the soma, and from the egg to the soma. May inheritable effects be communicated from the soma to the egg or sperm? (1) The character of the building material supplied to the egg, as varied by environmental influences, may work changes that pass from generation to generation. (2) Repetition of stimuli in a series of generations, so far as examined, is effective for heredity only when the germ-plasm has been acted on simultaneously with the soma. It is, however, difficult to induce bodily changes without affecting the reproductive elements; and the attention of biologists has therefore turned toward modification of egg or sperm alone. They have studied the influence of changed climatic conditions, of radiations, and of chemical solutions, and in every case have found that individually acquired or induced characters may be transmitted from one generation to another practically unchanged. Nevertheless, the case against the transmission of purely somatogenic characacters is not closed: the physiological mechanism would permit it; experiments on the action of external agencies upon the soma exclusively have hardly yet been seriously attempted; and certain adaptive correlations of organs, found in nature, distinctly suggest it. Coming back to the work on the germ-plasm, we must refer the inherited changes to departure from normal mitotic procedure or to disturbance of the autolytic action of the cell. The changes themselves were in some cases discontinuous (mutations), in others continuous (fluctuations). Some, again, consisted in increase or loss of existing capacities; others in the acquisition of characters new to the genus. The processes thus "disturbed or set in motion" appear to be "identical with some of those concerned in the main evolutionary development of organisms."

In the next address, on The Behavior of Unit Characters in Heredity, Professor W. E. Castle of Harvard University shows, by reference to the color-varieties of rabbits, the nature and influence of Mendel's unit characters. He traces the advance of Mendelism in recent years, and indicates some of its problems. He concludes that, "fragmentary as our present knowledge is, it is doubtful whether any category of organs, quantities, or parts, can be mentioned which is not subject to Mendelian inheritance." The style, clear and straightforward, is in pleasant contrast to the involved Latinism of the preceding paper.

Mutation is handled by Dr. C. B. Davenport, of the Carnegie Institution of Washington. "The mutation theory rests on the doctrine of unit characters and applies only so far as that doctrine applies." Darwin recognized saltation, especially in the *Variation under Domestication;* but he emphasized quantitative or fluctuating variation in the *Origin*, because he was there championing the continuity of nature as against the doctrine of special creation. Mutations have now been studied both in nature and under domestication. (1) The selection of fluctuations and their summation bring an improvement that (as Darwin himself recognized in certain cases) ceases after a few generations; the fortunate sport is then the centre from which further progress must start. (2) Mutations are subject to natural selection; even so close a case of adaptation as that of the leaf-butterfly may arise by a series of mutations (Bateson); and mutations may become adaptive by their possessors selecting a habitat that fits their organization. (3) The doctrine of mutation explains the swamping effect of inter-crossing, and the discontinuity between species. There may, no doubt, be cases of graduated development of unit characters, all the way from invisibility to strong expression; but for the present we shall do best to assume their normal discontinuity.

After Mutation comes Adaptation, which is discussed by Professor C. H. Eigenmann of the University of Indiana. An organism consists of adaptive characters, of vestiges (remnants of past adaptations), and of non-adaptive characters, that may perhaps be past adaptations or may become adaptive in the future. If we confine ourselves to the adaptive characters, two questions arise: that of the origin of adaptive faunas, and that of the origin of adaptations. The first is answered, in general terms, by reference to selective migration; suitable habitats are sought by species that are already, in the large, adapted to them. The second is far more difficult to answer. The author bases his treatment of it upon observations of the freshwater fishes known as Characins, and comes to the following conclusions. (1) The Characins furnish evidence both for orthogenesis and against its universality. (2) There is evidence both of mutation and of fluctuation. (3) Transmissible adaptations undoubtedly arise as the result of peculiarity of environment. Three ways are possible: use-transmission, direct affection of the germ-plasm, realization of nascent adaptive capacities. The latter must be ruled out, however, in the case of the bleaching of cave-animals. (4) Do transmissible variations arise as the result of functional adaptation? Apparently they do (progressive degeneration of the eyes of cave vertebrates), although the evidence has not so far carried general conviction. Further work, experimental, observational and systematic, is needed for the solution of this problem of problems, the origin of adaptations.

In the following paper, on Darwin and Paleontology, Professor H. F. Osborn, of Columbia University and the American Museum of Natural History, hails Darwin as, in succession to Cuvier, the second founder of paleontology. Darwin's view of the operation of natural selection is either confirmed or untouched by paleontological discoveries. On the other hand, the geological record shows clearly that new characters do not originate by selection of the fit from the fortuitous; "the law of gradual change in certain determinate, definite, and at least in some cases adaptive directions, through very long periods of time, and the absence of chance or non-direction in the origin of a large number of adaptive and other new characters, is the common working principle both in vertebrate and invertebrate paleontology." This law is not subversive of the doctrine of mutations; the paleontologist is favorably situated for the observation of continuous, unfavorably for that of discontinuous evolution; the question of mutation or fluctuation must be settled elsewhere. The law asserts, however, that certain origins are adaptive in direction from the beginning; and this 'principle of rectigradation' introduces a factor unknown to Darwin. Its causation is at present obscure. Use-transmission either fails to explain or proves to be unnecessary in explanation, though it should not yet be finally abandoned. The causation lies, probably, in the domain of heredity rather than in that of ontogeny, environment or selection; in view, however, of the inseparableness of internal and external processes, the initiation of certain adaptive origins may be found in the circle of ontogenetic or environmental causes. The address closes with a review of the method of evolution as exemplified by the Titanotheres.

The concluding chapter of the book, contributed by G. S. Hall, president and professor of psychology, Clark University, is entitled Evolution and Psychology. It first makes the point that "Darwin's method is always and everywhere objective and observational, never subjective or introspective." "Emotions are best studied in their outward expressions in gesture, will is investigated in the study of behavior, intelligence by massed instances of sagacity." Conceding the fact that the current, non-evolutionary methods have brought about "a prodigious and sudden horizontal expansion" of psychology, it then affirms that genetic synthesis is both necessary and impending. "Some of us are already convinced that the human soul in all its power is just as much a product of evolution as the body; but our faith needs to add the knowledge that can only come when all the authentic data are properly grouped." The address turns, next, to a consideration of rudimentary psychoses and psychical recapitulation. Consciousness is an epiphenomenon of mind; there are incessant and manifold affective and other processes going on in us that lack consciousness; such states and activities irresistibly suggest past evolutionary stages. "The child is not so much the father of the man as his very venerable and, in his early stages, half-anthropoid ancestor." Finally, in a forecast of the future, the author predicts the adequacy of genetic psychology to education, religion, psychiatry, and the domain of the unconscious, if only there shall arise a new "Darwin of the mind" to substitute for the pedantry of the schools "the essential facts of life as it is lived out by the toiling, struggling men, women and children, normal and defective, of our day."

P. E. WINTER.

The Eternal Values, by HUGO MÜNSTERBERG. Boston and New York, Houghton Mifflin Company, 1909. pp. xv, 436. Price $2.50.

This work is a modified author's translation of the *Philosophie der Werte*, reviewed in the *Journal*, xix., 1908, 408 f. "Much in this English version is newly added and much is omitted from the previous text. Many side issues, especially such as connected the work with particular German movements, are entirely left out, and not a few additions refer to recent American discussions. Other parts are greatly condensed." The writer's general attitude and manner of treatment are, of course, unchanged.

In my review of the German original I expressed the opinion that the book, notable as the expression of a strongly marked personality, could nevertheless not hope to appeal, vitally and enduringly, to any large body of academic youth. "The next reformation in thought must come", I said, "from within the sciences, not from the external realm of concepts." It is only fair, then, to quote the author to the contrary. "Far beyond the circle of philosophers it was greeted as an expression of the new desires of our time. . . The ethical idealism of the book seemed to touch the most widely different layers of the community." Whether the impression thus made is permanent remains to be seen. At any rate, nothing but good can come of the mental effort of sustained thinking which *The Eternal Values* demands of its reader. "More and more", remarks Professor Münsterberg, "the aim seems to be the writing of philosophy in brilliant epigrams and clever discussions. Especially our younger philosophers dash down their thoughts in an impressionistic style which captivates and does not need the slightest effort to follow. Who will doubt that such picturesqueness is stimulating and attractive? Yet after all it is serving the ultimate purpose of knowledge no better than a picturesque and epigrammatic mathematics or chemistry. Philosophy is a movement of thought which demands the thoroughness of the expert, and which can be followed only with concentrated attention. Everything depends upon inner consistency, and only a closely knit system can secure it. In all times only such systems have marked the great periods of philosophical insight." Saul also, then, is among the prophets; reaction has set in within the precincts of the temple itself. May the reaction prosper!

M. W. WISEMAN

Herbert Spencer. Von DR. KARL SCHWARZE. Mit einem Bildnis Spencers. "Aus Natur und Geisteswelt", Bd. 245. Leipzig, B. G. Teubner. 1909. pp. x., 131. Price Mk. $1.25.

When F. H. Collins published his Epitome of the Synthetic Philosophy in 1889, Spencer himself vouched for the accuracy of the work, and expressed surprise that so much of the original System could be compressed into so small a space. The fact is, however, that Spencer is of all systematic philosophers the easiest to summarize: partly because his thought moved linearly, idea following idea in single file, towards a well-defined goal; partly because, in his endeavor after clearness, he was not sparing of words; and partly also because he knew definitely, in every case, what he wished to convey to the reader.

Nevertheless, it is something of an achievement to give the gist of the Spencerian philosophy in just over a hundred small octavo pages, and this is the task that Dr. Schwarze has undertaken, and has successfully performed, in the book before us.

The frontispiece is a reproduction of the well-known portrait taken when the philosopher was seventy-eight. The introduction (1-18) gives a biographical sketch of Spencer's career. The synthetic philosophy is then taken up book by book (18-125): first the general philosophical foundations of the system (18-35), then the Biology (35-55), the Psychology (55-76), the Sociology (77-99), and the Ethics (99-125). The style is interesting, and the abstracting is competently done. A critical conclusion (125-131) seeks to place Spencer in correct philosophical perspective. Epistemologically, he is akin to Kant, the Unknowable corresponding to the *Ding an sich*. Metaphysically, he represents a pantheism like that of Spinoza. His dualistic standpoint recalls the Wundtian doctrine of physical and psychical causality. The finality which he claims for his systematization reminds us of Hegel, and the fate of the two systems is the same. On the whole it must be said that Spencer has left his philosophy a torso. If we carry out the figure of a statue, Spencer's systematized science is the trunk, and the trunk only. There should be a head, epistemology; but Spencer offers us in its place ethics, and an ethics that is incomplete because worked out one-sidedly from the naturalistic standpoint. There should be limbs; but Spencer provides nothing more than conjectures as to the future of the race, and these conjectures cannot support his science. His fame will therefore depend upon his scientific, not upon his strictly philosophical accomplishment.

L. TURLEY.

Psychologie de l'enfant et pédagogie expérimentale. Par ÉD. CLAPARÈDE. Deuxième édition, revue et augmentée, avec 9 figures. Genève, Libraire Kündig. 1909. pp. viii., 283.

This is the second edition of a little work published in 1905. The author, taking interest as the fundamental principle of mental activity, has sought to orientate the teacher in the field of child-psychology and experimental pedagogy. The exposition is extremely orderly and systematic; every chapter classifies and classifies again; indeed, the book may be considered as a sort of subject-index of problems and methods, made out in the service of the doctrine of interest.

The introduction, entitled *Psychologie et Pédagogie*, holds the balance between those who affirm that psychology is everything, and those who declare that it is nothing, for the teacher. The views of Stanley Hall, James and Münsterberg are cited and discussed. The author concludes that "even if the teacher has entirely forgotten his psychology, it is worth his while to have been, at the outset of his career, a sound psychologist."

Chapter i. consists of an *Aperçu historique*. Professor Claparède remarks that there does not exist, to his knowledge, a work which sets forth in detail the history of the modern pedological movement. Such a work is undoubtedly needed, and the author's chapter furnishes a useful beginning in this direction.

Chapter ii. treats of *Les problèmes*. Practical pedagogy has a fourfold aim: the preservation of health, intellectual and physical exercise (*gymnastique*), the furnishing of memory, and education in the strict sense of the term. The aim of education, strictly considered, is to establish character, to stimulate ambition, to develop will and personality. Here then are the problems of modern pedagogy, classified from the point of view of subject-matter. If we take another point of view, that of their relation to practice (*portée pragmatique*), the problems fall into two great groups as theoretical and practical, problems of pure and of applied psychology; and the second group divides again into problems of psychognostics and problems of psychotechnics. What are the laws of memory? that is a question for pure psychology. How may we measure the capacity of memory in the individual case? that is a question of psychognostics. What is the best method of memorizing a text in a given time? that is a question of psychotechnics. Lastly, if we classify by reference to results, the problems may be grouped under the headings general and special, the latter covering such things as mental type, correlation of traits in the individual mind, etc.

Chapter iii. treats, in the same way, of *Les méthodes*. If we classify by reference to the character of the observed phenomena, we have the methods of introspection and of extrospection. (The latter term is better than Binet's externospection; but there seems to be no reason for coining it; we already have the word inspection for external observation.) Reference to the general conditions of investigation gives us the distinction of observation and experiment. Reference to the procedures employed in assembling data gives us the distinction of individual and collective methods. Reference to the nature of the subject-matter gives us four types of method: the normal, the genetic, the comparative, and the pathological. Lastly, reference to the technique of our inquiry gives us the distinction of qualitative or descriptive and quantitative or psychometrical methods.

Chapter iv. discusses *Le Développement mental*. The primary question is: What is childhood for? To answer it we must first examine the phenomena of physical growth, and the reaction of growth upon the mental functions. This leads to the study of two functions of cardinal importance: play and imitation. Many theories of play have been formulated: the theory of recreation, of superfluity of energy, of atavism, of preparatory activity. The author inclines to a biological theory which combines the views of Groos and of Carr. Play may be classified as general and special. The former may be subdivided into sensory, motor and psychical; and psychical play subdivides again into intellectual and emotive. Special play takes five principal forms: rivalry, pursuit, social play, family play, and imitative play. Imitation, on its side, leads to acquisitions of two kinds, general and special. The general functions acquired by imitation are motor adaptation, voluntary movement and comprehension. The special functions are too numerous to list; we may mention language, and in the moral sphere the power of example. Now we may answer the question: What is childhood for? by replying: For playing and for imitating. But this means that education must be attractive. "En réclamant de l'enfant un effort de travail fondé sur autre chose que sur le jeu, on agit comme cet insensé qui, dès le printemps, secouerait un pommier pour lui faire donner des pommes." So we are led to investigate the psychobiological value of interest. The child passes through a number of stages, which follow each other in constant order; every stage corresponds to the ripening of a certain function or aptitude whose exercise is agreeable. Thus we have perceptual interests, linguistic interests, general intellectual interests, special interests, social or moral interests, which appear in the order given. The secret of pedagogy is to avail oneself of these aptitudes or functions, and to invite their activity by presenting to the child the objects which appeal to them; it is worse than useless to blame the child for the absence of something which, biologically, he does not and as yet cannot possess.

Chapter v. is entitled *La Fatigue intellectuelle*. Fatigue may be measured, first, by reduction of capacity. Here we have the methods of dictation, of counting letters, of addition, of copying letters, of combination. It may be measured, secondly, by measurement of the changes which it induces in various functions. Here we have the methods of esthesiometry, of algesimetry, of dynamometry, of tapping. Fatigue is itself conditioned upon age, sex, intelligence, individual type, season of the year, time of day, habit, training and interest, change of work, bodily posture, alimentary regimen (alcohol). The various subjects of study have different ponogenic coefficients. Physical exercise is decidedly ponogenic: hence educational gymnastics should be placed in the morning, hygienic gymnastics at the end of the school day. Fatigue offers a number of problems: the relation between objective and subjective fatigue, the relation between the physiological state of fatigue and power of effective work, the influence of will or interest on fatigue as physiological condition, the seat of mental fatigue, habituation to fatigue, the normal or abnormal character of fatigue, overwork or chronic exhaustion. These problems still await detailed study. Something may be said, however, in the light of our present knowledge. Thus, the idea of a reserve of energy throws light on various observations: on the dynamogenic value of certain stimuli, on the oscillation of work-curves, on the difference between fatigue and lassitude, on the effect of change of work, on sudden bursts of activity, on Janet's lowering of the mental tension. Overwork, again, seems to be due not to the amount or even to the difficulty of the work assigned, but to its nature; work of an inferior psychological kind is especially likely to induce it. Rest should be taken as soon as ever the signs of fatigue show themselves, though prevention is better than cure. The normal duration of the rest-period has not yet been determined. The best way to rest is to do nothing; and the best way to do nothing is to sleep.

Such, in outline, are the contents of the book. Every chapter has appended to it a selected bibliography. W. JENKINS.

The Revival of Scholastic Philosophy in the Nineteenth Century. By JOSEPH LOUIS PERRIER, Ph. D. New York, The Columbia University Press. 1909. pp. viii., 344. Price $1.75.

"One of the movements that have excited the interest of the world of thought in the nineteenth century had been the revival of Scholasticism. The philosophy of the Middle Ages had been for centuries past buried in deepest oblivion. . . Suddenly, to the astonishment of all, Scholasticism has awaked from its slumber. It has appeared again in the face of the world, has been accepted by great minds, has been expounded and defended by powerful writers, and has given rise to a great number of interesting philosophical works. Its admirers have even tried, not only to prove its congruity with modern scientific results, but to show that it is the only system capable of explaining them." These sentences stand at the beginning of the Introduction of the work before us. As to the reasons for the scholastic revival, the author sums them up as follows. "An honest endeavor to seek the true philosophy in modern systems had been made for several centuries. But, from a Catholic standpoint, this endeavor had completely failed. . . . The more recent systems, Materialism, Kantism, Hegelianism, Positivism, were opposed to the Catholic faith. The influence of these systems had led many Catholics to advance dangerous theories. . . Was it not better to return frankly to the philosophy which had reigned for centuries in the schools, . . . to find out whether the old Scholastic philosophy was not the true system? . . . Such is, in my judgment, the fundamental idea which inspired the neo-Thomists."

Neo-Scholasticism is, then, a philosophy thought out in conformity with Roman Catholic theology. Dr. Perrier has set himself a twofold task with regard to it: an exposition and discussion of those principles of Scholasticism, a knowledge of which is indispensable to an understanding of the Scholastic revival; and an historical survey of the rise and progress of Scholasticism in the modern world.

The first part (chs. i.-viii.) opens with a chapter entitled: What is Scholastic philosophy? After weighing various definitions, the author concludes, somewhat vaguely, that "Scholastic philosophy is primarily and essentially the philosophy of the Middle Ages, and reflects the essential characters of that time. The greatest power in the western world . . . was doubtless the Roman Church. The Middle Ages were above all an age of faith." Hence "the harmony between philosophy and theology, although not peculiar to Scholasticism, is certainly its most distinctive trait." The modifications introduced by Neo-Scholastics may be classified under three heads: language and method (although the *Philosophia Lacensis* and Urraburu cling to Latin and the Thomistic method of argumentation); appreciation of historical studies (although Orti y Lara regarded historical inquiry as vain bibliomania); and respect for modern science (although Mazella holds to a doctrine of creation *in statu perfecto*).

The following chapters deal with scholastic logic, scholastic metaphysics, scholastic cosmology, scholastic psychology, scholastic natural theology, and scholastic moral philosophy. The exposition is both cursory and controversial. Experimental psychologists, we are told, "limit themselves to measuring on the skin of the forehead the degree of fatigue produced by an intellectual work." Their harmless preoccupation with one anothers' foreheads enables metaphysical psychology to enjoy an independent existence. Metaphysical psychology then spends its time in drawing a distinction between Phantasm and Idea, by the help of which it hopes to settle the question of abstract ideas; in tabulating under six heads the characteristics which separate sense from intellect, and in explaining that the soul, "properly speaking, is not in the body at all; it simply acts upon the body, and touches it as a piece of bad news touches our heart." This is a mode of contact unknown to experimental psychology; and while the formula *Tota in toto et tota in aliqua parte* "thus understood" may "lose all its material flavor," it certainly does not "step forth as a flash of genius, as a profound truth which commands our admiration and our assent."

The causes which led to the downfall of Scholastic philosophy in the fifteenth century were of two kinds, external and internal. Among the most important of the external causes the author reckons the humanistic movement, the progress of science, the rise of Protestantism, and the invention of printing. The chief internal causes were the substitution of vain subtlety for profound reasoning, the undue elevation of the argument from authority, and active opposition to the teachings of science. The reasons for the modern revival of Thomism we have given above.

The second part of the work contains an historical sketch of this revival in Italy, in Spain, Portugal and Spanish America, in Germany and Austria, in France, in Belgium, in other European countries (Hungary, Bohemia, the Netherlands, England), and in the United States and Canada. This part of the work seems to be competently written and is of distinct value. It concludes with a bibliography of the Neo-Scholastic philosophy, which comprises 13 periodical publications and over 2,000 books and articles. S. POST.

Is Immortality Desirable? By G. LOWES DICKENSON. The Ingersoll Lecture, 1908. Boston and New York, Houghton Mifflin Company. 1999. pp. ii., 63. Price 75c.

While recognizing that there are many indifferentists and pessimists, the lecturer believes that in the matter of personal immortality most normal persons are optimists. He sums up his own feelings on the question as follows: "The conception that death ends all does not empty life of its worth, but it destroys, in my judgment, its most precious element, that which transfigures all the rest; it obliterates the gleam on the snow, the planet in the east; it shuts off the great adventure, the adventure beyond death." The lecture ends with a plea on behalf of the enquiries undertaken by the Society for Psychical Research.

F. JONES.

Race Questions, Provincialism, and Other American Problems. By JOSIAH ROYCE. New York, The Macmillan Co. 1908. pp. xiii., 287. Price $1.25.

In this little volume Professor Royce has brought together five addresses, delivered at various times and dealing with various subjects, but all illustrating that general doctrine about life which he has set forth in *The Philosophy of Loyalty*. The book may, then, be regarded as "an auxiliary to its more systematic predecessor."

The first address, on *Race Questions and Prejudices*, was read before the Chicago Ethical Society in 1905, and afterwards published in the *International Journal of Ethics*. It is an effort to express and to justify, in the special case of the race problems, the spirit elsewhere defined by the author as 'loyalty to loyalty'. Professor Royce begins with an appeal to the concrete, with the mention of two instances which bear upon the meaning of race prejudices: the lesson of human energy and devotion recently taught us by Japan, and the lesson to be learned, in a more restricted field, from the success of English administration and English reticence in Jamaica and Trinidad. Passing then to a wider consideration, he discusses race and the tests of race, and comes to the conclusion that "there is hardly any one thing that our actual knowledge of the human mind enables us to assert, with any scientific exactness, regarding the permanent, the hereditary, the unchangeable mental characteristics which distinguish even the most widely sundered physical varieties of mankind." We do not scientifically know what the true racial varieties of mental type really are." What then are our race-problems? They "are merely the problems caused by our antipathies." Antipathies are elemental and momentous, because we are by heredity doomed to be interested in all facts which may prove to be socially important. But we may not sanctify our illusions by the name of science.

The second address, on *Provincialism*, was read as a Phi Beta Kappa address at the State University of Iowa in 1902. A 'province' is defined as "any one part of a national domain, which is, geographically and socially, sufficiently unified to have a true consciousness of its own unity, to feel a pride in its own ideals and customs, and to possess a sense of its distinction from other parts of the country." And 'provincialism' means, "first, the tendency of such a province to possess its own customs and ideals; secondly, the totality of these customs and ideals themselves: and thirdly, the love and pride which leads the inhabitants of a province to cherish as their own these traditions, beliefs and aspirations." Provincialism, as thus understood, serves to correct three evils in the American world: the evil due to the presence of a considerable number of not yet assimilated newcomers in most of our communities; the evil due to excess of imitation, itself an aspect of the constant tendency of modern life to the mutual assimilation of various parts of the social order; and in particular, the evil arising from the rule of the mob-spirit. These evils may, by the help of provincialism, be met in four ways. The province should be, to all of us, an ideal rather than a boast. Provincialism should mean, again, a determination to use the spiritual gifts that come to us from abroad in our own way and with reference to the ideals of our own social order. It should mean, further, the determination to find, if possible, a place for our youth in their own communities. And, finally, the province should more and more seek its own adornment.

The third address, first delivered at Vassar College, is entitled *On Certain Limitations of the Thoughtful Public in America.* Professor Royce points out that 'idealism', taken in a broad and non-technical sense, is in fact more characteristic of modern America than is commercialism: witness, in particular, the whole of the recent educational movement. But it is also true that this idealism is, in the main, ineffective. "The great limitation of our thoughtful public in America remains its inability to take sufficient control of affairs." Reform must come from within, and the appeal must lie to the individual. The author's advice is: "overcome your limitations, first, by minute and faithful study of a few things and by clearness of ideas about them; then by childlike simplicity in the rest of life, by faithfulness to enlightened leaders, by resignation as opposed to restlessness, and above all by work rather than by idle curiosity. Organize through a willingness to recognize that we must often differ in insight, but that what we need is to *do* something together. Avoid this restless longing for mere novelty. Learn to wait, to believe in more than you see, and to love not what is old or new, but what is eternal."

The fourth address, on *The Pacific Coast*, further described as a psychological study of the relations of climate and civilization, was prepared for the National Geographical Society in 1898. As the essay on provincialism discussed, in general terms, the need and uses of that spirit in American life, so this more special paper sketches the bases upon which rests that particular form of provincialism to which the author, as a native Californian, personally owes most. After a survey of topography and climate, he sums up the correlated mental traits as individuality, but of a particular type, very different from the individuality, for instance, of the New England farmer; and a tendency, despite this individualism, toward agricultural conservatism and a definite social organization. The most representative expression of the spirit of California, of the "tension between individualism and loyalty, between shrewd conservatism and bold radicalism", is to be found, perhaps, in the poems of E. R. Sill.

The concluding address, read before the Boston Physical Education Association, is entitled *Some Relations of Physical Training to the Present Problem of Moral Education iu America.* The rational solution of the moral problem rests on the principle: Be loyal. "This principle, rightly understood, involves two consequences. The first is this: Have a cause, choose a cause, give yourself over to that cause actively, devotedly, whole-heartedly, practically." The second is: Be loyal to loyalty; that is, regard your neighbor's loyalty as something sacred. "Justice, kindliness, chivalry, charity,—these are all of them forms of loyalty to loyalty." Now systematic training of the physical organism may assist moral education in three ways. Skilful and serious physical exercise involves true devotion; physical training, in so far as it is a part of the life of a social group, can more directly aid the individual to learn to be loyal to his group; and physical training, in so far as it can be used to give expression to the spirit of fair play, may be an aid toward the highest types of morality, namely to those which embody the spirit of loyalty to loyalty. "There is," however, "nothing that fatally secures the attainment of any of these three results. All depends upon the spirit, the skill, and the opportunities of the teacher, and upon the awakening of the right spirit in the learners."

J. RILEY.

Zur Grundlegung der Logik; ein Beitrag zur Bestimmung des Verhältnisses zwischen Logik und Psychologie. Von Dr. STEPHEN MATICEVIC. Wien und Leipzig, W. Braumüller, 1909. pp. iii., 192. Price Mk. 3.

This work, which is a revised and enlarged edition of a doctor's dissertation presented to the Vienna faculty in 1906, seeks, as its title implies, to determine the relation between psychology and logic. The writer makes his position clear in the preface. The controversy between psychologism and anti-psychologism has been regarded by some, he says, as a mere continuation of the age-long quarrel of empiricism and rationalism, and has accordingly been passed by in silence and apathy. Others, impressed by Husserl's campaign against psychologism, have accepted, along with that author's critical results, his positive advocacy of a scholastic 'pure logic.' Dr. Maticevic himself aims to prove that one may be anti-psychologistic without thereby becoming a 'pure' logician.

The Introduction is followed by three chapters, on the basis (Begründung) of logic at large, on logic as transcendental psychology, and on empirical psychologism. Limits of space forbid more than a brief summary of the Conclusion.

We must take as datum the empirical antithesis of subject and object. We must recognize also that, wherever a theory of reality is in question, the objective is prior and superior to the subjective. Now logic, the most general, formal knowledge of the nature of things, is objectively determined, has the objective as its basis; and it is this objective character that distinguishes logic from psychology, and stamps it as an objective natural science of the same kind as geometry or arithmetic. The customary interpretation of the laws of formal logic as merely analytical formulas elevates the act of thought at the expense of the content of thought, and so transforms logical laws into psychological. In truth, the objective element cannot be eliminated from any form of knowledge that is to be valid for the objectively real, and the laws of logic are, despite their poverty of content, synthetic propositions that express certain properties of objects.

How are these contentions to be established? By the proof, brought in the body of the work, that all the psychologies, transcendental or other, however strenuously they endeavor to expel the object, trip over it in their doctrine of the *Ichbewusstsein* or the *Bewusstsein überhaupt*. It is true that, so far as the synthetic nature of the laws of formal logic is concerned, this proof is negative only; the formulas are shown to be more than tautological, but the character of their content has not been particnlarized. It may be suggested, in detail, that the objective content of the law of identity must be constituted by the empirically real identity (constancy) of things themselves; the proposition '$A=A$' is best formulated in the terms 'A remains A', which is an existential proposition, and therefore a synthetic judgment. The laws of contradiction and of excluded middle are simply the obverse and specification of the law of identity, and need no further explanation. It is possible that the underlying objective, in all three cases, is to be found in space-perception; but this question requires a separate discussion. Finally, the principle of sufficient reason, in its general form, simply expresses the requirement that a logical derivation of some sort be possible; its logical content depends, therefore, in part upon this general character, in part upon the special problem of the moment; recourse may be had, with greatest prospect of success, to the principle of subsumption. In a word, then, "unser ganzes Wissen um die Natur der Dinge," logical knowledge included, "ist bei den Dingen selbst zu suchen." F. JONES.

Therapeutics of the Circulation. By Sir LAUDER BRUNTON, F. R. S., etc. Published under the Auspices of the University of London. With 240 illustrations. Philadelphia, P. Blakiston's Son & Co. 1908. pp. xi., 280. Price $1.50.

This book consists of eight lectures, which were delivered in January, February and March, 1905, in the Physiological Laboratory of the University of London. The distinguished author has set forth, with great originality and with unusual wealth of experimental detail, though (as he himself admits) not in strict systematic order, the physiology, pathology, pharmacology and treatment of the circulation and its disorders. He devotes special attention to such subjects as the self-massage of the heart and vessels, and the conduction of stimuli to the heart, which are dealt with only briefly, if at all, in ordinary text-books.

The numerous illustration are in general clear and well-printed; a few, of which the cut of Mosso's ergograph (Fig. 166, p. 137) is a conspicuous example, are printed from worn platés and should have been redrawn. Most of the apparatus figured is familiar to experimental psychologists, and some of it exists in more accurate form than is here shown.

A series of appendices by the author deals with certain functions of protoplasm, with recent instruments for measuring the blood-pressure in man, and with exercise in angina pectoris. A final appendix contains a number of notes by Professor Hugo Kronecker (to whom the lectures are dedicated) relating to his own work and that of his pupils on the heart.

The volume is admirably indexed. Besides an analytical subject-index of 34 pages, there are a special index of 4 pages to the appendices, a classified list of illustrations according to subjects, and a list of illustration in numerical order. TH. WALTERS.

The People at Play. By ROLLIN LYNDE HARTT. Boston and New York, Houghton Mifflin Company, 1909. pp. ix., 317. Price, $1.50.

This is, as the publishers' announcement declares, "a volume of delightful reading," and it is delightfully illustrated by the author, in a frankly amateur fashion. Whether the book is to be considered, in the words of the same announcement, as "a contribution to sociology" depends upon one's ideas of that rather elusive discipline. Mr. Hartt himself prefers ethics. "As regards the spirit we take with us, it is that of comparative ethics. In the seventeen years the author has kouwn the people at play, he has learned, he trusts, to discern their worth. They differ from you, good reader, less in character than in intelligence. No one will deny that some measure of evil attends their amusements, but are our own invariably without stain? Before reforming the lowly, let us reform ourselves. And before berating their shortcomings, let us inquire whether the charitable attitude is not, on the whole, more scientific as well as more just. It has been the author's purpose to maintain throughout these pages a fairness and a sympathetic considerateness that may perchance lead his readers toward a more genial regard for their humble fellow creatures." The purpose has been very successfully attained in a series of chapters on the Home of Burlesque, the Amusement Park, the Dime Museum, the World in Motion, Melodrama, Society, the Muses in the Back Street, and the National Game.

As a sample of the writer's genial philosophizing, we quote the conclusion of the chapter on burlesque. "Beyond question, it fosters hope. Next week a new army of mendacious posters will lure the same silly fellows back to the same silly booby-trap. . . Also it fosters temperance and honesty; temperance because, since the Folly does n't sell drinks, . . . it becomes a citadel of refuge for inebriates, who can't go out between the acts, as no entr'-acts are provided; honesty because it pens up a herd of sneak-thieves and pickpockets for two hours and a half at a stretch. Moreover, it elevates industry and even prevents loss of life. What, think you, would happen to the trades, were those clowns and men-singers allowed a hand in them? What to the art of cookery, were the "40 La Belle Parisiennes 40" restored to the kitchen? . . But I sometimes suspect the institution affords more profit to the world outside it than to the world within. It enables the belligerently ethical to dog it with hired detectives (whereby they obtain much growth in grace), and it grants the sociological prowler a most fruitful opportunity for eavesdropping at the Underworld's confessional." W. JENKINS.

The Fragments of Empedocles. Translated into English Verse by WILLIAM ELLERY LEONARD, Ph. D. Chicago, Open Court Publishing Co. 1908. pp. viii., 92.

An introduction, treating of Empedocles the man, the philosopher, and the poet, is followed by a bibliography, and this again by the collected fragments ('On Nature' and 'The Purifications'), given both in the original Greek and in verse translation (unrhymed iambic pentameter), after which comes a final section of Notes. The translator has had the critical assistance of Professor Newbold and of Professor McGilvary. Many of his renderings are very happy. In some cases, however, as in the simile of the water-clock preserved in Aristotle's tract On Respiration, a poetical version is out of the question, and should not be attempted.

The volume is attractively printed. We have noticed no errors, except that on page 81 'cyclops' is used as a plural. W. JENKINS.

BOOK NOTES

A Text-Book of Psychology, by EDWARD BRADFORD TITCHENER. Macmillan, New York, 1909. 311 p.

This work, the author tells us, was written to take the place of his "Outlines of Psychology," which was stereotyped in 1896 and could no longer be revised. It is necessarily larger than the earlier work, although in general it follows its lines. Little space is given to nerve physiology, which it is held the physiologists can better teach, the psychologist needing all the time for his own problems. Statements of the physiological conceptions are necessary, though at present largely hypothetical, but beginners are not ripe for discussion of unsettled points. The book is well printed, with good indexes, forty-four illustrations, and the reputation of the author makes it needless to say that it is sure of a commanding position in its field.

Letters, Lectures and Addresses of Charles Edward Garman. A Memorial Volume. Prepared with the co-operation of the class of 1884, Amherst College, by ELIZA MINER GARMAN. Houghton, Mifflin Co., Boston, 1909, 616 p.

This volume is dedicated by the widow of Professor Garman to "the students with whom my husband loved to work and to the friends whose sympathy and appreciation he so highly prized." It is, we fancy, about the kind of memorial, the subject of it would have preferred. The first part contains philosophical papers, most of which had been unpublished before, on such topics as automatism, Hume on the limits of knowledge, Kant on dating and locating, Science and theism, the Universe and God, the Will and the sentiments, Pleasure or righteousness, Expediency as a working principle, Authority and punishment, Members of the state, the Right of property, Scientific ideals and social practice, the Coming reform, the Training of a boy, Recreation, a Plea for philosophy in the pulpit, Sunday in the mountains, in Memory of President Seelye, Mary Lyon, the Mount of temptation, etc. Some of these are letters and others addresses to classes. The third part contains a collection of letters to the various classes which Professor Garman taught, appreciations, his letters to individuals and testimonials from his students.

The Case of Eusapia Palladino, by J. COURTIER. Bulletin de l'Institut Général Psychologique, 1908, Nov.-Dec., Nos. 5-6. Au Siège de la Société, Paris, 1908. pp. 415-578.

This comprises a study of Eusapia Palladino by J. Courtier during the years 1905-08, together with discussions and observations upon the subject during the above three years. First the phenomena of raps, levitations, cabinets, transition of objects, and luminous phenomena are described. The subject herself is then described, with a brief biography, and various experiments involving the use of scientific apparatus are reported. The investigators believe they have proven displacement of objects in Eusapia's vicinity, sometimes without contact. The subject seems to discharge electroscopes at a distance and produce molecular vibrations. The luminous phenomena they cannot explain. She often entered into a veritable second state and had cutaneous hyperæsthesia for some time afterward, claiming partial amnesia during the séance.

Éssai sur les Principes de la Métrique Anglaise, by PAUL VERRIER. H. Welter, Paris, 1909. 3 v.

The first volume discusses prosody, including sounds, accents, syllables, intonation, pauses; the second, rhythm, including feet, verse, its beginning and end, homophony, the strophe; the third, metrics, including its simple variations, combinations, traditional forms, technical terms, signs, etc. The perception of rhythm is next considered, beginning with measures of space and time generally, the functions of optical illusions, functional rhythms, modification of cells, tension, defects of rhythm. Under æsthetics of rhythm the author discusses the rhythms of nature, utility, art, and the origin and evolution of poetic metres, together with the emotional value of the forms of rhythm, homophony, etc.

A Theory of the Genetic Basis of Appeal in Literature, by HOMER CLYDE HOUSE. A dissertation presented to the Faculty of the University of Nebraska in partial fulfilment of the requirements for the degree of Doctor of Philosophy. 77 p.

This is an interesting genetic study. It is chiefly devoted to natural phenomena in literature including trees and woods, heavenly bodies, fire, frost, the sea, rivers, mountains, animal life, each of which is thought to be potent in the inspirations of literature. The author then proceeds to consider personality, the emotional life of nerves, dominant themes in literature, viz., love, death, contest, mystery, travel, etc., and the sources of their appeal.

Allgemeine Geschichte der Philosophie, von Wilh. Wundt, Herm. Oldenberg, and others. Teubner, Berlin, 1909. 572 p.

This is the fifth division of the first part of the series of volumes on the Culture of the Present, edited by Hinneberg. Wundt treats of the beginnings of philosophy and the philosophy of primitive people under the rubrics logic, psychology, philosophy of nature, ethics, with retrospect and prospect of the coming philosophy. Oldenberg deals with Indic philosophy, Goldziher with that of Islam and Judea, Grube with Chinese, Inouye with Japanese, von Arnim deals with the earliest European philosophy, especially that of Greece, Baeumker with the European philosophy of the middle ages, and Windelband with modern philosophy to Schopenhauer, but with thirty pages of contemporary French, English and German philosophy.

Le Régime des Aliénés, par FERNAND DUBIEF. Rousset, Paris, 1909. 350 p.

In the first part the author treats criminal insanity, drunkenness, the half idiotic and half responsible, the death penalty for such, and vagabonds. In the second part, devoted to treatment, he deals with arbitrary sequestration, medical certificates and the property of the insane. The third part is devoted to hospitals and deals with quarters for observation, patients cared for by their families, medical principles involved. In the appendix, texts adopted by the Chamber of Deputies, and the St. Gall law concerning habitual drunkards, etc., are treated.

Die Mnemischen Empfindungen, von RICHARD SEMON. Engelmann, Leipzig, 1909. 392 p.

We have here a very important work which the author deems the skeleton of a new psychology. The first part is devoted to original sensations, the synchronous phase, the idea of sensory fields, homophony and sensory differentials, the acholathuic of original sensations, the mnemic sensations, the single engram and its complications, the store of engrams won in the individual life, eckphoria and the various forms under which association appears, the eckphorious valuation of components, the irreversibility of mnemic sequences, how mnemic sensations differ from original ones, their proportionate variability, the repetition of stimuli as creating all pre-conditions for mnemic homophony, manifestations of undifferentiated homophony and abstractions by its means, their differentiation and modality, the engramic activity of homophonic components, rivalry of original and mnemic sensations, with a few final conclusions.

L'Année Psychologique. Publiée par Alfred Binet. Quinzième Année. Masson et Cie, Paris, 1909. 496 p.

This volume in interest seems fully to equal that of preceding numbers and has nearly a dozen original memoirs, the longest of which is on the intelligence of imbeciles. Others are on the memory of insects, by Plateau, an analysis of dreams, by Jung, new clinical theories concerning dementia, taste, psychology of painting, etc. These occupy 358 pages, the rest being devoted to reviews.

Die Seele des Menschen, von JOHANNES REHMKE. 3d rev. ed. Teubner, Leipzig, 1909. 132 p.

This little handbook describes the soul as changeable, as will and especially as consciousness, objective, perceptive, judging, thinking, causal, etc. It is not illustrated, the print is fine, the standpoint rather idealistic.

Rééducation Physique et Psychique, par H. LAVRAND. Bloud et Cie, Paris, 1909. 121 p.

This little handbook, after a general introduction, describes psychic and motor re-educations, the latter as seen in ataxia, paralysis, tics, language troubles, deaf-muteness, together with re-education of hearing, respiration, circulation, the organic system, etc.

The Inaccuracy of Movement, by H. L. HOLLINGWORTH. Archives of Psychology, No. 13, June, 1909. The Science Press, New York, 1909. 37 p.

The author first discusses the methods of studying movements, extent, time and force, the illusions produced by impact, indifferent points, relations between extent and duration, memory for extent and duration, influence of the degree of contraction, criteria of the judgment of extent.

A Quantitative Study of Rhythm, by HERBERT WOODROW. Archives of Psychology, No. 14, June, 1909. The Science Press, New York, 1909. 66 p.

This is a Columbia contribution, the contents of which are as follows: historical, apparatus and procedure, intensity, rate and intensity, duration, the meaning of rhythmical groupings, summary.

Der Begriff des Ideals, von ABRAHAM SCHLESINGER. Engelmann, Leipzig, 1909. 228 p.

Schlesinger here continues his analysis of ideas, treating first the systematic psychological representation of typically composed ideal theories. The second section is devoted to a valuation of the same.

Mental Medicine. Some Practical suggestions from a spiritual standpoint, by OLIVER HUCKEL. Thomas Y. Crowell & Co., New York, 1909. 219 p.

After an introduction by L. F. Backer the first conference is on mental and spiritual factors in the problem of health, the second on the theoretical value of faith and prayer, third on the possibilities in the control of the unconscious, the fourth, some elements of morbid moods, fifth, higher faculties in the re-education of the nerves, especially relaxation and work.

De l'Illusion — son mécanisme psycho-social, par LE PRESTIDIGITATEUR ALBER. Bloud et Cie, Paris, 1909. 118 p.

This handbook has four chapters entitled respectively illusions, illusionists, the spectators, and the experiences.

The Influence of Emotional States on the Functions of the Alimentary Canal, by W. B. CANNON. From the American Journal of the Medical Sciences, April, 1909. 8 p.

The Collection of Rosaries in the United States National Museum, by IMMANUEL M. CASANOWICZ. Government Printing Office, Washington, D. C., 1909. (Reprint from the Proceedings of the United States National Museum, Vol. XXXVI, pp. 333-360.)

Zur Geschichte und Theories des Telegrammargumentes in der Lehre von der psychophysischen Wechselsirkung, von ALOYS MÜLLER. (Sonderabdruck aus Zeitschrift für Psychologie, Bd. 49. J. A. Barth, Leipzig, 1908. pp. 440-446.)

Ueber psychophysische Wechselwirkung und das Energieprinzip, von ALOYS MÜLLER. (Sonderabdruck aus Zeitschrift für Psychologie, Bd. 47. J. A. Barth, Leipzig, 1907. pp. 115-140.)

Ueber die Möglichkeit einer durch psychische kräfte bewirkten Änderung der Energieverteilung in einem geschlossenen System, von ALOYS MÜLLER. Fritz Eckardt, Leipzig, n. d. (Sonderabdruck aus Zeitschrift für Philosophie und philosophische Kritik, Bd. 134, pp. 151-165.)

Two Extensions in the Use of Graphs in Elementary Logic, by WILLIAM ERNEST HOCKING. The University Press, Berkeley, Cal., 1909. (University of California Publications in Philosophy, Vol. 2, No. 2, pp. 31-44.)

SUBJECT INDEX

NAMES OF AUTHORS

(The names of those who have contributed original matter are printed in SMALL CAPITALS.)

6