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THE PSYCHOLOGICAL REVIEW

THE ABSOLUTE LIMITS OF COLOR SENSITIVITY AND THE EFFECT OF INTENSITY OF LIGHT ON THE APPARENT LIMITS

BY C. E. FERREE AND GERTRUDE RAND

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INTRODUCTION

In describing a general plan of investigating the chromatic sensitivity of the peripheral retina in an earlier paper (1) the following were mentioned as two of the problems which we wished to take up: (*a*) a point to point determination of comparative sensitivities to the different colors from the center to the periphery, and (*b*) an investigation of the limits of sensitivity. The former of these problems has been made the subject of a recent paper (2). The latter will be treated of here.

The investigation of the limits of sensitivity may be considered from two points of view. As indicated in the first of the papers referred to above, (*a*) it may be made a part of the investigation of the comparative sensitivities of the peripheral retina to the different colors; and (*b*) it may be considered more specifically in relation to points of theory. In the former case the limits should be obtained with stimuli equalized in energy. The results will then represent positions on the retina at which the stimuli for one of the intensities which it is possible to employ have the same or nearly the same threshold value.[1] In the latter case the problem con-

[1] Strictly speaking the threshold value may be considerably less at this point than the intensity of the stimulus employed, because the stimulus may be increased much above the threshold value in the far periphery of the retina without changing the limits by a detectable amount. That is, the stimulus value of the just noticeable

sidered in relation to its historical development divides into two,—a determination of the relative or apparent limits and a determination of the absolute limits. In the second of the papers referred to above it was shown that the determination of the apparent limits was given an undue importance in relation to theory by Hess and his followers because of their failure to realize that great irregularity and not uniformity characterizes the decrease of sensitivity from the center to the periphery of the retina. The details of that demonstration need not be repeated here. The determination of the absolute limits of sensitivity, however, does sustain an important relation to theory, especially to theories of the paired process type; for if it be found that sensation can be aroused farther out from the center of the retina for one of the paired colors than for the other, that fact must tell against the theory unless some supplementary concept is provided to explain the discrepancy. For one thing we have undertaken, therefore, to determine the limits of sensitivity with stimuli any further increase in the intensity of which tends to decrease rather than to increase the chromatic component of the response. For another we have determined the effect of a given range of variation of intensities on the apparent limits. Our reason in part for doing this was to supplement at higher intensities the work of former papers (3) in which we called attention to the large variations that are required in any of the factors influencing the chromatic response (intensity being the most effective of these) to change the limits of sensitivity as much as 1 degree especially when a certain degree of excentricity has been reached, pointing out in particular with regard to the work of previous writers, (*a*) the importance of taking into account deviations of 1–3 degrees from coincidence of limits when conclusions with regard to comparative sensitivities are to be drawn from the results, and (*b*) the futility of making a brightness equalization of the stimuli, with its attendant disadvantages,

difference in limits is much greater than the stimulus value of the difference limen of intensity. In other words, a given point in the peripheral retina may be considered the limit for a range of stimulus intensities, varying in magnitude with its degree of excentricity.

for the determination of the limits with lights of medium and high intensities and perhaps for any but intensities so low as to give very narrow limits. For the latter point three reasons may be given. (1) The brightness equation does not equalize the stimuli in power to arouse the chromatic response, the only subjective equation, so far as we can see, that could rightly be given a place in a determination of the limits of chromatic sensitivity, and this only in a determination of whether or not the same ratio of sensitivity holds for the limits as for the center or other point at which the equation was made. (2) It does not equate them in intensity (the equation is merely of the very selective achromatic response to the stimuli). And (3) so far as the effect of the achromatic on the chromatic component of the excitation is concerned (the final variable that might be considered), it has already been shown by one of the writers in a previous paper (4) that there is not enough of this effect for the colors ordinarily used to change the limits by a detectable amount. The particular bearing of the present work on this question is to give a clearer and more definite idea of just how much difference in intensity or equivalent influence is required to change the limits by detectable amounts in the mid and far periphery of the retina. In the beginning this was in fact our chief incentive to undertake the work.

The Problem

The investigation was given the following form. (1) An attempt was made to find out whether by means of our spectroscopic apparatus, which was designed especially to give high intensities of light, stimuli could be obtained which could be sensed as color to the limits of white light vision. (2) The effect on the extension of the limits of sensitivity of varying the stimuli through quite a wide range of intensities was investigated. And (3) the determination of the limits was made in 16 meridians with all of the lights made equal in energy to the blue of the prismatic spectrum employed and with 1/32 of this amount of energy.

Conditions under which the work was done

The conditions under which the work was done fall under five headings: (1) the wave-lengths of light employed and the means used of getting greater purity of light than is found in the prismatic spectrum; (2) the energy content of the stimuli used and the method of measurement; (3) the control of the brightness of preëxposure and surrounding field; (4) the control of the general illumination of the optics room; and (5) the method of rendering the amount of light entering the eye independent of variations in the size of the pupil, without the use of an artificial pupil. These conditions are so nearly identical with those used in the work of the immediately preceding papers that at the request of the Editor space has not been taken for their repetition in the present paper. For a description of the conditions the reader is referred to 'Chromatic Thresholds of Sensation and their Bearing on Color Theory, Part I.,' this journal, 1919, **26**, pp. 18–25.

The stimulus used was the circular aperture of the campimeter, 15 mm. in diameter, filled with light by the focusing lens. At a distance of 25 cm. from the pupil of the eye, on which the light from the objective slit of the spectroscope was focussed, this aperture subtended a visual angle of 3° 26′. The time of exposure was 1 sec. and the interval between exposures varied between 3–5 min. depending on circumstances and the need for precautionary measures. If the stimulus was sensed in its proper color at any time during the 1 sec. interval of exposure, the retina was called color sensitive at that point. (At the limits of white light vision the red stimulus, for example, of the intensity used was sensed as a tint of red.) The field in the 16 meridians was always mapped for one color before the work on another color was begun.

Systematic results were obtained for all of the points of the work for only one observer. This was the observer whose results were published in the immediately preceding papers: 'Chromatic Thresholds, etc., Parts I. and II.' For data with regard to the various ways in which the normality of both the chromatic and achromatic sensitivity of this observer, central retina, and chromatic sensitivity, peripheral retina, has been confirmed, the reader is referred to pp. 26–32 of the first of the papers noted above. Data on additional points, important in a general specification of the ocular condition of the observer, have also been published in various places: *e.g.*, on the dioptric or refraction condition and power to sustain acuity in *Trans. Illum. Eng. Soc.*, 1915, **10**, p. 1128, and in other papers by us on lighting in relation to the eye; on muscle strength, muscle balance, muscle lag, photopic acuity, near point, range of accommodation, and refraction condition (more recent), *Trans. Amer. Ophthal. Soc.*, 1918, **66**, pp. 142–163; and on scotopic acuity and amount and rapidity of scotopic adaptation, *Trans. Amer. Ophthal. Soc.*, 1919, **67** (in press). The more important points such as the coincidence of the limits of red, yellow and blue with the limits of white light vision; the narrower limits for green; the interlacing of limits for stimuli of medium intensity of equal energy, or of the same general order of intensity; and the large differences in amount of light required to change the limits of sensitivity by a detectable amount in the mid and far peripheral portions of the retina, have been confirmed in a less detailed and systematic way by one or more check observers.

Results

The following results were obtained. (1) It was quite easy to obtain an intensity of light for the red, yellow and blue wave-lengths that could be sensed to the limits of white light vision. In fact these wave-lengths in the spectrum employed were considerably above the threshold at the limits of white light vision in the sixteen meridians investigated. The limits of the green of this spectrum, however, fell far short of the limits for white light; nor could the zone of sensitivity be widened as much as 1 degree by increasing the current in the Nernst filament from 0.6 to 0.8 ampere. The energy entering the eye from the spectrum of the Nernst filament operated by 0.6 ampere of current with the width of collimator slit employed was for the red 9096.639×10^{-10}

watt; for the yellow, 4065.624×10^{-10} watt; for the green, 1562.388×10^{-10} watt; and for the blue, 882.025×10^{-10} watt. The energy value of the threshold at the limits of white light vision in the nasal meridian, for example, was for the red 277.836×10^{-10} watt; for the yellow, 268.95×10^{-10} watt; and for the blue, 264.368×10^{-10} watt. The intensity of light for these colors in the 0.6 ampere spectrum was, therefore, strongly supra-liminal at the limits of white light vision, as is stated above. In the 0.6 ampere spectrum, the energy of the green light, it will be noted, was greater than the energy of the blue, but less than the energy of the red and yellow. It was, however, nearly six times as great as the threshold value of these colors at the limits of white light vision. Moreover, when the current was raised to 0.8 ampere this value was considerably increased, but there was still no detectable extension of the limits. Since then the sensitivity to green at the center of the retina and for several degrees towards the periphery is approximately the same as to blue and considerably greater than to red and to yellow, and since so large an increase in the energy value of the stimulus made no detectable difference in the limits and any further increase lessened rather than increased the chromatic component of the response, it seems highly improbable that the limits could by any means whatsoever be extended the 20–35 degrees needed to make them coextensive with the limits of white light vision. It seems fairly certain, therefore, that while the far periphery of the retina is only deficient in its chromatic sensitivity to red, yellow and blue, the blindness to green for the observers used is absolute.

(2) In the investigation of the effect of changes of intensity on the limits of sensitivity eight intensities were used, sustaining to each other the following relations: 1, 1/2, 1/4, 1/8, 1/16, 1/32, 1/64 and 1/128. The highest intensities were taken respectively from the prismatic spectrum of a Nernst filament operated by 0.6 ampere of current and from a spectrum made equal in energy to the blue of this spectrum. These spectra will be designated as Spectrum

A and Spectrum B. The reductions were produced by means of an aluminum sectored disc of 180, 90, 45, etc., degrees open sector. The energy values of the different intensities of light, as has already been stated, were obtained by radiometering the highest intensities and computing the lower from the simple law of the disc. It had been our intention to make the investigation systematically with the eight different intensities in the sixteen meridians of the retina. However, for the purpose of the present paper a briefer substitute plan has been adopted. A preliminary investigation was made with the eight intensities of Spectrum A in two meridians of the retina, the nasal and the temporal, which meridians represent opposite extremes with regard to breadth of zone of sensitivity, in order to get some idea of the amounts of reduction that would be needed to be effective in changing the limits. It was found, for example, that a reduction of the red light to 1/32 of its value at intensity A was not sufficient to narrow the limits in the nasal and temporal meridians, the meridians designated in the tables and charts as 90 degrees. At this value the stimulus was still slightly supra-liminal in these meridians at the limits of white light vision. This amount of reduction, however, was sufficient to narrow the limits for the other stimuli by quite considerable amounts. Also a further investigation showed that it was enough to narrow the limits for red in 12 out of the 16 meridians employed. It was decided, therefore, to make the final determinations in the 16 meridians with the full intensities A and B and with 1/32 A and B. The amount of narrowing for the yellow of the prismatic spectrum in the different meridians produced by this reduction ranged from 3–11 degrees; for the green from 5–17 degrees; for the blue, from 10–18 degrees; and for the red, from 0–8 degrees. For the equal energy spectrum the amount of narrowing for the yellow ranged from 5–18 degrees; for the green, from 5–15 degrees; for the blue, from 4–18 degrees; and for the red from 3–25 degrees.

(3) In case of the equal energy spectrum of the higher intensity, all of the lights with the exception of the green

were seen in their proper color to the limits of white light vision in each of the 16 meridians. Made equal in energy to the blue of the prismatic spectrum (882.025×10^{-10} watt) the red and yellow were considerably less in energy value than was the green of the prismatic spectrum, still the red and yellow were sensed to the limits of white light vision while the green which represented a considerably greater amount of energy fell short of those limits by amounts varying from 20–35 degrees in the different meridians. There can be no reasonable doubt, we believe, that the difference found here represents an actual difference in sensitivity. It obviously can not be attributed to the relative intensities of the stimuli employed.

Landolt has also investigated the effect of high intensities on the extension of the limits of sensitivity. Writing of this work (5), he says: "In ein absolut dunkles Zimmer fiel nur durch eine kleine Öffnung im Finsterladen directes Sonnenlicht. Dieses wurde auf das äusserste Ende des Perimeterbogens gelenkt. Während wir unser Auge ins Centrum des Bogens setzen, bracht man in die kleine, intensive beleuchtete Stelle farbige Papiere von möglichster Intensität der Färbung. Nun bewegtet sich das Auge langsam vom entgegengesetzen Ende des Bogens nach Scheitelpunkte zu und es zeigte sich dabei, dass wenigstens mit der innern Netzhautpartie alle Farben schon bei 90° erkannt wurden. Die Grösse des Objectes betrug weniger als 1 cm^2.

"Als dieselben Prüfungen auch mit Spectralfarben zu machen, entwarfen wir ein Sonnenspectrum im sonst dunkeln Zimmer und liessen es durch eine achromatische Linse auf einen Ende des Perimeters befindlichen Schirm fallen. Dieser hatte eine verändliche Spalte, mittelst welcher man die einzelnen Farben aus dem Spectrum isolieren konnte. Während wir nun wiederum nach langer Adaptation, und bei verbundenem zweiten Auge das eine Ende des Bogens fixierten, würde von einem Assistenten irgendeine Farbe des Spectrums auf die Spalte gelenkt, und wir drehten nun, unter stehter Fixation unserer Fingerspitze, welche sich auf dem Bogen bewegte, das Auge allmählig der Farbe entgegen.

Es zeigte sich auch hier wiederum dass alle Farbe schon bei 90° erkannt werden, wenn sie intensiv genug sind."

Landolt's investigation was made, it will be noted, in a dark room while ours was made in a light room. We have

Table I

A. The Effect of Intensity of Stimulus on the Limits of Sensitivity, Prismatic Spectrum

In this table are given the results of a preliminary investigation in two representative meridians to show how much reduction is needed to produce a significant change in the limits of sensitivity. Eight intensities of stimulus were used: A, 1/2A, 1/4A, 1/8A, etc.

Meridian Investigated	Stimulus	Limits of Sensitivity for							
		Intensity A	Intensity 1/2A	Intensity 1/4A	Intensity 1/8A	Intensity 1/16A	Intensity 1/32A	Intensity 1/64A	Intensity 1/128A
Nasal........	Red (670 μμ)	92	92	92	92	92	92	88	86
	Yellow (581 μμ)	92	92	92	92	91	89	88	88
	Green (522 μμ)	69	69	69	66	63	62	59	56
	Blue (468 μμ)	92	92	87	83	79	78	77	76
Temporal.....	Red (670 μμ)	61	61	61	61	61	61	46	44
	Yellow (581 μμ)	61	61	61	61	55	47	46	45
	Green (522 μμ)	45	45	45	42	34.5	32.5	30	29
	Blue (468 μμ)	61	61	56	45	43.5	43	43	43

B. The Energy Values of the Stimuli Used

Total energy of light at campimeter opening and at eye

(watt $\times 10^{-10}$)

Intensity	Red (670μμ)	Yellow (581μμ)	Green (522μμ)	Blue (468μμ)
A[1]	9096.639	4065.624	1562.388	882.025

[1] The energy values of 1/2, 1/4, 1/8, 1/16, 1/32, 1/64 and 1/128 A may be obtained by dividing the above values by the appropriate factor.

The energy density at the campimeter opening (watt per sq. mm.) may be obtained by multiplying the above values by 0.005659; the energy density at the eye, by multiplying them by 0.303.

not as yet had opportunity to repeat the work of the present paper with the dark adapted eye. However, determinations somewhat rougher and less detailed than those described here have sufficed to show that for our observers the far periphery of the retina is color-blind to green also with the dark adapted eye. With reference to the relative insensitivity of the peripheral retina to green, it may further be noted that in our results with the Hering papers with a different set of observers the limits for green fell much nearer to the center of the retina than for red, yellow and blue. The results represented in Fig. 5, for example, were taken from this series of observations. That the limits for green are narrower than for red, yellow and blue with stimuli of the same order of intensity has, moreover, been verified many times in the work of our undergraduate laboratory.

In Table I., A, are given the results of the preliminary investigation in the nasal and temporal meridians to find out whether an intensity of light may not be gotten sufficiently high to make the limits of color sensitivity coincide with the limits of white light vision, and once this intensity is attained how much reduction is needed to produce a significant narrowing of the limits. We have already indicated in this and in previous papers the large changes of intensity that are needed to change the limits by a significant amount when a certain degree of excentricity has been reached. How very large these changes have to be for the far periphery of the retina is shown in this table.

In Table I., B, is given a specification of the energy values of the stimuli used in making the determinations represented in Table I., A. Four energy values may perhaps be considered of importance for each determination: The total value at the campimeter opening, the density per sq. mm. at the campimeter opening, the total energy entering the eye, and the density per sq. mm. at the eye. For the sake of brevity, however, only one of these values is given in the table, namely, the total energy entering the eye; and the factors needed to convert this value into density at the eye and at the campimeter opening are appended in a footnote. Since all of the light from the campimeter opening is focused into the image on the pupil, the figures expressing the total energy at the eye and at the campimeter opening are the same.

The most important of the four specifications noted are probably the total amount of light entering the eye and the density at the campimeter opening. The latter value, for example, sustains a fixed but unknown ratio to the density of the image formed on the retina.

Table II

The Brightness Values of Preëxposure and Surrounding Field

In this table are given the brightness values of preëxposure and campimeter screen in candlepower per square inch[1] for the determination of limits given in Table I. In all cases in which it was possible the brightness of the preëxposure and campimeter screen was made equal to that of the stimulus at the limits of sensitivity.

Meridian	Stimulus	Brightness Value of Preëxposure and Campimeter Screen for							
		Intensity A	Intensity ½A	Intensity ¼A	Intensity ⅛A	Intensity $\frac{1}{16}$A	Intensity $\frac{1}{32}$A	Intensity $\frac{1}{64}$A	Intensity $\frac{1}{128}$A
Nasal....	Red (670 μμ)	0.05088	0.05088	0.05088	0.05088	0.05088	0.05088	0.03093	0.02116
	Yellow (581 μμ)	0.05088	0.05088	0.05088	0.05088	0.05088	0.05088	0.03663	0.02686
	Green (522 μμ)	0.05088	0.05088	0.05088	0.03663	0.03663	0.02686	0.02116	0.01384
	Blue (468 μμ)	0.05088	0.05088	0.03663	0.02686	0.01262	0.01140	0.00643	0.00578
Temporal	Red (670 μμ)	0.05088	0.05088	0.05088	0.05088	0.05088	0.05088	0.03093	0.01791
	Yellow (581 μμ)	0.05088	0.05088	0.05088	0.05088	0.05088	0.05088	0.02686	0.02116
	Green (522 μμ)	0.05088	0.05088	0.05088	0.03663	0.02686	0.01384	0.01140	0.01140
	Blue (468 μμ)	0.05088	0.05088	0.03663	0.03093	0.01262	0.01140	0.00716	0.00643

In Table II. are given the brightness values of the preexposure and campimeter screen for the work represented in Table I. As stated earlier in the paper the preëxposure and the campimeter screen were selected from the Hering series of standard papers. In case of the higher intensities of light used, No. I of this series (the standard white, coefficient of reflection about 75 per cent.) reflecting the light of the room was not as bright as the stimulus light. These cases may be identified in this and the following tables by the brightness value of this paper under the illumination of the room, namely, 0.05088 cp. per sq. in.

[1] The above values may be converted into millilamberts by multiplying by 486.8.

In Table III. are given the limits of sensitivity in 16 meridians of the retina for the highest intensity of light used for the work of Table I., intensity A, and for 1/32 A, an intensity representing the order of reduction needed for all of the colors to produce any considerable narrowing of the

Table III

The Effect of Intensity of Stimulus on the Limits of Sensitivity, Prismatic Spectrum

In this table are given the limits of sensitivity in 16 meridians of the retina for intensity A and 1/32A of Table II. The upper vertical meridian is numbered 0° and the lower vertical 180°. Reading down to right or left they are 25°, 45°, 70°, 90°, 110°, 135°, 155°, and 180°.

Meridian Investigated		Limits of Sensitivity for							
		Intensity A				Intensity 1/32 A			
		Red (670μμ)	Yellow (581μμ)	Green (522μμ)	Blue (468μμ)	Red (670μμ)	Yellow (581μμ)	Green (522μμ)	Blue (468μμ)
	0°	65	65	36	65	64	57	29	50
Nasal	25°	86	86	49	86	83	79	36	72
	45°	90	90	52	90	89	83	38	76
	70°	92	92	67	92	92	86	59	77
	90°	92	92	69	92	92	89	62	78
	110°	91	91	68	91	90	87	59	78
	135°	88	88	61	88	87	84	49	75
	155°	86	86	47	86	84	78	35	67
	180°	57	57	36	57	53	47	27	42
Temporal	25°	65	65	44	65	64	55	31	52
	45°	65	65	50	65	57	56	33	51
	70°	62	62	46	62	55	50	35	44
	90°	61	61	45	61	61	47	32.5	43
	110°	58	58	37	58	57	49	31	48
	135°	55	55	32	55	55	48	27	20
	155°	54	54	30	64	50	43	25	41

limits at the extreme periphery of the retina. The 16 meridians used are designated as follows. The upper vertical meridian is marked 0 and the lower vertical 180 degrees. Beginning with 0 and reading down to left or right they are 0, 25, 45, 75, 90, 110, 135 and 180 degrees. The specification of the energy of the stimuli at intensity A and 1/32 A is given, it will be noted, in Table I., B. For all of the stimuli at intensity A, No. 1 of the Hering series of papers was used as preëxposure and campimeter screen, as has already been noted. This paper illuminated by the light of the room was too dark (0.05088 cp. per sq. in.) for all of the four colors at the limits of sensitivity. However, for the want of a suitable pigment surface of higher reflection coefficient it was used. For intensity 1/32 A, it was darker than the yellow stimulus

Table IV

A. The Effect of Intensity of Stimulus on the Limits of Sensitivity, Equal Energy Spectrum

In this table are given the limits of sensitivity in 16 meridians of the retina for stimuli all made approximately equal in energy to the blue of the prismatic spectrum used in Table I., and for 1/32 of this intensity,—intensity B and 1/32 B.

Meridian Investigated		Limits of Sensitivity for							
		Intensity B				Intensity 1/32 B			
		Red (670μμ)	Yellow (581μμ)	Green (522μμ)	Blue (468μμ)	Red (670μμ)	Yellow (581μμ)	Green (522μμ)	Blue (468μμ)
	0°	65	65	34	65	62	56	29	50
Nasal	25°	86	86	47	86	74	70	34	72
	45°	90	90	50	90	74	78	36	76
	70°	92	92	63	92	78	85	50	77
	90°	92	92	69	92	81	87	59	78
	110°	91	91	64	91	79	85	58	78
	135°	88	88	44	88	74	80	42	75
	155°	86	86	36	86	61	74	32	67
	180°	57	57	31	57	46	39	25	42
Temporal	25°	65	65	39	65	57	54	30	52
	45°	65	65	39	65	47	55	32	51
	70°	62	62	42	62	46	48	30	44
	90°	61	61	41	61	40	45	30	43
	110°	58	58	36	58	45	45	29	48
	135°	55	55	28	55	49	41	25	40
	155°	54	54	26	54	47	38	24	41

B. The Energy Values of the Stimuli Used.

Total energy of light at campimeter and at eye (watt $\times 10^{-10}$)

Intensity	Red (670 μμ)	Yellow (581 μμ)	Green (522 μμ)	Blue (468 μμ)
B[1]	891.050	882.510	884.946	882.025

and approximately equal to the green and red. For the blue stimulus Nos. 9–14 of the Hering series (0.01404–0.0114 cp. per sq. in.) were used as needed in the different meridians. The photometric values for these intensities and intensity

[1] The energy value of 1/32 B may be obtained by dividing the above values by the appropriate factor.

The energy density at the campimeter opening (watt per sq. mm.) may be obtained by multiplying the above values by 0.005659; the energy density at the eye, by multiplying them by 0.303.

B and 1/32 B have not been given in detailed tabular form because of the large number of repetitions that occur.

In Table IV., A, are given the limits of sensitivity in 16 meridians of the retina for the four stimuli all made equal in energy to the blue used in Table I. and for 1/32 of this value. These values are, as we have already indicated, designated in

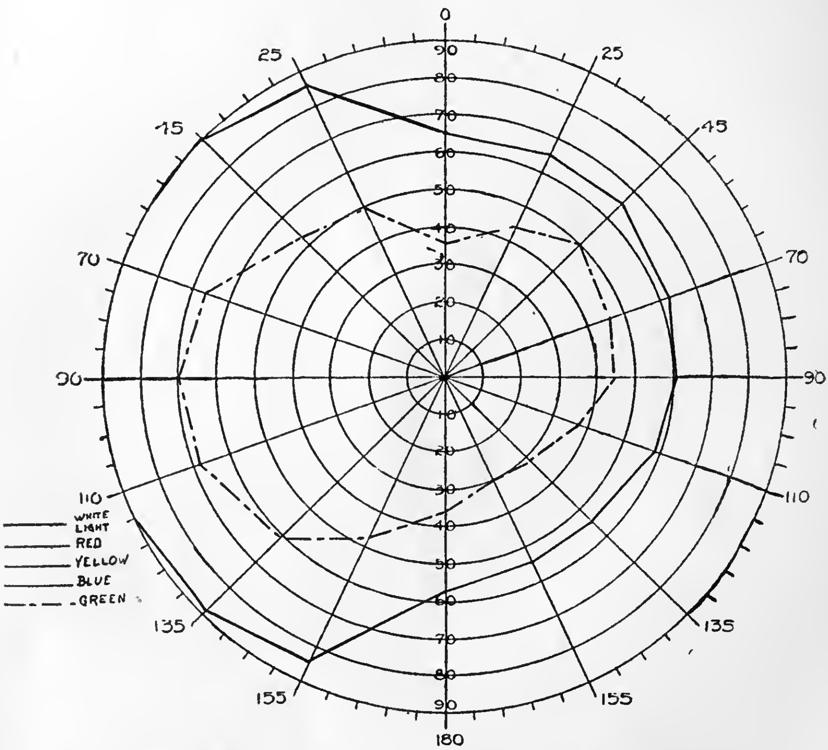

FIG. 1. The effect of intensity of stimulus on the limits of sensitivity, prismatic spectrum. In this chart are represented the limits of sensitivity for intensity A of Table I.: red 9096.639, yellow 4065.624, green 1562.388, and blue 882.025 watt × 10^{-10}.

the table as intensity B and 1/32 B. In Table IV., B, are given the energy values of the stimuli used for Table IV., A. For the higher intensity of these stimuli, intensity B, No. 1 of the Hering series of papers (0.05088 cp. per sq. in.) was used for the preëxposure and campimeter screen. Again it was darker than all of the four colors at the limits of sensitivity. For intensity 1/32 B it was slightly darker than the yellow.

For the green of this intensity the no. 2 gray of this series was used (0.0366 cp. per sq. in.); for the red nos. 10–14 (0.01384–0.0114 cp. per sq. in.) varying for the different meridians; and for the blue, nos. 7–14 (0.01791–0.0114 cp. per sq. in.).

A graphic representation of the results of Table III. is given in Figs. 1 and 2. In Fig. 1 are shown the limits of

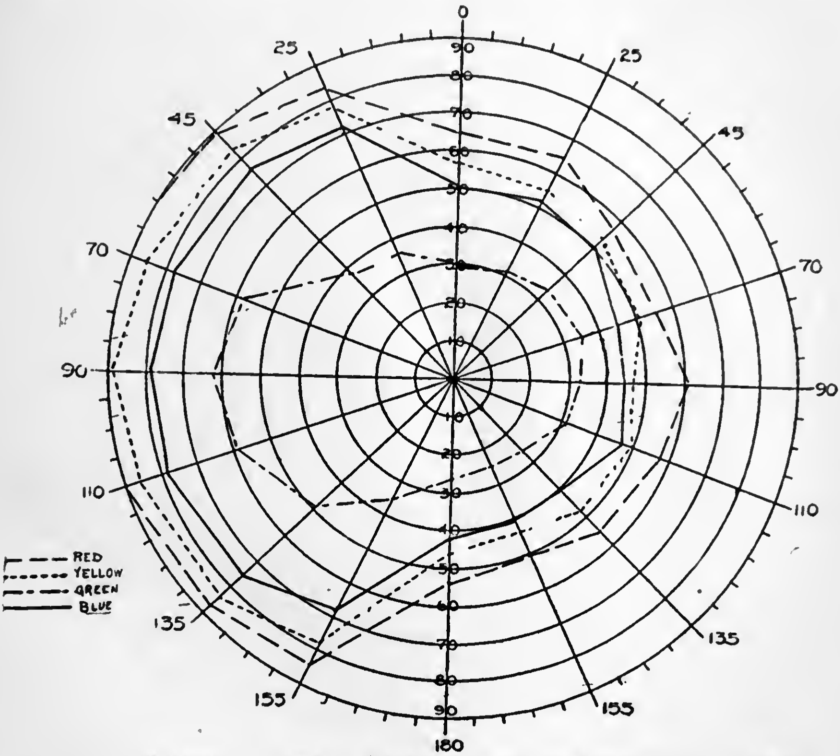

FIG. 2. The effect of intensity of stimulus on the limits of sensitivity, prismatic spectrum. In this chart are represented the limits of sensitivity for intensity 1/32 A of Table I.: red 284.27, yellow 127.051, green 48.825, and blue 27.563 watt $\times 10^{-10}$

sensitivity to the four stimuli in the 16 meridians for the intensities represented in the prismatic spectrum *A*. The limits for the red, yellow and blue stimuli at this intensity are, it will be remembered, coincident with the limits of white light vision. All four limits may be represented, therefore, by a single tracing, an unbroken line in black. The limits for green are represented by a broken line. In Fig. 2 are represented the limits of sensitivity for the four stimuli at the intensities represented in the prismatic spectrum, 1/32 A. In this case the zone of sensitivity to blue is outlined by an unbroken line and the zones for the other colors by broken lines as indicated in the figure. An inspection of this figure will show that the degree of excentricity of the limits is in the order of the intensity of the stimuli. In discussing the

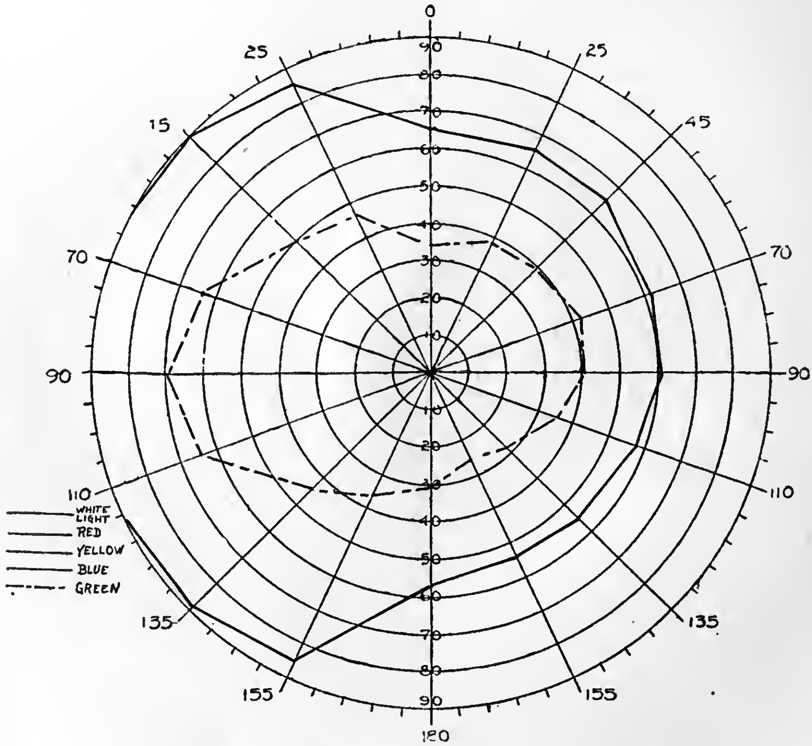

FIG. 3. The effect of intensity of stimulus on the limits of sensitivity, equal energy spectrum. In this chart are represented the limits of sensitivity for intensity B of Table IV.: red 891.05, yellow 882.51, green 884.946 and blue 882.025 watt $\times 10^{-10}$.

significance of the crisscrossing or interlacing of the limits obtained with the Hering pigment papers in previous work, this is what was pointed out would occur if there were a significant difference in the intensity of the stimuli. That is, if the zone of sensitivity to red, for example, is in one meridian wider and in another narrower than to green, etc., it can not be due to any difference in the intensity of the stimuli; for such a difference, if significant, would make one zone consistently wider or narrower than the other in all meridians.

A graphic representation of the results of Table IV. is given in Figs. 3 and 4. In Fig. 3 are shown the limits of sensitivity to the four stimuli in the 16 meridians for the intensities represented in the equal energy spectrum *B*.

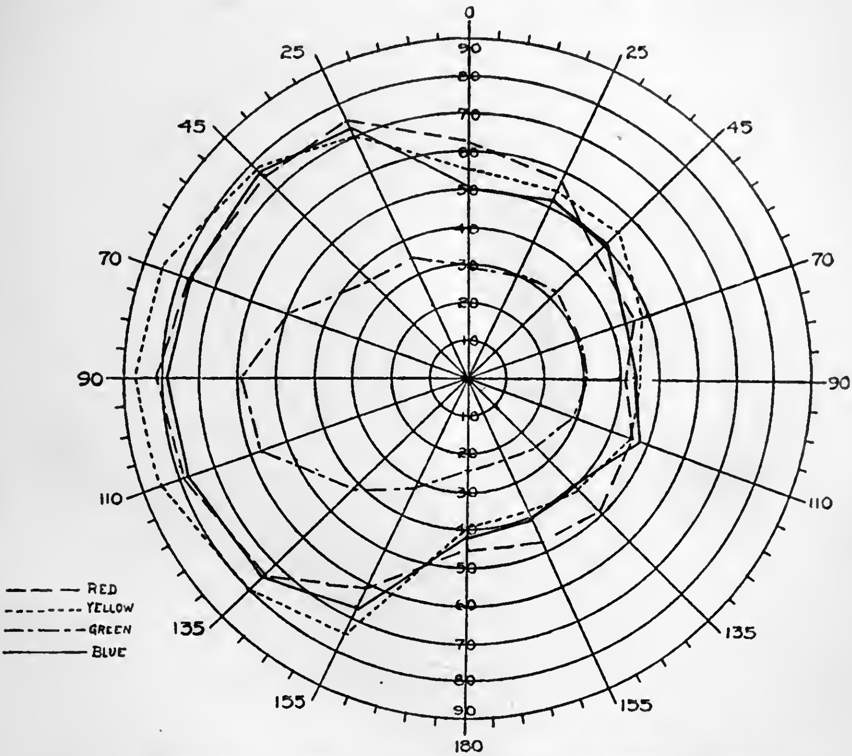

FIG. 4. The effect of intensity of stimulus on the limits of sensitivity, equal energy spectrum. In this table are represented the limits of sensitivity for intensity 1/32 B of Table IV.: red 27.845, yellow 27.578, green 27.655, and blue 27.563 watt $\times 10^{-10}$.

Again the limits for the red, yellow and blue stimuli coincide with the limits of white light vision and are represented by a single tracing, the unbroken line in black. The limits for green are represented by a broken line. In Fig. 4 are shown the limits for the four stimuli at the intensities represented in the equal energy spectrum 1/32 B. With regard to this figure the following points may be noted. (1) With stimuli of equal energy the limits of no one of the colors, red, yellow and blue, are consistently wider than the others. That is, their limits are characterized by frequent crisscrossing or interlacing. The limits for all three colors, however, are consistently wider than for green. And (2) Fig. 4 sustains a

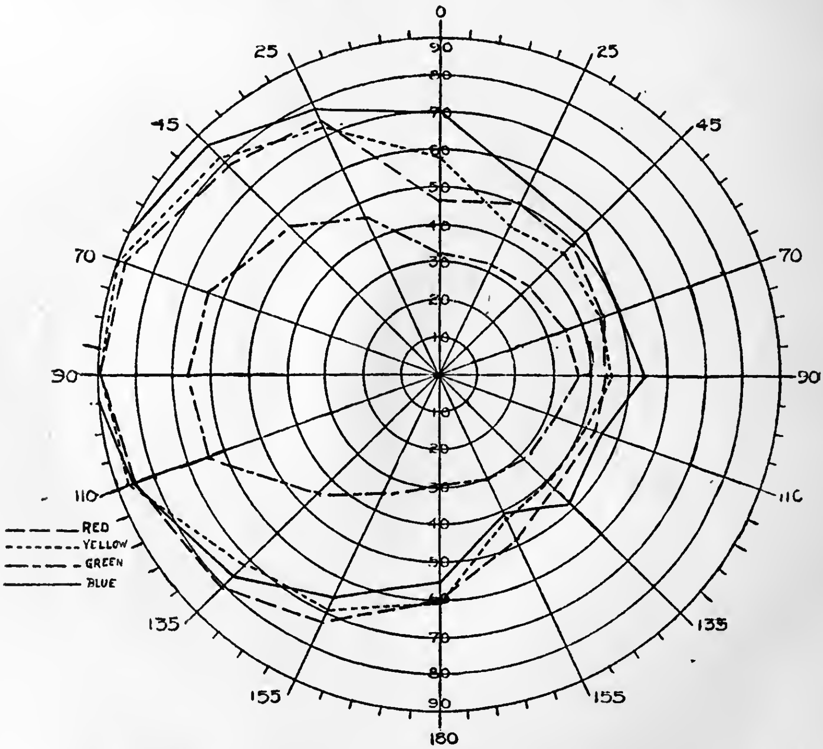

Fig. 5. The limits of sensitivity to red, yellow, green and blue of the Hering series of pigment papers, intensity of illumination, vertical component, 390 foot-candles.

somewhat striking general similarity to the charts obtained for the Hering pigment papers. One of these showing the limits with a surrounding field and preëxposure of the brightness of the colors employed is given in Fig. 5. While no conclusion can be drawn from this similarity with regard to the relative energies of the wave-lengths dominantly reflected by these papers; still it suggests that they may all, roughly speaking, be somewhere near the same order of value, at least much more nearly so than are these colors in the prismatic spectrum. The red, yellow, green and blue of the prismatic spectrum gave, it will be remembered, rather widely concentric, not crisscrossing limits.

In this general connection it may be of interest also to note the close correlation which obtains between the results of this investigation and those of the previous investigation of the sensitivity of the peripheral retina by the threshold method. That is, wherever the thresholds are found to be low the limits are found to be wide, and conversely wherever the thresholds are high the limits are found to be correspondingly narrow. Some interesting results follow from this. For example, in a given meridian the threshold curve for a given color is found to be very irregular, rising in some places slowly, in others quickly, and still in others dropping and rising again. These fluctuations in the curve are, moreover, different in the different meridians. This means, of course, that the shape of the zones of sensitivity for this color should change with the intensity of the stimulus employed, which is found to be the case. Furthermore, in the same meridian the threshold curves for the different colors differ from each other widely in the direction and amount of the irregularity: and this difference in turn varies from meridian to meridian. The result of this is that a crisscrossing or interlacing of limits must take place whenever stimuli of such relative intensities are used that the limits are of the same general order of excentricity. In other words, as was pointed out in our discussion of this phenomenon in earlier papers, crisscrossing can mean only that there is a lack of uniformity in the relative sensitivity to the different colors in the different meridians. For example, when it occurs in the limits for blue and yellow, it indicates that the ratio of sensitivity to blue and yellow changes in passing from meridian to meridian. In short any investigation at all comprehensive either of the thresholds or limits of sensitivity shows that striking irregularity and not uniformity characterizes the distribution of chromatic sensitivity in the peripheral retina. This is in direct opposition, it will be remembered, to the claim made by Hess (6) that constancy of ratio of sensitivity to the paired colors prevails throughout the retina which claim, it will be remembered, was advanced by Hering (7) in support of his own theory and in refutation of Fick's (8) and Leber's (9) modifications of the Helmholtz theory to explain the color blindness of the peripheral retina. So far as we are able to determine no one intensity or set of conditions will give coincidence of limits in all meridians for any two colors inside the limits of white light vision.

In conclusion it may not be out of place to point out the bearing of these results on the work of the clinic. In the practice of perimetry as applied to diagnosis it is commonly accepted that the field of vision for the normal eye may be divided concentrically from periphery to center in the following order: white light and form, blue, red and green. It is obvious from the fore-going results (*a*) with stimuli taken from the prismatic and equal energy spectra and (*b*) from the effects obtained by varying the intensity of the stimuli that the responsibility for such a rating of the color fields rests for the greater part with the relative intensities of the pigment stimuli used in the work of the clinic. That is, the limits of sensitivity to red, yellow, blue and white light for stimuli of high intensities are coincident; for stimuli of lower intensities taken from the prismatic spectrum they are rather widely concentric; and for stimuli of equal energies of the order of intensity of 27.563×10^{-10} watt they are interlacing.

Another feature of interest is the claim that has been made by certain clinicians, but not generally accepted, we believe, that the interlacing of the limits for blue and red indicates a pathological disturbance in the relative distribution of sensitivities. While we are not disposed to dispute this conclusion because of a too meager knowledge of all of the data that should be taken into consideration in its evaluation, still we do think it fair to note that pathological disturbances are only one set of factors that may contribute to such a result and that widely different results may be gotten with the same eye with no greater differences in the test conditions than may occur from time to time in the same clinic or laboratory unless a clear understanding is had of the factors which affect the apparent powers of response of the peripheral retina and adequate means are exercised for their control. These factors are, so far as we are able to list them, composition, area, intensity of the stimulus and duration of the stimulation, breadth of pupil, the intensity of the general illumination and the state of adaptation of the retina, and the brightness of the preëxposure and surrounding field. Obviously if the determination of the apparent limits is to be given clinical significance the work should be done under conditions of work which have been most carefully standardized, for the apparent limits are a resultant of these conditions as well as of the actual distribution of sensitivities.

The degree of importance that is attributed by at least one clinician to the absolute and relative distribution of sensitivities over the retina may be indicated by the following quotation from a recent work on perimetry. "Contraction of the form fields shows the degree of disease of the visual tract. It is better evidence of the real condition of the visual path than an ophthalmoscopic study can possibly furnish. The evidence is minute and analytical. The color fields and color changes moreover furnish a more delicate test in the early stages of the disease and at times furnish a clue to the seat of the trouble before an appreciable change has taken place in the form field" (**10**).

Summary of Results and Conclusions

The more significant features of the above results may be summarized briefly as follows:

1. The far periphery of the retina is not blind to red, blue and yellow. It is merely deficient in sensitivity to these colors. That is, with stimuli of sufficient intensity the limits of red, blue and yellow coincide with the limits of white light vision. The blindness to green, however, is for our observers absolute.

2. The amount of change of intensity required to produce a detectable change in the apparent limits of sensitivity in the more remote parts of the retina is very great. This

amount changes very irregularly from center to periphery of the retina in a given meridian and from meridian to meridian as might be expected from the great irregularity in the distribution of sensitivity in the peripheral retina. (Cf. 'Chromatic Thresholds of Sensitivity from Center to Periphery of the Retina and their Bearing on Color Theory,' Psychol. Rev., 1919, **26**, pp. 16–42.)

3. Two other important phenomena may also be mentioned as a result of this irregularity. (*a*) The shape of the zone of sensitivity to a given color changes with the intensity of the stimulus employed in making the determination. And (*b*) when stimuli of equal or of the same order of intensity are used the limits for red, yellow and blue are found to interlace or crisscross each other irregularly rather than to coincide in complementary pairs as was reported by Hegg, Hess and Baird in a more limited investigation of the retina's powers of response. The former phenomenon is the direct corollary of the difference in the rate of decrease of sensitivity to a given color in passing from the center to the periphery of the retina in the different meridians; the latter, to the change in the ratio of sensitivity to the different colors from meridian to meridian. The lack of uniformity of grading of function from point to point in the periphery of the retina, reported in this and previous papers, while striking, can scarcely be considered as surprising. It is in fact just what might be expected of those parts of a sense organ which are little used and poorly developed.

4. The responsibility of the accepted clinic rating of limits in the order from widest to narrowest of blue, red and green doubtless for the greater part rests with the relative intensities of the pigment stimuli used in the work of the clinic. With stimuli of high intensity the limits for red, yellow and blue coincide with the limits of white light vision; for stimuli of lower intensities, taken from the prismatic spectrum, they are rather widely concentric; and for stimuli of equal energies of medium intensities they are interlacing.

5. The interlacing of limits for red and blue is a normal result for stimuli of equal energy of medium intensities. It

may not therefore be due to pathological disturbances in the distribution of sensitivities as has been claimed by certain clinicians. In all responsible work on the determination of the apparent limits it is obviously of great importance to bear in mind that the results are dependent both upon the actual distribution of sensitivity and the numerous factors which affect the apparent powers of response of the peripheral retina.

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SOME FACTORS IN THE PERCEPTION OF RELATIVE MOTION. A PRELIMINARY EXPERIMENT

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The perception of motion is relative. The observer perceives the motion of one object in relation to some stationary object or set of conditions. In the perception of motion an appreciation of the stability of the one object is just as essential and important as the appreciation of the motility of the moving object. The observer himself constitutes the center of reference in most perceptual acts.

The perception of motion involves two aspects, an appreciation of the fact of motion, and an appreciation as to which of the two objects is stationary and which is in motion. These two aspects are to some extent independent variables. One's appreciation of them may be based upon entirely different sets of conditions. One may correctly perceive the fact of motion, but judge erroneously as to which of the two is moving. If a fixated visual object is moved toward an observer, there results a change in the sensory conditions which mediate his judgment of distance. But exactly the same changes will be induced by a movement of the observer toward the object. Changes of these distance criteria as intensity, size of retinal image, accommodation and convergence, etc., serve to induce in both cases an appreciation of a change of the distance between the object and the observer,—an appreciation of the fact of motion. But these sensory changes give no clue as to which object moved and which was stationary. Judgments of the relative motility of the object and the observer depend upon other factors, presumably certain characteristics of either one or both of the two objects concerned.

When the organism constitutes one of the objects in the perceptual situation, the judgments concerning the motility-stability relation are based in part upon certain sensory aspects of the observer. The nature of these organic stimuli has been pretty well determined. The judgments of relative motion are based upon the presence or absence of such factors as the intention or expectation of moving, the sight or feeling of muscular activity, friction, air currents, the static sense, organic sensitivity, etc. As a rule these factors mediate correct judgments, but exceptions occur as in the illusions of the haunted swing, moving trains, etc. But these organic factors do not constitute the whole of the conditions which influence the judgments of relative motion. These judgments are also based in part upon certain characteristics of the observed object. The haunted swing illusion is probably due in part to the fact that one does not, on the basis of past experience, expect buildings to undergo rotary motion. Likewise the perceptual situation is frequently confined to two observed objects when all spatial relation to the perceiving organism is pretty well excluded. The cloud and moon illusion illustrates such a perceptual situation. It is extremely improbable that this illusion is due primarily or mainly to any erroneous judgment as to the spatial relation of the observer to either of the objects. Rather it seems that the judgment of relative motion in this case depends upon certain peculiarities of the objective situation.

The existence and nature of these objective factors have never been adequately considered. It was the purpose of this experiment to attempt a preliminary investigation concerning the possible influence of certain features of the objective situation. The experiment presented a condition somewhat similar to the cloud and moon illusion. The observer was seated in a dark room and was required to judge as to the relative motion of two small lights whose intensities were such that no other objects were visible. By this procedure it was hoped that the judgments might be based mainly upon certain characteristics of the two lights rather than upon any perceptible relation between them and the observer.

Two 2 c.p. electric lights were each enclosed within a small wooden box containing a small circular diaphragm opening covered with ground glass. Each box was placed upon a horizontal double rod track. The vertical distance between the centers of the two lights was 72 mm. Between the two tracks was a piston which was attached to a metal disc, and this disc was rotated by a motor with a worm gear speed reducer. By an appropriate device either one or both of the light boxes could be easily and quickly attached to or disengaged from the moving piston without stopping the motor. The motion imparted to the lights was thus a vibratory or pendular one.

The observer was seated at a distance of 8 ft. from the lights with his line of vision at right angles to the direction of motion. A cloth curtain screened the lights from view while the apparatus was being adjusted and set in motion. The screen was then removed and the observer was requested to fixate a specified light during four complete vibrations. The lights were again covered while the report of the subject was recorded and the apparatus adjusted for the next exposure. Twenty such judgments constituted a day's test for each subject. The subjects had been informed that either one or both of the lights might move in an irregular temporal order. As a matter of fact but one light was moved at a time, and the two lights were moved an equal number of times in each day's test of 20 trials. The observer was requested to report the movements as perceived rather than to attempt to guess at the objective situation. The reports were scored as correct or illusory. The judgment was correct when the moving light was perceived in motion and the stationary light as stable. Illusory judgments were of two sorts: Both lights were perceived as moving in opposite directions. In this case the two movements might not be equal in rate or extent. In the second case the moving light was perceived as stationary and the stationary light as moving. The relative number of the two types of illusion was not a function of the variable factors studied and hence separate tabulations are unnecessary.

The five factors of the size and intensity of the lights, their extent and rate of motion, and the direction of fixation were chosen for study. The size of the lights was controlled by iris diaphragms. Three magnitudes were arbitrarily chosen, whose diameters were 4 mm., 15 mm., and 32 mm. These magnitudes will be termed *A*, *B*, and *C* respectively. The intensity of each light was controlled by a rheostat. Three intensities were chosen and these will be referred to as 1, 2, and 3 in their order of brightness. Intensity 1 was chosen as near the limen of visibility as possible without inducing discomfort in the observer. Intensity 3 was the maximum possible without illuminating the room to the point of visibility. Intensity 2 was approximately a mean between the other two. Since the lights were fed by a storage battery giving a constant current, it was possible to reproduce these intensities approximately by calibrating the rheostats. The extents of motion chosen were $^1/_2$ inch, 1 inch, and 2 inches. The amplitude of the movement was controlled by varying the point of attachment of the piston along the radius of the rotating disc. The rate of motion was controlled by varying its amplitude and by altering the speed of the motor by a rheostat. The rates chosen for investigation were $^1/_4$ inch, $^1/_2$ inch, 1 inch and 2 inches per second. The conditions were arranged so that the moving and the stationary lights were fixated an equal number of times. The upper light was fixated during one day's test of twenty trials and the lower light was then fixated on the succeeding day. Since the upper light moved in one half of the trials, fixation was distributed equally between the stationary and the moving lights. It was thus possible to determine which direction of fixation was the more conducive to correct perception.

Some illusions were obtained for practically all experimental conditions. The efficacy of any factor must thus be determined from the relative frequency with which correct judgments were obtained as that factor was varied. The percentage frequency of correct judgments was determined for one condition and this value was compared with that obtained for a second condition. For the individual records we utilized Yule's formula, standard deviation = $\sqrt{pq/n}$, where n is the number of judgments and p and q represent the percentages of correct and wrong responses. Unless otherwise stated, the number of judgments for each condition was 80. In general any difference of at least 12 between two percentage frequencies for an individual is significant. The averages of the individual records for the group are also given in the tables. A difference of 10 in these values indicates a high degree of probability.

1. *Effect of Size.*—The percentage frequencies for the variations of size are given in Table I. The first vertical column specifies the magnitudes employed. In the first condition the magnitude of both lights was *A*. In the second condition the size of the lower light was *A* and that of the upper light was *B*. In the third condition the magnitude of the upper light was changed to *C*. The horizontal rows of figures are the percentage values for the individuals and the average values for the group. Below are specified the various conditions which were kept constant as the magnitudes of the lights were altered. The intensity of both lights was 1, the amplitude of movement was 1 inch, the rate of motion was $^1/_2$ inch per second, and each of the two lights was moved and was fixated the same number of times.

TABLE I.

PERCENTAGES OF CORRECT PERCEPTIONS WITH VARIATIONS OF SIZE

Sizes	J.	K.	L.	M.	S.	Ave.
1. *A–A*...........	47	22	49	69	34	44
2. *A–B*...........	54	29	75	82	32	54
3. *A–C*...........	50	52	66	77	46	58

Constant conditions: Intensities, 1; Amplitude, 1 in.; Rate, ½ in. per sec.; Equal number of movements and fixations.

Conditions 2 and 3, as compared with 1, are evidently conducive to perceptual accuracy. With those conditions the individual percentage values are the larger in nine of the ten comparisons, and five of these are statistically significant. The average values are also the larger, the differences in both cases being significant.

Comparing conditions 2 and 3, two of the individual records favor the third condition and both of these are significant. The average values also indicate the greater efficacy of the third condition, but the difference between them is slight. The records do not permit of any very confident statements. Either we may say that the two conditions do not differ in efficacy, or that the third condition is the more effective with certain individuals.

The comparative data are ambiguous in one respect. It is impossible to determine from this experiment whether the number of correct perceptions is a function of relative or absolute size. On the one hand it is possible to assume that the perceptual accuracy is greater when the two objects are unequal in size than when they have the same magnitude, and that the accuracy of some individuals is proportional to the degree of this inequality. On the other hand it is equally valid to conclude that perceptual accuracy varies directly with the magnitude of the combined areas of the two objects.

2. *Influence of Intensity.*—Three intensity conditions were investigated. In the first the intensity of the lower light was 1, and that of the upper was 2. In the second condition the intensity of both lights was 2, while in the third condition the intensity of the upper light was changed to 3. The percentage frequencies of correct judgments for the three conditions are given in Table II.

TABLE II

PERCENTAGES OF CORRECT JUDGMENTS WITH VARIATIONS OF INTENSITY

Intensities	B.	C.	Ha.	Jo.	P.	Ave.
I. 2–1	52	35	81	54	61	57
II. 2–2	22	19	56	27	51	35
III. 2–3	31	27	56	44	50	41

Constant conditions: Magnitudes, 2; Amplitude, 1 inch; Rate ½ inch per sec.; Both lights fixated and moved an equal number of times.

Condition I. gives the maximum of perceptual accuracy. Its values are the largest for each individual and for the group as a whole, and of these ten individual comparisons six are significant.

Condition III. is slightly more effective than II. Three individuals secured the larger values for this condition and one of these comparisons is significant. The remaining two individuals of the group gave practically identical records for the two conditions.

Perceptual accuracy is thus a function of both relative and absolute intensity. On the one hand an inequality in the intensity of the two lights gives more correct judgments than does equality. When both lights are unequal in brightness, the greater number of correct responses are secured with the lower illumination. Of the two factors, the degree of illumination is possibly the more effective one.

3. *Amplitude of Motion.*—The percentages of correct judgments with variations of the amplitude of movement are given in Table III. Our apparatus permitted but two amplitudes for each rate of motion.

TABLE III

PERCENTAGES OF CORRECT JUDGMENTS WITH VARIATIONS OF AMPLITUDE

Amplitudes	Ba.	D.	G.	H.	Mi.	Ave.
1. When rate of motion is ½ inch per sec.						
½ inch	72	47	26	20	16	36
1 inch	70	42	24	31	15	36
2. When rate of motion is 1 inch per sec.						
1 inch	79	41	27	42	26	43
2 inch	94	49	27	11	22	41

Constant conditions: Intensities, 2; Sizes, *A*; Equal distribution of movements and fixations between the two lights.

The ability to perceive correctly does not appear to depend in any pronounced manner upon the amplitude of motion. Six of the ten individual comparisons indicate the greater efficiency of the smaller extents of motion and one of these is significant; three comparisons indicate that more correct perceptions are possible with the larger movements and one of these is significant; one comparison favors neither assumption. The results of but one of the five individuals (Mi.) consistently favor either assumption; both comparative values of this subject indicate the greater efficacy of the lesser amplitudes, but the differences in both comparisons are quite small. From these data one is not justified in asserting that perceptual accuracy is dependent upon the amplitude of motion so far as this factor was varied.

4. *Influence of Rate of Motion.*—Table IV. gives the percentages of correct judgments for variations of rate of motion. Our apparatus permitted of but two variations of speed for each amplitude of movement.

TABLE IV

PERCENTAGES OF CORRECT JUDGMENTS WITH VARIATIONS OF RATE OF MOTION

Rates of Motion	Ba.	D.	G.	H.	Mi.	Ave.
1. With amplitude of ½ inch						
¼ inch per sec.	57	45	2	15	20	27
½ inch per sec.	72	47	26	20	16	36
2. With amplitude of 1 inch						
½ inch per sec.	70	42	24	31	14	36
1 inch per sec.	79	41	27	42	26	43
3. With amplitude of 2 inches						
1 inch per sec.	94	49	27	11	22	41
2 inch per sec.	51	66	62	16	17	40

Constant conditions: Sizes, *A*; Intensities, 2; Equal distribution of movements and fixations between the two lights.

In each of the first two series the faster rate gives the greater number of correct judgments in four of the five individual comparisons; of the eight favorable comparisons four are significant. In the third series three of the individual comparisons favor the faster rate. Two individuals, G. and H., were invariably more accurate with the faster rates. Individuals Ba. and D. were more accurate with the faster rate in two of the three series. Mi. was more accurate with the faster rate in but one of the series.

The facts indicate that the rate of motion is effective at least with some individuals, and that in the majority of cases perceptual accuracy is greater for the faster rates of motion.

5. *Individual Differences in Perceptive Ability.*—In Table V. the individuals of each group are ranked according to their ability to perceive the objective situation correctly. Group 1 was composed of five subjects. These individuals were tested for three series in which the size of the lights was varied (See Table I.). These individuals were ranked according to the number of correct judgments. Subject M. secured the highest percentage in all three tests and is ranked first three times with a total score of 3. Subject L. stood second in every case with a total score of 6. From these data it is obvious that individuals differ in their ability to perceive the situation correctly. In group 2 the five subjects maintained the same relative ranking in all three tests. Some individuals are able to perceive a relative movement situation with a high degree of accuracy while other indivi-

TABLE V

RANKS OF INDIVIDUALS IN RESPECT TO NUMBER OF CORRECT JUDGMENTS

Group 1

	M.	L.	J.	S.	K.
	1	2	3	4	5
	1	2	3	4	5
	1	2	4	5	3
Total.......	3	6	10	13	13

Group 2

	Ha.	P.	Jo.	B.	C.
	1	2	3	4	5
	1	2	3	4	5
	1	2	3	4	5
Total.......	3	6	9	12	15

Group 3

	Ba.	D.	G.	H.	Mi.
	1	2	5	4	3
	1	2	3	4	5
	1	2	4	3	5
	1	3	4	2	5
	1	2	3	5	4
	3	1	2	5	4
Total.......	8	12	21	23	26

duals are more prone to perceptual illusions. It is possible that this experiment might constitute a good test for determining the relative suggestibility of different individuals.

6. *Influence of Direction of Fixation.*—In any series of 80 judgments, the moving light was fixated 40 times and the stationary light was fixated an equal number of times. The percentages of correct judgments for each kind of fixation were computed for each individual for all experimental conditions. These percentage values are given in Table VI.

TABLE VI

PERCENTAGES OF CORRECT JUDGMENTS WITH VARIATION OF FIXATION

Fixation	L.	M.	G.
Moving	50 70 60	65 90 72	25 27 27 27 2 45
Stationary	48 80 72	72 75 82	22 25 27 27 2 57
	Ha.	Jo.	Ba.
Moving	65 57 75	5 47 42	55 50 75 87 67 2
Stationary	47 56 87	50 40 65	85 95 82 100 47 100
	S.	B.	H.
Moving	5 15 40	7 12 35	20 5 12 5 5 5
Stationary	62 50 47	37 50 70	42 35 72 17 25 27
	J.	C.	Mi.
Moving	57 77 72	30 52 62	5 2 0 0 30 0
Stationary	38 30 27	7 2 7	22 30 52 45 10 35
	P.	K.	D.
Moving	42 45 35	15 27 42	32 45 17 30 30 35
Stationary	60 55 87	30 30 62	52 50 65 67 60 97

Subject L was tested for three experimental conditions. His percentages of correct perceptions when the moving light was fixated were 50, 70 and 60. These values are to be compared with the percentages of 48, 80 and 72 secured when the stationary light was observed. Each percentage value of the table is based upon 40 judgments. With this number of cases a probability that direction of fixation is an effective factor in the perception of relative motion will be indicated by any difference of 20 or more.

With four subjects, L., M., G., and Ha., the direction of fixation did not appreciably influence the number of correct perceptions. The comparative values do not consistently favor either mode of fixation, and none of the differences are large enough to be significant.

With two subjects, J. and C., fixation of the moving object is the more conducive to perceptual accuracy. This mode of fixation is favored in all comparisons and all of the differences are significant.

With the remaining nine individuals, the greater perceptual efficiency was attained by observing the stationary object. For these subjects 36 of the 39 comparisons favor this mode of fixation, and of these 36 comparative values 27 are significant.

Table VII

Percentages of Correct Judgments when each Light was Stationary

Subject	Stable Light	Experimental Condition						
		1	2	3	4	5	6	Ave.
Ba.	Upper	60	72	62	80	95	52	70
	Lower	80	85	82	35	92	50	71
D.	Upper	30	27	42	30	27	57	35
	Lower	55	55	52	60	70	75	61
G.	Upper	45	42	42	2	40	60	38
	Lower	2	12	10	2	15	42	14
H.	Upper	30	42	15	15	12	12	21
	Lower	32	42	25	15	10	20	24
Mi.	Upper	22	30	25	40	25	17	26
	Lower	5	22	7	0	20	17	12

Constant conditions: Lights equal in size and intensity; Equal number of fixations and movements.

7. *The Influence of Position.*—In Table VII. are listed the percentages of correct judgments when the upper light was stationary as compared with the percentages when the lower light was immobile. The upper and lower lights were equal in size and intensity for all six experimental conditions. The two lights were fixated an equal number of times. The data are given only for the third group of subjects, since the two objects were not always equal in size or intensity for groups 1 and 2. In the first experimental condition, Ba. correctly perceived 60 per cent. of the 40 cases in which the upper light was stationary and the lower moving. He perceived correctly 80 per cent. of the 40 cases in which the

lower light was stable and the upper one was moving. This comparison was instituted to determine whether either position tends to be associated with stability. A difference of 20 in the individual values and of 11 in the average values indicates a probability of the efficacy of the position factor.

Position did not exert any appreciably consistent effect with subjects Ba. and H.

Subject D. consistently perceived the situation more correctly when the lower light was stable and the upper in motion than in the reverse case. Five of the seven comparisons are statistically significant. With this individual the lower position is associated with immobility and the upper one with motility.

Subjects G. and Mi. exhibited more correct perceptions when the stationary light occupied the upper position. Both subjects were consistent in this preference with one exception in which the two percentage values were equal. These individuals tend to associate stability with the upper position.

8. *Summary.*—The subjects were required to judge concerning the relative motion of two lights, one of which was always stationary.

Perceptual accuracy was promoted either by a difference in size of the two lights or by an increase in their combined area. Possibly both conditions may have been operative.

Perceptual accuracy was favored by an inequality of brightness of the two lights and by a decrease of their combined illumination.

The amplitude or extent of motion so far as this factor was varied did not exert any effect upon perceptual ability.

Perception was generally more accurate with the faster rates of motion.

The individuals exhibited consistent differences in their ability to perceive the situation correctly.

The majority of the subjects were able to perceive the situation more correctly with stationary eyes, *i.e.*, when the stationary light was fixated and the moving light was perceived with indirect vision. Some individuals gave the better records when the moving light was fixated. Other individuals were able to judge equally well for both conditions of fixation.

Some individuals were able to perceive more correctly when the stationary light occupied the upper position, while the opposite condition obtained for other subjects. The position of the stationary light was not effective for other observers.

The influence of such factors as the degree of illumination and the rate of motion upon perceptual ability is obvious.

The influence of fixation indicates that certain individuals solved the problem in part by an indirect reference of one of the objects to the organism. It is practically impossible to exclude all spatial reference of the objects to the observer. Some subjects were apparently able to detect the moving object by means of the eye sensitivity involved in following it. Others were able to identify the stationary object by the absence of eye movement while fixating it. Individual differences in this respect do not admit of a ready explanation.

We have no explanatory suggestions to offer for the influence of inequalities of size and brightness.

The influence of the position of the stationary light may be explained by the supposition that the subjects apprehended the perceptual situation much as though the two lights constituted the ends of a swinging pendulum. Individuals who exhibited any tendency to conceive the situation as similar to a simple suspended pendulum will obviously associate the upper position with stability and hence will make a greater number of correct judgments when the stationary light occupies this position. On the contrary those who preferred to regard the situation as analogous to a metronome will associate the lower position with immobility and give the better records when the lower light is stationary. Certain subjects were not influenced by the position of the stationary light. This fact may be explained by supposing that these subjects conceived the situation as a compound pendulum with the center of rotation located between the two lights, that they were able to assume equally well either of the first two attitudes, or that they failed to adopt any pendular attitude.

Individual differences in perceptive ability may likewise be explained in part by differences in apperceptive attitude. Individuals who are able to adapt the first two attitudes to the objective sequence will naturally make unusually high scores. Subjects inclined to regard the movements as similar to a compound pendulum will obviously make poor records. No introspective records were taken in regard to these apperceptive attitudes during the experimentation. The hypothesis is suggested on the basis of its a priori plausibility.

The possibility that the subjects may adopt some pendular attitude toward the situation suggests that it is advisable in future experiments to eliminate this complicating condition by locating the two lights in a horizontal plane.

A NEW OBJECTIVE TEST FOR VERBAL IMAGERY TYPES

BY SAMUEL D. ROBBINS

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The purpose of this experiment was to devise an objective test by means of which the vividness of auditory, visual, and kinæsthetic verbal imagery of persons who cannot be depended upon for intelligent introspection may be quickly and accurately determined.

I had for subjects six students who were studying psychology. Four of these subjects were men, two were women. Two other women were subjects only in the first test described. No subject was under twenty years of age or over thirty. All of the women were born and educated in this country and had all their lives spoken English fluently. All the men, on the other hand, were born and educated in foreign lands, had spoken English but a few years, had a distinctly foreign accent, and had limited vocabularies of English words differing much from each other.

A number of series of card pairs containing a single column of five monosyllabic nonsense words of three or four letters were very carefully typewritten (double spaced) so as to look exactly alike except for one word which differed from the corresponding word on the other card in a single letter. The following types of nonsense groups were tabulated separately, the card pairs being shuffled so that the subject could not predict the nature of the next change: (1) the spelling was changed without altering the pronunciation, as veek-veak; (2) two long vowels were exchanged, as zoke-zake; (3) a short vowel was exchanged for the same long vowel, as koss-kose, there being another change, a visual one, in most cases; (4) two short vowels were exchanged, as meb-mib; (5) two consonants were exchanged which gave quite different auditory and kinæsthetic impressions, as aze-ane;

(6) two consonants were exchanged which sounded much alike but gave quite a different kinæsthetic impression in a trial series, as miz-niz; (7) two consonants were exchanged which gave about the same auditory and kinæsthetic impression in a trial series, as bim-pim. As some changes necessitated four-letter words, at least one four-letter word appeared on each pair of cards. There was an equal number of changes at the beginning, middle, and end of the word and for each of the five positions of the word in the column; hence the position of the change could not be predicted.

The nonsense words were given to the subjects in three ways: (1) They were read loud by the experimenter, the subject being careful not to repeat, spell, or visualize the word; (2) the subject repeated each word as it was read to him before the next word was read, being careful not to spell or visualize it; and (3) the cards were exposed in a tachistoscope and the subject was asked to whisper each word. (Subjects were asked to whisper rather than to read aloud because most stammerers can whisper without stammering, and these results are later to be compared with those obtained from stammerers.) The first method permitted the subject to employ auditory imagery alone, the second permitted both auditory and kinæsthetic imagery, and the third auditory, kinæsthetic, and visual imagery. There was an interval of five seconds between the reading of the first card of a pair and the first word of the second card, during which the subject was requested not to think of any of the words on the first card.

The subjects followed this direction remarkably well, and formed surprisingly few associations with these nonsense words. They forgot the nonsense words on the earlier cards before the later cards were read, hence memory of words on the first test played little if any part in the recognition of changes by the second and third methods; changes which were detected when the cards were read by the first method were frequently not detected when the same cards were subsequently read by the same subject by a different method. In the few cases where a change was remembered from a previous test, the letter 'A' was written on the record so that this change could be omitted in compiling the results if enough associations were formed to alter the averages materially. Each series was read at the speed which each subject found most satisfactory on a trial series.

The subjects were asked to report on a scale of 0 to 3 whether there was a change. 0 indicated that there was no change or that the subject did not know whether there was a change. 1 meant there might have been a change of which the subject was not at all sure. 2 denoted that the subject was pretty sure there was a change. 3 showed that the subject was positive there was a change. If a subject reported the right position of the change, he was credited the score he reported even if he gave the wrong word or could not remember the word. If he reported there was no change when there was a change, his score for the change was 0. As a matter of fact, every card was changed, except when a subject reported so many changes in succession that I feared he would mistrust my instructions that some pairs of cards had one change and others had no change.

The subject was also required to report what the changed word was on both cards, stating how sure he was of the word he reported on the same scale of 0 to 3. For his word score, he was given the score of the word of which he was less sure. If he forgot one of the words or reported one or both words incorrectly his word score was 0. If, on the other hand, he detected the main change correctly but made the same mistake in some minor letter of both words, he was given full credit.

Each subject was given a long trial series[1] by each method before the cards representing the scores compiled in table 1 were used. Subjects were therefore familiar with the experiment before the series reported was used.

Table I. shows the average score each subject obtained in each of the seven groups of changes by each of the three methods of presentation. The four columns in division 6

[1] One trial series consisted of sense words. This series was discarded because the words proved unequally familiar to the men of foreign birth.

TABLE I

AVERAGE SCORE FOR DETECTING EACH TYPE OF CHANGE

Subject	1 Change Only in Spelling	2 Long Vowel				3 Short Vowel for Same Long One				4 Short Vowel				5 Consonants Having Different Auditory and Kinæsthetic Impressions			
	W.	L.	R.	W.	Av.	L.	R.	W.	Av.	L.	R.	W.	Av.	L.	R.	W.	Av.
W.	1.50	2.70	2.35	2.95	2.67	3.00	3.00	3.00	3.00	1.75	2.00	2.40	2.05	1.50	2.40	2.40	2.10
E.	1.50	2.70	2.70	3.00	2.80	3.00	2.40	3.00	2.80	1.75	2.10	2.55	2.13	1.40	1.20	1.80	1.47
R.	1.90	3.00	2.95	2.85	2.93	3.00	3.00	3.00	3.00	2.40	1.20	2.95	2.18	3.00	2.20	2.10	2.43
M.	1.70	3.00	2.85	3.00	2.95	3.00	3.00	3.00	3.00	2.30	1.65	1.80	1.92	2.40	1.80	2.50	2.23
D.	1.70	3.00	2.55	3.00	2.85	2.40	3.00	3.00	2.80	1.50	1.50	2.20	1.73	1.00	1.40	2.40	1.60
T.	2.40	2.70	2.70	3.00	2.80	2.00	1.20	2.80	2.00	1.10	1.05	2.75	1.63	1.20	1.50	2.00	1.57
A.	0.60	2.55	1.95	2.40	2.30	2.40	2.40	1.40	2.07	0.80	0.95	0.95	0.90	0.50	0.30	0.90	0.57
S.	1.30	2.15	2.25	1.85	2.08	2.80	2.40	1.80	2.33	1.20	0.80	1.05	1.02	0.20	0.40	1.10	0.57
Av. for men	1.55	2.60	2.46	2.52	2.53	2.55	2.25	2.25	2.35	1.38	1.00	1.92	1.43	1.22	1.11	1.52	1.28
Av. for women	1.60	2.85	2.61	2.99	2.82	2.85	2.85	3.00	2.90	1.82	1.81	2.24	1.96	1.57	1.70	2.27	1.85
Av for all O's.	1.58	2.72	2.54	2.76	2.67	2.70	2.55	2.62	2.63	1.60	1.41	2.08	1.69	1.40	1.40	1.90	1.57

Subject	6 Consonants Having Different Kinæsthetic Impressions				7 Consonants Having Similar Auditory and Kinæsthetic Impressions				8 Average Change Score for all Types.				9 Average Word Score for all Types			
	L.	R.	W.	Av.	L.	R.	W.	Av.	L.	R.	W.	Av.	L.	R.	W.	Av.
W.	2.13	2.13	2.40	2.22	1.13	1.60	2.40	1.71	1.98	2.14	2.45	2.20	1.39	1.55	2.22	1.74
E.	1.00	1.73	2.20	1.64	1.40	1.80	1.60	1.60	1.81	2.04	2.27	2.05	1.21	1.88	1.93	1.68
R.	1.07	0.47	2.20	1.24	0.27	0.00	1.20	0.49	2.04	1.49	2.34	1.97	1.76	1.25	2.06	1.71
M.	0.60	1.27	1.53	1.13	1.20	0.53	1.47	1.07	2.02	1.76	2.08	1.96	0.79	0.93	1.47	1.08
D.	0.80	0.80	1.80	1.24	0.67	0.67	1.87	1.07	1.58	1.55	2.26	1.82	0.94	0.85	2.02	1.30
T.	0.60	0.73	2.13	1.16	0.60	0.33	2.20	1.04	1.36	1.32	2.50	1.76	0.58	0.76	2.00	1.15
A.	0.20	0.07	0.80	0.36	0.20	0.60	0.40	0.40	1.06	0.98	1.12	1.06	0.79	0.42	0.73	0.65
S.	0.13	0.07	0.60	0.22	0.13	0.07	0.33	0.18	1.02	0.93	1.10	1.02	0.68	0.68	0.85	0.74
Av. for men	0.50	0.33	1.43	0.86	0.30	0.25	1.03	0.53	1.37	1.18	1.77	1.45	0.95	0.77	1.41	1.04
Av. for women	1.13	1.48	1.98	1.53	1.10	1.15	1.83	1.36	1.85	1.88	2.27	2.01	1.08	1.30	1.91	1.43
Av. for all O's.	0.82	0.91	1.71	1.14	0.70	0.70	1.43	0.94	1.61	1.53	2.02	1.73	1.02	1.04	1.66	1.24

give the average word scores for all of the groups taken as a whole; the other columns give the average change scores. In this table there are four columns each in divisions 2, 3, 4, 5, 6, 7, 8 and 9. Of these four columns the one headed L contains each subject's average score in the first test where he *listened* to the words as they were read to him; that marked R contains those in the second test where the subject *repeated* each word after the experimenter; that marked W contains those in the third test where the subject *whispered* the words exposed in the tachistoscope; and that marked Av. contains the *average* of the other three columns. Division 1 is not divided into four columns because the subject only whispered those sets in which the changed word was spelled differently yet pronounced exactly like the first word exposed in the tachistoscope. At the bottom of this table are given the average scores in each test for the men, for the women, and for all eight subjects. Subjects D, E, M and W are women, and subjects A, R, S and T are men.

This table shows that the women as a class did much better in every test than the men and that subjects who did especially well in detecting some change also did especially well in remembering exactly what word appeared on each card, the correlation[1] being + 0.929.

This table shows also that long vowel changes are much more readily detected than short vowel changes; it makes little if any difference whether a long vowel is changed to the same short vowel or to a different long vowel. Conspicuous consonant changes are noted almost as readily as short vowel changes, inconspicuous ones far less frequently. The whispering test gave the highest and the most uniform scores for all subjects. The subject of vowel versus consonant changes will be more thoroughly discussed in a later paper comparing the verbal imagery of normal speakers with that of stammerers.

Columns 1, 2, 3 and 4 of Table II. give the vividness of each subject's verbal imagery on a scale of 0 to 3 as

[1] All correlations were derived from the formula, $r = \frac{\Sigma(xy)}{\sqrt{(\Sigma x^2)(\Sigma y^2)}}$, where x and y are the deviations of the two traits from their mean in any single individual

determined from his ability to detect changes by auditory imagery, kinæsthetic imagery, and visual imagery. These ranks are determined as follows: The visual rank is the average score of the subject in detecting by whispering changes

TABLE II

COMPARATIVE EFFICIENCY OF DIFFERENT TYPES OF VERBAL IMAGERY

Subject	1 Auditory	2 Visual	3 Kinæs-thetic	4 Total	5 Spelling Backward	6 Copying Long Non-sense Words	7 Copying Perverted and In-verted Print	8 Writing Perverted and Inverted	9 Column 8 of Table 1
W.	2.5	1.5	1.7	5.7	0.82	5	7	49	2.20
R.	2.8	2.0	0.4	5.2	10.12	49	28	3	1.97
T.	1.9	2.4	0.9	5.2	1.33	13	77	7	1.76
D.	2.3	1.7	1.1	5.1	6.03	1	34	201	1.82
S.	2.1	1.3	0.1	3.5	7.25	25	80	20	1.02
A.	1.9	0.6	0.1	2.6	22.05	43	157	171	1.06
Av. for women	2.5	1.6	1.5	5.6	3.43	3	21	125	2.01
Av. for men	2.2	1.6	0.4	4.1	10.19	32	106	50	1.45
Av. for all O's	2.3	1.6	0.9	4.9	7.93	23	64	75	1.64
Correlation with col. 4					+0.90 without R	+0.94 without R	+0.90	+0.83 for men	+0.95

in words that are pronounced alike but spelled differently, such as zeat-zeet. The auditory rank is the average score obtained by the subject in detecting vowel changes in all three vowel groups when these are read aloud to him. Trial experiments showed that kinæsthetic imagery played little if any part in these groups and that it actually reduced the score in many cases. The kinæsthetic rank is the average score the subject obtained when he repeated after the experimenter those cards in which changes had been made in words which sound much alike but give quite a different kinæsthetic impression (such as vad-vab), and in which he failed to detect changes when they were read aloud to him. The kinæsthetic and auditory imagery are so closely connected that I could find no better way to separate them. Trial series showed that there is little likelihood the subject will be able to detect a second time a slight auditory change which he does not detect the first time by the same method. The changes in this group were so chosen from the introspections on a trial series as to reduce to a minimum the auditory element, and to make the kinæsthetic element as prominent as possible. There is a close correlation, + 0.950, between the average ranks of subjects for the entire series as recorded in the average column in division 8 in Table I. and the total ranks obtained by adding together their three ranks in columns 1, 2, and 3 of Table II. as recorded in column 4. It seems to make little difference, therefore, what type of imagery is employed for the test as a whole; the subject possessing the greater sum total of verbal imagery attains the higher rank in this experiment.

Before beginning any tests, each subject was asked to answer the questionnaire given on pages 195–200 of E. B. Titchener's 'Experimental Psychology, Student's Manual, Qualitative.' A careful examination of the question blanks showed there was no correlation between verbal and non-verbal imagery. It is incorrect to assume, therefore, that because one has very vivid non-verbal imagery of a given type he must also have very vivid verbal imagery of the same type.

The four experiments which follow were performed to confirm the reliability of the preceding test.

After the test with nonsense words had been completed, each subject was asked to spell backward twenty words of increasing length, beginning with two letters and ending with twenty-two, and to report the method he employed for both the short and the long words. The experimenter kept a record of the time spent in spelling each word and of the number of errors. It is obvious that the efficiency of a subject in performing such a task depends upon both the speed and the accuracy with which it is accomplished; the greater the speed and the fewer the mistakes, the higher the efficiency. If this efficiency is represented by the product of the time in which a unit of the task is performed by the number of errors made, the lower product will denote the greater efficiency. These products for the six subjects who performed this test will be found in column 5 of Table II.

It will be seen that, with the exception of R, the lower product corresponds in every case with the higher sum total of verbal imagery recorded in column 4, the correlation without R being + 0.90. This shows that efficiency in this test is proportional to the total amount possessed by the subject of verbal imagery of types useful in performing the task. R reported that he employed visual imagery alone for this test because he believed it would increase his speed. As he possessed the strongest auditory imagery of the six subjects and as the trial series showed him to have about the weakest visual imagery, he should have employed auditory imagery or a combination of both, rather than visual imagery.

After spelling backward, each subject was asked to copy fifteen long nonsense words of from twelve to twenty-eight letters, to make a dash every time he looked at the copy, and to report how he copied these words. The experimenter kept a record of the time spent in copying each word and of the number of errors. Column 6 of Table II. gives the relative efficiency of the subjects in this test, this being the product of the average time in seconds it took the subject to copy each word by the average number of mistakes per word; and, as in the spelling backward test, the lower the product, the greater the efficiency. With the exception of R, the lower product corresponds closely with the higher sum total of verbal imagery, the correlation being + 0.94 without R. R employed auditory imagery alone, looking at the first twelve words but once and spelling them as he pronounced them. As long groups of consonants made it impossible to pronounce these words, he naturally made many mistakes.

After copying these long words, each subject was asked to copy two or three lines of each of six type-written selections, arranged in the following ways: perverted, backward perverted, inverted, backward inverted, perverted and inverted, and backward perverted and inverted. By backward I mean spelled from right to left instead of from left to right. By perverted I mean written so as to be read in a mirror. By inverted I mean written so as to be read by one facing the writer. The subjects introspected as in the previous tests, and the experimenter kept a record of the time it took to copy each line and of the number of mistakes. Column 7 of Table II. gives the relative efficiency of subjects in this test. This efficiency is denoted by the product of the average time per line in minutes by the total number of mistakes; and here again the less the product, the greater the efficiency. With the exception of T, the lower product corresponds in every case with the higher sum total of verbal imagery, the correlation being + 0.90 even with T included. T used an unfortunate method on the first line which caused him to make nearly as many mistakes as he made on the other twelve lines combined; when this one line is discarded, his score becomes practically equal to D's.

After copying these unusual kinds of printing, the subject was asked to write 'United States of America' so that it would be read normally by a person facing him; 'Harvard University' so that it would be read normally *through* the paper without inverting it; 'Cambridge, Massachusetts' so that it would be read normally through the paper *inverted;* and 'European War' so that it would be read forward *through* the paper without inverting it but with each letter perverted. The subject introspected as before and the experimenter kept a record of the time required to write each phrase and of the number of errors. Column 8 of Table II. gives the relative efficiency of the subjects in this test. Here the product of the average time per letter in seconds by the total number of mistakes gives the efficiency; so, as usual, the smaller product denotes the greater efficiency. With the exception of the women, who found this test far more difficult than did the men, there is a fairly good correlation, + 0.83, between the efficiency of each subject in this test and his total verbal imagery score.

As all of the spelling, copying, and writing tests just described require the coöperation of auditory, kinæsthetic, and visual verbal imagery, the high correlation between the efficiency of a subject in these tests and his total verbal imagery score (which is the sum of the scores of the separate types of verbal imagery used by a majority of the subjects in performing each of these tasks), shows that the efficiency of subjects in performing a given task is proportional, approximately, to the sum of the scores of the separate types of verbal imagery commonly employed in performing that task.

Any one wishing to use a similar but shorter test can score auditory imagery by reading aloud to his subjects thirty cards such as I used containing the following vowel changes: koss-kose, nume-nurm, dack-dake, girn-gine, neff-neaf, afe-ofe, eag-oag, ane-une, ipe-epe, fese-fose, bose-buse, nafe-nufe, vife-vefe, obe-ube, zoke-zake; ank-enk, ald-uld, nef-naf, ilt-ult, nop-nep, baf-bof, eft-uft, ilm-ulm, zep-zop, alk-olk, maz-muz, oln-uln, mev-muv, tig-teg, zab-zib. These cards should be well shuffled with one another and with a second group containing at least twenty consonant changes which give a distinctly different kinæsthetic impression such as the following: ubs-uds, vab-vad, nis-niz, gem-gen, bis-tis, vot-vob, sef-zef, tov-pov, ips-ids, zup-zut, pax-dax, mub-nub, abf-atf, ost-ozt, emk-enk. These groups should then be repeated after the experimenter, and in the case of the kinæsthetic score the record should be kept only of those consonant changes detected that were not noticed when the experimenter first read the second group. The third group, used for scoring visual imagery, should contain at least ten nonsense words which are spelled differently yet pronounced alike such as mije-mige, bick-bik, fis-fiss, gax-gaks, veet-veat, doxe-doax, klab-clab, zoll-zol, boze-bose, sibe-cibe. After the subject has thus repeated the first and second groups after the experimenter, the third group should be shuffled with ten pairs from other groups and exposed in a tachistoscope long enough for the subject to read each card aloud. The experiment should be conducted and the results scored as outlined at the beginning of this report.

Summary

1. Women possess more vivid verbal imagery than men.

2. Long vowels receive more attention than any other letters.

3. Short vowels receive much less attention than do long vowels.

4. Consonants receive less attention than vowels.

5. A change of one letter in a pair of nonsense words can be detected most readily if the subject reads the words aloud from copy.

6. Those persons will perform a given mental task most efficiently who possess the most vivid types of verbal imagery commonly employed to accomplish that task.

NOTE ON THE VERBAL IMAGERY OF STAMMERERS AND NORMAL SPEAKERS.[1]

I stated that the subject of vowel versus consonant changes would be discussed in a later paper comparing the verbal imagery of normal speakers with that of stammerers. As my experiment shows that the average verbal imagery for twelve normal speakers and twelve stammerers is practically identical, a brief note will suffice to summarize my results.

The same objective test described in the above named article was given to four additional speakers, two of them men, and two women, and to twelve stammerers, six of them men, and six women; and the average score for the twelve normal subjects and the twelve stammering subjects was compiled as in my earlier article.

The average change score showed that the stammerers detected changes a very little better than the normal speakers when the card pairs were read to them, 1.7 compared with 1.6, and that the normal speakers detected changes a little more readily when they looked at the card pairs and whispered them, 2.0 compared with 1.8. They averaged the same, 1.5, when they repeated the card pairs after the experimenter, and averaged the same, 1.7, when all three methods of presentation were averaged together.

The vividness of each subject's visual, kinaesthetic, and auditory verbal imagery was determined as in the earlier paper and the stammerers and normal speakers were found to have the same average auditory imagery, 2.2, and the same average kinaesthetic imagery, 0.8. The visual verbal imagery of the

[1] Prepared after the article was in type.

stammerers was but 75 per cent. of that of the normal speakers, however, 1.2 compared with 1.6. As few persons employ visual verbal imagery to any extent in speech, there is obviously nothing in verbal imagery of any kind to account for stammering. There is a suggestion, however, that certain letters may attract the stammerer's attention unduly; this problem I am now investigating. The following facts came out in this investigation.

The stammerers did not detect the long vowel changes so readily as the normal speakers, 2.4 compared with 2.7 in the listening test and 2.2 compared with 2.6 in the repeating test; yet they detected the short vowel changes more readily, 1.9 compared with 1.6 in the listening test and 1.7 compared with 1.4 in the repeating test.

Averaging the three series composed of the three types of vowel changes I found that the stammerers and normal speakers scored alike, 2.2, on the listening tests, and that the stammerers did not do quite so well as the normal speakers on the repeating tests, 1.9 compared with 2.1, or on the whispering tests, 2.4 compared with 2.5.

Averaging the three series composed of the three types of consonant changes I found that the stammerers excelled in the listening tests, 1.15 compared with 0.95, and on the repeating tests, 1.05 compared with 0.95, but scored only 1.44 compared with the normal speakers' score of 1.68 on the whispering tests, the difference being due, no doubt, to the better visual imagery of the normal speakers. The average for the three methods of presentation was the same for normal speakers and stammerers, 1.19. The maximum and the minimum scores averaged the same for stammerers and normal speakers in the vowel changes and the maximums averaged the same in the consonant changes, but the normal speakers' minimums averaged twice as low as the stammerers in the consonant changes. It would seem, therefore, that stammerers pay more attention to consonants than do normal speakers; this is confirmed by the work I have done in connection with the correction of stammering.

A FUNCTIONAL INTERPRETATION OF HUMAN INSTINCTS

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Recent developments in the study of human behavior make it possible to begin a reinterpretation of instincts and related phenomena which today admittedly constitute the darkest chapter in psychology. In this paper the writer attempts to suggest a functional interpretation of human instincts and their integration into instinctive conduct. The functional psychologist aims to start from an unbiased naturalistic standpoint and therefore hopes to achieve some progress in the understanding of some of the adaptational equipment of human beings. At the very inception of such a study we observe the imperative necessity for a scrupulous discrimination between the acts which are properly called instincts, and the more complex reactions developed from them which we will call instinctive conduct or behavior.

I

The Nature of an Instinct.—An instinct is a comparatively simple and direct response to a specific stimulating object or condition. It is in fact the functioning of a connate potential reaction system[1] which is organized from simple psychophysiological dispositions or tendencies to respond to stimuli. That instincts are so highly spontaneous may be accounted for by the fact that the specific way in which the reaction system functions, depends upon the stimulating conditions. It is this moulding of the response by the surrounding con-

[1] A reaction system is a complex function involving cognitive, conative, affective, muscular, glandular and neural factors. *Cf.* Kantor, 'Conscious Behavior and the Abnormal,' *J. of Abnorm. Psychol.*, Aug., 1918. An example of a reaction system is the response, 'August, 1914' (with all its accompanying organic resonances) to the stimulation, 'when did the hostilities of the great European War begin?' This response is potential in all those who have acquired the informational reaction.

ditions which is the source of the many marvellous tales of intelligence among the lower animals.

An instinct being a primary act and therefore entirely "undebauched by learning," must be looked upon as one of the primary functional elements in the embryological development of the human organism. For the instinctive reaction patterns are functions of animal adaptation developed from the simple functions of organized matter.[1] Owing to this development instincts may be classified as (1) food-getting, and waste eliminating responses, (2) sexual reactions, (3) expressive acts, and (4) protective responses. These classes represent specific adaptations to particular adjustment-situations, that is to say concrete actions, and with the random movements and reflexes form the matrix of the entire series of human behavior.

The function of human instincts is to adapt the person to the various surroundings in which he is found, pending the development of the intelligent responses usually required for such adaptations. These modes of instinctive response develop in the species of organism during its interaction with its environment; consequently there is an entirely natural genesis of the instincts paralleling the growth of the human being in the evolutionary course of the animal species to which he belongs. Every organism possesses a series of these reaction systems which in the presence of adequate stimuli become responses. The response and the stimulus together constitute an act, that is to say, a specific adaptation. From a definitively psychological standpoint the individual at any particular moment is this series of reaction systems.

If we have correctly described the origin and development of instincts, we have sufficiently indicated that the instincts of the human organism are very different from those of the lower animals. The reaction systems, as the units of the organism on the action side, must naturally be just as diverse in dissimilar organisms as are the structural parts. Thus we find differences of a wider or narrower sort in both the mental and physiological factors of the specific functions.

[1] These are usually described by the zoölogist as irritability, metabolism, reproduction, motility, etc.

Obviously, the most striking variation between human and animal instincts is the extreme modifiability of the former. In fact, human instincts are so distinctly transitory in character that they disappear very early from the reaction equipment of the human organism, and in the adult individual are completely absent. These human instincts become integrated into more complex types of responses, while the animal instincts remain as permanent acquisitions of the organism, and change only by becoming more adaptable through practice to the situations in which they frequently function.

The Nature of Instinctive Behavior.—In contrast to the instincts, instinctive conduct comprises adjustments which are essentially acquired tendencies of response, and in most cases constitute intelligent behavior. It must be noted, however, that the reaction systems of instinctive conduct, which, by the way, include the greatest portion of our actual responses, are developed as elaborations of a prominent core of organized innate reaction patterns. In all cases of instinctive conduct we have integrations of concrete human acts; so that if, for example, we start with the walking act of a child, the exigencies of the surrounding conditions may condition that initial act to become a locomotor response to the call of the parents, or to any other stimulating circumstance acting upon our illustrative child. The results of observations of human behavior demonstrate that the rapidity and complexity of the integrations are owing to the responsiveness of surrounding objects; that is to say, a responsive object forces the individual to apprehend the possibility and necessity of varying his response, and therefore to learn to react with a meaningful behavior to the stimulating response of the other object. This sort of interaction with the environment constitutes the basis for social phenomena of various types and is excellently illustrated by the constant interstimulation between two boys during the preliminaries of a fistic combat. In this situation each individual is intently posed in an anticipatory attitude, requiring only the slightest sign of change in position on the part of the opponent as an effective stimulus to bring about a telling response. When we recall that a single individual can serve both as stimulating and responding object we can appreciate the importance of this self-stimulation as a factor in the rapid integration of the simpler forms of behavior.

Since the natural environment of the human organism consists primarily of responsive objects we see why the human adult has no instincts, that is, performs no acts which are actualizations of exclusively innate dispositions, but always responds with a partially acquired reaction pattern. The view that man has more instincts than the lower animals, for which James[1] is in part responsible, could only obtain credence so long as the precise nature of a conscious act remained unanalyzed. That James did not entirely ignore the facts concerning instinctive conduct is manifested by his observation that human instincts do not remain blind.[2] The physiological viewpoint, which always influenced James, prevented him from fully appreciating the psychological changes which transform instincts into more complex actions. To think of the non-rational activities of the human organism in terms of reflexes which are somehow coupled with impulses, means the capricious disavowal of the variety and richness of the instinctive forms of behavior.

The contrast between instincts and instinctive behavior is made clearer by dispelling somewhat the confusion existing in the conception of the differences between the instincts and the more simple reflexes which differ widely from the former in organization and function. The reflex action involves the functioning of a more definite and fixed reaction system than does the instinct, and the result of the stimulation is a genetically simpler form of behavior. The relative rigidity of the reflex response allows comparatively little room for adjustment between the organism and the stimulating conditions while the action system is functioning.

Further, it has been frequently observed that instincts involve a much larger conscious function than is the case with reflexes,[3] since the latter are on the whole much simpler,

[1] *Cf.* 'Principles,' pp. 393, 441.
[2] *Ibid.*, 390.
[3] Stout, 'Manual of Psychology,' 1915, p. 343.

but we must guard against the idea that reflexes are merely neuro-muscular actions.[1] While Stout is entirely correct in his assertion that 'instinctive conduct does, and reflex action does not presuppose the coöperation of intelligent consciousness,' he is mistaken in supposing that the absence of intelligent consciousness implies the complete absence of a conscious factor in the response.[2] Instincts and reflexes imply, then, the functioning of two distinct types of connate reaction patterns, both of which are to be distinguished from instinctive conduct which is never the functioning of a purely innate reaction pattern, although it is to a certain degree developed always from instincts.

II

The Range of Instinctive Conduct.—The distinction between instinctive conduct and instincts paves the way for the consideration of the large place which the former holds in human life. We have already suggested that most of our ordinary behavior is instinctive conduct, but this does not mean in any sense that complex actions such as we perform are the expressions of a few inborn impulses. Such a manner of thinking represents a vestige of scholastic simplicity which is genuinely subversive of all understanding of human behavior. What is meant is that even our very complex actions are in great measure conditioned by the instincts from which they have developed. To be sure, the simplest instinctive conduct is very largely the functioning of an innate reaction system, although conditioned by acquired factors. The proportion of innateness in the reaction pattern is measured by the directness of the connection between the stimulus and the response, or in other words by the character of the appreciation which the individual has of the meaning or significance of the stimulating object. In the simplest case the meaning of the object does not emerge as a striking factor in the act; it merely represents a modification in the response owing to a previous contact with the stimulating object. In a general way, we may very properly consider

[1] *Cf.* Stout, *Brit. J. of Psychol.*, 3, p. 244.

[2] Stout's separation of the conscious and movement components of a response clearly exemplifies the difference between his position and a functional viewpoint.

the simplest instinctive behavior as called out by the environment, and largely controlled by it, and not by the organism. As examples we may quote all those activities usually described by psychologists as subconscious or unconscious, which are very prominent in manual learning, and technical operations of all sorts.

On the other hand, the more complex instinctive conduct is more independent of the stimulating object and includes in its reaction systems a larger component of acquired factors. Here the meaning of the object serving as a definite foresight of the act, functions in a more precise manner, and in still more developed behavior includes an effective appreciation of the consequences of past responses to stimulating objects. The instinctive behavior at this stage may involve an elaborate series of memorial and thought functions, and when so complicated its specific characteristic as an instinctive behavior is the fact that it is perceptually stimulated, that is, the act is not initiated by a problematic situation. In this last class we may place all the involved social behavior which constitutes many of our daily responses. We must conclude, then, that instinctive conduct composes a considerable portion of practically all adjustments from the simplest to the most complex.

The Intelligence in Instinctive Behavior.—We may sum up the essential characteristics of instinctive behavior by pointing out the invariable presence in it of at least the rudiments of intelligence. Thus in many cases the reaction system, although a response to an immediately presented perceptual stimulus, is still carried out by predominantly acquired reaction factors, as is convincingly exemplified by much of our socially restricted behavior. Such acts are spontaneous responses to definite perceptual stimuli, but they are performed in roundabout ways and in many instances tend toward concealment. The openness and frankness with which such acts are originally performed are by virtue of social disapproval more or less successfully repressed.

Distinction of Instinctive from Rational Conduct.—The great variety and complexity of instinctive responses make it necessary to distinguish them from rational acts, a discrimination which is all the more pertinent when we consider that in the final analysis all of our acquired reaction systems are at some level integrations of elementary instinct acts. As a response to a problematic situation the rational act is probably always initiated by an indirect stimulating object through some highly developed meaning function. Unlike complex instinctive behavior the rational act is not only guided to its conclusion by intelligent functions, but is originated by a reflective consideration of ways and means. Thus it becomes the basis for all transformative conduct, that is, action which remakes the environmental conditions through some function of creative imagination, while in the case of instinctive conduct the result is usually merely an adaptation to those conditions.

III

The Specificity of Instincts.—Whether or not instincts are specific in their functioning is a crucial inquiry for the understanding of them, and a problem which may throw considerable light upon the distinction between instinctive behavior and instincts. It is important to note that since instincts are simple and immediate responses to specific stimuli which bring innate action systems into function, they presumably must be specific in their results.

This view, however, is not generally held by psychologists, although some adhere to it so tenaciously that the observation of the indeterminateness and indefiniteness of human behavior influences them to deny the existence of instincts in the human being. While it is entirely demonstrable that mature persons possess no instincts, this must not be interpreted to mean, as Stout does, that human behavior in general has no instinctive foundation in the form of concrete action patterns.[1] To believe in the absence of instincts in the human

[1] Our interpretation of Stout's position is in no wise invalidated by his reluctant inclination, expressed in the third edition of his 'Manual' (p. 360), to make the term instinct refer to general capacities, such as 'innately organized interest,' 'attention,' and 'power of learning by experience in certain directions.' On this basis he asserts that 'the whole development of human minds has its root in connate tendencies of this sort and is inexplicable apart from them.' From our standpoint in appears that Stout is here avoiding the essential problem of instincts.

individual because instinctive conduct is contrasted with intelligent conduct is to overlook entirely the facts (1) that we are studying concrete conscious behavior, and for that reason we need not think of an instinct as a permanent spring of action, the absence of which at the present time indicates that it was never present; and (2) that intelligent behavior is developed by the integration of simple types of action, a fact which enables us to understand how the reaction pattern of an instinct becomes elaborated and developed into a complex intelligent response.

An inquiry into the views entertained concerning the definiteness of instincts reveals the fact that what is frequently meant by an instinct is a neuro-muscular function. Thus Stout, for example, describes an instinct as a "purely biological adaptation comparable to the prearrangement of structure and function which in human beings subserves the digestion of food."[1] Upon examining this conception we are impressed with its inadequacy to represent human behavior, although we are in hearty agreement with Stout in rejecting such a view as that of Bergson-Carr, stated by Stout as the belief that there is a special form of psychical activity which requires the technical name of instinct.[2] We insist that not because human behavior has no instinctive basis do we not find instincts, but because the latter have become developed into intelligent behavior in the course of the individual's contact with his surrounding conditions. This fact Stout could have seen had he not been prevented by his general psychological standpoint from appreciating that the psychologist is interested in modes of response to stimuli, and not in expressions of mentality. Apparently, Stout assumes the specificity of instincts, and from such a premise he concludes that there are no instincts in the adult human being because he does not find man performing acts which express a mental process, through innately coördinated motor mechanisms.[3] Stout consequently fails to appreciate the

[1] *Brit. J. of Psychol.*, 3, 243.
[2] *Ibid.*
[3] *Cf.* McDougall, *Brit. J. of Psychol.*, 3, 269 ff.

large place which instinctive conduct plays in the life of the human individual.

When we turn to the work of Thorndike,[1] who is attempting to investigate the 'original nature of man,' we find much to commend in his description of the specific instinct responses. Beginning with the admirable intention to describe concrete facts of behavior, he scouts the viewpoint which makes of instincts generalized tendencies to bring about some vague result presumed to be beneficial to the organism.[2] Thorndike stands upon firm scientific ground when he looks upon instincts as specific types of unlearned responses to definite kinds of stimulating situations, but his work presents us with grave difficulties. Conceived in neuro-biological terms, it implies that man's 'original nature' remains forever a prominent part of his behavior equipment. From this fact arise several implications tending to misconstrue the actual character of instinctive behavior.

In the first place, such a viewpoint cannot escape the implication that the human individual acts precisely as does the animal, since the former is fitted with a similar sort of neuro-muscular structure, and secondly, a more serious difficulty is that such a position leaves no room for the development of behavior.

The first difficulty must be understood as referring to the obvious faultiness of the attitude that human behavior is permanently like that of the lower animals. It is true that in the case of infants the acts are like those of the simpler organisms, but this is because we are observing simple instincts. In older children and adults the behavior has become integrated into intelligent conduct and is thus qualitatively different.

In answer to the possible reply of Thorndike that a sufficient differentiation of conduct in man and animals is allowed for by the combination of neural elements, we might suggest that such a way out of the difficulty would only result in describing complex abstractions instead of observable behavior. Human conduct is infinitely more complex in every

[1] *Educational Psychol.*, 1913, Vol. 1.

[2] *Cf.* James ('Principles,' 1890, II., 183), whom Thorndike follows.

phase of adaptational character than can be accounted for on the basis of the combination of neural elements. Obviously the neural connections are essential mechanisms in all behavior, and since the activities of man are more complex than those of animals, these mechanisms must necessarily be more elaborate, but the nervous function cannot do anything more than mediate the spontaneous movements of the individual.[1] It is because the neural hypothesis was developed in connection with work on animal instincts that it has any significance as an explanatory principle, inasmuch as the animal instincts are very simple activities and so lacking in intelligence as to be almost mere biological functions.

We cannot agree with Drever,[2] who is essentially a follower of McDougall, in his criticism that the lack of differentiation in Thorndike's theory between human and animal instincts points to the nonspecificity of instincts. Drever insists[3] that there is no genuine specificity in Thorndike's instincts, since, for example, the 'instinct to escape from restraint' is so complex as to involve in the case of a little child 'stiffening, writhing, and throwing back the head and shoulders' and in the older child also 'kicking, pushing, slapping, scratching and biting.' Drever declares that the instincts mentioned belong with the six others enumerated by Thorndike[4] under McDougall's heading of pugnacity, and that the precise factor of unity is the accompanying emotion of anger. As a further argument against the specificity of instincts, Drever indicates that in some cases we cannot predict what a specific response will be, and the individual may try many different ones in succession. Thus, for example, under some conditions of stimulation the person may respond by flight or concealment, and in some cases by both reactions in turn.

[1] It is unfortunate that psychologists appear to overlook the fact that constructive biologists do not think in terms of isolated nerve functions, but in terms of neuro-musculo-glandular systems. In this connection it appears that if Thorndike has avoided 'mystic potencies' ('Educ. Psychol.,' p. 11) he has done so only by translating them into neural terms.

[2] 'Instinct in Man,' 1917, p. 155.

[3] *Op. cit.*, p. 166.

[4] *Op. cit.*, p. 68 ff.

The writer is satisfied that instead of proving the non-specificity of instincts, what Drever really shows is that human beings respond only by means of instinctive behavior and not with instincts. To repeat, instead of responding merely with an innately organized reaction system, the individual reacts with a complex acquired reaction pattern, which in the course of his development has had an increased knowledge and affective factor added to it. It is for this reason that the anger or fighting situation calls out such a wide and varying series of actions. In order to explain such conduct it is entirely unnecessary to invoke a dubious interpretation involving an unwarranted conception of the nature and function of the emotions as Drever following McDougall does.

Drever seems to realize that human behavior is a complex function developed in interaction with stimulating circumstances, when he writes that "behavior will be largely determined, first of all, by the circumstances of the case, by what kind of response will best secure safety. It will be determined, in the second place, by the intensity of the fear aroused, and two individuals may behave in two entirely different ways in response to the same situation, according to the degree of fear aroused."[1] This unimpeachable observation, which certainly controverts Thorndike's position that an instinct (instinctive behavior) is the functioning of a neuro-muscular apparatus, should have led Drever to see that the actions which he quotes from Thorndike's description are phases of intelligent instinctive conduct, and not the expressions of a mysterious 'general' instinct. Were Drever thinking in terms of concrete behavior he would easily see that instinctive conduct is not the functioning of an 'end,' or 'instinctive impulse' with an intelligence entity to carry it out,[2] but that it is a definite response to a stimulus which involves in its specific mode of action the integration of numerous previous experiences. In all cases of actual instinctive behavior the 'end' is gratuitously imposed upon

[1] *Op. cit.*, p. 163.
[2] *Ibid.*, p. 122 ff.

the situation.[1] What actually happens is that at any particular time certain combinations of surrounding circumstances stimulate the person to perform definite acts, provided that he has the necessary equipment of reaction systems. The adequate consideration of the stimulating auspices of behavior entirely removes the necessity of postulating teleological powers in the organism.

The spectator may profit by Drever's attack upon Thorndike's position by observing that on the one hand in his endeavor to avoid "mystic potencies" Thorndike refuses to interpret behavior as it actually occurs, preferring to lean upon unreal if not mystic potencies, while on the other, Drever,[2] following McDougall,[3] describes behavior in a more acceptable manner, but does not hesitate to explain it as the result of metapsychological agency.

The second difficulty with Thorndike's view of instincts, namely, that it disregards the development of behavior, may be considered as a derivation from the first difficulty. It results in the misinterpretation of human action, which as we have seen has as its primary characteristic the process of integration. A critical study of such behavior indicates conclusively that not a single act of an adult person[4] is an original response, but always a complex development of acquired reaction systems. It appears that Thorndike must think of instincts in the adult as drives or potencies of some sort, that is to say, at this point they have lost their specific character. In his failure to distinguish between instincts and instinctive conduct Thorndike vitiates his original excellent intention to describe actual psychological occurrences. Consequently, his interpretation leaves unfulfilled

[1] In this connection it is extremely edifying to observe the highly moral ends that are sometimes imposed upon the instincts, such as 'heavy and unremitting toil on behalf of the offspring' in the case of the parental instinct. *Cf.* McDougall, 'Soc. Psychol.,' 1916, p. 269.

[2] "Instinct is the 'life impulse' becoming conscious as determinate conscious impulse," *op. cit.*, p. 88.

[3] "For I hold that the instincts are essentially differentiations of the will to live that animates all organisms and whose operation in them makes the essential difference between their psychophysical activities and the physical processes of inorganic nature." *Brit. J. of Psychol.*, 3, p. 258.

[4] Excluding the reflexes, of course.

his original functional promise, and ignores therefore one of the extremely important factors in conscious behavior, namely, the stimulating circumstances. In not allowing for an interpretation of the actual responses which an organism makes in adapting itself to surrounding conditions, Thorndike's position results in an inert structuralism which prolongs the intellectual tradition of a permanent self.[1]

The most zealous advocate of the non-specificity of instincts is probably McDougall who approaches the problem from the angle of social behavior. This author impugns the theory of social action which assumes 'that man is a reasonable being who always intelligently seeks his own good and is guided in all his activities by enlightened self-interest.'[2] Unfortunately McDougall's easy victory over such a vulnerable position has resulted in his substitution of another absolute spring of action as the basis for all human behavior, namely the series of instincts. The ubiquity and persistence of certain types of action no doubt has influenced him to propound the hypothesis that the human 'mind' is constituted by the sum of innate tendencies which bring about the specific actions of the individual. As will appear in the course of our discussion these tendencies McDougall believes to be permanent psychic entities. The assumption that instincts are the 'essential springs or motive powers of all thought and action' necessarily implies therefore that they are general capacities to bring about certain actions, for otherwise there would be required an infinite number to account for all the variety of social behavior.[3] The supposition of the non-specificity of instincts in turn creates a presumption in favor of the perseverance of dispositions as permanent tendencies of human actions.

An impartial investigation of behavior clearly demonstrates the extravagance of assigning any absolute foundation for human conduct. Thus for example, to insist upon instincts as the exclusive springs of action is to lose sight of

[1] As a series of physiological mechanisms.

[2] 'Soc. Psychol.,' p. 11.

[3] "Lightly to postulate an indefinite number and variety of human instincts is a cheap and easy way to solve psychological problems." 'Soc. Psychol.,' p. 26 ff.

the actual fact that many human actions are in a genuine sense rationally motived. As a consequence of seeking an absolute factor in human behavior, McDougall reaches the same result as Thorndike, namely, a form of abstractionism which adds little to the comprehension of such behavior.

The impuissance of McDougall's conception of instincts as an interpretation of conduct is instructively intimated in the existence of an uncrossable barrier between his exposition of instincts and his discussion of social behavior. Although he starts out with the assertion that an instinct is a psychophysical disposition, not only to act but also to perceive, attend, and feel, that is to say, a concrete action,[1] he really thinks of it as an enduring condition or faculty of some sort.[2] The hypostatic nature of McDougall's thinking appears in its most overt form in his protest against using the term instinct to denote an action.[3] There is apparently no way in which such instincts can develop into complex social behavior excepting by some form of crude mechanical agglomeration.

In all fairness to McDougall it must be said that he realizes the appalling chasm which separates his instincts from the complex behavior of the social type, for he develops a theory to account for the fact that instincts, while substantial elements, can still be the basis for all complex human action. This theory assumes that an instinct can be divided into 'three corresponding parts, whose activities are the cognitive, the affective and the conative features respectively of the total instinctive process.'[4] Now the emotional factor is assumed to be unmodified throughout all the various changes which involve the other two factors;[5] so that not only can you find the same dispositions in animals as in man, but in man they can develop to any possible degree. Unfortunately for McDougall this theory glaringly exposes his indefensible position. For note, he allows for so much development in

[1] 'Soc. Psychol.,' p. 26 ff.

[2] *Brit. J. of Psychol.*, 3, 253. It seems clear that McDougall does not hold that the enduring condition of an instinctive act is a definite potential reaction system, that is to say, a concrete response pattern which will function when stimulated.

[3] *Brit. J. of Psychol.*, 3, 253.

[4] 'Soc. Psychol.,' p. 32.

[5] *Ibid.*, p. 34.

the dispositions that he almost gives up the idea that the springs of human actions are innate. In extricating himself from this difficulty McDougall further weakens his position, since in making the emotional aspect of the instinct the sole innate spring of action he runs counter to the fact which he himself admits,[1] namely, his inability to point out definite actual emotions in any but the 'principal' powerful instincts. The precariousness of McDougall's position is not at all mitigated by his highly questionable identification of the affective component of an instinct with an emotion.[2]

The conclusion that we may draw from the imperfection of McDougall's view is that he does not fully realize that he is attempting to interpret instinctive conduct, which is an entirely different matter from demonstrating the function of instincts in all the complex actions of human beings. He therefore starts from the wrong premises and is easily led to the bizarre idea of the substantial mental character of instincts. It is an egregious error for McDougall[3] to think that he is alone in believing that instincts are at the foundation of our mental life. It is almost a universal conviction among psychologists that all human behavior is based upon instincts as a foundation, but the important point is, that this foundation as an actual phenomenon is only a transitory phase of a maturation process.[4] For instance, when we observe the fighting reactions in the child and in the adult we are severely impressed with the qualitative difference of the respective reactions. In the first place, the specific responses in each case are different, implying that no enduring nervous basis can be inherited for the purpose.[5] Again, the stimulating situations may be absolutely different not only in the developing individual at different stages, but also in the same stage of growth at different moments, and in different individuals at the same moment. Not only will a fear situation in any of these cases call out different sorts of

[1] *Ibid.*, p. 46.

[2] *Cf.* Drever, *op. cit.*, p. 156 ff.

[3] *Brit. J. of Psychol.*, 3, 260.

[4] In the same sense as the fœtal structure which is the foundation for the adult physique is integrated in the course of development.

[5] *Cf.* 'Soc. Psychol.,' p. 29.

responses in the individual, but it may call out the same sort as an anger situation. In all cases the response of the individual will depend in part upon the multiplicity of circumstances immediately surrounding him. The fighting reactions, for example, will depend upon the presence or absence of onlookers and the regard one has for them if they are present, as well as upon the thing at stake in the contest. Such reaction will also be conditioned by all sorts of technical information and convictions one has acquired relative to fighting in general or to fighting under these specific circumstances. When we observe a complex social action we are convinced that even such elaborate and significant suggestions[1] which the penetrating students of human conduct enumerate, cannot fully cover its conditioning influences, although of course for some definite purposes not all of these influences are relevant.

McDougall's discussion involves the gratuitous assumption that the substantial instinct entity can be aroused under very different kinds of circumstances. For instance, at one time the instinct can be aroused by a natural stimulus and at another by a complex social situation, all through the medium of an emotion entity. There is apparently more than a smack[2] of the old faculty psychology in McDougall's thinking, a fact which is genuinely surprising when we consider that at certain points he almost realizes the distinction between instincts and instinctive behavior, as for example, in differentiating between the specific and general tendencies.[3]

McDougall's insistence upon the generality of instincts is based therefore upon the dubious premise that there are a few innate springs of all human conduct, rather than upon the observation that human behavior is a complex interaction of an experienced and intelligent person with a multiplex environmental situation. It is the very complexity of the total situation that seems favorable to the arbitrary analysis of it into a few constant factors. This is familiarly illustrated in the case of the complicated social and institu-

[1] Such as are found in Veblen, 'The Leisure Class,' etc.

[2] *Cf.* Drever, *op. cit.*, p. 16.

[3] 'Soc. Psychol.,' p. 20.

tional circumstances which are reduced to a few simple activities of the 'economic man.' McDougall[1] has gone only a step farther than Cousin, whom he severely criticizes, in the interpretation of the conditions of human activity, because the former fails to see that the dispositions to human action are all complex acquired functions and not a compound system of original sensori-motor arcs,[2] plus some type of antecedently functioning mental activity.[3] The explanation of McDougall's doctrine of generalized instincts seems to be the fact that he stands for a theory of psychological predestination,[4] and so he makes of the human individual a machine fitted with definite powers which require only an indifferent stimulus to make them perform whatever seems necessary to be done.[5] Although he condemns the practice in others, McDougall ascribes to the functioning of an instinct any frequent or constant form of action. Thus, the acquisition and building up of large estates are attributed to the acquisitive instinct.[6] It is a queer doctrine of magical potencies which can describe the development of such elaborate institutions[7] as we have in our complex life to the functioning of a dozen or so of instincts. And more anomalous still is the presentation of such a doctrine in face of the overwhelming facts pointing to the shaping of our instinctive behavior, by the lives and acts of persons and institutions.[8]

The entire controversy concerning the specificity of instincts is made possible only by an inclination toward a structural psychological position. When we take concrete

[1] *Ibid.*, p. 12 ff.

[2] 'Soc. Psychol.,' p. 29.

[3] Conative tendency—*cf. Brit. J. of Psychol.*, p. 261 ff. "The instinctive impulses determine the ends of all activities and supply the driving power by which all mental activities are sustained." 'Soc. Psychol.,' p. 44.

[4] "I hold to the reality of teleological determination of human and animal behavior." 'Soc. Psychol.,' Preface, second edition.

[5] The writer wonders whether McDougall considers the instincts as such absolute springs of action that they function either as determining the ends of all actions or merely by being suppressed. *Cf.* McDougall's discussion of the parental instincts, 'Soc. Psychol.,' p. 267 ff.

[6] *Cf.* 'Soc. Psychol.,' p. 323.

[7] "These impulses are the mental forces that maintain and shape all the life of individuals and societies." 'Soc. Psychol.," p. 44.

[8] *Cf.* Woodworth, 'Dynamic Psychol.,' p. 72 ff.

human behavior to be the province of psychology we are very soon impressed with the fact that instincts are necessarily specific in their functioning, but that the adult individual has no instincts. Furthermore, the obvious generality and unpredictability of adult behavior should lead us to observe that instinctive conduct is general because the environing conditions to which it is responsive are incessantly variable in their stimulating capacities.

IV

Relation of Instincts and Emotion.—The study of instinctive conduct has in recent years resulted in the almost universal agreement of psychologists that a very close relation exists between such behavior and emotions, although there are several doctrines as to the precise details of this relationship. It is held, on the one hand, that emotions are of instinctive origin and occur when the instincts are checked or in conflict, while on the other, it is believed that emotions are the correlates of instincts in some form. It must be granted that both these views are based upon observable conduct, and especially the fact that in many cases of instinctive behavior a powerful feeling element is involved; the importance of the data, however, intensified by the lack of uniformity in interpretation, demands a more adequate analysis.

The view that emotions are correlates of instincts is ably championed by McDougall, who, as we have seen, believes that the primary emotion is the affective element of the instinct. The primary objection to such an interpretation, as we have also seen, is that many instinctive actions do not involve emotions,[1] and that many emotional situations do not have such instinctive associates as are so convincingly discussed in the cases of anger and fear. As we have remarked above, the insistence upon the invariable presence of an emotion in every instinctive act is to reduce emotions in many cases to simple affective states.[2] It is significant that Drever, who closely follows McDougall, is forced to the

[1] *Cf.* Shand, 'The Foundation of Character,' p. 6, 370.

[2] *Cf.* Drever, *op. cit.*, p. 155 ff.

conclusion that only some instincts have emotional accompaniments.

The conflict theory, insofar as it insists upon a conflict situation as the basis of an emotional behavior, meets with few if any exceptions in fact, but the question arises whether the conflict is in all cases a conflict of instincts. The critical analysis of the emotional situation indicates that this is not true. Before proceeding to such an analysis of emotional behavior it is well to describe its chief characteristics.

An emotion is an interrupting form of response to a suddenly presented stimulus in which various organic processes are put into function, which in turn facilitate the immediate performance of a new act. Among the outstanding features of an emotional action are the confusion and excitement which pave the way for a new act by inhibiting the behavior which is taking place when the emotion-exciting stimulus is presented. Naturally such an act is replete with affective and organic resonance, and here we find the clue to the relationship between the emotional and the instinctive types of behavior.

What happens in the case of the emotional situation is that a dissociation of the reaction systems of the person takes place; so that in the most violent type of emotion the person is left only with the capacity to act with almost purely physiological (reflex) behavior. From this extreme case we find a gradation of emotional conduct in which the disorganization leaves free to function a series of reaction patterns ranging from the simplest to the most complex instinctive behavior. The fear and anger situations offer excellent examples of the disturbances of behavior which leave comparatively simple forms of instinctive response to adapt the individual. A person may be walking along through a wood, perhaps thinking over some problem, when suddenly there is a cessation of the thought activity and the person finds himself in a state of great excitement and unpleasantness, and in readiness to run from a tiny creature madly scurrying through the brush. In this illustration, the simple instinctive, danger-avoiding response might appear, as the most serviceable form of behavior under the circumstances. We see, therefore, that not because an emotion is a component of an instinct or a conflict between instincts is it closely related to instinctive behavior, but because under certain conditions of stimulation the reaction systems are so disorganized as to leave only some instinctive mode of behavior to function.

Upon the basis of such an interpretation we can understand the more refined emotional responses, namely, those in which no violent instinctive reaction is involved. In the functioning of the more subtle emotions[1] the environmental circumstances are such as to disturb only the most elaborate and definitely focused acquired reaction systems, for example, rational conduct, and thus leaves free to function such complex forms of behavior as almost entirely to dispel the appearance of a shock or conflict. The resumed activities in such cases are of course only slightly different from those interrupted.

V

In conclusion we might point out three cognate obstructive tendencies, which persistently hinder psychological thinking concerning instincts, and which prevent the scientific interpretation of instinctive behavior, namely (1) metapsychological speculation, (2) biological abstractionism, and (3) psychological simplification.

Metapsychological Speculation.—This motive has always been a prominent factor in discussions of instincts, and strangely enough is still responsible for the many inaccuracies and trivialities of those studies. The unmistakable theological implication of this attitude is manifested by the explanation of instinctive behavior in terms of a mysterious original force implanted in animals to carry out some primary end of life, as for example, the preservation of the species.[2] We have already had occasion to refer to the hardly less objectionable aspect of the metapsychological view, which makes

[1] Not the diffused feelings.

[2] Represented in psychology today by McDougall and Drever, who stand in the von Hartmann-Bergson line of development, *cf.* Drever, *op. cit.*, p. 89.

instinctive behavior stand for everything that is considered unknown or persistent in conscious behavior.[1]

Much the worst disservice of the metapsychological attitude is precisely that it maintains unknowables which prevent the adequate investigation of psychological phenomena. To assume that instincts are ultimates of animal nature and to seek to describe these putative elements, precludes the conception of instincts as definite forms of concrete responses.

Biological Abstractionism.—We may consider the historical rise of biological abstractionism as a protest against the extreme vagaries of the speculative psychologists. The biological influences in psychology transformed instincts into simple psychological phenomena explicable in terms of the nervous structure of the organism. Thus we find the statement that the instinctive factors in behavior 'depend entirely on how the nervous system has been built up through heredity under the mode of racial preparation which we call evolution.'[2] Instincts are consequently considered to be specific arrangements of neural mechanisms; so that James, following Spencer, spoke of them as conforming to the general reflex type.

An unacceptable issue of physiological abstructionism is the tendency to overlook actual phenomena to such a degree as to allow for no difference between such widely varying behavior as we find in man and animals. Our study of instinctive conduct has afforded sufficient intimation that much of the unsatisfactory interpretation of such behavior can be traced directly to the fact that it is the animal type of reaction that is uncritically employed as an exclusive basis of description. As a consequence this comparatively simple behavior is resolved into hypothetical neural elements which can in no way account for so conspicuous a variation as the rapid development of human instincts into intelligent conduct, and the practically stationary condition of the

[1] So that the success of a politician or business man is attributed to the presence of political or business instincts, the desire of a nation to govern itself to the functioning of a self-governing instinct, and the building of cities to the presence of a gregarious instinct.

[2] Morgan, *Brit. J. of Psychol.*, 3., p. 220.

animal instincts. It is small wonder, then, that the upholders of the physiological view fail to observe that the human individual merely passes through the stage of animal conduct just as he passes through the stage of simpler structural developments, and that the mature person is equipped with an entirely different series of reaction patterns than the animal or child. And so we find that, contrary to our expectation, the fact that the complete absence of instincts in the human individual forces us to resort to animal behavior in order to study them, does not influence the biological abstractionist to reflect upon the differences in the two kinds of behavior, but instead he is led to interpret human conduct in the same abstract terms as the simpler kinds of behavior. Incidentally, the failure to distinguish between the instinctive conduct of man and the instincts of animals results in the ascription of a degree of intelligence to animal conduct which is really not found there.

Probably the most serious defect of biological abstractionism is that it obscures the extremely dynamic character of human behavior. The principle of rigid neural functions is entirely inapplicable to the spontaneous and developmental aspects of our conduct, and favors the neglect of the stimulating circumstances which greatly modify it.

Psychological Simplification.—The unfortunate phase of the protest against biological abstractionism is the psychological simplification of human behavior, which reduces instinctive conduct to the functioning of psychical dispositions or impulses. As represented by McDougall and his followers, this view stands as a justifiable criticism of physiological abstractionism, but in its espousal of the subjective position as over against the objective, that is to say, the position of action and behavior,[1] it is hindered from interpreting instinctive conduct as it actually functions. Psychologists who are influenced by this viewpoint are unable to depart from a structural or content description of human behavior; they are prevented from conceiving of the complex non-rational conduct of man as the product of an intricate give and take

[1] *Cf.* Drever, *op. cit.*, p. 16.

process between persons and the social institutions which constitute their *milieu.* When such complex behavior is interpreted as an empirical consequent of numerous human conditions, we can readily see that religious conduct[1] cannot be 'a very complex and diversified product of the coöperation of several instincts,' that is to say, a 'compound' of simpler emotions. To describe religious behavior as the manifestation of a complication of simple mental elements is to forego the scientific advantage of observing the ramified interaction of persons with their surrounding political, economic and cultural institutions. The unwarranted simplification of human behavior means that instead of analyzing the social process in which are developed the deep-seated action patterns, the latter are gratuitously assumed as permanent elements of human character. The situation is not at all improved by asserting that complex 'impulses' are developed from simple 'impulses.'[2] To deny that instinctive conduct is socially formed reaction systems is to revert to the simplicity of the rustic who protests against the daylight-saving law as an interference with God's time.

A functional viewpoint of behavior, we submit, avoids completely the three insidious tendencies which we have just examined. Since the functional psychologist assumes the data of psychology to be concrete adaptational responses to surrounding things, he can whole heartedly reject all putative powers and elements, and confine his labors to the analysis of verifiable materials, such as human reaction systems are. Abiding by such a policy, a psychologist is *ipso facto* barred from an impatient out of hand solution of difficult problems. Especially in the matter of instinctive conduct, a functional viewpoint may lead to a scientific and significant, if tentative interpretation of an important series of psychological adaptations.

[1] Or religious emotion, *cf.* McDougall, 'Soc. Psychol.,' pp. 81–82, 89.

[2] "If we accept the doctrine of the evolution of man from animal forms, we are compelled to seek the origin of religious emotions and impulses in instincts that are not specifically religious."—McDougall, 'Soc. Psychol.,' p. 89.

IMMOBILITY: AN INQUIRY INTO THE MECHANISM OF THE FEAR REACTION

BY J. P. M'GONIGAL

Judging by the uncritical spirit in which psychologists have received recent physiological conceptions, it would seem that the long discussion of emotion precipitated by the James-Lange theory had exhausted the interest of the psychologists about the time that it secured the attention of the physiologists. The current conception of fear is a case in point. To Darwin, Ribot, Mantegazza, Mosso, Lange and others fear, with its dual impulses of flight and immobility, was the most baffling of the major emotions. Today one finds it generally spoken of as a complex of the impulses of flight and attack, the once perplexing impulse of immobility receiving scant, if any, attention. This shift in opinion is traceable to the demonstrations of the physiologists that under the influence of either fear or rage the organism undergoes profound physiological modifications which prepare the body for sustained motor activity.

Now, it is not the business of physiologists to analyze subjective states and psychologists have no right to quarrel with such expressions as "the animal was either frightened or enraged." If they were not prepared for such inexactness in matters of importance to them, the very tone of the physiological literature should have warned them. The physiologists have been, very properly, absorbed in tracing the interrelations of certain obscure mechanisms which produce, among many other intricate effects, emotional reactions, the particular *kind* of emotion being of secondary interest. Not so the psychologist. To him, fear neither looks nor feels like rage. Fear is a more primitive reaction, it appears earlier in the life of the human infant, it is more uniform and definite in its manifestations, less adaptive and more widely distributed throughout the animal kingdom. Such being the case, it would be strange, indeed, if the mechanism of fear were not sharply differentiated from that of anger. In the present paper the attempt is made to define the physiological basis of fear, to show that it is distinct from that of anger and to vindicate the earlier psychologists in their belief that the fundamental impulse in the emotion is that of immobility.

For the purposes of comparative study we shall define fear as the typical reaction to situations of imminent or potential danger. Tracing this danger reaction downward from man to the lower orders, it will be found that the impulse to hide, crouch down, stand still, etc., becomes more and more pronounced until, in some insects, we reach the trance-like state of the death feint. The term 'death feint' is not only inaccurate but is used with misleading looseness to cover both the tetany of the lower orders and the remarkable state of relaxation seen in birds and animals. The reaction is, of course, not a feint at all. There is seldom any resemblance between the postures of a feigning animal and one really dead, while the implication of a purpose to deceive is obviously absurd. The reaction is, in fact, one of the least adaptive and most difficult to account for teleologically in the whole range of behavior. In the first place, the strength and duration of the response bears no relation to the stimulus. In some insects the slightest stimulus will cause a profound feint which cannot, thereafter, be produced by stimuli of any strength. In other species the insect can be made to feign as many as fifty times in succession, the duration of the feints increasing for the first few times and then rapidly decreasing, the stimuli being uniform throughout the experiment. The distribution of the reaction among the species is as erratic as its character. In closely allied forms, one will possess the reaction and others will not. This last observation applies also to the relaxed form of the feint found in birds and the higher animals. *Some* species of *all* orders possess the characteristic, but it must, on the whole, be considered an exceptional manifestation. The very fact of this wide and erratic distribution would seem to indicate the existance of some mechanism common to all animals, which under certain conditions assumes the form of the death feint, but which in most cases, we must suppose, functions in some less conspicuous way.

The possibility of such a hidden existence of the mechanism is shown by an experiment of Holmes. In the lower orders, contact stimuli very generally cause reversal of tropisms. Holmes found this to be true of *Ranatra when in the water*. On land, however, the same stimulus produced the death feint, upon revival from which the insect showed the usual reversal.[1] In its customary environment there would have been no reason to suppose that *Ranatra* possessed the ability to feign. That a similar mechanism may, in this way, lie hidden behind the simple hesitancy of the startled individual of the higher orders is at least suggested by the gradation of the response in birds. Chicks, ducklings and plovers will, upon being startled, crouch down, pheasants stop dead and hold rigidly the posture in which they happened to be at the moment of fright; terns, peewits, land- and water-rail are 'feigners' that lie limp and allow themselves to be handled without showing signs of life. We shall assume, then, for the time being, that an unitary principle runs through all the various forms of danger response and proceed to take advantage of the opportunity afforded by its extreme expression in the death feint to study its mechanism.

As it appears in the lower orders, the death feint is not a state of mere quiescence. The muscular tension is often tremendous. A feigning *Ranatra* may be held horizontally by one of its slender legs, the whole weight of its body being sustained by the tiny muscles. Here, clearly, is a sharp redirection of nervous energy, which, since the posture may be retained for long periods, must depend upon some profound modification of the physiological state. Evidence of the same nature is given by Holmes, Towle, Yerkes, Parker and the Severins who have observed that upon revival the feigning insects show reversal of tropisms, indicating a change in sensitivity difficult to account for if the primary reaction is a mere reflex. Pointing in the same physiological direc-

[1] Holmes: 'Studies in Animal Behavior,' p. 104.

tion are the results of the Severins on the duration of successive feints in *Belostoma*. In a group of ten insects the average duration of the first thirteen feints were as follows: (time in minutes) 15.0, 32.4, 37.2, 24.0, 16.0, 15.9, 19.5, 12.8, 15.0, 9.1, 9.1, 5.4, 5.5. Specimen *A* of this group was made to feign fifty times for a total of 382 minutes, of which 256 were accounted for by the first ten feints. This distribution of time is typical.[1] Graphs of the feints of specially good feigners (made by the present writer) all show sharp peaks at the beginning and then a slow decline with a rhythmic tendency, the whole picture suggesting a reaction conditioned by some autocoid of which the supply is limited as in the case of adrenin. A similar suggestion is supplied by the reaction in birds. Lloyd Morgan mentions a water-rail that changed in an instant from a state of complete inanimation to the most violent and frenzied rushing about, while Holmes makes the same observation in regard to terns. Holmes could never get the birds to feign a second time. Remembering that the passage from the feint to the frantic struggles and blind flight occurs spontaneously and without any transitional stage, it would appear that the second state resulted from the exhaustion of some form of energy which served to maintain the first.

These various conjectures (of which we have no definite confirmation) that the death feint is the result of an unusual functioning of a mechanism common to all animals, and that it is due to the redirection of energy caused by some specific agent of the nature of an autocoid, are given support by the fact that under certain abnormal conditions a reaction strikingly similar, even in details, can be observed in the higher animals and man. This is the tetanic state produced by strychnine poison and tetanus toxin. Under the influence of these poisons the body becomes tense and rigid, the paroxysms, which are most easily induced by some form of contact, lasting, in man, usually about fifteen seconds. Two theories are advanced to explain the action of the poisons.

[1] Severin, Henry H. P., and Severin, Harry C.: 'An Experimental Study of the Death-Feigning of *Belostoma* (-*Zaitha* Aucct.) *flumineum* Say and *Nepa apiculata* Uhler,' Behavior Monographs, No. 3, p. 11.

The first states that the toxin increases the conductivity of the nerves to such an extent that coördinated activity becomes impossible, the chaos of currents resulting in tetanic rigidity of the muscles. The second theory, that of Sherrington, maintains that the reciprocal inhibitions normally maintained by the central nervous mechanisms are changed by the poisons to excitations. By using carefully graded doses of strychnine Sherrington was able to produce a state in which reflex relaxation was diminished and not replaced by excitation. "This phenomenon," he says, "shows well how little competent is the view of lowered spinal resistance to really explain the action of strychnine, for at this stage the stimulated arc that normally acts on the extensor muscle by inhibition is less able to affect it than before, so that on the spinal resistance view the resistance at this stage is actually heightened."[1] Sherrington's theory, in other words, attributes this pathological 'immobility' in the higher forms to a *redirection* of nervous energy analogous to that which the general nature of the reaction and the fact that it is accompanied by the reversal of tropisms, led us to assume in regard to the lower orders. Now, while the use of inducted poisons is a legitimate method of uncovering the mechanism of immobility, we have, fortunately, a more direct avenue of approach through the disorder of tetany, a disturbance similar to tetanus but milder in form. Tetany is exhibited in many diseases and is now generally believed to be of parathyroid origin. This view is based upon the undisputed fact that deprivation of the parathyroids produces tetany and that administration of parathyroid extract relieves the disorder. Schafer, who represents the later work on the endocrine organs, believes that this may be explained by the cholonic or restraining action of the parathyroid secretion which offsets in the normal animal the action of an autocoid of opposite tendency produced by the thyroid (thyrine).[2] Here we have an autocoid which, by redirecting the normal flow of nervous energy, produces a state of rigidity

[1] Sherrington: 'Integrative Action of the Nervous System,' p. 108; *vide* p. 106–13, 292–7.

[2] Schafer: 'The Endocrine Organs,' pp. 23 and 40.

and muscular tension that precisely duplicates the conditions (known and surmised) of the typical death feint of the lower orders.

In the healthy organism, of course, the existence of this mechanism of immobility is concealed by the action of the parathyroids. Is it merely concealed? It might be suggested that the action we have just described is totally abolished in the normal individual and plays no part in the danger response of the higher orders. Yet, undoubtedly, animals and men are occasionally 'paralyzed with fear' and a comparison of the readily recognizable 'expressions' of fear and the known effects of thyrine will show that in the less violent

Expressions of Fear[1]

(Expressions due to the skeletal musculature are omitted)

Quickened heart beat	Thyrine
Arrested respiration	Thyrine
Pallor	Adrenin
Perspiration	Thyrine
Immobility	Thyrine
Tremors	Thyrine
Dry mouth	Thyrine
Arrest of menstruation	Thyrine[2]
Arrest of lactation	Thyrine[2]
Protruding eyeballs	Thyrine[3]
Dilated pupils	Thyrine[3]

[1] Authorities: For expressions of the emotions—Darwin: 'Expressions of the Emotions in Man and Animals;' Ribot: 'The Psychology of the Emotions.' For effects of the autocoids—Biedl: 'The Internal Secretory Organs;' Schafer: 'The Endocrine Organs;' Gley: 'The Internal Secretions;' Cannon: 'Bodily Changes in Pain, Hunger, Fear and Rage;' Crile: 'Man—An Adaptive Mechanism.'

[2] While there is very little known in regard to these points there is an undoubted connection between the thyroid and the phenomena of lactation and menstruation. Ott and Scott state that iodothyrine inhibits lactation while Hertoghe claims the exact opposite. (Bell: 'The Sex Complex,' p. 89.) During menstruation there seems to be evidence of thyroid deficiency, since psychic disorders due to the disturbance are relieved by the administration of thyroid extract. Biedl believes that the tetany sometimes associated with menstruation, lactation and pregnancy is due to metabolic disturbances set up by the functioning of the female organs. (Biedl: 'The Internal Secretory Organs,' p. 44.) The arresting of menstruation in fear may possibly be due to the diversion of the thyrine from the genital organs. This, however, is pure conjecture. Ott and Scott also claim that adrenin inhibits lactation. I know of no record of any effect of adrenin in delaying or promoting menstruation.

[3] Also characteristic of adrenin but in a less degree. These are the most marked and distressing features of exophthalmic goiter, a disease of hyperthyroidism.

forms of the emotion there is clear evidence of thyroid activity. In making this comparison we should be on the lookout for manifestations of adrenal activity, for it is to these glands that current theory ascribes the organic basis of both fear and anger.

The chain of evidence seems complete. Starting with the death feint as an exceptional primitive form of the fear reaction, we were able to discover traces of a similar mechanism through the various orders up to man. In man our material pointed to the identity of this mechanism with the thyroid. Then by a comparison of the known expressions of fear and the known effects of thyrine we were able to show that the thyroid plays an overwhelming part in the production of the emotion, and, since the unrestrained functioning of the thyroid produces a state of complete immobility, it follows that the fundamental 'impulse' of the emotion must partake of this character. To complete our argument for a specific organic complex we shall proceed to a similar comparsion of the expressions and autocoids observed in anger.

Expressions of Anger

Flushed face	Thyrine
Deep respiration	Adrenin
Muscular contraction	Adrenin
Salivation	Adrenin

It is significant that the visceral expressions of anger are meager as compared with those of fear. Ribot says, "Anger and fear form an antithesis, but the former has, both physiologically and psychologically, a more complex character."[1] Darwin makes a similar assertion and later observers have been baffled by the paucity of the expressions of anger. The effects of adrenin betray a similar complexity. On the whole, it is accurate to say that adrenin speeds up the conversion of energy and prepares the organism for violent effort, increasing the contractibility of the muscles, immunizing against fatigue, promoting coagulation of the blood, etc. It places the body, as Cannon tersely puts it, "upon a war footing." It energizes and exhilarates, whereas thyrine depresses and disorganizes.

[1] Ribot: 'Psychology of the Emotions,' p. 220.

It is true that in regard to the constriction and dilation of the facial vessels these tables show a contradiction. It is difficult to account for this, though Cannon has suggested that the effect may be due to a purely mechanical result of blood pressure.[1] However, it would not affect our argument if it were shown that there were even more crossing of effect than this. It is not contended that the immobility reaction wholly accounts for the affective state of fear in the higher orders. An impulse to flee is distinctly recognized and in flight the energizing qualities of adrenin would be of decided service. Under the circumstance the organic complexes of the two emotions are surprisingly clear cut.

[1] Cannon: 'Bodily Changes in Pain, Hunger, Fear and Rage,' p. 227.

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THE PSYCHOLOGICAL REVIEW

CHANGES IN SOME OF OUR CONCEPTIONS AND PRACTICES OF PERSONNEL[1]

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The term *personnel* implies a contrast relationship to material. Just as material includes all the material equipment owned or used by an institution or organization, so personnel signifies all the individuals connected with such an institution or organization. If we define applied psychology as the science and art of the control of human behavior, a large proportion of the field of applied psychology is denoted by the term personnel administration.

Those of us who are engaged directly in personnel administration in such organizations as educational institutions, the army, the navy, industry and commerce, use the term personnel administration to include securing, testing, selecting, hiring, placing, training, supervising, disciplining, stimulating, directing, transferring, discharging and promoting each individual concerned; and in developing the morale, increasing the *esprit de corps*, and creating and sustaining a contented and efficient group of individuals.

In this sense personnel administration is as old as human institutions. The practice of personnel could not be postponed awaiting an adequate theoretical foundation. Every bad personnel practice doubtless resulted from a false conception and reflects itself in strengthening rather than in weakening the error. This vicious circle is counterbalanced by the fortunate circumstance that advanced personnel prac-

[1] Address of the president, before the American Psychological Association, Cambridge Meeting, December, 1919.

tice results from true conceptions and reflects itself in strengthening and purifying the truth. Theory and practice here as elsewhere are inseparable.

Because of the importance of personnel administration, because of its complexity and difficulty, and because of the very large amount of attention given to its various phases, no profound and lasting changes in either the conceptions or the practice of personnel are brought about suddenly or by a single one of the numerous workers in this field. However, significant developments are being brought about from the combined efforts of progressive workers.

During the nineteenth century many advances were made in our conception of the material world and in our practice of dealing with its various factors. The twentieth century is characterized by an appreciation of the personnel problem, by the possession of the behavioristic point of view in psychology, and by the presence of numerous trained experts devoting their energy to the development of the concepts and practice of personnel. Constantly increasing numbers of the members of the American Psychological Association are making contributions to the field of personnel. It may be contended that no other group of experts equals the members of this Association in the number and in the importance of the contributions made—certainly to the theory, and possibly to the practice of personnel. It is too early in the century to present the various contributions with adequate prospective, and to estimate their importance. My aim at this time is to call attention to certain typical changes that have come about in our conceptions and practices of personnel and to encourage the members of this association to renewed energy in producing further changes.

The first of these changes to which attention is called is the conception of the *Equality of Men* and the practices associated with that conception. In the ancient Athenian thought emphasis was put on the superiority of the Athenians and the comparative inferiority of all other races. The associated practice is found in the enslavement of foreign races and in the custom of according equal but unrestricted honor to all the citizens. No citizen was assumed to be superior to any other. Accordingly the casting of lots decided which one from among all the citizens was to be chosen to judge a dramatic or musical festival, to preside at court, to be a legislator, or for one day at least to hold the highest office in Athens, or to be an archon for a year and then a permanent member of the Aræopagus.

This same insistence on the equality of men was dominant in the writings of many of the great thinkers for two centuries. It found expression even in the writings of such a Royalist as Thomas Hobbes, as is indicated by the following quotation from his "Leviathan": "The question who is the better man has no place in the condition of mere nature; where, as has been shown before, all men are equal. . . .

"Nature hath made men so equal in the faculties of the body and mind, as that, though there be found one man sometimes manifestly stronger in body and quicker in mind than another, yet when all is reckoned together the difference between man and man is not so considerable as that one man can therefore claim to himself any benefit to which another may not pretend as well as he. For as to the strength of the body, the weakest has strength enough to kill the strongest. . . . As to the faculties of the mind, I find yet a greater equality amongst men than that of strength. For Prudence is but Experience; which equal time equally bestows on all men, in those things they equally apply themselves unto. That which may perhaps make such equality incredible, is but a vain conceit of one's own wisdom, which almost all men think they have in a greater degree than the Vulgar, that is, than all men but themselves, and a few others, whom by Fame, or for concurring with themselves, they approve. . . . But that proveth rather that men are in that point equal, than unequal. For there is not ordinarily a greater sign of the equal distribution of anything, than that every man is contented with his share."

It was no mere accident that Thomas Jefferson wrote into our first official document the doctrine that "All men are created equal." He was doubtless thinking primarily of men in their political relationship, but there seems no reason to interpret his meaning as confined to political relationships. The associated practice of universal citizenship must not be thought of as an isolated phenomenon but is to be grouped with all practices arising in connection with the theory of the equality of talents, rights, and responsibilities.

The theory of the equality of men as related to education is strikingly presented in the following quotation from Adam Smith: "The difference of natural talents in different men is, in reality, much less than we are aware of; and the very different genius which appears to distinguish men of different professions, when grown up to maturity, is not upon many occasions so much the cause, as the effect of the division of labor. The difference between the most dissimilar characters, between a philosopher and a common street porter, for example, seems to arise not so much from nature, as from habit, custom, and education. When they come into the world, and for the first six or eight years of their existence, they were, perhaps, very much alike, and neither their parents or playfellows could perceive any remarkable difference. About that age, or soon after, they came to be employed in very different occupations. The difference of talents come then to be taken notice of, and widens by degrees, till at last the vanity of the philosopher is willing to acknowledge scarce any resemblance."

The conception of the efficacy of education in modifying the inherent equality of men is responsible for much of the good and much of the bad in American educational practices. Among these may be cited, on the one side, our universal compulsory education and, on the other side, the introduction into the colored schools of the South of educational practices from the classical preparatory schools of the North.

An unquestioned acceptance of the concept of the equality of men results in inefficiency wherever applied. In the army it results in seniority promotion. In labor unions it results in an insistence upon an equality of wages for all the workers of a craft. In popular thought on matters of social control it leads to communism and syndicalism. In industry it results in the shaping of jobs to suit the capacity of the average man, with the consequent elimination of adequate stimulus to action for the superior individuals. The concept of the equality of all normal adult men is a psychological error that has perverted the thinking and weakened the action of all peoples inspired with a true and worthy ideal of democracy.

Possibly the greatest single achievement of the members of the American Psychological Association is the establishment of the psychology of individual differences. You have discovered that normal adult men differ greatly in all human capacities and attainments. You have demonstrated that such differences are much greater than had ever been imagined. You have found that individual differences are relatively small in such matters as height, weight, physical strength, and reaction-time, but that normal adults differ enormously in the so-called higher mental qualities. Guided by this new conception of individual differences you have entered the schools and insisted that pupils be grouped by their mental ages rather than by their chronological ages. You have entered the army and urged that enlisted men be assigned according to their fitness for army tasks rather than by the location of their place of enlistment. You have insisted that commissioned officers be promoted according to merit rather than by seniority. You have coöperated with progressive labor unions in developing a conception and practice adequate to provide protection for the weak and opportunity for the strong. You have entered industry and insisted that applicants be accepted according to fixed standards; that workers be promoted according to attainments and that each employee be inspired by the particular stimulus most effective for him. Your gospel of diversified talents is permeating our national thought and indicating, on the one hand, the wisdom of a democracy utilizing experts in all fields and, on the other hand, the hazard of all methods of social control based on the assumed equality of normal adults.

A second change in our conception and practice of personnel administration is seen in the decreasing importance ascribed to *Reason* as a factor in determining human action. For many centuries man was defined as the reasoning animal. Aristotle's "Logic" was the standard textbook for all students desiring to learn the best method of influencing and controlling men. For persuasion the syllogism was believed to be the most perfect tool. Arguments to be effective must be analyzed and presented in a logical form. The hearer was supposed to criticize appeals by the most rigorous of logical standards.

The change in this point of view has come about so gradually that we fail to appreciate its extent. At the hands of certain authors the importance of reason is minimized by an emphasis upon suggestion as descriptive of the process of influencing men. Others contrast reason and instinct, urging the important part instincts play not only in the behavior of young children but also in the more important acts of adults.

Still other scientists stress the significance of sentiments and emotions, of impulse and habit, or of other forms of human response not reducible to any standard type of reasoning. This change in our concept of the importance of reasoning is observable in the writings of modern psychologists, and is reflected also in the practices of the more progressive leaders in personnel administration.

The folly of treating workers merely as reasoning animals but the wisdom of recognizing the importance of sentiment is strikingly illustrated by the following instance:

The workers in the men's clothing industry, in Chicago, were discontented last spring, because of various conditions in the industry. To reduce this discontent some of the companies increased wages 10 per cent. Company X. posted a notice that on July 1, each worker who remained loyal to the firm until June 13, would receive "a special extra pay envelope." This promise failed to change the attitude of the workers. A few weeks after the posting of this notice the drive was on for the sale of Liberty Bonds and the President of Company X. purchased $34,000 worth of the bonds as a gift to his employees. Each worker was given a coupon good for his share of the $34,000 worth of bonds. The workers manifested no appreciation of this gift. On July 1, each worker received a special extra pay envelope containing a sum of money equal to that which he had received on the second week in May—a typical week. This generosity resulted in expression of discontent among the rank and file of the workers. The president of the company was much disappointed by the failure of his program and called into conference on the subject the local labor leader. I was asked to be present also. The following is the substance of the conversation between the president of Company X. and the labor leader.

President X: "I can't understand the lack of appreciation of my men. I gave them $34,000 worth of Liberty bonds and a special extra pay envelope of a full week's wages. The union agreement has now put all the firms on an equal wage basis. Although I did not increase wages 10 per cent. for the period preceding the union agreement I have given my men more than any other company by the extra pay envelope and the Liberty bonds. I can't see what more they want."

The Labor Leader: "Yes, Mr. X, you have done all you say and your people are not contented as the people are at the other houses. They wanted the 10 per cent. and felt that they had deserved it."

President X: "No, I did not give them the 10 per cent., but I did give the extra pay envelope and the Liberty bonds which amounted to much more than the 10 per cent."

Labor Leader: "Yes, I have figured it up and you gave them in extra pay and bonds somewhat over $10,000 more than they would have received by the increase they asked. But that is not what they wanted. They do not want the gift of the extra pay envelope and of the bonds but they do want the 10 per cent. even if it is less than the extra pay and the bonds. I believe they would be willing to refund the $34,000 worth of bonds if you would give them the $24,000 in what they regard as earned wages."

President X: "Very well, I will gladly make the exchange for I shall thereby gain $10,000.

Labor Leader: "I think the discontent will be greatly reduced by the exchange. I will take it up with the people at once."

The proposition was presented to the workers and was accepted enthusiastically even though it entailed a recognized monetary loss of $10,000. However, it restored their offended pride and left them happy.

The President reasoned as follows:

Major premise, All wage earners prefer the greater to the lesser amount of money.

Minor premise, The extra pay and the bonds is greater than the 10 per cent. increase.

Conclusion, Therefore, the workers prefer the bonds and the extra pay.

The experienced labor leader recognized that working people are influenced as much by pride and by sentiment as by the logic of the greater gain. He knew that strikes and demands for more pay and shorter hours are frequently but a defense reaction against offended pride, and that the rational interpretation placed on such action is usually as false as the interpretation of President X upon the actions of his men. The industrial leader who seeks to comprehend and to lead his men to-day finds little assistance in Aristotle's logic or in any conception that stresses the logical reasoning ability of man. He does, however, receive great assistance from the newer emphasis on the non-rational aspects of human actions, as expounded by the members of this Association.

The third betterment in personnel administration to which attention is called is that of the change in our conception and practice of *Education*. To the popular mind education is frequently assumed to be identical with learning, with the acquisition of information more or less useful, with the committing to memory of the deeds and thoughts of forefathers more or less worthy, and with the perpetuation of the classical culture whether that be interpreted to mean Greek, Roman, Hebrew, Turkish, Chinese, or Germanic.

Education is thought of as esoteric,—as a thing quite apart from everyday life. It manifests itself as culture for culture's sake, as art for art's sake, or as pure science uncontaminated with any possible practical results. A deep gulf is assumed to separate learning and doing, theory and practice, the school and the shop.

All these conceptions and distinctions cease to be significant as soon as we take the modern behavioristic point of view and define education as profiting by experience. Training is the result of the reactions made by the individual being trained. The laws of Solon may be 'imparted' to a phonographic record, a parrot may repeat certain phrases of the Koran and an imbecile can commit to memory the significant dates of Roman history. These instances are not descriptive of education as we think of it today because they are not instances in any real sense of profiting by experience.

In playing with fire the child secures training which in the fullest sense is education. He learns to set up a series of withdrawal reactions and to profit thereby in his increased ability to establish this particular form of reaction when facing similar situations.

The youth receives training in solving a mathematical problem if in solving it he has acquired a new way of analyzing a mathematical situation and can make use of it when such situations arise again. He receives training in the reading of Homer if in the study he acquires a new form of reaction and is enabled to profit by this new possibility of action whether it be in additional reading, in appreciating literary style in another author, in improved diction on his own part, or in comprehending human action.

The mechanic at the bench may be receiving an education if he is profiting by his experience. In solving a mechanical difficulty he may be acquiring a new form of thought that may be repeated more readily when a similar situation is met again. As a member of a group of workers he may acquire a form of social response that may appear more readily and be more effective with each repetition. As a member of a shop committee his contact with the employer's representative may change his entire point of view on industrial, social, and political philosophy. In dealing with subordinates he may accidently or thoughtfully acquire a type of reaction that fits him for more important executive duties. The reading of a magazine article, the action of an associate, the chance juxtaposition of two pieces of material may cause him to think a new thought or perform a new act which may better equip him to meet new and important duties. The significant thing about these new reactions is that they are new and can be repeated with benefit.

According to this conception there is no rigid demarcation between school and society, between the pupil's desk and the employee's bench or counter. In all these instances the individual has experiences. Whether such experiences should be classed as education or not depends less on the particular geographical location than on the response resulting from the experience.

When education is defined as profiting by experience, the personnel director is faced with the double responsibility, first, of providing educative experience, and second, of assisting the individual to obtain the maximum of profit from the experiences provided. The worker in repetitive manufacturing processes may not be provided with adequate educative experiences and the student in college may not be profiting sufficiently by his experiences no matter how rich they be in potentialities.

Our educators in institutions of learning are aware that richness of content does not guarantee educative response on the part of the student, so adequate responses are sought as an essential part of all courses of instruction. In business organizations need for varied experiences for each individual is beginning to be recognized. Steps are being taken to provide this variety by teaching the worker not merely one job but by teaching him many jobs or by providing richness of content in other ways.

In planning his training program the personnel director is coming to see that his responsibility is not met by providing formal classroom continuation school instruction for the youths who are ambitious or for others compelled by law to take such instruction. His is a greater responsibility and ideally demands that he should supply each employee with richness of experience and provide also that each worker should profit continuously by his experiences as an individual worker, as a member of the entire body of employees, as a prospective junior executive, as a member of a family, and as a citizen of the state.

A fourth change to which attention should be called is that of the emphasis of the *Biological Relationship* existing between the worker and his work. For a long time we have used the evolutionary-biological point of view in interpreting the relationship of man and the world in which he lives. We have gradually ceased to think of man and his environment as two contrasted and more or less independent entities. We no longer think of him as the result of a special creation coming into his world and subduing it. On the contrary, we think of man as a result of an evolutionary process in which man and the world were mutually involved. The general biological point of view is stressed in anthropology and sociology but rarely in attempts to interpret the industrial worker. This very necessary point in evaluating the concepts and practices of personnel administration is clearly expressed in extracts from an unpublished report of the Scott Company Laboratory on 'A Point of View in Industrial Personnel.'

"In order to attain an insight at all adequate into the field of industrial personnel, we must abandon any statement which contrasts, or which even sets off against one another, men and jobs. On the contrary, we must see clearly that the industrial situation, the productive complex, is organically not two things, but one; not men and over against them jobs, but in reality *workers-in-their-work.* In this active, shifting unity, this intangible, ever-changing reality, the vital problems of industrial adjustment exist.

"There is a basic difference between this concept and the older point of view toward personnel. The latter notion is perhaps most crudely stated as 'Putting square pegs into square holes.' For the sake of brevity and concreteness, let us call this the 'square peg' concept. Clearly, there is a family resemblance between the square peg concept and the statement that personnel work consists of man-analysis, job-analysis and the bringing of man and job together. Man-analysis is essentially discovering the shape of the peg; job-analysis is essentially discovering the shape of the hole. The phrase *The Right Man in the Right Place* is the slogan of a personnel philosophy of the square peg variety.

"The inadequacy of the square peg concept arises from its sharp discrimination between man and job—man on the one side, job on the other side, with an act of bringing man and job together. Here we have drawn for us two separate entities, static, self-sufficient; our task seems to be to make as good fits as possible. The coldness, the rigidity, the sterility of this point of view is evident; it is not surprising that it results so frequently in a mechanically impersonal, jigsaw-puzzle attitude toward the problems of industrial personnel. To fit the right man in the right place, to put the square peg in the square hole, these are relatively fruitless concepts for meeting the plastic, living problems of industrial adjustment. . . .

"Our point of view differs from the square peg concept in that it shows the worker-in-his-work as a unity, a developing, living situation, as a productive complex, organically one. We do not think of the hiring of the worker as the connecting of a man with a job; it is the creation of a worker-in-his-work situation, the birth of a new productive complex. We do not think of the release of a worker as the separating of a man from his job; it is the destruction of a worker-in-his-work situation, the death of a productive complex. Square peg philosophy regards as fundamental, structural diversity, the man and the job, the worker and the work. Our point of view regards as fundamental, functional unity, the man-in-his-job, the worker-in-his-work. . . .

"The problems of industrial adjustment are conceived to be not so much those of fitting together worker and work, but rather those of securing the healthy development of an organic unity, the worker-in-his-work. The worker-in-his work is viewed as a living and changing situation, as the functional element of the great industrial and social organism, plastic and unstable. . . .

"Personnel work involves the shaping of the growth of this productive complex in forms of greatest economic effectiveness and ultimate social value."

As a final point attention is called to the change that is being brought about in our conceptions and practice of *Vocational Guidance.* The caste system of India, and allied systems in other lands, the European guilds of the middle-ages, inheritance, opportunities for jobs available in the vicinity, social approval and disapproval of certain occupations—these are among the factors that have been dominant in vocational guidance. Such general, non-commercial, and unadvertisable systems have been unable to retain a monopoly on vocational guidance. There have sprung up special systems, proporting to be infallible, in the guidance or the selection of individuals. These systems have had great vogue in all ages and today many of them are being sold to the so-called hard-headed business men in every city in America. In the list of such so-called "infallible systems" are included the following: astrology, augury, chance as manifested in drawing of straws, casting of lots or the flipping of a coin, chirography, chiromancy, clairvoyance, character analysis, divination, fortune-telling, horoscopes, hypnotism, intuition, magic, mediums, mind reading, necromancy, omens, occultism, oracles, palmistry, phrenology, physiognomy, premonitions, psychological tests, soothsaying, sorcery, sortilege, subconscious hunches, stigmata, talismans, trade tests and telepathy. When none of the systems here cited have been depended on we commonly resort to the judgment of the maiden school teacher, of the indulgent mother, of the ambitious father, of the mercenary employment agent, of the hustling labor scout, or of the listless recruiting officer. Vocational guidance has been on an utterly unscientific basis and has been wholly inadequate. However, no great improvement could be expected until a comprehensive job analysis of available vocations had been made, until a technique of testing individuals had been provided by experimental psychology and until the point of view of the biological unity of the worker-in-his-work had been recognized. During the past few years fairly adequate job analyses have been made of most positions in many of our industrial and commercial organizations. Year by year progress is being made by our laboratories in measuring the talents, capacities, and skill of individuals. Great advance has been made in our understanding of human nature and in the creating of a practical biological point of view of the worker-in-his-work. The advance in vocational guidance may be adequately symbolized by the change from the stage in which dependence was placed on the casting of lots to the stage in which dependence is placed on the tonoscope and similar instruments of precision; or from the stage in which results were expressed in the ambiguous mutterings of mediums to the stage in which results are expressed by coefficients of correlation and by regression coefficients or by other exact statistical formulations.

Of the changes in our conceptions and practices of personnel administration mention has been made of five that are more or less typical of the many that might be cited. The importance of these changes is very great, both for the development of the science of psychology and for the welfare of the human race. It has been estimated that during the nineteenth century the power of the human race to produce food, clothing and shelter was doubled by the application of increased knowledge of the material elements of the universe. All the significant advances in knowledge of the material world were brought about by possibly a few thousand progressive minds devoted to that study.

It is quite probable that the productive power of the human race is being doubled again during the present century. The benefits of this advance will be divided between better adjustments of the material world to the needs of man, and the better adjustments of man to man. Such an increase in the efficiency of the race will probably be due to the advance in our knowledge of personnel rather than to further increase in our knowledge of the material universe. If a few thousand men in their study of the material world served their science and the race so effectively, those of us who are engaged in the study of personnel may get a glimpse of the responsibility and the opportunity that is ours.

AN ANALYSIS OF EFFORT

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The concept of will as outlined by the early faculty psychologists has been discarded, and rightly so, by modern genetic psychology. The will as a spiritual entity which served to redirect, supply and control energy for the different responses of an organism not only failed to explain many of the phenomena which gave rise to the formulation of the will concept, but in addition lent an air of mystery which in itself inhibited the scientific investigation of the processes it purported to explain.

The concept of dynamogenesis, which took the place of the will concept, assumes that a sensorial stimulus not only tends to find a direct response of some special sort but affects more or less remotely other parts of the organism. This diffusion of energy from an incoming impulse depends upon the condition of the organism as determined by its entire past history, upon other stimuli acting at the same time, as well as upon the nature of the stimulus itself. Psychologically the previous stimuli and experiences of the organism may be considered as setting the organism in a certain state of readiness or unreadiness. The task (Aufgabe) —the set of the organism, or the directions received—facilitates one reaction and inhibits another. The directions to add two numbers facilitate the adding of the numbers and inhibit any tendency to multiply or subtract. If a man has been placed in an attitude of anger this state of his organism will cause him to become highly irritated by a relatively mild stimulus. Such a simple reflex as the knee jerk is affected by other stimuli acting at the same time, such as martial music, the cry of an infant or other significant noises.[1]

[1] Lombard, W. P., 'The Variations of the Normal Knee-jerk and their Relations to the Activity of the Central Nervous System,' *Amer. J. of Psych.*, 1887, 1, 5–71.

Finally, a single stimulus if pleasant will cause a different reaction from a single unpleasant stimulus. One may smack his lips over a bit of candy but his whole organism may respond in an effort to eject some disgusting bite.

By very carefully marshalling the facts of stimulus and response, psychology has been able to explain most of human behavior by a direct relation of each response to some stimulus more or less remote. Some difficulties have arisen in connection with stimuli which elicited no immediate observable response and with responses for which there were no immediately preceding adequate stimuli. Many of the seeming difficulties disappear when the human organism is regarded as an integrating mechanism, and the working concept of modern psychology is no doubt the concept that every organism is a highly balanced system of forces, whose integrity depends upon the maintenance of a certain balance. Each stimulus is received into this system and is modified by the condition of the balance at that time and its response can only be interpreted in the light of its relation to all its previous experiences as well as to all the other forces acting upon it at that time.

Now, the question at the crux of this whole situation is, what is this tendency to maintain a balance? The term used in the will psychology to express this fact was effort. The individual was presumed to make an effort to maintain his 'personality.' If a hostile stimulus was received he made an effort to overcome it. The moral crisis was the place where this manifestation appeared in its supreme state. It is the sort of conflict which James describes in his classic fifth type of decision.[1] Why is it that "when a dreadful object is presented, or when life as a whole turns up its dark abysses to our view, the worthless ones among us lose their hold on the situation altogether . . . and collapse into yielding masses of plaintiveness and fear"; while the 'heroic mind' holds itself erect, faces the situation, and 'makes himself one of the masters and lords of life'? This question is still present even when the situation is restated in behavioristic terms,

[1] 'Principles of Psychology,' 1899, II., 534–535.

but it is simplified if one recognizes that such a question resolves itself into a query as to why individuals differ in their capacity to maintain their balance. We should expect the trait to vary; what we wish to know is: What is this trait? What is effort? What is meant by maintaining one's balance?

This paper is an attempt to show that *the concept of effort is an elementary principle of all organic life which is as fundamental as the most firmly established reflex.* To do this we will first indicate where effort is best seen, we will show facts which indicate its elementary nature and finally try to describe how the more complex situations in which effort is revealed are developments from this elementary background.

The customary definition of effort is: 'the result or display of consciously directed power.' This definition is itself a product of will psychology and in order to get away from the implications connected with the phrase 'consciously directed' we will have to change our definition to read 'the result or display of organic power.' This is not doing violence to the term. The central idea is the display of power; organic has been used in place of consciously directed. Power may result in either of two situations. (1) When a stimulus is received whose natural response would cause a loss of balance, the stimulus does not give rise to its normal reaction. Either 'power' is exerted by the other forces to divert the stimulus to an unusual response, or it is merely inhibited. (2) In the second case the organism may need to make a certain response in order to maintain its balance. Any opposition to this response will be opposed. The combined forces of the organism will tend to break down this resistance. It is effort in these two senses and not with its will implications which we will attempt to analyze.

Manifestations of Effort

Effort may be seen when an organism is learning to react to a novel situation. When an organism encounters a situation, the situation is in itself a stimulus to activity. If the activity produces a favorable impression the organism continues to respond in the same manner to that situation. If the result is not satisfactory a new reaction is tried. This may be considered an effort to meet this situation—it is the first stage of effort—the connection between stimulus and response is simple and direct; but the effort we mean to analyze is a more complex affair. Suppose the organism is placed in a new situation and makes the traditional random movements in reacting to this situation. If all the movements fail the organism could do one of two things; it could stop its activity and go to sleep or it could increase the violence of its responses to the point where it would be frantic in its struggles. The increase in power displayed in the activity of the organism when its responses fail to solve the situation is what we mean by effort. The situation is the stimulus to random movements; the *situation plus the failure* of the random movements *is the stimulus to effort* or the display of power. The failure is the added resistance in the situation and the effort is the reaction of the organism to meet the resistance.

As a specific example, suppose a dog tried to gain entrance to this room. He could come to the door and make several reactions to the fact that it is closed. He could whine, scratch, push at the crack with his nose and bark. After failing to gain access he could either go lie down or could increase the violence of his efforts until the whole neighborhood becomes disturbed by the intensity of his howling. In this sense effort is a direct response to a stimulus and the stimulus is the amount of resistance that the organism encounters.

Effort may be the response to a specific condition of the organism, such as fatigue, sickness, etc., which renders it difficult for it to make the normal response. An illustration of this rôle of effort is seen in the case of an athletic contest. The athlete starts the foot race as a response to the presence of the crowd and his competitors. Gradually fatigue sets in and the stimuli of his associates and the crowd weaken until he is impelled to stop. He feels as though he would die if he goes any further. He has no stimulus within or without to keep up this useless running but he keeps up impelled by nothing but the stimulus of the opposition his organism is receiving and the habits he has formed of persisting in spite of opposition.

In many different psychological analyses the phenomenon of effort is given a prominent place. In discussing the effects of practice the statement is made by investigators that effort must be directed toward improvement: that, if effort is directed toward increase in accuracy, an increase in accuracy will appear; if effort is directed toward an increase in speed, an increase in speed will appear; or, if effort is directed toward improvement in both speed and accuracy, both speed and accuracy will be improved. Investigators of memory make the statement that the learner must direct his efforts toward memorizing in order to make the memorizing effective. All discussions of attention point out two kinds of attention; involuntary and voluntary. Voluntary attention requires effort on the part of the individual. Successful reasoning is conditioned upon the reasoner holding the problem to be solved against irrelevant suggestions and distractions. This process requires the direction of effort against stimuli which are unfavorable for a proper solution.

Finally, effort is seen in the moral conflicts of the individual. The instincts or the acquired complexes of the individual tend to make him react in some particular way to specific situations. Society says he cannot act in this way without receiving punishment therefor. If the threatened social punishment is able to exert force enough the tendency to act may be inhibited; there may be a balancing of forces with no apparent activity. James describes a case in which social pressure is not sufficient to inhibit activity, where the individual feels that he either will not be caught or that he will be excused in case he is caught. He has nothing to lose by refraining from an act except his moral integrity, and even this loses its restraining potency. The individual feels that by his own wilful act he is making the decision. This is an illustration of the conflict of stimuli and must be carefully distinguished from the opposition to response. Where there is opposition to the normal response it was stated that the situation plus failure in response is the stimulus to effort. Here we have a balance between impulses neither of which can gain the ascendancy. When a decision is made it is usually with a violent impulse and not the slow response which is typical of a nearly balanced physical movement. The decision means the total inhibition of one or the other stimuli. It is evidence that *the balance*, the withholding of response, *is itself the stimulus for the organism to do something.* This stimulus arising from the conflict throws itself finally on one side or the other and a response occurs.

The Elementary Nature of Effort

Suppose that all the organisms in a group reacted in exactly the same way to the same stimulus, in other words suppose that there were no such thing as biological variation. In such a case it would be only through accident that organisms could ever become selective in their reactions. Suppose, however, that within a group two organisms responded just a little differently to the same stimulus. If this stimulus was favorable to the integrity of the organism the one which gave the most ready or the strongest response would tend to preserve its integrity better than the other. Suppose, again, that the stimulus was light and that light was very favorable for the organism in question. Those organisms which responded favorably and most vigorously to a light stimulus would have a better chance to survive than the ones which were indifferent to light. Such selection would eventually give rise to a species which would tend always to respond positively to light. (Positively phototropic). We might say that the organism 'tries' to stay in the light, that it 'exerts' itself in an 'effort 'to keep where it is light, but it can readily be seen that this is but a crude way of stating that through biological variation and selection a positively phototropic species has been evolved.

Again, suppose that from seeds which tended to throw out sprouts indiscriminately a variation arose through which some seeds tended to send sprouts toward moisture. If moisture was favorable to the integrity of the life of the seed the ones which showed the strongest tendency would survive those in which the tendency was lacking. It can easily be seen how through a long period of selection a species could be evolved which would force its roots into the crevices of rocks, even raising tons of weight to do so.

These principles of variation and selection form the groundwork for the development of any biological trait no matter how complex. The question is, can effort be explained as a biological development of this sort? If we can show that effort is a fundamental trait of organic matter at different levels, we believe that biological selection will account for its presence. Biological evolution has been so elaborately expounded in the literature and is so widely accepted that we scarcely need to defend it. All we need to do is to show that effort is a definite response to a certain condition and biological evolution will explain its existence.

1. Jennings[1] has shown that the behavior of lower organisms depends not only on the external stimulus but on what he calls the physiological state of the organism. Physiological states are of two kinds, those depending on the progress of the metabolic processes of the organism, and those otherwise determined. The latter are the ones which concern us. The 'physiological state' is a dynamic condition and not a static affair; it tends to produce movement. "This movement often results in such a change of conditions as destroys the physiological state under consideration. But in case it does not, then the second tendency of the physiological state shows itself. It tends to resolve itself into another and different state. Condition 1 passes to condition 2, and this again to condition 3. This tendency shows itself even when the external conditions remain uniform." For example the stentor[2] is capable of greatly different reactions under the same external stimulation. If the stentor is subjected to a stimulus which would not be injurious unless applied for a long time, if the stimulus and other external conditions remain the same the organism will respond by a series of reactions

[1] 'Behavior of the Lower Organisms,' Columbia University Press, 1906, 282–299.
[2] *Ibid.*, p. 176.

becoming more and more pronounced in character, until by one of them it rids itself of the stimulation. The changes in behavior may be summed up as follows:

"1. No reaction at first: the organism continues its normal activities for a short time.

"2. Then a slight reaction by turning into a new position—a seeming attempt to keep up the normal activities and yet get rid of the stimulation.

"3. If this is unsuccessful, we have next a slight interruption of the normal activities, in a momentary reversal of the ciliary current, tending to get rid of the source of stimulation.

"4. If the stimulus still persists, the animal breaks off its normal activity completely by contracting strongly—devoting itself entirely, as it were, to getting rid of the stimulation, though retaining the possibility of resuming its normal activity in the same place at any moment.

"5. Finally, if all these reactions remain ineffective, the animal not only gives up completely its usual activities, but puts in operation another set, having a much more radical effect in separating the animal from the stimulating agent. It abandons its tube, swims away, and forms another one in a situation where the stimulus does not act upon it."

This situation can be clearly translated into the terms of our thesis. The external stimulus was the cause of the reacting movement; the same external stimulus plus the fact that the movement was not effective in the removal of the stimulus formed the stimulus for a different movement until finally the organism made a very pronounced reaction, involving its whole body, to a stimulus which at first caused no noticeable response. While the variation in reaction at the different stages is important for psychology we are primarily concerned with the fact that there is an increasing intensity in the responses until the stimulus is removed.

2. A simple illustration will show that the same thing is present in animals of a higher order. A child upon being rebuked for pulling the cat's tail replied that he 'was simply holding and the cat was doing the pulling.' A stimulus upon the cat's tail which at first will cause only a slight reaction

will if that slight reaction fails to remove the stimulus give place to the most violent reactions.

3. At birth a child shows the same reaction. Nothing will arouse an infant to struggles and cries of seeming rage as quickly as holding its arms close to its sides so that it cannot move them. The child will resist movements of parts of its organism and will increase the intensity of its resistance if its first movements do not serve to remove the undesirable stimulus.

4. A living muscle adapts itself in its contraction to the resistance it meets. This fact has been known for some time to physiologists. Luciani says;[1] "According to the observations originally made by Fick, and afterwards confirmed by others, when the weight applied to the muscle is not great, and particularly when an elastic resistance is opposed to the muscle, so that its tension increases constantly during contraction, the shortening is greater when the weight and the initial resistance are increased. This paradoxical phenomenon is a specific property of the substance of living muscle, and shows that the sudden pull of the muscle and increase of tension during shortening act as a stimulus on the contractile substance, and increase the effect of the electrical stimulation."

5. It has been found in experiments with human subjects that the force used in pulling a weight is determined by the magnitude of the load.[2] "After one has been pulling a weight of 2,440 grams with what he supposes to be the maximum force he is able to exert, when unexpectedly a weight of 7,770 grams is substituted for the lighter one, his force at the very beginning of the pull is on the average 2.5 times as great as the supposedly maximum force previously used." The time taken for this adjustment ranges from 25 sigma to 91 sigma with an average of 54 sigma. This is much shorter than the simple reaction time, which under the most favorable circumstances can scarcely be reduced to 100 sigma. Since

[1] 'Human Physiology,' Trans. by F. A. Welby, Macmillan, Vol. III, pp. 13, 15, and 46. See also 'The Speed and Accuracy of Motor Adjustments,' by the writer of this paper, *Jour. of Exper. Psychol.*, 1917, 2, 225, 248.

[2] Morgan, J. J. B., 'The Overcoming of Distraction and Other Resistances,' *Archives of Psychol.*, 1916, No. 35, Chap. VII.

this adjustment is so rapid it cannot be a conscious reaction. It must be a reflex or a local muscular adjustment. In either case the adjustment is certainly elementary.

6. In experiments on the distraction of attention it has been found that individuals oppose distractions with increased effort as well as by introducing other factors into their work which will help them to overcome the distractions. These adjustments are not made consciously but reflexly; the subjects often asserting that they do not use the help that their reactions indicate. For instance a customary reaction was to articulate the material used in the process involved. Breathing records were taken which indicated articulation and the subjects were watched through a peep hole. Some subjects who actually moved their lips in articulating denied that they had made any such movements. In such a process as overcoming distractions the exertion of effort is unconscious.[1]

We have seen that the force exerted by an organism is a direct response to resistance encountered. This adjustment is seen in an organism as elementary as the stentor, in higher animals, in infants at birth, in a nerve muscle preparation, in muscle intact in the organism, and finally in complex human activities such as resisting distraction. Using the definition of effort given above we can say that *effort is an immediate response to the stimulus of failure.* Failure is used to mean the persistence of an unsatisfying situation in spite of the normal reaction to that situation.

Let us now see whether complex types of mental effort can be analyzed as derivatives of this elementary adjustment.

Effort in Practice

Let us take first the case of the effort involved in long continued practice. In periods where no improvement is made and a plateau appears in the learning curve, the ordinary incentives fall off and subjects feel tempted to stop. In fact many learners do stop at such points and those who persist do so by what they call sheer will power. In this case no

[1] *Ibid.*, Chaps. I–VI.

resistance is added but the customary drive to activity disappears. When the man starts he sets before himself the goal of becoming proficient in the line of work in which he is practicing. His rapid progress at first enthuses him and stimulates him to work for greater improvement. When improvement comes to a standstill doubts assail him as to the possibility of his ever becoming proficient. His goal seems to fade away and he has no motive to continue except stubborn persistence.

Now if an individual did not have the power of continuing work in the face of obstacles all work would stop at such times. The only way in which we could train ourselves to work would be to learn which incentives are efficacious and how to keep them before us. We all need incentives to start us on a task and we need incentives to tide us over hard places; but as we grow older we should need fewer and fewer incentives, we should have developed this primitive trait of resisting opposition to the point where we can surmount an obstacle without any outside aid.

The ability to surmount obstacles is developed in some such way as this. A young child innately opposes any restraint upon its body or any forced movement of its members. It wants to be free to move without restraint and nothing will arouse a fighting, struggling reaction as quickly as pressing its arms to its sides and holding them there. As it gets older and wants something it will resist any interference in the way of its obtaining the thing it wants. The wise parent or teacher will not fight back when the child struggles for something, but if it should not have the desired article will substitute something else for it, thus reinforcing and redirecting rather than inhibiting the tendency to resist interference. Such training strengthens the capacity of the child to oppose force against resistance. Finally the child comes to the point where it should learn some uninteresting subject. Here the teacher presents proper incentives, attaches derived interests to the work and thus induces the child to pursue the subject. The time will come when the interests will fall off and the child will want to stop. The teacher will add other incentives, give the child a helping hand over the hard place and in so far has trained him to oppose force against resistance. If help is not given at the proper time the child may give up and in doing so has trained himself to submit rather than resist.

It may be thought that the teacher in giving the extra incentives is simply helping the child to form a habit which will enable him to do this particular task. This is not the case. The child's whole growth is a struggle between the tendency to continue a thing once started and the opposition of outer circumstances against continuance. In ordinary cases the tendency to increase effort with an increase in opposition wins the day; but when the opposition becomes too great and the child is about to give in, this is the time the teacher should give the help. Too much help will leave this tendency in a dwarfed state, too little will train it to retreat at the slightest opposition.

Viewed in this light punishment is not as beneficial a factor in training as rewards. Punishment inhibits a certain response and hence works against the tendency to carry through a thing once started. Rewards are a reinforcement to the tendency to carry through a thing regardless of opposition. Punishment if given at all should be coupled with positive direction. Not only should an act in a specific direction be inhibited but at the same time a substitute should be provided and the offender encouraged in this alternate act.

As the child grows up under such training he is learning to persist in certain types of activity and to give up in other lines. He has learned that if he persists he can overcome resistances and come off victorious. What is there in persistency in practice more than this? If there were not an original tendency to persist upon which the teacher can build such training would be impossible; with such a tendency and proper training nothing else is needed. No mysterious exercise of a subtle faculty of effort is needed to explain such persistent practice on the part of the learner any more than it is necessary to say that when the child opposes restriction of its limbs it is consciously saying, "I will not have this man holding my hands, I will exercise my will power and show him I will not be thus dominated." An adult makes such statements and thinks he is using a special gift of will power when he does so. If he acts on his resolution he does so because he has been schooled in using effort against resistance. If he has not so schooled himself his asseverations will be as idle as for a novice at typewriting to say, "I *will* use this machine." He can use it if the resolution marks the beginning of long practice; and a novice at mastering situations can become master if his resolution marks the beginning of practice in so doing. In both cases practice must begin with simple problems. We are all familiar with the 'jack of all trades,' the fellow who has never learned to persist in any one trade long enough to become master of it. Such a man's greatest lack lies not in the fact that he has not mastered a trade but that he has never schooled himself in the meeting of obstacles by persistence in some one trade. He has started a dozen and continued until he struck a 'snag' and then stopped.

Effort in Attention

Spontaneous attention is based upon the reflex response of an individual to an adequate stimulus. If the stimulus causes a response without involving conscious control it is a simple reflex, if the individual is vividly conscious of the response we say that he attends to it. If the first response is followed by successive responses to other details of the same object we have continued involuntary attention. An object which is able to cause a series of responses to its different details is said to be interesting to the individual. Attention to such objects involves no effort, it is a native response. It is necessary in order for one to get a proper course of training to fit him for modern social life to attend to a number of things which are not naturally interesting, to objects to which he would not naturally attend. One does this at first by attaching some outside interest to the object. The drive from this outside interest makes us attend to otherwise uninteresting objects and we still are exerting no effort to do so. At times, however, the derived interests lose their potency, objects assail our senses which have more interest for us than the objects to which we should attend, and we do attend to them only by intense effort to do so. Here the effort takes the form of reinforcing the desired subject and inhibiting irrelevant subjects. Ability to exert oneself in this way is only acquired after practice. One cannot natively inhibit irrevelant stimuli which interest him. A child's attention will waver from one subject to another, and at the instant you think the child is listening to what you are saying he will break out with the remark, "Daddy, what is in your pocket?" A feeble-minded individual who has never been trained to resist irrelevant stimuli shows the same lack of control. Now if an individual did not have a tendency to oppose effort against distractions he would never learn to do so. All through life it would be absolutely necessary to attend to the most interesting thing and the only way to keep one's attention on a subject would be to make it more interesting than the distractions. When one starts to master a lesson the primitive tendency to persist in a line of activity once started comes in to dispel irrelevant distractions. When the distractions become too strong one can either remove them or add extra derived interests to his task to 'boost' him over the hard place. Each time he succeeds in dispelling a strong distraction it is easier for him to do the same thing the next time. He learns tricks to help him do so, such as straining his muscles, articulating words, etc. He uses these aids to help him over the difficult places but in so far as he is holding his attention by sheer force he is using a modification of the inherent characteristic of all organisms to oppose disturbances by increased effort.

Opposition to Innate Tendencies or Learned Habits

A man may awaken to the fact that he has an irresistable tendency to do some thing which is contrary to the moral codes of the society in which he is living or the doing of which would result in injury to himself. This tendency may be due to inheritance or due to some habit he has formed, the end result is the same. His whole organism impels him to do the act, the system of forces of which he is composed is horribly unbalanced, all the weight is in favor of the act that would harm him; still he knows that if he does it he will receive the unanimous condemnation of his fellows or will suffer in some other way. If the realization of the consequences is vivid enough the tendency to do the act will be counterbalanced and the man will refrain. This is the usual type of activity in so-called moral decisions, when the individual is combating an inherent desire to do something or is fighting a habit he has learned. Victory comes without display of effort by making the consequences so forceful as to inhibit the performance of the act. In some cases however when some specific temptation comes the individual loses sight of the consequences, he believes he will not be caught, his organism and the outer stimuli affecting it all urge him to gratify his impulse. He feels that he will gain nothing by resisting except the maintenance of the integrity of his character. It is a battle royal with the odds greatly against the man and only the exceptional man will win. When the victory is won the victor feels that it was only through the greatest effort of his will-power. Such a man is the heroic type of which novelists write and poets sing. He is the ideal to set before the young, he is the standard by which we measure ourselves. How did he get the ability to display such force—to resist when all help had deserted him? Such a victory would never be won by a man who had never been schooled for the battle. The teacher of morality takes great pains to see that the child is given a proper chance to show its resistance to temptation but is also careful to help it across the hard place. No one would expect a child to fight a difficult moral battle, one never blames the child if it fails but blames the elders for their lack in properly guiding and helping the child. The adult who is weak morally is the one who has been so shielded that he never has had to fight a battle, or the one who was never helped so that he never won one. If there were no innate tendency to meet opposition with increased effort it could never be developed.

Summary

Every organism tends to maintain its integrity in the face of situations which would destroy it. Whether one explains the facts of the struggle which ensues from this state of affairs by reference to the concept of will or by reference to a tendency which developed through an evolutionary process, the nature of the struggle which the organism must put forth remains unexplained. Attempts to resolve all response into terms of stimuli must account for a variation in response with the same objective stimulus. This account has been made by reference to the physiological condition of the organism on the theory that every stimulus, besides the direct discharge which it causes, diffuses energy throughout the organism and leaves it in a different condition. This difference in condition results in the second response to the same objective stimulus being different from the first. What is meant by physiological condition (or the correlative psychological term dynamogenesis) needs to be more carefully defined and this paper is an attempt to define one phase of this explanation.

The thesis of this paper is that effort in the sense of a tendency to oppose any stimulus which would destroy the integrity of the organism is a reflex response. Given an inimical stimulus plus the failure of the normal response of the organism to that stimulus to remove it, an increased effort will result.

The paper proceeds on the assumption that the reason for the presence of any trait can be referred to biological variation and selection, the thing which is required is to show that the trait exists at various stages of organic life and that complex manifestations can be referred to the elementary forms and explained as developments from them.

Following this line of reasoning it is shown that the increase of effort to the stimulus of failure appears in an organism as elementary as the stentor, in higher animals, in infants at birth, in a nerve muscle preparation, in muscles intact in the organism, and finally in complex human activities such as resisting distraction. From these facts the conclusion is

drawn that effort is an immediate response to the stimulus of failure.

The exhibitions of effort in such complex mental processes as solving novel problems, persisting in practice, effort in attention and in moral conflicts are then traced as developments of the reflex tendency present at birth to oppose resistance to free activity.

Much work has been done in determining the laws of retention and reproduction because it was felt inadequate to explain present activity by a general reference to past impressions. Just so, psychology cannot rest with the explanation that the response depends upon the physiological state of the organism. The physiological state (set of the organism, directions received, or any other term which may be used) certainly depends upon certain laws which are discoverable through proper research. This paper in an attempt to formulate one of these laws.

A COMPARISON OF COMPLETE VERSUS ALTERNATE METHODS OF LEARNING TWO HABITS.

BY J. F. DASHIELL

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In the main, the study of habit formation has been limited to the study of one or another single kind of habit. True, in human psychology interest has widened from a technical study of a single learning process to include the investigation of two or more, and in animal psychology a few isolated researches have been devoted to one aspect of the matter; but the early researches upon the interference between habits and the more extensive studies of the problems of transfer of training seem not to have stimulated much activity along still other lines of possible investigation in the matter of the development of more than one habit. Putting it very generally, one might divide the past lines of interest in multiple habit formation into (1) the investigation of the relations between one process of learning and a simultaneous process of learning, and (2) that of the relations between earlier and later learnings (of different problems). The former interest has been little shown in animal psychology, and in human psychology only after the latter interest has been rather exhaustively handled. The latter embraces well-known questions as to the transfer of training.

The work here to be reported was a preliminary comparison between the practice methods referred to in (1) and in (2) above. For a subject that is to learn two different habits is it more economical to practice on them both at the same time as nearly as may be, thus learning them together or 'alongside' each other, or to practice on one only after the other has been completely learned by itself? To approach a reliable answer to this rather general question different

[1] The experimental work was done in the Oberlin College Laboratory.

types of subjects and of habits were used. This paper will summarize these separate studies under the headings:

I. Maze Running by Rats;
II. Maze Running by Children;
III. Maze Running by Adults;
IV. Card Sorting by Adults;
V. Adding by Adults.

While this research was in progress Pyle's brief article appeared[1] in which he shows that experiments in card distribution lead to the inference "that it is not economical to form at the same time two mutually inhibitory sets of habits. The better procedure is to form one, and then the other." The present study may then be taken as a research similar to Pyle's but extended over a wider range of habits.

I. Maze Running by Rats

Nothing, so far as the writer is aware, has been done with animals on the question as to the relative economy of Complete and Alternate methods in double habit formation.

The general method used by the writer embraced two procedures: (1) that of training one group of white rats in a single maze *R*, and when that was completely learned in a second maze *L*; (2) that of training a second group of rats in the same *R* and *L* mazes alternately, *e.g.*, if maze *R* be used on Monday, Wednesday, Friday, and Sunday of a given week, maze *L* was used on Tuesday, Thursday, Saturday, and the next Monday. The first method of training will be called "Complete," the second, "Alternate." As implied, single daily trials were set. The animals had been previously accustomed to a feeding at 4:30 P.M., the hour adopted for the experiments, and for a few days before the beginning of the experiment were fed in the food box to be connected with the maze. Uniformity of hunger conditions from day to day were obtained by letting the animals feed as long as they would (less than half an hour) after each day's run, then removing them to the nest box where they had no food until the next run, twenty four hours later.

[1] W. H. Pyle, 'Transfer and Interference in Card-Distributing,' *Journal of Educational Psychology*, 1919, 10, 107–110.

The mazes were of cork-composition flooring, galvanized iron partitions, and glass tops; and all runways and alleys were 4 inches in height by 4 inches in width. The entrances were closed behind the animals by raising a galvanized iron piece hinged at the floor.[1]

The particular patterns of mazes used are shown in Fig. 1. They were exactly inverse to each other. It was believed

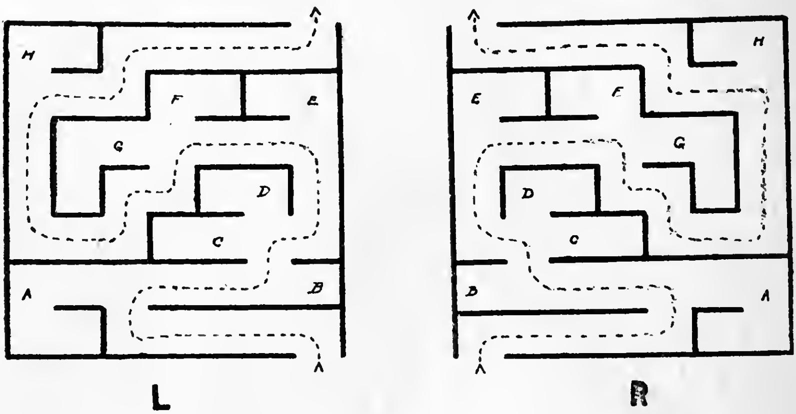


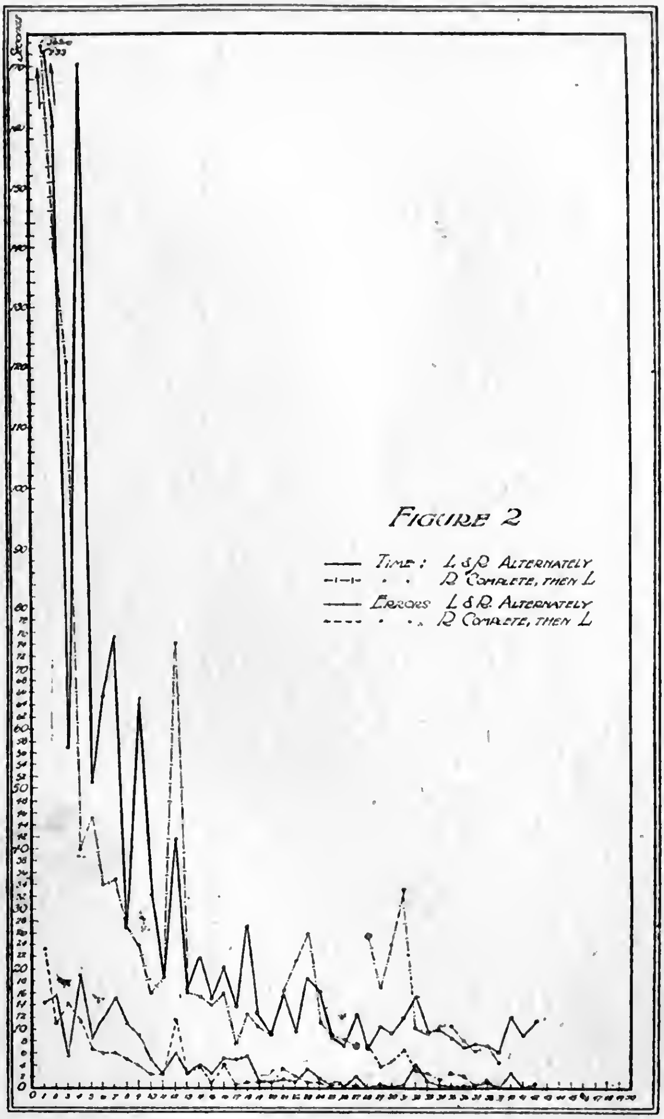
FIG. 1.

that the identity of pattern when inverted would be unimportant to the animal subject, and it had the advantage of making the mazes equivalent in difficulty. It will be observed incidentally that the eight culs-de-sac furnished a variety in position (*a*) with reference to runway, and (*b*) with reference to each other. As preliminary to further studies into the respective influence of different sorts of culs-de-sac notes were taken regarding the functioning of these in this experiment, but the data will not be presented in this place.

In order to provide as complete controls as possible a single litter of white rats was used, one half of the litter serving as the controls for the other half. This necessarily made the number of subjects small, six being used, three in each group. They were exactly eight weeks old at the beginning of the experiments.

[1] For details of construction of these and other mazes in the Oberlin laboratory see *Psychol. Bull.*, 1919, 16, 223–230.

The data obtained for both groups in time consumed and errors made for the various runs are given graphically in Fig. 2. The 28th trial marks the first practice by the Com-



plete group (group learning by the Complete method) in maze *L*.

Comparison of the numerical records of the two groups would show little difference between the methods of Complete and of Alternate learning in regard to total number of trials required. The Complete group shows respectively, 37, 37, and 36 trials (total, 110); the Alternate group, 30, 49 +, and 32 (total, 111 +).

As to type of curve it is to be said that not as much difference was found as would perhaps have been expected. The curves for the subjects learning the *R* maze Completely before being given the *L* maze conform in a general way to the type found in numerous experiments with a single maze, if we take them as far as the twentieth trial. The only unusual feature is the highly increased time and errors at the twelfth trial—attributable to rat *C*'s relapse alone. In the twenty-first to twenty-fourth trials the slight rise of curves is due to the fact that rat *J*, having learned maze *R*, was resting, and the average represents only the achievements of the other two rats. The twenty-eighth trial was the first one on the new maze *L*, and the curves are accordingly high. That they are not decidedly higher is evidence of the transfer generally found in the learning of one habit just after the learning of a somewhat similar one. The average time taken for the first trial in maze *L* is not as great as that for the eighth trial in the previous maze *R*; the average number of errors in the first trial in *L* is only slightly greater than that for the fifth in *R*. To what extent were transferred factors operating and to what extent was there interference? The former seem to have been more in evidence than the latter. Apparently, the need is here indicated for a careful analysis of the degrees and sorts of transfer of different features of the whole maze learning procedure.

For the Alternating group the time curve shows again the general form of the learning curve for the white rat in the maze problem. The error curve is to a lesser degree of that type. As is to be expected, these curves for learning Alternately two mazes show poorer records than do those for rats learning the *R* maze only, *i.e.*, longer times and more errors. This difference in amounts of scores made, however, is not very great.

A feature that appears more definitely is the difference in regularity and irregularity in the curves for the two groups of rats. It is especially evident in comparing the performances in trials 2 to 20 inclusive. The curves for the Complete group are fairly regular and smooth, those for the Alternating group decidedly more irregular, and the time curve so throughout its length. That this feature was not due to a difference in difficulty in the two mazes in alternate use is shown by the fact that the changes are not in the form of a regular alternation between better and poorer scores. Consider such successive changes as in trials 5–6–7–8, in 9–10–11, in 18–19–20, or in 23–24–25–26. To obtain a more definite statement of the differences in amount of regularity in the work of the two groups the writer hit upon the following method:[1] (a) finding the amount of improvement or loss between each two successive trials for each animal in time and in errors; (b) summing all changes of both sorts (both gains and losses) for each animal; (c) averaging these totals for each group; (d) determining the average number of trials taken in each group; (e) writing the number obtained by (c) over that obtained by (d), to express the group average of changes between each two successive runs. (The same method can, of course, be adapted for application to individuals.) Let us call this the "Index of Irregularity." The relation between the two groups may then be exhibited in the form of a fraction or a ratio between the two indexes of irregularity. In this way the Alternate group shows the greater irregularity in reduction of time consumed by the ratio 24.51 : 21.58. The same group shows a very slightly greater irregularity in elimination of errors, 3.48 : 3.41.

The writer cannot refrain from remarking that had the first trial of subject *J* of the Complete group been not so disproportionately great in time and errors, the differences

[1] Application of the method of average of deviations from the average of performance for each group (the well known "A. D.") was tried and discarded at once—the two groups gave the same A.D. for errors, 4.32. The large difference in the initial trials of the two groups (time and errors for Alternate group being 233.0 and 14.3 to 365.6 and 23.6 for the Direct group) is enough to offset a very large amount of greater irregularity by the Alternate group, if the ordinary A.D. be the measure used.

between the two groups would have been materially increased. Time on first trial for each subject of the Alternate group was *E*–92, *T*–402, *A*–205; for subjects of the Complete group, *H*–108, *J*–845, *C*–144; errors on first trial for Alternate group were *E*–8, *T*–17, *A*–18; for the Complete group, *H*–9, J–54, *C*–8.

We would seem to be warranted in concluding that for white rats learning two different mazes with one run daily, it is more economical to practice one Completely, then the other, than to run them Alternately. This greater economy is shown in the form of a greater regularity of performance. As to a difference in total number of trials required by the two methods the data are not conclusive.

II. Maze Running by Children

Surely one important justification and *raison d'être* of the science of animal psychology is to be found in the possibility that principles and laws empirically arrived at in this field may be found applicable in some degree to human psychology and education—fields more complex and difficult of experimental as well as theoretical analysis. It may be safely stated in this connection that such application from one department of psychology to another will be the more warrantable as the materials and methods involved in the two cases are the more nearly identical. It was in accordance with this principle that the author made a study of maze learning by young children.

The eight children used were attending a kindergarten and were in their fifth or sixth years (four or five years old). In this experiment it was necessary because of lateness in the school year to have the child make two runs daily about twenty minutes apart.

The material used was a multiple unit set of screens 4 feet by 3 feet hooked end to end to form the partitions,[1] and was set up indoors on the floor of a large room. To serve as incentive, silk flags on a small upright stand were shown the children and then 'hidden somewhere inside,'

[1] Described elsewhere, *cf. supra*, note p. 114.

and the children were sent in to find them. This artificial incentive quickly gave place to the 'problem solving' interest which persisted throughout in good strength.

On account of the limited floor space available it was impracticable to have two entirely separate mazes erected; and the same condition necessitated the employment of a simpler pattern than the two used for the rats. Figure 3

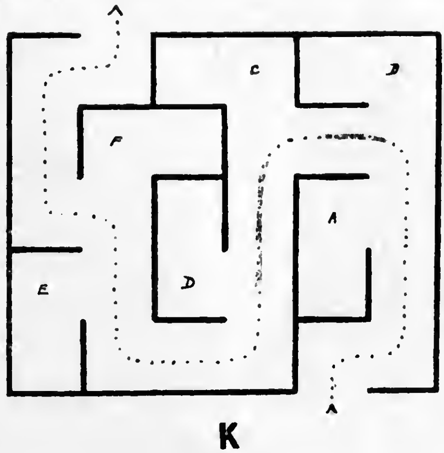
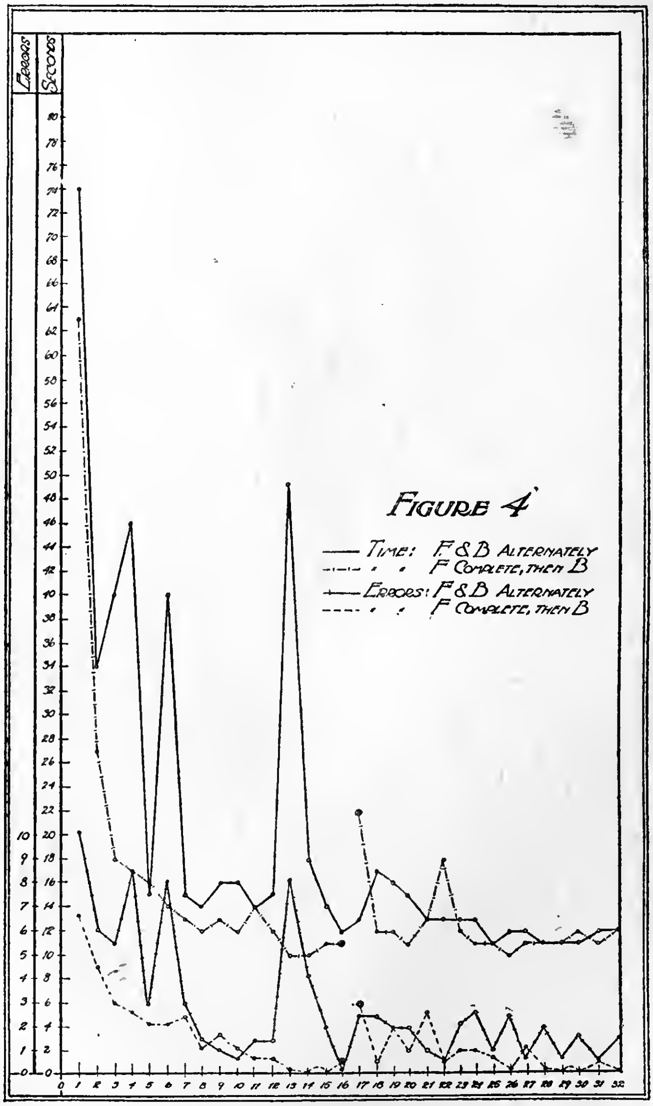


FIG. 3.

shows the ground plan of the kindergarten maze. As one problem the subjects learned to run this maze forward, in direction indicated by arrow heads and passing culs-de-sac in alphabetical order (called problem *F*); the other problem consisted in learning the reverse path, passing the culs-de-sac in the order *F-E-D-C-B-A* (called problem *B*). Similarly to the plan employed with the rats, the method here used was to have one group of children practice problem *F* only until learned Complete (indicated by three successive errorless runs), then the problem *B* only; and to have the other group practice Alternately on the two problems, taking problem *F* first on each day and *B* second.

The data obtained for both groups in errors and in time consumed for the runs are plotted graphically in Fig. 4. The seventeenth trial marks the first practice by the Complete group upon the problem *B*, the subjects who had learned problem *F* earlier continuing to run on it until this trial.

Comparison of the numerical records of the two groups would show some slight advantage for the Complete method in so far as the number of trials required is the criterion, the



children learning by this method requiring respectively 33, 32, 28, and 23 trials as against 34, 37, 30, and 35 + trials for the other group.

The type of curve is again of the well-known negative acceleration kind for both groups.

The feature that again strikes the eye is the difference in regularity or irregularity in the curves for the two groups of subjects. Using the fraction described above in connection with work with rats, it is found that the respective "indexes of irregularity" compare as follows: the Alternating group again shows greater irregularity over the Complete group in the reduction of time consumed by the ratio, 6.62: 3.63; the same group shows a greater irregularity in elimination of errors, by the ratio, 7.00 : 3.44.

Another difference between the work of the two groups is to be found in the amount of errors and time shown for single trials. Consider especially the differences in errors in the trials numbered 2 to 8, 11 to 15, 22 to 26, 28 to 32, and the differences in time in trials numbered 2 to 4, 6 to 16, 18 to 20. The group learning by the Complete method showed fewer average errors in 25 out of 32 trials and shorter average time in 22 out of 32 trials.

III. Maze Running by Adults

Another approach to the general problem of the research was made by using adult human subjects with pencil mazes, and with a somewhat different program.

Four students of college grade in a summer session were enlisted as subjects. They will be denoted by initials, *H*, *K*, *B*, and *W*.

The mazes were constructed as follows: the design of each maze (see Fig. 5) was laid out on cardboard and then with a narrow band saw cut out of 'beaver board.' This was given two coats of shellac to produce sufficient hardness and smoothness of edges. The 'beaver board' was nailed firmly over a piece of cardboard placed upon a wooden base. The runways were $^1/_4$ inch wide and $^1/_4$ inch deep, and had the smooth cardboard for their floor. Two of the mazes (*S* and *s*) were designed on a rectangular and straight line plan, to make a suitable problem for the development of two habits, and were respectively $8 \times 9^1/_4$ inches and $8^3/_4 \times 9^1/_4$ inches in outside dimensions. The other two (*R* and *r*) were designed on lines similar to one another but quite distinguishable from the first two: they were of circular outside plan with radius of $4^1/_8$ inches, and the runways were arcs of the same curvature. A stylus was fashioned by rounding the end of a $^3/_{16}$-inch round brass rod, 6 inches long. A rubber

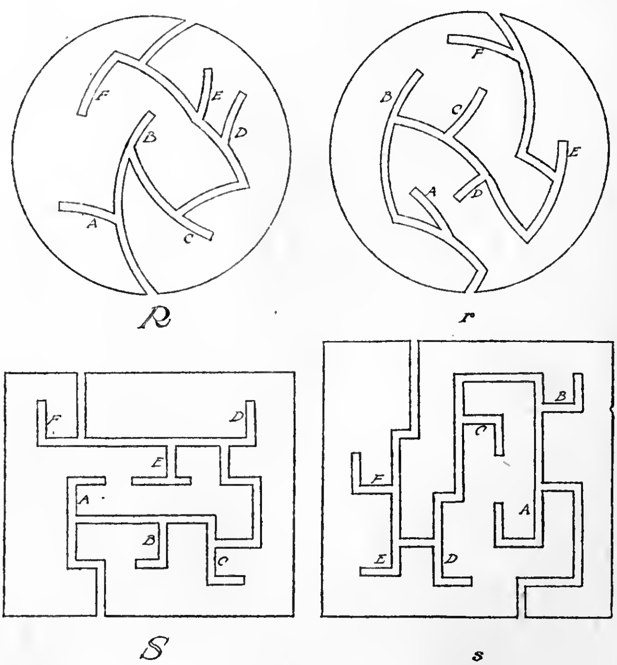


FIGURE 5

band was wound around the stylus 2 inches from the lower end. The stylus moved easily in the runways when held vertical, but was of sufficient diameter to prevent too great looseness and loss of contact with sides of runways.

The program of the experiments was arranged so that each subject might serve as his own control, by having him learn one pair of mazes by the Alternate method, the other pair by the Complete method. Instead of the trials being distributed over a long series of days, as had been done with children and especially with rats, the maximum of 40 trials allowed to each problem was given all at one sitting (of 45 to 60 minutes). The order of use of the two pairs of mazes, and the order of use of the two methods was varied, as seen in Table I.

TABLE I

SHOWING PROGRAM OF SITTINGS

Subject	Tuesday	Thursday
H......	Mazes *R* and *r*, Complete method	Mazes *S* and *s*, Alternate method
K......	Mazes *R* and *r*, Alternate method	Mazes *S* and *s*, Complete method
B	Mazes *S* and *s*, Complete method	Mazes *R* and *r*, Alternate method
W......	Mazes *S* and *s*, Alternate method	Mazes *R* and *r*, Complete method

The subjects were uninformed throughout as to the object and the exact methods of the experiment and as to the order in which the mazes were to be used. After every trial the experimenter removed the maze and made a "business as of changing mazes" before placing maze for the next trial. This procedure was followed whether the same or the alternate maze was to be used. Remarks or questions by the subjects as to the apparent sameness or difference of mazes used in successive trials were not answered. Thus in addition to learning mazes in the usual sense the subject had to discover inductively their number and the order in which they were set. (As mentioned above, the entrance paths to all mazes were different—to furnish a clue at the beginning of each run that could sooner or later become definitely recognized and used.) In operation this was found to have considerably increased the difficulty of the learning and to have introduced important ideational elements that may be partially responsible for the minor differences in results between this and preceding parts of this research.

For the experiment the subject was seated at a table upon which the maze was placed, with a screen fixed above it to shut off vision of it. The instructions given the subject were as follows: "You are to take this brass stylus (shown) in your fingers much as you would a pencil, but holding it

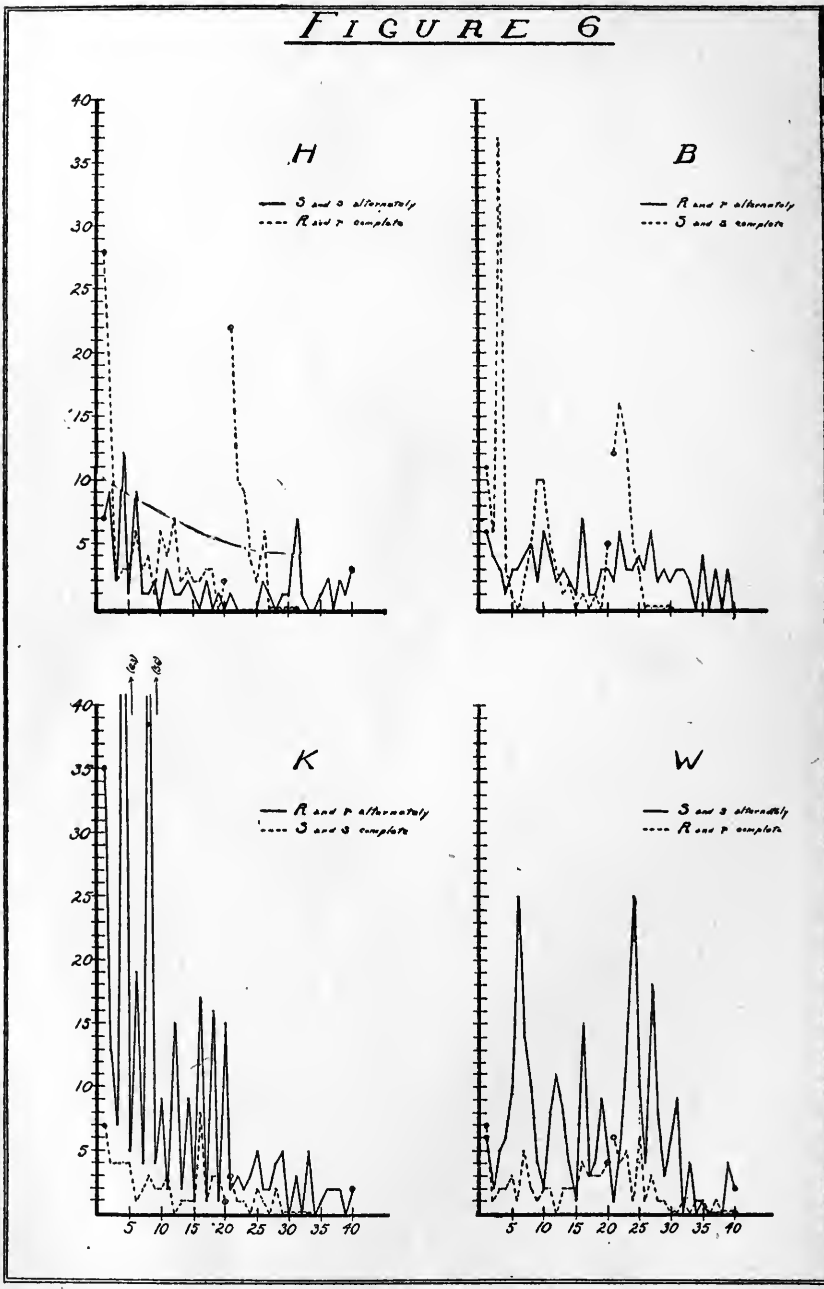


FIGURE 6
H
S and s alternately
R and r complete
B
R and r alternately
S and s complete
K
R and r alternately
S and s complete
W
S and s alternately
R and r complete

vertical. Do not place fingers below the rubber band. As you hold it I will insert the point at the beginning of a groove cut in a wooden floor, which you are not to see. You are then to move the stylus within this groove until you reach a point at which I say, 'Stop!' Time will be taken for the trial but you are not to feel hurried. Three points are to be remembered: Do not lift the point of the stylus from the floor of the groove; always keep the stylus in strictly vertical position, never let it slant; never let your fingers or any part of the hand touch the floor." In actual operation the subject's stylus upon reaching the exit went down off the edge of the one inch base board.

The results of this experiment are best presented graphically in Fig. 6. Only curves for the errors are shown. The time and error curves were in every case practically identical, as the human subjects did not show the rapid pick-up of speed shown by the rats, *i.e.*, a relatively greater elimination of surplus time than of errors. It will be seen that with the subjects practicing by the Complete method first (*H* and *B*) a great loss of time and a great number of errors accompanied the first runs on each maze learned by this method. The slightly better performance by these same subjects when using the Alternate method later is, however, possible of interpretation in terms of practice—practice on the first two mazes being advantageous for practice on the later two, being an evident case of transfer at least of the more general elements in the learning situation. This is confirmed by inspecting the curves for *K* and *W*, who used the Alternate method first. In both of these subjects the improvement in learning two mazes Alternately is slow and, what is most

TABLE II

SHOWING INDEXES OF IRREGULARITY IN ELIMINATION OF ERRORS

	By *C.* Method	By *A.* Method
Using *C.* Method First:		
H	3.55	2.25
B	4.36	2.00
	7.91	4.25
Using *A.* Method First:		
K	1.03	11.37
W	1.42	5.35
	2.45	16.72

striking, very irregular. Remembering its limitations, we may again employ our index of irregularity to bring out the last point. See Table II. It is also instructive to note how rapidly the second habit by the Complete method was learned, in all cases.

IV. Card Sorting by Adults

So far the question as to the relative efficiency of Complete and of Alternate methods in practicing two habits has been studied in connection with maze learning. Certain general principles have been found to hold for maze habits for different kinds of subjects with different kinds of maze materials. The question arises, can the findings be demonstrated for other sorts of habits? Since the running of a labyrinth is a typical sensori-motor or perceptual-motor habit, it occurred to the experimenter that it might be enlightening to apply the same methods of approach to some other style of perceptual-motor habit. The one chosen, card sorting, possessed the advantage of being a familiar one in psychological literature, having been already studied somewhat with regard not only to questions concerning single learning processes but also to some questions with respect to the formation of multiple habits.[1]

In the present series[2] the general program was similar to that used in maze-running experiments with adults. The material needed was two packs of cards of quite different kinds. For one a 'flinch' deck was used which, with all numbers of 11 and higher discarded, furnished ten cards each for the numbers 1 to 10, one hundred in all. For the other pack, one hundred blank cards of size, shape, and general 'feel' similar to the 'flinch' cards were obtained, and upon them were printed autographs in script by means

[1] For studying the interference of habits it has been used by Bergström, Brown, Culler, Pyle. The study of Pyle, referred to above, which is almost identical in some regards with this section of the present paper, appeared while these experiments were in progress. The corroboration of his findings has its own value, and hence this section is included in the paper.

[2] For the data on the card sorting experiment the writer is indebted to Miss Helen G. Smith.

of rubber stamps obtained from men on the campus, one autograph for each ten cards, ten autographs for the whole pack. The general procedure was to have each subject use one of the packs for learning to deal to two different distributions or lay-outs on the table by one method (Complete or Alternate), and to use the other pack for learning two different distributions by the other method. See Table III.

TABLE III

SHOWING DISTRIBUTION PATTERNS USED

With "flinch" cards:

Pattern *F*					Pattern *f*				
3	8	4	7	10	2	8	10	5	3
6	9	1	5	2	7	1	4	9	6

With autographed cards:

Pattern *A*					Pattern *a*				
J	S	C	L	M	G	B	N	L	F
N	G	R	F	B	C	J	S	M	R

The subjects used were four college students, Juniors and Seniors, referred to as Br, Bu, P, and H. They all coöperated well throughout.

They worked at the experiment daily for ten days. They were given a total of twenty-five deals in which to learn each lay-out pattern; but these deals were arranged in the two different orders, the Complete and the Alternate. Table IV. shows in detail the program of the work as carried out. The letters denote the lay-out patterns, as given above in Table III., the figures indicate the number of deals on each day to that lay-out.

The subjects were instructed to make each deal as rapid as possible and were warned that time would be taken. A misdeal had to be corrected before continuing.

The graphic method again recommends itself as the clearest way of presenting the results. See Fig. 7, in which the individual records are shown separately.

It is to be observed at once that the Alternate method of practice in dealing to two different distributions is unquestionably inferior to the Complete method. This is strikingly true in the matter of actual amounts of time taken, shown by the lower vertical positions on the graphs. What is less

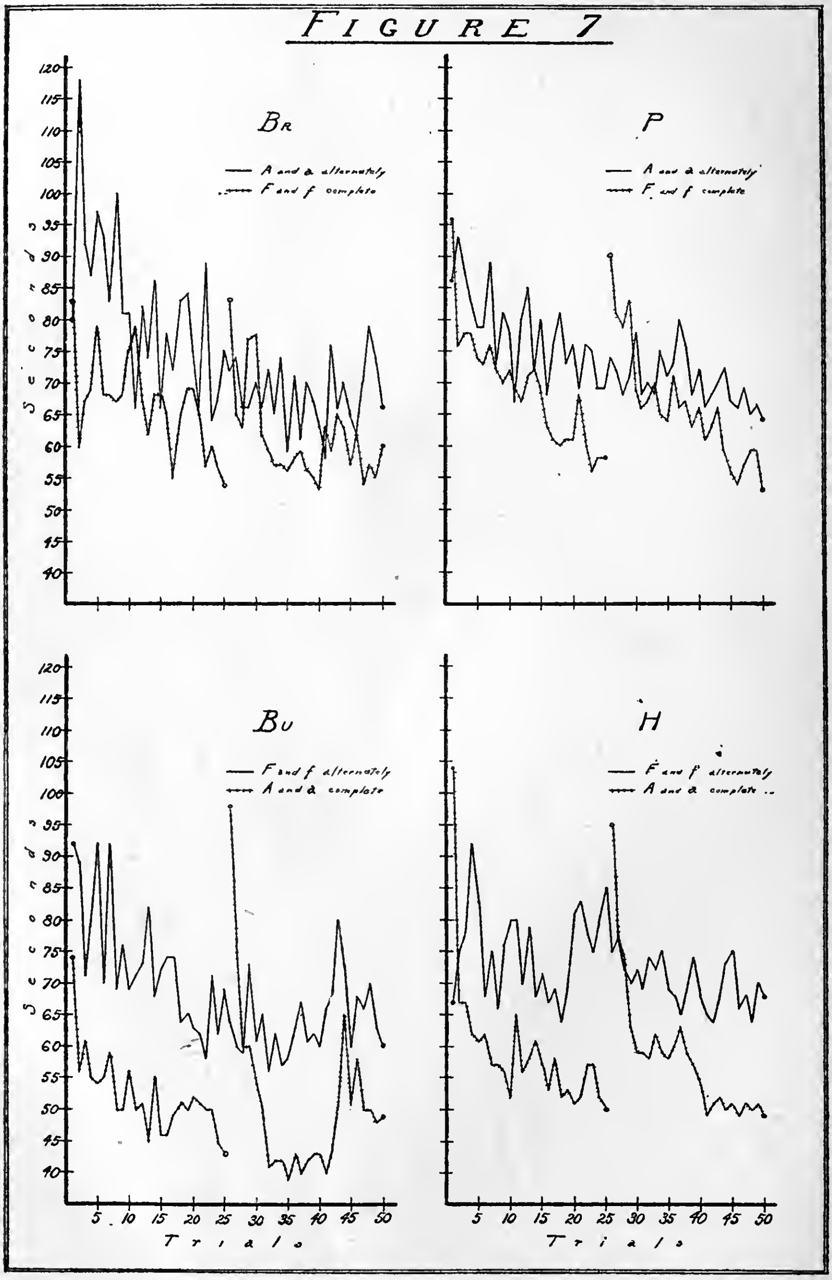


FIGURE 7
Br
A and a alternately
F and f complete
P
A and a alternately
F and f complete
Bu
F and f alternately
A and a complete
H
F and f alternately
A and a complete
Seconds
Trials
120
115
110
105
100
95
90
85
80
75
70
65
60
55
50
45
40
5
10
15
20
25
30
35
40
45
50

TABLE IV

SHOWING PROGRAM OF CARD-SORTING EXPERIMENTS

Subjects	First Day	Second Day	Third Day	Fourth Day	Fifth Day	Sixth Day	Seventh Day	Eighth Day	Ninth Day	Tenth Day
Br.	F^{10}	A^5 and a^5 altern.	F^{10}	A^5 and a^5 altern.	F^5 then f^5	A^5 and a^5 altern.	f^{10}	A^5 and a^5 altern.	f^{10}	A^5 and a^5 altern.
Bu.	A^{10}	F^5 and f^5 altern.	A^{10}	F^5 and f^5 altern.	A^5 then a^5	F^5 and f^5 altern.	a^{10}	F^5 and f^5 altern.	a^{10}	F^5 and f^5 altern.
P.	A^5 and a^5 altern.	F^{10}	A^5 and a^5 altern.	F^{10}	A^5 and a^5 altern.	F^5 then f^5	A^5 and a^5 altern.	f^{10}	A^5 and a^5 altern.	f^{10}
H.	F^5 and f^5 altern.	A^{10}	F^5 and f^5 altern.	A^{10}	F^5 and f^5 altern.	A^5 then a^5	F^5 and f^5 altern.	a^{10}	F^5 and f^5 altern.	a^{10}

apparent in the curves but is shown in the numerical data is also a greater irregularity in the rate of progress in learning by the Alternate method. Table V. gives the warrant in figures for both these conclusions.

TABLE V

SHOWING SCORES IN CARD SORTING

Subject	Method	Average Time Taken (Seconds)	Index of Irregularity
Br.	Complete	63.9	5.7
	Alternate	74.8	9.5
Bu.	Complete	51.6	5.7
	Alternate	68.6	6.9
P.	Complete	67.4	4.2
	Alternate	74.0	5.3
H.	Complete	58.7	4.7
	Alternate	72.5	5.1

What has been demonstrated for maze learning is found to hold true also for card sorting. It would seem to follow that the principle would be found to apply to all forms of true sensori-motor or perceptual-motor habit formation.

V. ADDING BY ADULTS

If certain principles are found to hold true of a particular region in the whole field of learning, an important question then arising is, will they hold true of all learning in general? To make one further step in this logical direction, the author sought an answer to the fundamental question of this research in connection with learning on a 'higher' plane than the perceptual-motor. As a more 'purely mental' process that is yet sufficiently of the habit type to be easily recorded and measured objectively, numerical computation suggested itself. Addition was taken as a particular form of computation the improvement in which might serve as an interesting task to students, especially in view of the practical value of adding ability.

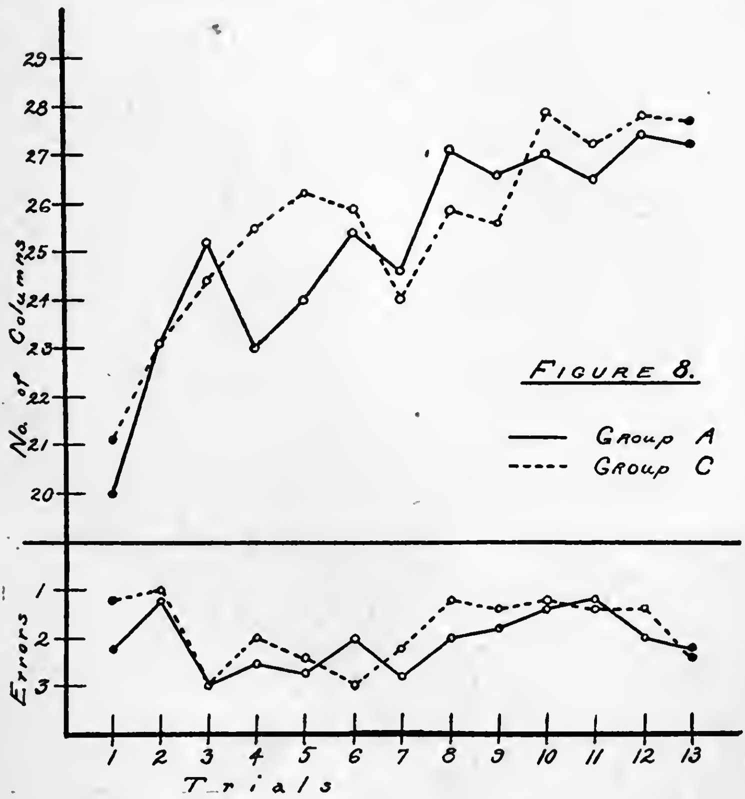
The subjects were ten summer session students of college grade, members of the writer's class in the psychology of training. The adding work was given during the first few minutes of each class hour, the class meeting usually five days in the week. The students were fully informed of the problem in hand; in fact, it was treated as a concrete side of the material of the course. Their daily individual and group records were exhibited at the following meeting, both numerically and graphically. The writer was convinced, and was so assured by the students, that their interest in the adding was throughout high, generally being keen.

The material used consisted of mimeographed addition blanks, with thirty examples on each sheet, each example consisting of ten two-place numbers in vertical order. Six sets of the material were provided, and the sets used in rotation.

The experiment was conducted as follows: One copy of the examples to be used for that day was given face down to each student at his desk. At a signal, all turned their papers over and immediately set to adding, continuing until the stop signal was given aloud at the end of five minutes.

Both in order to eliminate some of the elements of psycho-physical adjustment to the work, in a sense somewhat extraneous to the problem at hand, and in order to have some basis for dividing the class into two groups as equal as possible, a preliminary series of trials in simple addition was given. In these trials the subjects added the numbers vertically digit by digit and column by column. The results were taken in terms of single columns or half columns added. This was done for the five-minute period at thirteen successive class hours. On the basis of the individual scores made the experimenter divided the class into two groups of five each, for the formal experimental series. Fig. 8 gives the average scores of the individuals as so grouped.

For approaching the question as to the relative efficacy of learning two kinds of addition by the Complete or by the Alternate procedures, it was necessary to fix upon addition methods that, while having some elements in common, would



yet differ in important ways. Moreover, the addition methods had to be novel. The plan hit upon was to use for the one method or habit to be learned, the adding of two-place numbers horizontally, from left to right, adding the units first and then the tens. The other habit decided upon was the adding of the two-place numbers vertically, but by grouping them: adding first the odd-place numbers together and noting down the sum, then the even-place numbers likewise. Thus with the blank partly given in Table VI, the horizontal method would involve adding successively the digits 4–5–2–9–8, etc., for unit place in the sum, and 8–3–5–7, etc., for the tens and hundreds; the vertical method would require adding the digits 6–9–7–5–1 and 3–5–7–4–9 for the total 308 of the odd numbers, and then adding 8–4–2–3–4 and 6–3–6–2–8 for the total 271 of the even numbers. As in the simple addition in the preliminary series a complete sum obtained horizontally counted as two columns; a sum of only odd-place or of only even-place numbers obtained vertically counted as one column.

TABLE VI

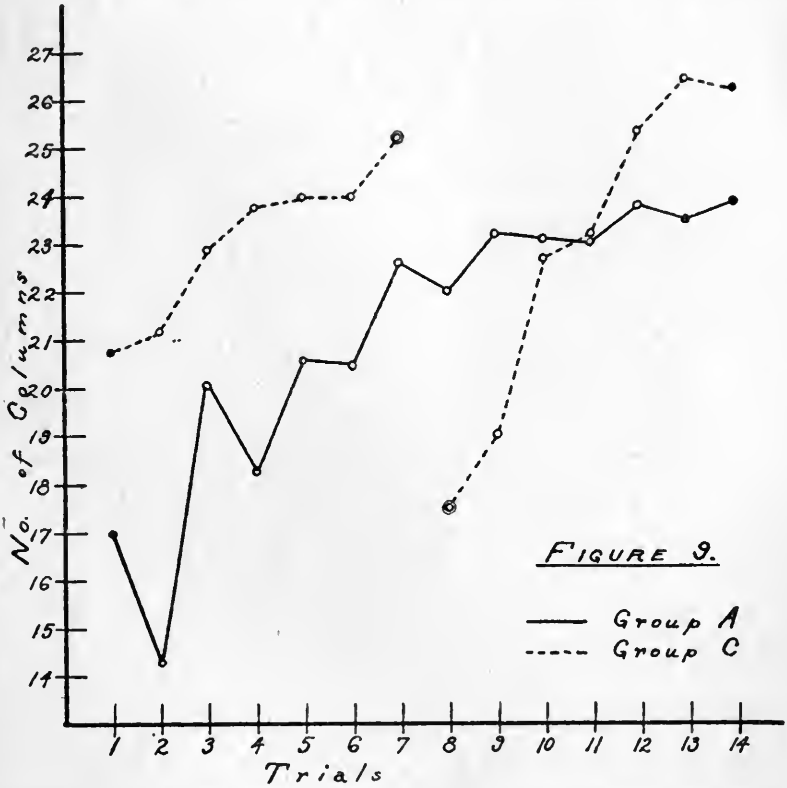
SHOWING PART OF SAMPLE ADDITION BLANK

84	35	52	79	18	44	63	85	59	27
91	—	—	—	—	—	—	—	—	—
23	—	—	—	—					
45	—	—							
62	—				Etc.				
77									
34									
59									
68									
36									

The program followed was for one group (*C*) to practice the horizontal habit for seven trials in succession then to practice the vertical method for the remaining seven trials; and for the other group (*A*) to practice at horizontal adding on the first, third, fifth, etc., days, and at vertical adding on the second, fourth, sixth, etc., days.

For simplicity's sake the results of the formal series will be given in terms of number of columns added, corrected for accuracy by deducting a half column for each error.

It was early observed that these two habits were not of equal difficulty (as had apparently been the case in all the preceding experiments), the vertical habit being clearly the harder. The gross average number of columns added vertically by group *A* was found to be only 83.8 per cent of the number added horizontally, the corrected average of the number added vertically being 84.3 per cent of the corrected average of the horizontal additions. For group *C* the corresponding uncorrected and corrected averages bore the ratios 89.6 per cent and 89.9 per cent, respectively. In order, then, to be able to show the relations between the two habits it was necessary to make them more commensurable by using the above ratios of corrected averages for the two groups as bases for weighting. Thus, the group *A* average for each trial by the vertical method was considered as 84.3 per cent and raised to 100 per cent; the group *C*



average for each trial by the vertical method was similarly increased from 89.9 per cent to 100 per cent.

The resulting data for both groups by both habits are plotted graphically in Fig. 9. Here we see a very clear superiority in the work done by the group adding by the Complete method. The curve showing their horizontal adding is consistently high. What is more striking, however, is the extreme rapidity of improvement in vertical addition when once it was undertaken and practiced without interruption. One doubtful feature of the record is the interpretation of the relatively good performance by the *C* group at the very first trial. If our preliminary scores for the two groups are reliable (see Fig. 8) such high initial score is not due to greater initial general ability in adding.

In the matter of regularity in improvement the curves speak more clearly than numerical figures. The index of irregularity found for group *A* is 1.3, that for group *C* is 1.5. Unquestionably, this difference does not speak for a lesser irregularity in improvement for *A* so much as for the great drop by the *C* group in starting the learning of the second habit. In any case the difference is small, and the outstanding feature of the results is the much more rapid progress shown by the group learning one habit at a time.

Summary

We have approached the question as to the relative efficacy of learning two habits by practicing them alternately (the Alternate method) or by getting one to some extent fixed before practicing the other (the Complete method). The approach was made with the use of mazes for rats, children, and adults, then extended to include another perceptual-motor habit, card sorting, and further still to include a habit involving very little of the motor element, addition.

The particular technique of the different experiments was intentionally varied considerably: (*a*) in temporal distribution of trials, (*b*) in stage at which shift was made from one to the other habit by the Complete method, (*c*) in arrangement of controls—division of subjects into groups, (*d*) in methods of scoring, (*e*) in incentives used, (*f*) in subjects' previous familiarity with the habits to be learned, (*g*) in subjects' knowledge of the number and order of the habits to be learned, (*h*) in subjects' knowledge of the nature of the problem investigated. Thus, the general results found may be considered as independent of particular details of technique and to be of general bearing.

For results, it has been found that in all the forms of double habit formation studied, learning by the Complete method is more economical than learning by the Alternate method. This is indicated in the different sets of experiments in terms of the different criteria of efficiency respectively applicable. They include: (*a*) the number of trials necessary to fix a habit, (*b*) the degree of regularity in improvement, (*c*) the average amounts of scores made on individual trials, (*d*) the rate of acceleration of improvement.

THE TONAL MANIFOLD

BY R. M. OGDEN

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Psychologists have often resorted to graphic representations, both bi- and tri-dimensional, in endeavoring to express the interrelationships which obtain among the concurrent aspects of elementary sensation. The most successful of these attempts has been in the field of vision where the color triangle, the color pyramid and the color cone are well known as means of setting forth the dominant features of the chromatic and the achromatic series of visual sensations. Similar schemes have not been wanting for the other senses, but no very useful representation has become current to elucidate the characteristic features of auditory sensation. Sometimes the tonal manifold has been represented as a straight line to suggest the rise of pitch from low to high tones, and sometimes a spiral has been used in order that the recurrent similarities of the octave might be indicated by points directly above one another in the spiral progression. But the latter succeeds only in emphasizing the recurrent likeness of octaves; for it fails to distinguish the similar relations obtaining between other consonant intervals. In the light of recently acquired knowledge concerning the volume and the intensity of sound, the relations of pitch and consonance can no longer be regarded as the dominant features of the tonal manifold, and it is now possible to regard the octave-quality of a tone as perceptual in its origin. Although tonal consonance is still a debatable question, it seems fairly obvious, upon analysis, that the octave, or any other consonance, *subsists* in the relation of a musical interval; and that its elemental nature is therefore either an implicit relation, as Stumpf understands it to be, or the product of an implicit or an explicit act of perception in which not one tone alone but two different tones are involved. In neither case does a quality of consonance or an octave-character attach itself directly to the simple element of tonal experience, as does, by contrast, its pitch, its intensity, or its volume.

Unless we revise our whole conception of sensory analysis, the characteristic aspects of an elemental sound are now recognized to embrace at least four attributes; namely, pitch-brightness, volume, intensity, and duration. Although each of these is a variable, the particular degree of each which attaches to any given sound is determined once and for all by the psycho-physical conditions under which it exists. The octave-quality, on the other hand, is a characteristic which attaches equally to all tones within the musical range when an appropriate reference has been made to another tone of the series. Furthermore, a particular tone, though it can have octaval relationship with but two other tones, one below and one above it in the series, may establish numerous consonances with various tones both above and below it in pitch.

In addition to the octave-quality of a tone there is also a debatable quality which is supposed to enable one to assign a tone to its appropriate place with reference to certain fixed regions of the scale. This would explain the occasional ability of a person to judge the 'absolute pitch' of a tone, and it has also been thought to explain certain outstanding regions of pitch with which the vocal qualities seem to be associated. Whether a c-ness, d-ness, e-ness, etc., of tones is immediately apprehended without involving a somewhat complicated act of perception is a question we shall not here attempt to decide, but in view of the fact that Oriental peoples employ scales in which these harmonic designations have no significance, it seems best to reserve judgment for the present and test the possibility of some other explanation before we proceed to base the elusive phenomena of absolute pitch upon a universal quality inherent in tones which enables us to assign them to their appropriate places in the musical scale. All things considered, the case for the outstanding octaval regions with which the chief vowel-sounds are associated is a better one; but even here there are other possibilities of interpreting the phenomena, like the one suggested by Watt,[1] in accordance with which the vocal apparatus is assumed to find the utterance of sound at one region of pitch easier than at another; hence the tendency to give prominence to vocalization at this region and likewise at other regions, above and below, which are in octaval relationship with it.

For our present purposes we shall ignore the conflicting claims as to the qualitative aspects of tone other than the features of pitch-brightness, volume, intensity and duration which have already been mentioned, and shall confine ourselves to the first three of these in the graphic representation which accompanies this paper.

We have before us, then, a representation of the psychological aspects, or attributes, of a series of pure tones in a progressive manifold extending throughout the range of audibility. It should be noted that the dimensions of our graph are measured in terms of *psychological* and not in terms of physical components, and though we may refer to vibrational frequency and vibrational amplitude, these are to be understood as the conditions under which the psychological entities of our manifold are controlled and produced; and not as being themselves involved in the scheme.

The particular tones we have chosen to represent are the successive octaves conditioned by vibrations ranging from 16 to 32,768 per second. Each tone is pictured with a certain spread on the base-line to suggest its volume; thence rising to a peak which indicates its pitch. The height of this peak above the base-line measures *inherent intensity*, and denotes the relative sensitivity of hearing at different degrees of vibrational frequency. Duration, since it involves movement, is not included in the scheme.

It will be observed that the total spread or volume of the lowest audible tone comprises within its range the volumic emplacement of all higher tones, the extreme upper point of emplacement being identical for all tones. This accords with Watt's theory[2] and seems to be justified on the grounds he has advanced. But Watt's further assumption, that 'when

[1] *Cf. Brit. J. Psychol.*, 1914, 7, pp. 12–13.
[2] 'The Psychology of Sound,' 1917.

octaves are played, the upper tone coincides with the upper half of the lower tone . . .,'[1] is no longer acceptable in the light of Rich's determination of the threshold for volume.[2] If the increments of vibrational frequency necessary to produce liminal differences of volume depend upon a constant fractional increase in the middle range of the scale, then octaves within this range must differ by a constant number of steps, which precludes the possibility of Watt's assumption that the volume of the upper tone of an octave should always be half the size of the lower tone.

Decrease in the spread or size of volume by a constant amount is indicated for octaves throughout the musical range of the tones here pictured (64 to 2,048 vibrations). Both above and below this range, however, the fractional increase is presumed to vary. The volumes of the lowest tones are represented to be greater than the normal increase would warrant, and their pitches are displaced to the right, indicating the known tendency of low tones to appear higher in pitch than they should. Similarly the highest tones are shown decreasing more rapidly in volume than they do at the middle range; while their peaks are displaced to the left—indicating the tendency to regard tones of the four-accented octave and above as flat. The total range of volume has been divided somewhat arbitrarily into 228 steps, each step representing a discernible interval as determined by a clearly defined difference of volume. According to the investigations of Rich,[3] the threshold of volume is approximately .02 to .03 of the vibrational frequency. Since the interval of the semitone is about .06, we have taken .03, or the quarter-tone, as being the threshold for interval-distance, and have plotted the curves with abscissæ measuring 228 just noticeable differences of interval from the lowest to the highest tones.

In the middle range of the scale the volume for each octave is so plotted that it diminishes at the constant rate of 24 quarter-tone intervals. In the highest and lowest ranges of

[1] *Op. cit.*, p. 212.
[2] *Amer. J. of Psychol.*, 1919, 30, pp. 122 ff.
[3] *L.c.* and *J. of Exper. Psychol.*, 1916, 1, pp. 13 ff.

the scale, however, judgment of intervals is known to be less certain. Tones below 40 vibrations appear to be a little higher than would be warranted by the rate of vibration; while in the upper range tones of 3,000 vibrations and above seem flat, and at about 4,000 vibrations, according to von Maltzew,[1] accurate judgment of intervals breaks down completely. Equal decrease of volume as a basic feature in the determination of octaves and other musical intervals extends, therefore, only through tones that range from about 50 to about 3,200 vibrations in the second. The volumes of the lower tones are made relatively larger, and of the higher tones, relatively smaller, than the normal variation of the middle register would allow.

Turning now to the pitch of tones, this is indicated by the central point or salient in the upward-rising mass of volume. It will be noted at once that as volume decreases the pitch becomes more salient, or pointed. This suggests the brightness characteristic. As pitch rises it emerges more and more clearly; it becomes more and more salient. The upward trend from the base-line also suggests the variation of inherent intensity attaching to tones of different pitch-levels. The curve which circumscribes the salient peaks of these progressive tones is the one determined by Max Wien in his study of auditory sensitivity for tones of different pitch.[2] According to Wien's investigation sensitivity to tones increases rapidly from the lowest audible tones to those of about 2,000 vibrations when it begins to diminish, first slowly, and then more rapidly.

This curve of sensitivity is of especial interest because of the indication it gives as to differential sensitivity for pitch. In the lower range, successive tones coincide to so large an extent that the sensitivity for pitch is not much greater than the sensitivity for volumic differences. The pitch-salients of low tones are vague and indefinite, and an appreciable distance or interval is therefore requisite before one pitch can emerge distinctly from another. At a higher level this is not the

[1] *Zsch. f. Psychol.*, 1913, **64**, pp. 161 ff.
[2] *Cf. Pflüger's Archiv.*, 1903, **97**, pp. 1 ff.

case, for with salient tones one pitch distinguishes itself from another even though there is no perceptible volumic difference upon which a judgment of interval can rest. Thus the number of discriminable pitches within an octave increases steadily until we reach tones in the region of 2,000 vibrations when it begins to decrease. Decrease of sensitivity in the upper range, together with inability to judge volume accurately, both correlate with a falling off in ability to discriminate pitch; although the absolute difference of vibrational frequencies required for a given interval being progressively greater in the higher range, may therefore occasion a larger number of discriminable pitches per interval than is to be found for the same interval at a lower level of the scale.

We have thus represented in our figure the progression of tones throughout the range of audibility and have indicated in a general way the course taken by volume, pitch, and intensity. It remains to add a few words regarding *brightness*. In a previous paper on the attributes of sound,[1] I have suggested that brightness be added to the list of auditory attributes. It is obvious enough that sounds are characterized not only as big or little, loud or soft, long or short, high or low, but also as *piercing* or *dull*. But it is still an open question whether this latter characteristic, variously referred to as *brightness-dullness; shrillness-mellowness* and sometimes as *vocality*, merits consideration as an independent variable. Rich, who made some study of brightness in his recent experimental investigation upon the attributes of tone, comes to the tentative conclusion that while the term is valid for descriptive purposes, brightness is not independent of pitch, since each appears to have the same threshold for differential judgments. He therefore suggests that *pitch-brightness* would be a more appropriate description for a single attribute hitherto called pitch.[2] This is very probably the case as regards the tonal manifold, but as mentioned above brightness has also been linked with vocality, and it is in this connection that I still desire to recommend further investigation before

[1] *Cf.* Psychol. Rev., 1918, 25, pp. 227 ff.

[2] *Cf. Amer. J. of Psychol.*, 1919, 30, p. 157 f.

we discard it as unnecessary to the complete description of sound.

Thus far in this paper we have confined ourselves to but one species of sound—namely, tone. But there are at least two other perceptual objects of sound: the *vocable* and the *noise*. The latter may perhaps be dismissed from our present reckoning, for noise is commonly regarded as a complex sound all of whose components can be described either as tones or as vowels. Although such a conclusion is by no means certain, an additional attribute which may furnish a basis for the perception of noise is not at present under consideration.

In the case of vocalic sounds, however, we have a type of percept which offers some interesting features when it is compared with the perception of tones. The investigations of Köhler,[1] Miller,[2] and Schole[3] have indicated the regions of vibrational frequency which seem to characterize the chief vowels. Yet Köhler's conclusion that the vowel is defined by a certain pitch has not been confirmed by other investigators. Jaensch,[4] has made an apparently successful attempt to produce vocalic sounds synthetically. His results indicate that a compound of pendular-formed vibrations, varying but slightly from one another in frequency, possesses a vocalic character which passes over into noise as the mean variation of the vibrational components increases beyond a certain point. Unfortunately Jaensch's method does not permit us to determine just what vibrational frequencies combine to give an optimal vocalic effect, and as the investigation has not been repeated, his results have thus far received less attention than they seem to merit.

It is highly desirable that this matter should be re-investigated, for if it be true that the characteristic feature of a vocalic sound is obtained by compound regional vibrations, rather than by a simple pendular-formed wave, the difference between vocalic sounds and tones might be manifest when a

[1] *Cf. Zsch. f. Psychol.*, 1910, **58**, pp. 59 ff.

[2] *Cf.* 'The Science of Musical Sounds,' New York, 1916.

[3] *Cf. Arch. f. d. ges. Psychol.*, 1918, **38**, pp. 38 ff.

[4] *Cf. Zsch. f. Sinnesphysiol.*, 1913, **47**, pp. 219 ff.

variation in brightness takes place without an alteration of pitch. An investigation of Baley[1] demonstrates that the pitch of such a combination is indeed that of its mean tone, and if the components of the sound-mass are very near one another in frequency, it would certainly be impossible to analyze them perceptually. Hence the sound must be of elemental simplicity. The question is, does it possess the vocal character which Jaensch has assigned to it? If so, this change must be occasioned by an attribute other than pitch, intensity, volume, or duration. Such a variant is indicated by the term brightness, represented in our graphic scheme by the pointedness or salience of pitch; for salience must be reduced when a number of different tonal components of slightly varying frequencies are combined. While the pitch does not vary, brightness must necessarily decrease and pass into dullness; thus the characteristic of the vocalic sound is presumed to appear.

I have myself been unable to make but a casual observation with the aid of an Appunn tonometer upon the effect of compounding tones of slightly varying pitch. If one adds successive tones of slightly varying pitch on this instrument the sound becomes rapidly confused and noisy, though the tonal character of the whole does not readily disappear. But there is also noticeable a nasal quality which seemed to be most clearly manifest after the addition of a single tone. The nasal quality impressed me as vocalic and since the smallest increment on the tonometer was four vibrations, I suspect that the threshold for vocalic sounds would lie within this range of vibrational difference; though possibly the number of components necessary to reduce the saliency of pitch is more important than the range of the components.

Until further investigations have been made the whole matter is merely one of conjecture. But Jaensch's experiments are certainly suggestive, and I find nothing in the results of Köhler, Schole, or Miller to discredit them. Hence I conclude that even if brightness and pitch correlate directly with increase of vibrational frequency in the upward trend

[1] *Zsch. f. Psychol.*, 1913, 67, pp. 261 ff.

of simple tones, brightness may nevertheless manifest a variability independent of pitch if the conditions of sound-production are such that a varied number of closely graded vibrational components unite to produce a series of sounds; for under these conditions a series is conceivable which would pass gradually from tone through vowel to noise without a noticeable change of pitch or intensity; and, at least so far as concerns the appearance of the vowel sound, without an alteration of volume.

As the sounds become noisy, we would be dealing with

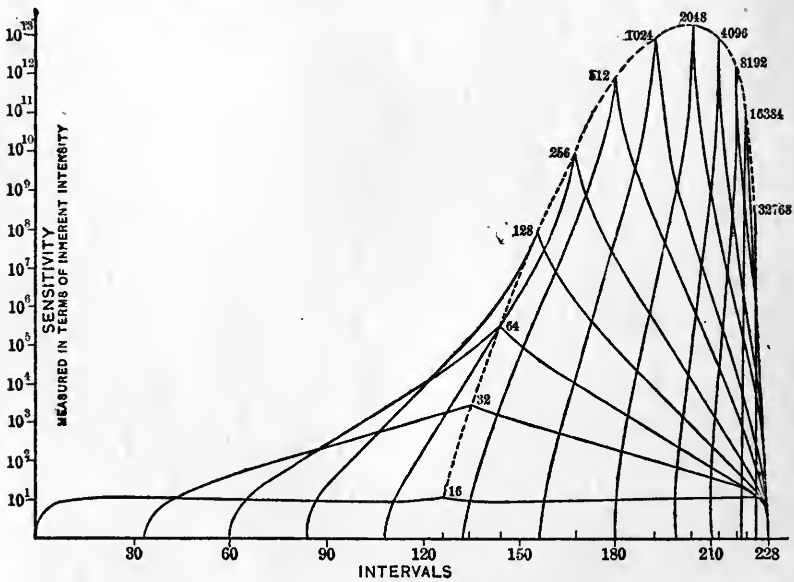

FIG. 1

components which no longer hold together in a single uniform impression, after discriminable pitch, intensity and volumic differences had one or all made themselves felt. In this connection the variation of inherent intensity with difference of pitch is also worth noting, for excepting the region of 2,000 vibrations and thereabouts, one cannot alter vibrational frequencies very much without introducing a noticeable difference of intensity which would tend to destroy the uniformity of effect even if the objective conditions of vibrational amplitude were to remain fairly constant.

Addendum

Since the preceding article was submitted for publication an opportunity has been afforded me to discuss the physical aspect of Jaensch's theory with Professor Dayton C. Miller. Although he had not read Jaensch's papers on the subject, Professor Miller finds no evidence for the "mixed sine curves" of Jaensch in his physical analysis of the vowels. But although he accepts the Helmholtz hypothesis, rather than Hermann's formant theory, his analyses show that the characteristic of a vowel is a *fixed* region of resonance, within which region must appear some partial of the fundamental tone upon which the vowel is uttered. Lacking an appropriate partial the vowel is not given. Furthermore the region of resonance in each case extends over a considerable range of pitch and the physical aspect of the significant partial is not that of a sharply defined tone, but rather that of a distribution of energy in which the amplitude of the partial in question is limited by the form in which the energy is distributed over the fixed region of resonance. Thus, if the partial falls near the middle of the resonating region of a certain vowel, its utterance is more pronounced than if it falls near the extremes of the region. But in any case the adjacent parts of the tonal region are also involved, since the distribution of a fairly constant amount of energy over the entire region is requisite to produce the vowel.

Miller's results are therefore not entirely at variance with Hermann's theory of the formant or fixed tone, which Jaensch also accepts; although Miller regards the production of the formant as a phenomenon of sympathetic resonance, while Hermann refers it to an independent "anaperiodic" blowing of the mouth resonator.

As regards the point at issue, whether the vowel is differentiated from tone by an attributive variation, Miller's analysis seems to show that not one but a number of auditory receptors are involved in its production. This is likewise the view of Jaensch. Whether the "mixed sine curves" are products of the sound wave or functions of the ear mechanism is of secondary importance if the mode of stimulation is in either case such that what we hear is a mixture of adjacent pitches. If, as Miller agrees, a "sharp" tone in the characteristic region of resonance would tend to destroy the vowel effect, then a *dull* tone aroused by stimulating a region of resonance rather than a single resonator is the phenomenal basis of the vocalic sound. Our tentative assumption that brightness may vary independently of pitch is therefore feasible, and the way is open to determine the threshold of this variation by devising experimental means for securing a regularly graded transition from the simple, sharply defined resonance of a single vibrational frequency to the regional resonance involving a more extended series of receptors. The experimental problem is perhaps none too simple, for the variations may involve shifting amplitudes and differences of phase rather than a direct attack upon several receptors at once. In a brief paper presented before the Sixth German Congress for Psychology,[1] Jaensch suggests that the synthetic effects which occasion vocalic sounds are a result of successive waves which introduce complications of amplitude not subject to the Fourier analysis; yet the brevity of his report leaves me uncertain that I clearly understand what he means. However, despite all technical difficulties in the control of the experimental conditions, empirical methods would probably yield a means of demonstrating whether or not brightness and dullness can be varied without a corresponding alteration of pitch; and we must await such an experiment before we can be assured that brightness and pitch are separable aspects of sound.

[1] Cf. *Bericht*. Leipzig: Barth, 1914, pp. 79 ff.

IS LACK OF INTELLIGENCE THE CHIEF CAUSE OF DELINQUENCY?

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A great deal of controversy has raged about the question of the relative importance of intelligence (or the lack thereof) as a causal factor of delinquency. In the present paper I wish to discuss the bearing which actual modern statistical findings have upon this issue, in order to clear up a certain amount of confusion and misconception which seems to me to exist. Of course, in the last analysis, the answer arrived at through statistical or other methods comes back logically to the definitions of intelligence and delinquency explicitly or implicitly used by some particular author. But, to some minds, the fact that the statistician deals as a rule with objective fact, and uses methods which, as methods, are logically beyond criticism, means that the findings arrived at are also beyond cavil. It seems worth while therefore to point out that the very opposite is true. Statistical findings need not only all of the scrutiny and criticism so lavishly given to conclusions arrived at by less objective methods, but in addition there is need of the careful checking of the conclusions so far as they are interpretations of the statistical constants.

As a case in point let us take the statistical findings of Goring.[1] Goring found a correlation of +0.66 between criminality and mental deficiency, and this coefficient is considerably higher than any he found to exist between criminality and any of the other factors which he investigated. He sums up as follows: "Our final conclusion is that English Criminals are selected by a physical condition, and a mental constitution which are independent of each other—that the one significant physical association with criminality is a generally defective physique; and that the one vital mental

[1] 'The English Convict.'

constitutional factor in the etiology of crime is defective intelligence."[1] ". . . our interim conclusion is that, relatively to its origin in the constitution of the malefactor, and *especially in his mentally defective constitution*,[2] crime in this country is only to a trifling extent (if to any) the product of social inequality, or of adverse environment, or of other manifestations of what may be comprehensively termed 'the force of circumstances.'"[3] While the second conclusion is stated tentatively, Goring makes it plain that he believes that it should be accepted in the absence of contrary evidence based on better data. His position seems to me to be well stated by Miner[4] as follows: "While there is always a possibility of finding some other factor closely related to delinquency and independent of capacity, nevertheless we should hardly urge this possibility at the present time as overweighing the accumulation of negative evidence which has been assembled in recent years, especially at the Galton Laboratory."

In other words, Goring guards himself sufficiently against the possibility that further research *may* reverse his findings through the discovery of new and better evidence. He does not seem to see that the future *must* reverse his conclusions, or, better, that his conclusions simply do not follow from his statistics. He is correct when he states that the Intelligence-Delinquency relation is the most important relation measured so far statistically. He is absolutely wrong when he claims that, in the absence of other data, we must accept his conclusions as the nearest approach to the truth attained thus far. For if the coefficient of the correlation between intelligence and delinquency is + 0.66, the correct conclusion to be drawn is that it is exceedingly probable that factors other than intelligence are of *greater* importance as determinants of crime than intelligence.

This conclusion follows from the following considerations. If we have a number of variables one of which is of special interest, it is possible to express the relation of these variables

[1] 'The English Convict,' p. 263.

[2] Italics mine.

[3] *Ibid.*, p. 287.

[4] 'Deficiency and Delinquency,' p. 228.

in a single equation. Let D stand for delinquency, I for intelligence, and N for a combination of all factors other than intelligence. The equation will then take the form[1]

$$d = i\left[r_{di \cdot n}\frac{\sigma_{d \cdot in}}{\sigma_{i \cdot dn}}\right] + n\left[r_{dn \cdot i}\frac{\sigma_{d \cdot in}}{\sigma_{n \cdot di}}\right]$$

In order to understand the meaning of this equation a certain amount of explanation will be necessary for the reader not versed in this form of mathematics. The explanation however has been confined to the minimum which is absolutely necessary, and, if the reader will take the mathematical assertions for granted, he will be able to follow the argument of the rest of this paper. The expressions in brackets are the coefficients of partial regression. They are, in effect, the measures of the relative importance of intelligence and of the other factors. For the sake of simplicity of statement let us assume that the sum of the factors other than intelligence can be summed up under the term *economic status.* Then if the measure of the intelligence of any given individual is known, and if that measure is multiplied by the coefficient of partial regression of intelligence on delinquency, the result will be the measure of the delinquency of that individual which one would expect from his intelligence, independent of his economic status; and if the measure of the economic status of that individual is multiplied by the coefficient of partial regression of economic status on delinquency, the result will be the measure of the delinquency of that individual which one would expect from his economic status, independent of his intelligence. The sum of the two terms will be the total expected delinquency of the individual.

The expression $r_{di \cdot n}$ is the coefficient of partial correlation of intelligence and delinquency. It measures the correlation of delinquency to intelligence, independent of economic status. If, for example, the entire population were arranged into classes according to economic status, so that all individuals within any one group were equal in wealth to all other members of their group, then, within any such group,

[1] In this equation, D, I, and N are expressed as deviations from their respective means in terms of their respective standard deviations.

differences in delinquency cannot possibly be related to differences in economic status, because, within any such group, there are no differences in economic status. Therefore, within any such group, the correlation of intelligence to delinquency is independent of economic status. Similarly, the coefficient of correlation $r_{dn \cdot i}$ is the measure of the correlation of economic status to delinquency, independent of intelligence.

The reader should be careful not to confuse coefficients of correlation with coefficients of regression. The one is a measure of relation; the other is a measure of relative importance. Consider for example the effect of the moon, the sun, and the planet Jupiter upon the height of the tides. If we were able to measure with absolute accuracy the influence of each of these heavenly bodies, the partial correlation between the position of Jupiter and the height of the tides would be well nigh perfect, but the relative importance of the position of this planet would be negligible.

Having explained the meaning of the terms of our equation, we may return to the consideration of the equation itself. In any given actual case, the right and left sides of this equation will not be exactly equal. For example, if we estimate the degree of delinquency of any given individual from his known intelligence and from the other known causes, the estimated delinquency will be likely to differ from the actual delinquency because we are dealing with imperfect measures of intelligence, environmental influence, etc., and because we are sure to have left some of the causal factors out of account. If however we imagine ideally perfect conditions—if all the causes of delinquency were known and accurately measured—the two sides of the equation would be exactly equal. Further, both $r_{dn \cdot i}$ (the coefficient of the relation of delinquency to factors other than intelligence, intelligence being constant) and $r_{di \cdot n}$ (the coefficient of the relation of delinquency to intelligence, all other factors being equal) would be equal to unity, because, under the ideal conditions imagined, the degree of intelligence would of course be a perfect measure of delinquency, so far as caused by lack of intelligence, and "all other factors" would measure perfectly "all other resultant delinquency."

The above considerations open up the possibility of subjecting Goring's conclusions to *quantitative* criticism. For the present I would direct the attention of the reader to the following quantitative problem. Given a correlation of + 0.66 between lack of intelligence and delinquency, under what circumstances will the correlation to delinquency of a combination of all other causal factors be greater than + 0.66?

TABLE I

1 *DN·I*	2 *DI·N*	3 *DI*	4 *IN*	5 *DN*
1.00	1.00	0.66	−0.20	0.604
1.00	1.00	0.66	−0.14	0.652
1.00	1.00	0.66	−0.13	0.659
1.00	1.00	0.66	−0.12	0.667
1.00	1.00	0.66	0.00	0.751
1.00	1.00	0.66	0.20	0.868
1.00	1.00	0.66	0.40	0.953
1.00	1.00	0.66	0.60	0.997
1.00	0.66	0.66	0.66	1.000

Table I. supplies the answer to the problem. Column I. shows the coefficients of partial correlation between delinquency and the factors other than intelligence, intelligence being constant. The coefficients are 1.00 in every case under the assumed ideal conditions. Column 2 shows the coefficients of partial correlation of delinquency to intelligence, all other factors being constant. These coefficients are 1.00 in every case except in the special case where r_{dn} is 1.00, when $r_{di \cdot n}$ will take the value of + 0.66 for reasons which will be easily understood. Column 3 shows the coefficients of correlation of delinquency to lack of intelligence, which are + 0.66 in every case by definition. In column 4 there are a number of assumed coefficients showing a number of possible values of the correlation of lack of intelligence to all other factors. Column 5 shows the coefficients of correlation between delinquency and all factors other than intelligence which would *necessarily* result from the conditions assumed in the other 4 columns. Thus, taking the first horizontal line, if the coefficient of correlation of lack of intelligence to delinquency is + 0.66, if the coefficient of correlation of intelligence to the other factors − 0.20, and if the analysis is ideally complete as indicated by the coefficients of partial correlation of columns 1 and 2, then the coefficient of correlation of the factors other than intelligence to delinquency will necessarily have a value of + 0.604.[1] And from the rest of the table we see that it will have a value equal to or greater than + 0.66 if the coefficient of the correlation of lack of intelligence to the other factors is between − 0.13 and + 0.66.

Common sense would indicate (in a situation involving only 3 variables), that the above also shows the limits within which the factors other than intelligence are of greater importance than intelligence. But as a quantitative statement of the coefficients of importance (regression) may be of some interest, I have tabulated them in table II, which is merely an extension of table I with the first three columns omitted. The reader will remember that the coefficient of correlation of intelligence to delinquency is 0.66 in every case.

TABLE II

4 Correlation *IN*	5 Correlation *DN*	6 Regression *DI · N*	7 Regression *DN · I*
−0.20	0.604	0.8134	0.7667
−0.14	0.652	0.7658	0.7587
−0.13	0.659	0.7586	0.7577
−0.12	0.667	0.7505	0.7567
0.00	0.751	0.6603	0.7513
0.20	0.868	0.5068	0.7667
0.40	0.953	0.3306	0.8197
0.60	0.997	0.0968	0.9391
0.66	1.000	0.0000	1.0000

Columns 4 and 5 are identical with columns 4 and 5 of Table I. Column 6 shows the coefficients of partial regression of intelligence on delinquency, all other factors being constant, and column 7 exhibits the coefficients of partial regression of

[1] The coefficients of column 5 were computed by solving the conventional equation for a coefficient of partial correlation for r_{dn}. For this formula as well as the other formulæ utilized in this paper, see Yule's 'An Introduction to the Theory of Statistics,' Chap. XII.

the factors other than intelligence on delinquencey, intelligence being constant. Thus under the conditions of the first horizontal line of Tables I. and II., the importance of intelligence is to the importance of the other factors as $+0.8134$ is to $+0.7667$. We see that "the other factors" begin to be of more importance than intelligence when their correlation with intelligence is -0.12, and, as this last relation becomes positive and increases in value, the other factors become twice, and even ten times as important as intelligence. Although I do not mean to enter into the psychological aspects of the case in the present paper, I may remark in passing that these figures become highly suggestive if one believes that *character* is closely related to *intellect* without being in any sense identical with it.

At any rate, it follows from these figures that lack of intelligence is *not* the most important factor in the causation of delinquency *unless we have a right to assume that the coefficient of correlation of intellignece to the other factors is negative and greater than* -0.12. But according to the best of our present knowledge, the very opposite is very probably true. We may divide the causal factors of delinquency other than lack of intelligence into environmental factors and factors peculiar to the individual. So far as the environment is concerned, we know that poverty, absence of parental care and supervision, and other evils in and out of the home predominate amongst the ignorant. Indeed it is often urged by those who wish to minimize the importance of the environment that the poor are stupid not because they are poor, but that they are poor because they are stupid. For our present purpose, we are not concerned with this issue except to note that ignorance and poverty and other environmental factors go together and cannot therefore be negatively correlated. Therefore, so far as we have gone, we have no reason for believing that Goring has shown that lack of intelligence is the most important factor in the causation of delinquency. Our evidence, so far, points in the opposite direction. So far as the factors peculiar to the individual are concerned nothing very definite can be said in the present state of our knowledge.

Definitions in this field are either lacking or are so vague and general that they tend to confuse rather than clarify issues such as the one we are considering. Intelligence, for example, is usually defined as the ability to adapt to the environment, and, inasmuch as anything which makes for detected delinquency necessarily makes for maladaptation, other factors would be ruled out by definition. It is however worthy of note that many authors hold to the existence of factors other than intelligence and therefore depart from the all comprehensive definition of intelligence, implicitly at any rate. Obviously then it is impossible to say anything very definite about the probable sign of the correlation existing between intelligence and other causes of delinquency. In the near future the writer hopes to show that intelligence and moral character, when subjected to functional psychological analysis, have many factors in common, so that there is every reason for believing the relation between them to be positive. At present it must suffice to point out that there is no reason for believing it to be negative. And, as there is good ground for believing that the relation of intelligence to the environmental factors is positive, there would seem to be good grounds for holding that lack of intelligence is not the most important cause of delinquency, and no grounds at all for holding that it is.

But even though lack of intelligence is not of greater importance than all other factors taken in the aggregate, it may be urged that a correlation of + 0.66 shows that it is likely to be the largest *single* factor. The following hypothetical example will show that that is not at all likely to be the case. Suppose that in addition to lack of intelligence there are six other known causes of delinquency, X_1, X_2, X_3, X_4, X_5, and X_6, or seven causes altogether. Let the correlation of each of these seven causes to delinquency be + 0.66, and let all the possible intercorrelations of the seven causes with each other be + 0.50. Let the relation of delinquency to these seven causes be expressed in a single equation, as on page 149. Then suppose that the degree of delinquency and the intensity of the seven causes under consideration is known with perfect accuracy for a very large number of individuals and that, for each individual, these values are entered into the equation. Then, if our seven causes have furnished us with a complete analysis of the causes of delinquency, the right and left sides of the equation will be exactly equal in each and every individual case. If the analysis is incomplete, that is if there are causes not included under our seven, there will be differences between the actual and the estimated degrees of delinquency. Now if the coefficient of the correlation existing between the actual and the estimated values be determined, we have in that coefficient a measure of the closeness of our approach to completeness of analysis. We shall designate this special coefficient by the symbol R. If the analysis is complete, R will be equal to one. Otherwise it will be less than one and the amount by which it falls short will indicate the importance of the causes left out of account.

Now in our hypothetical example R is equal to $+ 0.873$. In other words, our analysis is incomplete even though we have taken into account six other causes of delinquency as highly correlated with delinquency as Goring found lack of intelligence to be. An idea of the incompleteness of the analysis may be gained from the fact that the average difference between the actual and the estimated delinquency would be half of what this difference would be if R were zero.[1] Indeed, if I had not shunned the labor of computation, I could easily have taken twenty causes related to delinquency as highly as our seven and still have reached an R unmistak-

[1] R would be zero only if the coefficients of correlation to delinquency of each of our seven "causes" were zero. In that case they would, of course, not be causes at all. Nevertheless our regression equation would still yield the most reasonable estimate possible in the circumstances of the degree of delinquency of any given individual. Having no knowledge at all of the causes likely to produce delinquency, it would be most reasonable to estimate the degree thereof as the average degree. For example, if one were to estimate the height of John Doe, John Doe being any individual whatever about whom nothing at all is known except that he lives in Chicago, it would be most reasonable to take his probable height to be the average height of the male citizens of Chicago. Now if we were to estimate the degree of delinquency of each and every individual by means of the regression equation based upon our seven assumed causes, our average error would be half as great as if we were to estimate that degree of delinquency to be the average degree of delinquency of the entire population.

ably below one. The truth of this last statement can be strongly suggested by showing how much each additional cause adds to the value of R in our hypothetical example. I have done so in Table III.

TABLE III

$R_{d(i)}$	= 0.660
$R_{d(ix_1)}$	= 0.762
$R_{d(ix_1x_2)}$	= 0.808
$R_{d(ix_1x_2x_3)}$	= 0.835
$R_{d(ix_1x_2x_3x_4)}$	= 0.852
$R_{d(ix_1x_2x_3x_4x_5)}$	= 0.864
$R_{d(ix_1x_2x_3x_4x_5x_6)}$	= 0.873

The first R is computed on the basis of one cause only; the second on the basis of two; the third on the basis of three, etc. It will be seen that R increases rapidly at first and then more and more slowly. It seems that a great many more causes would be needed to reach a value of one, if indeed it can be reached at all, for R may be approaching a limit of less than one. That is, if causes are intercorrelated to the degree which we have assumed in our hypothetical example, it may be impossible to reach completeness of analysis even with an infinity of causal factors.[1]

It would be possible to keep ringing the changes on the various hypothetical combinations of causal factors which might be formed. I trust what has been done will be enough to validate our claims. Summing up, they are:

1. The claim that Goring's statistics *prove* lack of intelligence to be the chief cause of delinquency, at any rate until better data are at hand, is due to a mistaken interpretation of his statistical results.

2. His results *do* show that very probably lack of intelligence is of *less* importance than all other factors combined.

3. They show, also, that lack of intelligence is probably of less importance than one or more other factors, taken singly.

In conclusion I wish to say that, in the present paper, terms such as intelligence, delinquency, etc., are used un-

[1] *Cf.* Pearson, *Biometrika,* Vol. 10, p. 181.

critically and naïvely. Whatever definitions of these terms may be implicit in Goring's data are accepted by me without question. For the present paper addresses itself only indirectly to the larger problem of the *real* significance of the causal factors of delinquency, and is concerned mainly with the correct interpretation of statistics. In the near future I hope to be able to publish some views on the more important *psychological* analysis of the factors which are summed up under such headings as intelligence and character, so far as they are causes of delinquency.

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THE PSYCHOLOGICAL REVIEW

MANIFOLD SUB-THEORIES OF "THE TWO FACTORS"[1]

BY C. SPEARMAN

PRESENT SITUATION

Few competent judges would dispute that among the most unexpected events in the psychology of the last dozen years has been the sudden spring of 'general intelligence' from an almost universal incredulity to a no less universal investment with the highest importance. In this movement I venture to claim having been among the earliest participators. Already in 1904, I went so far as to put forward an explanatory theory, namely, that of 'Two Factors.'[2]

The purport of this theory is that the cognitive performances of any person depend upon: (*a*) a general factor entering more or less into them all; and (*b*) a specific factor not entering appreciably into any two, so long as these have a certain quite moderate degree of unlikeness to one another.

The proof offered was that, on eliminating from any set of mental tests any that happened to be very obviously like others in the same set, then the intercorrelations may still remain large, but will now admit of being tabulated in a 'hierarchy.' This mode of describing the proof was soon afterwards reduced to the exact mathematical equation:

$$r_{ap}/r_{aq} = r_{bp}/r_{bq}, \qquad \textbf{(A)}$$

where a, b, p, and q indicate any of the tests and r is the

[1] In this paper I owe a double debt to Professor Nunn: firstly, for stimulating me to write; and secondly, for pointing out to me several serious obscurities when I had done so.

[2] 'General Intelligence,' objectively determined and measured, *Amer. J. Psychol.* 15, 1904.

(product moment) correlation between them.[1] This equation was said, both to hold good of experimental results, and also to prove the said theory. A corollary of the equation is that in any table of correlations as ordinarily set out, every column will have a perfect correlation with every other one. Thus the three criteria, the 'hierarchy,' equation (**A**), and a perfect intercolumnar correlation all indicate the same theory.

Now, this theory has had a curious fate. It was soon backed up by a seemingly overwhelming mass of evidence.[2] Above all, even the data expressly brought forward to refute it were without any exception obliged, Balaam-like, to give it their complete support. In particular, a very valuable, but certainly not friendly, investigation of Simpson was shown on more careful reëxamination to fulfill the hierarchy just as exactly as all the results obtained before.[3] And as for any attempt at disproving this fulfillment by either the data of Simpson or any others that had been adduced, I have laboriously searched the vast literature of mental tests for such in vain. In spite of all this, *mirabile dictu*, hardly any writer (outside of those working in more or less intimate connection with myself) has so far uttered a sign of being convinced!

Of the reasons that might be alleged for this obduracy, some are obviously improbable, such as that so many psychologists should be biased by their preconceived doctrines, or that they would decline to make themselves acquainted with the newer mathematical aids to research.

A more plausible explanation would be some widespread belief that, although the truth of the theory would necessitate the fulfillment of equation (**A**), yet this fulfillment might not, inversely, necessitate the truth of the theory. But even

[1] This equation was at first expressed in a slightly different form, see *Zeit. f. Psychol.*, 1906, pp. 84–5. Then the form (A) was communicated by the present writer to Mr. Cyril Burt, who employed it in his well-known admirable paper, *Brit. J. Psychol.*, 3, 1909.

[2] See in particular, 'General Ability, its Existence and Nature,' *Brit. J. Psychol.*, 5, 1912, and 'The Heredity of Abilities,' *The Eugenics Review*, p. 8, 1914.

[3] See 'The Theory of Two Factors,' Psychol. Rev., 21, 1914.

this suggestion now fails, since a recent luminous paper by Garnett has shown that also this inverse relation holds good.[1] As his demonstration is rather long and difficult (but proportionately instructive), the following very simple one may be of service. It has been used by myself for many years, but never published.

Let r_{xy} denote the correlation between two variable tests, x and y. It can be written as $f(p)$, where p is any one of the elements entering into x and contributing to its correlations, the remaining elements being regarded as parameters. Similarly, r_{xz} may be written as $\phi(q)$, where q is any of the elements entering into and contributing to the correlations of y. But, by equation (**A**),

$$f(p)/\phi(q) = r_{xy}/r_{xz} = \text{constant}.$$

Hence, p and q cannot possibly be independent of each other; there can be only one independent; and this necessary singleness is at once extensible to the whole set of functions in question.

"A Hierarchy without a General Factor"

Faced, then, on all sides by this elusively silent 'passive resistance' to the theory of Two Factors, it was a great pleasure to myself, anxious to get the matter settled, when at last one (and only one) writer did step into the open field and challenge the above-mentioned evidence on definite grounds. This was G. H. Thomson, the tenor of whose argument was to admit that the theory is proved whenever equation (**A**) is satisfied *exactly*, but to deny that it even approaches being proved whenever (**A**) is only satisfied with close approximation.[2]

Very possibly, indeed, it is just this paper which has been in large measure responsible for the said 'passive resistance.' This would appear to be indicated by such statements as the following:

"Thomson has shown in a recent paper that Spearman's

[1] 'On Certain Independent Factors in Mental Measurements,' *Proc. Roy. Soc.*, A, **96**, 1919.

[2] 'A Hierarchy without a General Factor,' *Brit. J. Psychol.*, 8, 1916.

method of calculation of data which led to his conclusion of the existence of a general intellective factor—a 'general ability' as against special abilities—is open to the gravest criticism. Thomson attacks the concept on purely mathematical grounds, but his reasoning would appear to be unquestionably accurate."[1]

As to whether this final comment can be accepted without reservation we shall see shortly. But in any case Thomson's method of proof is a notable contribution to the subject and will serve, I believe, to clear up much obscurity and misunderstanding.

He constructed tables of correlations artificially, by an extension of the method of Weldon.[2] Each function—here representing the marks obtained in a single test by a single individual—consisted in the total throw of a set of dice. Of course, there was a separate throw made for each individual. But there was not a completely separate throw for each test; instead, some of the dice were marked beforehand and their resulting points were counted for two or more tests in common. The result is a correlation amounting, as Weldon showed, to the proportion of dice used in common; thus, if 4 dice were counted to two tests in common, whilst 6 other dice were thrown for each test separately, then the correlation between the two would on a large number of throws tend towards 4/10 = .4. Moreover, instead of going through the tedious operation of making the throws actually, the result of an infinite number of them can easily be obtained theoretically; it is given by

$$r = 1/\sqrt{(c+m)(c+n)}$$

where c is the number of dice used in common, while m and n are those used each for one only of the two functions.[3]

Proceeding in this manner, Thomson constructed the fol-

[1] Fernberger, *J. of Appl. Psychol.*, 1, p. 197, 1917.

[2] Cited by Edgeworth, Encycl. Brit., 10th ed., XXVIII., p. 282.

[3] The equation at once got from the 'Correlations of Sums or Differences,' *Brit. J. Psychol.*, 5, p. 419, equation (2). The term on the right hand becomes:

$$\frac{c(r=1)+S(r=0)}{\sqrt{c+m+2S(r=0)}\sqrt{c+n+2S(r=0)}}$$

lowing arrangement, in which 36 of the elements (dice) 'run through more than one test each, but never through all.'

TABLE I

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
a..........	x	x	x	x	x		x	x	x	x	x	x	x	x	x			
b..........	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x			
c..........	x	x		x		x	x			x	x			x				
d..........	x		x		x			x				x			x	x		
e..........		x			x				x				x				x	x
f..........			x				x			x							x	
g..........				x				x					x					
h..........						x						x					x	
k..........											x				x			x
l..........											x				x			x

	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	*S*
a.......					x			x		x	x	x		x	x	x	x	x	0
b.......														x	x	x	x	x	0
c.......						x	x	x		x	x	x		x	x	x	x	x	1
d.......						x	x	x	x	x				x	x	x	x	x	3
e.......						x					x		x	x	x	x			9
f.......	x	x							x		x								14
g.......	x		x	x				x					x						16
h.......			x		x					x									20
k.......		x	x									x							22
l.......		x		x	x														24

In this table, the heading numbers denote the dice thrown, while the letters are the tests to which they were counted. Under the heading S, are given the number of throws counted only to a single test, namely that indicated by the letter in the same row.

Now, from this arrangement of the dice there resulted the following set of intercorrelations of the tests [Table II.]

In this Table, he says, the columns correlate with one

TABLE II

	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*k*	*l*
a........		867	730	593	356	174	167	120	116	112
b........	867		650	550	341	143	137	088	084	082
c........	730	650		500	292	143	091	088	084	041
d........	593	550	500		244	095	091	088	042	041
e........	356	341	292	244		093	089	043	041	040
f........	174	143	143	095	093		044	042	040	039
g........	167	137	091	091	089	044		040	039	037
h........	120	088	088	088	043	042	040		037	036
k........	116	084	084	042	041	040	039	037		034
l........	112	082	041	041	040	039	037	036	035	

another, not indeed exactly, but quite as well as in the cases adduced by me on behalf of the theory of a general factor, and yet in his arrangement given above there is 'nothing approaching a general factor.' From this he concludes that the question as to whether mental tests really do demonstrate the existence of a general factor will require 'a very much more extensive set of experiments than has yet been attempted.' And as he remarks that even a thousand cases would be insufficient, the outlook for further research would appear to be arduous.

This demonstration, unfortunately, was brought forward at a time which to me appeared inopportune for scientific controversies. Hence, my reply consisted—unpardonably, save for the extraordinary circumstances—in a brief note, not so much giving my arguments, as indicating what general lines they would in due course follow.[1]

For one thing, it was indicated that this new view could be shown not to rest upon a solid foundation by the fact of its proposing to settle the point through more extensive experiments; my note suggested, on the contrary, that not even a million cases could possibly produce a hierarchy distinguishable from such as could be constructed by the new method. This suggestion I will now endeavor to justify, showing that by such a method even the most perfect hierarchy can be constructed. Suppose, for instance, that we wanted to get the following tables of correlations between the tests (or other functions) *a*, *b*, *c*, *d*, and *e*, where the columns intercorrelate quite perfectly and equation (**A**) is satisfied exactly.

Table III

	a	*b*	*c*	*d*	*e*
a..........		40	30	20	10
b..........	40		24	16	08
c..........	30	24		12	06
d..........	20	16	12		04
e..........	10	08	06	04	

Following the new method precisely, we can at once get this Table III. if we put together our dice or other elements in the following manner [Table IV]:

[1] *Brit. J. Psychol.*, 8, 1916, p. 284.

TABLE IV

Tests	Dice													
	1 to 20	21 to 35	36 to 47	48 to 57	58 to 65	66 to 71	72 to 76	77 to 80	81 to 83	84 to 85	86 to 91	92 to 105	106 to 129	130 to 165
a....	x	x		x			x							
b....	x		x		x			x			x			
c....		x	x			x			x			x		
d....				x	x	x				x			x	
e....							x	x	x	x				x

The above table means that, for instance, the first 20 dice have entered into tests *a* and *b* only; the next 15 dice into *a* and *c* only; similarly, the others. Obviously enough, purely specific dice may be added to any extent and in any manner, without disturbing the hierarchy at all.

Thus, the new argument against the theory of Two Factors proves *too much*. By its results—since the theory is admittedly true when (**A**) is satisfied exactly—it comes to suicide.

THE DICE HIERARCHIES FURNISH NO EVIDENCE

But the preceding conclusion leaves us with a paradox. How are we going to explain the seeming fact that the arrangement given in Table IV. does *not* assign any of the dice to all the tests, or indeed to more than two? My original note asserted that Thomson had, after all, let in a general factor by a 'back door.' Where is there any such thing? And of what nature is the general factor really? And of what nature is it?

Moreover, some justification is needed for the charge in my note against the dice arrangements as being "arbitrary." We will take this last point first. Let M_{ax} and M_{px} denote the total marks obtained by the individual x in the tests a and p respectively, so that

$$M_{ax} = A_1\alpha_{1x} + A_2\alpha_{2x} + \cdots + A_m\alpha_{mx} + R_{ax},$$

and

$$M_{px} = P_1\alpha_{1x} + P_2\alpha_{2x} + \cdots + P_m\alpha_{mx} + R_{px},$$

where the α's denote the elements (dice) common to any two or more of *all* the tests belonging to the domain in question, any of the A's or P's may be either $+1$ or 0, and the R's denote the sums of the elements specific to the two tests respectively.

Let each M and also each α be measured from its arithmetical mean for all individuals as origin; and let the units of measurement be such that the sum of the squares of M for all individuals is in each test = 1. Then, $A_h \cdot P_k \cdot \Sigma_x(\alpha_{hx} \cdot \alpha_{kx})/n = 0$, if the elements h and k are different; but if they are the same, it may be written as $A_h' \cdot P_k' \cdot \Sigma(\alpha^2)/n$, where $\Sigma(\alpha^2)$ is constant for all elements. Hence, we get for the correlational coefficient between a and p:

$$\begin{aligned} r_{ap} &= \Sigma_x(M_{ax} \cdot M_{px}) \\ &= A_1' \cdot P_1' \cdot \Sigma_x(\alpha_{1x}^2) + \cdots + A_m' \cdot P_m' \cdot \Sigma_x(\alpha_{mx}^2) \\ &= \Sigma(\alpha^2)(A_1' \cdot P_1' + \cdots + A_m' \cdot P_m'), \qquad \textbf{(B)} \end{aligned}$$

Using analogous symbols for another test q, and assuming the validity of equation (**A**), there results immediately:

$$A_1' \cdot P_1' + \cdots + A_m' \cdot P_m' = \lambda(A_1' \cdot Q_1' + \cdots + A_m' Q_m'), \quad \textbf{(C)}$$

or

$$A_1'(P_1' - \lambda Q_1') + \cdots + A_m'(P_m' - \lambda Q_m') = 0. \quad \textbf{(D)}$$

Let us now, keeping the tests p and q constant, allow a to change its constitution. For every variation of it we get, if (**A**) holds good, another equation of the same form as (**D**). But there cannot simultaneously subsist a greater number of independent equations having that form than $m_b' - 1$, where m_b' is the number of A's having any of their coefficients in these equations not zero. Hence, the variation of a satisfying (**A**) are confined to $m' - 1$ points, and therefore cannot possibly cover any continuous (or even 'dense') region at all, however minute; for any such has an infinity of points. We cannot even take m' to be very large, for then the luck of the throwing would be equalized for the different individuals, giving them all equal average marks, so that no correlation would be determinate at all. This proves that, whatever import the dice hierarchies may possess, at any rate they do not touch the evidence arrayed on behalf of the Theory of Two Factors, for there the test a was by no means confined to a set of selected points, but, in general, varied at random. The objection against arguing from values selected arbitrarily appears, then, to be substantiated.

Where the General Factor Lies

But although the dice hierarchies are thus once more put out of court as regards the controversial employment attempted, nevertheless their intrinsic interest may well claim for them some further examination. In particular, we have not yet found out what actually constitutes the general factor in tables such as II. and III.

Now, for M_{ax} really to consist of a latent general factor together with a specific factor (as maintained by the theory in question) it is *necessary* if these factors are additive, and it is *sufficient* in any case, that we should be able to re-write

$$M_{ax} \text{ as } w_a \cdot g_x + s_{ax} \equiv w_a \cdot h_x \cdot g + s_{ax}, \qquad (E)$$

where g_x is some function of the α's, g is its mean value and thus constant throughout, h_x is constant for the individual x with all tests, w_a is constant for the test a with all individuals, whilst s_{ax} varies from individual to individual independently of both g_x and s_{bx} (b indicating any further test).

Let us see, then, whether and how the above conditions for g_x, s_{ax} and w_a are fulfilled by equation (A).

We require, first, $r_{(g_x)(s_{ax})} = 0$.

But this coefficient $= r_{(g_x)(M_{ax}-g_x)} = \sigma_g \cdot r_{ag} - w_a \cdot \sigma_g^2$, expanding by known formula[1] and remembering that

$$\sigma^2_{M_{ax}} = w_a^2 \cdot \sigma_g^2 + \sigma_{sa}^2 = 1.$$

Hence, our condition will be fulfilled by $r_{ag} = w_a \cdot \sigma_g$, which we may write as w_a'.

We also want $r_{(s_a)(s_b)} = 0$. But this equality, as we see by similarly expanding r_{ab} into $w_a \cdot w_b \cdot \sigma_g^2 + \sigma_{s_a} \cdot \sigma_{s_b} \cdot r_{(s_a)(s_b)}$, will hold good when

$$r_{ab} = w_a \cdot w_b \cdot \sigma_g^2 = w_a' \cdot w_b' \qquad (F)$$

Actually by equation (A), on the other hand,

$$r_{ab} = r_{ap} \cdot r_{bq}/r_{pq} = r_{bp} \cdot r_{aq}/r_{pq}$$
$$= [(r_{ap} \cdot r_{aq})^{1/2} \cdot r_{pq}^{-1/2}][(r_{bp} \cdot r_{bq})^{1/2} \cdot r_{pq}^{-1/2}] \qquad (G)$$

for all combinations of p and q; it therefore may be written as $v_a \cdot v_b$ and thus has precisely the form required by (F).

[1] 'Correlation of Sums or Differences,' *Brit. J. Psych.*, V., 1913, p. 419.

There only remains, then, to so determine the function g_x that w_a' or $r_{ag} = v_a$. This done, w_a can at once be taken as v_a/σ_g.

Such a determination is readily supplied by the sum of the scores of the individual x in all the tests. For then, expanding as before,

$$r_{ag}^2 \equiv r^2{}_{(a)(a+b+\cdots+z)}$$
$$= [1 + r_{ab} + \cdots + r_{az}]^2/[z + 2S_p(r_{ap}) + 2S_{pq}(r_{pq})],$$

where p and q take all values different from each other and from a, whilst z is the number of tests,

$$= [(z-1)(z-2)\overline{r_{ap}\cdot r_{aq}} + 2(z-1)\overline{r_{ap}} + (z-1)\overline{r^2{}_{ap}} + 1]$$
$$\div [(z-1)(z-2)\overline{r_{pq}} + 2(z-1)\overline{r_{ap}} - 1]$$
$$= \overline{r_{ap}\cdot r_{aq}}/\overline{r_{pq}} = v_a^2, \text{ again just as required,} \qquad (H)$$

where the scorings indicate means for all combinations of p and q, in the case of the equation (A) holding good for any finite domain (as it actually does, according to the empirical evidence), so that z becomes infinitely large and its lower powers vanish in comparison with the squares.

Even in the case of equation (A) applying only to some isolated points, our expression for g_x remains an approximation, and can easily be made exact by appropriately changing the relative weights of the α's entering into it.

On the whole, then, g may be regarded as the unit of "General Intelligence," h_x indicating how much of it is possessed by the individual x, whilst w_a shows what relative scope for its influence is afforded by the test α.

This analysis of the general factor and its properties derives no little interest from the fact that our whole argument, although based on the dice correlations, is at once capable of generalization to all product-moment correlations whatever, merely by removing the restriction of the A's, B's, etc., to the values of $+1$ and 0. This follows readily if we (with Bravais himself) assume that any of the functions concerned can be represented with sufficient approximation by Taylor's expansion to the first differentials. For then we get, using f as the symbol of function:

$$f(\alpha_1, \alpha_2, \cdots \alpha_z) = A_1\cdot\alpha_1 + A^2\cdot\alpha_z^2 + \cdots + A_z\cdot\alpha_z,$$

where the A's are now the first differential coefficients and may have any values.

Form of Expression

There was yet a fourth pledge in my original note, namely to show that the dice hierarchies, besides re-introducing the general factor unnoticed, did so in a *form* that was psychologically altogether untenable.[1] This pledge also, I will now try to redeem, but with the qualification that such psychological untenability must not be taken to imply psychological lack of importance and interest.

If we consider generally the form and the fact of dependence, the difference between them appears to be profound. As regards the fact, a function contains, in general, a determinate number of totally independent variable elements; this number measures its grade of freedom, its dimensionality. But the function's form of expression, on the contrary, can introduce any number desired of what may be called partially independent variables. For example, x can always be transformed into $a_1x_1 + a_2x_2 + \cdots + a_nx_n$ where x_1, x_2, x_{n-1} as also a_1, $a_2 \cdots a_n$, may have any values whatever, so long as the power is retained of connecting them together again by means of the still remaining x_n. The latter need only be made $= (x - a_1x_1 - \cdots - a_{n-1}x_{n-1})/a$. Or, instead of introducing x_n among the variables, some *condition* imposed on them (as done by equation **A**) can have just the same effect. Again, any function of any two variables, x and y, can equally well be expressed as functions of two fresh variables, for instance, of r and θ where $r^2 = x^2 + y^2$ and $\tan \theta = y/x$; r and θ are just as independent of each other as were x and y.

In many cases, the choice of form between the infinitely numerous possible alternatives is dictated merely by convenience or lucidity. But in other cases, the form, even if it does not positively state, at any rate strongly suggests, some facts. In physics, for instance, all the units employed are mathematically reducible to those of mass, length, and time.

[1] *Brit. J. Psychol.*, 1916, 8, p. 284.

Yet in practice, such highly significant units are usually employed as 'force' (= MLT^{-2}), or of 'work' (= ML^2T^{-2}), or even of 'power' (= ML^2T^{-3}). Here, whilst the number of totally independent elements constitute the fundamental theory, the form of expression implies a *sub theory*.

If, now, we look at the properties of our general factor shown to be latent in the dice hierarchies, we see from (D) that they happen to be of the class that do imply facts. For they are such that any of the α's entering into any test indicated by a can disappear when the a passes to b indicating another test, but then some other α is obliged to arise in its place in order to keep the correlation with any further test b still in accordance with equation (D).

Does not this imply that, the more the test b resembles the first test indicated by a, the more it must resemble the second test so indicated however unlike the first one?

And is not this result absurd? The answer to both questions, I think, must be in the affirmative, so long as the α's denote any *phenomenological* characters whatever. And by 'common elements' psychologists do, as a matter of fact, habitually mean something phenomenological. But suppose that, instead, we transcend the universe of phenomena and take the α's to denote portions of some hypothetical underlying 'force' which is equally effective for varied phenomena. Upon this basis, there is no longer any absurdity, but a necessity, that for any lost α, there should always become available another one. With such interchangeable force, however, we have arrived at neither more nor less than the hypothesis of a common fund of energy (and incidentally, we have in large measure transferred the burden of further proof from psychology to physiology).

It may be observed that essentially the same position reoccurs in the recently published view of Otis, according to which each test has its own particular degree of 'spread' of elements.[1] For on this view, the fact of test p 'spreading' so as to contain many common elements with test a_1 involves its also 'spreading' so as to contain many common elements with any other test a_2.

[1] *J. Educ. Psychol.*, 1918, 9, p. 345.

Another form of expression may perhaps be derived from the very interesting work of McCall.[1] He finds that in several cases the intercolumnar correlations, far from approaching to + 1.00, come very nearly to the exact opposite, − 1.00. This, he urges is in polar contradiction to the Theory of Two Factors. But such an argument rests upon a mere misapprehension (quite possibly due to unprecise wording on my part); for it has long been noticed by Webb[2] that equation (*A*) is just as well satisfied by negative as by positive intercolumnar correlations, so long as they approach unity. Moreover, it is even perfectly true to say that the intercolumnar correlation is always + 1.00, so long as the reservation is made that the units of measurements should be chosen suitably. By this reservation, the positive sign can at once be restored throughout the intercolumnar correlations of both Webb and McCall. But even then, there may be indications of a new and important sub theory.

Yet another instance of a sub theory is in the course of being elaborated by the investigator already quoted, Thomson; this is that of a hierarchy arising from "Two Levels" of mental process.[3] At present, his only published evidence is 'an arrangement actually arrived at in a trial': and the value of such a single arbitrarily selected case in a statistical investigation seems to me, I must admit, open to gravest question. But when he has supplemented this by a determination of the amount of intercolumnar correlation produced by his conditions *on an average*, and has found that this average approaches to unity—thereby showing that the general factor has come in once more—then his view may lead to very interesting developments.

To close, a word may be allowed as regards my own attitude towards all these sub theories. For a long time, I have suggested their possible diversity. And—although my present preference may have made itself manifest—I

[1] Teachers College, Columbia University Contributions to Education, No. 79, 1916. The very newness of his results as compared with those obtained by other researches, however, must make us demand a proportionately ample corroboration.

[2] 'Character and Intelligence,' *Mon. Suppl., Brit. J. Psychol.*, 1915, p. 56.

[3] *Brit. J. Psychol.*, **9**, 1919, p. 337 ff.

certainly never have claimed that any considerable evidence has been produced *as yet* (either by myself or by any one else) for or against any one of them. Nor, I am inclined to think, will they ever admit of absolutely decisive evidence, since they appear to involve hypotheses incapable of more than relative probability; these hypotheses, like those involved in the theory of light, or of electricity, may always —even after a long reign of acceptance and usefulness—be suddenly upset again.

But as regards the fundamental theory, I venture to maintain that this has now been demonstrated with finality. It involves no hypothesis whatever, but is a direct mathematical deduction from equation (**A**). Nothing I can conceive will shake it, unless it be detecting some flaw in the mathematical logic, or convicting all those investigators whose work supports (**A**) of a vast conspiracy. So it would seem as if psychologists have now got definitely to accept this Theory of Two Factors; it becomes a Bed of Procrustes, into which all our doctrines must somehow or other be made to fit, even though the so doing may at times involve a not unpainful surgical operation upon them.

GENERAL VERSUS GROUP FACTORS IN MENTAL ACTIVITIES

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I. Objects of the Paper

The objects of this paper are to give a general summary, in non-mathematical language, of various scattered papers which in the writer's opinion prove the invalidity of the reasoning upon which Professor Spearman has based his Theory of General Ability, or Theory of Two Factors: to submit an alternative theory, which may be called the Sampling Theory of Ability, explaining the known facts of mental correlation at least equally as well as does Professor Spearman's theory: and, while admitting that the existence of general ability is still possible, though unproven, to give reasons in support of the greater probability of the Sampling Theory. The two theories are not necessarily exclusive of one another.

II. Historical

The controversy as to whether ability in any individual is general, or specific, or in groups or 'faculties' is a very old one, but for the purposes of the present article it is not necessary to go back prior to the above mentioned paper,[1] the first of a series in which Professor Spearman has developed his Theory of General Ability, or Theory of Two Factors, as it is alternatively named.

Professor Spearman's method in that paper was to measure a number of mental abilities, some of them school subjects, others artificial tests, in a number of persons, and calculate the correlation coefficients of each of these activities with each of the others. These correlation coefficients, he then noticed, had a certain relationship among themselves, a rela-

[1] 'General Intelligence objectively determined and measured,' C. Spearman, *Amer. J. Psychol.*, 1904, 15, pp. 201–293.

tionship which may be called hierarchical order, and is explained in detail in the technical papers on the subject. He saw, quite rightly, that the presence of a general factor would produce this hierarchical order among the coefficients and, reversing this argument, he concluded that the presence of hierarchical order proved the existence of a general factor

A number of experimental researches on these lines, in some of which Professor Spearman himself took part, followed during the next eight years, but with very conflicting results, some experimenters finding the hierarchical order among the coefficients, others finding no such order. Two of the best articles of this period are those of Mr. Cyril Burt,[1] who found practically perfect hierarchical order, and Dr. William Brown,[2] who found no trace of such order. The experimental work in each case was psychologically of a high degree of excellence.

Things were in this very unsatisfactory state when an important article by Professor Spearman, in coöperation with Dr. Bernard Hart, appeared in 1912.[3] In this article the difficulty of making an unbiased judgment as to the presence or absence of hierarchical order was recognized, and a form of calculation was given for obtaining a numerical criterion of the degree of perfection of hierarchical order, which criterion would be independent of any bias on the part of the calculator. This criterion ranges theoretically from zero, for absence of hierarchical order, to unity, for perfection of hierarchical order. But their formula can, arithmetically, exceed unity.

The authors applied their criterion to all the experimental work available, work dating from various periods, and representing the researches of 14 experimenters on 1,463 men, women, boys and girls. From beginning to end the values of the criterion were positive and very high. The mean was

[1] Cyril Burt, 'Experimental Tests of General Intelligence,' *Brit. J. Psychol.*, 1909, 3, pp. 94–177.

[2] William Brown, 'Some Experimental Results in the Correlation of Mental Abilities,' *Brit. J. Psychol.*, 1910, 3, pp. 296–322.

[3] 'General Ability, its Existence and Nature,' by B. Hart and C. Spearman, *Brit. J. Psychol.*, 1912, 5, pp. 51–84.

almost complete unity. That is to say, Dr. Hart and Professor Spearman claimed that all the data then available showed perfect hierarchical order among the correlation coefficients, even the data of workers like Dr. Brown and Professor Thorndike, who had been unable to detect any such order.

The reasons why the hierarchical order among the correlation coefficients was not obvious at a glance were, according to these authors, two. In the first place, their theory did not entirely deny the presence of group factors of narrow range, and tests which were too similar were, according to them, to be pooled, before the hierarchical order would become apparent. Only in very few cases, however, did they find it necessary to pool tests in the data used. In the second place, the obscuring of the perfect hierarchical order was, according to them, due to the fact that only a small sample of subjects is examined. For this error allowance is made in the formula for calculating their criterion.

Dr. Hart and Professor Spearman therefore considered their 'Theory of Two Factors' proved. This theory considers ability in any activity to be due to two factors. One of these is a general factor, common to all performances. The other is a specific factor, unique to that particular performance, or at any rate extending only over a very narrow range including only other very similar performances. "It is not asserted," they say, "that the general factor prevails exclusively in the case of performances too alike, but only that when this likeness is diminished, or when the resembling performances are pooled together, a point is soon reached where the correlations are still of considerable magnitude, but now indicate no common factor except the general one."

In the same paper Dr. Hart and Professor Spearman consider, and in their opinion confute, two other theories, (*a*) the older view of Professor Thorndike, viz., a general independence of all correlations, and (*b*) Professor Thorndike's newer view of 'levels,' or the almost universal belief in 'types.' If the former were true, their criterion would, they consider, show an average value of about zero: if the latter, a low minus value.

Many experimental researches were inspired by this paper of Dr. Hart and Professor Spearman, of which, as a good example, may be cited one in 1913 by Mr. Stanley Wyatt.[1] It is I think not too much to say that in practically all of these the application of the Hart and Spearman criterion gave values closely approximating to unity and therefore supporting the Theory of General Ability. But complications began to arise, of which the first of importance will be found in Dr. Edward Webb's monograph on 'Character and Intelligence,' in 1915.[2] Dr. Webb considered that he had found (in addition to Professor Spearman's General Ability), a second general factor, which he calls 'persistence of motives.' Other writers began to find that their data required for their explanation large group factors, of wider range than those contemplated in the original form of Professor Spearman's theory.[3] Quite recently Mr. J. C. Maxwell Garnett, discussing the data of a number of workers with the aid of mathematical devices which he has introduced for the purpose, concludes that in addition to the single general factor of Professor Spearman, there are two large group factors which are practically general[4] (one of them being indeed almost identical with Dr. Webb's second general factor), which he calls respectively 'cleverness' and 'purpose,' both distinct from general ability.

It is clear therefore that in any case the simple original form of Professor Spearman's theory is becoming complicated by additions which tend to modify it very considerably. Meanwhile, however, the present writer had come to the conclusion that the mathematical foundations upon which it was based were in fact incorrect. Before developing the line of argument which led to this, it will be well to re-state Professor Spearman's case in its simplest terms in a few words.

[1] Stanley Wyatt, 'The Quantitative Investigation of Higher Mental Processes,' *Brit. J. Psychol.*, 1913, 6, pp. 109–133.

[2] E. Webb, 'Character and Intelligence,' *Brit. J. Psychol., Monog. Suppl.*, 1915, 3, pp. ix and 99.

[3] See especially E. Carey, 'Factors in the Mental Processes of School Children, *Brit. J. Psychol.*, 1916, 8, pp. 170–182.

[4] J. C. Maxwell Garnett, 'General Ability, Cleverness, and Purpose,' *Brit. J. Psychol.*, 1919, 9, pp. 345–366.

It is entirely based upon the observation and measurement of hierarchical order among correlation coefficients. It states that after allowance has been made for sampling errors this hierarchical order is found practically in perfection. And it finally states that such a high degree of perfection can only be produced by a general factor, and the absence of group factors, which would mar the perfection.

III. The Case against the Validity of Professor Spearman's Argument

It is possible, by means of dice throws or in other ways, to make artificial experiments on correlation, with the immense advantage that the machinery producing the correlation is known, and that therefore conclusions based upon the correlation coefficients can be confronted with the facts. Working on these lines, the present writer made, in 1914, a set of imitation 'mental tests' (really dice throws of a complicated kind) which were known to contain no general factor. The correlations were produced by a number of group factors which were of wide range, and, unlike Professor Spearman's specific or narrow group factors, they were not mutually exclusive.

These imitation mental tests, containing no general factor, gave however a set of correlation coefficients in excellent hierarchical order, and the criterion was when calculated found to be unity, so that had these correlation coefficients been published as the result of experimental work, they would have been claimed by Professor Spearman as proving the presence of a general factor.[1]

In a short reply Professor Spearman laid stress on the fact that this arrangement of group factors which thus produced practically perfect hierarchical order was not a random arrangement, and that it was exceedingly improbable that this one special arrangement should have occurred in each of the psychological researches of many experimenters, so improbable indeed as to be ruled entirely out of court.[2]

[1] Godfrey H. Thomson, 'A Hierarchy without a General Factor,' *Brit. J. Psychol.*, 1916, **8**, pp. 271–281.

[2] C. Spearman, 'Some Comments on Mr. Thomson's Paper,' *Brit. J. Psychol.*, 1916, **8**, p. 282.

It is clear that Professor Spearman did therefore definitely admit that at any rate one arrangement of group factors existed which would give hierarchical order of sufficient perfection to satisfy completely his criterion. He did more than this, however, for he claimed already to have published, without proof, in an earlier paper, what the effect of a really random overlapping of all the factors in his opinion is, namely that in this case his criterion will be of the same value as the average correlation between the tests.[1] Now the average correlation between the tests employed in the psychological researches under consideration is not as a rule low. Indeed in those tests which really play an important part in the calculation of the criterion it is usually very high. So that this criterion would, if Professor Spearman's admission be correct, apparently be high on the random overlap theory; that is to say sheer chance would produce considerable though not perfect hierarchical order. This already puts the proof of the Theory of General Ability into a very different position from that which it appeared to occupy immediately after the publication of the paper of Dr. Hart and Professor Spearman in 1912. For in that paper the alternative theories gave values of the criterion which were either zero or negative, and the fact that it actually came out to be almost unity seemed conclusive. But now the comparison is much less definite, for here is a theory which may give high positive values. The criterion must not merely be high and positive to prove the Theory of Two Factors, it must be absolutely unity. True, in Professor Spearman's calculations it does come to unity with most remarkable regularity. But if it can be shown that these calculations are in any way erroneous, then the fact that the comparison is with a theory which can give a high criterion, and not merely with theories which give zero or less, is of great importance.

One reply which Professor Spearman might make to this step of the argument is contained by implication in the footnote on page 109 of the already quoted 1914 article in the

[1] C. Spearman, 'The Theory of Two Factors,' Psychol. Rev., 1914, 21, p. 109. See also E. Webb, Character andIntelligence,' *Brit. J. Psychol. Monog. Suppl.*, 1915, No. 3, on page 57 and Appendix, page 82.

Psychological Review. He appears to think that on the random overlap theory the criterion and the average correlation, though equal, will both be zero or very small on the average. In other words, on this view random overlap will produce hierarchical order if it produces correlation at all, but usually both will be zero. To return to the reply of Professor Spearman to the 'Hierarchy without a General Factor,' the reply namely that this special arrangement would doubtless give such order, but was too improbable to be seriously considered, and that a random arrangement of Group Factors, though it might give some hierarchical order, would not give it in the perfection actually found: the obvious way to find out if this is so or not is to try it, with artificial 'mental tests' formed of dice throws. This the present writer did in November and December of 1918, after an unavoidable delay of some years. Sets of artificial variables (analogous to the scores in mental tests) were made, in each of which the arrangement of group factors was decided by the chance draws of cards from a pack.[1] It was found that in every case a very considerable degree of perfection of hierarchical order was produced, quite as high as that found in the correlation data of experimental psychology. A further test was made on that set of data, from among these artificial experiments, which appeared to yield the least perfect hierarchical order. The true values of the correlation coefficients being known, the true degree of perfection of hierarchical order could be correctly calculated, and was 0.59 (perfection being represented by unity). Dice throws were now made to obtain experimental values of the same correlations, and Professor Spearman's criterion applied. As it has done in the case of so many experimental researches in psychology, *it gave the value unity.*[2] This set of correlation coefficients, therefore, if it had been published as the result of experiments on mental

[1] Godfrey H. Thomson, 'On the Cause of Hierarchical Order among the Correlation Coefficients of a Number of Varieties taken in Pairs,' *Proceedings of the Royal Society of London*, 1919, A, **95**, pp. 400–408. See also, by the same author, 'The Hierarchy of Abilities,' and 'The Proof or Disproof of the Existence of General Ability,' in *Brit. J. Psychol.*, 1919, **9**, pp. 321–344.

[2] Godfrey H. Thomson, 'On the Degree of Perfection of Hierarchical Order among Correlation Coefficients,' *Biometrika*, 1919, Vol. 12, pp. 355–366.

tests, would have been claimed by Professor Spearman as additional proof of the existence of a general factor, although in fact there was no such general factor present, and the correlations were due to *randomly* selected group factors.

The conclusions which appear reasonable from this are (*a*) that hierarchical order, unless perhaps when it is absolutely perfect, is no proof of the existence of a general factor, and (*b*) that the Hart and Spearman criterion for hierarchical order is somehow incorrect, and exaggerates the degree of hierarchical order present.

The errors which cause this exaggeration are pointed out in the last cited article in *Biometrika*, and are mainly two. In the first place, Dr. Hart and Professor Spearman assumed certain quantities to be uncorrelated when they are really strongly correlated, though in a peculiar manner. This error causes the possible values of the criterion to be distributed, not from zero to unity, but from zero to infinity. In the second place, they employ a 'correctional standard' which rejects all the values greater than about $1\frac{1}{2}$. The possible range for the accepted values is in practice from about $\frac{1}{2}$ to $1\frac{1}{2}$, and their average is naturally about unity. In other words, the remarkable regularity with which this criterion gives the value unity is not a property of the investigated correlation coefficients at all, but is a property possessed by the criterion itself, due to errors and the action of the 'correctional standard.'

In the writer's opinion the work outlined in this section of the present paper finally proves the invalidity of Professor Spearman's mathematical argument in favor of the Theory of Two Factors. If this be so that theory returns to the status of a possible, but unproven, theory.

IV. Hierarchical Order the Natural Order among Correlation Coefficients

The fact is that hierarchical order, which Professor Spearman was the first to notice among correlation coefficients, is the natural relationship among these coefficients, on any theory whatever of the cause of the correlations, excepting only theories specially designed to prevent its occurrence. It is the *absence* of hierarchical order which would be a remarkable phenomenon requiring special explanation; its presence requires none beyond what is termed chance.

An analogy from the simple repeated measurements of a linear magnitude may help to illustrate this. Indeed it is rather more than an analogy, being in fact the same phenomenon in its simplest terms and dimensions. It is well known that many measurements of the same quantity, made with all scientific precautions, under apparently the same conditions, and with an avoidance of all known sources of error, nevertheless do not give a number of identical values. The values are all different, but are not without law and order in their arrangement. They are grouped about a center from which the density decreases in both directions, and it is found that this grouping is for most practical purposes closely represented by the Normal or Gaussian Curve of Error. Experimenters are not surprised to find their data obeying the Normal Law, nor do they require a special theory to explain it. On the contrary, it is the departures from the Normal Law which if wide would cause alarm and require special investigation, and if confirmed would require a special theory. In the same way hierarchical order among correlation coefficients should not cause surprise, though any marked variation from this order would demand investigation.

Correlation coefficients are themselves correlated, and n correlation coefficients form an n-fold or n-dimensional correlation-surface. The particular and convenient form of tabulation of correlation coefficients adopted by Professor Spearman and followed by most other psychological workers brings to light, in the form of "hierarchical order," one of the properties of this correlation-surface of the correlations.

It is true that in the ordinary form of the theory of correlation of correlations,[1] the variations in the correlation coefficients to which the correlation of these coefficients refers, are variations due to sampling the population; *i.e.*, to taking

[1] K. Pearson and L. N. G. Filon, 'On the Probable Errors of Frequency Constants and on the Influence of Random Selection on Variation and Correlation,' *Phil. Trans. of the Roy. Soc. London*, 1898, **191** A, pp. 299–311.

in our case a class of only perhaps 50 English grammar school boys of age 12, instead of all such boys: whereas the hierarchical order we desire to explain is already found in the true 'theoretical' correlation coefficients. This difference is however one of point of view only. It was left partially unexplained in the above cited article[1] although it was referred to. Further consideration leads to the following resolution of the difficulty.

Suppose that n variates (in our work the scores in mental tests) are so connected by factors that the correlations are all equal and positive. Then let a small sample of the population be taken. The *observed* correlations will show departures from equality, and will be found to be in hierarchical order. This hierarchical order is due to sampling the population.

Now consider why the correlations do not come out at their true values. They give of course the true values *for the sample.* The reason of their departing from the true values of the whole population is that (*a*) some of the factors which really are links between the variates (the mental activities) happen to have remained steadier than usual during the sample. In the limit a factor might happen to retain exactly the same value through the various individuals of the sample. That is, some of the linking factors do not in reality come into action, or not in their full force. (*b*) On the other hand, some factors which are really different and unconnected may happen by chance to rise and fall together, throughout the sample, and more or less to act as one. That is, fictitious linking factors are created, which would disappear with a larger sample.

Clearly therefore a hierarchy of correlation coefficients, caused by sampling the population, is due to chance having caused a change in the apparent factors acting. It follows that if we make a real change in the factors acting, we shall get a hierarchy, and this is what we do when we choose the mental tests to be employed in any research. Each mental test is a test of a sample of abilities.

The laws governing the correlation of correlation coeffi-

[1] Godfrey H. Thomson, *Proc. Roy. Soc. London*, 1919, A, **95**, pp. 407 and 408.

cients which vary because of sampling the population can, in fact, be applied without hesitation to the relationships between 'true' correlations in the whole of any population simply because any such population is itself a sample. English grammar school boys of 12 are themselves a sample of a larger boyhood; the whole human race indeed is a sample of 'what might have been,' selected by the struggle for survival.

The whole question clearly has philosophical bearings on the degree of reality of causal connections; for on this view those chance links in a small sample which were a few paragraphs ago termed 'fictitious links, which would disappear with a larger sample,' do not differ except in degree from the 'real' causal links which we only term real because they persist throughout the largest sample with which we are acquainted.

In another direction there are connections with the difference, which is one of degree only, between what is called 'partial' correlation and 'entire' correlation.[1]

The conclusion to be drawn from this section of the present paper is that hierarchical order is the natural order to expect among correlation coefficients, on a theory of chance sampling alone, and that therefore, by the principle of Occam's razor, its presence cannot be made the criterion of the existence of any special form of causal connection, such as is assumed in the Theory of Two Factors.

V. A Sampling Theory of Ability

In place therefore of the two factors of that theory, one general and the other specific, the present writer prefers to think of a number of factors at play in the carrying out of any activity such as a mental test, these factors being a sample of all those which the individual has at his command.

The first reason for preferring this theory is that of Occam's razor. It makes fewer assumptions than does the

[1] See Karl Pearson, 'On the Influence of Natural Selection on the Variability and Correlation of Organs,' *Phil. Trans. Roy. Soc. London*, 1902, A, **200**, pp. 1–66.

Godfrey H. Thomson, 'The Proof or Disproof of the Existence of General Ability,' *Brit. J. Psychol.*, 1919, **9**, pp. 321–336.

more special form of theory. It does not deny general ability, for if the samples are large there will of course be factors common to all activities. On the other hand it does not assert general ability, for the samples may not be so large as this, and no single factor may occur in every activity. If, moreover, a number of factors do run through the whole gamut of activities, forming a general factor, this group need not be the same in every individual. In other words general ability, if possessed by any individual, need not be psychologically of the same nature as any general ability possessed by another individual. Everyone has probably known men who were good all round, but Jones may be a good all round man for different reasons from those which make Smith good all round.

The Sampling Theory, then, neither denies nor asserts general ability, though it says it is unproven. Nor does it deny specific factors. On the other hand it does deny the absence of group factors. It is this absence of group factors which is in truth the crux of Professor Spearman's theory, which is not so much a theory of general ability, or a theory of two factors, as a Theory of the Absence of Group Factors. And inasmuch as its own disciples have begun to require group factors to explain their data, its distinguishing mark would appear in any case to be disappearing.

Such group factors as are admitted by Professor Spearman are of very narrow range, and are mutually exclusive, that is they do not overlap. Both these points follow from the sentence used in the 1912 article with Dr. Hart, where it is said that, in the case of performances too alike, 'when this likeness is diminished, or when the resembling performances are pooled together, a point is soon reached where the correlations are still of considerable magnitude, but now indicate no common factor except the general one.'

Since this point is soon reached, the group factors must be narrow in range. Since pooling a few performances will obliterate any group factors, they must be exclusive of one another. For if *A*, *B*, *C* and *D* are four tests, in which *A* and *B* have a group factor common to them, and *C* and *D* another, then of course by pooling *A* with *B* and also *C* with *D* we can obtain two pools *AB* and *CD* which have no link. But if *A*, *B* and *C* have one group factor, and *C* and *D* have another then these group factors cannot be separated into specific factors. In fact, a specific factor is a separated group factor, and Professor Spearman's theory asserts that group factors, if any, are separable and mutually exclusive. This is to the present writer the great stumbling block in the way of the acceptance of the Theory of Two Factors, unless 'specific factor' is interpreted in the way suggested later in this article.

It is a fact which will be admitted by most that the same activity is not performed in the same way by different individuals, even though they are equally expert. Not only are specific factors therefore required by this theory for every separate activity, excluding only any which are very closely similar; but also specific factors of different psychological natures are required for each individual. Further, the same individual does not always perform the same activity in the same way. A man using an ergograph will, as he tires, begin to employ muscles other than those naturally used at the outset. When we are returning from a cycle ride muscles are used in a different manner from the style adopted at the start, indeed sometimes deliberate changes are made to give relief. And in the same way a mental task is performed by different methods at different times. Does this then mean a different specific factor for each way of doing a task? All these difficulties appear to argue against the Theory of Two Factors, and seem to the present writer to be considerably cleared up by the Sampling Theory.

Finally, the Sampling Theory appears to be in accordance with a line of thought which has already proved fruitful in other sciences. Any individual is, on the Mendelian theory, a sample of unit qualities derived from his parents, and of these a further sample is apparent and explicit in the individual, the balance being dormant but capable of contributing to the sample which is to form his child. It seems a natural step further to look upon any activity carried out by this individual as involving yet a further sample of these qualities.

VI. The Difficulty of 'Transfer of Training'

Although Professor Spearman's Theory of Two Factors has been chiefly based by him on the line of argument which, it is suggested, has now been proved invalid, viz., the 'hierarchy' argument, yet there is another and powerful form of reasoning which can be brought up to its support, based upon the fact that, according to some experimenters, improvement in any activity due to training does not transfer in any appreciable amount to any other activity, except to those very similar indeed to the trained activity. And even those workers who do not agree that this is an experimental fact are usually content to take a defensive attitude and say that transfer is not disproved. Few if any will say that it is proved.

This certainly seems to point to the absence of group factors, and to support Professor Spearman's theory, which only needs to add to itself the assumption that the specific factors are, while the general factor is not, capable of being improved by training, to fit the case admirably. Of course, if transfer really occurs, the argument proves the opposite. And although psychological experiment points on the whole to the absence or the narrowness of transfer, yet popular opinion among business men, schoolmasters, and others is in favor of transfer to a considerable extent. Assuming no transfer, however, how can the Sampling Theory, with its numerous group factors, explain this?

It is necessary to assume that the group factors are all unimprovable or only slightly improvable by training, though they may change with the growth and development of the individual. The improvement which certainly takes place when we practice any activity is due, it may then be assumed, not to improvement in the elemental abilities which form the sample, *but to a weeding out*, and selection of these. The sample alters, mainly no doubt is diminished, though additions are also conceivable. It becomes a more economical sample, and waste of effort in using elements which are unnecessary is avoided. Improvement in any mental activity may on this view be compared with improvement in a manual dexterity, in which it is notorious that the improvement consists largely in the avoidance of unnecessary movements.

When another activity is then attempted, the elemental factors are just the same as they would have been had the practice in the first activity not taken place. The new activity will be performed by a new group of factors, which sample will as in the first case be in the beginning wasteful and will include many unnecessary elements. Transfer of improvement gained in the first activity will therefore not take place except insofar as the second activity is recognized as a mere variant of the original one, in which case the weeding out process which has taken place in the first case may be done at the very first attempt, at any rate to some extent.

To use another analogy, the improvement which takes place when a football team practices playing together for a series of matches is due more to team work than to individual improvement. A new team, even though it contain a large proportion of players from the first team, will not have this unity of action. There will be little transfer of improvement.

According to the view here developed, it is the weeding out of the sample of elemental abilities which is specific. The team work is specific, though the players play for several clubs. This would appear to enable a reconciliation to be affected between the almost universal belief in 'types' of ability (to which Professor Spearman refers) and the experimental facts concerning both correlation and transfer. If there be a general factor at all, it might be the power to shake down rapidly into good team work, in a word, educability. But there seems no objection to assuming that this, instead of being a general factor, is a property of each elemental factor, varying from factor to factor.

To sum up this section: if transfer of training really does not occur to any great extent, then it has to be admitted that the Theory of Two Factors readily explains this. But the Sampling Theory can also do so, in a manner which is perhaps not so easy to set forth, but which nevertheless appears to the present writer to be more illuminating and less artificial than the alternative theory.

VII. The 'Faculty Fallacy'

Since the group factors spoken of in this Sampling Theory are, in the fact that they are supposed to come into play in many different activities, similar to the banished 'faculties' of the mind (though the writer conceives of them as being smaller units than were those faculties) it is probably necessary to defend the theory against the charge of committing what is known as the 'Faculty Fallacy.' This defence is easy. It is only necessary to point out (*a*) that a person who believes in 'faculties' or 'types' or 'levels' does not necessarily commit the above-mentioned fallacy, and (*b*) that any charge of being 'faculties' which may be brought against the group factors can of course also be brought against the general factor.

The clearest account of the faculty fallacy known to the writer is given in the older edition of Professor G. F. Stout's 'Manual': "An effect cannot be its own cause, and cannot, therefore, afford its own explanation. But it is a fallacy of not infrequent occurrence to assign as a cause what turns out on examination to be only the effect itself, expressed in different language. . . . The classical instance of this confusion is the answer of Molière's physician to the question: 'Why does opium induce sleep?' 'Opium,' he answers, 'produces sleep because it has a soporific tendency.'"

Now it is to be clearly noted that there is no logical objection to the physician saying either that opium produces sleep, or that it has a soporific tendency. All that he must avoid doing is to give the one as the cause of the other. And in a similar way there is no logical objection to anyone believing, on the ground of experiment, that if a man has a good memory for historical matters he will, as a fact, have a good memory for all other matters. But if he believes this without any other ground than that the name memory is given to these diverse activities, then indeed he is committing the fallacy in question. Even if a man uses the form of words: "Robinson will be a good man for this post, because he has a good memory," he is not necessarily committing any logical fallacy. He may very well mean by this short statement something like the following: "I have noticed that a man who remembers one class of facts well is also frequently good at remembering other classes of facts. I know that Robinson can remember such and such things easily and accurately, therefore I think it very probable that he will be above the average in this job, which requires the memorizing of certain facts." And in this there is no fallacy, whether the conclusion be true or false.

The existence of the group factors spoken of in this paper is deduced with more or less probability from the known experiments. Their existence is an hypothesis which explains these facts, though it is not the only hypothesis to do so. If, as is very probable, the language used in any part of this paper is open to an interpretation which would involve the fallacy, then it can only be said that this is not the interpretation which is intended.

VIII. Conclusions

Professor Spearman's Theory of Two Factors, which assumes that ability in any performance is due to (*a*) a general factor and (*b*) a specific factor (group factors being absent, or at any rate very narrow in range and mutually exclusive) is based chiefly on the observed fact that correlation coefficients in psychological tests tend to fall into 'hierarchical order.' It has been shown, however, that the criterion adopted for evaluating the degree of perfection of hierarchical order present is untrustworthy and has led to overestimation. Such hierarchical order as is actually present is in fact the natural thing to expect, and it is the absence of such which should occasion surprise. The proof of the Theory of Two Factors which is based on the presence of hierarchical order therefore falls to the ground. The theory remains a possible explanation of the facts but ceases to be the unique explanation. As an alternative theory there is here advanced a Sampling Theory of Ability, in which any performance is considered as being carried out by a sample of group factors. This theory is preferred because it makes fewer and less special assumptions, because it is more elastic and wider, and because it is in closer accord with theories in use in biology and in the study of heredity.

SUGGESTIONS TOWARD A SCIENTIFIC INTERPRETATION OF PERCEPTION

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Much of the criticism directed toward the results of psychological investigation might serve as a direct challenge to psychologists to clarify their interpretations of psychological phenomena; for a study of those criticisms amply reveals the bizarre views attributed to psychologists. Although this peculiar situation obtains with respect to all psychological descriptions, it is especially striking in the case of perception. Thus, a recent writer[1] finds it necessary to point out that an object is not merely a thing which 'starts a chain of vibrations which eventually results in its own creation.' To the present writer this specific criticism does not really call for a defense of the psychologist's position, since the critic holds substantially the same view as most psychologists, but the very fact that a writer will find much to criticize in any one who supports a similar doctrine is a symptom of a confusing situation which demands at least a restatement of perception.

Naturally enough the confusions mentioned reach deeper than the mere matter of exposition and in fact arise directly from the types of conceptions held concerning the process under discussion. A careful reading of psychological literature on perception creates the suspicion that the descriptions fail to tally with the actual facts in the case. As a striking example we find that perception is described as in some sense a creative process which functions in the organization of the discrete qualities constituting the objects of our reaction. In effect, we find practically all current perceptual doctrines very strongly reminiscent of Berkeley's subjectivism albeit modified somewhat à la Reid; the latter modification results in the view that there exists a percept as well as an object of percep-

[1] J. B. Pratt, *J. of Phil., Psychol., etc.*, 1919, 16, 596 ff.

tion. Psychologists cannot but consider the problem of perception as crucial, since the admission of a non-scientific subjectivism at this point will bring disastrous consequences into the entire science of psychology. In this article the writer attempts to suggest a description of perception, which, so far as it goes, consistently complies with the rigorous canons of natural science.

I.

General Description of Perception.—Perception is the conscious behavior through which are developed the meanings of objects and relations which operate in the adaptation of the individual to his surroundings and in the control of them. It is precisely in the process of perception that the individual, in direct contact with objects, develops reaction patterns enabling him to differentiate and distinguish the various objects affecting him.

At the outset it must be noted that the act of perception[1] is an adjustmental reaction, an actual interaction of one natural object with another. But the precise difference between this kind of interaction and some other is, namely, that one of the interacting objects is a psychophysiological organism to whom the results of the present interaction will become significant in influencing future contacts of this object (person) with the same or a similar object. Consider, that what was formerly a mere interconnection between objects becomes what we might now call a knowledge process because the reaction becomes a means to some other form of reaction; that is to say, the first natural contact with an object is the basis for the development of an anticipatory reaction system. If the person is once burned, the object which produces this effect will upon a future occasion stimulate a touch inhibition reaction rather than a touch response. An empirical fact it is, there-

[1] While the writer is in complete sympathy with Watson in his revolt against subjectivism, and in his assertion that functional psychology is just as guilty in this respect as the structural view, he cannot assent to Watson's implication that perception among other processes is not properly the subject matter of a non-subjectivistic psychology. Nor indeed does Watson omit perception when he is interested in 'integrations and total activities of the individual.' His rejection of the terms is obviously to allow room for a predominantly physiological tone to his discussion.

fore, that all developed[1] perceptual responses operate as knowledge reactions, for in this way only do we learn to discriminate between objects, and to anticipate the specific response we should make to a particular object. But it is of extreme importance to notice that the perceptual reaction is not in its primary occurrence a knowing. To overlook this fact is to fall into the error of finally resolving the objects of our reactions into knowledges of some sort, and the history of psychology stands to witness that on the basis of such premises we invariably land in a mentalistic world in which objects are reduced to sensations, and the world of fact and science disappears in our description.

Only upon the assumption that the perceptual reaction is a natural psychophysiological response, the writer submits, can we achieve a natural science interpretation of the development of discriminative meanings. By thus investigating all the components of an act we may hope to obtain a scientific description of the total response and escape the arbitrary and confusing concept of a mental content, which is an unavoidable consequence of the presupposition that perception is a knowledge process.[2]

We must, then, look upon the perceptual reaction as a complex adjustment from which is derived the significance of objects through the integration of reaction patterns. This meaning of objects we shall see may be resident in the response pattern, or it may be more remotely connected with it, even to the point at which the act is no longer a perceptual but a conceptual reaction; in the latter case we observe that the meaning is detached from any overt act, and as a matter of fact we find that such detached meanings constitute the implicit functioning of the original reaction which ultimately generated the conceptual meaning.

Primary Perception and Simple Apprehension.—Upon the basis of the specific operation of meanings we may distinguish two definite forms or degrees of perceptual response which we

[1] Note the distinction drawn between perception in development and perception in use on another page of this paper.

[2] Expressed in the statement that perception is the consciousness (awareness) of an object present to sense.

will call primary perception and simple apprehension. In the former case, the meaning of the object responded to resides within the reactional movement of the person,[1] as illustrated by the perceptual process of an instinctive act. The meaning of a 'danger' object for the person is merely the startled jump which constitutes the operation of a connate reaction pattern. It must be observed that in this situation the neuro-muscular and neuro-glandular factors in the response are very prominent, and as a record of fact, the cognitive component merely consists of a simple appreciation of the presence of the stimulating object.[2]

In simple apprehension the meaning becomes more and more detached from the immediate condition of response. Instead of the mere presence of an object calling out a specific reaction, the object may serve as a symbol for some action. In consequence, the discriminated significance of the object will be attached not to the direct movement as in primary perception, but to another response which is to follow. Evident it is that a meaning of this type is an implicit response in the form of an anticipatory process similar to that we invariably find as an important factor in all delayed responses, whether simple acts or chains of acts. This capacity to detach meanings gives the person a greater control over the objects of his environment, for, if the meaning of the object is appreciated before an overt response is made, the type of response can be widely varied between limits.[3] In contrast to primary perception the meaning in simple apprehension is always correlated with an awareness-attention process.

Implicit perception functions in adaptational situations in which there are more definite appreciations of the surrounding objects. We might take the case of meeting a friend in which there is a complete and definite meaning element. Consequently the overt action which takes place is more conditioned

[1] We might just as well say the meaning is in the object, but it is clear that unless there is an action involved the problem even of the location of meaning does not arise.

[2] The components spoken of are, of course, abstracted from the actual response by logical analysis.

[3] *Cf.* my brief suggestion concerning the detachment of meanings, PSYCHOL. REV., 1919, 26, 2 ff.

by the meaning component. If he is an American friend, I may merely shake hands with him, but if he is a foreigner, I will probably also raise my hat and bow. Clearly the entire course of my behavior in this situation presupposes my familiarity with the person. It must not be overlooked that we do not exclude from our description of simple apprehension the simpler immediate reactions which occur in primary perception. For the fact is, that since simple apprehension is always the development of an act of primary perception, it involves therefore an integration of the simpler acts. Of prime importance here is the fact that it is precisely through the integration of the simpler acts that a person profits by past experience. For instance, my reaction to this person is conditioned by the numerous integrations of responses representing my previous contacts with him.

Thus through the constant growth of the reaction pattern does the perceptual process undergo a continuous development. Not only does a given response serve at any specific time as an adaptational function, but also as a developing potentiality for some future contact between the person and the object.

Analytic Description of Perception.—Although the perceptual response is a thoroughly organic process, we can nevertheless analyze it into a series of specific stages or act components which we can tabulate as follows:

1. The attention function in correlation with contact media (light rays, for example).
2. Functioning of a reaction pattern which involves
   (*a*) Discrimination and appreciation of specific qualities and relations of objects coupled with conative and affective factors.
   (*b*) Neuro-musculo-glandular processes.
3. Emergence of meaning (new).
4. Overt adaptation follows.

1. The attention factor is the selective process which serves to prepare the individual for a new reaction. At any moment of time innumerable possibilities for action naturally exist because of the previous acquisition of many reaction systems. The change in the surrounding medium or media of the person,

which occurs when the individual comes into the presence of new objects, or when objects change their positions with respect to the person, puts him into a condition of readiness to react to some new object. It must be observed that the attention processes depend not only upon the stimulating object and its setting, but also upon the condition of the organism at the time, that is to say, the selection process depends very directly upon what the activities of the person were prior to the present contact. Such activities condition also what precise phase of an object we will react to at a given time. Thus, for example, the problem as to why at one time our attention is attracted to a red solid instead of a smooth surface, when both form the phases of a book, is solved by an investigation of the previous activities of the person.

2. Following the selection function, the reaction pattern is brought into activity, and we find thus a highly coördinated series of processes taking place. These may be enumerated separately, although they constitute merely descriptive phases of a unitary process. Here we find the discriminative process which enables the organism to distinguish the various qualities and relations of things. This phase may be thought of as the cognitive aspect of the reaction system, and to a degree we may look upon this phase as conditioning the mode of operation of the entire complex. The conative factor in this complex, being very closely connected with the attention function, may be considered as the aspect which conditions the occurrence of a response at all. Of primary importance are the affective processes, which in part predispose the organism to act. Every reaction pattern involves of course also the elaborate functioning of musculo-neural and neuro-glandular processes, which are so prominent as to convince some observers that they constitute the total reaction pattern.

3. As a result of the operation of the reaction pattern a new effect is or may be produced upon the organism. Should the object or person reacted to, with all the involved relations, remain constant, no new reaction is called out; that is, the previously developed reaction pattern remains unmodified despite the present contact. The object, then, will not take

on any new meaning and the overt act following the appreciation of the identity of the object may be precisely like one that has previously occurred. We can readily determine this to be the case of perception in use. On the other hand, should the previously developed reaction system prove inadequate for the purpose of the present contact, new features may develop. Instead of involving some given system of receptors in connection with certain neural and muscular processes, additional factors may be put into operation. Thus, for example, should the apple previously sound and firm to touch now offer no resistance, it will call out different muscular responses. Similarly, should it now present color surfaces varying in hue, turning from red to brown, the object will take on new meaning, and we will react in a different way to the now deterioriated apple. Thus, indefinitely many modifications are developed in the course of the exercise of so intricate a psychophysiological response pattern.

4. Following upon the operation of the definitively perceptual reaction system, the person performs some sort of overt act. The latter is directly conditioned by the emergence of the meaning brought out through the course of the specified contact with the object. It must be observed that the specific perceptual process is a coördinate process with some other type of reaction system. Thus, we should look upon the perceptual function as a part of a perceptual-instinct, perceptual-emotional, or perceptual-voluntary action, etc. To look upon it in this way obviates the dangerous view that in the actions mentioned we have isolated activities. As a preliminary or partial action the perceptual process represents an evaluation of the object which leads to a definite overt response. It is at this point that the perceptual reaction becomes a knowledge function, since it stands for some actual adjustmental act. Whether the apple of our illustration will be eaten, or thrown away, depends upon the information elicited through the operation of the perceptual reaction system and its modifications. At this point, we must not overlook the fact that the appreciation of an edible or non-edible meaning depends upon the surrounding conditions of the object. Even

if the knowledge elicited from the object itself is favorable to its consumption, that event will not occur unless conditions are otherwise favorable. We mean to point out here that the specific kind of response patterns that will act as a series in any given situation will depend upon that situation. This fact indicates the close interaction between stimuli and responses.

An important reservation to the above description of the perceptual activities must be made in the light of our distinction between primary perception and simple apprehension. It is only in the case of simple apprehension that the distinct series of factors are found; for it is only there that a definite meaning factor is isolated in the total act. In primary perception the overt act is identical with the original system, and the perceptual process itself constitutes not exclusively a definite knowledge factor in an adjustment, but it is the whole adjustment itself.

Perception in Development and Use.—Of primary importance for the understanding of the perceptual reactions is the distinction between perception in development, and in use. In the former type of reaction with objects meanings are developed; that is to say, a definite form of reaction pattern is acquired; so that the future contact with this object will be of a definite and peculiar sort, because the reaction pattern developed will then be put into use. The distinction made indicates the extremely complex and constantly varying character of the perceptual reactions and points to the mechanism of elaboration of such functions.

Since clearly the original perceptual contacts with objects occur in the instinct stage of development, we may date the origin of a meaning or reaction pattern from the first instinct contact of an organism with any given object. The point is, that the hypothetical, original contact of an organism with an object is the result of a direct arousal of a connate reaction pattern through the instrumentality of various physical media such as light rays or air waves. If we dare speak of a meaning possessed by an object at this stage, it is merely that of 'response eliciter.' This contact is as mechanical as a conscious behavior act can be, and here we find the full significance of the statement that we have innate tendencies to discriminate colors and other physical qualities. The fact is that our connate reaction patterns are brought into function by the stimulation of the specific receptor systems whose activities form a part of them. At this stage the simple psychophysiological response as a whole, symbolizes the meaning of the object. Now, when the action just mentioned occurs, some effect will be produced upon the organism; so that the next contact with this object will involve a modified reaction system, or we might say, the object has taken on a new meaning. The perceptual processes thus represent a constant integration of a reaction pattern depending upon the number of contacts with the same physical object under varying conditions of surrounding auspices. In general, it is clear that the perceptual reactions are entirely genetic in their functioning, hence only by studying them in their development can we hope to understand them.

Another form of integration in the development of perceptual reactions is the establishment of a definite interactional relationship between the stimulating object and the reaction system. Not only must there be a coördination of specific factors of a response system, such as for example, visuo-muscular, visuo-glandular and neuro-muscular processes, but there must also be a connection between this total reaction pattern as a functional representative of the organism at the time and the stimulating object.[1] Just how this intimate relationship between stimuli and response systems, which is the essential factor in perception in use, is established, can be experimentally studied through various types of conditioned reflexes. Excellently is perception in use illustrated by the story of the discharged veteran, quoted by Spencer, who had had the auditory object 'attention' so integrated with a particular response system as to lose his pie when a practical joker uttered the command.[2] When the integration has been accomplished, the reaction pattern can be stimulated by one or more of a large series of phases of the object, which become

[1] This connection between the response pattern and its simulating object constitutes the primary and fundamental type of psychological association.

[2] 'Psychology,' I, p. 499.

differentiated because of the different media through which the contacts between the organism and the objects are made. Thus, a reaction pattern involving a ball-meaning may be put into action by either a visual, auditory or tactual stimulus. As an illustration of the arousal of a complex system of perceptual responses through the mediation of a simple type of stimulus, we can take the case of the visual contact with ice, which arouses coldness, smoothness and hardness meanings at the same time. The effective adaptation of the organism depends to a considerable degree upon the complexity of the two sorts of integration described.

Because perception in use as just described involves putting a complex reaction pattern into operation by some phase of an object, we find in such adjustments the beginnings of a differentiation between the explicit and the implicit functioning of a reaction system; the latter case gives us the detached meaning. The implicit functioning of a reaction pattern is clearly discerned in the many cases in which the visual contact is the only direct one; and the meaning of the object, which may be very elaborate, though not attached directly to an immediate response, is most certainly acting. A striking example of the implicit functioning of reaction patterns is the situation in which a banker, while otherwise preoccupied, for a moment will begin to respond as though at a director's meeting, when stimulated by the crumpling of a crisp paper. Again, the 'wave of feeling' brought on by the perusal of a literary description indicates the living over of some crucial situation by the incipient operation of reaction systems. It is this implicit functioning of reaction patterns in perception which shows the way toward the development of the conceptual and memorial processes.[1]

From our description of perception in development and in use it must appear that these are not two distinct operations, but rather two mutually interrelated processes. Since the perceptual activities are constantly developing we have in practically every new operation of a perceptual reaction system

[1] As a matter of fact, so far as psychophysiological mechanism goes, there is only a difference in degree between perception and thought, but from the standpoint of results effected through these reactions the variation is or may be enormous.

a more complex integration of the component action elements with the stimulating situation. If we consider the perceptual reaction as the use of meanings stimulated by direct contact with objects, we find that the distinction between the development of perception and its use, depends upon the amount of direct stimulation which is required to elicit the response. Perception in development requires a relatively larger series of direct contacts to effect an equally complex response than is true in the case of perception in use, since in the latter case the meaning attaches to an incipient reaction pattern. We repeat, the development of perception is a process of so integrating acts that only a minimum of receptors may be necessary to effect the appropriate response. If we remember that this development never ceases, provided that we have occasion to react to the given stimulating object, then it is clear that perception in use is merely the condition of responding on the basis of a previously acquired reaction system, pending its modification by the present contact with the object in question.

The Specific Mechanisms of Perception.—A more penetrating analysis of perception than we have yet made will yield information as to the specific integrations which operate in the perceptual reactions. To a certain point we can trace the precise organization of the component processes, such as the muscular, cognitive, glandular, neural, etc. Our ability to do this is made possible by the fact that underlying all these modifications is a simple psychological law which may be formulated as follows. *Every integrative modification of a reaction pattern is a direct function of a differential contact with actual things.*

By far the most important problem of perception arises just here, namely, what are the specific means of contact between the organism and objects? The interest in this problem emerges because of the inevitable incorrect inference from the customary psychological premises, namely, that the cognitive qualities are existential processes somehow aroused in 'consciousness' which bring about the movements of the organism. Now as a matter of fact, it is easily seen that in any description of perception the qualities mentioned (odors, colors, etc.) are abstracted from the objects. The discrimination of these qualities as it occurs in the actual response is in part the perceptual act; that is to say, the discriminative process constitutes part of the perceptual act as distinguished from the overt action which follows it. The discriminative factors are thus seen to be phases of concrete psychophysiological processes, and this means in effect the total extrusion from the perceptual act of any substantial mental or subjectivistic quality.

Responsible for the view of the existence and primary functioning of sensation qualities, is the psychological tradition which makes knowledge the differentia between biological and psychological acts. Taking conscious behavior as our starting point, we may catch a glimpse of the true significance of the perceptual reaction as a knowledge process which brings about adequate, psychological adaptations, and still keep our descriptive analysis of the facts within the range of observational interpretation. The favorable prognosis for the scientific development of psychology depends in large measure upon the rejection of a theory implying that the adjustmental responses of the individual are due to a mystic potency resident in 'consciousness.' In place of such a theory should be substituted a verifiable interactional mechanics of natural things. Upon the basis of such an interactional mechanics it is possible to avoid the assumption that perceptual responses are primarily cognitive operations or that they are 'consciousness,' that is to say, awareness of something, rather than adjustment acts.

Thus, the problem of the contact of the individual with objects is reduced to the description of the precise manner in which a reaction pattern or system is put into operation by the stimulating object. Here we have to assume that the reaction is that of a conscious organism, which has the capacity to react to colors and other qualities. As a matter of fact, the notions we have of such qualities are historically developed through the discriminating evaluations of such conscious beings. Now, although it may be impossible to develop a detailed analysis of all that takes place in a perceptual reaction, we can isolate series of systems which play their part in such reactions. These systems are logically ordered sequences of events which occur when a perceptual reaction is made. An example of one of these systems is the cycle beginning with the reflection of light rays of definite sorts which set up differential processes in the retina, followed by definite happenings traced out in the neural pathways and in the cortical areas of the brain. The completion of the cycle involves the consideration of changes taking place in the association tracts and the motor localities of the cortex, the happenings in the efferent transmission system and in the effectors located in muscles and glands.[1]

Of extremely great consequence is the series of appreciative and feeling processes which are factors in the operation of the total reaction system under discussion. The important point here is that the perceptual reaction must be looked upon as one of the ways in which a psychophysiological machine is operating. Above all, what we wish to avoid is the conception that the physico-neural functions constituting part of the perceptual act, are the causes or the parallels of conscious action. A very simple means to avoid this confusion is to remember that we are dealing here with two phases of a natural happening which for scientific purposes are differently classified, but never separated, and also that no process is any more tangible than another. Physical processes are not tangible physical substances, nor are the physiological factors biological material; neither are both of these functions absolutely distinct from the mental processes which naturally do not reduce themselves to mentality, a substance the existence of which we all join in denying. What we must describe here is a psychophysiological reaction, for it is only such a reaction which can be the object of our observations. While observing

[1] The reader who is interested in a more detailed discussion of the mechanisms of conscious behavior is referred to Watson's recent volume, 'Psychology from the Standpoint of a Behaviorist,' which contains the best description in psychological literature of the behavioristic components of a reaction system. Because of the author's resolute attempt to suppress the mentalistic components of the reaction pattern, the book contains merely suggestions, though frequently very important ones (especially in the chapter on Emotions), concerning those phases of a conscious response.

a psychophysiological organism we can discriminate between acts involving a response pattern of predominantly mental factors and others having the physiological factors more prominent. It is the former type of psychophysiological act which is usually called subjective, and which is in part responsible for the inexcusable separation of the mentalistic and behavioristic phases of a unitary act.

Since we can analyze many of the isolated factors of a perceptual reaction we can describe specific correlations between the qualities of objects and the particular phases of the reaction pattern. Thus colors, sounds, tastes, hardnesses, etc., can be coördinated with specific receptor systems, because during the evolutionary development of the organism the receptor systems became differentiated in sensitivity to particular kinds of stimuli, which objects initiated. For example, the retinæ are normally sensitive only to light rays reflected by the colored surfaces of objects, and the cochlea to air vibrations, which emanate from sounding bodies. In passing, we might point out that our analysis has provided no basis for the assumption that 'objects as perceived' are synthesized in some form out of qualities produced in the mind or in the organism by stimuli set up by objects. After many detours this view just mentioned has seeped into current psychology from the Berkeleyan head waters, and for a long time has been effective in preventing the conception of psychological phenomena in a scientific way. In contrast to the Berkeleyan view, we must look upon the stimuli which constitute the middle link between objects and organisms as natural predisposing conditions, mediating changes in the activities of the latter, much after the fashion in which an electric current produces changes in a machine. The undesirable consequences of thinking that in perception there is a synthesis of objects is well illustrated by the conception that space and time are somehow compounded by some additional attribute of the mental 'contents' called sensations.

The Relational Character of Perception.—Observations upon the perceptual interaction with things convince us that not only are all perceptual reactions not merely responses to specific qualities, but also that they are not confined to isolated objects; they are more than either of these descriptions indicate, namely, responses to a complete object in all its setting. We might generalize this fact by saying that we always perceive situations, not isolated things, and of course our conduct is conditioned accordingly. Thus a chair which ordinarily would be responded to by being sat in, will not call out such a response when it is occupied by some object or when there are individuals present before whom it is impolite to make such a response. In every such case the meaning of the object will depend upon the contextually related objects. When the chair itself is reacted to, we respond to a unified object, and not to simple elements (back, seat, legs, etc.); that is to say, we react to an object to-be-sat-in, and not to isolated fragments which require to be somehow connected. This relational character of perception is excellently illustrated by our responses in which words and not letters are the stimulating objects, and in which the words are directly and inseparably attached to other words.[1]

That we can immediately appreciate a complex situation apparently comprising many diverse elements is owing to this relational character of perception. Thus, in looking at a landscape the objects all seem to be in their proper places; distances are correctly located and the lights and shadows properly distributed. The total situation is the customary object of our reactions and is thus the stimulus to a unified primary response or simple apprehension. The meaning of the total situation can be readily and completely confounded by placing ourselves in a position incapacitating us for response, such as looking at the landscape with our head upside down. Much the same effect is produced by looking at an inverted painting. In such situations what happens is that the series of integrated reaction systems are thrown out of their customary harmonic organization and must be reorganized before the object can be correctly perceived. Experiments on space perception have shown that

[1] James points out with his characteristic description the unnatural aspect which a word takes when looked at in protracted isolation. "It stares at him from the paper like a glass eye with no speculation in it. Its body is indeed there, but its soul is fled." 'Princ. of Psychol.,' Vol. II., p. 81.

by practice disorganized response systems can readily be reintegrated.[1]

The difference in the responses to objects when they appear in different contextual relations illustrates the extremely subtle interaction between the stimulating object and the reacting person, and also shows the operation of perception as an adjustmental reaction to surrounding objects. Pliableness of the individual in this sense constitutes an important factor of general intelligence and exemplifies the law of integrated modifications of reaction systems mentioned above.

The Interpretative Function of Perception.—Since every psychological phenomenon is a product of two factors, namely, the stimulus and the response, our discussion of the influence of the stimulating circumstances upon our perceptual reactions naturally leads to the consideration of the influence of the individual's stock of reaction patterns upon any given present reaction. An observable fact it is, that the reaction systems which the individual has developed in his constant contact with objects, play a large part in any present reaction; for in a genuine way such reaction patterns constitute the individual at the moment. And since as we have indicated, these response patterns have been developed in the individual's previous experience, every perceptual reaction may be thought of as an interpretative function. In effect, this means that the person will respond to objects much as he has been accustomed to do under previous conditions of contact with similar objects. It is this fact which gives origin to the idea that perception is a kind of habit.[2] Being equipped with a response system to react to stimulating objects, is fundamental as a condition of every recognition behavior. The element of novelty comes into a response situation precisely at the point at which the person is unable to offer a complete response to the present stimulating object. Since the meaning of the object is not fully comprehended the person can respond in a way which is only a partial reproduction of a previous form of response. The lack of complete recognition means that the person is not

[1] *Cf.* Stratton, Psychol. Rev., 1897, 4, 341–360, 463–481.

[2] *Cf.* Angell, 'Psychology,' 1910, p. 157.

supplied with a reaction system to respond immediately to the object in question. In such a case the pressing need for a response to the object results in an incipient trial and error process ending in a clear-cut appreciation of its meaning and a consequent thinking reaction.

The interpretative function of the perceptual reactions is observable in many instances of daily occurrence. In the case of reading and speaking we find that there is very little stimulating material, but the response is not at all interfered with. In listening to a familiar voice, or familiar written material, we can easily demonstrate to ourselves that our response patterns are aroused by no considerable amount of excitation. No doubt the explanation for this lies in the individual's possession of dispositions organized for particular forms of situations and any prominent feature of those situations will set off the reaction patterns. It is here that we find the bases for the incorrect or unexpected responses commonly called illusions. For the same reason a person with a limited experience will be ready upon fewer occasions to respond to objects, and on those occasions will be slower to make the reaction. It has been aptly said,[1] that 'the artist sees details while to other eyes there is a vague and confused mass; the naturalist sees an animal where the ordinary eye sees only a form.' That the child reacts to objects in monotonously similar ways, is true because it has been impossible for him to build up many reaction systems. And so the significance of the pony and what can be done with it are the same as in the case of the dog, with only a variation in size. The classical illustration of the observable facts in this case is found in the name response (big dog) which the child makes to the pony.

The Elaboration of the Perceptual Functions.—The constant development of the perceptual response serves as one of the individual's important means for a growing mastery over his environment. As the reactions to an object multiply, that is, as the number of responses which it calls out increases, the object takes on more and more meaning. It is owing to this

[1] Lewes, 'Problems of Life and Mind,' 3d series, p. 107.

increasing elaboration of the perceptual response systems through the addition of meaning factors that the organism is enabled to make its way with greater facility through the maze of its surrounding objects. This facility is further increased by the fact that this elaboration of the perceptual response systems makes it possible for the person to adapt himself to many situations without invoking a definite problem of adaptation. Because of the absence of such a specific problem, and the consequent exclusion of a thought function, the simple form of the perceptual reaction allows for an immediate response to objects.

As a hypothetical illustration of the growth of the perceptual responses we might consider the reactions of a child to a typewriter. Allowing for a definite development already attained, the machine may be at first merely a thing which can produce a series of sounds when the keys are pressed. The machine, then, as soon as it is seen, has merely the sound-making meaning in the immediate response. With a more extended acquaintance with the machine the child learns that it can stimulate different and additional responses, and it thus has a different meaning when perceived. Finally, the machine takes on the complete set of meanings which are derived from all the responses the child can make to it. The point is, that what sort of perceptual reaction an object will call out at any time, or what it will mean, will depend upon the sum total of the person's contacts with the object in question. The perceptions of persons grow continually, and the growth depends upon the addition of new features to the response patterns and of completely new patterns of response.

The development of perceptions by the growth of responses is well illustrated when we are at the point of substituting an object for another in the face of an immediate need. Thus, a chair becomes a barricade, or step ladder, or typewriter table. As a consequence of the person's being forced to make new and unusual responses to objects, the latter become endowed with a range of new meanings. In the above illustration we also observe the active relating function as it occurs on the perceptual level. The similarity between objects is of course a fundamental causative factor in the perception building activity, since otherwise the possible reactions to objects would be at such variance as not to admit of any correlation.

II

If the brief description of the perceptual reaction which we have essayed is correct, it obviates some of the most salient errors in current discussions of perception, and places the interpretation of such processes upon a definite natural science level. Let us first observe, then, that the perceptual reaction is always a reaction and not a thing, namely a complex organization of subjective qualities. Moreover, a perceptual reaction is a psychophysiological reaction as all data of psychology are. That is to say, the perceptual act is not in any sense the act of an ego, or mind of whatever description, nor of a nervous system, but a complex reaction system involving all the functions of a conscious being. Notice that the vexing problem of a self, vexing, that is, once it is allowed, plays no part whatever in the interpretation we have made above. For the sum of the reaction systems which adjust the individual constitute the person, and since each person because of his particular interaction with things and persons, has developed definite types of reaction patterns, the problem of character or personality is thereby solved. Since psychology is interested only in such reaction systems there is naturally a perfect coordination between psychology and the other sciences of the individual, such as anatomy, for example, which is interested in the structure whose functions form part of the reaction pattern.

That we cannot assume that in the perceptual act we have besides an object stimulating the organism, and the organism (frequently taken to be merely the nervous system) also an object of perception, that is, a sum of mental qualities, we indicate by the statement that there are only two interacting things in a perceptual as in any psychological act, namely, the organism and the physical object. The fact is that the physical object contains all qualities, colors, sounds, tastes, hardnesses which we can ever analyze out of it, and the organism learns to distinguish these and to name them because of specific psychophysiological effects which contact with objects brings about in organisms.

Our view may be illustrated by the following example. When we perceive a blue object, in no sense is there started up a 'consciousness' of blue by an antecedent or accompanying neural activity. As the matter is stated by practically all psychologists there comes to be at this point a blue consciousness or a blue sense quality. Now we maintain that the only blue involved is a blue object, independent of a perceiver and in no wise modified by the specific perceptual act; any change in the object must be effected by the overt action following the perception. What occurs in the above illustration of a perceptual act, is that the light rays set into function a complex reaction system which involves the specific meaning of this object, in the sense that the immediate effect produced by the object on the person may now result in a specific act, perhaps in the exclamation, 'I see a blue flower.' The effect upon the person, we repeat, is muscular, neural, glandular, cognitional and perhaps affectional. Let us remember that at this stage we must consider the activity as perception in use, which has developed through a series of previous contacts with the object; for otherwise many kinds of direct contacts besides those mediated by light rays would be necessary, in order to arouse so definite a meaning of the object as to be followed by a definite act.

Clearly, the specific perceptual act is an abstraction from an empirical interaction of a person and an object; that is to say, the perception proper is abstracted from the preceding and following acts of the person, while the object is abstracted from its setting which includes many other objects and persons. The description of a perceptual act is always a deliberate rationalization of a complex event, a fact which is at least implicitly recognized by all psychologists; even those who despite their Berkeleyan adherence to mental states agree that the perception of blue is an abstraction from a blue object (of perception).[1] This abstracting process can be made out

[1] The writer refers here to the statements by psychologists that sensations are always abstractions.

most clearly perhaps by a thoroughgoing analysis of the development of the naming reaction performed in denoting things.

While it is almost impossible to describe so intricate an organic activity as a perceptual act, and at the same time avoid completely falling into a logical instead of a psychological analysis, it is still possible so to guard one's description as to prevent an essential misconception of the behavior. But unfortunately such misconstruction is rarely guarded against, for most analyses of perception merely amount to the isolation of the qualities of an object and the transformation of them into sensations, which in their totality are presumed to constitute the known object as over against the material object, which by hypothesis must remain forever beyond the pale of mental things.[1]

At the basis of the current, primarily logical analyses of psychological phenomena which may be taken as symptomatic of the unscientific character of such description, lies the prejudice deep rooted, that psychology is the study of mental states, a kind of stuff (masked by the veil of process)[2] which is different from physical material. In no matter what form this subjectivistic view is presented it must be looked upon as a vestige of religious thought in psychological dress. Today, it must be rigidly extruded from scientific thinking, since it is a prejudgment of facts to be observed. On the contrary, genuine scientific thinking must start with observable phenomena, and naturally enough when we start in this way, we never meet with mentality or physicality as the psychologist deals with them.

The immediate development from this false dualism is that the domain of psychology is that of knowing, for consciousness is thinking stuff.[3] Now thinking or knowing is assumed to be

[1] Sometimes the percept is considered as distinct from sense qualities as in the statement by Stout, "The general possibility of the transition from sense-impression to percept depends upon the existence of the percept as something distinct from the sense-presentation to which it seems as a rallying point and center of connection," 'Analytic Psychology,' Vol. II., p. 31.

[2] In spite of the veil no other interpretation is possible of a thing which has attributes.

[3] The trend of modern thought as influenced by Descartes.

the most intangible and inaccessible stuff or process, and thus has arisen the esoteric psychology of introspection. Clearly there can be no science which has as its subject matter intangible and invisible subjectivistic states, and for this reason the history of psychology mirrors much groping about for some concrete material with which to work. Finally, psychologists seized upon the nervous system as a tangible basis for the intangible consciousness. In our own day the behavioristic movement at least in one of its phases assumes that it is really the nervous system with which psychology has to deal and not at all with consciousness. This behavioristic view, though clearly mistaken in that it is still based upon a dualism one phase of which is rejected, should be credited with much scientific acumen, since it must be taken as a protest against the obviously unscientific character of a mentalistic psychology. For no science can be built upon things or processes which are not observable.[1] When we consider a perceptual act as an adaptational response to some natural object, we find no necessity for the dual interpretation of psychological phenomena such as leads to the problem how a mental state can be made to know or refer to an external object. For the functional psychologist there can be no such problem; what he is concerned with is the way a definite sort of interaction takes place between two natural things, a person and some other object which may or may not be a person. Thus, the functionalist does not create for himself the question as to how a conscious state can be initiated by a previously or simultaneously occurring neural process.

Berkeleyan and Reidean influences in psychology are maintained by the confusion of the products of logical analysis and the concrete facts of conscious behavior. Thus, the relational and interpretative character of a psychophysiological reaction

[1] It is not to the point to argue as Stout ('Manual,' 1915, p. 18) does that 'mental dispositions' must be assumed to exist in the way that 'mass' and 'energy' exist, though not directly observable, for as he himself points out, physical things and processes are inferred from directly observable phenomena. This can not be said for his mental dispositions, which are not descriptive of actual facts. And furthermore, mass and energy are obviously useful categories of physical science, but mental dispositions are only necessary because of an erroneous subjectivistic interpretation of human behavior.

is assumed to be the growth of a mental state which is called a perceptual object. From this standpoint, animals and possibly infants are presumably supposed to have no perception because they cannot possibly have the knowledge which a human adult has. In detail, a perception is assumed to be a complex organization of sensation qualities with meanings attached. Thus meaning is further assumed to be the definite self-conscious interpretation of the sensation qualities, clearly an epistemological view. In contrast to the above view, we have already suggested that what occurs in nature is the building up of a reaction-system, which at first is simple, that is, the object has little meaning, and later further contact with the object complicates and expands the reaction system, which fact is interpreted as giving more meaning to the object by the increased number of possible reactions which can now be made to it.

The consequences of the view that perception is a mental structure, are clearly brought out in the issue raised by James[1] concerning the confusion of the object perceived with sensations or perceptions of the object. James saw clearly the fallacy of Stumpf's analysis of the sensation of oil of peppermint into the sensations of taste and temperature; for James would have it 'that we perceive that objective fact, known to us as the peppermint taste to contain those other objective facts known as aromatic or sapid quality, and coldness respectively.' We cannot sympathize with Stout's[2] fear that the view of James involves 'the impossibility not merely of the "analysis of presentation" but of all analysis properly so called,' although we do agree with him that the psychologist's interest is in the psychological and not particularly in the physical object. We cannot agree with Stout, however, against James, (1) because for the former the psychological process must be purely mental, and (2) because he assumes that a perception must be a compound of sensations such as can be analyzed. The issue between a functional and a structural view is definitely brought out here. Stout thinks that because he can remember that oil of peppermint has certain definite qualities of taste and

[1] 'Principles,' I., p. 521 ff.

[2] 'Analytic Psychology,' 1896, p. 56 ff.

temperature he has analyzed a purely mental thing. Now as a matter of fact the memorial behavior is primarily the implicit functioning of a reaction system developed in direct contact with an object and is therefore most certainly a psychophysiological action. It is precisely because Stout does not see that a reaction system, that is to say the system built up in perceptual contact with objects, can be put into function by a substitution stimulus that he means to perpetuate the mentalistic tradition in psychology. If we assume that what is studied in psychology is the development of the complex reaction patterns and the means whereby they are put into complete or incipient function by various types of stimuli, we need never invoke any mysterious or inscrutable entities.

The literature on space perception clearly demonstrates the hopelessness, from a scientific standpoint, of the mentalistic doctrines. For, the problem of space in mentalistic psychology is the problem of building up or constructing space instead of the observation of the specific means whereby a person performs space reactions and adapts himself to objects variously placed. When space reactions and not geometry is made the subject matter of the psychology of space the problem of the genetic or *a priori* character of space drops out of sight. In the observation and interpretation of space reactions there can be no question of innateness or acquisition of knowledge of space, for a space reaction is not essentially knowledge as we have indicated in our description of the perceptual reaction. There is no doubt, however, that our knowledge of space is derived more or less directly from the space reactions, but this is a problem of logic and not of psychology. The study of the literature on space perception shows us clearly how the psychologists persist in forcing into their science epistemological problems which should have no place therein. Curiously enough the epistemological view gains impetus from an attempt to give psychology a scientific setting as is familiarly illustrated by the influence of Helmholtz's ideas of mathematical space upon the development of the psychology of space perception.

The ascription to current psychologists of a subjectivistic heritage from Berkeley and Reid may call for some explanation. The statement that we are still working and thinking in the Berkeleyan tradition does not exclude the fact that current introspectionism was established and elaborated by the work of the German physiologists. It is of course a matter of common knowledge that the introspective view was made possible and plausible by the physiological experiment which, dealing with isolated physiological functions, had to assume a correlated mental state to complete the description of the reaction observed. It is thus that the work of the German writers from Herbart through Fechner to Wundt, although designed to place psychology upon a sound scientific basis has in reality, because of its maintenance of the subjectivistic tradition, accomplished the opposite.

The proposed interpretation of the perceptual reactions suggests the extrusion of the separation doctrine from psychology and thus makes toward the removal of what is probably the greatest hindrance to the thorough establishment of psychology as a science. For, as long as psychology deals with conscious or mental states of any sort whatsoever it cannot ever attain to the dignity of a science as Kant long ago asserted. This statement holds whether consciousness is taken as an attribute of the psyche or mind, or of the states of consciousness and unconsciousness which are presumed to be the mind.

In conclusion, we might point out that although the organic conception of psychological phenomena appears to some psychologists as widely accepted,[1] the manifest predominance of the mentalistic and behavioristic views would seem to indicate the contrary. The apparent prevalence of the organic conception may be accounted for on the principle that insofar as a psychologist is to describe some actual psychological fact, the description must in some fashion correspond to the fact, regardless of the private view of the writer. Thus, much of current practice may be organic, but the question remains whether psychology can make much progress toward scientific

[1] *Cf.* Carr, Psychol. Rev., 1917, 24, 182. "The conception is unorthodox only in relation to prevailing definitions of psychology. To my mind it is essentially in harmony with the dominant point of view of the science, and it is not wholly inconsistent with much of current practice."

stability if psychologists do not fully appreciate the character of the materials with which they deal. While it is certainly true that definitions may linger far behind practice, the scientific practice in which this occurs, lacks much in desirable effectiveness. Even if scientists were forced to recognize all the component functions of a reaction, they might still be lacking in a full appreciation of the organic interpretation of such a reaction. That there is little genuine interest in the psychophysiological view among psychologists is evidenced by the general paucity of articles written from that standpoint.[1] Undoubtedly true it is, that the biological influence in psychology has fostered the unitary conception of organisms, but it has not resulted in any complete modification of viewpoint. In fact, the rise of the behavioristic movement urges the belief that there is no general tendency to look upon psychological phenomena as they naturally function but as they are traditionally supposed to operate. A sympathetic acceptance of the objective functional view must result in the description of the complete actual psychophysiological reaction pattern, and the consequent rejection of the exclusively mental or physical interpretation.

[1] Two notable exceptions must be here referred to, namely, Carr, 'The Relation between Emotion and its Expression,' PSYCHOL. REV., 1917, 24, 369, and Peterson, 'The Functioning of Ideas in Social Groups,' PSYCHOL. REV., 1918, 25, 214.

INSTINCT AND PURPOSE

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This paper will be roughly divided into two parts. The first part will present a definition of instinct. The second part will use this definition in a psychological analysis of purpose. The discussion will be throughout from an objective, external standpoint, that is, the interest will be in how purpose works rather than in how it feels.

By way of introduction let us liken the human being to a slot-machine. The pennies will represent the stimuli, *i.e.*, the sights, sounds, printed symbols and the like which we may apply to the machine, and the resulting pieces of candy the words, action, and movements which issue forth. If the penny be a word of praise, the answering candy may be a blush or sparkle of the eye. If the coin we apply be an insult or a blow, the resulting packet will probably contain vituperation. If the penny be the word 'white' spoken suddenly and in no particular connection, the answering phonograph sound will in all probability be the word 'black.' In every case, if we but knew the mechanism well enough, we could predict a particular action as the result of a particular stimulus.

But let us see in what ways this picture of the simple slot-machine is inadequate. We may note that sometimes when a stimulus is presented to the human machine, nothing externally observable issues forth; or, again, that something quite different from a previous response to the very same stimulus comes out. These facts of the uncertainty and changeableness of response in the human machine, though one and the same stimulus be presented, require the assumption of two principles not contained in the simple machine so far described. The first of these principles is that the nature of the response on any given occasion, or whether in fact there is any overt response at all, is dependent upon the general internal adjustment of the organism at the moment. To make a slot-machine adequate to such a situation we would have to imagine a complex machine capable of various different adjustments such that, when one adjustment was in force, the succeeding pennies produced musical sounds, when another was present, the same pennies introduced into the same slots produced different kinds of candy, and so on for each different adjustment. Finally, we would have to assume in addition that some of these internal adjustments might act like partial locking devices such that, when they were in force, no response at all would be produced from some single penny or for some particular group of pennies.

These internal adjustments would sometimes arise primarily as the result of just preceding external stimuli and sometimes as due almost wholly to automatic changes within the organism itself. If a man refuses food (*i.e.*, if the eating response is locked), it may be because of a preceding stimulus such for example as a slap in his face which has aroused the internal adjustment which we call anger (which locks the eating responses); or it may be because of some automatic physiological condition (*e.g.*, lack of hunger) which, though not positively locking, at least does nothing to unlock the eating responses. If a man responds to one and the same book on one day with tears and on another with laughter, the change in internal adjustment bringing about the change in response may be due either to a specific preceding stimulus or to some mere physiological rhythm.

The second principle which the simple slot-machine lacks and which it should possess, if it is to adequately represent the human organism, concerns the changeableness of response in the human organism which is due to learning. Everywhere we find that, simply as a result of previous experience, the organism exhibits new responses to stimuli. When a child learns to play the piano it acquires a series of finger movements in response to black marks on paper to which, before, it did not react at all. When one learns to read and write, to play tennis, to open and close doors, to lace and unlace shoes; when, in short, one learns any of the million and one things which one does learn, one is merely attaching responses to stimuli which did not originally call them out.[1]

No easy way of representing such alteration in the case of the slot-machines, however, suggests itself. We will, therefore, have to think of the latter as illustrating but single stages in the human organism and imagine a new and improved machine as a result of each acquisition of new traits and habits.

To sum up: the human being is a mechanism which makes responses to external stimuli. The nature of these responses and whether there is any overt response at all, however, is a variable matter. This changeability depends, first, upon the possibility of different internal adjustments (either called out by specific external stimuli or as the result of internal physiological rhythms), and second, upon the changes in the internal structure of the organism due to learning.[2]

Our task must now be a more specific classification and description of such responses and internal adjustments. Different classification would no doubt be possible, but for our interest, which is concerned primarily with a definition of instinct, the necessary classification is simple. It contains but three groups: (1) independent reflexes, (2) subordinate acts, and (3) determining adjustments.

By an independent reflex we shall mean any response to a stimulus which takes place always in the same manner and relatively independently of what the rest of the organism is doing. The kick of the foot in response to a tap on the knee, winking in response to a movement before the eyes, sneezing in response to tickling the nose, yawning in response to certain internal sensations, are examples. These always occur in much the same way and each is relatively complete in itself and independent of what the rest of the organism may be doing.

Activities on the other hand, such as biting, chewing,

[1] And this holds, be it noted, not only for actual overt responses, but also for the internal adjustments we have just discussed. Thus, for example, the internal adjustments originally appropriate to and only aroused by such things as loud sudden noises and really startling objects may as a result of training get attached to a whole series of secondary associated objects such as the dark, strange faces, etc.

[2] Also upon changes resulting in the course of natural growth, *e.g.*, the appearance of new sets of instincts as the child matures.

swallowing, which form part of larger wholes—in this example, eating—would be classed in the second group: *i.e.*, termed subordinate acts. The members of this group are almost infinitely numerous. The leg movements of walking, the handlings of curiosity, the cries and shouts and strugglings of anger, the sighing and tears of sorrow, the facial expressions, words and gesticulations of love, would all be examples. In fact all the things we do, not as separate and independent reflexes, but as parts of bigger groups of activity, belong to this second class.

Finally we have as our third group what we called determining adjustments. These are, in fact, to be considered as identical with the internal adjustments just described in our picture of the slot-machine. They determine and set in readiness the subordinate acts. Whether one responds to one and the same stimulus with the subordinate acts of handling and manipulation, those of destruction, or those of rejection, depends upon which particular determining adjustment has first been aroused—whether one of curiosity, one of anger, or one of fear.

It is to be noted that determining adjustments often occur in hierarchies. What may be called the lowest one of the hierarchy is then the immediate determining adjustment for the actual subordinate acts. The next higher one of the hierarchy releases this lowest one. A still higher one releases that, and so on. For example, we may suppose that on a given occasion an individual's leg and foot movements are directly subordinate to what may be called the walking adjustment. This walking adjustment, however, we may assume is subordinate to an anger adjustment. (The man may be on his way to confront a business opponent.) This anger adjustment will then be subordinate to a business adjustment and, finally, this business adjustment itself may be assumed to be subordinate to what may be called the man's general sociodomestic adjustment. In the case of such a hierarchy of adjustments it is obvious that the function of all, save the lowest one in the sequence, consists in a release of a lower determining adjustment rather than in a release of actual subordinate acts.

One further point. In the case of activities such as eating, running, walking, is it legitimate to talk of a determining adjustment as something existing in addition to the individual walking or eating movements themselves? The reason I assume that there is a distinct walking adjustment rather than that the individual walking movements are released directly by the next higher adjustment (for example, the anger of the above illustration) may be indicated first by the case of the child. In the case of a baby, the individual walking movements are obviously very irregular and variable. Yet (when the child is in the 'walking vein') they are all *walking* movements: they all fall within that one general class. Now, wherever these two phenomena occur, of variability within a class of movements and persistence of the class as a whole, my thesis will be that we must assume a specific determining adjustment.

In an adult the situation (in the case of walking) would seem to be somewhat different because of the added influence of habit. With the advent of habit there come fixed and invariable sequences (in the case of walking, fixed and invariable sequences of foot and leg movements). This being the case, the assumption of an immediate walking adjustment to release and maintain walking movements, as such, would not seem so necessary. The total complex of movements is nearly equivalent to a single act and as such would seem a candidate for the *immediate* control of a higher adjustment, such, for example, as the anger of the preceding illustration. It may be noted, however, that in unusual situations such as unevenness or obstacles in the path, this unitary and automatized character of walking may break down, in which case the original walking adjustment would seem again to have to come to the fore to release further walking movements not part of the automatized act.[1]

This discussion has brought out three important points

[1] This last point, however, I do not necessarily desire to stress. I would be willing to admit the possibility that with the growth of habits the original determining adjustment upon which these habits are built up recedes and may even entirely disappear (waning of instinct). And, if such is the case, the total habit becomes an alternative act *directly* at the service of higher adjustments.

concerning determining adjustments which it will be well to summarize. (1) The determining adjustment sets in readiness a particular group of subordinate acts. One and the same external or internal stimulus may call out quite different groups of subordinate acts according to the particular determining adjustment which happens at the time to be aroused. (2) Determining adjustments often occur in hierarchies, the higher ones calling out the lower ones and the lowest one of all calling out the actual acts. (3) The essence of the determining adjustment and the reason for it consists in the variability of the subordinate acts. If such variability has disappeared, as is the case where habits have developed, the determining adjustment tends to atrophy and may, perhaps, even disappear altogether.

Let us here stop and assert that determining adjustment as thus characterized is a definition of *instinct.*

It would appear that such a definition tends to differ primarily from most others by virtue of its two-level conception (determining adjustment underneath, subordinate acts on top). Most other definitions seem to reduce in the last analysis to the assumption that an instinct (on the objective side and before learning has affected it) is a definite and stereotyped action (*i.e.*, that it is an inherited reflex pattern).[1]

Two authors, however, I have found who suggest views similar to mine. They are Woodworth[2] and Kempf.[3] The former's conception of 'drive and mechanism,' and the latter's 'autonomic and projicient systems,' both suggest a two-level account.[4] My idea of determining adjustment and subordinate

[1] To take an introspectionist on the one hand and a behaviorist on the other: such a view seems to be that of both McDougall and Watson. See W. McDougall, 'Social Psychology,' p. 29 and following, and J. B. Watson, 'Behavior,' Chs. IV. and VI.

[2] R. S. Woodworth, 'Dynamic Psychology,' Chap. II.

[3] E. J. Kempf, 'The Autonomic Functions and the Personality,' Nervous and Mental Disease Monograph Series, No. 28. See also an article by G. A. de Laguna, Psychol. Rev., 1919, 26, especially page 419, for a discussion of emotion significant for the present theory.

[4] Woodworth might object to his theory being called two-level, since he seems to hold that one and the same apparatus may function either as a drive or mechanism according to circumstance (*i.e.*, that the difference between drive and mechanism is functional rather than structural). But from the point of view of function, if not structure, his *is* a two-level theory.

act would differ from either of theirs only, first, in leaving speculation as to the actual mechanism of the thing open; and, second, and most importantly, in emphasizing the *variability* among the subordinate acts.

It is this variability which I now particularly wish to emphasize. It will be found especially significant when we turn to the analysis of purpose.

By way of introduction to that analysis, let me now quote two concrete descriptions of animal behavior. First an account of nest building by Prof. Swindle.[1] The bird observed was a male, one of a pair of Brazilian birds, in a large outdoor cage in a zoo.

"Early in April, I noticed that as this bird walked about in its cage, it occasionally bit in the air as if it were grasping an object. At times, however, it bit the bars of its cage, a branch of a tree, and even the naked earth. Sometimes, it sprang and ran rapidly, and it very often flew to a one-and-a-half meter post on the top of which a wide shallow basket was fastened. On April 18, 1915, the following movements were observed: The bird stood at first motionless in the basket, shortly it began biting in the air as if attempting to seize something, and occasionally it seized, lifted, and then dropped certain branches which lay in the basket. It threw a stick out of the basket onto the ground, gazed for a few seconds at the sky, grabbed still another twig which it had previously thrown out of the nest, flew back into the basket with these, beat them quickly here and there without releasing them, let them fall in the basket, bit and arranged them there, and then remained for many minutes by the side of the female which was then also in the basket. Presently the male, half-springing and half-flying, reached the ground, ran quickly to and fro in the cage, gazed for a while at the female as he walked round and round the post, and finally sprang and seized a twig which projected from the basket. This twig was unfortunately so badly tangled with the others of the nest that a great many were drawn out with it. But in spite of the fact that the nest was occasionally mutilated by the builder, a neat nest was eventually constructed."

[1] E. P. Swindle, *Amer. J. of Psychol.*, 1919, 30, 180.

The startling thing about this account is that it indicates that, even in the case of those supposedly perfectly adaptive instinctive activities such as nest-building, careful observation may show a considerable amount, and in this case indeed a positively shocking amount, of variability, the very point we wish to emphasize. If the reader is inclined to doubt the validity or general applicability of this one case, I may quote a word or two more.

The author says, further: "The fact deserves emphasis that birds often work very crudely while building the nest. It is really astonishing how often a bird allows objects of building material to fall, apparently without responding further to them. A bird frequently stands or walks among objects which it could well use in constructing its nest but suddenly runs or flies away without grasping any of them. I have observed the Blue Jay to tear the leaves, branches, and feathers from another bird's nest before it seized an object of the foreign nest and flew to the one which it had started; and it often seemed to arrange the objects on the foreign nest as if it were preparing to deposit its own eggs there, which it did not do." [1]

And, again: "Many birds can build their nests at only certain portions of the trees which are adapted to hold the collected objects, and generally, these places are located by the birds only after a number of trials. That this fact is not well known seems to be due to the circumstance that it is very difficult to observe a bird with sufficient scientific accuracy in freedom. It should also be mentioned in this connection that birds occasionally start two or three nests simultaneously and later destroy some of them to obtain the material for a single nest." [2]

Let us turn, now, to our second case; to wit, Prof. Thorndike's classical experiment of a hungry kitten shut up in a cage with food outside. We quote his words: "When put into the box the cat would show evident signs of discomfort and of an impulse to escape from confinement. It tries to squeeze through any opening; it claws and bites at the bars

[1] Op. cit., p. 178.
[2] Op. cit., p. 183.

or wire; it thrusts its paws out through any opening and claws at everything loose or shaky; it may claw at things within the box. It does not pay very much attention to the food outside, but seems simply to strive instinctively to escape from confinement. The vigor with which it struggles is extraordinary. For eight or ten minutes it will claw and bite and squeeze incessantly."[1] It may be added that in the experiment as arranged by Thorndike the kitten usually got out eventually because one of its strugglings quite *accidentally* hit upon a release mechanism arranged to open the door. These mechanisms were always very simple: a hanging loop of wire which required but the slightest clawing, or a wooden latch easily lifted by the nose. It is to be observed that with the opening of the door, a new final act occurred, the cat went out and ate.

I draw attention to the significant feature of both illustrations, the variable or random character of the subordinate acts. In the one case, squeezings, bitings, clawings; and in the other, picking, dropping, carrying. First one act and then another occurs in a perfectly haphazard order. The whole thing seems to be mere chance.

Let us stop, however, and analyze the thing further. Is there any principle underlying the actual order of these, to all outward appearances, purely random acts? It must be supposed that there is. To return to our slot-machine, we must suppose that each one of them is set off by some particular penny, if we could but detect the penny. Now, undoubtedly the pennies are in part internal conditions such as the senations arising from muscle strain. In addition, however, it is equally certain that they are in part external objects. The stimuli to which the cat's clawings, biting, etc., are the responses are in part particular features of the cage itself. In so far as they are such features, a definite principle underlying the succession of the responses can be actually observed. The cat, at any given instant, responds to a feature of the cage with which its just previous reaction has brought it in contact. In other words, it carried out a definite train of movements. One

[1] E. L. Thorndike, 'Animal Intelligence,' 1911, p. 35.

feature of the cage calls out one response. As a result of the movement made by this response the cat is brought in contact with another feature of the cage. This new feature calls out still another response and so on. If, in between these responses to the successive parts of the cage as such, we imagine a few responses to purely internal conditions, we shall have a fairly exact picture of the cat's total behavior. An identical account could be given of the bird's nest-building activities. Although to all outward intents the acts are purely haphazard and random, still they follow definitely traceable sequences. Finally, at the risk of hammering the point to excess, we may again emphasize that in each case the particular subordinate acts are what they are, rather than other equally possible responses to the same stimuli, by virtue of the particular sensitizing effect of the determining adjustments.

The second feature about the process, to be emphasized, is that the individual random responses continue *until* some one of them presents a new stimulus, the final response to *which*, removes the condition or stimulus which was the original cause of the determining adjustment itself. In the case of the bird, the activity continues until a nest eventually gets built. When this happens we have a new stimulus, the completed nest. And the responses to the completed nest, those of laying and setting, are such as to remove the internal physiological condition which was the original cause of the nest-building adjustment. In the case of the cat, its random acts eventually open the door, whereupon food is presented and the response to food is such as to remove the internal sensation of hunger which was the stimulus to the original food-getting adjustment. In each case the determining adjustment continues, *until* some of its subordinate acts removes the stimulus and with it the adjustment itself.

We have here a fundamental phenomenon. In as much as only one act will remove the adjustment and the adjustment continues *until* that act occurs and in as much as further the adjustment sensitizes and hence, so to speak, supplies the group of acts among which this final successful one appears, it seems to me that we have a situation which may truly be characterized as one of purpose. A determining adjustment provides the purpose. The subordinate acts (which the adjustment sensitizes) are the means which the organism adopts to fulfill that purpose and the removal of the determining adjustment itself (as a result of one of these subordinate acts) constitutes the fulfillment of that purpose.

If this analysis be accepted, then the goal of this paper, an objective psychological analysis of purpose, is in sight. All that remains to be done is to transfer the account to man. Before attempting this latter, however, let me draw attention to the relation between this definition of purpose, and that of Professor Perry.[1]

In Professor Perry's analysis the emphasis is put upon *learning*, upon the fact that with repetition the cat gradually *learns* the successful act. It is in the fact that the successful act is selected (learned) and the other acts rejected, that Professor Perry sees the primary justification for calling the situation teleological. I, on the other hand, believe that even without learning, the situation is teleological. Even though the cat showed absolutely no evidence of learning to get out in a shorter time on succeeding trials, the mere fact that on each single trial it hits about *until* it gets out, seems to me to be sufficient to characterize its activity as purposive. The cat hits about *in order to* get out, *for the sake of* getting out—expressions which Professor Perry himself designates as the 'most unmistakably and unqualifiedly teleological expressions in common use.'

It will be noted that such situations do not imply anything essentially non-mechanical. Given the environment and the total condition of the organism, the complete response (*i.e.*, the particular succession of subordinate acts and the time of the appearance of the final successful one) can all be predicted in a wholly deterministic way. This, however, is no criticism of the definition. When we are talking mechanism we would be very much upset to find something which was not mechanical.[2]

[1] R. B. Perry, 'Docility and Purpose,' Psychol. Rev., 1918, 25, 1–20.

[2] It should be emphasized that my definition differs from Prof. Perry's principally in not going quite so far. The essential idea for my conception was for the most

To return, now, to our further problem: we wish to show how this mechanism of determining adjustments (or instinct) and subordinate act works in man. One preliminary remark, however, may not be amiss. In the preceding descriptions we spoke as if the random strugglings of the cat or the bird always ended in success. As if, that is, when the activity ended, it was always because a response was finally made which removed the initiating stimulus for the determining adjustment. But such an ending, though from the point of view of purpose the successful one, is by no means the only mechanically possible one. Instead of the cat's getting out and eating the food, some other powerful adjustment may intervene and replace the food-getting. Thus it may become frightened by the bruises and bumps that it receives as a result of its strugglings so that a *fear* adjustment gradually sets in and replaces the original food-getting one. Hence when the door finally does open, the cat either continues to struggle or runs off and hides instead of eating. In such a case the original food-getting adjustment has not been satisfied but merely replaced by another. A second way in which the original determining adjustment may not be 'satisfied' may be as a result of exhaustion. The cat may become so utterly exhausted that all responses cease to be made. 'It gives up trying.' *Only if some subordinate act takes place which was released by the determining adjustment and which removes the stimulus to that adjustment can the purpose as such be said to be fulfilled.*

Let us now turn to man. In the case of a single purpose I believe that the situation is essentially identical to that represented by the cat struggling to get out of the box or of the bird struggling with sticks and straws. An instinct or determining adjustment is aroused. This facilitates and sensitizes one particular class of subordinate acts. Some one or group of these, if they occur, remove the stimulus to the original instinct and, if they do thus occur, we say, speaking in purposive

part drawn from Perry's discussion. It may be noted, however, that determining adjustment and subordinate acts, though analogous to, are not exactly identical with Professor Perry's 'higher propensity' and 'lower propensities.' I believe, indeed, that my two concepts are, for the purposes of behavioristic treatment, more precise and definite than his.

language, that our problem has been solved, that the right means have been chosen.

We may make the issue more concrete by an example. Imagine a man trapped in a burning hotel. He *may* rush madly about in the same blind fashion as does the cat in the cage. If so, his behavior and that of the cat would seem to be entirely identical. It may happen, however, that instead of thus rushing blindly he stops to *think*. If such be the case, he does not attack all the exits of his trap indiscriminately, but only some one which is apparently suggested to him by his 'thoughts.' We have in these thoughts a new principle which does not hold or, if so, to an infinitesimal extent in the case of the cat.

What, now, we may ask, is this thought and when and why does it occur? It will be remembered that in the initial statement of our program we declared that it was an objective, behavioristic account rather than an internal subjective one that we should aim to achieve; that we were interested not in how purposes felt, but in how they worked. Can we, now, shift our point of view and begin to talk about apparently internal subjective things such as thoughts? My answer is that thoughts, or at least the kind of thought with which we are here concerned, can be conceived from an objective point of view as consisting in internal presentations to the organism (on a basis of memory and association) of stimuli not actually present but which would be present, if some hypothesized action were carried out. Such a definition says nothing about the subjective 'immediate-feel' side of thoughts as such. It is concerned with thought simply in so far as the latter has significance in an objective, behavioristic, stimulus and response account. A complete treatment of thought on its subjective (immediate-feel) side and of its epistemological significance we can leave to the combined researches of introspective psychologist and philosopher.[1] The one point we here mean

[1] In what follows I present one sample of the way in which 'thoughts' may, it seems to me, be properly introduced into what claims to be a purely behavioristic (stimulus and response) account. It is my belief that such examples might be indefinitely multiplied and that a whole system, properly to be called behavioristic psychology, might be built up in which thoughts (on their behavioristic side) would still find as much place as do sense-stimuli.

to make is that over and above whatever these functionaries may have to say, a significant behavioristic aspect of thought still remains.

To return to our definition itself. What we mean by thought in this particular case as 'an internal presentation to the organism (on the basis of memory and association) of stimuli not actually present but which would be present, if some hypothesized action were carried out,' can be made clear perhaps, only by a concrete example. We come back to our man in the hotel. Instead of trying all possible parts of his burning trap, we find him stopping to *think* and then on the basis of that thought reacting to certain parts only. What is this stopping to think in behavioristic terms? It consists, I would assert, in what may well be called, not random subordinate acts, but random subordinate *thoughts-of-acts*.

You will remember that the cat reacted to a part of the cage with which the just preceding response had brought him in contact. As a result of each successive response the cat was automatically provided with a stimulus for another response. How now in the case of the man? He sees a door but instead of actually responding, he merely thinks of responding. He hypothesizes a response and on the basis of this hypothesized response he achieves mentally, a new stimulus, *i.e.*, the mental image of what is beyond the door. (This mental image is provided by memory or association. He may actually remember that this door leads to a corridor or merely by association based upon the general position be merely led to imagine that it leads to a corridor.) In either case this idea or mental image of what is beyond acts as a stimulus to a new thought-of-act. He thinks of going down the corridor and this will lead, mentally, to still a new stimulus of what is at the end of the corridor, perhaps stairs or an elevator. These will lead in a flash to a mental image of an open road to the outside. When the image of the latter occurs he will in all probability actually react. If no such vision of an open road beyond stairs or elevator occurs, he will mentally rush back, or perhaps more truly speaking jump back, to his starting point and mentally attack some other feature of his environment.

One point in need of immediate further elucidation emerges. Why is it that the man thinks in the first place? We have already said that sometimes he does not think but merely rushes blindly about as does the cat. Stopping to examine the matter more carefully, it would seem that those times when he does not think, but does thus rush blindly about, are times when he is excessively frightened. Occasions, in short, when the original determining adjustment is especially potent. If, therefore, a particularly potent adjustment produces action, I should suggest that when action does not occur, it is when some inhibiting or checking process which works against or controls the determining adjustment is also acting. Just what, physiologically, this inhibiting or checking process may be, I shall not venture to say, though I shall assume that like all other physiological processes it follows perfectly definite mechanical laws. The significant thing, for us, is simply that it works against the determining adjustment in such a way as to *shunt* the latter's activities, so that instead of producing subordinate acts, it, the determining adjustment, produces merely thoughts-of-acts. For this reason, let us call it the thinking or rationalizing adjustment. We may note in passing several interesting things about this tendency. First it seems to be much stronger in some individuals than in others and secondly, to be very much subject to training and practice. In fact it can sometimes be so over-trained as to result in an almost complete inability to act at all, a condition which is sometimes supposed to be characteristic of the typical college professor.

We may now ask, how is it that this thinking, rationalizing tendency, having once got going, ever ceases, in order to allow action to take place. In answer, we would posit the general principle that action eventually occurs because of what we shall call a prepotent stimulus. A stimulus may be prepotent for either of two reasons: (*a*) because it is the stimulus to an act to which the original determining is particularly favorable or (*b*) because it releases some other favorable adjustment. To return to the example of the man in the hotel. The first case would be represented when as a result of his trains of thoughts-of-acts he arrives at the mental image of open road beyond stairs or elevator. Such a sight of the outside, if present in perception, would be the *one* stimulus to which the man would have been *most* sensitive. If the door in the cat's cage had been left open, the response of going out would have been *first* to occur. It would have taken precedence over all other responses such as those of squeezing, clawing, biting. So, here, the thought of the open road beyond stairs or elevator is the stimulus to which the given determining adjustment makes the man most susceptible, so susceptible in fact that the impulse is enough to break down the inhibiting effect of the thinking propensity and action results.

Turn, now, to the second way in which the stimulus may be prepotent: the case in which it is prepotent because it releases a second adjustment favorable to the given action in addition to the original adjustment. Suppose that as a result of social intercourse our man had acquired a general maxim to the effect that stairs and elevators become perfect smoke-stacks and that much the best thing to do in case of fire is to run to the window and call for help. Simply on the basis of his original determining adjustment alone the thought of the window would tend to call out the subordinate act of going and calling. If, now, in addition such an act is supported by what we may call a general 'social-subservience' adjustment, a tendency to do those acts recommended by society, this act becomes doubly ready to go off, so much so that it does actually occur.

To sum up: thought ceases and action supervenes whenever thought arrives at the image of a prepotent stimulus. And a stimulus is prepotent either (*a*) because it tends to call out a subordinate act which is especially favored by the original determining adjustment or (*b*) because it tends to call out in addition to the original determining adjustment some other adjustment which is also favorable to the act.

This is all there is to a case of single purpose.[1] An original determining adjustment provides the purpose. Subordinate

[1] The problem of what happens in the case of a conflict of purposes is more complex, but the general principles of explanation would be the same.

acts are either actually called out or merely thought of. Eventually one occurs which removes the stimulus to the determining adjustment and the purpose is satisfied. Or, if no such subordinate act occurs, it remains unsatisfied until, perhaps, mere exhaustion causes the determining adjustment to disappear.

In conclusion, we may briefly enumerate the more important points we have advanced and which we most wish to emphasize: (1) a two-level (*i.e.*, determining adjustment—subordinate act) theory of instinct; (2) Purpose as interaction of determining adjustment and subordinate acts; (3) images of memory and imagination (thoughts) as properly included in a behavioristic non-introspective account; (4) the satisfaction of purpose as consisting in the removal of the stimulus to the determining adjustment as a result of one of the subordinate acts which the determining adjustment itself releases.

BRAIN MECHANISMS AND MENTAL IMAGES

BY S. BENT RUSSELL

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When you think of an absent friend you may seem for an instant to see his face, *i.e.*, you have a mental image of his face. This is commonly said to be due to memory. But what then is memory? There may be readers of this article who have considered memory only from the subjective point of view. Let us for the moment now consider memory apart from the matter of images and consider it from the objective point of view. It can be shown that memory is accounted for by the operation of nerve processes. There are pathways along nerve fibers for nervous impulses. Some of these are known as association pathways. There is good reason to believe that changes of resistance at the points of junction between nerve fibers known as synapses, determine the course of nervous impulses along one path or another. The frequency and recency of previous impulses determines the synaptic resistance. Hence the nerve paths are developed by individual experience and thus memory is evolved.

The dual common path theory furnishes an explanation of associative memory. The common path is open to impulses coming from two tributary or private paths. An impulse from one lowers the synaptic resistance for an impulse from the other. We need not go further into this theory in this discussion. Let us take it as an explanation of associative memory for the purposes of this demonstration.

Before you can have a memory image of an object you must at one time be conscious of the object itself. Let us consider briefly how perception can be explained in terms of nerve processes. A man is never conscious of an object unless there be communication from the object to his brain. We may say further that there must also be a molecular change at some brain center or centers that bears a correspondence to the object. In other words, the object causes a nerve impulse from a sense organ to a brain center and the impulse causes a response (molecular) at the brain center. To mention a special case, when a point on the retina is stimulated by a ray of light from some object, there is an impulse sent by way of a certain nerve fiber to a certain nerve center in the cerebral cortex, where it provokes a molecular response that corresponds to the retinal stimulation. We see that each visible point of the object has a line of communication by way of the retina and a particular nerve fiber to a brain center. All the points together produce a pattern molecular response in the brain. This joint response simulates the object in view.

Let us for convenience use the term 'mimetic response' to express the molecular response that simulates the object as represented at a sense organ.[1]

To put it another way, there is a registering mechanism in the brain center that transforms the afferent impulse into light equivalent, heat equivalent or other sensation equivalent as the case may be. That is, it in effect reverses the transformation that occurred in the sense organ. When a ray of red light for example falls on the retina, there is a change in the brain center the same as if a red light had penetrated it. Let us term this hypothesis 'the mimetic theory of perception.'

This explanation of perception is not complete unless we make allowance for the effect of association mechanisms. You will see this if you think of a trained musician listening to a well-known melody on one hand, and a man without any musical training listening to the same melody for the first time on the other hand. The brain correspondence will be far greater in the former case. Every individual has to be trained to see and hear things as they are. Brain correspondence increases with knowledge, *i.e.*, with the development of associa-

[1] The writer has presented an explanation of mechanisms of associative memory and of their functions in intelligent and purposive behavior and in consciousness in the following articles in the PSYCHOLOGICAL REVIEW. 'The Effect of High Resistance in Common Nerve Paths,' 1916, **23**, 231–236. 'Compound Substitution in Behavior,' 1917, **24**, 62–73. 'Advance Adaptation in Behavior,' 1917, **24**, 413–425. 'Communication, Correspondence and Consciousness,' 1918, **25**, 341–358.

tion mechanisms. In other words, a man's habits determine the definiteness of his perception.[1]

We must also make allowance for the effect of language and other forms of expression upon perception. Naming and numbering are great helps in perception. In learning to describe his environment, a man develops association nerve paths that serve to increase his brain correspondence with the world he lives in.

Keeping these matters in mind, we can say we have in the mimetic theory, a satisfactory explanation of consciousness as it reflects a man's present environment. The purpose of this discussion is to bring out an explanation of the memory image in terms of nerve processes. Before attacking the problem, let us note the difficulties we have to meet. It is not so hard to conceive how a ray of red light can cause a disturbance in a brain center that is characteristic of redness as it is to conceive of a nerve impulse aroused in the ear by the sound of the word red, for example, producing a disturbance in a brain center that is so characteristic of redness that the subject will for an instant see a red color in his mind. That is, he is conscious of red when there is in reality no red to be seen. There is, you will allow, an apparent paradox. Why should an impulse coming from the ear, arouse an image of light? We can think of a ray of red light penetrating a brain center and causing a characteristic molecular disturbance like a light ray acting on a photographic plate. We can go another step and think of a ray of red light stimulating a sensory nerve ending and so causing a nerve impulse which goes to the brain center and by aid of a special mechanism reproduces there the disturbance at the sensory ending caused by the red ray and characteristic of redness. But why should some other kind of stimulus produce a molecular disturbance that has a quality belonging to red?

In other words, if we seek explanations in terms of nerve processes, the one for consciousness of the past is far more difficult than that for consciousness of the present.

On account of the obvious resemblance, the memory image

[1] W. B. Pillsbury, 'The Essentials of Psychology,' 1911, p. 157.

is often considered as composed of sensations that are centrally aroused instead of coming from the effect of the environment acting directly as in perception. In these terms we would note that an explanation of a centrally aroused sensation by nerve processes is by no means as simple as in the case of a true sensation. If the reader can see the point of difficulty it will help him grasp the demonstration given herein.

As the next step let us take for illustration the case of a young man of normal mind sitting in a boat and drifting down a river. As he drifts along he observes different objects along the bank and of course is conscious of each in turn. Let us say it is the second time he has made this voyage. He will have from time to time mental images of objects that have passed out of view and sometimes he will have an image of an object ahead of him that will soon come into view. Suppose at one time he passes a gravel bar on which he sees a flock of white cranes. Later on he passes another gravel bar and has a memory image of the white cranes. At one time he passes under a railroad bridge and is startled by the roar of a train. Later on he passes another bridge and has a memory image of the roar. Let us see if we can find an explanation of these memory images in terms of nervous mechanisms.

In a book published some years ago, Professor Kirkpatrick describes images as the result of the functioning of brain centers that are made active in perception by impulses coming from sensory centers, while in imaging they are made active by impulses coming from some other direction.[1] The theory that will now be advanced does not conflict with Dr. Kirkpatrick's view.

Returning to our illustration, when the man observes an object in the first instance, there is, as we have already stated, a molecular change in the cerebral cortex that simulates the object and which we term the mimetic response. That is not the whole story, however, as there is another process too. In the case of vision for example, the mimetic response only lasts while the retina is stimulated by the object or for a moment more. There is a second process that is a recording

[1] E. A. Kirkpatrick, 'Genetic Psychology,' 1909.

process. Let us call this record making process the 'tuning process.' It is a specific molecular change that is more permanent than the mimetic response. There is one tuning process for light, one for sound, one for heat, etc. After the tuning process has occurred in a certain center, any nerve impulse that reaches the center will cause a molecular disturbance that simulates the original stimulus disturbance in the sense organ. In this way we have correspondence of a brain center with a past environment. This simulating process in the nerve center is, we will say, a conditioned molecular response. Let us term it the 'sounder response.'

In our illustration when the man is conscious of his present environment, there is a series or procession of impulses through certain brain centers. In other words the mimetic response is a series of molecular changes in the brain centers that bears a correspondence to the environment. The tuning process is also a series of molecular changes that bears a correspondence to the environment. At the same time it leaves an impression on the molecular structure that also bears a correspondence to the environment. We may liken the mimetic response to a gust of wind passing over a sheet of water and causing a ripple that is soon gone and we may liken the tuning process to a gust of wind passing over a smooth sand bed and leaving the familiar ripple marks that remain for a time.

The sounder response is conditioned by the tuning process. It is a series of molecular changes that bears a correspondence to the environment that caused the tuning process. There must of course be a specific sounder response that corresponds to light and another that corresponds to heat and another for sound and another for taste, etc. When a man has a memory image of a noise he heard a minute ago, there is in one of his brain centers a sounder response that is provoked by a nerve impulse coming, we will say from some association nerve path. The effect is the same as if there were communication from the noise-making object to the brain center that took a minute to reach the brain center. It is something like the thunder peal that reaches the ear some seconds after the lightning stroke that made it.

In the same way when a man has a memory image of an object that has recently passed out of view, there is in one of his brain centers a sounder response that is provoked by an impulse from some association nerve path. The effect is the same as if the object were still in communication with his brain. In his mind's eye he sees perhaps a red triangle although there is no red triangle now present to stimulate his organs of vision.

Let us now consider the tuning process again. A harp string can be tuned by turning the peg that holds it and thus changing the tension. With a given string a certain tension will produce a certain note, say C. Now a certain tension means a certain relation of the molecules to each other. The tuning operation produces a certain molecular arrangement that corresponds to the note C. As long as this molecular arrangement is preserved the string when struck will give the sound of C. The note C is of course a vibration at a certain rate and thus we see the molecules are so arranged by the tuning operation as to give when struck, a certain response. We may use the harp string as an illustration of a brain center. We may suppose that a nerve mechanism exists by which a certain brain center can be brought into a certain molecular arrangement so that thereafter any nerve impulse will produce a particular response or movement.

The tuning process is, we have stated, a change in the molecular structure. We may compare it to the alteration in litmus paper that is changed in color from red to blue when it is wet with an alkaline liquid. We note that after the change the paper gives off blue rays when exposed to light. We may also compare the tuning process to the case of paper that is tinted with some color that is not fast. If a ray of sunlight falls on it, a faded spot remains that will be seen whenever there is any light on the paper and so is a sort of memory image of the sunlight ray. Hence we may think of a bright object in view of a man as causing a little faded spot in his brain that is brought out by any passing nerve impulse that comes later on.

Let us remember that the man is never conscious of the mimetic response or the sounder response. He is conscious of the object itself that furnished the stimulus, whether it be in the present or past environment. In the case of an image, the effect on consciousness is the same in kind as if the object were still in communication with the brain center.

It may make these explanations more clear to you if they are illustrated by a diagram or table like the one below. Let the features of the object seen in the first instance be represented by the letters A, B, C, D, E, and the impression on the retina be represented by the letters A1, B1, C1, D1, E1, and the mimetic response by A2, B2, C2, etc., and the tuning process by A3, B3, C3, etc., and the sounder response by A4, B4, C4, etc. The table follows:

Object	A	B	C	D	E	Object
Retinal image	A1	B1	C1	D1	E1	
Mimetic response	A2	B2	C2	D2	E2	Perception
Tuning process	A3	B3	C3	D3	E3	
Sounder response	A4	B4	C4	D4	E4	Mental image

Let us now go back to our case of the man drifting down the river. We will take the experience of the bridges. There is a certain center in the man's brain that is reached by a certain nerve path coming from the ear. It is also reached by a certain association path that has one connection with the ear and one with the eye. When the man sees the first bridge, there is an impulse from the eye via the association path and when he hears the roar of the train there is an impulse from the ear via the association path and one via the other path from the ear. The sound impulses cause a tuning process in the brain center. Later on the man observes the second bridge and there is an impulse from the eye that follows the association path more easily as it is now more open because the two impulses in succession, caused by the experience at the first bridge, have made it so. When the impulse reaches the brain center, there is a conditioned molecular response and the man has a sound image of the train roar at the first bridge. Please note that the response simulates a sound although the provoking impulse originates in the eye. This is the apparent paradox that we remarked in the beginning.

You may comprehend the case better when you see it presented in a diagram as in Fig. 1. The circle *V* represents the visual organ, the circle *A*, the auditory organ and the circle *R*, the brain center. At the sight of the first bridge an impulse travels from the eye at *V* by the dotted line path *C* to the center at *R*. The roar of the train sends a second impulse from the ear at *A* by the common path *C* to *R*, and a third impulse by the dotted line path *B* to *R*. The last two impulses provoke a tuning process in *R*. After the first and second impulses have passed over path *C* it is more open to later impulses. The sight of the second bridge later on sends an impulse from the eye at *V* by the path *C* as shown by a heavy line, to the brain center *R*. There is now a sounder response at *R* that simulates the roar of the train at the first bridge. A path line on the diagram of course represents a plural number of nerve fibers.

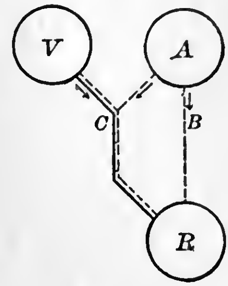

FIG. 1.

On consideration you will find that the explanation just given for a particular case will answer for all memory images. There are many cases of course, where a large number of association nerve paths are required to produce a definite memory image. The larger the number, the higher will be the degree of correspondence with the environment at the time of observation.

The association nerve paths must be developed by individual experience. A baby cannot have mental images like an older person. A baby must learn to talk before he can hear words in his mind. He must learn to sing before he can hear a song in his mind. He can only coördinate impressions that have become familiar. He learns to form images step by step. He must learn to walk before he can image distance relations and space relations. We have previously observed that perception is determined by a man's habits to an important extent and the same thing is true of mental images. The study of language and expression, also, must have an important effect in developing the mechanisms for making mental images.

A man who has learned how to write a good description of a landscape has no doubt increased the definiteness of his mental images. You are perhaps able to look at a new model of an aeroplane and memorize it so as to have a good memory image of it a week afterwards and have never remarked that your ability to do this is the result of long and repeated practice on similar objects. On consideration you will see that these statements fit in with our theory that the image is due to a conditioned molecular response that simulates a past environment at one time in communication with the brain centers responding.

We find then that a typical memory image accompanies a compound sounder response and is due to the operation of tuning process mechanisms and association mechanisms, the latter serving to coördinate the former.

We have found an explanation of how the brain can simulate a past environment. Let us pass on to the case where the image corresponds with a coming environment. Let us return to our illustration of the man on the river. Suppose he sees in the distance ahead of him a column of smoke and then has a mental image of a landing pier at a village around the next bend. At first thought this appears to be an image of a future environment but on second thought we find that the man's mental image is the result of his previous voyage over the same route. The same rule will apply to all cases where the image appears to anticipate the object. The mental picture of things to come is made up of elements derived from past experience.[1] A man can imagine an aggregate that he has never seen but it will be made up of familiar units. The power of combining mental images is acquired by degrees. It must be largely due to social environment and to language associations. What one man has observed another man can image by the medium of language. The younger are taught analysis and synthesis by their elders.

It is evident then that an image of a coming environment is due to the same nerve mechanisms that serve for the image of a past environment. All mental images are really memory images in kind.

[1] Pillsbury, *op. cit.*, p. 131.

In considering these brain processes, one should keep it in mind that the power to form an image diminishes with time. You perhaps have a clear mental image of a particularly fine cigar that was given you yesterday, but had it been a week ago you probably would have no image to speak of. The longer the time, the less is the conductivity of the association nerve paths and sooner or later the mechanisms will fail to coördinate so as to simulate the past environment.

The kinæsthetic impulses from actual and incipient muscular movements have much to do with provoking sounder responses. Acting, talking and thinking are all linked together by association mechanisms. Behavior habits and language habits serve to prevent the untimely occurrence of sounder responses as a rule. It is not always true, however, as a man can sometimes be reading aloud and at the same time, have mental images having no connection with the words which he is subconsciously repeating.

When a man is awake but resting quietly in an unchanging environment, he is usually unconscious of his environment or, as we say, he is lost in thought. From our point of view there are no mimetic responses in his brain centers and his nerve-muscle system is occupied with a series of incipient muscular movements accompanied frequently by a series of sounder responses in the brain centers. There is a constant flow back and forth of efferent and afferent (kinæsthetic) impulses which penetrate one cortical center after another. Those centers that have been tuned by past experience give a characteristic conditioned molecular response. It is thus that mental images transpire more commonly. The man may be thinking of future events but it is always in terms of what he has seen and heard before.

Having reached these conclusions regarding the mental operations of a man, it will not be amiss to consider briefly the mental processes of animals lower than man. Do monkeys, dogs and other intelligent animals have mental images? We know that the nervous system in these animals is much the same as in man and in behavior also there is much resemblance. The brute learns by experience as does the man. In man we find great superiority in expression as he has an articulate language which the brute has not. There is, however, communication with the brutes. The baying of a beagle on the trail of a rabbit, no doubt has in its changes more meaning to the other beagles than even to the experienced hunter. The available evidence indicates that the nervous mechanisms in the animal are of the same kind as those in man. Therefore it is probable that a squirrel has a mental image of a nut that he has hidden away for future use, the same as a man would have, and that a fox has a mental image of the bone that he buried and will one day dig up. A bird probably has a mental image of her nest that she built herself with the aid of her mate just as a man has of a hut he has constructed. In the man's brain the association mechanisms are far more complex. The number of association nerve paths is much greater.

In conclusion let us say that a mental image can only occur when there is a coördinated molecular response in a brain center that is conditioned by a former environment. The response is conditioned by means of registering mechanisms in the center and of association mechanisms. The response is the same in kind as if the environment in question were still acting upon the sense organs and so upon the center. The man is not conscious of an image as such but of the environment as it was. He sees it around the corner, one might say. Usually, of course, the response is very much fainter than in the case of direct stimulus from the sense organ in perception.

The registering mechanism at the brain center when in communication with the environment, acts by retransforming the nervous impulses into specific molecular changes that simulate the environmental action upon the sense organ. The result is a permanent set of the molecules that is a factor in subsequent responses to excitation by any nervous impulses that reach the center. The responses in the registering centers are further conditioned and also coördinated by mechanisms of association. These mechanisms function in the same way as they do in the case of conditioned reflexes which cause muscular movements. The same sort of association mechanisms govern the responses in the registering centers as govern the responses in the muscles and other effectors.

The knowledge that a highly intelligent man has of the world about him is due to the brain mechanisms that have gradually become organized and developed from his birth up. This applies also, of course, to a man's knowledge of himself.

Finally we cannot escape the conclusion that in a man's consciousness of past and future, brain mechanisms are an essential factor. Without brain mechanisms a man would have no more mental power and no more consciousness than an apple tree.

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THE PSYCHOLOGICAL REVIEW

THE MODIFICATION OF INSTINCT FROM THE STANDPOINT OF SOCIAL PSYCHOLOGY

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In the discussion which follows I have attempted a relatively comprehensive statement of the modification of instinct with particular reference to the topic as incorporated in social psychology. The formulations have been made with reference to a statement of general principles rather than with a view to a summary of such experimental data as might be available. I think most social psychologists approach the topic of instinct with the feeling that while it should be of great importance, from an *a priori* standpoint, yet that the actual developments which the topic receives fail somehow to justify the expectation. Social psychology is, of course, as interested in the experimental facts concerning instinct as is normal human adult psychology, but it seeks more insistently to put the data together in a manner significant for the understanding of human nature so far as this is modified by its social environs.

In section I., I have sought merely a reformulation (possibly more detailed than usual) of the modification of instinct on its afferent or efferent sides, or on both simultaneously. This section is preparatory for those that follow. In sections II. and III., types of instinctive modification are considered which are new to social psychology and all but unsuggested in the other fields of general psychology. These sections discuss (1) the fact and significance of the temporal position of the modifications as occurring before or after the first instinctive performance, and (2) the modification of the biological purposes of inherited responses.

I. Modifications of the Sensory and Motor Aspects of Instinct

Previous writers have attacked the question of the modification of instinct at three chief points: (*a*) indicating that an increase in perfection of response through practice does take place; (*b*) disentangling (partly) the separate rôles of maturation and use in the increasing perfection; and (*c*) pointing out that modifications concern either the stimulus or the response side of the instinct. It is this latter point that we wish to formulate in the present section.

Shortly after birth an individual will, through heredity, manifest the fear reaction upon the presentation of certain stimuli. By virtue of associations, these stimuli may later become ineffective and new stimuli be secured which were previously indifferent. Thus birds on desert islands show no fear of man until the frequency of his appearance, coupled with effective stimuli for fear, finally endows the perception of man himself with the capacity to arouse fear. Studies of the conditioned reflex are laboratory observations of this same phenomenon. The protective reflex of the finger, *e.g.*, has as its unconditioned (inherited) stimulus injury to the finger; but by a frequent simultaneous presentation of sound and injury, sound also becomes an effective stimulus producing withdrawal of the finger. The internal mechanism of this need not concern us in the present discussion. It should be stated, however, that habits as well as inherited forms of response are susceptible to this type of modification, the distinction being that we deal with conditioned reflexes directly when the changes effected are made from the original stimulus rather than from stimuli which in themselves may be one or more removes from the hereditary status of the response.

From the side of changes in effector activities proper, the same statements are true although the term conditioned reflex seems not to have attached to such modifications, undoubtedly due to the accident governing the choice of laboratory procedure. The protective reflex and the salivary reflex, *i.e.*, the effector activities proper, have been kept constant in such studies and experimentation directed toward the analysis of stimulus changes. However the physiological changes effected are presumably no different from those which occur in the contrary case when experience changes the type of response while the stimulus remains constant. The illustrations of this are legion. One may cite the changes which occur in the "expression" of fear and anger as the human individual matures in a social environment, or one may consider the modifications which occur in animal behavior during the process of learning. In the latter case, a total situation is presented to a white rat placed in a visual discrimination box, calling forth exploratory movements; but under the influence of punishment, reward, and frequency, the exploratory movements are inhibited and give way to well-defined food responses. One may state such an outcome either as the inhibition of an instinctive response to a given stimulus by acquired responses, or as the acquisition by the food-getting response of a new stimulus. Perhaps both are involved.

The social values of the above types of change in instinct have been so widely recognized that we need not elaborate the problem further. This is not true, however, in the case of those modifications termed sublimations. The sublimation of instinct in the human individual is an example of the simultaneous modification of the afferent and efferent phases of the response. Anger becomes righteous indignation by the substitution of a new and (in this case) an ideal stimulus for the sensory (animal) one and by the conversion of the gross bodily attack into the response of denunciation, purchasing Liberty Bonds, or longer hours of labor. Sex impulses may be sublimated in artistic activity, in dancing, in religious activity, or, when joined possibly with the parental impulse, in social service. Instances of sublimation are those where the inherited impulses are placed at the service of activities which bear little or no resemblance to the activity which normally embodies the impulse. The cases are not due to the suppression or elimination of the instinct in its entirety; only the somatic, skeletal responses are inhibited while the visceral continue probably at full intensity. The individual may entirely fail, and usually does fail, to identify the persistent behavior complex, because to the uninitiated, instincts are identified in terms of their somatic components. *It is this difficulty of identification which permits the sublimation to proceed unimpeded by emotional conflict, and unthreatened by the failure which would almost surely be its lot did the subject realize the origin of his impulses in their proper (unconditioned) instinct.*

Although the non-technical use of sublimate means to purify, or to idealize, the preceding analysis would indicate that the physiological mechanisms involved need not include the equivalent of ideals. The stimuli for artistic activities, for dancing, for charity and social service may be as concrete as for the arousal of any other form of modified instinctive performance. The presence of syncopated music and members of the opposite sex initiates dancing, and the awareness of suffering and poverty calls out charitable relief in those individuals possessing the sublimated behavior. And so, although one would hesitate to apply this term to animals below man, the understanding of instinctive modifications is better when one realizes the essential continuity of the phenomena. Thus a dog can by training be made angry by whistling, and the instinct can then be modified on its effector side by training the dog to vent his pugnacity in some unusual manner. Behavioristically, the instinct is as trully sublimated as in man, although the social significance of the change may be infinitesimal.

It is proper that we should place beside the above statements the following remarks by Woodworth[1]:

"Freud's 'sublimation' is an attractive concept. It is 'nice' to believe that crude motives, that cannot be allowed their natural outlet, can be drained off into other activities, so that a libidinous infatuation, sluiced out of its natural

[1] Woodworth, R. S., 'Dynamic Psychology.' New York: 1918, p. 175–6.

channel, can be made to drive the wheels of an artistic or humanitarian hobby. But there is no clear evidence that this can be accomplished. What does happen sometimes is that, in the effort to escape from, and distract oneself from a strong but unwelcome impulse, one turns to some other activity capable of enlisting interest; and, since the unwelcome impulse is not easily resisted, one has to become as absorbed as possible in this other activity. Under such conditions, interest in this other activity may grow into a strong motive force and effectually supplant the unwelcome impulse. But this is distinctly not making the unwelcome impulse do work foreign to its own tendency. This impulse is not drawn into service, it is resisted. If there were no other and contrary motive force, the impulse in question would have its own way. We did see that the tendency towards a 'consummatory reaction' acted as the drive to other mechanisms, but these were mechanisms that subserved the main tendency, whereas 'sublimation' would mean that the tendency toward a certain consummation could be made to drive mechanisms irrelevant or even contrary to itself. There seems to be really no evidence for this, and it probably is to be regarded as a distinctly wrong reading of the facts of motivation."

We must agree with Woodworth that compelling evidence of sublimation is difficult to secure. We do believe however that the psycho-analysts have made a good case for its existence; and when we remember the introspective difficulties besetting the identification of visceral components of response and of minor somatic responses in general, we are tempted to conclude that the case will always lack that clear-cut evidence which is desirable. However, the James-Lange theory of emotion meets the same type of difficulty and yet has managed to survive its severest critics because of the intrinsic merit of its claims. We shall indicate schematically in a following paragraph how the neural processes may proceed in sublimation; but here, in the light of Woodworth's remarks, renewed emphasis should be placed upon points already stressed.

1. Sublimations do not arise suddenly in an effort to control an unruly impulse that is recognized as undesirable;

they are the end-products of modifications whose formation has probably extended over several years. The behavior which may be said to undergo this modification may indeed never make its actual appearance, due to the fact—which we shall emphasize later in the paper—that certain habits or customs have been fixed upon the individual before the normal time for the instinct to appear. Therefore when the instinct manifests itself, it does so from the very beginning in modified form.

2. The visceral responses which constitute the physical basis of the impulse and emotion of the sublimated behavior can be identified by skilled introspection as closely similar to the visceral core (or "feel") of the unsublimated form of the response. Indeed this is a chief reason for insisting that such behavior as righteous indignation, *e.g.*, is a refined and derived form of animal anger. Or again, the alleged similarity of the emotional quales is a prominent reason for the insistent attempts to identify the sex and religious activities.

3. One need not assume, as Woodworth does, that in sublimated forms of behavior the "drive" does only work foreign to its natural purpose. On the contrary, an introspective description of the cases would suggest that, did we have adequate recording methods, widespread visceral and somatic responses would be found present at low intensity in contrast to the high intensity marking the untransformed behavior. What is important is that the behavior initiated by the sublimated impulse shall not impress the observer as a surviving (or anticipating) part of the original instinct. The uninitiated subject may only feel the restlessness due to visceral changes without recognizing in any degree the total response to which this restlessness normally belongs. He may therefore proceed to make use of this impulsive tendency in some socially acceptable behavior, the frequent repetition of which may constitute his idiosyncracy or even his profession. We shall see later that many instinctive impulses may be made to work out purposes other than those for which the instinct was apparently designed. In sublimation the situation is the same, a behavior component becomes trans-

ferred from one total response to another through the so-called conditioned reflex type of association and so does duty in the service of a purpose not originally its own.

A formulation in terms of the neural diagram of Fig. 1 may help give definiteness to the preceding account. In-

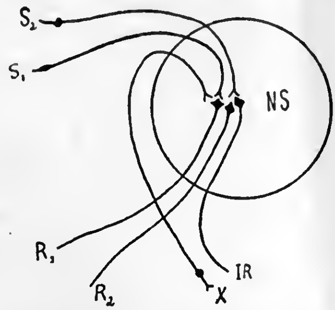

FIG. 1. Schematic representation of the neural elements involved in the modification of instinct. *NS*, the central nervous system. S_1, the original stimulus; S_2, an acquired stimulus; R_1, the original somatic response; R_2, a new or modified somatic response; X, the visceral sensory component of the stimulus; and IR, the internal, or visceral, response. The mutual relations of these elements are discussed in the body of the paper.

stincts belong to either of two classes: those having a conspicuous visceral component in the stimulus and those that do not. This visceral component corresponds on the physiological side to the appetite or desire prominent in food-getting and sex, *e.g.*, relatively absent in fear and anger, and totally absent in the simpler instincts (reflexes) of walking, standing, grasping, and even in such responses as collecting, curiosity, and others. This visceral component is represented in the figure by X. Normally the instinctive behavior R_1 is produced by the unconditioned stimulus S_1 acting alone or in conjunction with internal stimulus X. In many cases these afferent conditions also produce visceral effects, IR. Modifications of this original inherited equipment, so far as the elements of the neuro-physiological mechanisms are concerned, may be thought of in any one of the following ways, or in combinations of these: (1) S_2 acting alone or in conjunction with X may by use become an effective stimulus for the responses R_1 (somatic) and IR (visceral). The organism now fears some new object, has adopted some new article of diet, or (as is beautifully illustrated for animals below man in Craig's work with pigeons) has acquired some new sexual object. The internal appetite is still present, the responses of the skeletal muscles are unmodified, the visceral effects underlying the consciousness termed emotion are in full vigor, only the external stimulus has changed, although it may have changed to something which no longer suggests S_1 to the experiencing subject. (2) S_1, in the cases where by heredity the coöperation of X is necessary to give the afferent activity control of the final common path to R_1 and IR, may by use secure the power to arouse R_1 when X is absent. Here belong the cases where an instinct is aroused in the absence of the normal appetite or desire, jaded instincts, in a word. (3) Modification 2 may occur after S_2 has become the effective stimulus. (4) By use, or through the absence of the proper S, X may become so vigorous, so intense, so voluminous, that in the absence of an effective S, or even of any discoverable S, it may secure possession of the final common path to R_1 and IR. As examples we may cite: Breed's chicks, when they gave the drinking reaction in the air with no observable outside stimulus present; the case of a starving man or one perishing with thirst who swallows totally inadequate and normally non-effective stimuli; unreasoning, groundless fears; and finally cases of gluttony, alcoholism, and abnormal sex hunger. (5) S_1 or S_2 may by practice secure the power to arouse R_1 not only in the absence of X but without involving any noticeable visceral changes, IR. This is the instinctive behavior devoid not only of normal appetite but of the normal emotional satisfaction which accompanies its exercise. Again the most striking illustrations come from the field of food and sex responses. (6) The modification of the instinct may proceed with S_1 and X unchanged but with the response shifted from R_1 to R_2,—or from a clumsy and unskilled R_1 to an efficient performance of the same response (as, *e.g.*, in Breed's experiments). Again it should be noted that R_2 may be so different from R_1 that an observer not knowing the genetic facts would be unable to detect a relationship between the two activities. (7) The final case of modification occurs when the effects of practice, or use, have substituted $S_2—R_2$ for the original behavior with or without abnormality in X and IR. These are the typical cases of sublimation; and, if X and IR are unmodified, they are the cases where the desires and emotions (satisfactions) of one original response are put at the service of, or incorporated into, derived forms of behavior. Stated in this manner and placed in relation to other forms of stimulus and response changes, sublimation loses any mystical character it may have been thought to include and stands forth as a peculiarly important type of the modification of instinct.

II. The Temporal Position of the Modifications

So far our analysis has concerned those phases of instinctive modification which can be formulated in terms of change in the elements of the stimulus-response situation. Two other problems now remain to be emphasized, problems which although of fundamental importance in the modification and control of behavior are unnoticed in the social psychologies, and are at the best treated only by implication by the technical students of instinct. These problems are: (1) The temporal position of the modification, whether coming prior or subsequent to the first instinctive performance; and (2) modifications of the biological purpose, or end, involved in the inherited behavior.

The modifications of instinctive performance are not all variations (of the stimulus, of the somatic response, or of the visceral response) produced after the instinct first appears. Instances which do belong here we have already illustrated. Other modifications occur because of influences at work before the instinct makes its initial appearance. These changes will clearly affect the instincts in proportion to the length of the interval between birth and the instinct's appearance and in proportion to the social value inherent in a modification of the instinct in question. The dates and order of the appearance of the various instincts are sufficiently known to serve our present general purpose. Figure 2 indicates for man the early appearance of the responses of feeding, fear, anger, and vocalization, the final appearance of the sex and parental responses, and the intermediate appearance of such responses as play, acquisition, locomotion, construction, etc. We do not mean to imply by the use of this diagram any more

Fig. 2. Diagram indicating: the early appearance of the instincts of feeding, fear, anger, and vocalization; the intermediate appearance of such instinctive responses as play, acquisition, locomotion, etc.; and the late appearance of sex and parental behavior. The curves for each instinct suggest the appearance of component elements before the complete instinct matures and is active. No emphasis is to be placed either upon the relative order of the intermediate instincts or upon the form and length of the several curves.

than concerns our immediate purpose. The instincts and instinctive tendencies in man are as a rule too indefinite in their manifestations to enable a very satisfactory listing, and the question of their temporal order of appearance is one calling for much additional experimental work. Particularly is it important for the problem of the modification of instinct that the early traces be noted of instincts which appear late.

Our very simple diagram (Fig. 2) enables us to visualize clearly the possibility of the temporal aspects of the modifications above mentioned. It also serves to suggest that the instincts which will be most open to change by virtue of preëxperience will be the ones listed farthest to the right. Feeding, fear, and anger, *e.g.*, appearing as they do practically at birth, offer no other possibility than modifications subsequent to their appearance; while the temporal interval antedating sex, *e.g.*, makes possible the acquisition of many responses which will serve to modify (and control) that instinct.[1]

Although our chief interest does not lie in the historical aspects of our subject, it will be worth while to indicate the types of comment and experiment already available in the literature so far as they may concern the modification of instinct by preëxperience. We derive the first suggestion from the writings of Karl Groos on play (1895) where the following statements occur:[2]

". . . there can be no doubt that instinct plays a part in all this adaptation for the struggle for life and preservation of the species, so necessary in man and other animals. Further . . . it would be entirely in harmony with other phenomena of heredity if we found that these instincts appear at that period of life when they are first seriously needed. Just as many physical peculiarities which are of use in the struggle for the female only develop when the animal needs them; just as many instincts that belong to reproduction first appear at maturity; so the instinct of hostility might first spring up in the same manner only when there is real need for it; and so it might be supposed with other instincts in connection with related activities. The instinct for flight would only be awakened by real danger, and that of hunting only when the animal's parents no longer nourished it, and so on. In this case it would be necessary for the special instincts to be elaborated to their last and finest details. For if they were only imperfectly prepared, and therefore insufficient for the real end, the animal might as well enter on his struggle for life totally unprepared. . . .

"Without play practice it would be absolutely indispensible that instinct should be very completely developed, in order that the acts described might be accurately performed

[1] In what follows we shall have much to say concerning sex behavior, but this must in no way be interpreted as an adoption of the Freudian point of view that sex is the dominant instinct. Our emphasis upon this response results because (*a*) of the strength of its impulse, (*b*) of the lateness of its appearance, and (*c*) of the fact that it assumes a more definite instinctive form than other late instincts.

[2] Groos, Karl, 'The Play of Animals.' Eng. trans. by Baldwin. New York: 1898, pp. 73–4; 79.

by inherited mechanism, as is also the case with such instinctive acts as are exhibited but once in a life time."

". . . instincts are not so perfectly developed, not so stamped in all their details on the brain, as they would have to be if their first expressions were to be serious acts. Therefore they appear in youth, and must be perfected during that period by constant practice."

One need not accept Groos's theory of play in order to admit the essentials of the above quotations. There is a playful exercise of those elements of an instinct which appear prior to the complete appearance of the inherited behavior, and this exercise does, by the law of habit, have an effect upon subsequent arousals.

Lloyd Morgan, writing in 1900, speaks much more definitely on our present problem:[1]

"Even in the case of the very first exhibition of such a deferred instinct as the moor-hen's dive, although that organized sequence of acts which constituted the behavior as a whole had never before occurred, although there was no gradual learning how to dip beneath the surface, and to swim under water, still many of the constituent acts had been often repeated; experience had already been gained of much of the detail then for the first time combined in an instinctive sequence. So that if we distinguish between instinct as congenital and habit as acquired, we must not lose sight of the fact that there is continual interaction, in a great number of cases, between instinct and habit, and that the first performance of a deferred instinct may be carried out in close and inextricable association with the habits which, at the period of life in question, have already been acquired."

This point of view Morgan continues to discuss down to 1912,[2] without, however, so far as I am aware, commenting upon its social significance or attempting any general analysis of the problem. I can find no discussion of this temporal aspect of the modification in the current textbooks of psy-

[1] Morgan, C. Lloyd, 'Animal Behavior.' London: 1900, p. 106.

[2] Morgan, C. Lloyd, 'Instinct and Experience.' New York: 1912, chs. 1 and 2.

chology, nor in the social psychologies[1] and more special treatises on instinct and behavior. Undoubtedly in the observational literature on instincts many instances could be unearthed. We shall cite but one, perhaps the best one, however, and then proceed with our comments on the general problem.

This illustration is drawn from the work of C. O. Whitman on pigeons, and is as follows:[2]

"If a bird of one species is hatched and reared by a wholly different species, it is very apt when fully grown to prefer to mate with the species under which it has been reared. For example, a male passenger-pigeon that was reared with ring-doves and had remained with that species was ever ready, when fully grown, to mate with any ring-dove, but could never be induced to mate with one of his own species. I kept him away from ring-doves a whole season, in order to see what could be accomplished in the way of getting him mated finally with his own species, but he would never make any advances to the females, and whenever a ring-dove was seen or heard in the yard he was at once attentive."

("It may be remarked by the editor that the discovery of this principle furnishes the key to Professor Whitman's success in hybridizing the various species of pigeons. A novel and important principle of behavior is here involved. The range of stimuli to which an instinctive tendency will respond may be modified by habits acquired long before the first expression of the instinct. The first expression of a delayed instinctive tendency may thus be in part a function of all that the organism has previously acquired.")

While we cannot agree with Carr that the principle of behavior involved in Whitman's work is novel, yet we must agree that it is important far beyond any recognition yet

[1] Baldwin skirts the edge of the problem in his account of social heredity as developed in "Social and Ethical Interpretations," New York: 1906, 4th edition, Pt. 1, ch. 2; but he seems not to have stated explicitly that social heredity may get in its work on the individual before the instinct (physical heredity) has appeared in that individual.

[2] Whitman, C. O., 'Orthogenetic Evolution in Pigeons,' Vol. 3, 'The Behavior of Pigeons.' Edited by Harvey Carr. *Carneg. Inst. Washington, Publ.* No. 257, 1919, p. 28. I have added Carr's editorial comments in parenthesis.

given it. This modification of instinctive behavior by experience encountered prior to the first appearance of the inherited response may be conceived in any of the following ways: (1) The early appearance of component elements of the final total behavior (as indicated by the curves of Fig. 2) may involve their own modificaton on the basis of use in such a manner that when the total instinctive response appears it does so in a manner not entirely determined by heredity. This modification may be either on the stimulus or on the motor side of the early appearing tendencies, and is in this respect a case under section I. of this paper. This would be the type of case covered by Groos's theory of play and by Morgan's description above quoted. (2) Perhaps independently of any early component tendencies of later instincts, the individual may be instructed in the nature of the socially accepted stimuli and forms of response so that when occasion arises he will respond in the socially accepted manner.[1] So thorough and far reaching may these modifications be, that the individual may never know the animal form of the instinct; and yet we must believe that this persists, in the form perhaps of synaptic connections, because something very like it appears when the bonds of social restraint are relaxed.[2]

The responses of feeding, fear, and anger, as we have said, appear too early in the individual's life for this general type of modification; but such responses as play, constructiveness, sex, display, and the parental instinct, occurring later, offer the individual and the social group an opportunity to determine prior to the onset of the behavior the stimuli which

[1] This instruction which precedes the maturing of relevant interests is undoubtedly very uneconomical from the standpoint of the laws of learning; but the vital problem is not the speed and efficiency of the acquisition, it is rather the mere fact of acquisition, the importance of building up controls while the organism is yet young.

[2] It seems hardly believable, in view of this last fact, that any psychologist should deny that man possesses true instincts. Present-day society has so modified the individual and his environment, that the individual seldom experiences the sheer animal form of the response—indeed some may never do so. However, occasionally in moments of great stress, the individual is literally swept off his feet by a gust of animal-like passion. Perhaps once or twice I have approached such an experience. My own testimony would be that in such a case one is for the moment an all but unconscious automaton.

shall ordinarily arouse it and the form which it shall take. Play activities vary in their content in dependence upon the social environment, as do constructiveness and sex also. Long prior to the maturing of the latter instinct and even longer before its usual manifestation, society has set before the individual a pattern which, like the Great Stone Face of Hawthorne's tale, shall serve more or less unconsciously to instruct and guide him in the accepted stimuli and responses of that behavior. Religious training likewise can, and does in many cases, take the young individual and so shape his religious symbols and responses that when religious activities do appear definitely in adolescence, it shall seem but natural to turn to one sect or one religion for their gratification. Society in this type of modification is giving the individual the benefit of its own experience, not by permitting the instinct to manifest itself in the crude animal form and then modifying it, but by building up the proper controls prior to the emergency.

III. Modifications of the Biological Purpose

The final problem which we have set ourselves is now at hand. Modifications of the instincts are not only of the sorts which have been outlined above, but they may involve essential changes in the biological purpose of the response. By the biological purpose of an instinct, I mean the adaptive purpose which it secures or tends to secure. Thus the biological end in view in the case of fear is the removal of the organism from the dangerous stimulus; in the case of anger, it is the injury of the offending object; in sex, it is the reproduction of the species, etc. There is, as I understand, no dispute on this point, viz., that instincts are adaptive forms of response. This statement carries no implication that the purpose is a conscious one or that it has been instrumental in molding the behavior. The statement is a straightforward, scientific, objective formulation, implying nothing of vitalism or of other speculative interpretations of the place of purpose in nature.

Inasmuch as the animals below man give as yet no evidence of possessing the behavior equivalent of thought processes, it is a probable assumption that they never possess an awareness of the purpose of their acts. At some time in the evolution of man, therefore, the consciousness of the purpose served by his responses has appeared. At first, undoubtedly, only the more obvious purposes have been grasped, such as those found in the protective reflexes, in hunting, in display, and in parental behavior. Particularly in the case of the sex instinct there is reason to believe that the race has only recently, *i.e.*, recently as one estimates time in terms of man's existence on the earth, discovered the connection between the sex instinct and reproduction. By way of illustration, we may quote from Spencer and Gillen's account of the tribes of Central Australia. Writing in 1899, they say:[1]

". . . we have found amongst the Arunta, Luritcha, and Ilpira tribes, and probably also amongst others such as the Warramunga, the idea firmly held that the child is not the direct result of intercourse, that it may come without this, which merely, as it were, prepares the mother for the reception and birth also of an already formed spirit child who inhabits one of the local totem centers. Time after time we have questioned them on this point, and always received the reply that the child was not the direct result of intercourse."

Writing again in 1904, they say:[2]

"Indeed Mr. Roth's latest work in Queensland shows clearly that the idea of spirit children entering women, and that sexual intercourse has nothing to do with procreation, is a very widespread belief amongst the Australian aborigines, and is by no means confined to the tribes amongst whom its existence was first described by us" (p. xiii).

"The cermonies [of initiation] can never have had any reference directly to procreation, for the simple reason that the natives, one and all in these tribes, believe that the child

[1] Spencer, B. and Gillen, F. J., 'Native Tribes of Central Australia.' New York: 1899, p. 265.

[2] Spencer, B. and Gillen, F. J., 'Northern Tribes of Central Australia.' New York: 1904, Pp. xiii and 330–331.

is the direct result of the entrance into the mother of an ancestral spirit individual. They have no idea of procreation as being directly associated with sexual intercourse, and firmly believe that children can be born without this taking place. There are, for example, in the Arunta country certain stones which are supposed to be charged with spirit children who can, by magic, be made to enter the bodies of women, or will do so of their own accord. Again, in the Warramunga tribe, the women are very careful not to strike the trunks of certain trees with an axe, because the blow might cause spirit children to emanate from them and enter their bodies. They imagine that the spirit is very minute,—about the size of a small grain of sand,—and that it enters the woman through the navel and grows within her into the child."

In all cases a definite and accurate formulation of the adaptive value of the behavior has waited upon a clear perception of cause and effect relations among objects and events, which in many cases means waiting upon scientific analysis. Until the individual and society know the biological purposes of instincts, only accident can identify the purposes which society approves and fosters with those which heredity is seeking. But once this knowledge is forthcoming, society and the individual may proceed consciously and definitely to foster the purpose, or they may change the environment in such a way that the biological purpose can give way to a new purpose, or, finally, the biological purposes may be satisfied incidentally so far as the conscious plans of the individuals are concerned.

Nor should the present type of instinctive modification be confused with the voluntary exercise of a response that may at times be automatic and inherited. Such a case would occur when one winks voluntarily at a joke, and so might apparently be said to have modified the biological purpose of protection normally subserved by this response. In order to subsume the winking response under this third type of modification, the winking would have to be produced by the individual's voluntarily placing himself in front of a stimulus which would automatically bring about the response and then for a social purpose which might or might not be the same as the biological one. Perhaps in the last analysis so-called voluntary activity is precisely of this nature, consisting of a highly elaborated conditioned reflex whose stimulus is an idea. But for the purposes of the present discussion there is an active participation and a feeling of control in voluntary activity which contrasts strikingly with the automatic, impulsive, compelling characteristic of the instinctive response (characteristics which are as definitely present when the instinct is "used" for social purposes as they are when it accomplishes purely biological ones).

The two great modifications which have been made in biological purposes appear to be these: (1) purposes which are inimical to civilized social life are supplanted by new and more acceptable ones; and (2) the biological purposes in all of the more powerful instincts are occasionally or habitually secondary to the use of the instinctive behavior as a pleasure giver. To be sure, in so far as the original synaptic connections persist—and it is my opinion that they are rarely if ever lost—the original biological end of the behavior will tend to remain and be satisfied, although perhaps only surreptitiously.

Table I has been drawn up with reference to the two types of cases suggested above. Here an attempt is made to state the biological purposes subserved by certain of the instincts and to place over against these the recognized social purposes which usually or occasionally dominate them. In some cases the two will be identical, due at times to accident and at times to foresight. The principle involved in this third type of modification of instinct is not dependent for its validity upon the accuracy of the analysis of Table I.; it is dependent rather upon the fact of variation between the two types of purpose whose detailed nature is there suggested.

There are certain features of Table I. which invite definite comment. In column three I have placed only what have seemed to be social purposes that are widely recognized in social practice. No attempt has been made to indicate

TABLE I

Instinct	Biological Purpose	Social Purpose Definitely Fostered
Anger	Defense of organism by removing offending object.	*Used in hostility and competition to stimulate great endeavor. Put at service of customs.
Fear	Defense of organism by removing it from offending environment.	*Used for taboos in maintenance of social organization.
Acquisitiveness	Accumulation of food and nest supplies.	*Accumulation of objects possessing general value or power to satisfy human wants. Fostering prestige.
Vocalization	Stimulation of certain instincts and habits in associates.	*Communication of ideas; stimulation of any instinct or habit in *self* or others.
Hunting	Securing of food and mates.	*Recreation, health, and prestige.
Rivalry	Domination, particularly in sex and play activities.	*Domination in all fields of activity.
Feeding	Nourishment.	*Nourishment, pleasure, and social solidarity.
Sex	Reproduction.	Pleasure, and reproduction. Begetting of offspring in order that parents may be cared for in sickness and old age.
Parental	Protection of young.	*Protection of young.
Display	Sex excitant, arousal of fear in others.	*Sex excitant, arousal of fear in others, prestige, creation of caste.
Religious	Protection from "Great Danger."	Protection from "Great Danger," protection of morals, social service.

The * indicates that biological purpose is not specifically combated.

Present occidental society fosters all instincts in some degree for health and pleasure as well as for the social purposes above enumerated.

the vast multiplicity of purposes for which the instincts may on occasion be used. With the appearance in man of ideational processes and ideational methods of behavior control, it has become possible to use the instincts not for their biological ends alone but for almost any end that the manipulator may have in mind. The demagogue and the propagandist by placing certain stimuli before the crowd may utilize the resultant fears, angers, acquisitivenesses, or religious activities to satisfy ulterior purposes of much or little merit. This is a matter of great social importance, but what we have indicated in the table differs in at least two vital ways from the uses of instinct made by the individual social manipulator. In the first place, the social purposes or utilities there listed are definitely sanctioned by present occidental society; and in the second place, the individual in whom the instinct manifests itself may be, and usually is, well aware of the social purpose to be attained, inasmuch as much social or group effort is directed toward instructing him on this very point.

So far as our analysis can reveal, the social purposes permit the accomplishment in a more or less incidental manner of the biological purposes without any attempt to combat these purposes save in the case of the sex instinct and the religious tendency. In the hunting instinct, *e.g.*, the purposes of recreation, good health, and prestige are not incompatible with the food- and mate-getting end; nor does society repress the latter. The occasions on which the instinct appears may be limited by law, but when it does appear the biological end to be attained is laudable. This is true also in the cases of fear, anger, and the other responses whose social purposes are indicated with an asterisk. Society definitely favors the use of display (in clothing and physical prowess) as a sex excitant as well as an enhancer of prestige and a creator of class distinction. The original form of the stimulus and response is usually modified, and sublimated instincts may have been added to the complex, but when the instinct appears its biological purpose is approved. In the case of the religious tendency, on the other hand, society is tending to negate the biological purpose of protection from *great danger* or the *mysterious threat* (or however one may formulate the unseen characteristic of objects with which primitive man seeks to get *en rapport* through definite religion and magic).[1] In its place it is putting social service and the maintenance of moral conduct as the proper goal of the religious impulse. The change is not that of stimulus and response or of the accretion of other instinctive impulses alone, nor is it a limitation of the

[1] I do not know, of course, that this is the biological purpose, nor am I certain that the religious tendency is instinctive. The response is, however, coextensive with social groups, and the apparent purposes subserved at the lowest level are here stated as biological.

occasions upon which the impulse may manifest itself. This is not to say that the use of the religious tendency as a defense mechanism against the imperfections of the present does not receive great social sanction; it is to emphasize that much of the time, and in some groups most of the time, when the behavior appears, its biological purpose is combated.

Before extending our comments to include the sex instinct, we may best return and take up the thread of our argument as left on page 264 where it was stated that the second fundamental manner in which biological purposes are modified is the use of the instinct as a pleasure giver. It should be noted that with all instincts (not merely with that of sex) there is a pleasure and satisfaction in the experiencing of inherited muscular and glandular activity where the experiencer is free to turn his attention to the response as opposed to the stimulus. In the arousal of the instinct under conditions that realize or tend to realize the biological (and certain social) purposes of the response, the attention of the individual is definitely focused upon the stimulus which initiates and controls his behavior. Thus in a fire where the individual is in danger, it is not the emotional thrill which is in the focus of consciousness but the dangerous aspects of the situation. The bystanders, on the other hand, who have congregated, can enjoy the thrills of fear aroused by the fire because in the background of consciousness is the understanding that, so far as they are involved, it is all make-believe. It is beyond our intention to offer an explanation for this enjoyment of inherited forms of response under the conditions described; it is enough to indicate the fact and its implications for the modification of instinct. Within the limits of the apparently harmless, society sanctions the arousal of instincts for purposes of pleasure. Forms of art vie one with another in subtle stimulations of the instincts, while in the fringe of the beholder's consciousness the feeling of make-believe permits him to enjoy the resultant behavior. The individual confronts himself upon the stage and the screen with stimuli for all of the instincts—fear, anger, hunting, acquisitiveness, religion, sex, etc.—and then enjoys the result much as a child in play will pretend the existence of hobgobblins in order to enjoy the thrill of fear, or wiggle a sore tooth or finger for the pleasure of the resultant pain.[1]

It so happens that the sex instinct is through heredity accompanied by a greater pleasure than pertains to the exercise of any other instinct, and it is therefore not unexexpected that the history of the modifications of this instinct should be peculiar. In the animals below man, where there is no awareness of the biological end, the instinct functions solely for reproduction. No social purpose exists. The use of sex for pleasure, so far as I know, has its first beginnings among the monkeys, although here the probable absence of thought processes would count against its conscious use for that purpose. Moreover, among primitive men and even among peoples as advanced as the Central Australian natives, the biological purpose of reproduction is unknown (undoubtedly because of the great temporal interval between the activity of the instinct and the birth of the offspring), and yet there is sufficient development to insure the presence of definite social purposes. The result is that the sex instinct is recognized by society as a type of behavior whose purpose is the production of pleasure. Women are property, and the violation of chastity is the violation of a property right.

With the development of man to the point where the biological purpose of sex is understood, comes the possibility that society and the individual may definitely sanction the biological purpose. This they have done. Certain individuals and certain groups have maintained that the only conscious purpose to be sanctioned is the biological one; and yet in practice society at the present time sanctions the modification of this instinctive behavior by utilizing it in the ancient manner as a pleasure mechanism. This it does through emphasis upon birth control and the make-believe stimulation of the instinct on the stage and in certain phases of art in general.

[1] G. T. W. Patrick has made extensive use of the pleasurable aspects of instinctive activities as they appear in playful form. See his 'Psychology of Relaxation.' New York: 1916.

In the case of the food-getting instinct society and the individual do not at present combat the biological purpose, although they do relegate it to the background and satisfy it incidentally in many cases. In instances of perversion, however, the nutritive purpose has been definitely combated. Thus it is said that the Roman voluptuaries practised artificial vomiting in order that their banquets might proceed unhindered by the limited capacity of the individual. While our own banquets lack this interesting feature, nevertheless they are conducted for pleasure and not for the purposes of nutrition. Custom has from time immemorial recognized the effect on social solidarity of "breaking bread" together, utilizing the pleasurable aspects of feeding in the creation of consciousness of kind. This and similar uses of instinct to satisfy social rather than biological purposes is fundamental in understanding social phenomena.

IV

Summary.—The social significance of instinct cannot be brought out by analyses of the nature of specific forms of response, but must come largely from a consideration of the types of modification that instinctive forms of behavior undergo. These variations come fundamentally from the influence of habits and other forms of intelligent behavior. The present paper has elaborated the topic with reference to the following points: (1) Modifications of the structural elements, including (*a*) changes of the stimulus in its external or internal aspect, (*b*) changes of the somatic or of the visceral response, and (*c*) combinations of these in sublimated behavior; (2) the temporal position of the modification as it occurs before or after the initial appearance of the instinct; and (3) modifications of the biological purpose or adaptive value of the response.

THE NATURE OF THE RHYTHM EXPERIENCE

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Recent years have witnessed a marked decrease in the number of contributions to the experimental psychology of rhythm. Dunlap, writing in 1916, remarks, "It is a significant fact that experimental work on the perception of time and of rhythm has nearly ceased. One research on time, a statistical study of speech rhythm and a minor study on rhythm effects are all that have appeared in the last two years" (**7**, p. 206). With the exceptions of one or two studies, this statement is still applicable; in fact, the subject of time and rhythm has been dropped from the reviews in alternate years.

The reason for the discontinuance of scientific work in this field is not a knowledge of all the facts about rhythm, for such is not the case, but the lack of a working hypothesis for the nature of the rhythmic experience. The tentative bases which have existed up to this time have been given up, for the greater part, as the result of a tendency to eliminate time as an element of rhythm perception, and of the recognition of rhythm in others than the auditory field. Although these factors are the necessary result of each other, it is not until recently that they have been recognized, and that more than one aspect of the rhythm experience has become subject to analysis.

The earliest introspective and empirical studies,[1] and the modern theories of literary scholars, although characteris-

[1] Brücke, 'Die physiologischen Grundlagen der neuhochdeutschen Verskunst', 1871 (5).

Riemann, 'Katechismus der Musik,' 1888, p. 1 (36).

Lobe, 'Katechismus der Musik,' 25 Aufl., 1893, p. 4 (20).

Sully, 'The Human Mind,' Vol. 1, 1892, p. 271 (54).

Lanier, 'The Science of English Verse,' 1880, p. 62 (19).

Gurney, 'The Power of Sound,' 1880, p. 127 (12).

tically vague,[1] have not seriously questioned the necessity of an absolute regularity in the recurrence of the objective elements. The view represented and assumed by them, has given way in the case of the former, under the influence of studies pursued under experimental conditions, until in 1903 MacDougall wrote: "There is properly no repetition of identical sequences in rhythm. Practically no rhythm to which the æsthetic subject gives expression, or which he apprehends in a series of stimulations, is constituted of the unvaried repetition of a single elementary form" (**22**, p. 319).

Under experimental conditions, rhythm was also removed from the field of objective stimulation,[2] to the field of subjective perception,[3] and finally to that of motor experience in time.[4] There has likewise been a tendency to limit the definition of rhythm greatly, from the cosmic recurrences of the universe (**3**, p. 146), to the field of human experience (**38**, p. 305), and here to the voluntary as opposed to the organic rhythms (**27**, p. 3).

Although most of the experiments with the voluntary

[1] Guest, 'A History of English Versification,' 1882, p. 1 (10).
Schipper, 'History of English Versification,' 1910, p. 3 (42).
Gummere, 'A Handbook of Poetics,' 1885, p. 134 (11).
Alden, 'An Introduction to Poetry,' 1909, p. 156 (1).
Mayor, 'Chapters on English Meter,' 1901, p. 4 (24).
Omond, 'A Study of Meter,' 1903, p. 2 (30).
Saintsbury, 'Historical Manual of English Prosody,' 1910, p. 291 (40).
Matthews, 'A Study of Versification,' 1911, p. 12 (23).

[2] Köstlin, 'Aesthetik,' 1869, p. 90 (17).
Riemann, 'Elemente der musikalischen Aesthetik,' 1900, p. 135 (37).
Cf. Wundt, 'Grundzüge der physiologischen Psychologie,' III., 1903, p. 158 (64).

[3] Squire, 'A Genetic Study of Rhythm,' A. J. Psy., XII., 1901, p. 586 (50).
Meumann, 'Untersuchungen zur Psychologie und Aesthetik des Rhythmus,' *Phil. Stud.*, X., 1894, pp. 272, 304 (25).

[4] Miner, 'Motor, Visual and Applied Rhythms,' Psy. Rev. Monog., V., 1903 (27).
MacDougall, R., 'The Relation of Auditory Rhythm to Nervous Discharge,' Psy. Rev., IX., 1902, p. 466 (21).
Stetson, 'A Motor Theory of Rhythm and Discrete Succession,' Psy. Rev., XII., 1905, p. 258 (52).
Ruckmich, 'The Rôle of Kinæsthesis in the Perception of Rhythm,' *A. J. Psy.*, XXIV., 1913, p. 305 (38).
Weld, 'An Experimental Study of Musical Enjoyment,' *A. J. Psy.*, XXIII., 1912, p. 298 (59).
Patterson, 'The Rhythm of Prose,' 1916 (31).

rhythms were in the auditory field because of the comparative ease of experimentation, it is a curious fact that writers of general accounts and text-books consider rhythm possible only in certain fields, whereas special investigators are confident that it is theoretically possible in every field. Thus Miner says: "There would be no reason *a priori* why a series of stimuli addressed to any one sense should not produce an experience of rhythm. I am quite confident that they would under proper circumstances; that rhythms of smell, taste, touch or vision are just as possible as rhythms of hearing" (**27**, p. 40). So Meumann, with temporal qualifications (**25**, p. 261); and according to Wundt, 'no series of impressions exists that cannot in some way be comprehended as rhythmic' (**64**, p. 62). Ruckmich asks whether a rhythm cannot be induced which shall be perceived principally in terms of those sensations that correspond directly to the nature of the stimuli given (**39**, p. 247); and Woodrow: "To produce an impression of rhythm, it is necessary to have a series of stimuli," enumerating some (**60**, p. 5); and Dunlap: "The facts seem to be that all sorts of sensations lend themselves to serial grouping" (**6**, p. 350).

Such pervasiveness of rhythm, however, is not understood by writers of general accounts and elementary texts. For them, rhythm is limited to definite fields of sensation, to the auditory, kinæsthetic, tactual and visual fields (**34**, p. 329); to the first three (**29**, p. 301); to the first two (**8**, p. 484); to the first (**18**, p. 389); and according to Titchener, there is rhythm in one and only one field, the kinæsthetic (**56**, p. 345). From this, it can be seen that the special investigators did not hesitate to generalize from their limited results, but that the general accounts were more cautious, and did not theorize. Among neither group, however, is there agreement. The result has been a condition chaotic in the extreme. In 1917 Ruckmich wrote: "Much experimentation has been done in the last two and a half decades on the general subject of rhythm. Theories have almost equalled these attempts in number, and difficulties have arisen out of all proportion to the facts discovered" (**39**, p. 326).

The Rhythm of Prose

In the latter part of 1916, however, appeared a new and important contribution to the rhythm of prose (**31**), in which it is stated unequivocally that a 'new standard' is established 'for passing judgment upon the rhythm of a sentence or paragraph' (p. 13). Although the title refers to the rhythm of prose, the book is concerned with the nature of rhythm in general.

From the example of syncopating rhythms of Indian music, where there are two levels of rhythmic stimulation, often in different times, as a melody in three-time against a tom-tom accompaniment in four-time (p. xx), it is suggested as the 'new standard,' that all rhythm is composed of two levels, an objective level and a subjective temporal measuring scale of 'unitary pulses,' 'elastic' in their nature (p. xx). "The 'boom! boom! boom!' of subjective time-units, such as rattle along in the consciousness of an aggressively rhythmic person, may be accelerated or retarded, within certain limits defined for each such individual, without destroying their value as a subjective foot-rule with which to correlate all experience" (p. 47).

The origin of the subjective time-unit may be the breathing rate. "Segments of breath-waves, each segment marked by a slight reinforcement in the flow of air, and measured, in turn, by so many concomitant heart-beats—when these are consciously felt—may easily register for us our mental seconds. It is only by such mental time-beats or 'unitary pulses' that we are able to make anything like accurate judgments of time. Suppressed articulation usually assists us in counting; our memory images record the number" (**32**, p. 259). Or the origin of the subjective time-unit may be the walking rate, and the memory of it, the basis of measuring. 'Perhaps, as each step is taken, the concomitant tension of some obscure muscle of the head occurs, which thus furnishes the means of repeating the walking-rate, without carrying the innervation as far as the legs' (**31**, p. 65).

Both levels, that of the objective stimulation, and that of the subjective time-units are characterized by (1) acceleration and retarding (pp. 3, 47), (2) substitution of one long time-interval for several equivalent short ones, or vice versa (p. 3), and (3) syncopation, by which is meant the correlation of two sets of time-intervals, concomitant but not coincident (p. 4), as in the case of the Indian music mentioned above. "The impression which results from the combination in consciousness of the auditory (syllabic) sensations (including their effect upon attention) and the subjective time-units, may be compared to a melody and its accompaniment, with attention focussed, not upon the pitch relations so much as upon the relations of time and stress" (p. 69). The 'new standard' makes rhythm a temporal affair in that the subjective 'unitary pulses' measure time. "The ultimate basis of all rhythmic experience, however, is the same. To be clear-cut, it must rest upon a series of definite temporal units" (p. xxii).

Visual and Auditory Rhythm

Almost side by side with this study appeared a new contribution by Ruckmich in the field of visual rhythm (**39**, p. 231). Miner in 1903 found that "the experience of rhythm in the field of vision is identical in its essentials with that in the auditory field. Since the experience is novel, it is at first more vague than with sounds, but it becomes quite precise with practice" (**27**, p. 71). Koffka has concluded that "no essential difference between auditory and visual rhythm has shown itself," that 'series of visual imagery can be the sole associates of the experience of rhythm,' that 'for rhythmical experiences visual and auditory images are equivalent throughout' (**16**, pp. 96–97).

From these and other considerations of visual rhythm (**39**, p. 232f.), it seems clear that visual rhythm cannot be placed in a category different than that of the auditory. "All of the special studies which have been undertaken in this field are at one in pointing out that visual rhythm does not differ essentially from any other kind of rhythm" (p. 236). Nevertheless, Ruckmich has found that a purely visual rhythm can be experienced as such (p. 247), that "it is possible to obtain rhythmical perceptions from stimuli that are visually presented and that differ objectively only in color quality. It is furthermore possible to obtain from such stimuli experiences of rhythm which are visual in their very essence, *i.e.*, in which no other processes play an important part" (p. 253).

Time and Rhythm

Although it may not appear so at first sight, there is a distinct conflict in the field of rhythm between the results of Patterson and Ruckmich. It is in the application of the 'new standard' to visual rhythm that difficulty arises. It is not clear the part that 'unitary pulses of subjective time' whose rate is 'about .7 sec.' (**31**, p. 67), would play in a rhythm induced in terms of stimuli that are not auditory, but visual. Visual rhythm can result from stimuli in terms of difference of color quality (**39**, p. 253), of difference of intensity of the members (**38**, p. 356), and what is of greatest importance, in terms of spatial structure,[1] as well as from the duration (**27**, p. 71), and temporal arrangement of the members (**16**, p. 104). Because of the nature of rhythmic stimulation, time is an element but it is ancillary to the rhythm. It is a prominent item in auditory rhythm, since, because of the nature of the end-organ, only series of discrete stimuli can be presented. Visual rhythm, likewise, is temporal in the aspect of serial stimulation, in that, since two stimuli cannot occupy the same place at once, one must follow the other in a time sequence.

While, therefore, it is true that recurrence is present in rhythm, and takes place in time, it does not follow that the perception of rhythm is due to a measurement of the recurrence by a 'subjective foot-rule.' Recurrence measured by time as an integral part of rhythm involves a confusion of one of the physical factors of rhythm with the nature of rhythm. According to Squire, 'Temporalness, in its connota-

[1] Meumann, *op. cit.*, p. 262 (25).
Koffa, *op. cit.*, p. 97 (16).
Miner, *op. cit.*, p. 43 (27).

tion of regular succession, is the basal principle of rhythm. This, however, is quite another thing than saying that the character of the grouping is dependent upon the time order" (**50**, p. 541). As Stetson points out: "The time judgment is much too vague to determine rhythmic intervals, and accurate judgments of time founded on rhythms are secondary and derived" (**51**, p. 258).

If absolute periodicity were indispensable in rhythm, a subjective temporal measuring scale would be necessary to ascertain whether rhythm were present or not. It has been shown, however, that regularity of recurrence is not characteristic of rhythm (**22**, p. 319). What function, then, can a temporal measuring scale have, unless to show that the recurrence is irregular? Yet regular recurrence is not of itself felt as unrhythmical. Nor can the time 'pulses' be a measuring scale as to whether the rhythm can be attended to. The action of attention is organic in its nature; in any case, its duration need not be subjectively measured. The only function a temporal measuring scale could have would be as a test for the presence of rhythm, and then only for irregular rhythm. An illustration may make the relation clear. A clock is a temporal measuring scale for the recurrence of night and day. But night and day are not dependent on the clock for their recurrence. Rhythm takes place in time, but time is not rhythm. According to Brown: "The regularity of the motor performance and the equivalence of the resulting feelings lead naturally to the introduction of the impression of temporal regularity; but that impression is really subsequent to the rhythm itself" (**4**, p. 44).

There is a further objection to a rhythm based on time measurement as determined by the 'new standard.' Patterson says that on the appearance of objective stimuli 'sufficient in number to suggest serial grouping' there is an adjustment of the 'pulses' by means of instinctive processes (**31**, p. 66). This would mean that a constant experience of rhythm would continue during the greater part of waking consciousness from presented stimuli, whether organized or not. This is not the case; the experience of rhythm is unique and unmistakable.

Finally, visual rhythm may take place in space. "The perception of rhythm may be aroused by visual impressions, whether by simple series of discrete stimuli, presented under laboratory conditions, or by the sight of rafters on a corridor ceiling, or of the recurring ornaments of a façade" (**56**, p. 345). Miner says, "There seems to be good reasons for believing that the perception of groups among repeated decorative figures, lines, etc., is a real rhythmical experience depending upon the repetition of a like accompaniment of strain sensations" (**27**, p. 43). Lastly Koffka found that visual rhythm may "easily assume, especially in the higher groups of beats, a spatial structure" (**16**, p. 97). "We will merely say this, that according to the result of this study, we can speak about rhythm in space just as in poetry or music. Aside from motor and auditory impressions and independent of them, purely visual impressions can serve as the sensory basis of group formations, and as a starting point of that inner activity which conditions accent and thereby rhythm. In the space arts, we find this realized in the repetition of an ornament to a considerable extent. The eye moves along and keeps meeting the same forms, and in this way, rhythm arises" (p. 109).

If visual rhythm differs from the auditory so as to have a different basis for its experience, it will be necessary to assume a heterogeneous nature for rhythm. Many factors point, however, to its homogeneity. First of these is the predominant presence in both of kinæsthesis (**39**, p. 249). Then there is the appearance, in both, of the secondary characteristics of rhythm, accentuation and grouping (p. 245). There are also similar illusions in visual rhythm, "apparently the same as those that have been noticed by other observers for sounds. They include the lengthening of the interval between groups, the intensive accent, and the shortening of the time between unaccented units in the three-group" (**27**, pp. 67–68). Ruckmich says, "Many of the phenomena which accompany other kinds of rhythm manifested themselves. Intervals were under- and over-estimated; attributes were subjectively assigned to the members; subjective rhythmization occurred; and redistribution of the groups was common" (**39**, p. 254).

Kinæsthesis in Rhythm

Tentatively eliminating time as an integral part of the rhythm experience, we must return to the predominant elements which are characteristic of rhythm in general. Titchener, believing that rhythm was homogeneous in its nature, had come to the conclusion that there was one and only one rhythm, the ultimate basis of which was kinæsthetic. "The author was formerly disposed to attribute a separate rhythmical perception to hearing, but recent observation has convinced him of the existence of kinæsthetic sensations due to the contraction of the *tensor tympani* of the middle ear" (**56**, p. 345). As to rhythm aroused by visual impressions, "in the author's opinion, this rhythm is always kinæsthetic, based upon eye-movement, upon slight movements which tick off the successive impressions, or upon some other form of intermittent kinæsthesis" (p. 345).

The importance of the kinæsthetic factors had, however, been pointed out as early as in Bolton's work. "Each impression as it enters into consciousness tends to find expression in a muscular movement . . ." (**3**, p. 325). Even before, Gurney, than whom there were few keener observers, had called attention to them—"let the sounds become regular, and instantly the impulse comes to tap the hand or move the foot concurrently with them" (**12**, p. 128). They have been pointed out numerous times since. "By far the greater number of investigators and systematic writers on the subject of rhythm emphasize the primary importance of kinæsthesis and of motor response in rhythmical perceptions" (**38**, pp. 308–9).

Ruckmich, however, who says "in point not only of frequency of occurrence but of the importance of the part played, motor factors are almost indispensable items in the rhythmical consciousness" (**39**, pp. 246–7), states that after rhythm is initiated it may continue in the absence of kinæsthesis (**38**, pp. 342, 359). Bolton, however, says that if movements were attempted to be restrained in one muscle they were likely to appear elsewhere (**3**, p. 234). Meyer (**26**, p. 37) and MacDougall (**21**, p. 466) found that the activity need not be visible in order to give feelings of movement, and Miner states that one subject who gave 'no response whatever to the metronome beat with her hand, head or body' showed considerable reflex response to the beats under hypnosis (**27**, p. 27f.). Lastly Weld found "when actual movements were inhibited one of three things usually occurred. In some cases the rhythmic effect was decreased; in others a tendency to movement appeared in some other part of the body; or, again a motor image or a visual image served as a substitute for the actual movement" (**59**, p. 265). We can therefore disregard Ruckmich's statement that rhythm may continue in the absence of kinæsthetic processes.

Mere kinæsthesis, however, as Titchener thought, is not sufficient of itself to explain rhythm. It limits rhythm to the kinæsthetic field as Patterson's theory limits it to the auditory field. Ruckmich has demonstrated a visual rhythm distinct from both. In addition, as Ruckmich has pointed out, the kinæsthetic factors although most prominent in the organs to which the stimuli are directed, are present in other fields as well as in the field of stimulation (**39**, p. 246), and the clearest part of the perception are the sensations and images corresponding to the stimuli given (p. 254). It is for this reason that he rejects the kinæsthetic basis of rhythm.

Since, however, the kinæsthetic factors are present in all rhythms, it may be well to inquire into their nature. Practically no work has been done on the nature and degree of stimulus as affecting the motor response in rhythm. The contributions which treated of the latter simply recorded they were present and made little or no attempt to localize and measure them as to comparative rate, intensity, quality, or to differentiate rates as manifested through different organs. Bolton suggested there must be different degrees of muscular activity depending on intensity of stimulus (**3**, p. 235), and Weld recorded larger muscular movements corresponding to the musical phrase (**59**, p. 266). Accurate work has, however, been done on the reflex response in another connection, and a grading of intensity found (**47**, p. 71). But the most important item and the basis of the rhythm experience is found in the following: "The rhythm of the reflex has practically the same frequence whether the reflex be excited strongly or feebly: thus, whether the amplitude of the contractions be great or small, they recur with practically the same frequence" (p. 122).

The Basis of Rhythm

A consideration of the essential elements in the various theories of rhythm formulated from Bolton to Patterson will show that a single hypothesis was in every case at the basis of their demonstrations. Preliminary experiments show that the motor response except for simple forms and certain rates of rhythmic stimulation, is independent of the rate of the stimulus. This has been widely recognized, but not acted upon.[1] It is clear that the reflex response in rhythm 'represents a relatively undifferentiated type of reaction' in response to stimulus (**21**, p. 474), but there are several elements of reflex response which have been overlooked when it was advanced as an explanation of rhythmic activity.

First of these is that in the reflex arc conduction, as shown by Schäfer, the rhythm of the discharge of the motor cell is totally different from that of the action induced in the afferent cell by stimulation (**41**, p. 613). In the nerve trunk conduction, on the other hand, there is a close correspondence between the rhythm of the stimulus and the rhythm of the end-effect. It was this latter correspondence which was erroneously made the basis of the motor theories of rhythm. But if, on the contrary, the motor reaction is a serial reflex response resulting from stimulation, the rate of response of the organ cannot depend on the rate of the stimulus given. This is the point of departure from all theories of rhythm heretofore.

[1] MacDougall, Psy. Rev., IX., p. 474 (21).

Stetson, Harv. Psy. Stud., I, 1903, p. 458; Psy. Rev. Monog., IV., 1903, p. 458 (51).

Sherrington, *op. cit.*, p. 71 (47).

It has long been recognized that each organ has a rate of response characteristic and constant for its particular activity. This rate has usually been called the 'natural rate.'[1] Scripture defines it as the rate in which one 'can perform the greatest number of movements with the least fatigue' (**44**, p. 181), and cites the 'route' step on long marches where each man chooses his own step (**45**, p. 107). Smith says that every one has his own rate which is variable within set limits (**48**, p. 82), and Patterson tries to correlate the different rates for various activities in the case of each individual (**31**, p. 148). As Weld found, 'we estimate tempo in terms of our momentary ability to make that motor response which seems to be most fitting for the particular composition which constitutes our stimulus' (**59**, p. 268), and according to Squire, the 'natural rate of the individual' is the basis of the pleasantness of rhythm (**50**, p. 588). Scripture, furthermore, has shown that the natural rate varies with practice, fatigue, time of day, general health, and external conditions of resistance (**46**, p. 528). The determining elements of this rate are certain structural and physiological factors.

Structural Factors

The rhythmical reflex, the response sometimes resulting from continuous stimuli, as, for instance, the scratch reflex, is subject to a certain periodicity in its functioning. It is not only at a frequency independent of the rate of stimulation, but does not change for various modes of excitation, for grouped succession of stimuli, or for variation in the intensity of the stimulus (**47**, p. 45f.). In other words, its period of vibration is constant (p. 122). The rhythmical reflex because of its periodicity may be said to be pendular in its character. Confirmatory of this is the periodic nature and constancy of the rate of response (p. 122), its tendency toward regularity regardless of the number of stimuli (**41**, p. 613), its independence of the tempo of the rhythm and the amplitude of

[1] Stevens, 'On the Time-sense,' *Mind*, XI., 1886, p. 393 f. (53).
Scripture, 'The Law of Rhythmic Movement,' *Science*, IV., n.s., 1896, p. 535 (43).
Scripture, 'The New Psychology,' 1899, p. 181 (44).

the movement (**47**, p. 122). In terms of the law of the pendulum, the amplitude of the reflex varies directly with the nature of the stimulus, but the period of vibration characteristic of the organ remains constant.

The periodicity of the rhythmical reflex is bound up with another aspect of reflex movement, *i.e.*, the refractory phase of muscular contraction, the period in which stimuli are without effect. In all reflexes which are rhythmic and not tonic in their nature, the refractory period is of importance for the maintenance of the movement. "The reflexes of which the refractory phase constitutes a prominent feature are those concerned with cyclic actions occurring in rhythmic series; such as the scratch-reflex, reflexes of swallowing and blinking, and probably the rhythmically recurring reflexes concerned in the stepping of the limbs" (p. 97).

It has been shown that the maintenance of the organic rhythms over long periods is due to the refractory phase of the muscular contraction, and that in voluntary movements, if a sufficient interval is allowed between the contractions, no fatigue is apparent (**13**, p. 49). A similar phenomenon is observable in the nerve cell (p. 139). There may be a relation between the pendulum rate of response and the refractory phase of the nerve cell. It is not, however, the rate of the nerve impulse which determines the rate of response, inasmuch as the different organs are subject to wide variations dependent on their structure. Furthermore, as Sherrington has shown, when one group of motor cells, that of the scratch reflex, is stimulated to produce a weak reflex, and another distinct group, that of the flexors of the hip, is then stimulated alternately with the first, although the second group can respond to a quicker rhythm than that of the first, nevertheless, the rhythm appears of greater amplitude, but unquickened and unaltered, without even a break or interference in it (**47**, p. 122).

In addition, because of the pendulum nature of the response, increase in the intensity of the stimulus does not affect the rate of the rhythm or the length of the refractory phase. "Increase of intensity of the reaction does not show itself in increase in frequency of the rhythm of the reflex, or shows itself very slightly in that form, the refractory period being hardly curtailed at all. The increase reveals itself as greater amplitude of the individual beat of the rhythmic contraction. . . . The beats in response to a strong stimulus may have six times the amplitude of those evoked by a weak" (p. 71).

The motor response in rhythm, since it is also a reflex response, and operates through the same elements and external factors, would tend to show the same characteristic of regularity as the rhythmical reflex. The rhythmical reflex may be identified with the reflex response, with the difference between them that the latter is simpler and uncoördinated in its character; for many forms of stimulation, as some musical stimulation, are continuous, yet give rise to a reflex response, while, on the other hand, the rhythmical reflex is not affected by grouped succession of stimuli (p. 48f.).

Whether or not identical, the same periodicity has been found to govern the reflex response. As in the case of the rhythmical reflex, the basis of the rate and its regularity is, to a large degree, the result of the mechanical factors involved. Among these, the most important is the length of the member and of its parts; as in walking, the rate of time varies inversely with the length of the limb (**41**, p. 270). Wundt speaks of the principle of the isochronisms of like amplitudes of the limbs, and defines rhythmical movements as ones in which the voluntary energy of the muscles is operative only so far as is required to set the limbs oscillating in their joints and to maintain the movement (**63**, p. 174f.). Miner recognized the importance of this factor—"it must be some structural arrangement of our body by which a series of like impressions diffusing to the muscles produces not a separate wave for each impulse, but a longer wave corresponding to a group of impulses" (**27**, p. 34).

The structural element, here likewise, results in a pendulum movement. Stetson has described the mechanism which gives rise to the rate of muscular response as a contraction-relaxation process working between the positive and negative muscle-sets of the limb (**52**, p. 268f.). As in the case of the pendulum, the limb is carried past the point where the generating force is lost by momentum alone (p. 262). The force in the case of the limb is the contraction of the muscles involved. "Thus the limb is *thrown* back and forth, and caught in turn at the limits of its movement by the positive muscle-sets" (p. 262).

It seems clear from the governing effect of the structural factors and their relation to the refractory phase of contraction, that the periodic reflex response, like the rhythmical reflex, is not coördinated with the objective stimulation, but is dependent on the pendulum rate of the member of response. This is further indicated by the periodicity and constancy of its character (p. 263–4), its independence of the rate of stimulation and of the variation in the number of stimuli (**27**, pp. 36–37). Stetson found: "An obstacle against which the limb strikes does not affect the character of the movement; at the end of the normal interval the negative muscle-set contracts and withdraws the limb, as if the limb had shot to the end of the course unimpeded" (**52**, pp. 263–4).

The fact that the basis of rhythm is motor response, and that this motor response is periodic in its nature, and similar to the pendulum in its movement, leads to the thesis that rhythm may be defined as the *experience arising from the periodic, pendular, reflex response of characteristic organs to objective stimulation.*

This definition has been the foundation of so many theories and systems that it is difficult to see why it has not been formulated before. Each of the various theories which were scientific in their nature, recognized one or more of the elements, but were inadequate because they made the one element unduly prominent because of the type of apparatus employed, the sole basis. Bolton recognized that regular muscular response resulted from stimulation, but because of the key-board arrangement which he used, thought it was to every stimulus (**3**, p. 235). Miner saw this was not the case, that these movements were 'something more than one respnose to each stimulus' (**27**, p. 30), but because he found that a single response took place for a group of stimuli in the case of the metronome, assumed therefore that the muscular response was the basis of grouping (p. 12). Both are correct in what they saw, but neither explanation is the basis of rhythm. Stetson found that the duration of the muscular contraction was 'strikingly uniform,' and 'independent of either the tempo of the rhythm or the length of the stroke' (**52**, p. 261), that an obstacle against which the limb strikes does not change the normal interval or the character of the movement (p. 263), but he explains this on the ground of experience. "It is experience alone which teaches us to guide the ballistic stroke" (p. 263). To Patterson, 'syncopation' was emphasized; it is the basis of his rhythm—syncopation between the objective stimulation and the 'unitary pulses of subjective time' (**31**, pp. 4, 67).

Each of these contains an element of the nature of rhythm, but each is a theory of the apparatus. Bolton used a keyboard arrangement; Miner used a metronome; Stetson used a baton; Patterson saw the Indians dance; and all made the peculiarities which were emphasized, the basis of a system. Only MacDougall recognized that the muscular response was independent of the rate of stimulation, and yet not limited to one organism (**21**, pp. 466, 474), but he thought the recurrent stimulation exerted an inhibitive influence 'if its periodic phases are in opposition to those of the natural rhythm of the sensori-motor process itself' (p. 474). He also believed that in addition to the reflex response there was a physiological rhythm 'in the functioning of the central nervous system,'—'functional facilitation and reflex motor discharge, I conceive to be represented in the conditions which support the impression of rhythm' (p. 466). The first factor, the possibility of lack of coincidence of stimulus and response, was also recognized by Stetson: "What happens when a sound occurs out of place, early in the phase of relaxation, or just before or just after the climax of the contraction phase? Does it make it impossible to establish the coördination, or destroy it if already established?" (**51**, p. 458).

It is evident that at certain rates there may be two opposing tendencies, that of the periodic, pendular response, and that of the rate of stimulation. Weld found that when music seemed too fast, it was 'too fast for the particular motor reaction which seemed most natural to the observer' (**59**, p. 267). When not of identical occurrence, but when within favorable limits, the response may tend to approximate the rate of stimulation. Stevens found that intervals of a subject who beat time to a metronome, and continued after the metronome had stopped, agreed only when a particular interval was used (**53**, p. 401). Even here, however, there may be a gradual divergence, which, when it becomes appreciable, requires an adjustment. "The introduction from time to time of a single extra tap, with the effect of transposing the relations of the motor accompaniment to the phases of the metronome, has been here interpreted as arising from a periodically recurring adjustment of the reaction process to the auditory series which it accompanies, and from which it has gradually diverged" (**22**, p. 338).

When there is regular presentation and regular reflex response, if a favorable organ is available, there will result a correlation between the response and a certain number of stimuli. But the relation may not always be an essential one, and even Miner, who has made this relation the basis of his whole system, says, 'the length of the group does not increase proportionately to the number of elements in it,' and farther on, 'we know that the same individual varies greatly in the length of the group he chooses' (**27**, pp. 36–37).

Due to the many forms of reflex response, there is no one unit 'to correlate all experience,' no one basic rate of measuring as Patterson has ascribed to the walking rate (**31**, p. 64). Miner recognized that it is misleading to claim there is a 'standard length of group or that the normal group depends on respiration, fatigue or any particular physiological rhythm as determining its natural length' (**27**, p. 39). The walking rate which Patterson uses as the basic rate for the whole rhythmic experience, is only one manifestation of the motor response and is dependent on the pendulum rate resulting from the factors involved in walking, just as the nodding rate is dependent on the factors involved in moving the head.

There is still another characteristic of reflex action which influences rhythm, the after-discharge. It is usually a tetanic contraction after the cessation of stimuli, and is affected by increase in the number of stimuli and increase in the intensity of the stimulus (**47**, pp. 28, 30). The after-discharge throws some light on the pause in verse. In the rhythmical unit in verse, it has been found that the final element has greater duration and intensity than the other elements. Thus Snell records that the word or syllable in a verse immediately preceding a pause is marked by greater duration and probably intensity (**49**, pp. 39, 47). The discharge of an unusually strong impulse leaves the nerve cell exhausted and a certain time to be recharged is required. So Stetson found that in lyric verse, the verse pause was from one fourth to one third longer than the foot pause (**51**, p. 443), and that since the end of the verse is the natural climactic position, rhyme was also preferable at the close (p. 429). Snell found that in lyric verse, the end-pause is twice as long as the internal pause. In some verse rhythm, the rhythmic unit is also dependent on the summation of effect, when there is not a complete relaxation after each response (*cf. infra*).

The Initiation of Rhythm

In the initiation of rhythm, according to Patterson there is an adjustment by means of instinctive processes of the elastic unitary pulses and the objective auditory stimuli, sufficient in number to suggest serial grouping (**31**, p. 66). Practically all other investigators consider the kinæsthetic processes the basis of the initiation of rhythm. Ruckmich thought the kinæsthetic processes were references for its interpretation when it is first heard (**38**, p. 351), and that in the initiation of a difficult rhythm they may be even the most prominent item (**39**, p. 247).

The initiation of rhythm it is suggested presents the following phenomenon. Unless there is a preconceived pattern of response, stimuli not strong enough of themselves to evoke a reflex response may, when repeated, result in a summation of stimuli and produce contraction. This continues till the pendulum rate is organized, and the adaption to the refractory phase established. The organizations of most poetic meters is on a basis of the reflex pattern. If, however, irregular stimulation in the absence of a pattern of response is presented, confusion results at first, and until the adjustment is made; or if it is never made, or if the recurrent stimuli are too frequent and intense, the rhythm is never initiated.

Ruckmich states: "Should the rhythm be more than moderately difficult, and should it, therefore, not become definitely fixed, or should the mental set of *O* be such that he cannot make the rhythm 'fit in,' the pleasant affection may never be reported, and strain sensations may continue in a vague degree to the end. . . . Then, ordinarily, sensations of strain gradually die away, attention drops in level, kinæsthesis grows less intensive and extensive, and finally vanishes completely or becomes irrelevant to the rhythm. The rhythm is heard merely in terms of auditory perceptions" (**38**, p. 342). Scripture found that in the beginning of an experiment on a rhythm with a new period, the subject is quite at a loss for a few beats and can tap only spasmodically until he obtains a 'subjective judgment' of the period (**46**, pp. 527–8). Smith says, "It is doubtful if a rhythm is really perceived before a certain degree of facility or skill in the movement is attained" (**48**, p. 289).

The reflex pattern may result through the pendulum rate asserting itself, or through the establishment of the latter by presented schedules in its terms. Patterson found that on the first hearing a large number of observers found all the records which he used elusive and more or less irregular (**31**, p. 2). When organization through schedules was presented, various degrees of satisfaction were obtained (p. 64). Even when there is a schedule, however, confusion may result when there is a maladjustment of the pattern to the stimulation, as in an attempt to read anapæstic meter when the motor response is adjusted to iambic. Wallin found that schematic arrangement was an aid to such an extent as to differentiate prose and poetry (**57**, p. 64).

The case of involuntary movement is interesting in this connection. Miyaki found that "arhythmic movements have a constant tendency to become rhythmic, notwithstanding the voluntary effort of the subject to execute the movements at irregular intervals. The subjects of the experiments invariably agreed in confessing that the arhythmic tapping required strenuous effort and that the performance was very fatiguing" (**28**, p. 4). Voluntary irregular movement necessitates a disturbance in the refractory phase involved and the pendular aspect of movement.

Syncopation

The phenomenon of syncopation to which Patterson has drawn experimental attention, "in itself, involves a complex of mental processes. The most essential part of the phenomenon seems to be that we keep our impression of a series of subjective time-intervals, regular, accelerating or retarding, but find a pleasure in marking the beats objectively, either by different forms of motion, such as foot-taps alternating with hand-taps, or by what appears at first as omission of objective marking for certain beats. As a matter of fact, this is usually nothing but the interpolation of some concealed form of motor reaction such as eye, throat, tongue, or breath movement, which alternates with a more visible movement, such as nodding or tapping or dancing" (**31**, p. 4).

Stetson has described it in much the same terms. "Along with this precision of all the movements comes a tendency to beat a new rhythm. This accompanying rhythm is simpler and broader in character; it is a kind of long swell on which the speech movements ripple. This second rhythm may express itself in a new movement of hand, head, foot or body; when it has become more conscious, as in patting time to a dance or chant, it develops complicated forms, and a third rhythm may appear beside it, to mark the main stresses of the two processes. The negro patting time for a dance beats the third fundamental rhythm with his foot, while his hands pat an elaborate second rhythm to the primary rhythm of the dancers. . . . This regulation of the movement by the coincidence of several rhythms is the cause of the striking regularity of the temporal relations" (**51**, pp. 465–6).

In Patterson's definition (**31**, p. 4), syncopation is apparently manifested by the performer of the rhythm. Syncopation is used by Patterson in three senses, (1) as any full motor response (p. xix), (2) motor response in the performer of the rhythm (p. 4), (3) a correlation of the 'unitary pulses' and objective stimulation in the observer (p. 91). This analysis has shown that while there is coexistence, there is not necessarily correlation and rarely coincidence of the objective stimulation and the reflex response. Syncopation in the third sense exists, but it is limited to a comparatively small field of rhythm.

Full motor response is not so evident in modern rhythm. As Patterson says, "Modern sophistication has inhibited many native instincts, and the mere fact that our conventional dignity usually forbids us to sway our bodies or to tap our feet when we hear effective music, has deprived us of unsuspected pleasures" (p. xix). Patterson concludes: "What is left, then, but to conclude that the sentence which has in its structure the possibility of a maximum of rhythm must be capable of evoking in us a maximum of motor response? To test it, therefore, we must tap to it, nod to it, walk to it, sway to it, chop wood to it, if necessary. . . . If it is easy for us to nod or tap, or, for that matter, hoe potatoes to these salient 'drum-songs' . . . the first degree of rhythmic excellence is obtained" (p. 15).

The contortions of the polar bear which Patterson has called 'prose' merely present syncopation of the muscular responses of various organs due to the pendulum rates of the organs. They are not, however, 'harmoniously but intricately regulated by the incessant unitary "flap! flap! flap!" of those great white feet' (**32**, p. 261). Each is as independent in its own sphere as the walking movement is in its sphere. The large body of literature on rhythm, then, is not invalidated by the 'new standard.' On the contrary, it is enriched by the hitherto experimentally unrecognized field of syncopation.

The Factors of Attention

Although rhythm is intimately bound up with attention, the unsatisfactory state of knowledge about the latter prevents a wholly satisfactory correlation of the two. Ruckmich says that during the rhythm experience attention is at a high level (**38**, p. 342), and he believes that there is a typical rhythmical consciousness (p. 341). Bolton ascribes grouping and accentuation to a 'sequence of acts of attention' (**3**, p. 211), and with this position there is substantial agreement. Squire says: "One group corresponds to one pulse of attention, and the regularity of the subjective rhythm is due to the regularity with which the pulses of attention succeed one another" (**50**, p. 575). MacDougall posits a kinæsthetic level due to changes in attention, 'those elements which are emphasized being likewise more clearly attended to' (**21**, p. 468). Meumann says that rhythm may be regarded as an unlike energy division of the attention, an alternation of attending and not attending (**25**, p. 304). Arps and Klemm found that the greatest degree of attention occurs at the accented sound and the least at the second unaccented sound (**2**, p. 518f.). Rhythm was at one time thought to be solely a matter of attention (**35**, p. 164; *Cf.* **34**, p. 330).

In repeated stimulation resulting in rhythm, it is clear that there are two kinds of presentation, regular and irregular. There is no essential difference between them from the point of view of rhythm, other than that of degree. There are, however, two classes of stimulation which are different in their nature, that objectively accented and possibly grouped, and that undifferentiated. An example of the first class is most poetry or music; an example of the second class is the ticking of the metronome or the puffs of a locomotive. Neither grouping nor accent, however, are necessarily a part either of the objective stimulation or of the periodic response.[1] Neither are present in organic rhythm nor at certain rates in presented rhythm. It is evident that

[1] *Cf.* Patterson, *op. cit.*, p. 4 (31).

Squire, *op. cit.*, p. 540 (50).

Wallin, 'Experimental Studies of Rhythm and Time,' Psy. Rev., XIX., 1912, p. 297 (58).

they do not cause nor are they the result of the periodic response. Grouping and accentuation, it is suggested, are the result of the organic rhythm to which attention itself is subject (V. **33**, p. 70). Wundt in speaking of the periodic rise and fall of attention says it may become regular in its periods when there are special considerations favoring rhythmical succession (**63**, p. 255). Titchener says: "As for the effect of the anticipatory image, it is clear that, the more nearly the excitation correlated with the given stimulus coincides with a psychophysical excitation already in progress, the more easily will it make its way within the nervous system and the more dominant will it become" (**55**, p. 205).

The results of attention also appear with undifferentiated stimulation and give rise to accentuation and grouping. It is in the case of undifferentiated stimulation that the verification of the suggestion must be found. "It is the fact of periodical differentiation, not its particular direction, which is important. Further, as we know, when such types of variation are wholly absent in the series, certain elements may receive periodical accentuation in dependence on phases of the attention process itself, and a subjective but perfectly real and adequate rhythm arise" (**22**, p. 320).

The operation of rhythm can be thought of as, on one part, objective stimulation, regular or irregular; on the other part, regular serial reflex response. Bridging the two is attention, which acts in its own way. Rhythm arises from the reflex response; accent and grouping are the result of attention.

Supplementary evidence that this is the case is furnished by the illusions of the durations of the undifferentiated member and its contiguous intervals. "The effect of both intensity and duration in rhythm may be generalized as follows. If every second or third sound is made more intense or is made shorter, the effect on grouping is the same as if the interval immediately preceding that sound were increased relative to the other intervals. The effect of the more intense sound, when all the sounds are of equal duration, or of the shorter sound when all the sounds are of equal intensity, is a relative overestimation of the interval preceding the more intense or the shorter sound" (**60**, p. 66).

Ruckmich, following Miner, lays great stress on kinæsthesis as the basis of grouping and accentuation. He says, "Three points are certainly clear: (1) the kinæsthetic complex changes for accent and non-accent, (2) kinæsthesis on the accent is more intensive and is felt as strain or tension, while kinæsthesis on the non-accent is less intensive and is felt as relaxation, and (3) kinæsthesis, prominent as it is, may be temporarily or entirely replaced by visual or auditory complexes" (**38**, p. 336). "To the writer the group appears to be a complex of perceptions organized in terms of imaginal and kinæsthetic processes on the basis of affectively toned organic processes" (**39**, p. 254). MacDougall, however, places the kinæsthesis of attention on a different level than that of the motor accompaniment although he says it is concomitant with the sensory series (**21**, p. 467). The question arises whether this particular kinæsthesis of strain or tension is not of this nature and dependent on attention (*cf.* **3**, p. 211).

Poetic Rhythm

A word may here be said on the rhythm of poetry. The rhythmic experience arising from poetry is more satisfactory than that of prose although Patterson would consider it of an inferior order. "The aggressive 'timer,' of course, gets his keenest delight from prose in the fact that he feels no trammels" (**31**, p. 84; *cf.* **21**, p. 478). That this is not the verdict of experience is shown by the fact that all peoples in all times have chosen poetry as a vehicle to express their most satisfactory experiences. In poetic rhythm, there is the possibility of greater correlation between the regularity of the periodic response and of the occurrence of the objective stimuli. This does not mean, however, that they are coincident. Patterson says that poetry is the result of coincidence of the unitary pulses and the accented syllables (**31**, p. 91). Considering the unitary pulses as equivalent to one form of response, this may be true for a limited body of poetry, but it would be at the sacrifice of attention and interest. According to Weld, movement is not unison with the *Takt*, but is in accordance with the musical phrase (**59**, p. 266).

At the same time the methods of the 'stresser' and all schematic classical systems of scansion which Patterson so greatly condemns (**31**, p. 83), have been of use and still are in classifying certain forms of poetry. The unfortunate result of their use was to render difficult of analysis the nature of poetries, in which the motor response is not so nearly correlated with the objective stimulation. For this reason, the nature of biblical meter was obscured for nearly twenty centuries although scholars had worked on it steadily during that time. Its nature recently indicated (**14**, p. 20), shows as close an approximation to the motor rhythm of other poetries, but not through stereotyped metric forms. The motor response arises in connection with the normal unit for recitative. Its basis is the 'word-foot,' so that there is an identification between the word and the unit of response (p. 41). This is possible through the similar lengths of the words (p. 41). The rhythmic unit corresponding to the verse, however, is of definite and invariable lengths, consisting of three units and, in a certain form of poetry, of two units (p. 44). Furthermore, owing to the intense form of the poetry, there was not a relaxation after each response, but a simple and clear case of summation of effects resulted, which gave rise to the parallelistic structure (**15**, p. 114).

In the adult reading of the modern verse, the characteristic reflex response seems also to be in relation to the point of maximum emphasis rather than in any indispensable relation to the uniform metrical foot. It is dependent on the form of reflex response. Brown found that the verse in English poetry seems to be divided into short phrases which are fairly uniform in their length while the feet are not (**4**, p. 51).

Elements of the Definition of Rhythm

(*a*) Affective Tone

Whether affective tone should be included in the definition of rhythm has been put in issue between the extremes of Smith who says that rhythm no longer exists when affective tone becomes unpleasant (**48**, p. 287), to Squire who says that feeling is not essential to the perception of rhythm (**50**, p. 587). There are many intermediate views, and some that cannot be placed at all. Wundt defines rhythm as an emotion arising from the feelings of expectation and satisfaction (**61**, p. 311; **63**, p. 200), and says the pleasantness of rhythm depends on the repetition of feelings of tension and the contrast between feelings of tension and relaxation (**64**, p. 158f.). Meumann says that the affective tone of rhythm depends on the mood of a given time (**25**, p. 266), and according to Smith, although she gives no citation, defines rhythm as an emotion discharging itself in ordered movements (**48**, p. 292). Ebhardt places the main stress on affective tone and makes it the *sine qua non* of rhythm (**9**, p. 127). Ruckmich says rhythm may change from slight unpleasantness before it is grasped, through pleasantness when it is thoroughly perceived, to unpleasantness when it continues without change (**38**, p. 359).

Throughout these theories there runs an unconscious distinction between affective tone as a result of the rhythmic experience and affective tone as an element of it. If the latter is true, then the statement that 'there is no poor rhythm' (**48**, p. 292) is correct. The weight of the evidence, however, is against this. Rhythm may be unpleasant; at times it may be 'dreadful' (**3**, p. 221; **48**, p. 285).

Affective tone is the result of rhythm, but since rhythm is a continuing phenomenon, the affective tone aroused by the feelings of repeated, perfected movement, has been thought to be the cause of the rhythm coming. Squire says: "The affective tone increases in proportion as the summation of excitation increases, till a state bordering on ecstasy may be reached. Ecstasy, when it follows upon rhythmical stimulation, is due to a spreading of the excitations to a greater and greater number of centers, till the body and the whole of consciousness are set in co-vibration" (**50**, p. 588).

(*b*) Complexity

Every investigator whether or not he includes affective tone is his definition of rhythm is very certain and unequivocal as to its complexity. This is practically the only point that all are agreed upon. Thus Meumann says: "The error must be emphatically combated, that on defining any one of these elements, even of the so-called equality of the beats, we have defined the nature of the rhythm," but enumerates the various factors which are necessary for the experience of rhythm. "In this manner, in the rhythmical impression we shall have to seek for the elements of time, accent and pitch. By the side of these there must be distinguished a number of higher intellectual factors, whose operations we must seek in the inner comprehension, in the additions of subjective accent, in the strain and relaxing of the attention, the relating of the rhythmical groups to one another, their perception as repetition of the preceding and preparation for the following impressions" (**25**, pp. 305–6). So Wundt (**62**, pt. 2, I, p. 376f.); and Ruckmich says, ". . . the rhythmical perception is an exceedingly complex affair. . . . The grouping effect of a rhythm in any case may depend on visual patterns, on auditory imagery, on organic complexes, on changes of clearness, on alterations of temporal arrangements, on verbal ideas, on motor responses, and on many similar items" (**39**, p. 247).

Likewise Patterson is careful to point out this item of the experience. "Rhythmic experience may thus be roughly described as a complex of perception, emotion, and sensation, with all three elements subjected to the moulding processes of attention, both voluntary and involuntary" (**31**, p. 91). There is ample complexity here, yet elsewhere he says, "The final impression of rhythm derived from a sentence is, to a large extent, a fusion of elements, in which actual pitch, tone-color, thought, mood, capricious or logical attention, etc., enter as factors in addition to duration, stress, and the dim elements of pitch, actual or purely subjective, implicated in the drum-beat tune" (p. 70).

Summary

Rhythm is the experience arising from the periodic, pendular, reflex response of characteristic organs to objective stimulation. There are four elements in the impression of rhythm, the perception of the objective stimulation, the experience of the periodic reflex response, accentuation and grouping resulting from attention, and the affective tone arising from repetition of movement.

The pendulum rate is the rate at which an organ vibrates in the absence of voluntary factors, and is the result of the length of attachment of a member, and of the refractory phase of the muscles involved.

The reflex response is the result of, but independent of the stimulation, and depends on the pendulum rate of the member responding. Because of the periodic nature of the reflex response, regularity was read into the objective stimulation, and it was thought that the latter must be chronometrically proportionate. This belief gave rise to elaborate and sometimes artificial systems of meter, and prosody was erected into a science.

The objective stimulation has one prerequisite, that the discrete stimuli recur so as to give rise to a serial response. With this qualification, the stimulation may be regular or irregular, accented or unaccented, grouped or ungrouped.

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A NEW POINT OF VIEW IN THE INTERPRETATION OF THRESHOLD MEASUREMENTS IN PSYCHOPHYSICS

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I. Introduction

I propose in this paper to examine some of the ideas underlying the interpretations hitherto given of threshold measurements, and in particular (*a*) the measure used in comparing the sensitivity of one individual with that of another, (*b*) the measure of sensitivity used in testing Weber's Law, (*c*) the origin of this law, whether it be psychological or physiological, (*d*) the idea of a psychometric function, and (*e*) the notion of the probability of a judgment. These points cannot be treated separately, but must be considered in conjunction with one another. The ideas which I wish to express have their origin, however, chiefly in an extension of the last-mentioned point.

II. The Difference Threshold

To fix ideas, I shall discuss difference thresholds only, leaving the question of absolute thresholds aside. Further, I shall use the case of experiments on lifted weights, by the Method of Right and Wrong Cases.[1] In this form of experiment, comparison weights are contrasted frequently, one by one, by lifting them in a specified way, with a standard weight, and on each is expressed a judgment *lighter*, *undecided*, or *heavier*. When a sufficient number of judgments have been collected, the three categories are found to occur with varying frequency with different comparison weights, and curves similar to those shown in the accompanying diagram can be constructed.[2]

[1] Otherwise termed the Method of Constant Stimuli, and the Methode der drei Hauptfälle.

[2] For fuller details of the determination of this and other thresholds, and for references to experimental memoirs, the reader may consult G. T. Fechner, Elemente

In this diagram the points marked on the x-axis represent the comparison weights. The height AB represents unity, and the three curves give the proportional frequency of answers *lighter, undecided and heavier* respectively, the *undecided* curve being a bell-shaped curve, and the other two being what Galton termed *ogives*.[1] The difference threshold

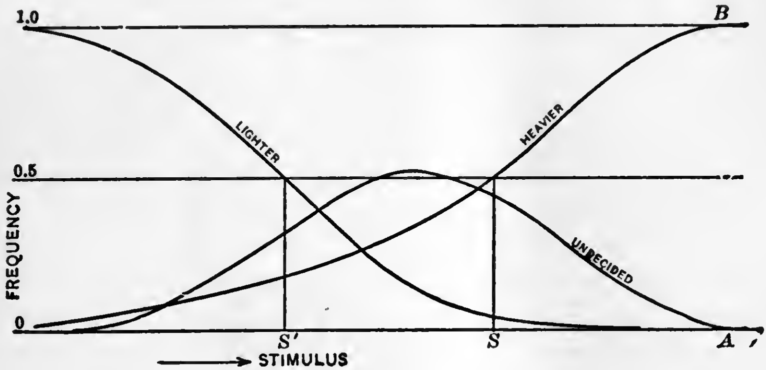

Fig. 1

is then decided by the positions of the points S and S' where the *lighter* and *heavier* curves cross the halfway line.[2] The distance $(S - S')/2$ or some closely similar quantity is what is called the difference threshold, and this quantity is commonly used in comparing the sensitivity of different subjects. The smaller $(S - S')$, the more sensitive the subject is said to be.

This distance however depends entirely on the subject's

der Psychophysik, 1869; G. E. Mueller, Die Gesichtspunkte und die Thatsachen der psychophysischen Methodik, Wiesbaden, 1904; W. Wirth, Psychophysik, Leipzig, 1912; and F. M. Urban, The Application of Statistical Methods to the Problems of Psychophysics, Philadelphia, 1908.

[1] The actual experiments of course do not give curves but only points through or among which the curves are then drawn smoothly, either by hand, by a flexible ruler, or by assuming mathematical equations for the curves. In the latter case if the equations contain sufficient constants, the curves can be made to go exactly through the points; otherwise the best fitting curves are drawn by some method such as the method of least squares or the method of moments.

[2] These points on an ogive are analogous to the medians of bell curves. Some experimenters, instead of calculating these medians, calculate points analogous to means. These differ slightly from S and S' but these differences have no bearing on our present argument. Questions of space and time errors also arise.

readiness to give the answer *undecided.* It measures therefore rather a moral characteristic than a physical sensitivity. It is a question of quickness of decision on small evidence just as much as a question of difference of amount of evidence. If persons A and B differ widely in the number of *undecided* answers which they give, it may just as well be their habits of forming decisions which differ, and not their power of actually discriminating the weights. The moral character of the measure $S - S'$ is above all seen from the fact that any subject who wishes may reduce it to zero, whatever may be his actual sensitivity, simply by determining that he will never give an answer *undecided.* Difference thresholds therefore are unsuitable for comparing the sensitivity of different subjects.

III. The Interquartile Range of the Point of Subjective Equality

There is however another measure which has been used for this purpose. This can be most conveniently described by considering first a case in which a subject gives no *un-*

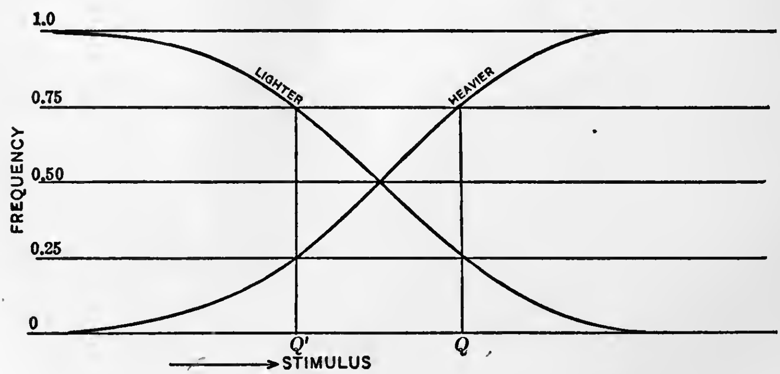

Fig. 2

decided answers. In such a case the central curve of the former diagram, that is the bell-shaped curve, disappears, and only the two ogives are left, as in the accompanying Fig. 2. The thresholds S and S' have come together and on the previous plan the subject's sensitivity would be considered infinite, and all subjects giving no *undecided* answers would be given the same infinite sensitivity, whereas clearly the subject's sensitivity is connected with the rapidity with which the curves pass from zero to unity or vice versa, and two subjects may differ very much in this respect even although they may both give no *undecided* answers. Under these circumstances a measure which has been used is the distance $Q - Q'$ which is sufficiently explained by the diagram. This measure (though in another guise) was used by Fechner also for the cases where *undecided* answers were given. In such cases he reduced the three curves to two by sharing the *undecided* answers equally between *heavier and lighter*. This measure has the advantage that the subject cannot increase his apparent sensitivity at will, as was the case with the threshold measure. $Q - Q'$ is the interquartile range of a hypothetical point of subjective equality. It and the difference threshold measure distinctly different things and subjects placed in order of merit by the one will be found in a different order when graded by the other. The measure $Q - Q'$ is more physiological than the threshold measure.

IV. Weber's Law

Although these two measures, the difference threshold and the interquartile range of the point of equality, measure different things and give quite different results when we are comparing separate subjects, yet in one and the same subject each of them obeys Weber's Law in so far as that law is obeyed by any measure. It would appear that some light might be thrown on the question whether Weber's Law is physiological or psychological in its origin by a comparison of the accuracy with which these respective measures obey that law, although such a comparison would not be crucial. If, however, the difference threshold were found to obey Weber's Law more exactly than does the probable error of equality, then I should consider this distinctly in favor of a psychological explanation of the law. But I should myself anticipate the other measure to obey the law more exactly, which, although by no means proving a physiological explanation to be correct, would nevertheless point in that direction.

V. Psychometric Functions

Turning now temporarily aside from the considerations which have occupied us up to this point, let us consider some questions arising out of what is known as the search for the 'psychometric function.' These functions are the mathematical equations which best fit the bell-curves and ogives into which psychometric data fall. They are clearly error functions of some sort or other, and there is no particular advantage in calling them psychometric functions. For our purpose here it is sufficient to consider the possibility of the "psychometric function" being the normal curve of error, and to leave the elaborations necessary when more complicated functions are used. What is here said about the normal curve is typical and true of other suggested functions such as Professor Pearson's types.

Consider Fig. 1. The suggestion has been made by many that the extreme curves, the ogives, are integrals of the normal curve of error. The bell-curve in the center is deduced by these writers by first forming the outer curves and then subtracting their sum from unity at each point. All the usual arguments, however, which support the view that the outer curves are integral normal curves would lead one to expect, when applied to the central curve, that it is a normal curve as it stands. But this is impossible. Two such normal ogives added together and subtracted from unity, ordinate by ordinate, do not give a normal bell curve.

VI. Extension of the Notion of the Probability of a Judgment

This difficulty can I think be overcome if we recognize the fact that these curves are not each of them complete error functions, but represent *one* error function divided into three parts (the division itself however being no doubt in turn subject to an error distribution).

This involves an extension of the idea of the probability of a judgment. In its simple form this idea compares the giving of a judgment *heavier*, *undecided*, or *lighter*, with drawing a ball from an urn containing say red, white, and blue balls, and ascertaining its color. For each stimulus the urn is supposed to contain different proportions of the colored balls.

In place of this I suggest the following. For each stimulus imagine an urn containing an infinite number of balls divided between black and white in a proportion varying in some way with the stimulus. A judgment is to be compared with taking not one but a handful of balls from the urn, and the kind of judgment is to depend on the proportion of black balls in the handful.

VII. The Resulting Curves

To clear our ideas let us take a concrete case with small numbers. Suppose an urn contains seven tenths black and three tenths white balls, and that a judgment corresponds to drawing four. The possibilities that can occur in such a lottery are five in number, namely

Occurrence	Frequency
4 black, no white	0.2401
3 " 1 "	0.4116
2 " 2 "	0.2646
1 " 3 "	0.0756
0 " 4 "	0.0081
	1.0000

where the frequencies are the terms of the binomial expansion $(0.7 + 0.3)^4$.

Now suppose that either 4 or 3 balls correspond to an answer *heavier*, 2 to an answer *undecided*, and 1 or 0 to an answer *lighter*. For the stimulus in question the relative frequencies of these will be:

$$\begin{array}{lr} \textit{heavier} & 0.2401 + 0.4116 = 0.6517 \\ \textit{undecided} & = 0.2646 \\ \textit{lighter} & 0.0756 + 0.0081 = \underline{0.0837} \\ & 1.0000 \end{array}$$

Imagine now this process carried out for a number of urns, thus

Composition of Urn	Frequency of Answers		
	Heavier	Undecided	Lighter
0.0 black	0.0000	0.0000	1.0000
0.1 "	0.0037	0.0486	0.9477
0.2 "	0.0272	0.1536	0.8192
0.3 "	0.0837	0.2646	0.6517
0.4 "	0.1790	0.3458	0.4752
0.5 "	0.3125	0.3750	0.3125
0.6 "	0.4752	0.3458	0.1790
0.7 "	0.6517	0.2646	0.0837
0.8 "	0.8192	0.1536	0.0272
0.9 "	0.9477	0.0486	0.0037
1.0 "	1.0000	0.0000	0.0000

These numbers are the basis of the three curves in Fig. 3 which it will be recognized are very similar to those actually found in psychological experiment. Skew curves are easily obtained by placing the points of division into the categories *heavier*, *undecided*, and *lighter* at unequal distances from the

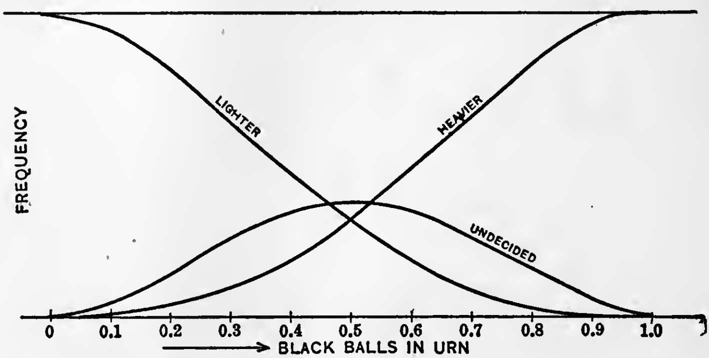

Fig. 3

center of the underlying error curve. The actual form of the curves depends on the way in which the x-axis is measured out. In this diagram the simplest assumption is made, namely, that the proportion of black balls in the urn is a linear function of the stimulus, and, between the limits at which the subject answers heavier or lighter with certainty, directly represents it. The number of balls taken in the sample which represents a judgment is also a factor. But ultimately this would be made very large, and Type III. curves would replace the binomials.

On this point of view, the standard, the variable stimulus, and the physiological make-up of a subject decide the proportion of black balls in the urn, but the decision as to what proportion in the sample is to be called heavier, what undecided and what lighter depends upon a conscious act of the subject, and can be varied, if he be so disposed, at his whim; and will vary with his mood at the moment.

The difference between this point of view and the older one may prove to be academic only,[1] for the disentangling of the factors which are here distinguished may in practice be impossible. Nevertheless the idea seems illuminating, and this sketch is put forward in the hope that some of the resulting curves may prove of interest to mathematical statisticians, and of use in psychophysics.

[1] Indeed it may be that mathematically the one reduces to the other, at any rate under certain conditions.

THE CORRELATION BETWEEN INTERESTS AND ABILITIES IN COLLEGE COURSES

BY JAMES W. BRIDGES AND VERONA M. DOLLINGER

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It is obvious that achievement in any vocation depends partly upon ability to do the work and partly upon interest in that particular kind of work. The latter factor is not less important than the former, for it supplies the necessary incentive to the greatest endeavor. The problem for vocational guidance is therefore to measure ability and interest.

The measurement of ability whether considered as special aptitude or as general intelligence presents no great obstacles. To obtain a test or group of tests whose results correlate highly with actual achievement in any vocation is a relatively simple psycho-statistical problem. The measurement of interest is a much more difficult matter. No objective method is available. The evaluation of interest must therefore depend entirely upon subjective estimate; and of course this estimate cannot be made until the subject has some knowledge of the work.

If it can be shown that interest is highly correlated with ability, the problem of vocational guidance will be simplified; because on the one hand, any interest on the part of the individual will give him definite assurance of ability in that direction, and on the other hand, the presence of a definitely determined ability will surely indicate an interest which perhaps merely awaits experience to be awakened. If however, the correlation is low, as the results of this study seem to indicate, then the practical problem in vocational guidance will be to determine the general ability and special aptitudes of the individual, to advise him as to the several occupations suited to his intelligence level and specific abilities and let him select from these on the basis of his interest, together with other considerations such as social status of the work and remuneration.

The problem of the relation between interest and ability has received considerable attention from educators from time to time; but the only experimental study of the subject known to the writers is that reported by Professor E. L. Thorndike of Columbia University.[1] His subjects, college students, arranged the courses of their curriculum, namely—mathematics, history, literature, science, music, drawing, and other handwork, in the order of their interest and then later in the order of their ability in them. The correlation between these two orders was found to be as high as .89, and the conclusion was drawn that "A person's relative interests are an extraordinarily accurate symptom of his relative capacities."

It seems quite probable that the correlation obtained, .89, is much higher than the actual correlation between interests and abilities, for a subject's ranking of his courses for the one is likely to be influenced by his ranking for the others. Thorndike points out that it would be "Better to get the measurements of relative interest and of ability . . . not from individual reports alone, but from objective tests." A satisfactory objective test for interest is not yet available; but an objective measurement of ability would seem to be afforded by college grades, especially where the proportions of the various grades assigned conform approximately to the normal distribution. It seemed, therefore, desirable to determine the correlation between interests, evaluated subjectively by rankings of courses, and abilities, measured objectively by grades obtained in these same courses.

With this object in view, several hundred students were requested at the beginning of the semester to fill out and return blanks upon which were printed the following instructions: "Arrange the courses you are studying *this semester* according to your interest in them. Place first in the list the course you are most interested in, then the others in order. Please make your judgments carefully and deliberately, and try as far as possible to avoid *influence by class grades* or preference for

[1] 'The Permanence of Interests and Their Relation to Abilities,' *Pop. Sci. Mo.*, 1912, 81.

'Early Interests, Their Permanence and Relation to Abilities,' *School and Society*, 1917, 5.

instructor." Just below a space on the blank for listing the courses a second instruction was given: "Now arrange the subjects you are studying this semester according to your ability in them. Try to make your judgments independent of your interests and of any class grades you may have received." Since these data were obtained near the beginning of the semester, the influence of class grades upon the students' rankings was probably negligible. At the end of the semester the grades actually made by each student on the courses ranked were obtained from the registrar's office. Ranging in order from highest to lowest, the grades used in the university are: M, G, A, P, and F, which are assigned approximately in conformity with the proportions: 5, 20, 50, 20, and 5.

Records were obtained from over five hundred students, and the number of courses ranked varied with different students from four to eight. In order to simplify the calculations only those records with five courses ranked were used since they furnished the bulk of the material.

The relationship between these interests and abilities was determined by means of Pearson's formula for mean square contingency[1] since this seems best adapted to such rough forms of evaluation of traits. The coefficient of mean square contingency (C) thus obtained is somewhat smaller than the coefficient of correlation (r), for in the case of a five by five-fold classification the former (C) cannot exceed .894. However, with coefficients as low as those actually obtained the difference becomes relatively negligible.

The relationship between rank in interest and grade earned was determined first for a specific course of study, namely, elementary psychology. All records that reported psychology were used irrespective of the sex or college year of the student. The data are presented in full in Table I., an inspection of which plainly corroborates the low coefficient obtained (.22). The relationship between rank in interest and rank in ability (as reported by the subject on his record blank, hereinafter referred to as estimated rank in ability) was next determined for the same course, and a coefficient of .59 obtained. Similar coeffi-

[1] See H. D. Rugg, 'Statistical Methods Applied to Education,' pp. 299–307.

TABLE I

RELATION BETWEEN RANK IN INTEREST AND GRADE EARNED IN PSYCHOLOGY[1]

Rank in Interest	Grade					Number Students
	F	P	A	G	M	
1	3	8	14	10	5	40
2	3	10	30	7	9	59
3	2	11	31	17	8	69
4	1	21	30	13	7	72
5	4	10	27	14	4	59
Number students	13	60	132	61	33	299

[1] The letter grades, F, P, A, G, and M, are assigned approximately in conformity with the proportions: 5, 20, 50, 20, and 5; and they have the following meanings: fail, pass, average, good, and merit.

cients were calculated for the English course with very similar results (see Table V.).

Coefficients might have been calculated in the same way for each course reported by the students; but it seemed more desirable to obtain a coefficient that would express the relationship between rank in interest and grade for all courses combined. With this object in view Table II. was prepared from

TABLE II

RELATION BETWEEN RANK IN INTEREST AND GRADE EARNED. ALL COURSES COMBINED

Rank in Interest	Grade					Total
	F	P	A	G	M	
1	6	42	99	58	51	256
2	7	48	113	57	31	256
3	12	53	115	55	21	256
4	13	76	103	50	14	256
5	25	78	106	37	10	256
Total	63	297	536	257	127	1,280

the records of two hundred fifty-six students of both sexes and all college years. The table shows the distribution of grades for the two hundred fifty-six courses placed first in interest, then for the two hundred fifty-six courses placed second in interest, and so on for the third, fourth, and fifth places. The grand total of twelve hundred eighty must accordingly be read as *student-courses*. The coefficient calculated from this table is .25.

The relationship between rank in interest and estimated rank in ability is shown in a similar manner in Table III. Each row of figures gives the distribution in ranks of estimated ability for a given rank in interest for 291 students. The coefficient calculated from this table is .57, which agrees very closely with the similar coefficients for the psychology and English courses.

TABLE III

RELATION BETWEEN RANK IN INTEREST AND ESTIMATED RANK IN ABILITY. ALL COURSES COMBINED

Rank in Interest	Estimated Ability					Total
	5	4	3	2	1	
1	9	19	49	63	151	291
2	18	44	72	94	63	291
3	37	69	69	66	50	291
4	63	102	65	45	16	291
5	164	57	36	23	11	291
Total	291	291	291	291	291	1,455

The low correlation so far indicated between rank in interest and grade and the relatively higher correlation between rank in interest and estimated rank in ability point to the mutual dependence of judgments of interest and of ability. A low correlation between estimated rank in ability and grade might accordingly be expected. Data presenting the actual relationship are given in Table IV, and the coefficient was found to be .28, which is very nearly the same as the coefficient for rank in interest and grade.

TABLE IV

RELATION BETWEEN ESTIMATED RANK IN ABILITY AND GRADE EARNED. ALL COURSES COMBINED

Estimated Rank in Ability	Grade					Total
	F	P	A	G	M	
1	3	26	97	58	44	228
2	7	48	82	62	29	228
3	11	44	112	41	20	228
4	12	61	105	39	11	228
5	20	76	86	38	8	228
Total	53	255	482	238	112	1,140

The relationship between rank in interest and grade and between rank in interest and estimated rank in ability was also determined for each sex separately, and for freshmen and upper class-men separately. The results show no significant differences. Indeed, all the coefficients calculated are surprisingly uniform—a fact which would seem to indicate their general validity. Table V, which sums up all the results,

TABLE V

COEFFICIENTS OF MEAN SQUARE CONTINGENCY (C)

	Between Rank in Interest and Grade		Between Rank in Interest and Estimated Rank in Ability	
	C	No. Cases	C	No. Cases
Psychology	.22	315	.59	158
English	.27	394	.57	194
All courses	.25	256	.57	291
All courses (males)	.26	171	.56	201
All courses (females)	.26	85	.54	90
All courses (1 yr.)	.25	157	.57	185
All courses (2, 3, 4, yr.)	.28	99	.50	106

will facilitate comparison. The coefficient obtained and the number of cases used in each calculation are given.

It will be noted that the contingency coefficients for rank in interest and grade range from .22 to .28, and for rank in interest and estimated rank in ability from .50 to .59. The product-moment coefficient of correlation in the former case would probably not be over .30 and in the latter not over .65. This latter figure is much lower than that obtained by Professor Thorndike (.89) in the experiment referred to above; but, as a measure of the relationship between interest and actual ability, it is probably much too high, and is merely a result of the subjective method of evaluating ability. When ability is measured by a more objective means, namely college grades, a very low correlation between interest and ability is obtained, so low in fact that one might well be justified in the statement: A person's relative interests are an extraordinarily *in*accurate symptom of his relative capacities. It might also be inferred from data here presented that a person's estimate of his ability is an extraordinarily inaccurate symptom of his real ability, for the correlation between the students' rankings of their courses in ability and the grades obtained is only .28.

There are, to be sure, many obvious objections to the use of college grades as measures of ability. First, a grade is also in part a measure of interest since the persistent application which earns the higher grades is based very largely upon interest. The effect of this would be to increase the correlation. That is to say, in so far as the grade earned depends directly upon the incentive supplied by interest, the coefficients given above are too high as measures of the relationship between interests and ability. Secondly, college grades are not sufficiently discriminative; and, consequently, a student may obtain the same grade in all his courses when his actual abilities are perhaps not so even. In so far as this is true, the coefficients reported are too low. Thirdly, grades are also dependent upon general intelligence (if there be one). This would tend to make the grades of each student uniform; and would have the same effect upon the correlation as the factor last mentioned. Finally, grades are also affected by other factors, such as personal relation between student and instructor, outside activities of the student, home environment of the student, and so forth. All these factors would, presumably, affect the correlation adversely.

The writers are therefore not ready to draw any certain conclusions from this short study regarding the actual relationship between interests and abilities. The problem is an extremely complicated one; and it cannot be solved until a more objective method of evaluating interests, as well as abilities, is forthcoming. Achievement, as has already been said depends upon both interest and ability. If these are not highly correlated, the conclusion of practical importance for vocational guidance is that *both must somehow be evaluated separately.*

VISUAL PHENOMENA IN THE DREAMS OF A BLIND SUBJECT

BY RAYMOND H. WHEELER

University of Oregon

The subject whose dreams are herein reported was a student at the University of Oregon from 1915–1918. He was a trained introspector and at the time of this investigation he had had several courses in psychology, including laboratory. After a preliminary period of training in recording dreams he found it possible to describe the important details in note form, in American Braille, immediately upon waking. These notes were subsequently edited by the writer with the help of the reagent, great care being taken to omit all uncertain or otherwise questionable details. The reagent lost his sight by accident when eleven years old. He is now twenty-seven.

Although the primary purpose of this paper is to report visual phenomena in the dream life of a blind subject after sixteen years of blindness, it is interesting to note, in addition, that in his dreams we find a peculiar association between visual images and images in other modalities. In his waking life the subject has very complicated synæsthetic phenomena. It has been noted in the literature[1] that in certain instances those individuals who possess associations between visual and other sensations in their perceptual processes also associate these modalities in a similar fashion in their imaginal processes. This is also true of our reagent. As far as the writer knows, however, such phenomena in dreams have not been described in the literature. For the convenience of the reader the reagent's descriptions of these associations are printed in italics.

Dream I

First I had a rather confused visual image of a portion of a room including one large window and the surrounding walls. The light which came through the window

[1] To be reported in a subsequent paper.

was dim, giving the appearance of a heavy fog or thick dust which seemed to fill the room. I could distinctly "see" the rays of light penetrating through the fog. The space in the room, penetrated by the light, seemed to be about six feet wide and three feet deep. *Accompanying this visual imagery was a marked unpleasantness*, a vivid organic and kinæsthetic experience consisting of a tension in the muscles of my arm, of a tightening in the vocal cords and of contractions in the muscles of my jaws, the latter resulting in a state of marked rigidity. Then I had a sense of "half pressure and half buzzing" in my ears and a diffuse and vague tension in the muscles of the brows, forehead, neck and chest. *The unpleasantness and kinæsthetic tensions were linked, in consciousness, with the foggy, yellow light.* The second group of experiences constituted an awareness that I was near the walls of the room.

Then I suddenly found myself in a second room in a house some distance away and to the south of the first. Here my imagery was somewhat similar to the preceding but lacking the yellow light and the affective accompaniments. I infer that this house was strange for there was nothing familiar about it. I was then conscious of looking from where I now stood toward the direction of the house I had previously been in. *Simultaneously with this change in my line of regard I had a vague flash of straw-colored light.* This meant "south" to me, which I innervated in vocal-motor fashion: "south." (My notion of "south" is associated, in waking consciousness, with straw-colored light.) I then noticed two friends in the room. Both were sitting to my right and a little in front of me. To my left and very near me was localized a very dark mass, somewhat of the size and general shape of a person, with a thick, hazy area as a fainter background. The lines of his shoulders, chest and arms to the elbows were the only distinct features of the figure. Instantly I had the faintest tendency to turn in his direction. This was the first intimation that I had had of being in this house with a companion. *I was unable to recognize him for the color of the imagery was too faded.* (In waking life I always identify people, in imagery, by their color.)

The friend to my right, seated nearest me was a vague form in sitting position, colored a very deep shade of blue. The only features which approached distinctness were outlines of his head, arms, legs and trunk. I imagine that the form appeared much the same as a person might look through a thick blue lens with the object much out of focus. *The other figure was fawn-colored yellow, of medium brightness and rather poor saturation.* I identified both of these persons by their colors.

Then there appeared a very irregular image of a large oak chair finished with yellow wood and black leather upholstering. The imagery was localized and distributed in space in a position corresponding to the details of a chair which are visible to the eye when one is sitting. I also had tactual imagery of the "feel" of the upholstering and kinæsthetic imagery of the bodily position assumed in sitting in this style of furniture.

I was then conscious of saying: "Hello boys; when did you get in?" There was no answer. My attention then became more rigidly fixed upon the two visual forms as I thought to myself in verbal imagery: "Why don't they answer?" I then spoke again: "I want you fellows to come over and see me while you are here." Again no answer. I then became very angry; I was conscious of intense tightening of muscles in my arms and chest together with a characteristic tension in the back of my scalp. (This latter is characteristic of anger in waking life.) *With the growing anger the colors of the two forms became very much brighter.* I then turned to the figure at my left, which represented my companion, and said: "Let's go back." I then found myself back in the first house but this time in a different room, for the window was on the west side and there was a telephone on the wall. The window curtains were streaks of drab-grey, which, I suspect, is my representation of white. The room was large, which I interpret from the fact that I experienced no pressure images. My awareness of the telephone consisted of visual and tactual imagery. I "saw" the brownish wooden box containing the mechanism of the telephone. In the middle of the transmitter was a circle of rather brilliant light which told me the exact place into which to direct my voice. The hard rubber mouthpiece was visualized as black; the receiver hook was a shining steel-gray as were also the bells. The outlines of these objects were fairly distinct but fleeting. I then had the vocal-motor: "I will call up the boys and ask them over to see me." I had kinæsthetic and tactual imagery of taking down the receiver and of holding it to my ear, with a distinct image of coldness as the edge of the receiver came in contact with my ear. No further imagery appeared until the vocal-motor: "They won't answer," whereupon I was extremely disappointed and angry. Here I had the characteristic sinking experiences localized in the region of the diaphragm, inhibition of breathing and tensions in the throat and chest. I then had the vocal-motor: "You can go straight to the devil." *At this moment I became conscious of a person in back of me, visualized as a bright and silvery form,* which meant to me that the form was a woman. I then had auditory imagery of her voice: "He has drowned in the creek." Simultaneously with the "he" there appeared *a dark visual schema to my right, very indistinct and not sufficiently colored for identification.* I next found myself searching for the creek. I was visualizing myself walking up a slope along a winding path. My consciousness of surrounding objects consisted only of kinæsthetic imagery of shrinking, dodging or otherwise avoiding branches, rocks and trees. I was distinctly conscious of an awkwardness with tensions in the trunk, legs, face and shoulders, all of which contributed to an awareness that I was uncertain where to step. I then found myself at a gate. I stepped back and watched a visual image of myself looking over the gate up the hill. Suddenly I lost the "visual me" and was looking at the gate at close quarters. The transition was almost instantaneous. I now visualized the two gate posts distinctly, together with the braces attached to the top of each post. Just beyond the braces was the woven wire, silvery in color, as if it had been galvanized recently. I was then conscious of fingering the wires, at which instant the visual imagery became more distinct. I was aware, next, that just beyond the gate was a hill. I did not visualize the hill distinctly; it was merely a brownish haze—a color which represents rank undergrowth to me; but I was distinctly conscious of tactual and auditory imagery of being in the shadow of a hill. The tactual experiences refer to changes in temperature and the auditory to changes in echo. I retraced my steps down the hill and while on the way noticed a group of oak trees which I had "seen" on my way up. I visualized their peculiar dark brown trunks, knotted and gnarled, and had tactual imagery of running my hand over the bark, covered with moss and lichens. I could see upward as far as the first branches but beyond that the trees faded into a hazy background, thence into nothingness. The brown of the trunks was faded and dim, the color irregular in distribution, giving the effect of a pouring rain on a window pane—a "wiggly" appearance.

At this point in the dream I was conscious of the person who went with me to the second strange house. This consisted of *a colored visual schema at my left, too vague, however, to identify,* and also of a peculiar motor "start," characteristic of a sudden consciousness of a person near you. We had a short conversation concerning the oak trees, the details of which I cannot remember. I recall that both his words and mine were in my own verbal imagery.

I next found myself at the edge of the creek, vividly aware that I was facing the creek and that the first strange house I had entered was back of me. *This was present in terms of a faint brownish haze off at the horizon back of me.* The water in the creek was sluggish and a dark opaque, oily green. The water almost overran the banks. Next to the edge of the stream I visualized masses of dead, pale, straw-colored grass, most of which was very hazy except for the larger tufts in which latter I seemed to "see" some of the individual blades. The grass lined both banks of the stream and extended over into the water. Suddenly I "saw" a collar and necktie floating down the stream, too far out to be reached. I viewed them indifferently until I had the vocal-motor: "It is *A's* collar!" Upon experiencing the verbal imagery I was conscious of intense grief, represented by marked feelings of stuffiness in my chest and by tendencies to sob. I awoke and actually found myself sobbing. Although I did not notice it particularly at the time, I am certain, on recalling the imagery of the collar, that it was white. The tie was blue with large brown bands running diagonally across it. I could not "see" those portions of the objects below the surface of the water.

Dream 2

I was seated in a passenger coach, midway between the ends, on the right side of the car and next to the window. I could "see" the vague form and outline of the sun-bleached red plush seat directly in front of me. For some little distance in front I could distinguish the yellow woodwork between the windows but farther down the coach the woodwork became very indistinct. A brilliant light was shining through the windows, illuminating the coach. Mixed with the visual images of the plush seats were tactual images of the friction of my trousers as they adhered to the plush, preventing me from making slight movements with ease. I also had clear tactual imagery of my arm as it rested in the sill of the car window.

The objects in the surrounding country were vague and fleeting and seemed to pass by very rapidly. They were of the brightness and tint of vegetation in the sandy desert. Now and then I was conscious of vague outlines of the rolling hills, stretching out in the far distance. I was travelling northeast, *indicated to me by a large area of dense blackness which was projected beyond the side of the car off to the front and to the left.* This dark or black horizon meant north. The relation of the black schema to the side of the car I interpreted to mean northeast. Then I noticed that my position in the coach had changed. I was riding backward. *The dark cloud was now at my back and to my right.* I became conscious that my brother was with me. This was in terms of *a gray splotch about the size of a man, in sitting position opposite me.* My brother said: "We are coming to the Blue Mountains." Although he seemed to be speaking these words, for I was listening in his direction, the words themselves came to me in my own verbal imagery.

Looking out of the window to my left I visualized a range of steep mountains rising abruptly from the desert. Here the brightness of the landscape greatly increased; various vaguely outlined hills passed by the window rapidly; the hills toward the south were dark blue, fading into a dull grey the nearer they were to me. Mixed with the awareness of this blue schema was a tactual image of tall thick grass soaked with rain. As the number of hills increased I was conscious of a distinct feeling of relief, referred to muscular relaxations about the brows, eyes and jaws. The thicker the hills became, the more marked became the relief until it merged into wonder and surprise. The muscles of my chest became tense; I was conscious of tendencies to smile. I noticed that on the sides of the nearest hills there were dark patches. Then I vocalized my brother's words as he spoke again: "The dark places are where they graze their sheep. The dry and sandy places are where the coyotes live." Following these latter words I had fleeting visual images of a comparatively level stretch of ground, covered with innumerable greyish rocks, scattered thickly over a faded, yellowish-brown soil.

Dream 3

I was in the middle of a river, astride a log. All about me the water for twenty or thirty feet was jet black and so dense that I could not "see" below the surface. The log was about eight inches in diameter and quite distinctly visualized as an old alder snag which was smooth, with no bark and a very light grey in color. The size of the log is an interpretation partly from the visual image and partly from the extent to which it protruded out of the water. Its smoothness was present to consciousness partly as an interpretation from the visualized surface and partly from the fact that my legs were adjusted in such fashion as to prevent my slipping on the surface.

I had very distinct motor imagery of balancing myself. This involved my legs, trunk and arms. I judge that I was about one third of the distance back from the front end of the log which I gather from the manner in which the log responded to my movements. I was going down stream rapidly. This latter consciousness came to me in terms of a visual image of a dark, ribbon-like streamer indicating how the water was being "cut" by the passage of my feet through it. I also had tactual imagery of water rushing rapidly by my hand, an experience similar to the sensations one receives when placing his hands in the water as he rides in a fast boat. Together with this latter visual imagery was auditory imagery of the swishing of the water. I do not know how fast the current was flowing but I seemed to be travelling faster than the stream. I had tactual-kinæsthetic imagery of being hurled rapidly through space in the direction of down-stream.

All about me, especially to the left and in front, small fish, about ten inches long, were continually jumping out of the black, inky water. They leaped only a few inches above the surface, looked at me and disappeared with a croak. I could not visualize the fish clearly but merely got their general shape and size. Sometimes they would appear only on the surface of the water; they would extend their heads upward until their gills appeared when they would utter a peculiar sound and disappear again. In many instances I had *auditory imagery of their croaks together with a flash of yellow light, hovering, temporarily, about the region of their mouths. In other instances I was aware only of the yellow light, which, however, meant to me that they were making their peculiar sounds.* Some of the fish were striped grey and black, the stripes running across their sides from the lower front to the upper back, thus making diagonal bars across their bodies. I was next conscious of the vocal-motor: "These are singing carp." Then I had a visual image of a fish net lying upon the log in front of me, and the vocal-motor: "I have a net and will get some."

Dream 4

I was under the south end of a bridge. I was facing the south with the stream in back of me. I have a very distinct remembrance of visualizing the supports of the bridge as I looked through them *toward a cloud of yellow light off in the distance. This cloud of yellow light meant south.* I did not visualize the bridge above me but was aware in auditory terms of the faint echo and in tactual terms of the nearness of the bridge to me. I then had visual and kinæsthetic imagery of piling rocks into a gunny sack. I was greatly disturbed both by the fear of getting caught and by my bull-dog which kept barking, and pulling vigorously at my trouser's leg. Here the tactual and auditory imagery was profuse and clear. *The imagery of the dog's form was indistinct save for a bright straw-colored schema which always represents this dog to me.* I was holding one corner of the sack with my left hand (tactual, motor and visual imagery) and the sack itself was a dark yellowish-brown. I could not "see" the weave and did not notice any tactual imagery of it. Then there appeared the verbal imagery: "I want *A* to see these before anyone finds me." Then I was vividly aware of the fact that *D* might find me. This consisted of tenseness about the chest and abdomen, with "sinking sensations" in the region of my stomach. *D was represented to me in terms of peculiar flashes of color which corresponded to his voice.* I then awoke.

Three striking peculiarities stand out in the dreams given above. First is the appearance of associations between visual and other sense modalities, which, in every instance conform to synæsthetic phenomena in the reagent's waking life. Schematic forms of persons are identified by color. Persons' voices are recognized by their color; directions of the compass, the "croaking" of the singing carp, the bull dog, are all identified by certain colors. The reagent's auditory imagery is exceedingly deficient both in dream and in waking life but this deficiency is largely compensated for by visual associations or synæsthetic phenomena.

Secondly, our subject has the peculiar tendency to vocalize the spoken words of another person's voice in terms of his own vocal-motor imagery. Colors provide the necessary qualitative differences for identification. This is true both of waking and of dream consciousness. Verbal imagery is very rarely syncopated or abbreviated and is very definite in details of enunciation and of expression.

Thirdly, there is to be noticed a peculiar tendency, in the subject's dreams, to visualize himself at a distance. Various details, which the writer has been able to obtain on this point, indicate that the subject often has a "visual me" in dreams. This "visual me" is evidently a product of repeated tendencies on the part of the subject to visualize himself as he walks about in order to ascertain, if possible, whether he showed his blindness in any peculiarities of walking. This "visual me" appears to him frequently, in dreams, if he is conscious of being watched. The visualized figure consists mostly of hands and feet, separated and in movement as if in walking. Peculiar feeling complexes are associated with this visual imagery but otherwise the experiences have not been definitely analyzed. Occasionally this "visual me" appears close to the subject but it is usually localized some distance away.

Visual imagery still outnumbers other modalities both in the subject's dreams and in his waking life. This imagery has now become vague in form and outline. For example, persons lack outlines of eyes, mouth, ears, fingers, and details of trunk and legs. The size and duration of the imagery, however, have remained practically normal. Visual imagery of small objects similarly lacks definiteness but possesses a greater degree of clearness than does visual imagery of larger objects. This is undoubtedly due to the fact that the act of handling small objects makes it easier for the reagent to visualize them. In fact, when the subject wishes to visualize an object clearly he always endeavors to explore it in tactual-motor fashion. In the first dream it will be noticed that as soon as the reagent found himself fingering the wire in the fence, its color increased in vividness and the outlines of the visual imagery became more distinct.

All visual imagery lacks details as well as form and outline. The room visualized in the first dream was devoid of furniture. In like manner were lacking the details of the telephone, the lace curtains, the chair, the scenery along the path, the trees, bushes, clothing, etc. A hill is a mass of color with light and dark patches; mountains are clouds of color; water is lacking in detail of wave or brightness; trees are visualized only in part, and so on.

Taking the place of details in visual imagery are auditory-vocal-motor, tactual and kinæsthetic-organic experiences. For example, the visual imagery of oak trees in dream 1 is pieced out with tactual imagery of exploring the surface of the trunk. The visual imagery of being seated in a chair is supplemented by tactual imagery. The same is true of talking through a telephone.

Kinæsthetic imagery is exceedingly clear and persistent. In many instances the reagent has hesitated in calling these experiences "images" owing to their vividness, and distinctness of localization. Such imagery is very readily recalled. Organic complexes constitute an important feature of our reagent's dreams. While experiencing joy, for instance, he feels the characteristic changes in respiration, the tendencies to smile, and the pleasantness; while in fear he is able to detect the tensions in the throat, the organic changes from the regions of the stomach and diaphragm, and the altered breathing. Again, in anger he is aware of the jaw tensions, the tightness of arms and hands, and the changes in facial expression, all of which seem to be exact copies of experiences in waking life. Organic processes seem to be recalled with greater readiness than visual details.

We believe that the above descriptions demonstrate that the introspective method—at least the terminology—can be applied in the description of dreams. Whereas it may be contended that the chief interest in dreams pertains to their function rather than to their content, it is obvious that the significance of dreams can be much better understood if their content is first described in minute detail. Such descriptions should be obtained by using a method patterned after introspection rather than after interpretation.

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THE PSYCHOLOGICAL REVIEW

THE PHYSICAL BASIS OF NERVE FUNCTIONS

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I. Introduction

Modern experimental investigations of the laws governing nervous action, combined with theoretical studies based upon these investigations, provide us with the materials for a very definite and satisfactory conception of the physical mechanism of the process. It is the purpose of the present article to sketch a rough picture of the probable nature of this mechanism, in the hope that the sketch may prove valuable to some psychologists who may not have followed closely modern developments in this field. I do not wish to claim any essential originality for the general ideas to be presented, although it is my belief that the total picture which I shall outline has not been offered heretofore and that some of the suggestions which will be made concerning details are new. My views are founded mainly upon the writings of Nernst,[1] R. S. Lillie,[2] and Lucas.[3]

II. The General Mechanisms of Excitation and Stimulation

It was for a long time suspected that the nerve impulse is essentially an electrical phenomenon, since the electrical

[1] Nernst, W., 'Zur Theorie des elektrischen Reizes,' *Arch. f. d. ges. Physiol.*, 1908, 122, 275–315; and other papers.

[2] Lillie, R. S., 'The Relation of Stimulation and Conduction in Irritable Tissues to Changes in the Permeability of the Limiting Membranes,' *Amer. J. of Physiol.*, 1911, 28, 197–223; and other papers.

[3] Lucas, K., 'The Conduction of the Nervous Impulse,' London, 1919. Also many papers.

action current of a nerve provided the only direct evidence of the existence of the impulse. Paradoxically enough, this index of excitation consists not in the appearance of a state of electrification in the nerve but rather in the partial disappearance of such a state which is already present in the resting cell; and which manifests itself through the 'current of rest' or 'demarcation current,' obtained when the cut end or injured surface of the nerve is connected through a galvanometer to the nerve sheath. The current flows from the sheath to the injured portion, indicating that the former is positively charged with respect to the latter. When the nerve cell is excited this positive electrification of the sheath decreases momentarily.[1]

The electrical theory of matter, and in particular the so-called theory of 'electrolytic dissociation'[2] offers us the materials for a simple explanation of the resting electrification of the nerve. A nerve fiber, like almost any other living cell, consists of an aqueous solution of many substances, both organic and inorganic. Among these are substances which become *ionized* in solution, that is, their individual molecules are broken up into electrically charged particles, positive and negative ones being produced in equal numbers. Prominent among ions of this sort are to be counted those of carbonic acid, the substance which is produced by the respiratory function of the cell. The ionic particles, in common with all of the molecules of the cell, are endowed with a rapid vibratory motion which represents the temperature or thermal energy of the substance. In accordance with the general theory of solutions, the motion of the dissolved particles among the molecules of the solvent, water, resembles that of the molecules of a gas in free space. This means that they must exert a pressure upon any surface which tends to interfere with their free diffusion.

[1] A simple account of the electrical phenomena exhibited by nerve and muscle will be found in Howell, W. H., 'A Text-Book of Physiology,' 1909, Chap. IV., pp. 96–110.

[2] For a simple presentation of the essentials of the modern electrical theory of matter, reference may be made to the book by the writer and D. F. Comstock, 'The Nature of Matter and Electricity,' 1917. On 'electrolytic dissociation' see pages 139–141, and on pressure due to molecular motion see pages 106–109 of this work.

The liquid mass of the cell is contained within a membrane which may be conceived as a more or less solid envelope. This membrane is not necessarily to be identified with the myelin sheath in the case of the nerve fiber, and it may simply consist in some peculiar condition of the molecules at the surface of the general cell mass. In general the bounding surfaces of liquid masses behave like actual membranes. Now it is obvious that the ions in the general cell mass will exert an outward pressure on this membrane, especially as there is probably an aqueous medium outside as well as inside of the cell. If the membrane is absolutely tight or *impermeable* to these ions there will simply be a tendency to distend it, but if on the other hand the membrane is somewhat porous certain of the ions may pass through. It is clear that small ions will pass through more readily than large ones, and in the case of those of carbonic acid the hydrogen ions will almost certainly be the smaller. Hydrogen is the smallest of the atoms and its positive ion is probably the smallest of all known physical particles. If bare hydrogen ions occur in solution it is probable that no membrane could possibly exist which would be capable of holding them back, since the membrane must itself have a molecular structure. The carbonate ions must be very much larger than the ions of hydrogen, not only because they contain a number of atoms in combination instead of one, but because the atoms themselves are many times larger than those of hydrogen. It is, therefore, almost *a priori* certain that if a membrane is relatively impermeable the hydrogen ions will still pass through it, while the carbonate ions will be held back.

Since the hydrogen ions are by nature positively charged, the outcome of this selective diffusion through the cell membrane must evidently be to form a layer of positive electrical particles on the outside of the cell. On the inside of the cell there will be a corresponding layer of negative particles which are held in position by the attraction exerted upon them by the external positive layer. This attraction is mutual and not only causes the diffused positive ions to remain in the immediate vicinity of the membrane but limits the number of them which can pass through the membrane, since when their concentration on the outside reaches a certain limit there will be just as many returning into the cell per unit time as leave it. Thus a definite state of equilibrium is established which involves an 'electrical double layer,' of a magnitude depending upon the exact permeability of the membrane and the internal concentration of the ionized substance. The existence of such an electrical double layer at the membrane may be said to constitute a *polarization* of the latter, this polarization being capable of variations in degree under different conditions.

It is clear that the polarization of the cell membrane just considered explains the demarcation current, even the direction of this current corresponding to that theoretically deduced. When an electrical connection is made between an injured and an uninjured portion of a nerve, this connection is virtually established across the cell membrane so that the mechanism of the membrane acts in a manner similar to an electric 'battery.' The energy of this 'battery' is derived from the compression of the ionized substance inside of the cell, and the continued flow of the demarcation current must involve a diffusion of some substance, *viz.*, hydrogen, through the cell wall. This process is similar to that which occurs in the special form of electrical 'battery' known to physical chemists as a 'concentration cell.'

The next problem which we face is that of determining the *mechanism of stimulation* of the resting cell. It is a familiar fact that an externally produced electric current or voltage provides a very ready means of stimulating nervous tissue. The great sensitivity of such tissue to electric currents is by itself almost conclusive evidence of the essentially electrical nature of the nerve process. The stimulating power of electrical currents depends, however, very radically upon the exact time relations of the current. An alternating current of the right frequency stimulates more readily than a direct current, but if the frequency is very high there may be no stimulation at all, even with very large currents. It is found experimentally that the intensity threshold for a given nerve is proportional to the square-root of the frequency. Nernst[1] succeeded in explaining this relationship mathematically on the basis of an assumption as to the physical conditions underlying the threshold. He supposed that in order just to stimulate a nerve cell, the current acting upon the cell must polarize the cell membrane to a certain critical degree, this polarization being a natural result of a current flow, which piles one kind of ions up on one side of the membrane and draws them away from the other side. By combining this assumption with the known laws of electrical action and diffusion Nernst arrived at a law connecting the time required to stimulate with the intensity of the stimulus. Although Hill,[2] and others have found it necessary to complicate Nernst's original theory by further details, the quantitative success of his original hypothesis stands out as a unique achievement in the application of mathematical physics to biological phenomena.

It is evident, however, that Nernst's assumption that the physical basis of the nerve threshold is the establishment of a certain degree of polarization in the cell membrane must be modified to fit the fact that the membrane is already polarized before stimulation. It was pointed out by R. S. Lillie[3] that this modification might take the form of a mere change of algebraic sign; in other words, stimulation of the nerve may require a critical *de*polarization of the membrane, this depolarization although definite in amount not necessarily being complete. Lillie further pointed out that this assumption is in harmony with many facts concerning conditions of stimulation of nervous tissue. It harmonizes firstly with Pflüger's law of electrical stimulation,[4] according to which there is stimulation at a negative pole (cathode) upon closing the circuit and at the positive pole (anode) upon opening the circuit, when both of these poles are applied to the

[1] *Loc. cit.*

[2] Hill, A. V., 'A New Mathematical Treatment of Changes of Ionic Concentration in Muscle and Nerve Under the Action of Electric Currents, with a Theory as to their Mode of Excitation,' *J. of Physiol.*, 1910, 40, 190–225.

[3] *Loc. cit.*

[4] For a statement of Pflüger's law or laws, see Howell, *loc. cit.*, pp. 89–91.

outside of the nerve. The cathode may be regarded as spraying the nerve with electrons,[1] which are negatively charged and which consequently combine with the positive particles on the outside of the nerve, neutralizing them and thus depolarizing the membrane. The anode, on the other hand, sucks electrons away from the outside of the cell, thus increasing its coating of positive particles at this point. Facts to be considered below lead us to suppose that under these conditions the membrane will react in such a way as to compensate for this increased polarization, so that when the circuit is opened and the auxiliary polarizing force of the anode is removed a depolarizing action will occur. It is a well recognized principle that anode stimulation at the 'break' occurs less readily than cathode stimulation at the 'make.'

Stimulation of nerve by many agents other than the electric current can also be explained on the assumption that the essential condition of stimulation is a certain depolarization of the membrane. One way in which to depolarize the membrane would be to place the cell in a solution containing positive and negative ions very similar in physical character to those within the cell itself. Under these conditions there would be just as much of a tendency for positive ions to pass into the cell as to pass out, so that the production or maintenance of polarization by the mechanism of selective diffusion above described would be impossible. This explains the stimulating power of various 'electrolytes.' It also explains why these substances, including carbonic acid, eventually bring about a permanent depression of activity or 'narcosis' in the nerve.

Another method which is obvious *a priori* for stimulating a nerve is to injure or destroy the membrane, which depolarizes the latter because negative as well as positive ions are permitted to diffuse through it. So-called mechanical stimulation of the nerve is evidently of this sort, depending upon a gross maceration of the cell envelope. The action of certain organic narcotics, such as chloroform, ether, alcohol, etc.,

[1] *Cf.* Comstock and Troland, *loc. cit.*, pp. 24–25.

which first excite and then depress the nerve, probably depends upon a *chemical* destruction or impairment of the membrane. The stimulating effect of other conditions, such as desiccation and heat, can be explained in similar terms.[1]

III. The Specific Mechanisms of the Threshold Impulse Propagation, and Other Neural Properties

It is evident that the Nernst theory of stimulation in the modified form outlined by Lillie demands a 'negative variation' of the 'current of rest,' as empirically found, since depolarization would necessarily manifest itself in this way. It does not follow, however, from any statements made heretofore that this negative variation will be *propagated* along the cell or fiber from the point of stimulation. Lillie has pointed out that to explain propagation of the variation we must suppose not only that the polarization of the membrane depends upon its differential permeability but that its relative impermeability depends upon its degree of polarization. Polarization and permeability, in other words, are the two essential factors in the nerve process, and they are related to one another in the propagation of the nerve impulse in a manner analogous to the relation of pressure and displacement in the propagation of sound[2] or the relation of the electric and magnetic vectors in the propagation of light.[3] These relations are all such that a change in one of the quantities always involves a change in the other, the locus of the secondary change not coinciding with or falling wholly inside of that of the primary change. This relation necessitates propagation.

It is easy to see why the degree of polarization of the nerve cell membrane should depend upon its degree of permeability, since it was from assumptions regarding the nature

[1] On the various means of exciting nervous tissue see Schäfer's 'Text-Book of Physiology,' 1900, pp. 459–468.

[2] See, *e.g.*, Duff, A. W., 'A Text-Book of Physics,' 4th ed., pp. 518–519; or Poynting and Thomson, 'A Text-Book of Physics,' 1904, 2, pp. 12–14.

[3] See, *e.g.*, Houstoun, R. A., 'A Treatise on Light,' 1915, Chap. XXII. This is a mathematical exposition; a simple, qualitative treatment is difficult to find.

of this permeability that we were enabled to deduce the existence of polarization. It is not so obvious, however, why the *permeability* of the membrane should alter when some external force, such as an electric current, brings about its depolarization. A moment's consideration, however, will show that such artificially produced depolarization should have some effect upon the constitution of the membrane. When the polarization is present the substance of the membrane is subjected to electrical stresses which are represented by parallel lines of force connecting the positive charges on the outside of the membrane with the negative charges on the inside. In accordance with the general electrical theory of matter these electrical stresses will necessarily produce some distortion of the molecular or atomic structure of the membrane, which distortion will have a direction determined by the impressed electrical forces. When these forces are removed by depolarization this distortion will tend to disappear. The distortion may well be of such a character as to render the membrane less permeable than would be the case if such distortion were absent. In this event, the depolarization would have the effect of increasing the permeability.

One conception of the molecular structure of the membrane which has occurred to me, pictures the membrane molecules as being considerably longer than they are wide, with opposite electrical charges on either side in the middle of the molecular length. The individual molecules are conceived to be rotatable, like compass needles, about their centers of mass. The electrical field produced by the polarization will then tend to dispose all of these molecules with their long axes at right angles to the field, that is parallel to the plane of the membrane, and in this position they will offer maximal obstruction to any particles tending to move perpendicular to this direction. When the depolarization field is removed, however, the molecules will tend to swing into position at right angles to the one just considered, on account of the mutual attractions and repulsions of their charges. In this position they will offer much less resistance to the passage of particles perpendicular to the plane of the membrane. This mechanism is of course only symbolic, but illustrates the general principles which are involved.

Apart from purely physical reasoning, we have excellent biological grounds for believing that stimulation of a living cell will result in an increased permeability of its enveloping membrane. It is through modifications in the permeability of this membrane that the cell regulates the income and outgo of chemical materials. Food substances absorbed from the environment and waste products excreted must pass through the membrane, and it is to be expected that both of these transfers of substance will be increased during a state of excitation of the cell, since this latter state involves an increased chemical activity. Lillie has cited a number of cases in which an increased permeability of the cell membrane resulting from stimulation can be clearly demonstrated. The recent work of Tashiro on the liberation of carbon dioxide by stimulated nerve fibers is evidence in the same direction, and also corroborates the idea that the ions which are involved in the polarization of the nerve cell membrane are those of carbonic acid.

Tashiro[1] finds that a small amount of carbon dioxide is given off by the nerve fiber even in the resting state but that this amount is markedly increased during stimulation. This indicates that the normal permeability of the membrane is such as to permit a slight diffusion of carbonate ions and that the depolarization accompanying excitation is the result of an increased permeability to these ions rather than a decreased permeability to the positive hydrogen ions. Our initial theory regarding the cause of the polarization of the cell membrane demands that an increase in the permeability should result in a decrease in the polarization provided the permeability is already practically perfect for the positive ions. That this requirement is fulfilled is indicated not only by the slight diffusion of carbon dioxide during the resting state but also by the free permeability of the membrane at all times to water and the ions of water.

[1] Tashiro, S., 'Carbon Dioxide Production from Nerve Fibers when Resting and when Stimulated,' *Amer. J. of Physiol.*, 1913, 32, 107–136.

The facts which we have just been considering show that the reciprocal relationship between permeability and polarization is a mutual or 'symmetrical' one. In other words, these facts prove that a decrease in polarization beyond a certain critical point results in an increase in permeability. The analogy between the principles of propagation of nervous energy and those of acoustic and radiant energy is thus very close, a change in either one of the two principal variables resulting in a reverse change in the other variable. The reciprocal relationship between these two factors in the case of the nerve impulse is not, however, absolutely symmetrical. It is almost certain that any initial change, no matter how small, of the permeability will result in a reverse change of the polarization, but the fact of the threshold indicates that an alteration of the polarization does not bring about an increase in the permeability until a certain critical depolarization has been developed. Indeed certain phenomena which we shall soon consider indicate that before this critical point is reached, there is a tendency for the permeability of the membrane to decrease below normal as a result of depolarization. The polarization and the permeability of the membrane appear to enter into a system in which there is a point of unstable equilibrium.

The nature of this system can be illustrated by a mechanical analogy which is effective as a classroom demonstration. Suppose that we tip a chair gradually forward by applying a finger to the back of the chair. At first the weight of the chair resists the tipping force, but when the center of gravity passes over the very small base of support the chair parts company with the finger and falls to the floor with a crash. The tipping of the chair up to the point of unstable equilibrium represents the changes in the character of the membrane system which must be produced in order to pass the threshold, while the crashing of the chair to the floor, once the equilibrium point has been passed, represents the liberation of the internal energies of the nerve cell which constitutes the state of excitation. The degree of energy thus liberated is evidently dependent almost wholly upon the inherent nature of the system and not upon the intensity of the stimulus or tipping force.

The laws of the nerve membrane system, formulated in more exact terms, would probably read somewhat as follows. An increase in permeability always produces a decrease in polarization. An initial decrease of polarization, however, first results in a decrease in permeability which tends to compensate for the initial change. But if the depolarization reaches a critical or threshold amount the 'sign' of the change is reversed, so that an increase in permeability results. As soon as this increase begins, the initial depolarization is further augmented by the law which makes polarization depend reciprocally upon permeability, so that the equilibrium of the nerve membrane system is completely upset, the polarization now decreasing to a minimum while the permeability increases to a maximum.

The supposition that the first reaction of the membrane to a decrease in polarization is a decrease in the permeability rather than an increase is in harmony with a variety of facts concerning the nerve function. It explains among other things DuBois-Reymond's law of electrical nerve stimulation, according to which a nerve is stimulated only by a *change* in an electrical current and not by a steady flow of electricity. The investigations of Waller,[1] Hill,[2] Lapicque[3] and others have shown that there is a certain rate of application of an electrical voltage which stimulates the nerve with the least total expenditure of energy. The failure of a slowly applied electric voltage to stimulate—at the cathode—can be understood if the first action of the membrane is in the direction of adaptation or compensation, the mechanism of which involves a decrease of the permeability below normal; thus tending to increase the polarization above normal or to maintain it at normal in the face of an external depolarizing agency. Such an action is evidently a physical possibility if, as we have found reason above to

[1] Waller, A. D., 'The Characteristic of Nerve,' *Proc. Roy. Soc.*, 1899, 65, 207–222.

[2] *Loc. cit.*

[3] See Lapicque L., and Legendre, R., 'Relation entre le diamètre des fibres nerveuses et leur rapidité fontionnelle, *Comptes rendus*, 1913, 157, 1163–1165.

suppose, the normal permeability does not completely interfere with the diffusion of the negative ions but simply impedes their movement to a certain degree. If the external depolarizing agency is applied very rapidly the membrane does not have time to develop its compensating reaction, so that the threshold depolarization is reached and excitation results.

We have seen that the 'break' stimulation at the anode is best explained on the assumption that the membrane tends to compensate for the excess of positive ions at the anode by decreasing its permeability at this point. This is apparently another instance of the general tendency of the membrane to adapt itself to a stimulus by permeability changes so as to maintain the resultant polarization constant. Stimulation at the anode upon interruption of the current will result only if this interruption is sufficiently quick, since if it is slow the membrane will have time to compensate again and the resultant change in the polarization will be insufficient to cause stimulation.

These views regarding the changes in the condition of the membrane at the cathode and anode respectively should lead us to look for some further specific effects of such modifications. The phenomena of 'electro-tonus'[1] are of this sort. It is to be anticipated that the stimulability, or the stimulation threshold, of the nerve will depend upon the condition of the membrane, in particular upon the dynamics of the equilibrium between its permeability and its polarization, so that a compensating change in the permeability will result in an alteration of the threshold. It is a familiar fact that the stimulation threshold decreases at a cathode while it increases at an anode, indicating in terms of our theory that decreased permeability results in increased stimulability and *vice versa*.

The above deductions are further borne out by a study of the alterations in stimulability which take place during and after any given process of excitation in the nerve fiber. It is well known that after such a fiber has once been stimulated it cannot again be set into excitation until a certain interval

[1] See Howell, *loc. cit.*, pp. 88–89.

has elapsed, this interval being called that of *refractory phase*.[1] Refractory phase is evidently represented in our physical hypotheses by the state of collapse of the membrane which results from the attainment of the threshold depolarization. If excitation consists in an increase of permeability to a maximum, then it is clear that further excitation is impossible until the original impermeability has been at least partially restored. So long as no restoration whatever has occurred the cell remains in what is known as *absolute* refractory phase.

The fact that this condition is only temporary proves that a mechanism of restoration of the normal condition of the membrane exists, this mechanism probably being identical with that which enables the membrane to react in a compensating manner to small depolarizing forces. The recovery is gradual and while it is in progress the cell is in a so-called state of *relative* refractory phase. In this condition its stimulability is depressed below normal to a degree which is greater the less the recovery which has been achieved at any instant. Since the recovery must consist essentially in a progressive decrease in the permeability of the membrane these facts evidently correspond with the view that stimulability is inversely proportional to the permeability in question or that the threshold of stimulation is directly proportional to it.

It is an empirical finding that the relative refractory phase is followed by a *phase of hyper-excitability* during which the threshold of stimulation is *lower* than normal. This effect can evidently be attributed to an 'over-shooting' of the permeability decrease which constitutes the recovery of the membrane. Such 'over-shooting,' according to our assumptions, should be accompanied by an increase in the stimulability of the nerve above that obtaining in the normal condition. The 'over-shooting' may be viewed as a delayed consequence of forces of compensation set into action by the initial operation of the stimulus.

[1] On refractory phase and the course of excitability after stimulation, in general, consult Bayliss, W., 'Principles of General Physiology,' 1915, pp. 389–390.

The above considerations are evidently in harmony with the facts which indicate that a nerve impulse set up during a state of refractory phase has a smaller amplitude or intensity than a normal impulse, while one which is generated in a phase of hyper-excitability possesses an amplitude or intensity greater than normal. The 'amplitude' of a nerve impulse must stand either for the amount of change in permeability which results from stimulation or for the amount of depolarization which is a consequence of this change. It should be clear why these amplitudes vary with the exact level of permeability which exists at the instant of stimulation. If a nerve impulse is represented as a wave of permeability change referred to normal permeability as a base line it is evident that a decreased amplitude is represented by an elevation of the troughs of the waves above the base line, whereas an increased amplitude must be referred to a depression of these troughs below this line. The 'all or none' principle, which we shall consider more specifically below, may be interpreted as to mean that the crests of all waves lie on a constant locus which represents the invariable maximum permeability of the membrane.

IV. The Energetics of Nerve Processes

It is almost a necessary consequence of the above outlined theory of the physical nature of nervous activity that such activity should involve metabolism. It seems almost inevitable that the process of restoring the nerve membrane to its rest condition after excitation should require the expenditure of new energy. This energy would naturally be obtained by the *oxidation* of some substance present in the cell. It is a fact that oxygen is required in order to maintain the nerve in a state of excitability. A nerve which is caused to function in the absence of oxygen eventually falls into a state of permanent refractory phase, exactly what would be expected if oxygen is necessary in order to rebuild the membrane.

It is a remarkable fact that although oxygen is required in the nerve function there is apparently no generation of heat due to the activity of the nerve.[1] Very sensitive heat detecting instruments capable of recording the heat produced by the oxidation of a single molecule in a portion of space easily visible under the microscope have failed to indicate any heat production whatsoever. This fact has led some investigators to believe that the oxygen is not required for metabolic purposes but has some other function, such as one of catalysis. They do not make clear, however, why in a non-metabolic function the oxygen should require constantly to be replenished. The idea that the oxidation actually is employed in an oxidative process may possibly be reconciled with the absence of heat production in the following way. The process of excitation, according to the theory herein considered, involves a diffusion of carbonic acid through the cell membrane and this diffusion, being of the general nature of evaporation, should have a *cooling* effect upon the cell. This effect is immediately followed by the oxidative change, and the heat generated by this latter change may be only just sufficient to counteract the cooling produced by the diffusion. It would be quite reasonable to suppose that these two quantities would almost exactly neutralize each other, owing to the generally cyclic nature of the process.

The apparent indefatigability of nerve tissue is probably a consequence of the fact that only a very small amount of energy is required in the nerve function, so that the substance of the nerve cell can supply this energy by oxidation during a very long period. It is clear that the oxidation which we have assumed to occur in the rebuilding of the cell membrane will tend to compensate for the loss of carbon dioxide from the cell which occurs during excitation, so that fatigue will set in only when the fundamental oxidizable material of the cell is exhausted. In *vivo* this substance will naturally be replenished, while in *vitro* the nerve becomes unfit for experimental tests due to other causes, such as desiccation, long before its metabolic fuel is exhausted.

Some views of the nature of the nerve impulse have

[1] On these points, *cf.* Bayliss, *loc. cit.*, pp. 378–379 and 390–391.

apparently regarded it as a true cyclic process in the thermodynamic sense.[1] Such a process, resembling the propagation of light in free space or of sound in a perfectly elastic medium, would involve no loss or gain of energy at any point, the original energy of the stimulus simply being transmitted from one part of space to another. It is inconceivable, however, that all cases of nerve functioning should be thermodynamically cyclic. The original process of stimulation apparently does not consist simply in an absorption of the energy of the stimulus by the nerve; on the contrary, the stimulus apparently serves merely to operate a trigger which releases energies latent in the nerve itself. Moreover, if propagation depends simply on the principle that one portion of the nerve can be stimulated by the excitatory state of an adjoining portion, it is natural to suppose that this trigger process is repeated at all points in the nerve during the propagation of the impulse.

It is, of course, conceivable that the energy released at the point of stimulation is simply passed along the nerve during the propagation without involving any further expenditure or release of energy. There are certain cases of propagation, however, to which this supposition can scarcely apply. One of these is propagation through a so-called 'region of decrement' in which the amplitude of the nerve impulse suffers a progressive diminution, but one which is completely recovered from when the impulse emerges from the region in question. We must suppose that the reduction of the amplitude of the impulse in such a region involves a loss of energy, and the restoration of the impulse to its original magnitude when it passes out of a region of decrement must involve the expenditure of new energy. Conduction through synapses apparently involves processes of the same general character.

Other reasons for refusing to believe that the nerve impulse is thermodynamically cyclic lie in the metabolic character of all other vital activities and in the nature of

[1] On the nature of a thermodynamically cyclic process consult Lewis, W. C. McC., 'A System of Physical Chemistry,' 1916, Vol. 2, pp. 29 ff.

the specific physical mechanism which we now believe is involved in the nerve process. This mechanism requires a change in the structure of the physical substance of the nerve membrane, a re-arrangement of its constituent atoms or molecules, and such changes always involve a degradation of energy. Nervous tissue undoubtedly has a lower degree of metabolism than any other living tissue, but this is not equivalent to saying that its metabolism is zero.

V. The Basis of the All-or-None Principle

Modern studies, mainly those of Lucas and Adrian,[1] have made it quite clear that the action of the individual nerve fiber follows a principle of 'all or none.' If a nerve cell is set into excitation at all its excitation is *ipso facto* the greatest which is normally possible for it. A stimulus of the highest intensity can cause no greater response than one of threshold intensity. The experimental demonstration of the validity of this principle for the individual nerve fiber has involved work of a very intricate and ingenious kind, but on the theoretical side it requires very little effort to see that the response of nervous tissue should be of the 'all or none' type. If what the stimulus does is to upset a condition of unstable equilibrium in the nerve membrane, the response of the nerve must depend upon its own inherent nature and not upon that of the stimulus. The old 'train of gunpowder' analogy for the nerve impulse involved an 'all or none' action, and the modern substitute for this classical mechanism makes the same theoretical demands.

It is interesting to note that the 'all or none' principle as applied to nerve activity forces us to think of such activity in terms of fixed units of energy, so that we have a system somewhat resembling that necessitated by the modern *quantum* theory of radiation.[2] The nerve process is *quantal*, or to use a more biological term, it is *isobolic*. The conceptions of atomism and discontinuity seem to be creeping into every branch of scientific analysis; not only are the chemical

[1] See Lucas, *loc. cit.*

[2] See Comstock and Troland, *loc. cit.*, pp. 46–49 and 182–189.

elements atomic but also electricity, light, the determinants of heredity, and finally the activities of nerve and muscle.

The acceptance of the 'all or none' principle for nerve action does not, however, imply that all nerve impulses are of the same magnitude. The magnitude or amplitude of a nerve impulse depends upon the characteristics of the nerve substance in which it occurs. These characteristics differ for different nerve fibers in their normal condition and for a single fiber in various abnormal, subnormal and supernormal conditions. They are different at synapses from what they are in the non-synaptic portions of the nerve.

In general, however, variations in the *quantity of nervous energy* transmitted along a given nerve in unit time must be conceived to depend upon the number of impulses or neural quanta which pass through a cross-section of the nerve during the time in question. This number is the *nerve impulse frequency*, and there cannot be the slightest doubt that the concept of impulse frequency is an absolutely fundamental one for the theory of nerve action. It is probably as important for the understanding of such action as is that of wave-length for the understanding of radiation phenomena. However, the characteristics of a given nerve current are not completely determined by a specification of its frequency, since the amplitude, the length and the form of the individual nerve pulse are not determined by frequency, although they may serve to limit the latter.

VI. The Mechanism of the Synapse

Facts summarized in a masterly way by Sherrington[1] indicate that nerve conduction through a reflex arc differs radically from conduction between two points in a single nerve fiber. A reflex arc always involves one or more *synapses* and all of the evidence points to the synapse as the locus of the factors which differentiate reflex arc conduction from simple nerve fiber conduction. Certain experiments of Lucas[2] provide us with facts which make it easy to construct

[1] Sherrington, C. S., 'The Integrative Action of the Nervous System,' 1911, pp. 14 *ff*.

[2] *Loc. cit.*, pp. 17–22.

a theory of the nature of the synapse. These experiments relate to the conduction of the nerve impulse through a so-called 'region of decrement.' Such a region is provided by a stretch of nerve fiber which has been narcotized, that is, which has been subjected to the action of a narcotic, *e.g.*, alcohol. An impulse in passing through a narcotized stretch of fiber decreases progressively in amplitude, the total decrease being proportional to the length of narcotized fiber through which it has passed. If the narcosis is sufficiently deep or the length of the region sufficiently great, the reduction of the impulse amplitude may be such as completely to extinguish it. If, however, the reduction does not carry the amplitude below a certain critical or threshold magnitude the impulse, upon emerging from the narcotized stretch, regains its normal amplitude and continues as if it had not passed through the narcotized region at all.

All of the evidence at hand points to the view that the action of a narcotic on a nerve cell consists essentially in a permanent increase of the permeability of the cell membrane, a physical state resembling that of refractory phase. Narcotics are apparently membrane destroyers and the depth of the narcosis is represented physically by the extent to which the membrane has been injured or made permeable. That this is a correct picture of the state of affairs in a narcotized region is indicated by the fact that such a region is electrically negative with respect to a non-narcotized portion of the nerve. It is also shown by the fact that narcotic substances, alcohol, chloroform, ether, etc., in general increase the diffusion of materials through the cell boundaries. The exact physical basis of 'conduction with a decrement,' however, is less easy to picture. Such conduction apparently demands that the degree of response of one portion of the nerve fiber should depend upon the intensity of the stimulus supplied by an adjoining portion. In other words, in a region of decrement the self excitation of a nerve fiber does not follow the 'all or none' principle. At present I see no plausible explanation of this change in law.

It will be recalled that if a nerve is restimulated during the relative refractory phase an impulse is generated which has an amplitude less than normal, while if it is restimulated during the phase of hyper-excitability the resulting impulse has an amplitude greater than normal. The experiments of Lucas show that the ability of the nerve impulse to pass through a region of decrement without being extinguished depends upon its initial amplitude. A normal impulse may pass successfully while a subnormal one generated during relative refractory phase may fail to pass. In another case, both normal and subnormal impulses may fail while one of supernormal amplitude generated during a phase of hyper-excitability may succeed. These facts are employed by Lucas to explain *inhibition* and *summation* on the assumption that synapses, in connection with which these two processes are most commonly found, are actually regions of decrement. A synapse, according to this view, is a naturally narcotized or auto-intoxicated portion of the nerve circuit. The so-called 'resistance' of the synapse is an expression of this condition. The 'resistance' is really a 'leakage,' but owing to the 'all or none' character of the nerve function the resistance cannot permanently lower the intensity of individual pulses but can only determine whether a pulse will pass through the synapse or not. The success or failure of a pulse in attempting to pass through a synapse will depend upon its initial amplitude and upon the depth of auto-narcosis of the synapse.

It is clear that if the individual impulses reaching a synapse are separated by distances so great that the condition of the nerve is restored to normal between each successive impulse, the frequency of the pulses can affect their ability to penetrate the synapse only by some accumulation of effects in the synapse itself. However, if the frequency is sufficiently high so that one impulse falls in the phase of hyper-excitability of the preceding impulse, then this increased frequency will aid the nerve current to pass through the synapse. A further increase in frequency, however, which causes one impulse to fall in the relative refractory phase of the preceding one, will render the nerve current less able than normal to penetrate the synapse. In case the synapse is of such a 'resistance' that only supernormal impulses can pass through there will be a relatively narrow range of nerve frequencies which will be capable of being conducted through the nervous arc, and the law governing the relation of conductibility to frequency will be similar to that of *resonance*, since impulse frequencies both higher and lower than the available range will fail entirely. If, on the other hand, normal impulses are conducted, nerve currents of all frequencies up to a certain critical frequency will pass the synapse but above this frequency there will be a complete block.

Adrian[1] makes it clear that these principles are adequate to explain many of the facts of inhibition as well as of summation. An increase in the frequency of any impulse above a certain critical value will evidently result in the inhibition of this impulse and any process depending upon it, provided a synapse is involved in the nerve circuit. Inhibition of one nerve current by another may occur if the second current impinges upon the same synapse as the first one and is of a sufficiently high frequency. If two currents of different frequencies combine, the resultant current must have a frequency at least as high as that of the highest component, and if the synapse blocks the high frequency component it will also block the low frequency one. This explanation of inhibition is clearly in harmony with the effects produced by strychnine, rabies, tetanus toxin, calcium salts, and other similar agents upon the nervous system, if we suppose that these agents decrease the permeability of the nerve membrane below normal or act in a direction opposite to that of narcotics. Upon this assumption strychnine would tend to obliterate synapses, in the physiological sense of the term; regions of decrement would be wiped out, and consequently all nerve impulses of whatever frequency would be transmitted. Inhibitions would be converted into excitations and the slightest stimulus would set the entire nervous system into action. It will be recalled that previously we have

[1] In the final chapter of Lucas's book, already quoted.

associated hyper-excitability with a decrease in the permeability of the nerve membrane below normal. The converse fact that narcotics acting upon the nervous system as a whole tend to convert excitations into inhibitions is also clearly in harmony with the given account of the synaptic function.

It is improbable, however, that the mechanisms of summation and of inhibition proposed by Adrian and Lucas on the basis of their study of conduction through regions of decrement are the whole story. Nevertheless most of the differences between reflex arc and nerve trunk conduction can be accounted for if we suppose the synapse to have a mechanism not differing qualitatively from that of the plain nerve fiber membrane, although differing quantitatively from the latter to a very considerable degree. There are undoubtedly certain physical *constants* which determine the processes of the nerve membrane. Among these are the threshold depolarization required to stimulate, the magnitude of the excitation or maximal depolarization, the rate of recovery of the membrane, etc. At the synapse these constants appear to suffer a radical change in magnitude, of such a character that all of the processes are retarded; the latent period is much longer, as is also the refractory phase, and if we suppose the synapse to have a phase of hyper-excitability this also is probably much prolonged. By a proper choice of the values of the various membrane constants the majority of the characteristic features of synaptic functions can be explained.

The synaptic process is evidently a *membrane process*, which means that it is localized in a region of space having the general form of a thin sheet. It should be noted, however, that this is equally true of the general process of conduction in a nerve. The synaptic function is probably more complicated than that of plain conduction, since it involves the combined properties of two membranes in juxtaposition. Some of the peculiar characteristics of conduction through synapses which may not be explicable by the postulation of mere quantitative differences between the synapse and the nerve trunk may perhaps be accounted for on the basis of quantitative *differences* between the two membrane elements in the synapse. For example, the fact that a synapse normally conducts in only one direction can be attributed to a difference between the intensities and thresholds of excitation for the two adjoining neurones, such that a more afferent neurone is able to stimulate a more efferent one but not *vice versa*. It is possible also that specially ionized substances may exist in certain synapses which introduce properties characteristic of these synapses.

VII. The Mechanism of the Receptor

Our account of the physical nature of nerve stimulation, conduction, and synaptic transfer needs to be supplemented by considerations bearing on the *receptor* function. Receptors, in general, are recognized to be especially differentiated cells, often more epithelial than neural in character, which lower the threshold of a given nerve path for certain forms of stimuli while raising it for others. It is probable, moreover, that in addition to being stimulus selectors, receptors are essential to the *continued* stimulation of nervous tissue by any fundamental force. DuBois-Reymond's principle tends to make the effect due to the direct action of any force upon the nerve a merely momentary one. In spite of the phenomena of sensory adaptation, stimulation through receptors produces relatively continuous excitation of the afferent nerve path. Adaptation itself appears to be attributable mainly to fatigue of the receptor process rather than of the nerve path.

In a previous article[1] I have suggested a plausible physical account of a manner in which the *visual* receptors may be conceived to produce a continued stimulation of the optic nerve fibers. This account is based upon the empirical finding that the retina is electrically negative, rather than positive as would be expected, with respect to the cut end of the optic nerve. This negativity of the retina, which

[1] Troland, L. T., 'The Nature of the Visual Receptor Process,' *J. of the Opt. Soc. of Amer.*, 1917, 1, 9–13.

is increased by the action of light, seems to imply that the essential ionized substance of the rods and cones has a negative ion which diffuses through the enclosing membranes of the receptor cells more readily than does the positive ion, producing a state of polarization opposite in direction from that of ordinary nerve fibers. It is clear that a receptor cell thus polarized and in contact with a normal nerve fiber, would tend to depolarize the latter at the place of junction, thus setting the fiber into excitation. The excitation would probably be mutual, resulting in a reduction of the polarization both of the nerve membrane and of the receptor membrane. We have reasons for supposing that the inertia of the receptor process is much greater than that of the nerve function, so that in all probability the nerve would recover from refractory phase considerably in advance of the recuperation of the receptor cell polarization. However, when this latter recuperation has reached a certain point, a second stimulation of the nerve fiber would result and this process would be indefinitely repeated. The result would be a stream of quantal impulses sent along the optic nerve fibers, and having a frequency determined by the rate of recuperation of the receptor cell.

In the case of visual response it is easy to see how this frequency can be caused to vary as a result of variations in the stimulus. The direct effect of light upon the sensitive substance of the rods or cones is probably one of increased ionization and we should expect the rate of *re*polarization of the receptor cell membrane to increase with increase in concentration of the ions within the cell. The optic nerve impulse frequency would thus tend to be augmented by the action of radiation on the retina and to a degree greater the greater the intensity of radiation of any given wave-length.

It is certain that the processes by which sensory stimuli excite receptors are as varied as there are different forms of adequate stimuli. However, it is conceivable that the mode of interaction of the receptor cell and the conducting nerve fiber is always of the same general sort. It may be a general characteristic of receptor cells to have a negative polarization, so that they tend constantly to depolarize and to excite the nerve fiber. A stimulus acting upon the receptor cell would in this case so operate as either to increase or to decrease its negative polarization. It is easy to imagine physical or chemical mechanisms through which such changes could be brought about by the action of almost any conceivable agency.

Rhythmic depolarization mechanisms of a sort similar to that above discussed may possibly be found in the central nervous system as well as in the periphery. The rhythm of the breathing center and other centers such as those determining certain types of peristaltic action, muscle tonus, etc., may be controlled by such mechanisms.

VIII. Psychophysiological Applications

The ultimate physical analysis of the nerve function must provide us, according to my view, with the fundamental materials on the physiological side for the construction of an ultimate psychophysical theory. The traditional doctrine of the interdependency of 'mind and body' teaches us that consciousness depends upon the existence and nature of central nerve processes. If we reject vitalistic fancies we must be forced eventually to describe these central processes in physical terms and thus to conceive them as certain configurations and changes in configuration of electrical particles.

Some modern thinkers, often officially catalogued as psychologists, subscribe to the view that there is no such thing as consciousness, and they of course cannot be expected to take much interest in the psychophysical problem. These same thinkers often appear to believe in the existence of a peculiar characteristic of physiological activity called 'the operation of the organism as a whole.' If, however, we define consciousness as simply *any given experience* we dispose of any difficulty about the non-existence of this entity, and the more we analyze the operations of 'organisms as a whole' the clearer it becomes that these operations are simply concatenations of many part processes. The ultimate account is one which expresses any process, no matter how complicated, in terms of the operations of irreducible physical elements.

The central nerve process, which in the traditional theory acts as the determinant of consciousness, consists essentially, according to the modern idea, of *synaptic* functions. Such functions, however, apparently differ only quantitatively from those of simple nervous conduction, and if consciousness is associated with synaptic processes it probably is also correlated to some degree with the simpler processes of conduction. Both of these related functions, as we have seen, are membrane processes, and it would therefore appear that the physical correlate of consciousness is localized in some definite configuration of sheet-like regions of space. This 'region of determination of consciousness' is almost without doubt located in the association areas of the cerebral cortex in the case of the human introspective field, which is the main object of study for pure psychology.

The introspective analysis of consciousness provides us with certain psychical elements, attributes, and modes of relationship, for each of which it is desirable to find definite physiological correlates. A careful study of the implications of ordinary laboratory psychophysics—which determines the relationships between stimuli and their conscious reactions—with the theory of nerve action should ultimately enable us to work out some of these direct psychophysical correlations. Our theory of the receptor process in the case of vision suggests that nerve impulse frequency, or at any rate the magnitude of the nerve current, is the determinant of what we call sensory *intensity*. The characteristic *qualities* of different 'sensations' are correlated by the traditional doctrine of specific energies with the identity of the nerve path which is excited. Mere abstract identity, however, will scarcely suffice. We must suppose that these qualities depend upon peculiarities, either structural or functional, in the cerebral synapses which are set into operation by different afferent nerves. In another paper[1] I have suggested that many of

[1] Troland, L. T., 'A System for Explaining Affective Phenomena,' *J. of Abnorm. Psychol.*, 1920, 14, 376–387.

the facts about affection, or pleasantness and unpleasantness, can be explained on the assumption that its neural correlate lies in the *rate of change of synaptic conductance* in that particular portion of the cerebral gray matter which is responsible for the introspective consciousness at any given moment. Clearness and other fundamental characteristics of factors in consciousness will eventually find their proper correlates in the cerebral nerve process.

It is certain that the physical correlate of the simplest introspectively discriminable factor of consciousness must, from the physical point of view, be enormously complicated. A point visual sensation, for example, can scarcely be attributed to the function of any cerebral component smaller than a single synapse, and it is certain that a single synaptic mechanism involves the simultaneous coöperation of millions of physical atoms, electrons, electrical and magnetic fields, etc. The coexistence in a single moment of consciousness of a multitude of sensory elements arranged in a definite pattern must depend upon the concurrent and unified functioning of a large number of cerebral synapses. Unless we are to employ assumptions which suggest a non-physical or spiritual factor in the determination of consciousness, we must suppose that the unity of consciousness depends upon some sort of unity in the total nerve process upon which consciousness depends.

I am attempting to work out plausible solutions for some of these fundamental psychophysical problems in connection with a general metaphysical theory which I have called 'Paraphysical Monism.'[1] This doctrine provides us with an explanation of the facts of psychophysical parallelism which eliminates the fundamental dualism of the Leibnitzian preëstablished harmony, permitting us to combine the facts of physics and psychology into a unified system. Psychology needs all of the stimulus which it can derive from the advances of physical and physiological science. Indeed, this need is so dire as almost to warrant the suicidal promulgations of those 'psychologists' who call themselves

[1] Troland, L. T., 'Paraphysical Monism,' *Philos. Rev.*, 1918, 27, 39–62.

'behaviorists.' The psychology of the soul is dead, and that of consciousness is suffering murderous attacks. Whatever we may think of the former, the latter is assuredly worth saving, and it is my impression that the data provided by modern nerve physiology will provide us with means for resuscitating the true science of physiological psychology as it was conceived by Fechner, Müller, Helmholtz, and other pioneers.

THEORIES OF THE WILL AND KINÆSTHETIC SENSATIONS

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Theories of the will in psychology owe their origin to the development of ethical views among the early Greeks. These theories, together with those held by the early Church Fathers and the Scholastics, may be classified as intellectual and absolute. Both of these groups made will a faculty of the soul. The intellectual theories derived their name from the importance which they ascribe to the faculty of reason as a constituent of willing; the absolute theories made all other faculties of the soul subordinate to the will. In the Aristotelian view the will consisted of a 'desire' to which a 'goal' or an 'end' was supplied by the reason. Desire was analyzed as a persistent state of unrest or striving and was the essential active or dynamic feature of mental life. During the middle ages theories of the will were concerned largely with the problem of determinism versus freedom, hence the relative importance of the two faculties, the intellect and the will, as agents in controlling human behavior, was a subject of paramount importance.

As a consequence of empirical and inductive methods, we find in England from the time of Hobbes to Hume the development of a new type of theory, the emotional theory. Hume regarded the feelings as essential constituents of every volitional process. Locke had previously held that disquietude or uneasiness constituted the origin and the dynamic feature of volition, a view which in its essence was but a rehabilitation of Aristotle's potential desire. On the continent from Descartes to Herbart the tendency was gradually becoming definitized of conceiving the will as the active side of mental life in general, or as a general striving tendency.

Up to the time of more modern psychology, then, there were in general three views of the will. In a broad sense of the term the will included all mental activity as such, an activity characterized as a potential desire or as a state of unrest. This tendency culminated in a definitized theory of conation. Secondly there was a voluntaristic tendency to employ the term will itself to mean this active feature of mental life or the source and cause of action. Thirdly there was a tendency to bring the will into intimate relations with the feelings where the feelings were regarded as essential constituents of volitional processes.

Modern theories may be classified according to the feature most characteristic of each, namely its emphasis upon the reductivity or the non-reductivity of will to a complex grouping or mode of functioning of simpler constituents. Those theories which reduce will to a peculiar order of sequence of sensations, images and affections and which do not emphasize the elementary nature of any one constituent we may call the *totally reductive*. On the other hand those theories which admit the existence of an elementary mental content—an elementary and unique volitional process—may be called *non-reductive*. In this latter group the will has an elementary content which is not necessarily independent, functionally, from other contents. Intermediate between these two groups of theories is another which we may call the *partially reductive*. Here the will is reduced partly to the functioning of the traditional elements of consciousness and partly to a unique elemental content. The latter might be said to constitute a structural and functional criterion or an essential conscious concomitant of the volitional consciousness. This group may be subdivided thus: (1) those theories which posit a conative element; (2) those theories which posit an intrinsically active ego; (3) those which regard the feelings as the essential constituents of will; (4) those which ascribe to mental processes a force or general innervation mechanism. A final group of theories may be called motor or behavioristic owing to the fact that emphasis in them is laid upon the principle of stimulus and response. Here the will, reduced to its simplest terms, becomes a system of coördinated reflexes or motor responses and the innervation mechanisms are not mental but physiological.

Non-reductive Theories.—Lotze[1] conceived the will to be an unanalyzable psychic process which functioned chiefly in choice and resolve. Since it was not constantly functioning as a datum of consciousness it was relegated to the domain of the subconscious where it found a resting place when not actually in operation. In the more recent literature Ach[2] and Michotte[3] might be mentioned among those who, in consequence of their failure to analyze the volitional process experimentally and in consequence of finding a 'feeling of mental activity' in 'genuine' volitional acts, make a certain part of the volitional consciousness, at least, elemental and non-sensory. James'[4] most often quoted 'fiat' consciousness was a subjective experience *sui-generis* which could be designated but not defined.

Partially Reductive Theories.—Herbart's[5] theory of the will is an early example of the conative theory. Here the will in its elemental form was to be found in the striving of ideas for existence in consciousness or for the possession of the conscious level. This striving process began in the realm of the unconscious. Volition consisted first of *desire*, a product of resistance between striving ideas and secondly in the opposition between groups of associated ideas (goals of purposes). Lipps[6] assumed a striving process not only in ideas but in feelings and sensations as well. Both affective processes and feelings of activity were essential constituents of willing and of the volitional consciousness. The conationists, among whom may be mentioned Stout,[7] especially, ascribe to each conscious state an inherent tendency to pass beyond itself into another conscious state, a striving process which is directly labelled conation and which, in itself, is present to consciousness.

Writing from the point of view of a self psychology Calkins described the content of willing as an active 'consciousness of

[1] 'Medicinische Psychologie,' Leipzig, 1852.

[2] 'Ueber den Willensakt und das Temperament,' Leipzig, 1910.

[3] 'Etude expérimentale sur le choix volontaire et ses antécédents immédiats,' *Arch. de psychol.*, 10, 1910, 113–321.

[4] 'Principles of Psychology,' 1890, Vol. II.

[5] 'Lehrbuch der Psychologie,' Leipzig, 1850.

[6] 'Von Fühlen, Wollen und Denken,' Leipzig, 1902.

[7] 'A Manual of Psychology,' London, 1913.

my active connection with other selves or with other things' ('First Book in Psychology,' 3d rev. ed., p. 226). The volitional consciousness is said to involve the essential non-sensory factor of the 'self-as-willing.' Ach and Michotte found an immediate and unanalyzable consciousness of the self or an intrinsically active ego in all genuine volitional acts. Meumann[1] assumed that the process of 'accepting' a goal idea, in a volitional act, involved an immediate consciousness of the self. Both partially reductive and totally reductive tendencies can be found in the analyses of volition which have been made by these latter writers.

The emotional theory of the will may be illustrated by reference to Bain[2] and Wundt.[3] Bain defined the will as all mental and physical activities insofar as they were guided or impelled by the feelings of pleasure or pain. Wundt's theory (which is voluntaristic as well as emotional) makes will the original energy of consciousness, the first and primary form of which is a simple impulse motivated by pleasure or pain. When an organism experiences a simple sensation there arises a feeling process which develops to a maximal state of intensity beyond which it overflows into movement. Such a sensation-feeling-action series of experiences and events is called a simple or primary act of will. Feeling processes, therefore, possess an innate capacity toward willing. This is either a capacity to arouse physical movements or to initiate into consciousness other mental processes. In more complicated forms of will the feelings or emotions may be observed to increase gradually in their intensity, beginning with pleasantness or unpleasantness, eventually developing into strain or excitement. These feeling states fuse into a 'total feeling of activity' which is an essential conscious concomitant of complex or secondary acts of will. In this category are found such acts as choosing and performing difficult tasks. Secondary volitional acts may be motivated by ideas which are associated with the feelings.

As an example of a 'force' theory may be cited Fouillée[4]

[1] 'Intelligenz und Wille,' Leipzig, 1913.

[2] 'The Emotions and the Will,' London, 1899.

[3] 'Grundzüge der Physiologische Psychologie,' Leipzig, 1903.

[4] 'L'évolutionisme des idées-forces,' Paris, 1893.

for whom sensations and feelings are at the same time conscious states and mental forces according to the viewpoint taken in regarding such mental processes as contents or as acts.

Totally Reductive Theories.—Münsterberg[1] held that the will as a datum of consciousness was a goal-idea which had come to be associated with other ideas or with muscular movements. This goal-idea involved the anticipation of an end. In other words it is a mental process of sensory origin having to do with preparatory motor adjustments. Ebbinghaus[2] in like manner held that volition consisted in the capacity to foresee the end of action by associating an image with an act in such fashion that the image would function as the stimulus for the subsequent act. In the views of Ach[3] and Meumann[4] we might have found totally reductive theories had it not been for the fact that both ultimately drag in a non-sensory experience pertaining to the self and feelings of activity. Meumann described the will as a selective process brought about by means of 'accepted' goal ideas, while in a similar fashion Ach found the clue to a volitional act in the acceptance of an *Aufgabe*. The capacity of goal-ideas to so control subsequent mental processes depended upon the previous forming of associations between the foresight of an end and the act which attained that end.

Behavioristic Theories.—The modern trend of descriptions of the will is obviously behavioristic, where the emphasis is laid upon the coördinated responses of the organism to its environment and not upon the mental contents as such. But owing to the lack of experimental evidence accurate accounts of the volitional process cannot be presented. According to Ribot[5] the will is to be regarded as the sum total of the organism's responses to environment. All mental processes tend to express themselves in some form of overt action and would succeed in doing so were it not for processes of inhibition. The continuity of mental states can be expressed only in terms of the continuity of these organized motor responses.

[1] 'Die Willenshandlung,' Freiburg, 1913.

[2] 'Grundzüge der Psychologie,' Leipzig, 1911.

[3] *Loc. cit.*

[4] *Loc. cit.*

[5] 'The Diseases of the Will' (trans.), Chicago, 1903.

Notwithstanding the numerous descriptions and interpretations of the will which have appeared in the literature we find little genuine progress in ascertaining the exact characteristics of the volitional consciousness. It is evident, however, that it cannot be described adequately in terms of structural contents alone. Functionally, two distinct problems are involved, namely those of the will in a broader and in a narrower sense. In the broader sense the problem of the will should be identified with the problem of the general sequence of mental processes. Such discussions seek to answer the question, how may the sequence of mental processes be envisaged in mental terms? Or stating the problem in objective terms: how may the continuity of the organism's responses to environment be best described? In the narrower sense the will should be identified with the problem of a particular portion or order of sequence where the question is asked how may one group of mental processes exert an apparent directing influence over subsequent mental processes? Stated in objective terms: how may one response lead inevitably to the making of a subsequent response?

Theories which have been formulated from a subjective point of view have reduced the will, in the broader sense, to a potential desire (Aristotle), to a state of uneasiness (Locke), to a striving process (Herbart, Lipps), to a conative tendency (Stout, Baldwin and others) to an intrinsically active ego (Calkins, etc.), to innervating properties of the feelings or emotions (Bain, Wundt) and to an alleged dynamic force (Fouillée). Other writers have concerned themselves with the general problem of sequence but from an objective point of view; hence the will has become the sum total of the organism's motor responses (Ribot and others).

Still others who have faced the same problem and who have borne in mind both its structural and functional aspects have failed to find any structural clue to the general problem of sequence, hence for them the problem becomes one of a unique or particular order of sequence. Here, again, failing to find elementary structural clues they have formulated theories of a totally reductive character. In such theories the important rôle is assigned to a goal-idea or *Aufgabe* which conditions the sequence of mental processes by means of productive or selective influences. On the other hand there have been many attempts to state clearly the problem both in its broader and narrower aspects. For example, the will in its narrower sense (volition proper) was envisaged by Aristotle as a rational desire; by Hume, Bain and Wundt as a highly organized sequence of potential emotional processes associated with ideas; by the conationists as a highly organized conative system the distinctive feature of which was the foresight of an end. Another view involves an assertive attitude of the self (Calkins). The problems have been the same throughout all these discussions; but how different have been the solutions!

The confusion found in these descriptions can be traced obviously to widely different points of view. Advocates of partially reductive theories have been obliged to appeal either to a volitional constituent in the feelings, to an alleged conative element, to an intrinsically active ego or to forces inherent in sensation and affection in order to account for the general conscious continuum in purely subjective factors while adhering at the same time to an atomistic conception of mind. It is the problem of getting elementary states of consciousness back into a working, active system again after they have been assigned the rôle of units or 'atoms.' It is the problem of making a river out of a succession of barrels in the stream bed. Witness the attempts of those writers who adhere to an atomistic and structural conception of mind but who have failed to discover any elemental conscious experience whose constant presence in mental life will explain the continuity of conscious states. Such writers (James,[1] Brentano,[2] Witasek[3]) have resorted to other factors such as to a differentiation between transitive and substantive states or between act and content. From a more objective point of view the recent motor movement in psychology seems to be based essentially upon an attempt to solve the general problem of sequence and unbroken continuum.

[1] *Loc. cit.*

[2] 'Psychologie vom empirischen Standpunkte,' 1874.

[3] 'Grundlinien der Psychologie,' 1908.

As for the narrower problem—the determination of subsequent processes by an antecedent process—one finds the same divergence in points of view and in results. The issue has been definitely sharpened by positing the existence of a determining tendency or directing of the course of the stream of consciousness. But this determining tendency may be either a driving, a *vis a tergo* exerted by the *Aufgabe* (Ach) or a leading—an attracting—by goal ideas (Meumann). Auxiliary principles such as associative tendencies, perseveration tendencies and constellations have been appealed to in an endeavor to formulate the problem and its solution more clearly. But the traditional principles of association have borne the burden throughout all these more recent discussions. The objective or behavioristic attack upon this same narrower problem includes the reflex-arc concept (Dewey)[1] and the principle of ideo-motor action.

The solution of these problems has been delayed, also, because of the very meager introspective analyses which have so far been made under experimentally controlled conditions. It is extremely doubtful whether a genuinely vigorous volitional act has ever, in the past, been subjected to adequate introspection.

It seems to the writer that insofar as the partially reductive theories have been based upon introspective evidence, the trouble has been in a failure to reduce conation, striving process, feelings of activity, etc., to a common process. We have every reason to believe that many of Wundt's 'feelings,' for example, are forms of kinæsthetic sensations. Is it not quite probable that what the conationists have called the immediate conative experience is kinæsthetic sensation? The writer is firmly convinced that the 'feeling of mental activity' described by Ach and Michotte is a complex of kinæsthetic sensations. Moreover is it not also possible that Meumann's consciousness of the self in the acceptance of a task or that Professor Calkins's intrinsically active ego are interpretations unwittingly based upon an immediately experienced but complex and diffuse kinæsthetic background and nothing else? Within the last

[1] 'The Reflex Arc Concept in Psychology,' Psychol. Rev., 1896, 3, p. 357.

few years there have appeared several elaborate introspective descriptions[1] of various conscious processes but in none of these do we find the slightest hint of such elements. Kinæsthetic sensations are with us always in mental life. Recent introspective works and more especially those of Fernberger and the writer show the importance of such processes. In fact, as the writer has shown in an introspective study of choosing,[2] kinæsthetic processes are essential and as a type the only essential form of elemental conscious process in the act of choosing.

It does not seem unwarranted, therefore, to conclude that the extreme variations in past descriptions of the will consciousness both in its broader and narrower aspects have been due to various interpretations of a consciousness which is so largely made up of kinæsthetic sensations. From these experiences we get our notions of striving, strain, activity, force, conation and the like. It may be open to question, also, whether the Freudian wish and its various cousins are not veiled and unconscious interpretations unwittingly based upon a consciousness of kinæsthetic strain.

To sum up, theories of the will in the history of psychology represent successive attempts to describe the conscious continuum as a whole and to describe the process of control in any given portion of the conscious continuum. The chief cause for the great variability of these descriptions lies in a further attempt to find evidence of this continuity in some unique mental process. Where such an elemental process has been found lacking we have been obliged to resort to the principle of act and content. Various points of view have added to the confusion. And in modern psychology, inadequate introspective data has led to inadequate interpretation. The unique mental process, we believe, is nothing more than kinæsthetic sensation.

[1] E. L. Woods, 'An Experimental Analysis of the Process of Recognizing,' *Amer. J. of Psychol.*, 1915, **26**, 313–387. S. C. Fisher, 'The Process of Generalizing; and its Product, the General Concept,' *Psychol. Mon.*, 1916, **21**, No. 2 (Whole No. 90). S. W. Fernberger, 'An Introspective Analysis of the Process of Comparing,' *Psychol. Mon.*, 1919, **26**, No. 6 (Whole No. 117).

[2] R. H. Wheeler, 'An Experimental Investigation of the Process of Choosing,' University of Oregon Publications, 1920, Vol. 1, No. 2.

This compels us to settle upon a point of view. It shows us the futility of searching for introspective evidence of continuity and places us in the position of the other sciences where we rightly should be—the position of finding continuity in data treated from an objective point of view. We should look for the solution of the problem, therefore, in behavioristic principles. The cry at present is, in some circles, to do away with introspection. But in the other sciences observations are made *via* the senses. Why can we not observe our own behavior in the same way? Introspection as sensory evidence of our own behavior ought to be as valid as sensory observation of any movement external to our bodies. Let the 'feel' of a response be as ample evidence of the existence of that response as the 'sight' of it in another person or an animal. To be sure introspective evidence should be verified wherever possible both by objective instruments and by similar reports from large numbers of observers. Our view is that in attempting to solve psychological problems in the future,—problems which were formally considered purely subjective—we are warranted in drawing behavioristic interpretations, in part at least, from introspective data.

A PURSUIT PENDULUM[1]

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One of the measurements used at the Nutrition Laboratory on the aviation candidates in the spring of 1917 was to record the adequacy of ocular-pursuit movements in following the swing of a pendulum. The subject was seated at a head-rest with the left eye covered. A polished metal bead suspended by an invisible cord was arranged to swing through a visual angle of about 40°. The pendulum made a double swing in 2 seconds. Its release was synchronous with exposure of the eye to the recording beam of light, after the manner of Dodge's photographic technique.[2] The repeated instruction was to watch the bead intently every moment of its swing. Six or more successive trials by a subject were photographed side by side on one plate. These records do not easily provide an exact quantitative score for accuracy of pursuit. However, it is convenient to rank these photographic records showing the reaction time occurring at the start of the pendulum's swing, together with the number and size of abrupt horizontal movements by which the subject supplements his inadequate pursuit, into five grades or groups of excellence. Such grouping gave a positive correlation of 0.40 with the subsequent progress of these men in learning to fly.[3]

[1] In abbreviated form this paper was read before the American Psychological Association, Cambridge, December 30, 1919.

[2] Diefendorf and Dodge, Brain, 1908, 31, pp. 451–489. See Plate II for illustrative records showing fully the characteristics of this type of eye-movements. For a description of the eye-movement recording apparatus as used on the aviation candidates, see Benedict, Miles, Roth, and Smith, 'Human Vitality and Efficiency under Prolonged Restricted Diet,' Carnegie Inst. Wash. Pub. No. 280, 1919, pp. 159 ff. and pp. 184 ff.

[3] Our subjects, the first groups of candidates to attend the Aviation Ground School of the Massachusetts Institute of Technology, were a very superior lot of men. Nearly all graduates of our best universities, these men had been prominent in athletics and many of them on their own initiative and at their own expense had taken some

At the time this result was found hardly any single test indicated a higher correlation with flying. Officials advised, however, that the ocular-pursuit measurement, as carried out photographically, was too complex for any general use in the preliminary selection of candidates for pilot training.

These details have been recited as they account for the simplicity of the device described below. The pursuit pendulum was an effort to meet a definite situation. Care was exercised to exclude all electrical and photographic or other graphic features, to make the apparatus its own gravity-operated chronometer and such that it could be used nearly anywhere and would give an immediate quantitative score for the accuracy of the eye-hand coördination in pursuit movement. Although an opportunity never came after the development of the test to try it on a group of aviators or men who were in this training, the possible general usefulness of the measurement to other laboratory workers and in industry may warrant the description of the pursuit pendulum, together with illustrative data for initial performance, improvement with practice, and changes in efficiency, *e.g.*, as produced by a superimposed nutritional factor such as alcohol.

From a suitable wall bracket a pendulum carrying a reservoir is arranged to swing over a sink or table, a small stream of water flowing from the lower extremity as the pendulum swings. The individual under test, seated before the sink, attempts to catch the water in a cup of limited diameter. A separate cup is used for each double swing and the volume of liquid collected represents quantitatively the adequacy of pursuit.

The bracket, *A* (see Fig. 1), extends from the wall about 45 cm. and is fairly rigid. The pendulum, *B*, 140 cm. long,

training in aviation. They were keenly interested in aviation problems and coöperated whole-heartedly, as did also the officials at the ground school. Although about 65 men were measured, the government found it imperative to send many of these to Europe immediately upon their having finished at the ground school, and they therefore received the flying training abroad. Most energetic efforts were made by Professor E. L. Thorndike to secure the flying scores for these men on whom Drs. H. E. Burtt, L. T. Troland, and myself had worked. Scores for 26 were finally obtained and it is for these that the correlation mentioned was found.

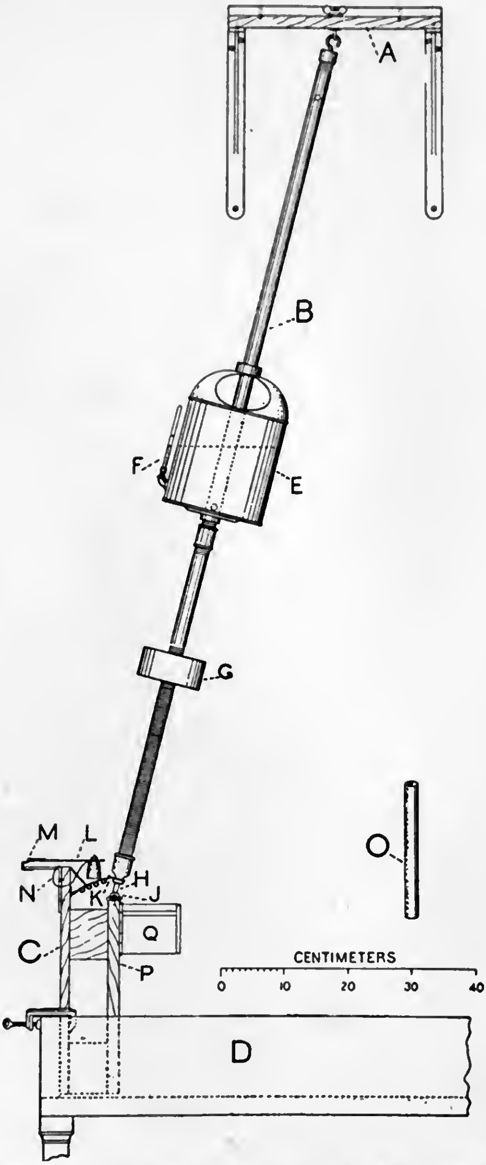

FIG. 1. Diagram of the pursuit pendulum. *A*, wall bracket; *B*, pendulum carrying reservoir, *E*. Gauge, *F*, determines volume of liquid in reservoir. *G*, adjustable weight regulating pendulum's period. *C*, wooden frame clamped to sink, *D*. Nozzle, *H*, of pendulum held by catch, *K*, against rubber tubing, *J*, until released by fall of hammer, *L*, hinged at *M*. Short section of chain, *N*, determines lift of hammer. *O*, cup of limited diameter, in which the expelled liquid is to be collected by the subject under test. *P*, position at which cup, *O*, is held at start. *Q*, short-stop for ending the catch.

is suspended by two screw eyes. Its shaft is continuous through the reservoir and is very stiff, being made of sections of galvanized-iron pipe (regular $\frac{1}{2}$-inch inside diameter). The reservoir, *E*, a 1-gallon galvanized-iron oil can, surrounds the shaft and is firmly secured and made leak-tight by the use of a "railing flange" soldered to its bottom. As the reservoir is located about midway the length of the pendulum, the head of water changes but little with the decreasing level in the can. An adjustable weight, *G*, of about 4 kilograms allows for regulation of the pendulum's period and makes the position of the center of mass much less dependent upon the exact amount of liquid in the reservoir. Openings are arranged in the pipe shaft on a level with the floor of the can and air vents are placed above. The water flows very freely from the reservoir, and at the lower end of the shaft is reduced to a stream 3 mm. in diameter by the nozzle, *H*.

A simple arrangement for retaining and releasing the pendulum is shown in Fig. 1 and separately illustrated by a top view in Fig. 2. The wooden frame, *C*, is clamped to one end of the sink, *D*, at such a height that when the orifice, *H*, is slipped up on a cushion, made of a short horizontal section of rubber tubing, *J*, a closure is made which is practically leak-tight. A catch at *K* retains the pendulum in this position until the fall of the rubber-headed hammer, *L*, hinged at *M*. The hammer is lifted by the operator and held in a nearly upright position, determined by a short section of chain (see *N*, Fig. 1). It is released on verbal signal from the subject and requires 0.3 second to fall and start the pendulum. This method of release corresponds to common industrial operation and the reactor, especially a subject without psychological training, likes it better than having the start occur at some arbitrary and more or less unexpected time beyond his control.

The cups in which the subject is to catch the expelled liquid (see *O*, Fig. 1) are made of thin-walled brass tubing, 19 mm. (regular $\frac{3}{4}$-inch tube) inside diameter and nearly 22 cm. long. At the start a cup is held at position *P*, against the wooden frame, nearly vertical from and about 2 cm. lower than the orifice, *H*. No water leaks into the cup and it is possible to begin the hand movement almost in register with the pendulum. While the subject follows the pendulum to the right, the operator turns the short-stop, *Q*, from position 1 into position 2. (See Fig. 2.) This metal screen stops the return movement of the cup at a distance of 2 cm. in front of position *P* and thus gives the operator a little space within which to catch the pendulum and replace it on *J* without spilling liquid into the cup, as otherwise an error would be introduced in the result.[1]

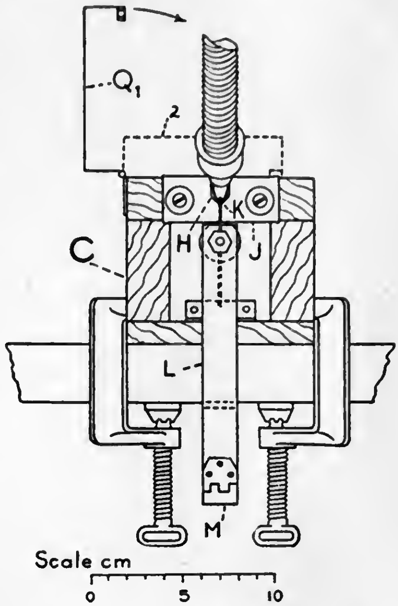

Fig. 2. Top view of the arrangement for retaining and releasing the pendulum. *H*, pendulum nozzle pressed by the rubber tube, *J*, to form leak-tight closure; *K*, hook by which pendulum is held in position until *K* is depressed by the hammer, *L*, which is hinged at *M*; short-stop, *Q*, at an appropriate time is turned into position 2 in order to cut the return pursuit slightly short and allow space in which the experimenter may catch the pendulum.

With two liters of water in the reservoir the period of the pendulum for a double swing is just 2 seconds. Naturally this time will increase somewhat as water is lost and the center of gravity lowered. Practically, the change is of no consequence to the test, as we find that with two liters of water 20 v. d. require 40 seconds, while with one liter 40.3 seconds are required, and when the reservoir and shaft are empty 40.9 seconds are required. Thus, for testing purposes the period of swing may be regarded as independent of the

[1] Another source of error must be guarded against with the coöperation of the subject. He should be cautioned not to slip the cup up over the nozzle of the pendulum or, indeed, to bump the cup against the nozzle and thus interfere with its motion. The stream of liquid does not spray out and there is no advantage, from this standpoint, in having the mouth of the cup very near the pendulum. This requirement not to touch the pendulum with the cup is a part of the coördination, but unfortunately does not show in the objective results.

amount of liquid in the tank. The amount of head of the liquid and the size of the orifice were arranged with the idea that 50 c.c. should be the possible catch per double swing. With 2 liters in the reservoir at the start, it is found that at the first double swing the subject can catch, possibly, 50.4 c.c. and at the tenth succeeding catch 49.7 c.c. For eight successive trials the amount delivered is thus within 1 per cent. of 50 c.c. It has seemed satisfactory to replenish the water every five or ten trials. If the subject is catching nearly all the liquid expelled, the opening in the cup should be reduced. A gauge, *F* (Fig. 1), on the side of the tank makes it a simple matter, when introducing water at the opening in the top of the reservoir, to determine that the volume of water shall be up to 2 liters. During the swing the orifice of the pendulum, as used in the collection of the data presented below, moved a horizontal distance to the right of 70 cm. This is a fairly large excursion, but most adults can follow the movement without swaying of the body, if they so desire.

More complex arrangements of such pursuit apparatus naturally suggested themselves, for example, the pendulum might be made the long arm of a siphon. An orifice, not a part of a pendulum, might be carried on a belt and given a complicated series of movements, prolonging the pursuit and requiring coördination for forward and backward as well as for lateral displacements.[1] After the experience with the ocular-pursuit measurement it was assumed, however, that in trying to contribute to the problem of selecting aviation pilot material the simpler the test apparatus the more serviceable it might possibly become. Therefore this model was made independent of electrical features, did not require running water or a sink, could be filled by hand from a pitcher and could be arranged over a table or inclined trough, as the operator's conveniences might permit.[2]

[1] In a personal communication Professor Carl E. Seashore informs me that, after trying the original test at the Nutrition Laboratory, he has arranged a very successful combination for testing motor ability to perform circular pursuit movements, by using a phonograph motor, a time-interrupted circuit, and an electric counter.

[2] A criticism which may be raised against the quantitative score which the apparatus makes possible is that this score is not a sufficiently graduated result. Prac-

In practice successive catches can follow each other rather rapidly, their speed being largely determined by the quickness of the subject's motions in replacing and taking up the cups and the promptness of his verbal signals for release of the pendulum. Twenty-five trials are easily made in five minutes. The 25 cups stand in order, as at *R* in Fig. 3, being conveniently held in a box frame, *S*. Each fifth cup has a black band near the top serving in the test as a signal to the operator to replenish the water in the tank. If there is no time immediately after the test to measure the results, the cover, *V*, is placed over the open ends of the cups, the name, date and hour are noted at *U*, and the box is set aside.

In measuring the results it is tedious to empty each cup separately into a small graduate and so determine the volume of liquid. Since the cups are all of the same inside diameter (as nearly so as brass tubing is commercially made) and all have the same inside depth, a graduated scale, *W*, cut from thin aluminum sheet attached to a cork float can be introduced into the mouth of each cup as these are held conveniently side by side in the box frame, *S*, and the volume of liquid can thus be very quickly determined for the successive catches. After the catches have been individually measured, provided that such analytical data are desired for securing a measure of variability, the frame, *S*, is grasped in such a way that the rubber tube, *T*, attached to the hinged door on the front, presses against all 25 cups so that their contents may be emptied and drained at once into the inclined V-shaped trough, *X*, and so into the large graduate, *Y*. The total score is in this way very readily secured.

If a good subject reaches such a degree of skill that, for example, he regularly catches more than 80 or 90 per cent.

tically, it makes no difference whether the pursuit is so accurate that the stream of water goes in at the center of the opening in the cup or over at one side, just so long as the whole stream is collected. Since the edge of the cup is sharp, tapered from the outside, if the stream strikes here it will be divided, part collected and part lost. With a slower-moving pendulum, a somewhat larger orifice, and a cup with the mouth the same size as the orifice, probably an arrangement could be made so that the subject could always catch a part of the liquid and thus small inaccuracies of pursuit would more properly be represented in the result.

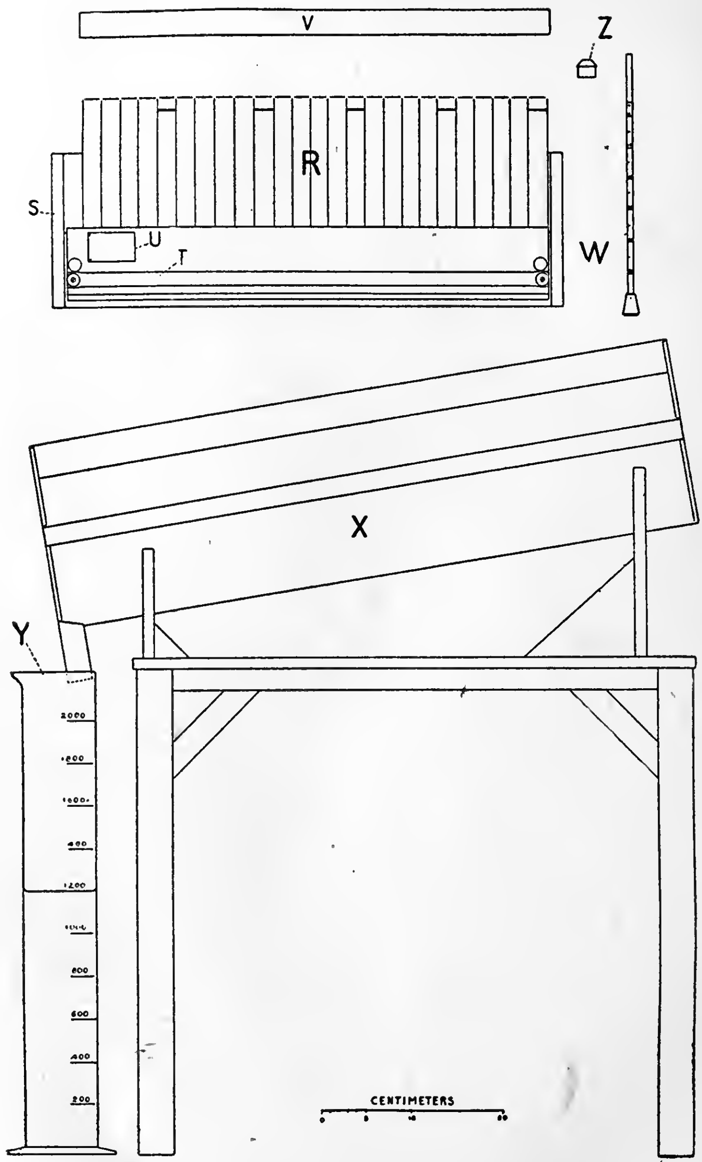

Fig. 3. Arrangement of the cups in a way convenient for measuring the quantity of liquid, by simple accessory apparatus. *R*, bank of cups in carrying frame, *S*; *V*, cover for cups; *U*, pad for noting name, date, and hour; *T*, rubber tube attached to hinged door of frame, *S*, which is pressed against cups in emptying; *W*, measuring float graduated in c.c.; *X* and *Y*, inclined trough and large graduate for receiving the contents of a whole bank of cups emptied at once; *Z*, collar to reduce the opening at the mouth of the cups, for especially expert subjects.

of the possible catch, the task may of course be made more difficult by decreasing the effective opening in the cups. A collar, Z, can be slipped into and withdrawn from each cup by the subject as he uses them in turn and thus the opening may be reduced to 10 mm. or to whatever size is deemed desirable to make the task satisfactorily difficult.

It is probable that any investigator who arranges such an apparatus as is here described will not make it an exact duplicate. Nevertheless illustrative data are of value in supplementing the description of the apparatus, as they give an idea of the type of results that may be expected from its use. In December, 1918, and January, 1919, considerable data for this test were obtained on a group of staff members of the Nutrition Laboratory, including ten women and eight men.[1] The pursuit test was given on 35 days, usually successive except for Sundays, and the amount of practice was 20 catches per day. At that time the equipment of cups consisted of two banks of ten each. It was hardly feasible that each individual should be tested at exactly the same time on each day, but care was taken not to measure subjects when they were fatigued or otherwise indisposed.[2]

The average results for a group of 18 adults are shown graphically in Fig. 4. Each plotted point on the curves represents 360 catches, *i.e.*, 20 catches by each of 18 subjects. Each of the two groups of ten catches made by a subject on a single day was dealt with separately when obtaining the average and standard deviation. This was done to show the progress made during the day. As might be expected, the second ten catches almost invariably averaged 1 or 2 c.c. higher than the first ten and the variability was usually smaller. For this brief paper we have averaged the two means

[1] The collection of these data, including its tabulation and elaboration, was successfully accomplished by an assistant, Mr. E. S. Mills, whose care and coöperation are gratefully acknowledged.

[2] In this early practice experiment it was thought that the subject should execute the pursuit by an arm movement not supplemented by a body movement. Therefore two rods were arranged to extend from the sink and to be brought in contact with the individual on both sides, somewhat above the waist. These rods, while not hindering the trunk from twisting, obviated the subject's swaying from side to side. Probably this restriction is unnecessary. It has not been used in later measurements.

and the two coefficients of variability secured for each of the 18 individuals on each day and have employed these 18 quantities to obtain the average represented by each plotted point on the curves shown in Fig. 4.

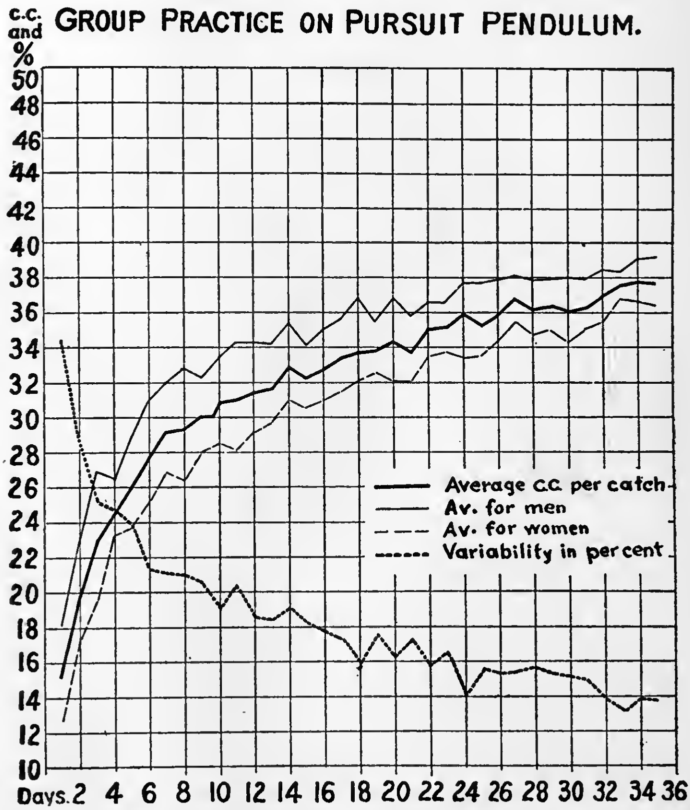

FIG. 4. Curves showing average results for ten women and eight men tested on 35 days with 20 catches per day practice.

The heavy broken line in the figure indicates the coefficient of variability (standard deviation divided by mean) in terms of per cent. The heavy solid line gives the average catch in cubic centimeters per day. On the first day the individual averages ranged from 8 to 29 c.c. per catch with a grand average of 15 c.c., which represents 30 per cent. of the possible catch. First trials by a number of other adults confirm this figure as about what may be expected for an initial performance, when the subject seriously tries the test and consistently makes 20 or 25 catches. The curve showing the average catch per day rises rapidly on successive days to 20, 23, 25, 26, 28, and, on the tenth day, to 31 c.c. representing 62 per cent. of the possible catch. Thus, on the tenth day of 20 trials the average efficiency has doubled over what it was at the start. After 25 days more of such practice the average increase above this level is only 7 c.c., bringing the figure to 38 c.c., which is about 75 per cent. of the possible catch. The practice curve is very regular in form and shows no definite indication of orthodox plateaux, and it is evident that the chief part of the rise due to practice can be quickly worked off by 200 or 300 catches, if it is desirable to bring the individual up toward the stage of a practice level. The curve for the coefficient of variability is practically an exact counterpart in form to that for the average catch. At the beginning the variation between catches equals about 34 per cent. and at the tenth day, when the average catch has doubled, the variability has decreased to 19 per cent. or not far from one half, and by the end of the series has decreased to about 14 per cent.[1]

It is recognized that the group of subjects employed in this experiment was relatively small and it is hardly justifiable to draw conclusions regarding such matters as the difference between men and women in their efficiency in executing such a pursuit movement. If the individuals are ranked on the basis of their total average catch per day, it is found that of the better nine there were six men and three women, while in the poorer half of the group there were two men and seven women. There were three women poorer than the poorest man, but only one man did better than the most efficient woman. The average difference between the groups of eight men and ten women is shown in Fig. 4 in the light line curves, which are above and below the curve for the general average

[1] The coefficient of variability for other neuro-muscular tests may be found by referring to Benedict, Miles, Roth, and Smith, Carnegie Inst. Wash. Pub. No. 280, 1919, pp. 551 et seq. Examples which may be mentioned are: eye-movement speed, 9 per cent.; eye reactions, 19 per cent.; word reactions, 9 per cent.; and electrical threshold about 6 per cent.

catch (heavy solid line). The curves for the men and for the women are fairly smooth, and maintain about a uniform distance apart, the men on the average catching 4 cc. more than the women.[1]

Individual practice curves, such as illustrated in Fig. 5, are naturally less smooth than the average for the whole group. In Fig. 5 results for one of the most efficient and also for one of the least capable subjects have been combined. Subject *C* made very rapid progress, starting with 21 c.c. and rising to 28, 30, 32.4, 35.8, and 36.7 c.c. on the next six successive days. (See Curve 1.) There is a slight decline on the seventh and eighth days and quite a definite decrease on the ninth day. The fluctuations usually range from 1 to 3 c.c. Undoubtedly these variations in the average would have been smaller had the number of trials on each day been larger. For a fairly long period, that is, from the tenth to the twenty-ninth day, the average for Subject *C* is very close to 37 c.c., which is 74 per cent. of the possible catch. The performance during this period may conceivably be classed as a plateau, for there is undoubtedly an indication of a definite stage of improvement following it, during the last six days. The coefficient of variability for Subject *C* (Curve 2) shows rapid improvement at first, corresponding to his improvement in the amount of the catch up to the seventh day. Beyond this time there are fluctuations, some of them quite large. From the seventh to the twentieth day, inclusive, the average variability is about 14 per cent., while from the twenty-first to the thirty-sixth day, although there are several instances as low as 8 or 10 per cent., the average is 12.5 per cent.

Subject *R* did poorly at the start, with an average catch of 8.5 c.c. (see Curve 3) and a variability of 54 per cent. (see Curve 4). Furthermore, poor learning ability is demonstrated by the results for the third and fifth days, when the

[1] Nothing extensive has been done with children. Probably in working with them the excursion of the pendulum should be reduced somewhat from the 70-centimeter swing employed with adults. However, preliminary trials with the apparatus as arranged for adults indicate that a nine-year-old child can catch at the beginning from 10 to 12 c.c. and a six-year-old child from 5 to 6 c.c.

average was in each case slightly lower than on the preceding days. The results show considerable progress between the fifth and the twentieth days, a change from 15.5 to 29 c.c. with some decrease in the variability, although the fluctuations here are quite large. The average for the last five days

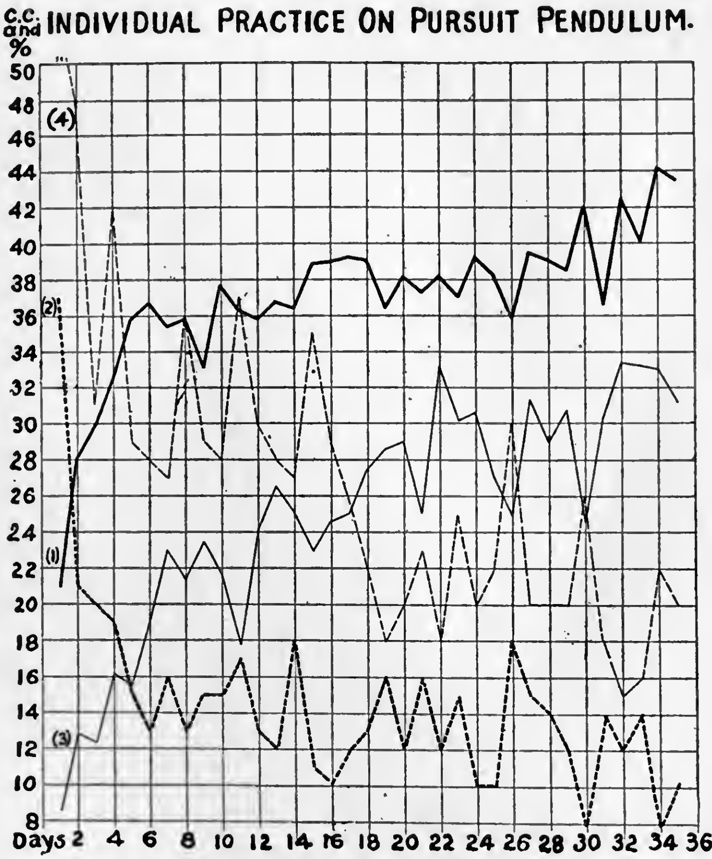

FIG. 5. Individual practice curves showing comparison between two subjects. Curve 1 shows the average catch per day and Curve 2 the average variability per day for one of the most skilful subjects. Curves 3 and 4 show the average catch and variability per day, respectively, for one of the least capable subjects.

is 32.3 c.c., or approximately 65 per cent. of the possible catch, with an average coefficient of variability for these same days of about 18 per cent., in contrast to the 75 per cent. catch and 14 per cent. variability for the total group of adults.

To illustrate a series of measurements made on an individual for the purpose of investigating the effect of some introduced factor on neuro-muscular efficiency the data in Table I are given. The pursuit-pendulum test was one of a number of measurements used in a recent alcohol experiment. This fragment of data is introduced for the sole purpose of illustrating the pursuit pendulum results and in no sense as a contribution to alcohol literature. The complete data are being elaborated for later publication as an alcohol research. The series of eight tests required 30 minutes. Following a light lunch, the subject carried through this series two times in succession. At the end of the second period he drank 1

TABLE I

THE PURSUIT PENDULUM AS A TEST OF NEURO-MUSCULAR EFFICIENCY. RESULTS SHOWING THE EFFECT OF ALCOHOL

Date	Successive Half-hour Periods					
	1	2	Drink	3	4	5
1919	c.c.	c.c.		c.c.	c.c.	c.c.
Nov. 5........	1,142	1,120	1 liter water	1,115	1,145	1,120
Nov. 8........	1,170	1,152	" " "	1,176	1,191	1,185
Nov. 19........	1,190	1,184	" " "	1,192	1,200	1,190
Nov. 21........	1,190	1,178	" " "	1,178	1,140	1,148
Nov. 22........	1,200	1,180	" " "	1,200	1,193	1,192
Av............	1,178	1,163		1,172	1,174	1,167
	1,170			+2	+4	−3
Nov. 6........	1,178	1,150	1 liter	1,092	1,108	1,096
Nov. 7........	1,156	1,166	2.75 alc.	1,137	1,122	1,066
Nov. 10........	1,172	1,185	27.5 grams	1,146	1,130	1,096
Nov. 17........	1,173	1,173	" "	1,130	1,043	1,065
Nov. 18........	1,172	1,208	" "	1,107	1,102	1,135
Av............	1,170	1,176		1,122	1,101	1,092
	1,173			−51	−72	−81
				53	76	78
				1,172	1,172	1,172
Av. loss, 5.9 per cent.				4.5	6.5	6.7

liter of water, or 1 liter of water in which 27.5 grams of ethyl alcohol had been diluted. The quantity and temperature of the liquid were not varied. Fifteen minutes were quite sufficient in which to drink the liquid. After the liquid was taken, the series of measurements was repeated three times, that is, periods 3, 4, and 5 of the day. Table I shows data for five normal days, on which only water was taken, and for five other days on each of which 1 liter of a 2.75 per cent. alcohol mixture was consumed. No effort was made to disguise the taste of the alcohol. The subject, an abstainer by habit, was in the best of physical condition. The values in Table I give the total catch in cubic centimeters for 25 cups, as measured by the method of emptying the whole bank of cups at once. (See Fig. 3.) The two preliminary periods for the five days on which water only was taken show total averages of 1,178 c.c. and 1,163 c.c., or a combined preliminary average of 1,170 c.c. (46.8 c.c. per catch), which compare favorably with the two preliminary values, *i.e.*, before alcohol was taken, on the alcohol days, namely, 1,170 c.c. and 1,176 c.c., or a combined average of 1,173 c.c. (46.9 c.c. per catch). Periods 3, 4, and 5 show only minor differences (+ 2 c.c., + 4 c.c., and − 3 c.c.) from the combined preliminary average in the case of normal days. On the alcohol days the differences are all minus, that is, less water was caught after the alcohol was taken by − 51 c.c., − 72 c.c., and − 81 c.c. for the total averages. Subtracting the alcohol differences from those for normal days, it is found that 53 c.c., 76 c.c., and 78 c.c. represent the alcohol effect for periods 3, 4, and 5, respectively. These decrements between normal and alcohol performance, on the basis of 1172 (the grand average for all preliminary trials on both groups of days), equal 4.5, 6.5, and 6.7 per cent., or an average loss of 5.9 per cent., which represents the alcohol effect on this test of coördination. The subject improved somewhat between November 5 and 22, but since there is only slight improvement within the day and the water and alcohol experiments alternate with each other, this practice change is not troublesome. Indeed, the data are very consistent, *e.g.*, at no time following ingestion of alcohol was the subject able to catch as much as he had in the poorer preliminary period for the same day.

A pursuit coördination test, such as has been here described, not only possesses the advantages of requiring very simple apparatus and of securing quantitative results which are immediately available without the painstaking reading of records, but it appears to have elements comparable to many industrial operations where the task not only requires quick reaction but also that a movement or movements be executed according to a fairly definite pattern. A reaction is usually only the beginning of coördinated movement towards some end or of a series of such coördinations, and probably in most instances in practical life the adequacy with which the series of coördinations is carried through is fully as important, if not more so, than the mere matter of speed in initiating them. Especially would this appear to be the case in movements for compensating, directing, aiming, or otherwise tending any moving object, where the pace and pattern are not set entirely by the subject himself. Quickness, precision, and steadiness of movement have long been tested in reference to stationary objects. The pursuit pendulum provides a simple means of measuring these factors in reference to a moving object and thus supplements the general psychological measurement of motor control and capacity. The pursuit test invariably challenges a subject's interest, but practically every one finds it more difficult than he at first expects.

THE LIMITS OF COLOR SENSITIVITY: EFFECT OF BRIGHTNESS OF PREËXPOSURE AND SURROUNDING FIELD

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Bryn Mawr College

Introduction

The difficulty of getting reproducible results in determinations of the color sensitivity of the peripheral retina is a common complaint among clinic workers. This difficulty is so great as to lead many seriously to question the value of such determinations in the work of diagnosis. Their value in diagnosing and in checking up the course of some of the most serious affections of the eye is readily conceded, however, provided the needed precision can be attained. The need of greater precision of working in the laboratory, while less important to human welfare, is no less insistent. These combined needs led us several years ago to make a study of the variable factors which influence the chromatic response, the details of which are still in progress. Some of these factors pertain to the control of the stimulus, some are peculiar to the response of the eye itself. All may be standardized and controlled. The normal eye is highly sensitive and complex in its responses but not inherently erratic. While the abnormal eye may be more erratic, one of the symptoms it may be of its abnormality, there should be so far as we can see no essential difference in the technique of the study and of the testing of its functioning. In fact a characteristic difference in this regard, which can be determined with certainty only when other variable factors are controlled, may well be found to serve as a clue to an early diagnosis of its abnormality.

The variable factors which influence the chromatic response of the retina are, so far as we have discovered, the wave-length and the purity of the stimulus, the intensity of the stimulus and the visual angle, length of exposure of the eye, accuracy and steadiness of fixation, general illumination and state of adaptation of the retina, breadth of pupil, and the brightness of the preëxposure and of the field surrounding the stimulus. We have already published considerable data on the effect of these factors in earlier papers (**1**). It will be the special purpose of the present paper to deal with the last two, the brightness of the preëxposure and of the surrounding field. A detailed explanation of the effect of these two factors on the amount of the chromatic response has been given in the second of the papers referred to above (**1**). A brief explanation and statement of principles will suffice here.

1. When a small colored stimulus, surrounded by a field, for example, of white or black is viewed, a sensation is given which consists of the color mixed with black or white, due to a contrast sensation induced from the surrounding field. The effect of fusing a color with white or black is twofold. (*a*) There is a quantitative effect due to the inhibition of the chromatic excitation by the achromatic. In general, in the central retina at medium and high illuminations, white inhibits color the most, the grays in order from light to dark next, and black the least. Also the amount of the inhibitive action varies with the different colors, with the part of the retina at which the stimulation takes place, and the state of brightness adaptation of the retina. The amount of induction depends upon the difference in brightness between the stimulus and the surrounding field; it increases with the distance from the fovea and with decrease in the general illumination; and, with a given difference in brightness between the stimulus and the surrounding field, it is greater with a white than with a black field—also the amount of increase of induction with decrease of illumination and with increase of distance from the fovea is greater with a white than with a black field. And (*b*) there is also a qualitative effect. The hue of certain colors is changed by the action of the achromatic excitation. The change is greatest when the stimuli are blue and yellow. For example, yellow when

mixed with black gives a greenish yellow which with the right proportion of components may become an olive green; and blue when mixed with white or light gray gives a sensation of reddish blue.

2. When making the color observation in the peripheral retina, the observer is given a short period of preparation before the stimulus is exposed, in which to obtain and hold a steady and accurate fixation. This introduces the factor of preëxposure for, during this period of preparation, the area which is to be stimulated by color receives a previous stimulation. This previous stimulation, when it differs in brightness from the color, gives a brightness after-image which mixes with the color sensation and both reduces its saturation and modifies its color tone. If the preëxposure is lighter than the stimulus color, it adds by after-image a certain amount of black to the succeeding color impression; if darker, it adds a certain amount of white. Since both white and black as after effect reduce the sensitivity to color, the eye is rendered more sensitive when no after-image is given, that is when the preëxposure is of the same brightness as the color. The preëxposure should, therefore, be a gray of the brightness of the color. No brightness after-image will then be added to the succeeding color impression to modify either its saturation or color tone. The only brightness change acting upon it will be due to the slight adaptation to this gray during the short time of preëxposure. Even closing the eye, as is frequently done before stimulating, is equivalent to giving a black preëxposure.

The general principle then is clear. There remains only to explain why in the peripheral retina the short preëxposure which takes place while the eye is obtaining a steady fixation has so much effect on the color stimulation immediately following. Two reasons are found for this. (*a*) The after-image reaction of the peripheral retina is extremely quick. While some slight variation is found at different angles of excentricity, in general the peripheral after-image seems to reach its maximal intensity with a few seconds of stimulation. This amount of time is usually consumed in obtaining fixation and preparing for the stimulation, hence in each observation there is fused with the color sensation about as strong a brightness after-image as can be aroused. For this reason alone it is readily seen why the brightness of the preëxposure is of so much greater consequence in the peripheral than in the central retina, where the maximal intensity of after-image is, roughly speaking, obtained from a stimulation of 40–60 seconds or longer. (*b*) There is apparently no latent period in case of the peripheral after-image. It flashes out at full intensity immediately upon the cessation of the stimulation. Thus there is no possibility of escaping the full effect of the brightness after-image on the stimulus color as might happen in the central retina where the latent period obtains, if there were a very short exposure to stimulus color.

Conditions under which the Work was Done

The determinations were made in an optics room of the type described in previous articles (2). The illumination was kept constant at a value at the point of work of 42 foot-candles, vertical component; 31.2 foot-candles, 45 degree component; and 12.5 foot-candles, horizontal component. Three investigations were conducted.

1. A determination was made of the effect on the apparent limits of color sensitivity of variations in the brightness of the field surrounding the stimulus. Three fields were used: the standard white of the Hering series, giving a surface brightness at the intensity of illumination used of 0.0209 candle-power per sq. in.; the standard black of the series, giving a surface brightness of 0.00094 candle-power per sq. in. and grays of the brightness of the color at the limits of sensitivity in each of the meridians investigated. These grays ranged in brightness in the different meridians from 0.00350 to 0.00395 cp. per sq. in. for red; 0.01445 to 0.0189 for yellow; 0.01058 to 0.01185 for green; and 0.00289 to 0.00366 for blue. In order to study the effect of brightness of surrounding field in separation, the preëxposure was in each case made of the brightness of the color at the point of investigation.

2. A determination was made of the effect on the apparent limits of sensitivity of varying the brightness of the preëxposure. Again three brightnesses were used: the standard Hering white; the standard Hering black; and grays of the brightness of the color at the limits of sensitivity in each of the meridians investigated. The photometric value of the white, black and the range of grays for each of the colors are given in 1 above. In this series of experiments the surrounding field was made in each case of the same brightness as the color at the point of investigation.

3. A determination was made of the combined effect of preëxposure and surrounding field on the apparent limits of sensitivity. The same three brightnesses were used as in the preceding investigations. In these cases, however, the surrounding field and preëxposure were both made of the same brightness, *i.e.*, both white, black or grays of the brightness of the color at the limits of sensitivity in the meridians investigated.

Since the results obtained were meant only to be comparative of the effect of varying given factors, it was deemed sufficient to make the determinations with pigment stimuli. So obtained the results are moreover more nearly what may be expected in the work of the clinic. The standard red, yellow, green and blue of the Hering series of papers were used. The work was done with the rotary campimeter described in previous papers (**3**). With the control of surrounding field afforded by the campimeter, this apparatus combines the rotary features of the perimeter. Without some apparatus combining both of these features we have not found it possible to make a determination of the apparent limits of sensitivity with an adequate control of the brightness of the surrounding field and of the preëxposure. The need of an apparatus in the clinic by means of which this control may be accomplished is obvious. Not only is it impossible to secure an adequate control of these two important factors by means of the standard perimeter, but a very great practical difficulty is encountered in daylight work in getting an equal illumination of the pigment stimulus at different points in the field of vision and a constant illumination from sitting to sitting. In case of artificial illumination the latter difficulty can perhaps be eliminated with care; but the task of securing an equal effective illumination of the stimulus from point to point in the same meridian and of corresponding points in different meridians is practically impossible in case of any perimeter now in use, because of the unequal shading of the moving stimulus by the observer, the varying inequalities of the incident and reflecting angles, etc. In case of the instrument used by us these difficulties are minimized by using a stationary pigment surface, 20 x 20 cm. placed with special reference to evenness of illumination at some constant distance (in the present work 45 cm.) behind the stimulus opening in the campimeter and by securing the excentric stimulation by shifting the fixation from point to point along an arm specially constructed for the purpose. For other points of criticism of the perimeter as an instrument of precision for either light or dark room work the reader is referred to former papers. The preëxposure was secured by inserting the appropriate pigment surface between the stimulus card and the stimulus opening in the campimeter. The duration of the preëxposure was kept constant at 2 seconds. The stimulus opening in the campimeter was 15 mm. in diameter. At the eye, 25 cm. distant, this subtended a visual angle of 3° 26′.

The more important results given in this paper have been confirmed repeatedly both in the graduate and undergraduate work in our laboratory. The determination of the effect of the brightness of preëxposure and surrounding field on the apparent limits of color sensitivity has in fact formed a part of the drill work in the undergraduate laboratory for several years. Space will be taken here for the results of only one observer—the observer whose results have been given in the preceding studies on the color sensitivity of the peripheral retina.

As has already been indicated, the effect of brightness of the preëxposure and of the surrounding field falls under the general heading of the inhibitive action of the achromatic excitation on the chromatic. This action takes place however the achromatic excitation is aroused—by the admixture of white light, by after-image, by contrast, etc. It may be strikingly and conveniently demonstrated to large numbers at once in the following lecture room experiments. (*a*) Set up side by side on three color mixers discs made up of 180 degrees of color, *e.g.* blue, and 180 degrees of white, 180 degrees of blue and 180 degrees of gray of the brightness of the blue, and 180 degrees of blue and 180 degrees of black. When mixed, although the eye receives the same amount of colored light from each set of discs, the mixture with black seems to have lost but very little, if any, color; the mixture with white is a lavendar with but little color; and the mixture with gray of the brightness of the color, in this case a very dark gray, is less saturated than the mixture with black. When different grays are used the saturation decreases apparently in graded steps as white is approached. The demonstration can be made on a single color mixer by compounding the color disc with white, black and gray discs of different breadths or radii. When rotated this gives the effect of a surface made up of three concentric zones or rings, one in which the color is mixed with white, one with gray and the other with black. The demonstration may be made roughly quantitative by determining the proportions of color required to give the chromatic threshold in black, white and the grays; also by determining the proportions of color and the achromatic series to give equal saturations.

(*b*) Prepare a preëxposure surface, half white and half black, 50 x 60 cm. Expose the eye 15–20 seconds and project the after-image on a colored surface, *e.g.*, blue, of the same dimensions. The half of the field preëxposed to black will appear a very pale unsaturated lavendar, while the half preëxposed to white will be a dark strongly saturated blue, although the eye receives the same amount of light from both halves of the field. As the after-image dies away the two halves of the field become more and more nearly alike in saturation and color tone. If desired, the preëxposure surface may be made of white, black and a series of graded grays, appropriately arranged. When this is done the graded loss in saturation due to the different brightnesses of the after-image may be observed. This demonstration also may be made quantitative by finding the threshold of color after the eye has been preëxposed for 15–20 seconds to white, black and the grays.

(*c*) Prepare contrast discs with narrow rings of color and inside and outside surfaces of black, white and a gray of the brightness of the color, respectively. Set up on color mixers side by side and rotate to smooth out all margins. The colors are lightened and darkened respectively by contrast induced by the black and white fields. The effect of these achromatic excitations on the hue and saturations of the colors is similar to that obtained in the former experiments. A more striking effect is produced if a mixed color, *e.g.*, orange, is used. The quantitative features noted above can also be utilized in this demonstration by employing for the contrast ring in each case a gray of the brightness of the color and enough of the color to give the threshold of color sensation when acted upon by the white and black inductions. The effect of induction and after-image, it will be remembered, are not nearly so striking in the central as in the peripheral retina. Much more induction with a given brightness difference between the inducing and the contrast field, for example, is produced in the peripheral retina, and only a short period of preëxposure (2–3 seconds) is required to give a strong after-image with no latent period.

Results

The following results were obtained: (1) The widest angular limits of the color zones were obtained when the preëxposure and surrounding field were of the same brightness as the color. (2) When the brightness of preëxposure and surrounding field were different from that of the color, the effect of surrounding field was less than that of preëxposure; and the effect of either is always less than the combined effect of both. (3) In some meridians the effect of surrounding field alone narrowed the limits as much as

11 degrees; the effect of preëxposure alone, as much as 17 degrees; and the combined effect of preëxposure and surrounding field, as much as 20 degrees.

(4) The amounts the limits were narrowed for red, yellow, green and blue, respectively, by a white preëxposure alone ranged in the different meridians[1] from 4–15 degrees, 2–17 degrees, 3–15 degrees, and 4–12 degrees; by a black preëxposure from 3–11 degrees, 3–10 degrees, 4–13 degrees, and 2–12 degrees; by a white surrounding field 1.5–10 degrees, 2–9 degrees, 2–11 degrees, and 2–10 degrees; by a black surrounding field 1–8 degrees, 1–8 degrees, 2–10 degrees, and 1.5–9 degrees; by a combined white preëxposure and white surrounding field 5–19 degrees, 2–20 degrees, 4–20 degrees, and 5–17 degrees; by a combined black preëxposure and black surrounding field 4–17 degrees, 5–12 degrees, 7–18 degrees and 5–18 degrees. When the effect of a white or black surrounding field alone was wanted, the preëxposure was made of the same brightness as the color at the point of investigation; similarly when the effect of a white or black preëxposure was wanted, the surrounding field was made of the same brightness as the color at the point of investigation. The value of the limits with a preexposure and surrounding field of the same brightness as the color served in each case as the standard value in terms of which to estimate the amounts the limits were narrowed by the white and black preëxposures and surrounding fields and their combinations.

These values, it will be remembered were obtained with a very precise control of the illumination of the working surfaces. It is obvious that a much greater variability of result should be expected had there been no better control of the constancy of illumination than is ordinarily exercised in office and clinic work, and too often in laboratory work. The effect on both the limits and hue of the color of such variations in the daylight illumination of the working surfaces as are apt to occur over long periods of time when no especial control is exercised, will be given in a later paper.

[1] In the order shown in the tables.

In order to realize how profoundly the powers of chromatic response must have been affected to change the limits of sensitivity by the amounts represented by the above figures one must bear in mind how abruptly sensitivity falls off in the far periphery of the retina. A determination of the thresholds of color in the temporal meridian with preëxposure

Table I

Limits of Color Field for Red

Showing the Effect of Brightness of Preëxposure, Brightness of Surrounding Field, and the Combined Effect of Brightness of Preëxposure and Surrounding Field on the Apparent Limits for Red

Meridian	Effect of Preëxposure[1]			Effect ot Surrounding Field[2]			Combined Effect of Preëxposure and Surrounding Field		
	Gray of Brightness of Color	White	Black	Gray of Brightness of Color	White	Black	Gray of Brightness of Color	White	Black
Upper 0°......	58	45	47	58	48	50	58	40	41
Nasal 25°......	49	43	43	49	46	46	49	41	39
" 45°......	49	43	41	49	46	44	49	38.5	37.5
" 70°......	47	43	42.5	47	45.5	44.5	47	41	40
" 90°......	43	38	37	43	41.5	40	43	38	38
" 110°......	47	42	42	47	43	43	47	41	42.5
" 135°......	50	46	45	50	48	47	50	45	44
" 155°......	51	47	47	51	48.5	48.5	51	46	46
Lower 180°......	60	53	56	60	55	57	60	52	56
Temporal 25° ..	73	59	68	73	66	70	73	55	62
" 45° ..	79	64	74	79	70	76	79	60	72
" 70° ..	85	75	80	85	80	82	85	69	78
" 90° ..	89	83	85	89	85	88	89	80	84
" 110° ..	89	82	85	89	84	86	89	80	83
" 135° ..	85	78	82	85	81	83	85	77	81
" 155° ..	75	62	65	75	65	68	75	60	64

and surrounding field of the same brightness as the color for red, yellow, green and blue at 5 degrees, 3 degrees, 2 degrees and 1 degree respectively from the limit shows the following values: for red, 132, 150, 250 and 320 degrees; for yellow, 100, 150, 240 and 330 degrees; for green 130, 145, 260 and 345 degrees; and for blue 130, 145, 200 and 310 degrees.

[1] In determining the effect of the different brightnesses of preëxposure, the brightness of the surrounding field was made equal to that of the color at the point of investigation.

[2] In determining the effect of the different brightnesses of surrounding field, the brightness of the preëxposure was made equal to that of the color at the point of investigation.

For red thus there was an increase of 172.7 per cent. in the threshold in passing to the limit from a point 5 degrees from the limit; for yellow an increase of 260 per cent.; for green an increase of 207.7 per cent.; and for blue an increase of 207.7 per cent. For a more detailed experimental analysis of the effect of preëxposure, surrounding field, intensity of

Table II

Limits of Color Field for Yellow

Showing the Effect of Brightness of Preëxposure, Brightness of Surrounding Field, and the Combined Effect of Brightness of Preëxposure and Surrounding Field on the Apparent Limits for Yellow.

Meridian	Effect of Preëxposure[1]			Effect of Surrounding Field[2]			Combined Effect of Preëxposure and Surrounding Field		
	Gray of Brightness of Color	White	Black	Gray of Brightness of Color	White	Black	Gray of Brightness of Color	White	Black
Upper 0°	47	41	37.5	47	41	39	47	38	36
Nasal 25°	42	39	38	42	40	39	42	38.5	37
" 45°	42	37	36	42	40	38	42	37	35.5
" 70°	46	42	40	46	44	42	46	42	39
" 90°	44	42	40	44	42	41	44	42	38.5
" 110°	46	42	38	46	43	41	46	41	37
" 135°	50	46	45	50	48	47.5	50	46	45
" 155°	48	44	44	48	46	46	48	43	43
Lower 180°	59	51	54	59	53	56	59	47	52
Temporal 25°	65	48	55	65	58	61	65	45	53
" 45°	73	63	70	73	68	72	73	62	67
" 70°	87	70	84	87	79	86	87	69	80
" 90°	89	75	85	89	80	87	89	72	84
" 110°	89	81	86	89	83	87	89	80	85
" 135°	87	80	84	87	82	85.5	87	78	84
" 155°	72	60	65	72	63	67	72	59	63

the illumination of the visual field, amounts of induction with different brightness relations of surrounding field to stimulus at different intensities of illumination, etc., and the effect of all of these on the thresholds of color and the limits of sensitivity the reader is referred to the first two papers cited in the appended bibliography (1).

5. In those meridians in which the limits are wide there is a general tendency for the white preëxposure and surrounding field to narrow the limits more than a black preëxposure

[1] Brightness of Surrounding Field: gray of the brightness of yellow.

[2] Brightness of Preëxposure: gray of the brightness of yellow.

and a black surrounding field. We have stated in our introduction that the amount of inhibition of the chromatic by the achromatic excitation varies with the color, the part of the retina stimulated and the state of adaptation of the retina. This statement applies also to the relative effects of white and black. In the central retina at medium and high illuminations white inhibits color much more than black.

Table III

Limits of Color Field for Green

Showing the Effect of Brightness of Preëxposure, Brightness of Surrounding Field, and the Combined Effect of Brightness of Preëxposure and Surrounding Field on the Apparent Limits for Green

Meridian	Effect of Preëxposure[1]			Effect of Surrounding Field[2]			Combined Effect of Preëxposure and Surrounding Field		
	Gray of Brightness of Color	White	Black	Gray of Brightness of Color	White	Black	Gray of Brightness of Color	White	Black
Upper 0°......	36	26	29	36	28	31	36	27	22
Nasal 25°......	35	30	27	35	31	29	35	26	21
" 45°......	38	30	28	38	32	30	38	29	24
" 70°......	39	34	31	39	36	31	39	32	27
" 90°......	39	35	33	39	37	35	39	33	28
" 110°......	37	31	31	37	33	33	37	31	30
" 135°......	37	32	29	37	34	31	37	31	25
" 155°......	33	30	29	33	31	30	33	29	26
Lower 180°......	37	32	31	37	34	33	37	28	26
Temporal: 25° ..	37	30	30	37	34	35	37	28	26
" 45° ..	42	34	36	42	39	40	42	30	33
" 70° ..	61	51	53	61	56	57	61	47	50
" 90° ..	69	56	60	69	60	62	69	50	53
" 110° ..	65	53	56	65	58	61	65	46	50
" 135° ..	57	42	44	57	46	47	57	37	39
" 155° ..	44	39	37	44	41	39	44	35	34

At these illuminations therefore a black preëxposure and surrounding field are much more unfavorable than white. At lower illuminations this difference in effect becomes less pronounced. In the far periphery of the retina the following are some of the conditions which contribute to make black as preëxposure and surrounding field give wider limits of sensitivity. (*a*) A condition of low illumination and a state of low illumination adaptation. (*b*) A darkening of all of the

[1] Brightness of Surrounding Field: gray of the brightness of green.

[2] Brightness of Preëxposure: gray of the brightness of green.

colors, particularly red and yellow (the Purkinje shift of the peripheral retina). This brings the brightness of the color nearer to black than to white and the stronger relative darkening of red and yellow than of their neutral or colorless preëxposures and surrounding fields, increases the contrast

Table IV

Limits of Color Field for Blue

Showing the Effect of Brightness of Preëxposure, Brightness of Surrounding Field, and the Combined Effect of Brightness of Preëxposure and Surrounding Field on the Apparent Limits for Blue

Meridian	Effect of Preëxposure[1]			Effect of Surrounding Field[2]			Combined Effect of Preëxposure and Surrounding Field		
	Gray of Brightness of Color	White	Black	Gray of Brightness of Color	White	Black	Gray of Brightness of Color	White	Black
Upper 0°	52	40	46	52	42	48	52	35	34
Nasal 25°	45	39	39	45	41	42	45	38	36.5
" 45°	48	44	46	48	45	46	48	40	40
" 70°	46	41	41	46	44	44	46	41	41
" 90°	52	42	42	52	48	47	52	42	40
" 110°	50	46	45	50	47.5	47	50	44	43
" 135°	52	47.5	47.5	52	50	49	52	46	46
" 155°	58	48	46	58	51	49	58	43	42
Lower 180°	70	63.5	62	70	66	64.5	70	61	59
Temporal: 25°	70	62	65	70	65	68.5	70	56	59
" 45°	79	71	73	79	73	75	79	65	69
" 70°	86	78	82	86	80	84	86	77	80
" 90°	91	86	85	91	89	89	91	84	84
" 110°	91	85	85	91	88	88	91	83	83
" 135°	89	84	83	89	86	85	89	83	83
" 155°	80	75	75	80	77	77	80	75	74

and after-image effect for white and decreases it for black. The darkening of red and yellow in passing to the far periphery of the retina is very great. In the nasal half of the retina with its wide limits, the effect of this darkening on the results of our determinations was, of course, the most pronounced. As colors darken, there is, when a certain point in the process is reached, varying with the color, a tendency for them to lose their saturation very rapidly. (*c*) Achromatic induction increases very strongly with decrease of illumination and therefore increases in passing from the center to the periphery

[1] Brightness of Surrounding Field: gray of the brightness of blue.

[2] Brightness of Preëxposure: gray of the brightness of blue.

of the retina. It increases much faster for white than for black.

In the meridians in which the limits are narrower the situation is more nearly as it is in the central retina. Here the tendency is for the limits to be narrowed more by a black

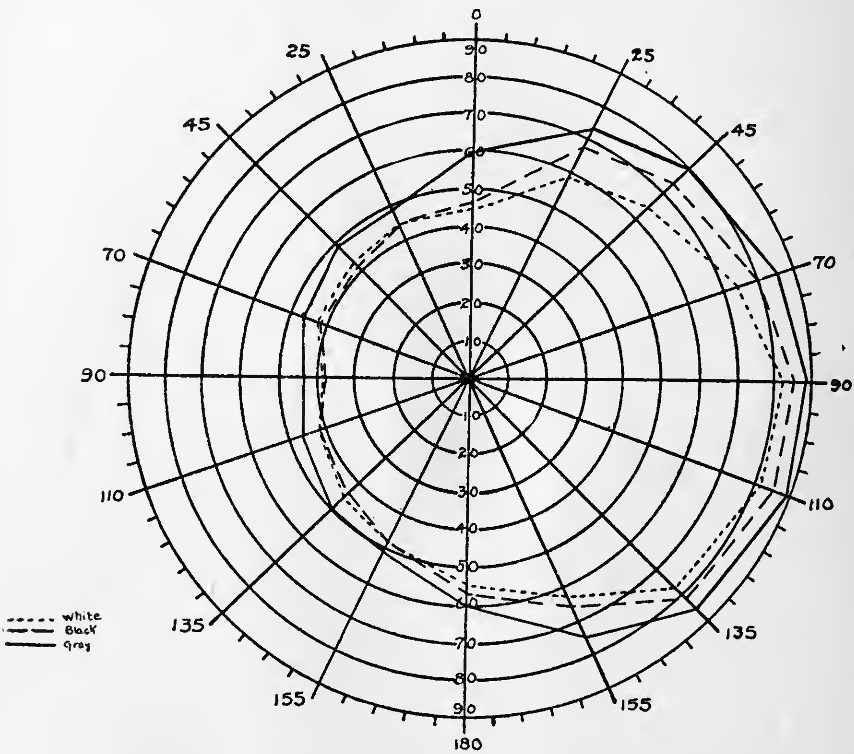

Fig. 1. Effect of brightness of preëxposure on the limits of the color field. In this chart are shown the apparent limits for red with preëxposures respectively of white, black, and gray of the brightness of the color at the point of investigation, surrounding field in each case gray of the brightness of the color at the point of investigation.

than by a white preëxposure and surrounding field. In some meridians the amount of narrowing is approximately equal for both. Another factor which tends to make the effect more nearly the same in these meridians for all backgrounds and preëxposures is the more abrupt falling off in sensitivity. That is, more effect on sensitivity is required here to change the limits by a detectable amount than is required in those portions of the retina where the sensitivity grades off more slowly.

A detailed representation of the results is given in Tables I–IV. and a graphic representation of a part of the results in Figures 1–6. In the tables results are given separately

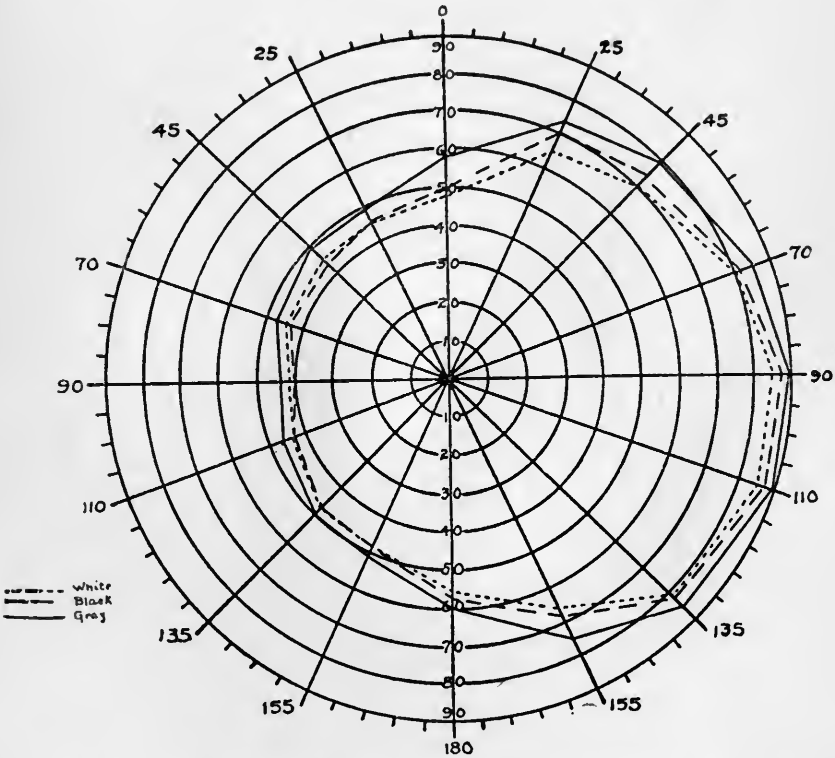

Fig. 2. Effect of brightness of surrounding field on the limits of the color field. In this chart are shown the apparent limits for red with a surrounding field respectively of white, black, and gray of the brightness of the color at the point of investigation, preëxposure in each case gray of the brightness of the color at the point of investigation.

for the effect of preëxposure, surrounding field and combined effect of preëxposure and surrounding field for each of the four colors: red, yellow, green and blue. In case of the figures, however, space has been taken to represent separately the effect of preëxposure and surrounding field for only one of the colors, red—Figs. 1–3. Figs. 3–6 show the combined effect of preëxposure and surrounding field on each of the four colors. In our previous papers the representation of results has been in terms of position on the retina. In this paper the representation has been made in terms of field of vision.

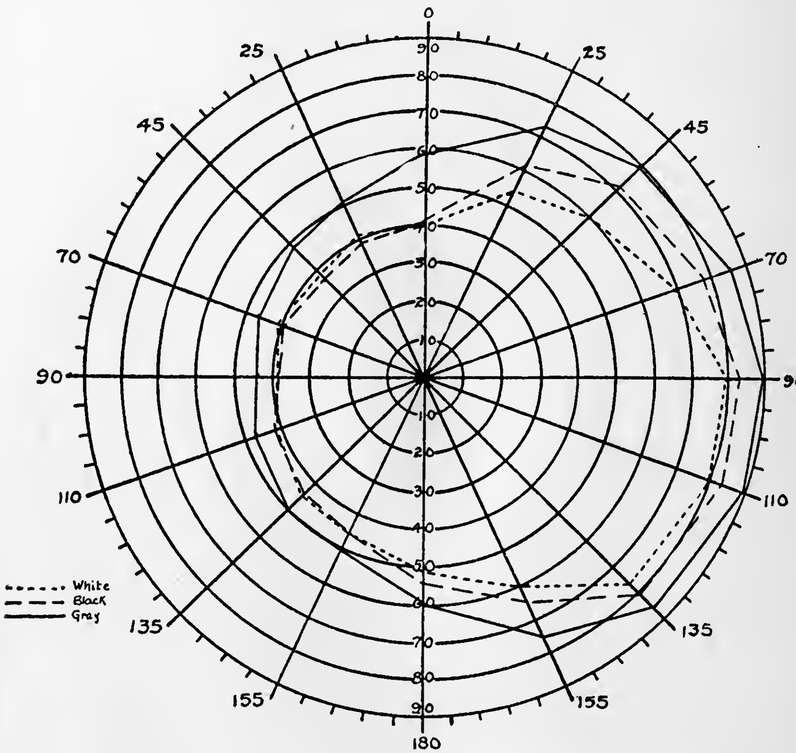

FIG. 3. The combined effect of brightness of preëxposure and surrounding field on the limits of the color field. In this chart are shown the apparent limits for red with both preëxposure and surrounding field respectively of white, black, and gray of the brightness of the color at the point of investigation.

CONCLUSION

It is quite obvious from the preceding data that reproducible results can not be hoped for in perimetric or campimetric determinations of the sensitivity of the peripheral retina unless the variable effects of preëxposure and surrounding field be eliminated from the conditions of work. This can be done completely only by making the brightness of the preëxposure and surrounding field in each case the same as that of the color employed and working under constant intensity of illumination. Among the effects of a variable intensity of illumination on the results of a perimetric or campimetric determination the following two may be men-

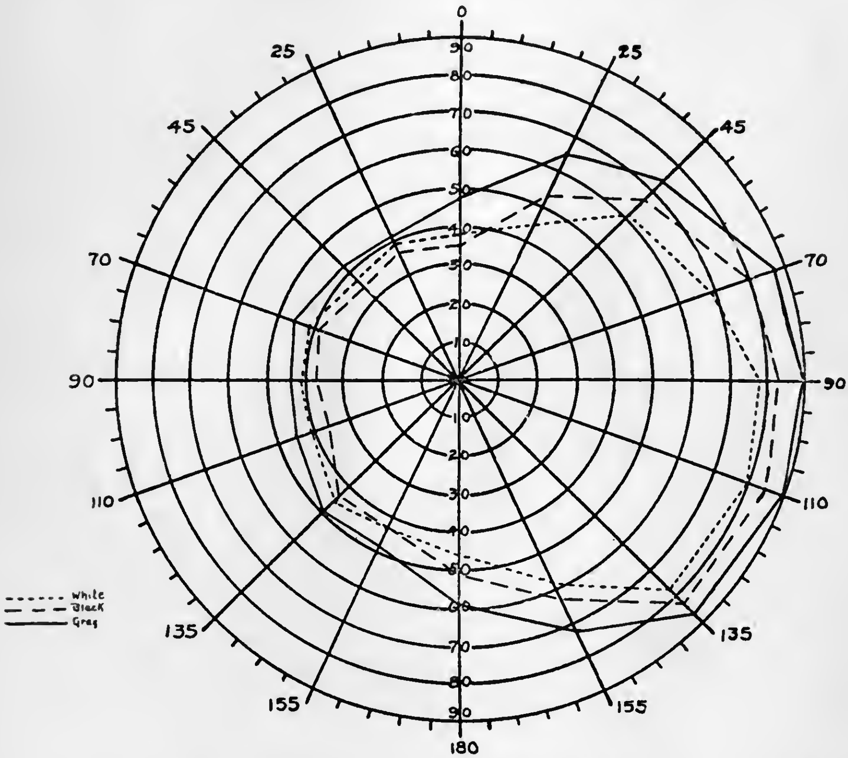

Fig. 4. The combined effect of brightness of preëxposure and surrounding field on the limits of the color field. In this chart are shown the apparent limits for yellow with both preëxposure and surrounding field respectively of white, black, and the gray of the brightness of the color at the point of investigation.

tioned. (*a*) When the color stimulation is given by light reflected from pigment stimuli of a given coefficient of reflection the amount of colored light obtained depends upon the intensity of light incident on the reflecting surface. And (*b*) a brightness match of preëxposure and surrounding field with the stimulus surface will not hold at different illuminations (the Purkinje phenomenon).

We have worked out in previous papers the conditions under which the desired standardization of intensity and color value of illumination and control of brightness of preexposure and surrounding field may be obtained in labor-

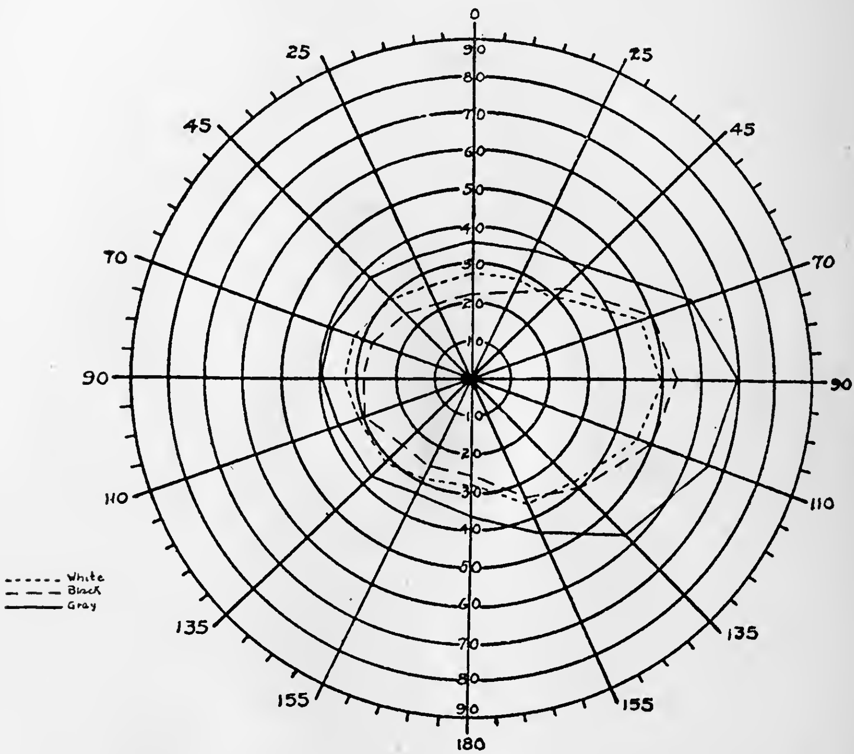

FIG. 5. The combined effect of brightness of preëxposure and surrounding field on the limits of the color field. In this chart are shown the apparent limits for green with both preëxposure and surrounding field respectively of white, black, and gray of the brightness of the color at the point of investigation.

atory campimetry (**4**). These conditions however are scarcely feasible for the work of the office or clinic. We have therefore more recently devised and constructed a perimeter by means of which equal illumination of the stimulus is received at every point on the perimeter arm in all meridians and the effect of brightness of preëxposure and surrounding field can be eliminated with an ease and speed of manipulation which should be feasible for office and clinic work and with a completeness of result that should be adequate for this type of work. We have in fact constructed two types of perimeter either one of which provides for the uniform illumination of the arm of the perimeter. The perimeters will be described in a later paper.

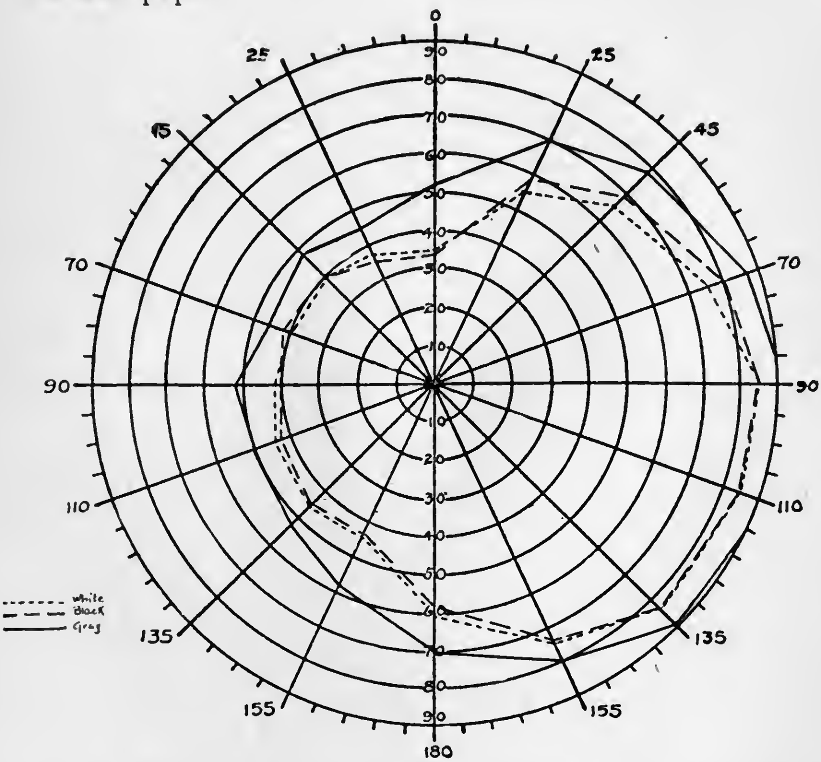

Fig. 6. The combined effect of brightness of preëxposure and surrounding field on the limits of the color field. In this chart are shown the apparent limits for blue with both preëxposure and surrounding field respectively of white, black, and gray of the brightness of the color at the point of investigation.

Comment

A much more detailed study of the quantitative relations of the chromatic and achromatic components of the visual sensation for different intensities of stimulus and for different states of the reacting eye is needed. There are many important practical bearings of the knowledge that would be gained by such a study. For example, it is often deemed sufficient to give a colorimetric specification of a light at one intensity alone in spite of the fact that the saturation, even the hue of the color, changes with the intensity as well as the composition of the light. We are all familiar in a general way with the fact that even the sensation aroused by a spectrum band of light begins as achromatic or colorless at very low intensities, passes through saturation and hue changes with increase of intensity of light and finally becomes colorless again at high intensities. We have pointed out many times in connection with problems of lighting (5) that while a specification of the composition of light is independent of intensity, a true colorimetric specification may not, depending on the method used, be definite unless it is accompanied also with a specification of intensity. Filters designed to give a certain coloration of light can not be depended upon to give this subjective coloration at all intensities even though the wave-lengths transmitted are in the same proportions. Indeed when used in connection with the same intensity of source the coloration of the illumination of an object as seen by the eye, particularly the saturation, will vary at different distances from the source. The lack of realization of this dependence of the color of light on its intensity as well as its composition has doubtless played no small part in the popular confusion which exists as to the comparative color values of different artificial lights and of the closeness of approximation of certain artificial lights to daylight. The surface of a Welsbach mantle, 0.7 per cent. ceria, viewed directly is, for example, whitish; but the reading page illuminated by it to ordinary working brightness appears distinctly yellowish green. Again the illumination given by the blue bulb lamp may be judged of different color values depending upon the intensity of light falling on the illuminated object. Complementary colors combined to gray at medium or high intensities may not be seen as colorless at low illuminations, *e.g.*, the gray produced by combining the Hering standard blue and yellow under daylight of good intensity becomes distinctly lavendarish under the same light at low intensities. Daylight itself is popularly said to become bluish at low intensities. Examples may thus be multiplied indefinitely of the apparently peculiar complexity of the selectiveness of the eye's chromatic response to intensity.

In addition to the practical bearings of the shifting of the quantitative relations of the achromatic and chromatic components in the visual sensation, with no change in the composition of light, there is the interesting problem of explanation. Many factors, it may be, are operative in the production of this phenomenon: a selectiveness of response to intensity, perhaps even a change in the range of the eye's chromatic response to wave-length with change of intensity, in case of spectrum lights; this and slight variations for change of intensity, in the cancelling proportions of the complementary colors and in the mutually inhibitive actions of the non-complementary colors, in case of mixed lights; a direct action of the achromatic excitation on the chromatic, for both simple and mixed lights; etc. It seems not only reasonable but necessary to infer this latter action because the same type of effect is produced on the color when the achromatic component of the sensation is varied in all of the following ways: by keeping the composition of the light the same and varying its intensity, by adding colorless light, by adding white or black to the sensation as after-image or contrast, and by the achromatic changes in adaptation. No other explanation seems possible when the phenomenon is produced as an effect of preëxposure and surrounding field or as we commonly say by after-image and contrast, as has been the case in the work reported in this paper.

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THE PSYCHOLOGICAL REVIEW

DO WE THINK IN WORDS?

BEHAVIORIST VS. INTROSPECTIVE CONCEPTIONS

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1. *Purpose of the Discussion.*—The following discussion takes its departure from the reading of Dr. John B. Watson's 'Psychology from the Standpoint of a Behaviorist.'

It is the purpose of the writer to discuss certain hypotheses which are put forth with seeming conviction in the text but which are believed by the writer to be false.

To discuss the whole subject of the Behaviorist point of view, in relation to the more generally accepted points of view in psychology would be quite impossible in the scope of this article. That a text in psychology should be written in which the author not only purposefully avoids the mention of such concepts as perception, ideation, association of ideas, consciousness, attention, will, etc., but even goes so far as to claim that these concepts are useless for purposes of psychology, is of course quite a source of wonder. The indispensability of the concepts avoided by Behaviorist psychology and of the use of introspection will be apparent, we believe, from the discussion of but one 'assumption' which it makes. We shall confine this article to the discussion of this assumption.

The hypothesis referred to is 'the point of view that has been advocated throughout the text, namely, that thought is the action of language mechanisms' (p. 316). The meaning of the expression, language mechanisms, is carefully defined by the author as referring to any of those muscles of the body which actuate to produce words whether spoken, written, or gesticulated (as by deaf mutes). The meaning of the word, thought, as used in this hypothesis is not explicitly stated, but may be inferred with confidence from various passages which we shall quote[1] and is here taken to be the same as the meaning of thought when used by those who are not Behaviorists.

To be sure, the author states in the preface that "the terms thinking and memory have been carefully redefined in conformity with Behaviorist psychology." On page 14 we find in italics the expression: '"thinking," by which we mean subvocal talking.' This may constitute the re-definition, but if so it obviously begs the question which we are discussing; namely whether 'thinking' as ordinarily understood does consist of subvocal talking. We shall therefore leave this re-definition out of account.

It will be realized that the adjustment of an individual to his environment may involve acts requiring mental activity of all degrees of consciousness, from the most automatic habitual or instinctive acts requiring little or no consciousness, such as moving the eyes toward an object it is desired to see, to the solving of problems requiring the

[1] "A man may sit motionless at his desk with pen in hand and paper before him. In popular parlance we may say he is idle or 'thinking,' but our *assumption* is that his muscles are really as active and possibly more active than if he were playing tennis. But what muscles? Those muscles which have been trained to act when he is in such a situation, his laryngeal, tongue, and speech muscles generally" (p. 15).

"We manipulate vocally" (when trying to think of the name of a familiar person) "by running over the names beginning with each succeeding letter of the alphabet, or by saying 'black hair,' 'blue eyes,' 'six feet tall,' and the like" (p. 305).

"The explicit and implicit language habits are formed along with the explicit bodily habits and are bound up with them and become a part of every total unitary action system that the human organism forms. . . . They are present in the simplest types of adjustment that he makes. We can see the functioning of language habits only slightly in certain activities, as, for example, in swimming, tapping on the table with a pencil, while in certain other types they form an integral part . . ." (p. 309).

"Our view is that overt language develops under social training. It is thus absorbed into and becomes a part of every total integration of the individual. Hence when he is making adjustments in the absence of other like beings language remains as part of the process" (p. 323).

". . . the maiden thinks of her lover in words . . . . the beautiful thoughts of the idealist for mankind as a whole or of the mother for her child . . . are couched in words or their equivalent" (p. 325).

most concentrated mental effort. It is hardly conceivable that the Behaviorist would claim that *all* such adjustment involves language mechanisms, as the passages quoted would imply if taken literally. To simplify matters, however, we will limit our discussion to that type of adjustment ordinarily referred to as 'thinking,' namely, those mental processes of the problem-solving sort which require some degree of conscious mental effort, since these are open to introspective investigation. That even these processes of adjustment do not necessarily require language we shall attempt to show by appeal to logic and common experience, omitting arguments *ad hominem.*

2. *Examples of Thinking.*—Let us consider one or two simple cases of problem solving and subject them to critical psychological analysis in order to determine whether they involve language.

Suppose I have unfolded a new map and am attempting to fold it again as it was. I have no complete habit, not having folded a map exactly like this before. Let us see what happens. Surely there is a better way than to let someone watch me and report his inferences. He would merely see me look at the map and, let us say, try to fold it in one way but fail and then try another way and succeed. He might infer that my method was the so-called 'trial and error' or 'perseverance' method. Or if my lips have moved he might infer that I arrived at the solution of the problem by means of the action of the muscles of my lips and other speech organs. This appears to be the method of the Behaviorist.

Let me introspect and report from direct observation what happened from the point of view of one looking on from the inside. The writer does not wish to be misunderstood as assuming that introspection is infallible. One's testimony is not infallible even when he observes with his own eyes an incident which transpires directly before him. Relatively speaking however, introspection is far more direct and reliable than inference based upon observation from the outside.

On introspection I report as follows regarding my action with the map. More or less mechanically, as we say, that is, while thinking about what I had seen on the map, I began to fold the map along one of the creases. After a moment I became aware that the map was not falling into its accustomed folds. I then became aware of the need of finding the correct way to fold the map and I unfolded it in order to begin again. I recalled from previous experience that the crease on which the first fold must be made is one which runs entirely across the paper. I therefore looked for such a fold and on finding it folded the paper on it and repeated the process until the map was entirely folded up, making no further error.

Now this adjustment which I have made to the problem of folding the map was 'thinking,' alike in the popular usage and that of the psychologist. The Behaviorist claims that thinking is the action of language mechanisms. Let us go over this example of thinking again and examine it very minutely to see whether there is any necessary connection between language and the solving of the problem.

First of all, how do I become aware that the map is not falling into its accustomed folds? If I go slightly back of this awareness I note a feeling of contradiction between a subconsciously expected feeling of flatness and the experienced feeling of bulginess. This contradiction, we may say, caused me to become aware of the improper folding of the map—caused the shift of my attention from the thoughts of what I had seen on the map to the matter of folding the map.[1] How did I then become aware of the need of finding the correct way of folding the map? The experience suggests no other explanation than merely to say that the idea of contradiction 'called up' or 'suggested' the idea of need. This idea in turn called up the idea of beginning again. We may explain this process by saying that it was probably

[1] That a subconscious awareness of contradiction may give rise to an idea of need, together with an affective state which effects a shift of attention (clear awareness) to the need, is a matter of so frequent observation in structural psychology as to be considered a scientific fact. Such a fact, however, is of course quite unthought of in Behaviorist psychology, being wholly outside of its scope.

the result of a previously formed habit. One has learned in such cases that it is best to begin again. When the idea of unfolding the map again has come to occupy more or less of the whole of consciousness, 'the thought takes form in action.' Behaviorist psychology concedes such a phenomenon, so we need not attempt to explain it. Having unfolded the map I recalled previous experiences regarding the folding of large sheets of paper. We will say that the perception of the paper before me and the idea of need of folding, together served to bring forth from my memory store those ideas which came to my mind. These together with the perceptions of the map during the process of folding served to educe that train of ideas which guides the folding to a successful termination.

Now what is the material of all this mental activity? What do these ideas consist of? They consist of images, visual, tactual, kinæsthetic, etc., of maps, and of certain aspects of these images such as creases, folding movements, flatness, bulginess, etc. They have nothing to do with language, necessarily. The idea of flatness is tactual or visual or both, the idea of a folding movement may be visual or kinæsthetic or both. The idea of the length or direction of a crease is visual or kinæsthetic or both. Possibly other types of imagery enter to a slight extent. But no language need be involved.

Let us now consider another type of thinking. Let the reader ask himself why it is more difficult to play a game of chess blindfolded than with the chess board visible before him. Obviously the answer is that the perception of the relative positions of the chess men is a great aid to the mental manipulation which constitutes the basis of the study of moves. Moreover, anyone who has played chess or checkers will immediately appreciate the aid that would be derived from actually making the trial moves that are contemplated, in more clearly appreciating the relations that such moves would introduce. If the thinking were done by means of subvocal language it would seem that seeing or not seeing the chess board would make no difference. The obvious answer is that the thinking is done by means of the perceptions of the board and men as they are, the mental imaging of the movement of the men into new positions and the appreciation of the spacial and temporal relations between the pieces and their possible moves as introduced after the mental manipulation. No language whatever is required. As we shall show, a person may indeed talk to himself while contemplating moves, but this activity is entirely secondary and supplementary.

3. *The Material of Thought.*—Thinking, as an adjustment of the individual to his environment, as the solving of problems, consists of the evolving of new ideas, concepts, or meanings, from old. This is accomplished by recombination of the elements of the old into new patterns. By ideas, concepts, and meanings are meant image patterns, whether they be of words, objects seen, sounds heard, things felt, tasted, sensed in any manner whatsoever, or any quality, attribute, or aspect of such image patterns as may be conceived separately by abstraction, such as shape, color, surface, volume, extent, duration, intensity, symmetry, movement, similarity, difference, causality, symbolism, abstractness or affective quality; or of whatever degree of clearness or attenuation or incipiency the images or image aspects may be. We may think, therefore; that is, we may evolve new ideas, concepts, meanings, in terms of image patterns of any kind whatsoever, or of the consciousness (idea) of any relationship whatsoever between these image patterns.

For example, I am thinking when I am effortfully engaged in composing a piece of music. I sit at the piano with music paper at my side. My mind is occupied with perceptions and images of tones, tone combinations, tone sequences, tone relationships, tone emotional effects, tone symbols (dots on paper) the making of these symbols, etc.

My effort consists in the maintenance of my attention to the work, the calling up of sequences of tone images,[1] the

[1] Strictly speaking I adopt the mind set that will result in the calling up of tone images, or that is calculated to do so. (Sometimes I may succeed better than at other times.) We cannot call up an image necessarily at will. Generally it is a case of taking a certain mental attitude ordinarily called 'trying to think' which usually results in the recall of the idea desired.

comparison of these, the appraisal of their respective æsthetic values, the choice of one or another, the calling up of the proper symbols of notation in which to write down the musical ideas, and the writing of these. No language is involved in any of this thinking (except perhaps a final translation of the results of thought into symbols). In this case also I may compose without the piano; but this is more difficult, since I am compelled to make my judgments upon images only, whereas with the piano I may employ the perceptions of the tones themselves in my judgments. If my musical thinking were all done by means of the action of speech muscles we do not see that it would make any difference whether the piano were struck or not.

Similarly, one is thinking when he is creating a new architectural design, or a drawing or painting or statue or stage setting, or conceiving of a new dance movement or inventing a new mechanical contrivance or playing tennis or searching for the cause of engine trouble. The material of one's thoughts in all these cases is in the form of images, which need be only visual, auditory, tactual, kinæsthetic, may be, in fact, of any kind whatever according to the requirements or to one's ability to call forth such images. As we have said, one may do any amount of talking to oneself while thinking—which is merely putting one's thoughts in words after they are thought—but the talking is not the thinking. It is supplementary to it in exactly the same way that describing a landscape is supplementary to seeing it.

Thinking may be called the controlled association of ideas, in contradistinction to the free association of ideas. In the free association of ideas, by which we refer to what is ordinarily called day dreaming or revery, ideas follow one another in a more or less unguided manner, yet in a fairly rational way as compared with the incongruous manner of idea sequence sometimes experienced in dreams. Doubtless there is some sort of control even in 'free association' though it may be the general interest in the subject of thought or the control occasioned by thought habits. However, in what we have called controlled association of ideas characterizing thinking, the ideas are guided in their sequence by some conscious aim, *e.g.*, a problem to be solved. Irrelevant ideas are discarded (attended from), relevant ideas are attended to. That which does the controlling is often also in the form of a definite idea. This is best illustrated when one is given two digits written one above the other: if told to add them, under the influence of this guiding idea they call forth their sum; if told to subtract one from the other, under the control of this guiding idea they suggest their difference. The same stimuli give rise to either one or another idea according to the nature of an additional and controlling idea.

Similarly, we may have occasion to think of the *opposite* of a given concept, or of a subordinate, or the superordinate or the symbol of a given concept. In any case one idea calls up a second under the guidance of a third.

We may not only occupy our minds with ideas of the color, size, shape, etc., of objects, as referred to above, but we may *compare* two objects as to color, size, shape, weight, motion, acceleration, symmetry, etc., and *judge* which is the best suited to our needs. Of two individuals we may compare the good looks, cordiality, sincerity, hospitality, integrity, adaptability, intelligence, etc., as conceived in ideas of conduct, feelings, appearance, facial expression, and of the many circumstances under which the impressions were gained. All these mental activities dealing with ideas as material—their association, recall, generalization, abstraction, comparison, judgment, etc., are elements in adaptation, yet they may be experienced or accomplished quite independently of words. The idea of a color is not a word. The idea of one color being more intense than another need not have anything to do with language. The choice of this or that color for an æsthetic purpose does not require language, nor does the act which the choice calls forth. Yet all this is adaptation.

4. *Words may be the Material of Thought.*—As has been suggested throughout the discussion, words may be the material of thought. The place of words in the range of material of thought may be stated as follows. The material

of thought, as explained below, begins with perceptions; then come images resembling perceptions, then more and more attenuated images or aspects of images singled out by abstraction, and finally symbols. By symbol is meant any concept which is used in place of another. The best illustration of thinking in symbols is in the case of the number symbol system used in arithmetic and algebra. The idea of eight (not in the word but the number: ********) is represented by the symbol: 8. The idea of seven (seven things: *******) is symbolized by the figure: 7. Now if we have the problem of finding the sum of these numbers (******** and *******) we may do so by translating them into their respective symbols and give our attention to the symbols only. Having previously formed an association between the symbols, 7 and 8, and the symbol of their sum: 15, the symbol 15 is called up when the symbols, 7 and 8, and the guiding idea of summation are in mind. We may then proceed to make other arithmetical computations in terms of number symbols only, letting these call up the number idea (****—) when needed. Similarly in algebra we may let x represent one number with which the problem deals and let y represent another number, etc., and then by means of habits established in connection with these symbols we may do thinking of a simple type in lieu of what would be far more difficult if done with the original concepts of number. This type of thinking is exemplified in the following algebraic reasoning:

If $x^2 - y^2 = z$ then $(x - y)(x + y) = z$.

There are of course many kinds of symbols. In addition to the number symbols just mentioned there are the symbols of operation upon numbers such as those of addition, multiplication, integration, involution, etc., there are the symbols of musical notation, symbols of punctuation (?, !, ", *, -), symbols on maps representing roads, trees, buildings, bridges, tunnels, etc. (an engineer can think very effectively in these symbols). There are even symbolic facial expressions used by actors to portray emotions which off the stage would not be expected to produce those expressions. A skull and cross bones symbolizes danger. The flag symbolizes country, etc. Last, and most important, of course, words and sentences symbolize thought of every description. Occasionally we feel that we have experienced some thought or sensation or feeling which cannot be expressed in words. But in general all ordinary thoughts and feelings can be represented by some word or sentence.

We see therefore that language constitutes only one of the various kinds of symbolization, and symbols constitute only one type of material of thought.

Words are themselves the material of thought under many circumstances. Whenever we have to communicate thoughts to another or learn the thoughts of another through language we have to deal with words. By far the greatest use of language of course is in the calling up of language symbols to represent meanings or the calling up of meanings represented by language. Occasionally however we may think in terms of language almost exclusively, as when dealing with the rhyme and rhythm of poetry. In the case of syllogistic reasoning we may be truly said to be thinking in words, when the expression "All *A* is *B* and all *B* is *C*" calls up the language idea: "All *A* is *C*," or when part of a sentence suggests the rest as "All is not gold that ——."

5. *Language the Symbolization of Meaning.*—We have attempted throughout this discussion to distinguish clearly between a *meaning* and the language by which it is symbolized. We cite the following illustrations to bring out this distinction still more clearly.

If one says: "I saw John Jones on the street this morning" the hearer will get the meaning of the sentence at once. "Getting the meaning" means to the ordinary person getting an image, more or less faint perhaps, of the speaker looking at Jones on the street. But let us take another sentence. Here is one in which the meaning of a new (coined) word is stated. Every word in the statement of the definition except the new one is perfectly intelligible and familiar and the statement is a perfectly logical and meaningful one, yet we are confident that the reader will not get the meaning from the language on first reading. This is the sentence: "Let us define the word, incration, as meaning the increase in the number of feet per second per second by which the motion of a body is accelerated." Anyone who has gotten the meaning of this sentence clearly should be able to point out immediately the error in the following statement, which if correct would follow as a corollary to the above definition: "The unit of incration is one foot per second per second." (The correction is indicated in a footnote.) If the reader is unable to point out the error it is merely because he has not gotten the meaning of the definition, which is something quite apart from the words by which it is symbolized and consists of images either of the motion of a body or of the path of its motion. Without such images we are confident the meaning can in no way be appreciated.[1]

As has been said, the utterance of sentences or of parts of sentences or of analogous statements often *helps to bring out* the meaning, that is, helps to call up the imagery necessary to build up the meaning, or helps to fix the meaning in mind by symbolizing it after it is appreciated. But the meaning may exist entirely independent and apart from any utterance, either overt or implicit.

One often hears the expression from pupils in school: "I know but I can't tell." This is generally a simple case of having a meaning or idea without the ability to symbolize it in language.

Moreover, as has been stated, even an adult may have experienced perceptions, ideas, or feelings which he will declare cannot be expressed in words. Even if they could,

[1] The correct statement is: The unit of incration is one foot per second per second per second.

A reasonable comprehension of the meanings of these statements may be built up with the help of the following leading statements. The *rate* of motion of a body is the number of feet per second which it moves. The unit of rate is one foot per second. The *acceleration* of a moving body is the increase in its rate, that is, the increase in the number of feet per second which it moves in succeeding seconds. The unit of acceleration is one foot per second per second, that is, one foot per second every second. And again, the *incration* of a moving body is the increase in its acceleration, that is, the increase (from second to second) in the number of feet per second by which its rate is increased, or in other words, it is the number of feet per second per second by which the motion of the body is accelerated. The unit of incration is the unit of acceleration every second, that is, it is one foot per second per second per second.

his inability to do so testifies to the independence of the thoughts from the language by which it would be symbolized. We say of our memory of a sunset that it was 'indescribable' and 'would have to be seen to be appreciated,' which is entirely true. We say, 'Words fail me' regarding the expression of our thoughts of past emotional experiences. The emotional experiences of love, hate, fear, anger, etc., may be the material of thought just as well as the experience of perceiving the color blue. Even our best attempts to express our thoughts of experiences in words often fail to carry full meaning to the hearer unless he has had a similar experience. Thus, one may say that an experience was like that of a sudden drop in an elevator or like flying in an aëroplane, but unless the hearer has had an analogous sensation or experience the expression is devoid of essential meaning to him. What can the expression, 'like being struck by lightning' or 'like finding oneself caught under the ice' mean to one who has not had the experience in comparison to what it means to one who has had the experience! Yet the language is identical in the two cases. One and the same expression from the lips of an oratorical person may convey a meaning to one hearer which will call forth tears, a meaning to another hearer which will call forth anger, and a meaning to still another hearer which will call forth laughter. Such a phenomenon would be of course entirely impossible if there were but one meaning to the expression, a meaning inherent in the language itself. It is a platitude that the meaning of language is something which is brought to it from the experience of the hearer, that it does not reside in the language.

6. *The Genesis of Language.*—Not only may we have thoughts for which we cannot think of the existing appropriate language, but we may often have an idea for which there is no corresponding word or phrase. Indeed language is built up by the coining of new words and phrases that are needed to symbolize new thoughts for which no corresponding language exists. The word, automobile, for example, did not come into existence until after there had been made,

or at least conceived, a machine which would move itself, and which needed a name. Similarly the expression, 'carry on,' came into use in response to the need of a name for an action which was well comprehended but for which no convenient language equivalent existed. Ideas originate first; afterward they are named—symbolized in language.

Perhaps the clearest example of the temporal relation between the genesis of ideas and their symbolization is the case of the naming of persons. First the child, which we perceive, is born. Afterward it is given a name. Why do we give a child a name? It is, of course, for the reason that to refer to it always by description would be cumbersome and inaccurate. When we think of a person deliberately we think of his form and features, his speech, manner, expression, etc. When we think of him more fleetingly, however, our imagery becomes attenuated, even perhaps to the extent exemplified by the representation of Roosevelt by merely a pair of glasses and a row of teeth. But for the purpose of one person conveying the thought of an individual to another, such attenuated imagery is inexpedient. We therefore symbolize the whole picture or idea of the individual by a single word (a name). The name then, in cases of rapid thinking, may nearly take the place of the concept of the individual as imaged. The name, however, still does carry with it something of the original imagery. The idea of 'James' to one person carries with it something which characterizes his brother, James. The idea, James, to another person, carries with it something which characterizes his uncle, James, a different individual. This additional something is needed to constitute the difference in meaning between 'James' for the one person and 'James' for the other. To a third person who knows no one by the name of James, the idea is merely a word, known to refer to some individual.

On the other hand the perception of a person whose name is not known does not carry with it any idea of a name, nor need any word come to the mind. When I see a man I do not think 'man.' When I think of a crowd of persons I do not think of a crowd of words! Language is as distinct from the ideas it represents as the name of a person is distinct from the perception of the person himself.

An infant of course does not use language habits until a year or so after birth. Yet an infant can think—perceive, compare, judge, choose, decide, act upon decision, etc.—before language habits begin. The material of his thoughts is the sights, sounds, smell, tastes, feelings, etc., which he experiences throughout his waking state.

To illustrate the genesis of language habits we must go back to the early days of a child's life when it is just beginning those 'Abbreviated and short-circuited actions (which) become a necessity if it is to hold its own in that environment and make progress' (p. 319). The child's perception of its doll, its desire for the doll, its idea of searching for the doll when not in sight, its idea of creeping toward the doll when seen, its idea of reaching for the doll when within reach, its idea of grasping and the new ideas which arise from the manipulation of the doll—assuming these ideas sufficiently well fixed by habit that the actions have reached an 'abbreviated and short-circuited' stage and are purposeful and adaptive—these ideas constitute the beginnings of thought (conscious adjustment to the environment).

The stimulus, 'tata' (p. 320), cannot call up the concept, doll, before the concept, doll, is in existence. Nor can it create the idea. The child must have some idea of the doll, formed from perceptions of the doll itself, *before* the idea of 'tata' can be associated with it. Language in general bears just the same relation to thought in general that the idea, 'tata,' as a word, bears to the idea of the doll, as something seen, touched, etc.

Similarly the idea of number is formed before any symbol representing number can be associated with it. We may talk about number in the hearing of a child for months after it has begun the use of language, but until the child has by observation and comparison become conscious of the two-ness of its hands, of the two-ness of its feet, and of the two-ness of many other separate things, so that the idea of two-ness as an abstraction becomes a separate idea in the child's mind—

until this time the sound of the word 'two' is meaningless to the child, and only attains meaning when finally associated with this abstract idea of the two-ness of any two things. The meaning comes first; afterward the language symbol (word) is associated with the meaning and may be substituted for it when occasion demands. One has but to attempt to teach a child, who is just learning to talk, to count and deal with number concepts to see how absolutely meaningless the words are to the child until he has had opportunity in the course of his daily experiences to make the abstractions necessary to form these ideas. We may teach a three-year child to pronounce perfectly the sentence: "The square root of twenty-five is five," and if the action of language mechanisms constituted thought we should expect the child to understand perfectly the meaning of what he had said! Further comment seems unnecessary.

It will be noted that in this discussion it has been necessary to use concepts which are not found in Behaviorist psychology. These are the concepts of 'meaning,' 'idea,' 'concept,' 'conscious,' 'purposeful,' 'association of ideas,' 'abstraction,' 'symbolization,' etc. Yet these are fundamental to structural psychology and from the above discussion we deem it apparent that a consideration of the acquisition of language habits, their function in thinking, and the material of truly non-language thought, is totally inadequate without these concepts. To be sure we find passages in a Behaviorist psychology attempting to state what goes on in the mind of an individual. Thus (p. 305): "We manipulate vocally" (in attempting to recall the name of a familiar person) "by running over the names beginning with each succeeding letter of the alphabet, or by saying 'black hair,' 'blue eyes,' 'six feet tall,' and the like." This seems to the writer to be one of several excursions quite outside the realm of Behaviorist psychology. He does not know whence these ideas came but judges that it was by some sort of inference, partly because the Behaviorist does not use introspection and partly because he is unable to corroborate them by introspection.

7. *The Inadequacy of the Behaviorist Conception of Thought.*—To illustrate what is believed to be the wholly inadequate conception of thought as entertained by the Behaviorist, we cite the soliloquy postulated on page 332. "The implicit word processes (aroused by whatever previous stimulus) 'it's a fine day, I think I will go to the races; it's twelve o'clock now, I have just time to catch the train,' serve to start you to get your hat and field glasses. Some unfinished work meets your eye or other conflicting word processes are aroused, as 'but I have to write those letters and I have a luncheon engagement with X.' These tend to drive the organism as a whole into some other form of action; for a time there is a conflict (inhibition). Finally when the conflict is over the final word act issues, 'Well, I guess I'll have to give up the races and write those letters and keep my engagement with X.' Here we see implicit word processes tending to arouse overt acts and actually arousing the initial steps. But since the human individual is a completely integrated affair, associated word processes arise which may drive the organism into a totally different form of activity from that which was first initiated."

The writer contends that the 'previous stimulus' together with the mental activity which called forth this soliloquy would be sufficient to start one to get his hat and field glasses, without the accompaniment of any action of language mechanisms, and that such action itself would not suffice. The reasoning is as follows. Let us suppose the previous stimulus to be the perception of the green grass and sunshine and warmth of the outdoors. This perception called forth by association the memory of previous days when races were attended and of the accompanying pleasure. These memories contained the urge to renew the pleasures. They gave rise to the decision which is expressed in the language: "It's a fine day, I think I will go to the races." The decision made, the thought took form in vocal expression. At this point either the clock struck twelve, this serving as a stimulus, or the idea of going to the races naturally called up the idea of when to go, which in turn suggested the idea of looking at the clock, resulting in the perception that it was just twelve o'clock. None of this mental activity required language. The perception of the time of day having been made in one way or another, the idea called up the words which would express it and the individual added, 'It's twelve o'clock now.' What happened next? Presumably at this point came the idea of going to the races by train, followed immediately by the idea which if expressed in words would be, 'When does the train go?' which in turn called forth the memory that the train goes (let us say) at twelve-fifteen. Immediately there came to the mind the idea of the preparation which is necessary to catch the train and a judgment is made as to how long this will take, based upon past experience. The individual must also go through a certain mental operation of determining how much time there is available before train time and make a comparison between these lengths of time in order to make the decision which when expressed in words is, 'I have just time to catch the train.' This idea possibly suggests the idea of haste which together with the idea of going to the races calls forth ideas of the appropriate preparation, getting the hat and field glasses, etc. These latter ideas take form in action.

In view of the obvious necessity for the mental activity of perception, judgment, decision, etc., intervening between the advents of the ideas which took form in the language quoted, we submit that, as stated above, it is impossible that the soliloquy postulated could of itself have given rise to the getting of the hat and field glasses. Moreover, the ideas themselves which suggested the soliloquy could have given rise to the acts and there need have been no language, explicit or implicit, involved whatever. Thus, the ideas of time may have been conceived in visual imagery—the imagery of the face of the clock and the movement or path of the minute hand. No language is required. The ideas of preparation for the train would consist of memory of the acts of getting hat and field glasses, walking or riding to the station, buying the ticket, etc., these consisting chiefly of visual and kinæsthetic images. No language is necessary.

The ideas involved in the judgment of distance (to the station) or of time required for preparation and traversing the distances, etc., would be kinæsthetic or visual or other ideas of space and motion, the comparison of these ideas of space and motion, etc., resulting in ideas of the relations between them. No language is required. Moreover, the ideas which take form in the acts of getting the hat and field glasses are visual, tactual, and kinæsthetic and are quite independent of language. The fact is, one could conceivably note the weather, decide to go to the races, make preparations, board the train, hand the conductor a ticket, note the progress of the train, get off at the race track, pay the entrance fee, and watch the races, all with mental activity and acts in no way involving language. Any amount of soliloquy or conversation may accompany the expedition, but this is wholly incidental, secondary, and unessential.

8. *Introspection.*—We believe that Behaviorist psychology is entirely sound *within its own sphere*, that is, so long as it confines its study to the behavior of the individual as seen from without. A psychology so limited, will, of course, necessarily leave untouched a vast field of useful knowledge which can in time be made scientific where not already so, after extensive investigation, comparison of findings, determination of general tendencies, and the careful observation of everyday experiences. But should one desire to explore the realms of psychology outside the scope of Behaviorism, he must then supplement his external observation by as thoroughgoing, extensive, and careful an examination of that which takes place within the mind—as seen from within—as is possible by highly practiced and trained introspection.

To direct the attention to the color of an object is a very easy matter. To direct the attention to the difference in shade between two colors may be slightly less easy but it is entirely possible. To direct the attention to the idea of the æsthetic value of the colors requires perhaps appreciably more practice, but it is none the less possible. However, to direct the attention to the nature of the mental process of choosing between two colors, and to the manner in which the choice gives rise to appropriate acts, may be quite difficult, not to say impossible, for the inexperienced person. Yet these phenomena are available for observation no less truly than the habit of typewriting is open to acquisition or the length of a rail is capable of being measured to the thousandth of an inch. These accomplishments require long practice or minute observation, but we do not say they are impossible. An unpracticed person cannot direct his attention to the less tangible aspects of thought any more than he can play a theme on the piano. Because it is difficult however, one does not forego the learning of piano playing, if he desires to learn to play. Again, it is possible that no two observers might obtain the same measurement of a rail to the thousandth of an inch. Nevertheless we do not say that measurement is of no use in physics. Observers can nearly all agree on the length of a rail to the tenth of an inch, and on the length of a needle to the hundredth of an inch not to say to the thousandth.

Similarly in psychology, those inexperienced in introspection may not be able to distinguish between middle C on the piano and the C an octave above, or to observe that they see objects double which are not focused upon. Moreover, persons highly trained in introspection may not always be able to distinguish between the perception of a very faint sound (as the distant ticking of a watch) from the auditory image of the sound (imagined sound) nor to state just what constitutes the mental element of difference between the emotions of fear and anger. But there should be little difference of opinion between persons of extended experience in introspection as to whether the material of our thoughts, when we create new ideas, and conceive new modes of activity in the fields of music, art, drama, mechanics, etc., is in the form of language or in the form of tones, visual pictures, etc.

9. *Summary.*—There may be no experimental proof whether or not thinking—conscious adjustment to the environment—is invariably accompanied by the actuation of some language mechanism as the larynx, lips, fingers, etc., in the incipient production of some form of language, spoken

or written, but the evidence would seem to favor the belief that no such invariable accompaniment is necessary. One uses the eyes in observing objects attended to almost throughout the waking state. It would seem more plausible to assert therefore that some form of "implicit" *eye* movement is a necessary accompaniment of all thinking. Much evidence such as that from the observation of a chess player studying his moves could be brought forth in support of this view. All this, however, is quite beside the point. The claim is made by the Behaviorist that "thought *is* the action of language mechanisms" (italics mine). Certainly the evidence against such an assertion is overwhelming.

Man is an organism highly adapted physiologically to his environment, provided with sense organs of sight, hearing, taste, smell, touch, pain, heat, cold, muscle movement, body position, etc. Each of these sense organs is capable of giving rise to sensations which take the form, in the mind, of images or image patterns. (The word image is used in a very broad sense as shown below.) The organism has at its disposal any or all of these incoming percepts or stored images or image patterns as material for thought, for working over into new combinations, new thoughts, which will give rise to new actions, new adaptations to the environment. In the event of the bringing together of two or more concepts—images—or of the dividing of one concept into two or more (as when a child first separates from the concept ball the concept roundness)—in the event of this working over of concepts, they are necessarily abbreviated, composited, exemplified, attenuated, or substituted for by others. If the substituted concepts are of a kind remote from the kind for which they are substituted but are more or less definite and commonly understood, we call them symbols. A careful description of the manner in which thought material is abbreviated, composited, attenuated (even to a point which is considered by some psychologists to be 'imageless'), etc., is of course quite impossible within the limits of this article.

However, the mental activity which brings forth a new act may be the result of the combination or division or other working over of any type of mental material—the bare sensation, the fresh vivid full percept, the fairly vivid memory image, or the image or image pattern when abbreviated, or attenuated, or composited, or exemplified, or in any manner generalized or particularized, or finally in the form of symbols. And language, as has been shown, is but one general type of symbol system.

In conclusion, then, let it be said that we *may* think in words, and when we do, the thinking may be accompanied by the action of language mechanisms. But thought—even conscious mental adjustment—is not restricted to the material of language any more than it is restricted to the material of musical tones or of architectural designs or of facial expressions, nor is it restricted to the action of language mechanisms any more than it is to the mechanism of hearing or of sight or of locomotion.

A BEHAVIORISTIC ACCOUNT OF SLEEP

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The phenomenon of sleep does not lend itself conveniently to explanation in terms of sensation, image, and feeling. Accordingly structural psychology can offer little by way of a description of the state of sleep. The behaviorist, on the other hand, believing that his definition of consciousness offers a statement not only of what consciousness is, but of what it is not, is in a position to explain sleep. If, as behaviorism asserts, mind is a matter of reflex connections always involving the movement, tension, or tonicity of muscles and always correlated with the activity of glands, then consciousness, in its varying degrees of clearness, is a matter of degrees of complexity and ordination among systems of muscular action. A high degree of consciousness thus becomes synonymous with an intricate and ordered complexity of tonicity in muscular systems; while a low degree of consciousness is equivalent to a complexity of low degree and an ordering of simple texture. Consciousness thus may be, speaking in a paradox, scattered, involving perchance abundant activity—by way of muscular tonus in various muscle systems—but activity of a low degree of intricacy in organization.

If consciousness, then, be a matter of the degree of complexity of interacting muscle systems, non-consciousness is a lack of activity or else a lack of this complexity. In either case the factor of complexity is vital and needs describing. Behaviorism's explanation of this concept is based on the continuative function of the sense endings within the muscles. Always stimulated by any muscular event, they afford the means for causing a single inaugurating stimulation to reverberate through a long series of tensions, or else to provide a continued hardening of some one set of muscles. The type of tensions running in a series is what is called the chain reflex; the continued is that called circular. Chain reflexes have much to do with that complexity of interacting muscle systems that makes consciousness. Carried on by means of proprioceptor organs of stimulation, they have a right of way and a special kind of clearance. This insures them continuity and lends a measurable degree of stability to the complexity of structure that makes up consciousness.

This description recognizes, first, that the muscularity of the body occurs in fairly well-defined systems: as those of the back, the legs, the head, the face, and the throat; and, secondly, that these muscle systems are set off one by the other and in a certain order. This order is based on priority. Priority of muscle systems is, in general, a matter of precedence in the development of working efficiency in reflex arcs. Those systems that have an early development history come to have a pronounced control over systems developed later, in that the systems developed later get their initial determinations from the workings of the habitual responses of the earlier. Their most intense determinations are, both earlier and later, then, conditioned by the determinations of the systems already habituated. Their capacity for quick and valuable response depends thus very largely upon their close coördination and coöperation with habitual reactions of systems determined at an earlier stage of development.

An understanding of sleep requires a description of this development order, a description of gross muscle systems and a statement of their superordination and subordination. This is found in an account of the operation of Pawlow's Law, as manifested in the conditioned, chain, and circular reflexes.

By Pawlow's Law a reflex arc may be a factor in new activities by the stimulation of sense endings imbedded in the muscles that are contracted by the operation of such an arc. Every motor process stirs a muscle; this stirring starts new impulses; and these impulses seek a new outlet. Two directions they can take: they can go around back by way of the motor nerve that stimulates this muscle, and so stimulate it again; or they can take some other motor nerve leading to a quite different muscle. The former of these processes is the circular reflex, the latter, when carried on through a series, the chain reflex. By means of circular and chain reflexes in various combinations the organism has within itself the machinery for carrying on activity, for a while, at least, without the intervention of peripheral stimulations. Only in this way can the continued activity of involuntary organic acts be accounted for, activities like heart-beat and breathing. In this way also is given a satisfactory account of the mechanism of such activities as catatonia, catalepsy, emotional complexes, fixed ideation—in fact, repetitive and continuative actions of all kinds.

To get the full significance of these circular and chain reflex systems in the coördination and superordination of actions, it is necessary to envisage them in connection with the development order of muscular systems. Systems developed early in the life of the organism are obviously determined strongly; in the case of the very earliest, heart-beat and breathing, no stimulus short of that adequate to stop life can divert them. Moreover, their continued activity is but little dependent upon other stimulation than that provided by the circular reflexes that keep them regular; at least so long as the muscles concerned receive nourishment from the blood. Systems developed one stage later, like those involving walking, reaching, turning the head, have within themselves much of the same continuative mechanism. Their ability to function, however, they have gained largely as an adjustment from the successful functioning of systems developed earlier. At the start of their functioning they do not possess a full measure of self-determination; they are of necessity dependencies, subject to the caprice of superiors holding power by a rule of seniority. Certain things they can do so long as the older systems go about their business in an orderly fashion; certain other things, under the same conditions, they are not privileged to attempt. Thus the use of the eyes, the hands, the legs, is conditioned very materially by the regularity of the beating of the heart and of breathing. Let, once, something go wrong with either of these older activities, and the organism loses complete control, eventually, of hands, legs, eyes, and of all other muscle systems. Under similar circumstances any determinations that are ordinarily well established give way to a recrudescence of the wildest of random movement.

Thus the muscle systems operate in a kind of hierarchy, with jurisdictions fairly distinct, though not exclusive. Most firmly enthroned of all are the primary reflex systems controlling heart-beat, flow of blood, operation of vital organs, and breathing. Next come those developed in the organism's earlier days, use of arms and legs, back, torso, and neck muscles; later, and probably overlapping the earlier systems, muscles of the eyes, ears, face, and head; lastly—coincident with the development of speech—the muscles of jaws, lips, tongue, and throat. Thus consciousness as complexity of muscle systems is a pyramid with the organic systems at the base and the muscles of thinking, reasoning, and speech at the top. Or, changing the figure, it is a hierarchy with the organic systems as autocrats and the other systems holding office on a descending scale of self-government, dependent always upon the commands of the autocrats ruling by virtue of prior possession of power.

This hierarchy operates to provide the difference between sleep and waking consciousness. Without the tension of head and face systems there is not complexity enough for consciousness. So vital are they to clear cognition that they are easily confused with the totality of consciousness; remove them altogether from the systems active at any one time, and unconsciousness occurs. Yet they are not autonomous; when fatigued and free from intense peripheral stimulation they normally yield easily to the relaxing of the lower systems and go out of function along with them. Any condition in which they refuse to stop functioning when free from peripheral stimulation or when fatigued, and when the lower systems have relaxed, is looked upon as abnormal. In fact psychopathic conditions can be described either in terms of an actual lack of the upper systems, or in terms of their failure to coöperate with the activities of the lower. Sleep is accounted for in the formula: Remove the higher systems from activity, and consciousness departs altogether; weaken the lower, and consciousness is in a precarious condition, especially if the higher systems are affected by fatigue. When the lower systems are thrown out of function, the higher circular reflexes either stop at once or, in abnormal cases, ultimately wear themselves out; in either case consciousness breaks up. Remove the lower entirely, and death is instantaneous.

A prime requisite of easy and deep sleep is freedom from stimulation for the eye, ear, nose, tongue, and parts of the skin not constantly pressed by clothes; forms of stimulation that have little to do with the organic systems. Yet sleep is possible even in the face of such stimulations; but only in cases where great fatigue throws lower systems, like those of leg muscles, back, and neck, out of commission. The chief power of estopping other systems, especially under conditions when fatigue is present, is authoritatively appointed to the organic systems; because they get their determination at a time when the organism is in its most plastic state—in its early stages.

Thus sleep becomes behavioristically a matter of the efficient domination of the upper systems by the lower, operating through the relaxing power of fatigue; while wakefulness and insomnia always imply that the higher and later systems are assuming dominance over their precursors. Wakefulness, so, is characteristically the dominance of the lower systems by the upper when fatigue is not present. Accordingly when wakefulness exists at the same time that fatigue is present, the condition is abnormal.

This means that when the muscles of the back, legs, and neck are relaxed, a powerful stimulator is lost to the muscles of the arms, hands, feet, and head. When arms, hands, feet, and head muscles in turn are relaxed, there is lost a powerful source of stimulation to the muscles of the face, jaw, tongue, and throat. While systems are undoubtedly more finely differentiated than this, still their hierarchical interdependence is on just such an order—and sleep can be explained by gross characterizations as well as by those more minute. In the inducing of sleep much significance must be attached to the order in which muscle systems go out of function. When the organism follows the development sequence, sleep is easy; when the order of relaxation is in any way reversed, restlessness and wakefulness follow. In cases of complete reversal of the order, we get such states as hypnosis, temporary high degress of attention, manic conditions, and forms of insanity.

The beginning of sleep then normally is the relaxation of the muscles that hold the body erect. As soon as these muscles are relaxed, the prime determiner of higher systems is taken away, the proprio-ceptor foundation, and the higher systems then are kept in function by only a veritable bombardment from the outside world or from very strongly determined circular or chain reflex arcs within their own system. Among these latter are emotional states, fixed ideas, tunes running through the head, repeated attempts to solve a problem, rhythmical verbalizing, and thinking in circles.

The next step in normal sleep is the sequential relaxation of each of the systems hierarchically dependent upon the erect-holding systems. Finally through the sufficient dissolution of the complexity that makes consciousness, sleep comes. Consequently once a person lies down, relaxing the muscles of legs, back, and neck, the beginning is made of sleep. Providing there is no interference from outside stimulations—chiefly those acting upon the sense endings in the head—and also so long as there is no intense circular reflex process going on in the muscles of the jaw, lips, tongue, or throat,—'thoughts that will not go out of one's head'— such a beginning once made leads to complete sleep and loss of consciousness.

Certain easily-made empirical observations as to sleep confirm this account. (1) Sleep is characteristically accompanied by relaxation of muscles. (2) Characteristically also it takes place with the body in a horizontal position, a position that induces first of all a relaxation of the muscles of the legs, back, and neck, all of which must maintain a high degree of tonicity to maintain erect posture. (3) When the muscles of the back, legs, and neck are relaxed, the muscles of the head, face, jaw, and throat all tend to relax in a short time ensuing. (4) These muscles also are most easily relaxed when freed from stimulation of the head sense endings, in the dark and in silence and free from intense taste or smell. (5) All these relaxations occur parallel with a scattering of consciousness, a defocalizing of attention; and the greater the degree of relaxation, the less the ability to perform any act implying a high degree of concentration. (6) Organic activities, though, are kept up despite any relaxation of the voluntary muscle systems. (7) Again, deep sleep implies complete relaxation; also it connotes rest and recuperation from fatigue, a retoning of muscles for future work. (8) The degree of sleep involved conditions the number and vividness of dreams had; deep sleep implying few dreams and light; light sleep implying many dreams or dreams that are vivid. (9) Deep sleep also leaves little recollection of dreams, except for the moment when one is coming out of sleep to consciousness. (10) Great numbers of dreams, or dreams that are vivid, it is generally assumed, are equivalent to defective rest, and restlessness is always accompanied by the inability to stop thinking or by numerous and intense dreams. (11) During widespread muscular activity there is no such thing as sleep; as during walking, eating, reading, talking, giving active attention in any way.

These general observations, and many others of similar nature, point clearly to the close relation existing between sleep and the movement or tonicity of muscles.

In the subjective terms of ideation, sensation, and feeling it is difficult to explain what happens to conceptual thought during sleep. The behaviorist, by assuming that thought of all kinds and in all degrees is a matter of muscular tonus, precisely as in walking or standing erect or moving the hands or talking, can give an account of sleep that fits in with his whole program. Sleep to him is nothing but a disorganization of muscle systems which in waking consciousness are closely interrelated hierarchically, the action of each determined in part by the continued action of the others. When conditions are set for the relaxation of sundry systems of muscles, consciousness begins to be more scattered, system after system drops out of function, and ultimately, in the soundest sleep, nothing is left by way of muscular activity but the functioning of the organic systems.

Dreams are clearly the result of systems involving throat, face, tongue, and lip muscles which remain in function when other systems have been thrown out of gear, systems which, if combined, would make consciousness. The Freudian dream psychology presents agreement with obvious facts in that it recognizes the existence during sleep of mental processes which seem very like others that go on in waking consciousness, yet which at the same time are partly unlike them. The behavioristic explanation of this is that in so far as a dream is a matter of the activity of muscle systems involving a high degree of complexity and coördination, in so far it is similar to waking consciousness; and so has a way of seeming logically coherent. The illogical dream, on the other hand, the freakish dream, the dream that seems to forbid explanation and interpretation, can be explained broadly as a type of organization and coördination not met with in that form in waking life; so that, speaking generally, the more unusual the combination left operative during sleep, the more fantastic the dream. From these suppositions can also be found the reason why dream analysis cannot be a matter of accurate interpretation and why the Freudians who assume to interpret all dreams give promises in reality beyond powers of observation to fulfill. So entirely beyond inspection and prediction can be the permutations and combinations of the hierarchy of muscle systems, that they can defy all powers of analysis.

From these observations follow certain therapeutic inferences worthy of note, most of them current already through the experience of the race. If you would sleep soundly, exercise much, in particular the muscle systems of the body below the head; for if the 'body' is tired, the 'mind' will rest also. If restless in sleep, study how to relax, first of all the muscles of the legs, back, and neck. Then reduce the breathing rate: high tension almost invariably is accompanied by rapid breathing; low tension by slow-breathing. Also hands and feet, fingers and toes, must be inert. If thoughts crowd thick and fast and will not leave, let the jaw drop, make the muscles of the cheeks and lips flabby, avoid screwing up the muscles around the forehead and the eyes, see that the tongue lies limp in the mouth, and make certain that the muscles of the throat are not in any way tensed. These last-named muscles, together with those of the jaw, tongue, and lips, are more likely than any other to get in the way of sound sleep. Make sure to observe the right order of relaxation of systems; gross lower systems first, then the finer systems below the head, and finally the fine systems of the head. Sleep is synonymous with carrying out the following order in relaxation: Reduce the breathing rate; then relax legs, back, abdomen, and neck; then arms, hands, fingers, and toes; next the muscles around the eyes, forehead, scalp, and ears; and finally those around the mouth, jaws, tongue, and throat—the muscles of speech and conceptual thought.

THE COMPENSATORY FUNCTION OF MAKE-BELIEVE PLAY

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It is the purpose of this paper to present in outline a view of play which will usefully supplement those theories which are generally entertained. Play is, of course, a phenomenon of extreme complexity and, for that reason, its complete explanation should hardly be looked for in any single statement. The suggestions have been made that the play of children is a chronicle of race activities, that it gives practice or preparation in functions of coming importance, that it furnishes an outlet for surplus energy, and that it is a recreational agency or means of relief from fatigue induced by other occupations.[1] Undoubtedly all of these things are true to some extent, but more important than any of them is the fact that play is essentially a compensatory mechanism having the same origin and impetus as the day-dream or fantasy.[2]

A compensatory function is especially evident in that type of play which involves the element of conscious shamming or make-believe. While it is possible to demonstrate that other types of play may operate as compensation,[3] I shall confine the following discussion to play which is clearly make-believe in character.

The child is driven by many inherited and acquired impulses, some of which are adequately and easily expressed and some of which find no direct outlet. These latter create a situation demanding compensation, and this compensation

[1] For one of the latest discussions of the various theories of play see Reaney, M. J., 'The Psychology of the Organized Group Game,' *Brit. J. of Psychol.*, Monog. Supplement, 1916, IV.

[2] The compensatory nature of the day-dream or fantasy is clearly brought out by Freud and other writers of the psychoanalytic school.

[3] Dr. Reaney, for example, holds that organized group games may have a compensatory function. *Op. cit.*

is as a rule secured through make-believe activities. Most common among such activities are play and fantasy. A child would fight, hunt, and make a home as particular stimuli arouse him. He is seldom in such an environment, however, and he is practically never so organized by inheritance or training that these undertakings can be fully carried out. There are inexhaustible inhibitors around him and within him which check free expression. And so he plays at, or has day-dreams of, fighting, hunting, and home-making. I have no desire at this time to say which of the unsatisfied impulses of childhood are inherited and which acquired; but, however they arise, we find that they are many and urgent, and consequently that every normal child must find compensation for their inhibition.

There are a number of factors which may act as inhibitors of the behavior tendencies of children. These may conveniently be divided into the *extra-organic* and the *intra-organic*, according to whether they are in the nature of environmental interferences or interferences which arise out of the child's own organism.

During his development the child is constantly running into *extra-organic* or environmental facts which are incompatible with the satisfaction of his desires. He may want to hunt. Perhaps the family cat supplies him with a stimulus to make this impulse felt. But this hunting impulse has become a particularized affair. Hunting is shooting, and he can not shoot because he has no gun. Instead of ignoring a stimulus to which he can not react adequately, he points a stick at the cat and shouts 'Boom!' He may then, and perhaps to his sorrow, try to drag in his 'dead' game by the hind legs. But the main and incontestable point is that the child is compensating, by means of his pretensions, for the inadequacies of the situation. He would like only too well to shoot a real gun and drag in game which is really dead, but his environment does not supply the appropriate circumstances. And so he plays.

Among the more important *extra-organic* factors which limit the child's expression are the people around him. Just as he discovers the splendid interior of his father's watch, someone takes the watch away from him. Just as he discovers the importance of certain corners of the pantry, someone carries him away to another room. Everywhere there are people and they are constantly interfering with his behavior.

As I have intimated, it is not only the lack of a physical world fitting in with every whim which causes the child to play rather than to act in earnest. He has also his *intra-organic* interferences arising out of his own complex little nature. For the pure joy of it he would, at times, like to bring down a stout club upon the head of his playmate—that is, he would like to do this if it were not for the disconcerting facts that he would not like to hear his playmate cry in pain, and that he would not like to feel the blows of his playmate's revenge. And so the two boys will play at fighting. Often, too, a child is hindered from acting as he would because of a realization of the smallness of his body and the slightness of his muscular strength. In such cases we are apt to have a mimicry of feats of strength and daring.

It is evident that there are instances of make-believe play and fantasy which apparently, at least, are not primarily compensatory. A child may straddle his hobby-horse, not because it is the best substitute for a real horse he would ride, but simply because he has been taught to do so by his parents. There is little doubt, however, but that the average child enjoys his playing the more where he perceives its symbolic relationship to a more serious pursuit. The fact that children's play is given much of its specific form by adults, does not, in the last analysis, indicate that it is therefore less compensatory. By custom and tradition we initiate various make-believe performances for children, but something in the nature of childhood must explain why children take to the make-believe with such enthusiasm. When we first teach a child to ride a hobby-horse he may be unaware of any connection between this activity and the actualities of horseback riding. But as he learns about real horses and real riding, his play will become more and more clearly compensatory in function. In other words, the rise of certain impulses in children is so inevitable that their compensatory expression may be provided for by the customs of the race. In the case of any one child a compensatory activity may be set up before the need for that particular compensation arises, but we may still consider the activity a typical product of child life and its characteristically incomplete adjustment.

Just as in certain individual cases a compensatory make-believe may arise before the need for that particular compensation, so specific habits of play and fantasy may be retained after the apparent need for their compensatory service is past. I know of successful men who find great pleasure in day-dreams of achievements which they would not care to have realized in any tangible fashion. In some of these cases the day-dreams express real desires which are denied direct expression because they run counter to other desires of a more powerful sort. In other cases, however, it is quite possible that day dreams which once had a compensatory function now operate as old habits and are retained because of their own repetition rather than because of any important compensation which they still render.

Play and fantasy are frequently concerned with situations more painful and disagreeable than any we should choose to meet in real life. A natural question arises as to the sense in which such make-believe can be considered compensatory. Children do not want to be in railroad wrecks nor to receive bullet wounds, and yet they enjoy pretending they are in such straits. So it appears on first thought, but, as a matter of fact, children do wish that just such things would happen to them, providing they might happen without pain or other ill consequences. In regard to railroad wrecks, if we could read a child's impulsive nature completely, we should probably find that he wishes he could be in a wreck and, at the same time, hopes he will not. He is in the same predicament as the boy who would like to club his companion and yet would not like to. And like that boy he compensates for his conflict by playing. In other words there are few, if any, situations in life which appeal to us in a purely negative way.

We do not, as a rule, want to suffer great misfortunes; yet there are certain factors, such as affectionate demonstrations on the part of our friends, the joy of being in the public eye, and the like, which give the majority of unfortunate circumstances a considerable amount of positive appeal.

Distinctly unpleasant play and fantasy may also provide for the compensatory expression of negative impulses. There is little reason to believe that fears, for example, do not require expression of some sort as urgently as more positive tendencies. Playing, day-dreaming and the telling of stories involving ghosts and goblins may well serve to express fears which must be inhibited in the world of actuality.

Holding the older view that childhood is a period of happiness and serenity, one could hardly accept an explanation of play in terms of compensation for incomplete or faulty adjustment—in terms of the partial resolution of conflicts between the child and his environment or between contradictory factors within his own character. I believe, however, that there is little need to argue against that older view. Childhood is primarily a period of incomplete adjustment, and we remember it as peaceful because we have forgotten its sorrows and because problems of great consequence to us in childhood mean little to us now. Full of impulses to do actual things, the child is equipped with a physique and surrounded by an environment which are constant obstacles. I do not believe, like some, that it is desirable, if possible, to remove these obstacles and make childhood a comparatively easy and comfortable state. Human life requires, and gets much of its value from, an abundance of nice adjustments which can come only as the result of long and necessarily arduous training. The child comes into the world with an inherited behavior equipment, but at best this equipment is an uncertain affair. Each impulse tends to operate in inappropriate as well as in appropriate situations. Each impulse, if the child is to become prepared for adult life, must be defined, and definition implies inhibition. The child must live through a period of paradoxes before he can become an individual of discrimination. If he were a perfect mechanism and if educational stimuli were perfectly coördinated, it is possible that he might be trained without being constantly thrown out of adjustment. Then, too, if the life for which society prepares him were more simple in its requirements, he might be spared some maladjustment. But the human organism is not perfect, and, while educational practice improves from time to time, the world rushes forward into new complexities. One who has any faith in the present direction of progress can hardly do other than accept the essentially incomplete adjustment of the young as a necessary product of that progress. The happy fact is that the conflicts of youth can be so adequately compensated for by the play and fantasy mechanisms.

While they are fundamentally natural and necessary phenomena of child life, play and fantasy can result in pathological as well as in normal compensations. And as normal compensations the forms which they take may modify the development of character to a marked degree. For these reasons their exhaustive study, as but different manifestations of a single process, is essential. Indeed it seems to me that few fields may be more profitably explored either by those interested in child life in general or by those interested in some particular child. From this compensation process, studied for what it really is, we may hope to gain some new and useful knowledge about the stresses and strains of human development.

Still, even at the present time, it is possible to point out some of the principles which operate in the compensatory behavior of children.

The distinction between play and fantasy is, of course, a distinction between overt and ideational behavior. Play, in so far as it is pretending, is never without an element of fantasy, but we may arbitrarily confine the application of the latter term to those forms of pretending which are lacking in overt bodily accompaniments. It will then be possible to distinguish between these two types of compensation and to note their interrelations.

Although we cannot be certain of it, play probably precedes fantasy in the child's life. The latter does appear quite early, however, in some children at least, and before the school age is reached both are clearly present. I remember the interesting evidence for the early rise of fantasy given to me by a little girl of not more than four, who said, placing her two chubby hands before her face, "Let's shut our eyes and play we're at Gran'ma's."

In play and fantasy there are two factors, which may or may not be consciously recognized by the child, determining to a large extent the nature of his pretending activities. In the first place, there is a tendency toward breadth and freedom of expression. The child must express impulses which are often clearly incongruous with his world of actuality, and the greater this incongruity the more lively will be the flights of imagination to which they give rise and the more apt will the child be to engage in private fantasy rather than in overt play. In the second place, the satisfaction which is derived from compensatory behavior depends to some extent upon its being within the limits of the child's own credulity. The impulses which drive the child are aimed at an actual world, and their indirect expression itself must not get too far beyond the realms of that actuality. Thus, we may think of these two main determinants of play and fantasy as (1) the child's natural tendency toward free expression, and (2) his need for a certain credibility in experience.

The tendency toward free expression leads to the establishment of all sorts of fictitious characters and objects within the playground. Toys and playmates which do not fit in with the completer, fancied world may be put aside. I remember that even up to the age of sixteen I frequently judged congeniality in terms of the readiness of others to disregard reality in favor of a world of pretty definite and well defined fancy. I always preferred to *knock grounders* with one particular lad because he coöperated so well in converting the procedure into the pretensions of a *big league* game. The same was true in boxing. Having read and memorized the details of most of the historic ring battles, we repeated many of these almost blow for blow upon the floor of my mother's laundry. And many were the Harvard-Yale football games in which I engaged with one other actual player, both of us, as often as not, playing on the same side. In cases of this sort, the meaning of ordinary play activity is widened by the liberal use of fantasy.

In the course of an individual's development many impulses arise which can not be expressed to any satisfactory extent in a coöperative fashion. Often a child is afraid of being laughed at for the world he would live in. Under such circumstances there may be a withdrawal from play to pure fantasy with its wider possibilities for pretending. Indeed, one of the signs of coming adulthood is the giving up of overt play and the switching over to compensatory behavior of a more private sort. Adults seldom play in the childhood sense of that term, unless it be in art. In the adult, compensations through pretending are more likely to be worked out in private day-dreams. The fact remains, however, that less compensation of any kind is necessary in the general run of adult lives, so that we may safely assume that fantasy as well as play is more common during childhood.

Along with this tendency toward free expression, we have a tendency to make that expression as realistic as possible. Children are constantly recognizing inconsistencies in their play life and trying to patch them over as best they can. When, as a very small boy, I played with tin soldiers and miniature locomotives, I always felt the inappropriateness of the size of my own body. The device which I hit upon to get around this difficulty I called *Playing You Are Nothing*. Every playfellow who entered into the world of my tiny armies and railroads was introduced to the proposition of suspending all interest in his own body. The running of the trains and the marching of the troops were to be considered as events independent of ourselves. There was one youngster who could not push a locomotive across the floor without playing *he* was the engineer. His fate was obvious. I never invited him to play unless I could get no one else; and, when he did come, it was to be made miserable by my constant insistence that he must play he was nothing. Our disagreement, of course, grew out of the fact that each of us in his own way was striving to give the play a more vivid atmosphere of reality.

Just as overt play often passes over into private fantasy owing to a struggle against the limitations of the actual social and physical world, so private fantasy often passes over into overt play in the interests of greater credibility. As a child I was full of baseball fantasies. Although I played baseball a great deal, these games did not satisfy certain standards set up by reading athletic stories and watching older and more skillful players. But the fantasies, too, often became unsatisfactory on account of their intangibility. As a result I formed the habit of laying out a diamond upon the lawn and there, without ball or playmates, carrying out the overt movements of an heroic baseball performance. Many a time, I pitched nine long innings to baffled athletes who swung immaterial bats at my imaginary curves. Here was fantasy improved and made realistic by the actuality of its muscular accompaniments.

The topics of private fantasy are perhaps even more apt to find increased tangibility by being brought into contact with a real social world. The child knows that his day-dreams are unreal, but the insistence of that fact becomes less troublesome if only he can get some one else to believe or act as though he believes in the reality of those imagined events. Many of the lies of children arise out of such circumstances. A boy longs for a pony and a box of tools. He fancies these things in his possession, and before a great while he somehow feels driven to tell his friends either that he already has the things he desires or that he has been promised them. An acquaintance of mine spent her earliest years on a farm which was more or less out of touch with the livelier affairs of the world. Now it so happened that an older sister in this household was sent to town to finish her education. Upon her return she had much to say of her experiences. These tales thrilled the younger sister and stimulated her to day-dreaming. Soon after this the little girl began her own education at a neighboring country school. As she tells of it now, almost her first intercourse with her school mates was marked by her own spectacular reports of what *she* had seen and heard while sojourning in the town which really she had never been near.

It is interesting to note here that the literary make-believe of adults contains within it evidence of the tendencies toward free expression and credibility, which I have mentioned as such significant factors in child life. Written fiction, for example, may be thought of as an instrument for free expression and the spoken drama as an instrument for giving human fancies increased tangibility. It is hardly necessary to point out the importance of artistic appreciation and production for the compensatory life of children.

This view of play as a compensatory mechanism does not pretend to refute the more familiar theories which, by the way, were not formulated with special reference to the make-believe. That theory which describes play as a recapitulation phenomenon simply states that the primary impulses expressed in play appear more or less spontaneously at set periods in the child's life, and that the child's activities during successive periods of his life are definitely reminiscent of the typical periods of racial development. Most of us would probably admit that there is a rough similarity between individual and racial development. But the view that play is a compensatory activity demands neither the acceptance nor the rejection of this theory. One need not know nor try to guess the exact origin and analogies of a child's impulses to realize their variety and the conflicts among them which demand the compensatory service of the make-believe.

The theory that play prepares a child for later life, if broadly enough interpreted, fits in quite well with the notion which has been developed here. Many impulses arise during childhood which, while they can not be directly expressed at that time, still demand preservation. A boy may be interested in machinery. If he is permitted or even encouraged in his play and fantasy to concern himself with machines, a very useful interest may be preserved for the time when it can find adequate expression. If it were not for compensatory expression through play and fantasy, it is quite conceivable that many such early rising interests or impulses would suffer repression and thus be lost as far as useful functioning is concerned.

The theory that play furnishes an outlet for surplus energy is somewhat vague, but as far as it goes it meets with no contradiction from the conception that play is compensatory in function.

Much the same may be said for the recreational theory, which really finds some little support in the type of facts which I have been presenting. A boredom which longs for some impossible or impractical distraction is often indirectly relieved by a compensatory make-believe. The school boy, tired of his lessons but afraid to dash from the class room, may partially satisfy himself with a fantasy of the swimming hole. The worried business man, whose unused muscles would not tolerate exertion, may yearn to play ball and take his yearning out in fantasy.

In conclusion, play, the more private forms of fantasy, much lying and story telling, and the appreciation of stories all serve the same fundamental purpose in human life. They are compensatory mechanisms. They are more typical of children than of adults, because it is in children that the most incongruity exists between different impulses and between impulses and the surrounding world of actuality. The nature of play and the other compensatory mechanisms is determined by the need of imperfectly adjusted organisms to express their impulses as freely as possible without too greatly straining the possibilities of their own belief.

It is essential, if not self-evident, that play should not be thought of as behavior which is usually undesirable or pathological simply because its function is compensatory. Neither should we think that, because play grows out of imperfect adjustment, we should strive for a world in which play is unnecessary. Simpler organisms than ourselves get compensation through play. The ancients in a comparatively simple civilization got compensation through play. And in all likelihood the further humanity advances upon its present path of progress, the more important will be play and its related phenomena, especially for the young of the species.

THE CONTROL OF ATTITUDE IN PSYCHO-PHYSICAL EXPERIMENTS

BY EDWIN G. BORING

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Dr. Godfrey Thomson has recently published some very illuminating discussions of the mathematical logic of psychophysics, and I for one feel myself too much in his debt to to indulge in the mere picking of flaws. Nevertheless his paper, 'A New Point of View in the Interpretation of Threshold Measurements in Psychophysics,' [1] gives me concern because it seems to cast aspersions upon what I have regarded as the most promising direction of development in psychophysics. It is true that Dr. Thomson's 'offences' are implied rather than actual and that ultimately I may discover him in agreement with my thesis; nevertheless his article furnishes a reason for placing the point of view that I have in mind squarely before psychologists. I may add that I conceive that I am merely explicating an idea that arose within the Cornell Laboratory. If there be any credit for its origin it is due Cornell, though the responsibility of the present exposition is mine.

Dr. Thomson's suggestion is as follows: In the test of Weber's law (he is thinking of lifted weights as the example) we may take the differential threshold, which is half the distance between the upper and lower limens (*i.e.*, half the 'interval of uncertainty'), as the measure of sensitivity; and such has been the usual practice. Dr. Thomson, however, prefers to take as a measure of sensitivity the interquartile range of the point of subjective equality. The threshold is proportional to the distance between the two points where the psychometric functions for 'greater' and for 'less' cross the 50 per cent. abscissa, and its amount

[1] Godfrey H. Thomson, 'A New Point of View in the Interpretation of Threshold Measurements in Psychophysics,' Psychol. Rev., 1920, 27, 300–307.

depends upon the number of judgments that fall within the third category of 'undecided' judgments (as Dr. Thomson styles them) or 'equal' judgments. Dr. Thomson would take these 'undecided' judgments and divide them equally for every stimulus value between the 'greater' and 'less' categories, thus establishing two new psychometric functions (instead of the original three) which of necessity will intersect upon the 50 per cent. abscissa and give zero limens. The interquartile range is the distance between the two points of intersection of these new psychometric functions with the 75 per cent. abscissa. It is independent of the 'undecided' judgments and dependent upon the measure of precision (h) of the psychometric functions, since the steeper the curves the less the interquartile range, and *vice versa.*

Dr. Thomson's preference for the interquartile range as a measure of sensitivity lies in his distrust of the relative frequencies of the 'undecided' judgments upon which the threshold depends. The threshold, he writes, 'depends entirely upon the subject's readiness to give the answer *undecided.* It measures therefore rather a moral character than a physical sensitivity.' 'The moral character of the measure $S - S'$ is above all seen from the fact that any subject who wishes may reduce it to zero, whatever may be his actual sensitivity, simply by determining that he will never give an answer *undecided.*' Thus the interquartile range 'is more physiological than the threshold measure.' The threshold is to be mistrusted because 'the decision as to what proportion . . . is to be called heavier, what undecided and what lighter depends upon a conscious act of the subject, and can be varied, if he be so disposed, at his whim; and will vary with his mood at the moment.' It is such a designation of things constant and measurable as 'physiological' and of things inconstant and uncontrolled as 'moral' or psychological that moves a psychologist to reply.

It is not my present purpose to inquire how much of Dr. Thomson's argument is actually new. Fechner wished to measure *Unterschiedsempfindlichkeit* by h and divided his *zweifelhafte* cases between the 'greater' and 'less' categories.[1] G. E. Müller opposed Fechner, arguing that the limen must be used as the basis of Weber's law.[2] But Fechner stuck to his guns,[3] as did also Müller.[4] In 1904 Müller was wondering 'wie in aller Welt kann man ohne weiteres voraussetzen' that the measure of precision, which is independent of many factors entering into the lifting of weights, could constitute the basis of Weber's law. Dr. Thomson may need to meet Müller's argument against Fechner.[5] I belong to the younger generation to whom Weber's law and *Unterschiedsempfindlichkeit* are less sacred than they once were, and I am willing to admit that the matter may well wait for supporting facts. Experimental studies are wanted that show both the threshold and the interquartile range as functions of the absolute magnitude of stimulus, and then we can determine how each fits the Weber-Fechner formula. But I am not willing to let Dr. Thomson dissuade us in advance from an interest in the threshold because its 'morality' can not be controlled. There is enough experimental work to indicate, so it seems to me, that accurate control of the third psychophysical category is possible and scientifically necessary.

The Experimental Control of the Psychophysical Judgments

1. In the first place, if we are to gain accuracy of definition of the psychophysical categories, we must exclude the doubtful judgments.

Dr. Thomson has ample historical ground for including them. They have been left in from the first. Fechner and Müller called them the 'z-cases' (*zweifelhaft*), as we have seen. Müller raised the question as to whether 'doubtful' and 'equal' judgments ought not to be separated, but concluded

[1] G. Th. Fechner, 'Elemente der Psychophysik,' 1889, I, 101ff.; 'Revision der Hauptpunkte der Psychophysik,' 1882, 24f.

[2] G. E. Müller, 'Zur Grundlegung der Psychophysik,' 1878, 28f., 33–36.

[3] Fechner, 'Revision,' 48f.

[4] Müller, 'Die Gesichtspunkte und die Tatsachen der Psychophysischen Methodik,' 1904, 104–109.

[5] E. B. Titchener summarizes the controversy: Experimental Psychology, 1905, II, ii, 278–285.

on experimental evidence that the positive impressions of 'equal' were rare, especially in trained observers, and that separate treatment of them was therefore not necessary.[1] Titchener in 1905 grouped both 'doubtful' and 'equal' cases under the heading 'u' (uncertain) or '?'.[2] Urban in 1908 had his subjects guess when in doubt which weight of the lifted pair was the heavier. Thus he obtained the categories 'heavier-guess' (hg) and 'lighter-guess' (lg), which he saw fit later to combine as 'equality' judgments.[3] In such a setting it is natural to call the region in which the 'equality' judgments are most frequent the 'interval of uncertainty.' Urban's pupils have followed his final practice. Fernberger wrote: "The equality judgment was more complex [than the other judgments] as it not only included cases of actual subjective equality . . ., but also all those cases where it was impossible for the subject to give either a lighter or a heavier judgment, usually termed doubtful cases." [4]

It is under this practice that Dr. Thomson would reject the interval of uncertainty as a measure of sensitivity because 'any subject who wishes may reduce it to zero . . . simply by determining that he will never give an answer *undecided*.' He is undoubtedly thinking of results like Fernberger's on the effect of attitude on the interval of uncertainty; but plainly he does not accept, as I think he should, Fernberger's contention that attitude can be controlled in the laboratory and that, in view of this state of affairs, it must be controlled.[5]

Fernberger's proposal was that attitude should be rendered constant by explicit instructions and observational training,

[1] Müller, 'Methodik,' 12f.

[2] Titchener, *op. cit.* II, i, 107ff; ii, 268.

[3] F. M. Urban, 'The Application of Statistical Methods to the Problems of Psychophysics,' 1908, 5f., 15, 99f., 106f., 110f., *etc.*

[4] S. W. Fernberger, 'On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli,' *Psychol. Monog.*, 1913, No. 61, 16.

[5] Fernberger, 'The Effect of Attitude of the Subject upon the Measure of Sensitivity,' *Amer. J. Psychol.*, 1914, **25**, 538–543. Fernberger refers here to another case in his own experiments and to Warner Brown's results. He has since called my attention to a study where three of eight subjects give no interval of uncertainty, presumably because of failure of attitudinal control: A. L. Ide, 'The Influence of Temperature on the Formation of Judgments in Lifted Weight Experiments,' 1919, 25 pp. (Univ. Pennsylvania thesis).

but I am going further in insisting that the instructions and experimental setting must assure a practically complete elimination of doubtful judgments. This is the point of George's study,[1] which merits, I think, considerable attention from psychophysicists. George's thesis is that in a psychophysical experiment we are dealing with a series of mental states which are a continuous function of the given series of stimuli: we vary the stimuli and note the concomitant mental variation. Under the rules of scientific experiment everything else must be kept constant including the attitude and psychophysical constitution of the subject; if they are inconstant we can no longer tell of what our judgments are a function. Moreover, if we find that any particular category is in itself an indicator of a change in attitude, we must so arrange the experiment that this form of judgment will not occur or rule it out from the results if it does occur, since the information that it yields is beside the point of the psychophysical problem, which always proposes the establishment of the dependence of judgment upon varying stimulus. George finds doubt and the doubtful judgments to be the great offenders against constancy. His method is the establishment of an *Einstellung* for constancy of attitude and the determination of (1) what categories the maintenance of the *Einstellung* rules out, (2) what categories, conversely, are noted by the observers as interfering with the *Einstellung* when they do appear, and (3) the objective evidence of attitudinal shift furnished by reaction times. George's article must be its own summary; I can not do it justice here. It is scarcely more than preliminary and his method itself may yet be called in question. It would be a slender weapon with which to combat the main body of psychophysical practice were it not for the fact that, like Dr. Thomson, many psychophysicists already know that something or other must be done to gain greater constancy of results, that the case against the doubtful judgment is plausible *a priori* [is a doubtful difference *ipso facto* less than an undoubted one?],

[1] S. S. George, 'Attitude in Relation to the Psychophysical Judgment,' *Amer. J. Psychol.*, 1917, 28, 1–37.

and that the attitude that George proposes as an ideal has already gained some slight support from experimental usage.[1] My personal prejudice for George's conditions I shall mention presently.

2. Doubt is the most persistent offender and the one presumably most responsible for Dr. Thomson's strictures upon the interval of 'uncertainty,' but other attitudinal seducers must also be dismissed. George makes a case against expectation, a case which indicates among other things that a haphazard method of presentation is to be preferred to a serial method with partial knowledge (*e.g.*, the method of least perceptible differences).

'Reflective' judgments are the general class under which inconstancy is apt to occur. George does not find that the reflective attitude necessarily means uncontrolled variability but merely that it favors it. The reflective judgment is very often equivocally determined; the subject is in a dilemma as to his report and *decides* what to report. He may be judging on the basis of more than one criterion, and may find in a given case different categories concurrently indicated by different criteria. If he must make a univocal report for such an equivocal situation, he must make some decision, even though the decision may be for the category 'undecided.' It is the occurrence of such uncontrolled 'decisions,' I take it, that makes Dr. Thomson wish to give up the threshold as a measure of sensitivity; but his rejection of the threshold is not necessary on this score, since reflective judgments can be avoided.

George shows that doubtful judgments tend to be reflective. The doubting subject is resolving a dilemma in favor of one category or the other, and does justice to the unreported category by labelling his judgment 'doubtful.' The 'or-judgments' (*e.g.*, 'equal-or-less') are compromises in which neither side has won out. The category 'no-difference' is also often a reflective compromise. Reflection and hesitation are, however, not synonymous; a judgment may be long

[1] L. B. Hoisington, *Amer. J. Psychol.*, 1917, **28**, 588ff.; M. Kincaid, *ibid.*, 1918, **29**, 227–232; A. M. Bowman, *ibid.*, 1920, **31**, 87–90; C. C. Pratt, *ibid.*, 1920, **31**.

delayed and yet come as a simple report of mental process without any indication of resolution or of attitudinal inconstancy. In fact it seems probable that the time of formation of the judgment is one of the mental factors which under a constant attitude is a serial function of the stimulus and a feasible subject for exact psychophysical investigation.

The type of judgment furthered by ordinary psychophysical procedure is shown in the introspective analyses in Fernberger's recent monograph.[1] These descriptions he obtained under conditions analogous to Urban's except in so far as the introspection itself interfered. Compared with what I have in mind as the ideal, they show a relatively complex—often very complex—process of comparing. I should not on the basis of them expect even the degree of constancy which one actually does get. The equality judgments are typically reflective in that they involve a 'verification process,' which Fernberger, as if in support of George, seems to equate to 'doubt.' [2]

Of course what is needed to support my argument, and what is lacking, is the companion introspective study made under George's conditions. Perhaps these consciousnesses would not prove so simple as I think. My conviction that the two consciousnesses would, however, be very different affairs is dependent, I must in honesty confess, upon personal experience. I was one of George's observers and I have observed in other experiments under his conditions. I have also observed with lifted weights on the turning-top table under conditions patterned after Urban's. And the two consciousnesses are to me almost unbelievably different. Under the conventional procedure I am constantly forced into resolutions, verifications, decisions, like those that Fernberger describes, and thus I am led into doubt and discomfort, and thence into a naïve uncritical attitude which affords no assurance of rigorous constancy. Under George's conditions enough effort is required to be sure, but the mental process is kept simple or else rejected. I do not have to report a

[1] Fernberger, 'An Introspective Analysis of Comparing,' *Psychol. Monog.*, 1919, No. 117.

[2] *E.g.*, *ibid.*, p. 160.

complex situation by an inadequately simple word, nor an equivocal setting by a univocal judgment. It seems reasonable, does it not, that a report under George's conditions should be worth more than a report under Urban's, because it leads to attitudinal constancy or the detection of inconstancy when it does occur.[1]

3. There is as much to be gained by the establishment of definite serial criteria of judgment as by the elimination of unsuitable categories of judgment. In fact, these two remedies are obverse and reverse. One can not get rid of doubt, for instance, unless the judgments are based upon a univocal criterion. Two criteria that may conflict are fatal to constancy since their resolution is left to chance.

The 'stimulus error' is a term which among other things characterizes judgments where a definite mental criterion is not established but judgment is left to chance habits. The term is undoubtedly much maligned as there are cases where the direction of the attention to the stimulus is psychologically useful, as in the preliminary investigation of a new perceptual field. But in general the stimulus attitude means indefiniteness and instability of criterion, as George pointed out.[2] A recent series of studies on the various criteria that may underlie the judgments of cutaneous duality shows how fundamental to accurate psychophysical work an avoidance of the stimulus attitude is.[3] In lifted weights Friedländer's

[1] In justice to Urban it must be said that I do not believe he would experiment now as he did in 1908. *Cf., e.g.*, the 'Statistical Methods,' 1908 (*op. cit.*), with his 'Ueber einige Begriffe und Aufgaben der Psychophysik,' *Arch. f. d. ges. Psychol.*, 1913, **30**, 113–152. It is a pity that he has not yet been able to return to experimental work since writing his systematic articles. Moreover, both Urban and Fernberger have been under a special disadvantage in that they were dealing with a peculiarly refractory material, lifted weights. Isolation of a homogeneous series of univocal mental correlates of the stimulus is very difficult in the lifting process, although it has been attempted: H. Friedländer, 'Die Wahrnehmung der Schwere,' *Zeitsch. f. Psychol.*, 1920, **83**, 129–120.

[2] George, *op. cit.*, 35f.

[3] E. J. Gates, 'The Determinations of the Limens of Single and Dual Impression by the Method of Constant Stimuli,' *Amer. J. Psychol.*, 1915, **26**, 152–157; Titchener, 'Ethnological Tests of Sensation and Perception,' etc., *Proc. Amer. Philos. Soc.*, 1916, **55**, 206–215; E. deLaski, 'Perceptive Forms below the Level of the Two-point Limen,' *Amer. J. Psychol.*, 1916, **27**, 569–571; C. L. Friedline, 'Discrimination of Two Cutaneous Patterns below the Two-point Limen," *ibid.*, 1918, **29**, 400–419.

experiments, although they leave much of psychophysical accuracy to be desired, seem at least to show that different numerical results follow when attention is upon the weight and when it is upon the sensory aspects of the lifting.[1]

The more general ground for the control of criteria is the one which Fernberger took in defense of the interval of uncertainty.[2] The subjects must be constantly and effectively *eingestellt*, and the test of an effective *Einstellung* lies in preliminary trials, the taking of introspections, and the observer's full and repeated characterizations of their attitude.

4. The need for the isolation of the single judgment within the series is perhaps worth especial mention since the matter has just been [implicitly] brought to fore by Fernberger's measurement of the effect of one member of a series upon a succeeding member.[3] Under Urban's conditions with the turning-top table Fernberger found that the judgment 'lighter' of a pair of lifted weights tends to be succeeded by a judgment 'heavier,' and *vice versa.* 'Fixing' a series so that one kind or the other of sequences predominates produces as startling effects upon the form of the psychometric functions as anything that Dr. Thomson is complaining of. Plainly some sort of expectational or rhythmic effect is operative; attitude is not remaining constant. Fernberger's solution of the difficulty is to balance one sort of succession against the other, and trust that they will cancel. For myself, I could not feel secure in such a procedure; an algebraic cancellation where the factors are so little understood can not be so satisfactory as an actual elimination. The members of the series should be separated by an interval—a distracted interval if necessary—so that the intraserial effects are broken up. This course slows down the rate of experimentation and robs the turning-top table of much of its charm; but, even when relative frequencies are aimed at, I do not conceive that numbers of cases can be allowed to weigh against rigorous scientific control.

[1] Friedländer, *op. cit.*

[2] Fernberger, *Amer. J. Psychol.*, 1914, 25, 538ff. (*op. cit.*).

[3] Fernberger, 'Interdependence of Judgments within the Series for the Method of Constant Stimuli,' *J. Exper. Psychol.*, 1920, 3, 126–150.

5. Lest the argumental sauce obscure the meat, let me summarize. I recommend (1) that every judgment in the psychophysical experiment stand absolutely independently in its own right. One member of the series must be separated from the others in time, and by the instruction to the subject that he judge it without reference to any other member. Haphazard presentation should be the rule; at any rate serial presentations with partial knowledge should not be allowed since they connect the members and interfere with their individuality. I urge further (2) that the criteria of judgment be laid down explicitly and univocally in psychological terms. These psychological terms will be sensory in the class of experiments especially under consideration. Judgments of stimulus are often desirable, but they are not the final ideal since they are *ipso facto* equivocal.[1] The univocal character of the criteria must be tested by introspection and by the subjects' report. (3) The total *Aufgabe* under which the subject judges must be made definite in instructions, and must be more fully determined by means of repeated characterizations by the subject of his attitude and procedure. This latter check is important since much of the subject's instruction is apt to be a self-instruction. (4) The subject must be both instructed and trained to maintain a constant attitude throughout the experiment and to report lapses from this attitude. When he has learned the full meaning of this instruction, he will not give doubtful judgments nor ordinarily be doubtful, provided his task is made sufficiently easy for him by the means of the three foregoing rules. He will in like manner avoid other reflective judgments that violate the constant attitude. (5) He will probably tend to give immediate judgments, and he will be greatly helped if he is encouraged to report quickly. His times, however, will vary

[1] I have met psychologists who smile superiorly when I mention the 'stimulus-error' and even have something to say in reply about 'bigoted introspectionism,' so I know that I ought not, on purely diplomatic grounds, to bring the stimulus-error in; but unfortunately for diplomacy it belongs in. I hope some day to show that the stimulus-error is not a figment of an epistemologizing or a quibbling mind, but that it is a very real scientific devil. In the meantime let those whom numerical measures alone will impress, see Friedline, and Friedländer, *opp. citt.*

and there will occasionally be long delays [they seem like 'inhibitory jams'!] without gross shift of attitude.

Undoubtedly research will bring more means of control to the fore, but the observance of these five rules alone will, I think, give Dr. Thomson data for which the thresholds will show the degree of constancy that he desires. If in the course of doing all this he thinks that we have made the thresholds 'more physiological' and less 'moral,' well and good. It is of this sort of stuff, nevertheless, that psychology is made.

The Nature of the Psychometric Functions

In the article which has caused me to write this paper, Dr. Thomson supplies us with certain suggestions as to the nature of the psychometric functions—an exposition that employs the familiar device of the urn and balls. He gains comfort from this analogue, I take it, because within it he can show how the number of black balls necessary for a given category 'depends upon a conscious act of the subject, and can be varied, if he be so disposed, at his whim.' He gains support for his analogue (1) from the fact that it gives psychometric functions that are in accord with present statistical analysis[1] in that they are not normal curves (they are not the *phi*-function of *gamma* nor the normal bell) and (2) from the fact that the three psychometric functions are founded upon a single underlying error curve. I am quite ready to be convinced of (1), but (2) seems to me scarcely a reason, since there are other ways of founding a set of psychometric functions upon a single error function. I have suggested such a derivation elsewhere,[2] and I desire here to raise the question which of these two analogues better represents our notion of the psychophysical organism, and, furthermore, whether experiment and curve-fitting may not ultimately decide between the two and thus throw light upon the nature of sensitivity.

[1] Cf., Thomson, *op. cit.*, 304–307; 'The Criterion of Goodness of Fit of Psychophysical Curves,' *Biometrika*, 1919, 12, 226–229.

[2] Boring, 'A Chart of the Psychometric Function,' *Amer. J. Psychol.*, 1917, 28, 465–470.

Dr. Thomson's parable runs thus. A stimulus (or a stimulus-pair) is like an urn with black and white balls in it in a given proportion. The experimental trial is the drawing of a given (constant) number of balls from the urn. Something varies so that different numbers of black balls turn up in the successive drawings. The number of black balls drawn represents the impression; there is a series of impressions possible for a single stimulus all the way from no black balls to all black balls. (At least so I interpret Dr. Thomson.) The subject reports on this series in terms of (say) three categories. Dr. Thomson thinks that the subject decides, 'if he be so disposed, at his whim' what proportions of black balls drawn shall be reported by each of the categories, *e.g.*, whether 40 per cent. to 60 per cent. shall be reported as 'undecided' with the reports 'less' for under 40 per cent. and 'greater' for over 60 per cent. I urge that rigid criteria should replace the subject's whim. The same thing happens for the other stimuli, which are represented each by other urns in which the proportion of black balls is different. The series of urns, analogous to the series of stimuli, shows a continuously increasing (or decreasing) proportion of black balls, but the relation between the place of the urn in the series (stimulus-value) and the proportion of black balls need not be linear. Psychometric functions result that are similar to those actually obtained in practice, although they are not normal functions.

My own parable, adapted to the differential limen and the urn, separates the stimulus from the organism. A given stimulus supplies a fixed number of black and white balls, since a stimulus is ideally constant. In stimulating the organism its effect is facilitated or inhibited according to the chance disposition of the organism, that is to say, the organism is an urn from which an additional number of black and white balls is drawn to be added to the fixed number determined by the stimulus. The total resulting proportion of black balls fixes the position of the impression in the impressional series, and the report in terms of predetermined categories occurs as it does in Dr. Thomson's reasoning. The constant tendency toward a given proportion of black balls is a function of the magnitude of the stimulus, and the variability about this constant tendency is a function of the organism (the urn). The proportion of black to white balls in the urn may be anything at all but is the same for all stimuli. The resulting ogive psychometric functions may, in a certain simple case, be normal, but there is no reason why they should be nor *a priori* presumption that they would be.

My first question is this. Is it not more reasonable to ascribe constancy to the stimulus and variability to the organism, and to assume a law of physiological variability that is fixed and independent of the magnitude of the stimulus? In other words, should not the organism be represented by a single urn with contents of fixed composition? I confess I am not clear as to what Dr. Thomson's urns symbolize nor as to precisely where in his scheme variability occurs. He has an urn of different composition for every stimulus. If variability resides in the organism, then the law of variability changes for every stimulus, and such an occurrence does not seem to me physiologically understandable.

To my second question I am not even prepared to suggest an answer. May it not be possible to determine empirically what urn-and-ball analogy best fits the facts, and thus analytically to learn something new of the laws of organic variability? A determination would be ever so much better than guesses—Dr. Thomson's or mine,—but for most psychologists curve-fitting is precarious work. If we had the proper data, would the mathematical solution be feasible? Perhaps Dr. Thomson will tell us.

THE PHYSICAL MEASUREMENT AND SPECIFICATION OF COLOR

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A complete understanding of the subject of color involves a knowledge not only of the nature of the sensation resulting from the action of the radiant energy upon the retina, but also of the exact physical composition of that radiation. The physicist has been inclined to overlook the nature of the sensation and to regard as sufficient for a complete specification of color a determination of the exact physical composition of the radiation considered. On the other hand, the psychologist has been prone to neglect the radiation factors and to regard as sufficient a specification of the nature of the sensation resulting when this radiation acts upon the retina. It is important for the sake of continued progress in this field that both phases be given due consideration. In this paper it is proposed to present an outline of the methods whereby the various factors necessary for the complete specification of color may be determined. It is the desire of the writers to treat this subject so as to give a broad general survey of the whole field in order that the interrelations between the various factors may be emphasized rather than to deal in detail with any specific phase.

A careful consideration of the subject as a whole leads to the conclusion that for the measurement and specification of the nature of the stimulus, *i.e.*, radiant energy, analytical methods must be employed, while in dealing with the sensation the methods are necessarily of the synthetic type. The necessity of using analytical methods in dealing with the stimulus is due to the fact that radiation in general is composite in nature and must be separated into its component parts in order that each may be measured. The most generally accepted theory of radiation postulates that radiant energy is transmitted as a form of transverse wave motion in which the wave-length or frequency may vary throughout wide limits. When radiation of certain wave-lengths reach the retina the sensation of light is produced. The wave-length is usually specified in terms of millimicrons ($\mu\mu$), the micron being one thousandth (.001) of a millimeter. The Ångstrom unit (.000,0001 mm.) is sometimes used and is one-tenth of a millimicron. The visual range is approximately from 400 to 700 $\mu\mu$ although radiation of shorter or longer wave-length may be perceived if sufficiently intense.

When any stimulus acts on the retina the resulting sensation gives no indication as to whether or not the stimulus is simple or compound in nature. This indicates that the retina is a synthetic receptor and does not recognize the individual component parts of the radiation as such but receives the mixed radiation as a single stimulus producing a single sensation. The sensation produced may be specified by two factors, brightness and color, the former being dependent on the intensity, and the latter on the quality (wave-length composition) of the stimulus. The Committee on Nomenclature and Standards of the Illuminating Engineering Society states: "Color of luminous flux is the subjective evaluation by the eye of the quality of the luminous flux. Any color can be expressed in terms of its hue and saturation." [1]

Since visual sensation is dependent upon the radiant energy emitted by some luminous source, it will be well to consider briefly the manner in which the quality of such emitted radiation may be measured and specified. This is most satisfactorily accomplished by separating the radiation into its component parts and measuring the intensity of the individual element. This factor is properly expressed in watts (or ergs per second) per square centimeter of the emitting source per unit difference in wave-length. Such values when plotted as ordinates against the various wave-lengths as abscissæ result in a graphic representation of the spectral energy distribution for the source considered. Such a curve is commonly referred to as a 'spectral energy curve' or

[1] *Trans. Illum. Eng. Soc.*, 1918, 13, 515.

'emission curve' and when determined for all wave-lengths within the visible range constitutes a physical specification of the quality of the emitted radiation, and hence of its color. The measurement of the energy values is accomplished by use of an instrument known as the spectro-radiometer in which either a bolometer or thermopile is usually employed as a receiving element. It will not be advisable to go into a detailed discussion of such instruments and methods at the present time, and for more complete information the reader

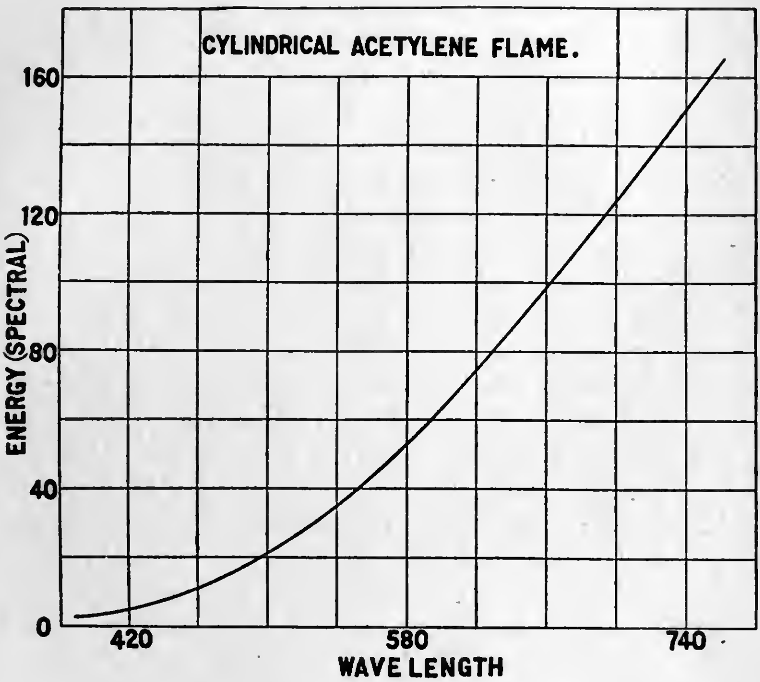

FIG. 1

is referred to the literature on spectro radiometry.[1] The spectral energy curves of many sources have been precisely determined and as a typical example, the curve of the cylindrical acetylene flame is shown in Fig. 1. A black body is probably the best standard for spectral energy distribution,

[1] Nutting, P. G., 'Outlines of Applied Optics.' Philadelphia: P. Blakiston's Sons & Co., 1912, p. 234, Chapter IX. Also see recent reports by P. D. Foote in *Trans. Amer. Inst. of Mining and Metallurgical Engineers.*

but a black body is difficult to realize in practice so that a standard acetylene burner operated under the conditions recently specified by Hyde[1] is practically identical with a black body operated at 2360° K., *i.e.*, 2087° C. Such a source is easily reproducible.

The precise determination of the emission curve of the source requires an elaborate equipment and considerable experience in manipulation on the part of the operator. However, if this function is known for one source that of any other can be determined indirectly with considerable ease by a method of spectrophotometry. The method consists simply of a comparison and quantitative measurement, wave-length by wave-length, of the intensity of the unknown source in terms of that of the known throughout the visible range. A spectrophotometer for this purpose consists essentially of an optical system such that two beams of light, one from the source whose spectral emission is known and the other from the source which is being measured, may be dispersed into their component parts. From the spectra thus formed narrow regions may be isolated and used to illuminate the parts of some suitable photometric field. In the path of one beam is situated some device such as a pair of Nicol prisms, a rotating sector, or a slit of variable width by which the intensity of that beam may be varied in a known manner. By the proper adjustment of this device, a photometric balance may be made and from the constants of the system the ratio of the intensity of the known to the unknown is determined. The measurement of this ratio at a sufficient number of points suitably spaced throughout the visual wave-length range provides the data from which the emission curve may be plotted. As examples of the most commonly used spectrophotometers may be mentioned the Lummer-Brodhun, the Brace, the Hufner, König-Martins, each of which has its peculiar advantages and disadvantages for the various special purposes in the field of spectrophotometry. Space does not permit a detailed discussion of this instrument and again the

[1] Hyde, Forsythe & Cady, *J. Frank. Inst.*, 1919, **188**, 129–130. See also Coblentz, *ibid.*, 1918, **188**, 299.

reader is referred to the literature on the subject for more complete information.[1]

Non-luminous objects are visible only by virtue of the radiant energy which they transmit, reflect, or otherwise divert, in such manner that it enters the eye and falls upon the retina. In case an object reflects or transmits to an equal extent all wave-lengths of the incident energy within the visible range, it is said to be visually non-selective or colorless. This class of objects includes all true grays, which form a scale varying only in intensity, and limited at the extremes by black and white. However, when an object transmits or reflects to an unequal extent the wave-lengths of the incident energy it is visually selective and is colored. Every known substance absorbs to some extent radiation of some wave-length and nearly all absorb very strongly at some particular wave-length or spectral region. The colors of opaque objects depend upon the ratio of reflecting to absorbing power for each wave-length.

A saturated color, that is a pure hue, reflects or transmits a very narrow region of the spectrum, *i.e.*, absorbs most of the spectrum and is, practically speaking, monochromatic. Such media are rare in nature and practice, as most objects we meet are far from being monochromatic. The spectrophotometer is used to determine the amount of transmission or reflection at each wave-length. Fig. 2 shows the curve of a green filter with wave-lengths as abscissæ and density as ordinates in one curve and transmission as ordinates in the other. This type of curve is rather typical of ordinary colored things with the maximum transmission in a certain spectral region and the somewhat gradual sloping to zero transmission on either side. A density of 1 allows ten per cent. of the incident light to pass so the corresponding transmission is 0.1, a density of 2 transmits 1 per cent. and so on. Density is the logarithm of the reciprocal of the transmission, *i.e.*, $D = \log 1/T$. If two filters are taken together their combined density is the sum of the separate densities and their

[1] Nutting, *op. cit.*, Chapter VIII. Also annual reports of Committee on Progress in Trans. Illum. Eng. Soc.

transmission is the product of the separate transmissions. The physical law of absorption stated that the absorption at any wave-length is an exponential function of the thickness of the absorbing media or in other words if a unit thickness transmits a fraction T, absorbs $(1 - T)$, then the next unit thickness will transmit the same fraction of what remains and a thickness X will transmit the fraction T^x. This law applies only to homogeneous media and monochromatic radiation.

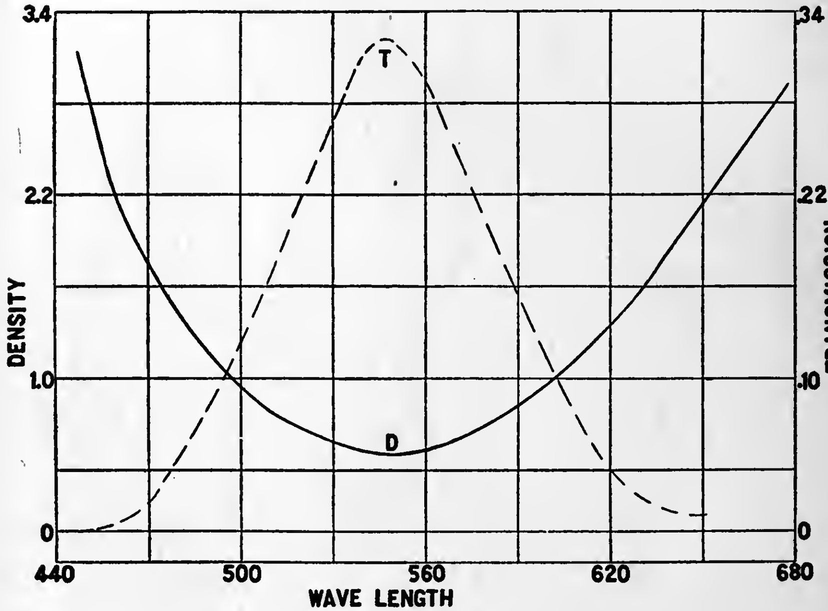

Fig. 2

A spectrophotometric curve gives the relative transmission of a filter but may be taken as indicative of color only when the composition of the incident light is known and specified as shown by the spectral energy curve. If we change the nature of the incident light through a filter, or on a reflecting surface, we also change the nature of the transmitted or reflected light. Another important thing to bear in mind is that equal energies do not produce equal brightnesses and this is illustrated in Fig. 3 which shows the so-called visibility curve for an average eye. This curve is determined by measuring the relative amount of energy necessary at each wave-length to cause equal sensations of brightness. If we

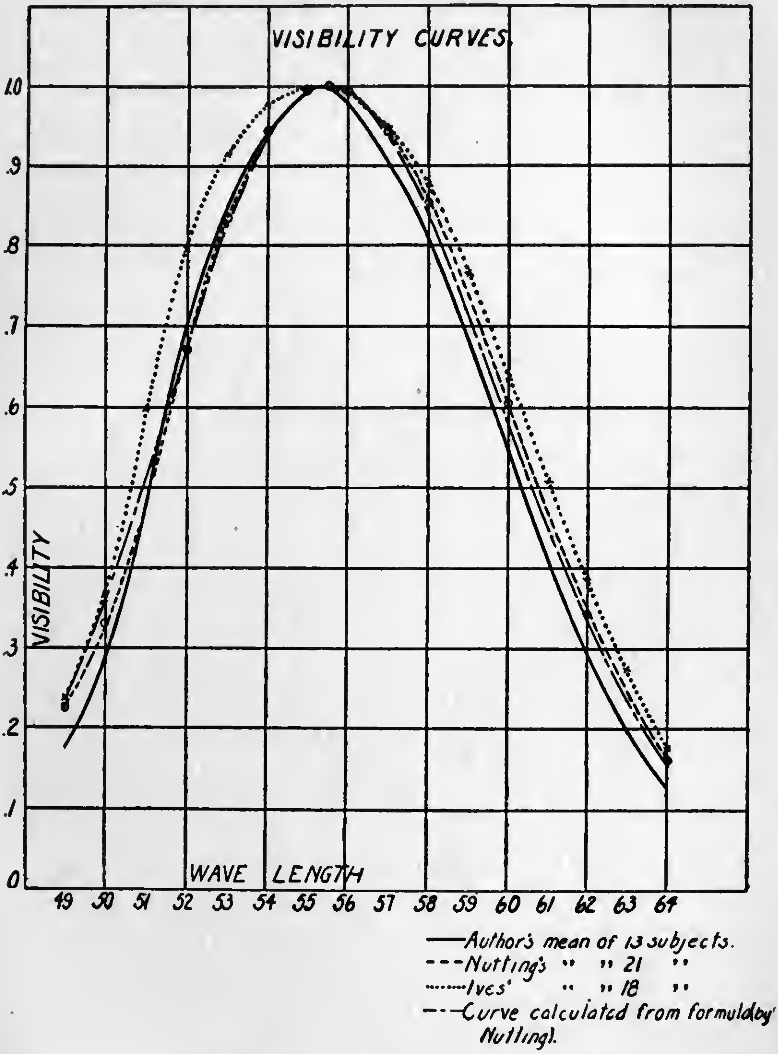

Fig. 3

take the maximum sensibility as unity (the point in the spectrum being 556 $\mu\mu$) the relative energy values for other wave-lengths in the visible spectrum are represented by the ordinates of the curve.[1] This shape and position of the maximum of the visibility curve varies with intensity of illumination as we might expect from dealing with peripheral or foveal vision. The product of spectral energy and visibility at each wave-length gives luminosity and it is really the luminosity curve that we consider when treating a visual stimulus.

Spectrophotometry gives the exact physical composition of the radiation in question, *i.e.*, analyzes the stimulus, but does not provide any direct specification of the subjective factors of the color sensation resulting when this radiation impinges upon the retina. This brings us then to the synthetic method by which the specification of the sensation is made directly in terms of the subjective factors, brightness and color. Brightness is dependent upon the intensity and color upon the quality of the incident radiation. These measurements are usually made by means of a colorimeter, the action of which is based on the fact that any color can be matched by a mixture in proper proportions of monochromatic light of the correct wave-length, with white light. The wave-length of the monochromatic light used is termed the "wave-length of the dominant hue" and constitutes the specification of the hue factor. The amount of white light necessary to make a match taken as a percentage of the total mixture is known as the per cent. white and represents the saturation factor. In case the color to be matched is a non-spectral color such as a purple or a magenta the complement of the color is found and the specification stated in terms of the complementary color. Brightness is measured by a suitable photometer which may or may not be an integral part of the colorimeter itself. In the case of reflecting surfaces the intensity factor is specified in terms of reflecting power, in case of transmitting media, by the total transmission and in case of emitting sources by the intensity of the source.

In the latest colorimeters it is possible to read hue to a fraction of a wave-length in the visible spectrum and a problem which arises from such a possibility is the determina-

[1] Reeves, P., 'The Visibility of Radiation,' *Trans. Illum. Eng. Soc.*, 1918, 13, 101. The most extensive work on this subject was published by Coblentz and Emerson. Sci. Paper 303, Bureau of Standards, issued September 12, 1917.

tion of a hue scale. If we desire to measure any quantity it is first necessary to have a unit of measurement which is constant throughout the entire scale. (For example, an inch is an inch, or a pint a pint wherever it may be taken.) When examining the visible spectrum, however, we find that equal wave-length intervals do not produce equal color sensation intervals at different parts of the spectrum, so the wave-length is not a satisfactory unit in the establishment of a scale of subjective color sensation and a hue scale established with fixed wave-length intervals as a unit is therefore unsatisfactory. The question as to whether or not the difference limen is equal at all points on any sensation scale has been discussed pro and con for years but from a physical standpoint it seems permissible to make these points equal by definition and call the least perceptible difference the sensation unit and use it as such in establishing sensation scales. So the experimental problem in establishing a fundamental hue scale is to determine the relation existing between the wave-length unit and the least perceptible difference in hue for the entire range of visible radiation. Two methods of procedure present themselves,[1] one method measuring the least perceptible difference at certain intervals, say every 5 or 10 $\mu\mu$, throughout the spectrum and the other progressing step by step and measuring each least perceptible difference in hue in the scale. Fig. 4 shows the results obtained from the latter method. The greater the least perceptible difference the less the sensibility, so the sensibility can be taken as proportional to the reciprocal of the least perceptible differences. Curve A represents the hue sensibility curve obtained from the reciprocal of the limens plotted against wave-length. Curve *B* is the scale reading curve obtained by integrating the sensibility curve. Or by taking least perceptible differences as units the hue scale may be determined by direct measurement and the sensibility curve obtained by differentiating the scale reading curve. Examination of these curves shows maxima and minima at different points in the spectrum. These results agree very well with previous results obtained,[2]

[1] Jones, L. A., *J. Opt. Soc.*, 1917, 1, 63.

[2] Steindler, C., *Sitzungsber. Wien. Acad. Wiss.*, 1906, 115, 1.

though, of course, the final values must be the average results from a large number of observers with normal vision.

For an exact determination of the hue scale, spectral light must be used, as the development of transmitting media or reflecting surfaces up to the present time has not furnished either material with monochromatic properties. So an apparatus which will illuminate independently the two parts of a photometric field with monochromatic light of variable

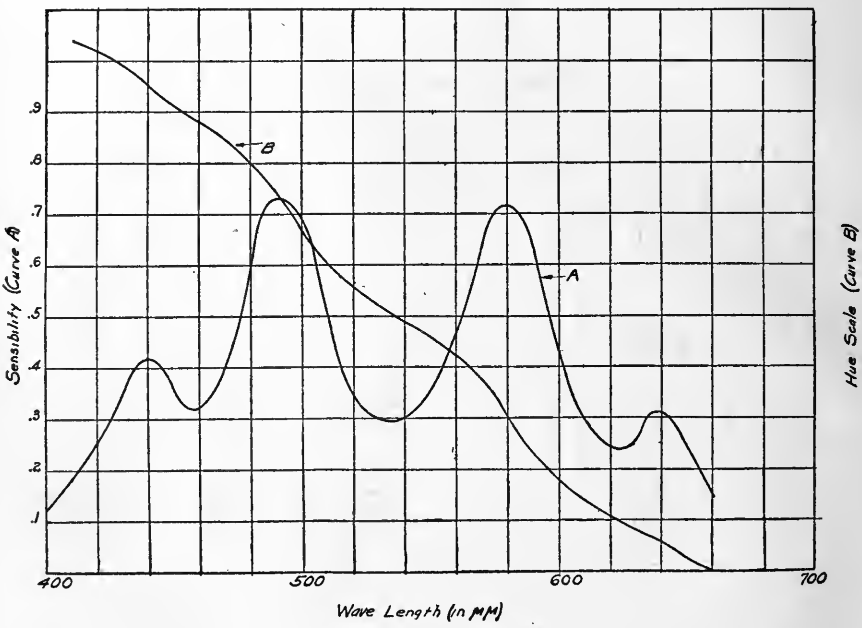

FIG. 4

quality and intensity must be used. It is quite essential that the wave-length in each part be easily varied and measured, and also important that the intensity be accurately controlled. In making a judgment of equality or difference of hue with two fields, it is important to have the intensity of those fields balanced so that an intensity difference does not influence the hue judgment. The most satisfactory apparatus used in our laboratory consisted of a Brace spectrophotometer used in connection with a Hilger spectroscope of the constant deviation type. The number of distinct hues between 400 $\mu\mu$ and 700 $\mu\mu$ was found to be 128 when the observer started at a given point and proceeded step by step through the visible spectrum. Several more steps (about twenty) are added if we examine the non-spectral purples and magentas. Hue sensibility is nearly independent of brightness, although it is found that the sensibility is somewhat higher for brightnesses of medium value than for those of extremely high and low values.

By mixing a pure hue with white and at the same time maintaining a constant brightness, a series of colors will be

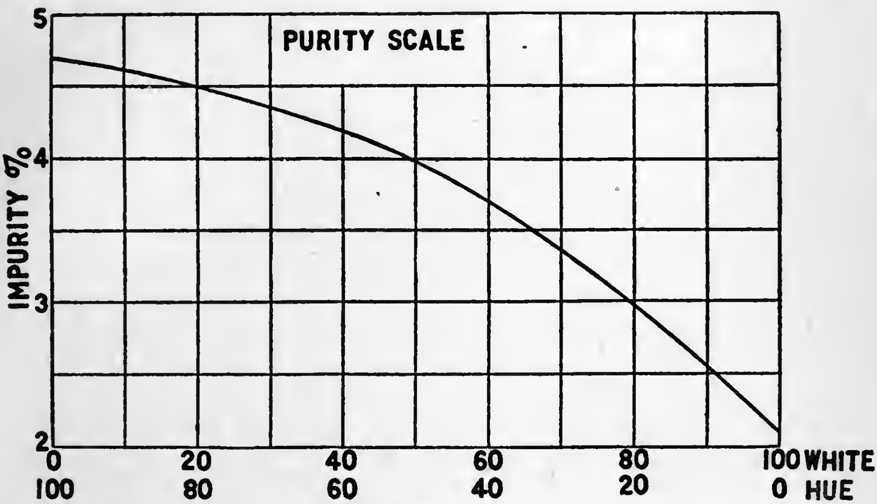

FIG. 5

obtained differing only in saturation. A specification of saturation, therefore, denotes the proximity of the color to a condition of monochromatism. Various terms such as purity, chroma, and tint are used, but the writers consider either saturation or purity the most suitable. If we start with a pure hue (monochromatic spectral light may be considered, for purposes of measurement, as having a saturation of 100 per cent.) and proceed to a saturation of zero, we find about twenty steps. Fig. 5 shows the purity scale with the per cent. white and the per cent. hue plotted as abscissæ and the least perceptible differences expressed as percentages, as ordinates.

At this point it might be well to review some of the factors involved in determining the number of visual sensations and compare the use of spectral light and physical controls with the ordinary method of mixtures on a color wheel. In either method the number of perceptible differences we find in a hue scale or purity scale will depend on the brightness region we choose for the hue scale and the initial saturation for the purity scale. In a suitable apparatus using spectral light we are able to select very narrow regions of the spectrum and know accurately the region selected from a direct reading scale. Spectral colors are saturated. When mixing white light ("white" in this usage means any gray in color mixing) we can get a very wide range, from the full intensity of the light source by removing the Nicol prisms to total extinction when the elements are crossed. Here again the physical specifications are easily obtained from a scale reading. For the purposes of comparison it is rather easy to obtain two identical sources and, furthermore, results obtained in one laboratory will be directly comparable with those obtained in any other laboratory. With the color wheel, however, we first meet the difficulty of obtaining monochromatic discs and discs approaching a physical white or black. Then we do not know the physical specifications of the mixtures obtained from the various proportions of the discs used. Discs from the same order may differ from one another and many of them are unstable even in darkness. These variable factors not only preclude intercomparisons but also make separate results doubtful except for demonstrations. In any case where discs are used it would be advisable to include in published data the physical specifications of the discs used.

Many text books in psychology make the statement that "yellow is distinctly lighter than green; violet is darker than the other spectral hues," and consequently picture the spectral belt in the color pyramid or color spindle as being tilted upward at the yellow region and downward at the violet. This type of statement is true only for certain spectra. The prismatic spectrum of sunlight will certainly bear out these statements but any number of other spectra show a different set of conditions and it is possible to get equally bright yellows and violets. We also find statements that violet and blue are the most saturated colors. From a physical standpoint it is possible to have any color 100 per cent. saturated and while it is true that 100 per cent. yellow is quite similar to white, so, too, other saturated colors seem to approach white when the intensity of illumination is increased. In the spectra ordinarily used the dispersion and luminosity factors easily lead one to the statements mentioned above but much more work must be done with spectral regions of equal saturations, equal brightnesses and equal luminosities before we can be positive as to the form and position of the spectral belt of the color pyramid.

The Nutting colorimeter[1] is one of the most satisfactory monochromatic analyzers, and is especially adapted to the determination of the dominant hue and per cent. white of reflecting surfaces or transmitting media. The dominant hue is read from a direct reading wave-length drum on a screw which operates a constant deviation dispersing prism. This prism controls the quality of the standard, the intensity of which is controlled by a pair of Nicol prisms and the purity by mixing white light from another source.

Another form of colorimeter is the trichromatic analyzer which is partly analytical and partly synthetic in nature. This method specifies a color by giving the relative intensities of some arbitrary red, green, and blue which when mixed together match the unknown. The red, green, and blue are obtained by the use of filters of glass, stained gelatine or other suitably colored material or by the isolation of narrow bands of the spectrum. A representative of this type of instrument is the Ives colorimeter.[2] Many other so-called colorimeters are in reality only color comparitors or tintometers and in many cases are based on arbitrarily chosen standards.

Although this paper has been a hurried review of some of the facts on color, it is hoped that the importance of accurate control of the stimulus in color experimentation has been emphasized. If results are to be duplicated in various laboratories and colored stimuli standardized some of the aforementioned facts must be observed.

[1] *Bull. Bur. Stand.*, 1913, 9, 1; *Zsch. f. Instkund.*, 1913, 33, 20.

[2] *J. Frank. Inst.*, 1907, 164, 421. *Ibid.*, 1915, 180, 673.

SUGGESTIONS LOOKING TOWARD A FUNDAMENTAL REVISION OF CURRENT STATISTICAL PROCEDURE, AS APPLIED TO TESTS

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The past three or four years have been notable in psychological history for the remarkable development of statistical methods as applied to the problems of mental measurement. This advance is undoubtedly of the very greatest importance. The writer has come to feel, however, that, with the first enthusiasm in such work, there has been a tendency toward over-elaborateness and diffuseness of treatment, and a lack of directness and incisiveness in the statistical procedure. And he wishes to point out certain limitations to the present concepts of "reliability" and "validity" as applied to tests, and certain objections to the customary use of the theory of the normal curve in test building, which he feels to be of distinct importance.

The situation can most readily be made clear by a very concrete example. Suppose, then, that a high-school principal desires to give a group test for measuring general intelligence to his entering class, in order to pick out in advance those who are likely to fail in their freshman work. He has a number of scales under consideration. And he wishes evidence as to the relative merits of these scales for this purpose,—for the selection of potential failures. He will very likely be given data with regard to the comparative 'reliability' and 'validity' of these scales; information may also be produced with regard to the organization of the tests, especially in respect to the normality of the distribution of scores yielded. The present paper aims to show that no one of these three sets of facts gives that close contact, which is desirable, with the practical problem.

I. Inadequacy of the Present Concept of 'Reliability'

The principal may be urged to use a particular scale because the scale has a high 'reliability.' The exact meaning of 'reliability' must, however, first be carefully looked into. The meaning of the concept can best be understood by considering the way in which 'reliability' is usually measured. The most common method is simply to give two duplicate forms of the same test, one after the other, to the same subjects. The ratings obtained by the subjects on 'Form A' and 'Form B' are then correlated. And the closeness of the correlation indicates the reliability of the test.

The significance, and the limitations, of a measure thus obtained are fairly obvious. Two such limitations are especially important. (*a*) The measure is evidently a measure of the reliability of the sampling,—of the particular type of performance involved in the test. When one speaks of the reliability of an instrument, one naturally thinks of its reliability *for some purpose*. Such a connotation must be guarded against here. One must not come insensibly to think of the reliability coefficient of a test of intelligence, for instance, as indicating the value of the test, as a measure of intelligence. Such a conclusion is sound only if a test is a simple sampling of the ability which it is sought to measure; and this happens much more rarely than might be supposed.[1] The term 'consistency' would, therefore, seem a more accurate term; the 'reliability' coefficient indicates only the extent to which a test is consistent with itself. And it is entirely possible that a test should yield highly consistent results which were, nevertheless, not at all measures of the function which it was desired to measure.[2]

[1] It might seem, for instance, that the Courtis Scale B was a simple sampling of ability in the fundamentals. But recent research has shown the situation to be by no means so simple. (See Thorndike, E. L., and Courtis, S. A., 'Correction Formulæ for Addition Tests,' *Teachers' College Record*, 1920, 21, 1–24.)

[2] Thus, not so many years ago, cancellation tests were frequently included in 'batteries' of tests intended for the measurement of mental endowment. (See, for instance, Pyle, 'The Examination of School Children,' Macmillan, 1913.) It now seems quite clear that cancellation tests are not good tests of intelligence. (See McCall, 'Some Correlations between Mental Traits,' Teachers' College, 1916.) But cancellation tests appear to be quite 'reliable' measures,—they are simply not good tests of general intelligence. They are, therefore, not 'reliable' for the purpose for which Pyle used them.

It must also be kept in mind (*b*) that such a measure of the reliability of the sampling may be considered an adequate measure for this purpose only if the scores obtained on 'Form A' and 'Form B' may be considered entirely random samplings of performance on such a test. Usually they cannot be so considered. There may be an initial difficulty with directions at the beginning of 'Form A' and a slight fatigue toward the last of 'Form B.' What is, with many of the tests, more important—the method as described above tells us nothing whatever about the 'consistency' of the results from one examiner to another, one scorer to another, from one day to another, or one time of the day to another.

To come back to the original problem, then: such a measure of the consistency of the test with itself, under certain circumstances, tells the high school principal surprisingly little as to the value which that test may have in distinguishing his potential failures from the rest of their class.[1] And information with regard to the 'validity' of the scale is naturally turned to, to settle this practical question.

II. The Artificial Nature of Current Concepts Regarding Validity

The principal is, then, urged to use a particular scale because the scale has a high 'validity' as a measure of general ability. That is, data are presented showing that the scale gives results having a high correlation with independent criteria as to general intelligence, and congruence with current theories regarding the nature of general intelligence,—there

[1] The writer is inclined to feel that most problems of consistency can best be dealt with in general terms. That is, what difference, in general, may one expect in test results if one tests Monday instead of Friday, at 9 o'clock instead of 3 o'clock? What difference, in general, may be expected, with a given type of directions, from one examiner to another? What differences, with various scoring methods, may be expected from one scorer to another? What differences may result in the score of an individual as the result of fluctuations from one time to another, in general feeling tone, energy, vigor, health? The writer believes that, until evidence to the contrary appears, it may be taken for granted that such factors affect all tests in more or less the same way; certain general theorems with regard to their operation should, then, be possible—or general precautions taken. The only problem of consistency that needs specific determination for each test would then be consistency as it relates to the subject matter of the test.

is a regular rise in score from year to year until maturity, a relative freedom from the influence of specific training, and so on. This concept of 'validity' is also, the writer feels, beside the point, if not misleading, so far as the practical problem of the high-school principal is concerned. And again there are two difficulties.

In the first place (*a*), since the extent to which general intelligence is the fundamental factor, in conditioning success and failure in the Freshman year of high school, is not known, the usefulness of the scale (even if proven a satisfactory measure of general intelligence) is still an unknown quantity. Stability of character, willingness to apply oneself even though the restraints of grammar school supervision are now removed, interest in the more mature subjects of the high-school curriculum—such elements are probably more important than is often supposed, in the total situation.[1] Differences in the adequacy of previous preparation may also be of importance. So proof of the 'validity' of a scale as a measure of 'general intelligence' is by no means proof of the value of the scale in sorting out potential failures among these high-school freshmen. In fact, it might almost be said that in proportion as the scale measured one element only, in a complex situation, to just that extent was the scale inadequate for dealing with that total situation![2]

It remains to be pointed out, however, that even though

[1] For a discussion of this tendency to overestimate the comparative importance of intelligence see Rosenow, Curt, 'Is Lack of Intelligence the Chief Cause of Delinquency?' Psychol. Rev., March, 1920.

[2] The more extreme theories in regard to general intelligence surely make up, in the aggregate, an extraordinary concept. It should surely be kept in mind that it is, in the first place, an analytical concept and so dependent for its character upon the methods of analysis employed. It should also be pointed out that such a concept naturally receives successive accretions in the way of theory and may, by a mental synthesis largely adventitious to the facts, acquire a reality which is very largely an artifact. Scores on various tests are lumped and the aggregate used as a measure of general ability. A more or less close relationship is naturally found between such an aggregate and the average marks of the children in school. Teachers, especially in the grades, naturally think of the child's work as a whole, and give marks showing high correlations between abilities in different subjects. And the children come to this attitude and react to their school work as a whole. And—the whole situation is cumulative. One might, in fact, imagine the concept of general ability thus developing even though abilties were, as a matter of fact, diverse and uncorrelated.

the demonstration of a close correlation between the scale in question and a general intelligence *were* supplemented by evidence that general intelligence was the fundamental factor in the situation, still the suitableness of the scale for the particular problem would remain to be shown. That is, (*b*) the usual method for stating relationships between two variables—the correlation coefficient—does not express satisfactorily the nature of that relationship, for diagnostic purposes, at a particular point in the distribution. The problem is: How unmistakably will the scale set off the lower 15 per cent. or so in scholastic ability? A correlation coefficient is only very general evidence in regard to this particular matter.[1] And it is evidence with regard to such diagnostic efficiency that the school principal should require.

III. The Irrelevancy of the Theory of the Normal Curve in Practical Problems in Classification

Proof of the validity of a scale as a measure of general intelligence is, then, not proof of the value of that scale for sorting out potential high-school failures, since failure is not conditioned by general ability alone, and since the diagnostic efficiency of a measuring instrument is not the same thing as the general relationship of that instrument to the factor concerned. A third set of facts may, nevertheless be introduced in evidence of the value of the scale in question. It may be pointed out that the tests of the scale are very carefully constructed so that equal increments on the scale represent equal increments in ability, and so that the total distribution of abilities yielded is closely similar to the distribution of abilities that would be expected according to

[1] See, for instance, Thurstone, L. L., 'Mental Tests for College Entrance,' *J. of Educ. Psychol.*, March, 1919 and Pressey, S. L., 'Suggestions with Regard to Prof. Thurstone's "Method of Critical Scores,"' *J. of Educ. Psychol.*, December, 1919.

The writer has often wondered whether the early introduction of the Pearson products-moments formula for calculating the correlation coefficient has not hindered rather than helped the study of relationships, in psychology. There are, of course, no right and wrong methods; methods are simply more or less adequate to the data and the problem in hand. One could almost say, dogmatically, that the particular type of data and problem to which the Pearson method is applicable were relatively rare. Most practical problems require a two or threefold division.

the theory of the 'normal curve.' Once more the writer would object to the relevancy of the information to the practical problem, and on two counts.

(*a*) Construction of the scale so that equal increments of ability are related to equal increments in score means, probably, transmutation of values in terms of the per cent. passing different items into positions on the normal curve or some such procedure.[1] It need only be said here that items which give a satisfactory scaling on such a curve need by no means be the most diagnostic items. An item may appear in a test because it is the only item appearing at 1.52 P.E. (when scaled as mentioned above) or it may appear in a test because most of the potential failures cannot pass it and most potential successes can. The last criterion is obviously the fundamental one if the problem in hand is to obtain a test that shall most completely differentiate the potential failures.

(*b*) It may also be pointed out shortly that for the particular practical problem under consideration a normal distribution of scores is hardly to be desired. If a scale sets off the potential failures very completely, it will lump the assured failures at the bottom and the assured successes at the top, and spread out the questionable cases in between. In short, equal increments of ability and a normal distribution of scores are *not* to be desired if the greatest efficiency, for the practical problem postulated, is sought.

IV. Discussion

Well—most of these points seem obvious enough, perhaps. But the concept back of them indicates a fundamentally different statistical attack, in the development and use of tests. If differentiation of the potential failures in high school is an important problem, why not build a scale specifically for that purpose? Select items simply according to their ability to make the desired division. Combine those items so that such a lumping of cases at the two extremes is obtained; the reverse of the normal distribution is the distribu-

[1] Of which procedures, transmutation of percents passing at different chronological ages into supposed units of mental growth is surely more questionable still.

tion to be desired.[1] Then measure the value of the test by measuring its 'efficiency' in dealing with the practical problem for which the scale has been designed. Deal with each important problem in some such empirical and concrete fashion. And, if, out of a large number of such attempts, there emerge certain unitary factors,—a general ability, a series of character types, or what not,—well and good. But the postulation of such elements in advance, with verification primarily by reference back to these postulates, is both an unscientific and a practically dangerous proceeding.

First a very specific problem; then, after that—*everything subservient to the solution of it!* Every item chosen with reference to that one problem, every method aiming only at the most direct and empirical solution of that problem—no hypotheses, as thoroughly empirical treatment as may be! The result will be, the writer believes, an essentially new statistical approach (methods now in use suggest something of this sort, particularly the methods used in the development of the army trade tests). Such a revision of methods is, the writer has come to feel, necessary, for a clarifying of the total situation.[2]

[1] Is this not really the solution of the problem of the normal curve in mental measurement? (See, for instance, Boring, *Amer. J. of Psychol.*, January, 1920). The actual distribution of various traits is a matter of academic interest only. But meantime, the distribution to be sought in test work will be determined by the problem.

[2] And now the apology! There is little essentially new in the paper, of course. (In fact, it should be said that a detailed discussion, with full use of the literature, was first attempted, but was found to extend beyond reasonable limits.) The important thing, however, is the total implication of the various points presented. Our statistical methods as applied to tests have been largely borrowed methods,—and methods borrowed from the descriptive sciences. So the question has been: What is the test measuring, and how accurately is this thing being measured? But mental testing is not a descriptive, but a technical science. And the question should be, instead: What are we trying to do, and how well are we doing it? The distinction is, the writer believes, of the very most fundamental importance, involving fundamental differences in statistical approach.

It remains to be mentioned that the points made apply equally to measures of achievement in the school subjects or other like tests. Instead of measuring "ability in arithmetic" in the eighth grade—and then commenting mildly on the extent to which arithmetical ability in the eighth grade overlaps on the seventh, why not tackle a definite practical problem,—attempt to define the passing point in arithmetic for the eighth grade? The distribution, again, should be bi-modal, not normal,—and the other points mentioned follow.