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A NEW METHOD OF HETEROCHROMATIC PHOTOMETRY

BY C. E. FERREE AND GERTRUDE RAND

Bryn Mawr College

In a former paper[1] it was stated that a satisfactory method of photometry should combine the following features. (1) It should enable us to detect small differences in luminosity and to reproduce our results for a given observer with a small mean variation and for a number of observers with a comparatively small mean variation. That is, the method should possess an adequate degree of sensitivity, and should give results with a satisfactory degree of reproducibility.[2] (2) It should be known either to possess of itself logical sureness of principle or its results must agree in the average with those of some method which can be shown to possess this sureness of principle. Methods having these features have been developed for the photometry of colorless light. The problem for the photometry of colored light, however, has presented great difficulty.

The methods for the photometry of colored light may be grouped under two headings: the direct and the indirect methods. In the former class we have the method of direct

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[1] Ferree, C. E. and Rand, Gertrude, 'A Preliminary Study of the Deficiencies of the Method of Flicker for the Photometry of Lights of Different Color,' *Psychol. Rev.*, 1915, 22, pp. 110–162.

[2] It is scarcely needful to point out in this connection that the method should give results with a satisfactory degree of reproducibility for observations separated both by short and by long intervals of time.

comparison or as it is sometimes called the equality of brightness method. This method is generally accredited with sureness of principle but it is deficient on the side of sensitivity and reproducibility when lights differing in color value are compared.[1] Of the latter class the method of flicker has received the greatest amount of attention and has been the most favored. But in our former paper it was shown (1) that the method of flicker, so far as it has been developed up to the present time, does not possess of itself the sureness of principle needed to meet the requirements of a satisfactory method; and (2) that as yet its results have not been found to agree on the average with those of any method which can be shown to have this sureness of principle. It was also stated that in a later paper a method of photometry would be described which possesses approximately as high a degree of sensitivity and of reproducibility as the method of flicker and gives results which agree very closely on the average with those obtained by the equality of brightness method, much more closely than results obtained by the method of flicker. It will be the purpose of this paper to give a brief description of this method. It was first used by the writers in connection with work in color sensitivity for the purpose of detecting small changes on the illumination of a room by daylight.[2] Although not so convenient perhaps as the equality of brightness method for many of the purposes for which photometry may be used, for this work the method did not present any great amount of difficulty, and it proved to be much more sensitive than the equality of brightness method. The equality of brightness photometers of the Sharp-Millar type, for example, are very insensitive for the determination of the illumination of a room by daylight, because the standard field

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[1] *Loc. cit.*, pp. 114–115.

[2] See Ferree, C. E. and Rand, G., 'An Optics-Room and a Method of Standardizing Its Illumination,' *Psychol. Rev.*, 1912, 19, pp. 364–373; Rand, G., 'The Effect of Changes in the General Illumination of the Retina upon Its Sensitivity to Color,' *ibid.*, pp. 463–490; 'The Factors that Influence the Sensitivity of the Retina to Color: A Quantitative Study and Methods of Standardizing,' *Psychol. Rev. Monog.*, 1913, 15, No. 62, pp. 79–166; see also Ferree, C. E., 'Tests for the Efficiency of the Eye Under Different Systems of Illumination and a Preliminary Study of the Causes of Discomfort,' *Trans. Ill. Eng. Soc.*, 1913, 8, p. 51.

illuminated by the tungsten lamp is deep orange in color, while the comparison field illuminated by daylight is clear white. This difference in color tone makes the judgment of equality of brightness difficult to make, and renders the instrument extremely insensitive for daylight work.

So far as the type of judgment is concerned, the method we are about to describe is in reality an equality of brightness method but it has the important advantage that the observer is never required to judge impressions differing in color quality. In making a photometric balance by this method, the feature is thus eliminated which has from the beginning of the work of measuring light intensities rendered the equality of brightness method difficult of application to heterochromatic photometry. Because of this factor we have at the suggestion of certain photometrists undertaken to work out the principle on which the method is based in a way that will be of general service to the work in heterochromatic photometry and to secure data on the points in terms of which a verdict is rendered for every method of photometry, namely, the sensitivity and precision of the method, and the agreement of its results with those obtained by the equality of brightness method.

The method is based upon the extreme sensitivity of the peripheral retina to brightness contrast, especially to the induction of black by a white screen. Not only is the peripheral retina extremely sensitive to brightness contrast, but, which is the crucial point of the method, very small changes in the illumination of the contrast screen produce a change in the amount of the contrast. That is, for any part of the retina, for example, the amount of contrast induced by the surrounding field upon a contrast stimulus increases with decrease of illumination. In the peripheral retina we have an extreme case of this. In this part of the retina very small changes of illumination indeed are required to produce a noticeable change in the amount of contrast induced, especially when the surrounding field is white and the stimulus is a gray of the proper brightness.

The apparatus needed for the method consists of a vertical screen 60 × 50 cm. with an opening 15 mm. in diameter midway between top and bottom and 16 cm. from one edge, a motor to carry the measuring discs, and a photometer bar. The surface of the screen is covered with Hering mat white paper, or preferably with a paper overlaid with magnesium oxide deposited from the burning metal. This screen is graduated in the horizontal meridian from the center of the 15 mm. opening. Over the opening between the white covering and the screen is placed the gray which is to serve as the contrast-stimulus. Nos. 3–5 of the Hering series of standard gray papers were found to serve this purpose best for the intensities of light we used in the work the results of which are given in Table I. The measuring discs were made up of a sector of the same gray as was used for the contrast-stimulus and a sector of black, the varying proportions of which produce the match which is needed between the stimulus gray darkened by contrast and the measuring disc. The measuring disc was carried by a motor the shank of which is at one end of the screen and normal to it. When in position, the inner edge of the disc extends approximately to the 20° point of the graduations, and is about 3 cm. in front of the screen.[1] Immediately in front of the disc and 25° from the center of the stimulus-opening to the temporal side is placed the fixation-point.[2] The photometer bar was placed in a

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[1] The measuring disc was placed this distance in front of the screen partly to give freedom of rotation but primarily to eliminate any induction by the screen on the disc. That induction is not present in any considerable amount under the conditions described above, is evident from the following considerations. (*a*) It is not directly detectable by the eye; and (*b*) a very high sensitivity is attainable by the method which could not be the case were induction present to any degree, for if it occurred in amounts great enough to influence sensation, the effect would obviously be to lower the sensitivity of the method. That there has been no lowering of sensitivity from this cause is rendered probable by the fact that approximately as small differences can be detected for lights differing in color quality as can be detected by Lummer-Brodhun photometers of the most improved type when the lights compared are of the same color quality.

[2] Care should be taken not to place the fixation-point so far from the stimulus-opening that the color used is seen of a different quality at this point than at the stimulus-opening. If this should occur for any color when the fixation is at the 25° point, a lesser excentricity should be chosen. The sensitivity of the contrast-effect to change of illumination is not lessened to any considerable extent, for example, by taking a 20° instead of a 25° fixation-point. We have found the 25° point to be very

plane normal to the screen midway between the stimulus-opening and the fixation-point. The contrast-stimulus and the measuring disc thus receive equal amounts of light from the source to be measured.

In Fig. 1 is given a picture of the apparatus in the rough form in which it is at present used. At *S* is shown the screen which is illuminated in turn by the standard and comparison lights; at *B* the photometer bar; at *D* the measuring disc; at *L* the lamp to be photometered; at *M* the mouthboard; and at *O* the opening filled with gray which is darkened in graded amounts by induction from the screen *S* as the amount of light received by it from the lamp *L* is varied. The screen *S* is supported by short rods screwed into heavy tripod bases. In order to provide for adjusting the height of the screen above the table the frame of the screen is fastened to the supporting rods by means of a collar and set screw. On the upper edge of this frame is attached a device for lining up the eye with the stimulus-opening *O*. This device consists of the following parts: A vertical arm carrying at its lower end a small circle 15 mm. in diameter extends

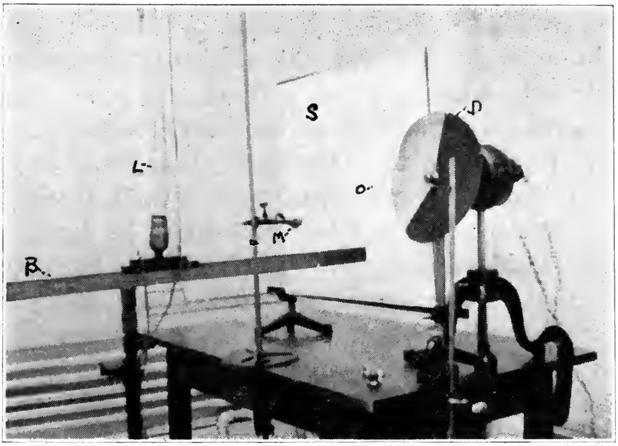

FIG. 1

satisfactory, however. For a fixation-point we used a black knot on a gray thread, stretched from a point on the screen *S* to a rod in front and on the opposite side of the measuring disc.

down to the level of the stimulus-opening *O*. Attached to the vertical arm just above this circle is a horizontal arm 15 cm. long which carries at its outer end and at right angles to it a disc 22.3 mm. in diameter. The size and position of the disc, the circle, and the opening *O* and their distances from each other sustain such relations that when the eye is in position 25 cm. in front of the opening *O*, the line of regard contains the centers of the opening *O*, of the circle, and of the disc; and the inner edge of the circle is just contained within the stimulus-opening; and the edge of the disc is just contained within the circle. That is, in effect the device is a peep-sight arrangement, and the alignment described above is possible only when the eye is in such a position that the line of regard is perpendicular to the stimulus-opening, the circle, and the disc at their centers. The device is attached to the metal frame by means of a screw with a milled head, so that after the alignment is made it can be readily turned out of the road and clamped. In order that the distance of the eye may be accurately adjusted at the same time its alignment is made with the stimulus-opening, a measuring device is also provided. This device consists of a slender brass rod 25 cm. long fitted at either end with two short right-angled arms 5 mm. in length. On the end of one of these arms is a ring which is just larger than the stimulus-opening, and on the other is a brass disc of the same diameter as the ring, provided at its center with a large pupillary aperture. In adjusting the distance of the eye this disc is rested lightly against the forward surface of the eyeball and the ring against the screen *S* concentric to the stimulus-opening. When the position of the eye is once determined, a mouthboard is adjusted and clamped in position so that the observer's teeth fit into impressions previously made and hardened in wax. This fixes the relation of the observer's eye to the stimulus-opening *O*. All that is needed, therefore, at subsequent sittings to bring the eye again into this relation, is to fit the teeth into the impressions on the mouthboard. As stated above, the photometer bar when adjusted stands in a plane normal to the screen *S* and midway between the stimulus-opening and the fixation-point. This guarantees that the stimulus-opening and the measuring-disc at the fixation-point shall receive equal amounts of light from the lamp on the bar. The nearer end of the bar is placed at a distance below the level of the center of the stimulus-opening and fixation-point equal to the height of the center of the filament of the lamp above the bar. Owing to the fact that the head may cast a shadow on the stimulus-opening when the bar is horizontal, the rear end of the bar is raised until the stimulus-opening and the screen for some distance around are free from shadow for any position of the lamp on the bar that may be needed to obtain the photometric balance. In order to provide for this tilting, the bar is made adjustable in height. It is supported on two heavy tripod bases each carrying a vertical stem made of hollow tubing split at the upper end and provided with collar and set screw. Into these tubes fit rods attached to the photometer bar by means of hinge joints. This arrangement permits of an independent adjustment of the height of the two ends of the bar. In order that a given amount of slant may be reproduced at any subsequent observation, an attachment is provided to indicate the angle which the bar makes with the vertical. This consists of a graduated half circle attached just above the hinge joint to the supporting rod furthest from the screen. On this rod just below the hinge joint is fastened a pointer. When the further end of the rod is raised this half circle is rotated past the pointer and the amount of rotation may be directly read from its graduated limb. Since the angle at which the bar is tilted is kept the same for the comparison and standard lights, the tilting interferes in no way with the accuracy of the photometric balance or the application of the law of squares to the determination of the relative luminosities of the standard and comparison lights.[1]

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[1] We hope soon to use the principle upon which the method is based in connection with apparatus which will be more conveniently applicable to the general needs of the photometrist than is the rough device which is described above. One of the improvements which we expect to make, for example, is to eliminate the need of a slanting bar. For while the slanting bar does not interfere with the establishment of a correct photometric balance at the screen, it is not the most favorable condition under

The method is applied as follows: The observer's eye is lined up with the stimulus-opening in the manner described above and the contrast stimulus is put in position. The standard lamp is placed on the photometer bar at a given distance from the screen, the eye is turned out to the fixation-point and the sectors on the measuring disc situated just back of the fixation-point are adjusted by means of a number of separate judgments to match the gray at the stimulus-opening darkened by contrast.[1] The relative value of these sectors becomes the index of the illumination of the screen for the standard lamp at the given distance. The standard lamp is then removed, the comparison lamp is placed on the bar, and its position is adjusted

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which to operate all types of lamps. This should not be difficult to do by moving the observer farther from the screen and using a correspondingly larger stimulus-opening and a greater linear distance from this opening to the fixation-point.

[1] That is, with the change in the amount of light falling on the photometer head made up of gray stimulus and surrounding screen of white, there is a change in the amount by which the gray stimulus is darkened by contrast from the surrounding screen. This amount is recorded on the measuring disc directly behind the fixation-point, by adding a sector of black to the gray sector, which was chosen, it will be remembered, of the brightness of the gray stimulus uninfluenced by contrast. The value of this black sector thus becomes the fixed index of the amount of light falling on the screen in any given case. In short, with a given brightness relation of contrast stimulus to surrounding screen, there is a finely graduated scale of contrast values inversely correlative with the amounts of light falling on the screen. That is, with a large amount of light or high illumination of the screen, the amount of contrast induced is low; with a small amount of light falling on the screen the amount of contrast induced is high. The amount of change of the illumination of the screen needed to produce a noticeable change in the amount of contrast induced is very small indeed, and because of this the method possesses a high degree of sensitivity. So far as we are able to determine, the amount of change in this illumination required to produce a just noticeable change in the contrast sensation is no greater than is needed to produce a just noticeable change in the positive sensation under the best conditions that have been as yet devised in photometric work for making the judgment, namely, the conditions presented by the Lummer-Brodhun photometer heads of the most improved type when the lights compared are the same in color quality. If this were not true, the method could not show, as it does, as great sensitivity for lights either the same or differing in color quality as does the equality of brightness method for lights of the same color quality. Also so far as we have been able to determine there is no greater variability in the eye's sensitivity to contrast from day to day than there is in its sensitivity to brightness itself. That is, photometric determinations by this method of a given light density on the screen show a very small mean variation from day to day, quite comparable in fact, with the mean variation found in judgments of brightness equality, when there is no difference in color quality to confuse the judgment. There is, moreover, so far as we have applied the method, a comparatively small variation for different observers.

until the contrast-stimulus again just matches the measuring disc. Since the proportion of sectors has not been changed from that which was determined for the standard lamp, it can be assumed that the illumination of the contrast screen is again of the same value it was for the standard lamp at the given distance. The comparative luminosities of the two light sources, thus, can be computed directly by the law of squares from distance readings on the photometer bar. The advantage of the method for color photometry is, as has already been pointed out, that in the judgment in case of both the standard and comparison lights, the surfaces, the brightness values of which are being compared, are both illuminated by the same light. The eye is thus never compelled to make its judgment of two surfaces differing in color quality. The method is in fact almost if not quite as sensitive for the

TABLE I

SHOWING A COMPARISON OF THE RESULTS OBTAINED BY THE CONTRAST AND EQUALITY OF BRIGHTNESS METHODS

Source of Colored Light[1]	Color	Distance of White Light Giving Equality of Illumination		Sensitivity or Amount of Change that Can be Detected by				Precision or the Mean Deviation from the Average			
		Equality of Brightness Method	New Method	Equality of Brightness Method		New Method		Equality of Brightness Method		New Method	
		Cm.	Cm.	Cm.	Percentage (Candle-power)	Cm.	Percentage (Candle-power)	Cm.	Percentage (Candle-power)	Cm.	Percentage (Candle-power)
87 cp. 41 cm. distant from photometric screen	Red	66.1	66.6	2.0	5.6	0.5	1.3	1.1	3.2	0.20	0.55
	Blue-green	59.5	59.5	2.5	7.85	0.5	1.65	1.5	4.85	0.22	0.97
52 cp. 38 cm. distant from photometric screen	Red	82.2	82.2	2.1	5.0	0.5	1.1	1.1	2.5	0.20	0.41
	Blue-green	70.9	70.5	4.0	10.4	0.5	1.4	2.0	5.5	0.25	1.1
13 cp. 38 cm. distant from photometric screen	Red	160.6	160	3.0	3.7	0.5	0.65	1.6	1.9	0.23	0.3
	Blue-green	134.5	134.9	4.0	5.7	0.5	0.76	2.2	3.2	0.24	0.46

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[1] The colored lights were obtained in these experiments by means of the Wratten and Wainwright filters inserted in openings in the front of lamp-houses. The red filter used transmits the spectrum from .768 μ to .65 μ; and the blue-green from .52 μ to .465 μ.

photometry of lights differing in color quality as it is for the photometry of lights of the same color quality or for colorless light; and its sensitivity for lights of the same color quality or for colorless light compares very favorably, by test, for the same observer with photometers of the best Lummer-Brodhun type. The indirect vision judgment, it may be said, however, presents more difficulty to the unpractised observer than is afforded by the flicker judgment, but this difficulty yields rather readily to practice. The method has been in almost daily use in our laboratory for four years. It has, for example, been employed exclusively for the standardization of the illumination of our optics-room in the determination of color sensitivity.

Table I. has been prepared to show a sample of a comparison of the results of this method and the equality of brightness method. These results are typical. They report the average of twenty-five determinations. Sensitivity and the average deviation or precision are expressed both in divisions of the photometer bar and in percentage candle-power. The lamps used for this work were standardized by the New York Electrical Testing Laboratories. They were operated on a storage battery circuit.

With some modifications the contrast principle described above can be used for the determination in terms of pigment standards of the white-black value of colored papers for a given degree of illumination of the papers. The method of doing this can perhaps best be explained by comparing it with the method just described. In this method, it will be remembered, we determined how much contrast would be induced by one surface upon another of a lower coefficient of reflection when a given amount of light was incident upon the two surfaces, and used this amount of contrast as an index of the amount of light falling upon the two surfaces in establishing the photometric balance, or equality of illumination at the screen. Here the relation of coefficient of reflection of the screen to the stimulus is fixed and the amount of light falling on the screen from the comparison lamp is varied until the right amount is obtained to give the photometric balance with the standard lamp. In the determination of the white-black value of a colored paper under a given illumination, the situation is a little different. The amount of light falling on the screen is kept constant and the relation to the screen of the pigment standard which is to express the white-black value of the color, must be varied until the right one is obtained. Here again we find out how much contrast is induced by one surface upon another of a lower reflection coefficient, in this case the colored paper; and this amount of contrast is taken as an index of the brightness relation of the colored paper to the surface of higher reflecting power, the screen *S* in Fig. 1, for the given intensity of illumination. The problem thus becomes to determine what gray sustains the same brightness relation to the screen *S* as does the colored paper. This gray should represent the white-black value of the color.

In detail the method is as follows: The stimulus-opening *O* is filled with the colored paper for which the determination is to be made and a disc of the same color is made a part of the measuring disc. The fixation is taken and black is added to the color on the measuring disc until it is sufficiently reduced in brightness to match the stimulus color darkened by contrast. The amount of black that has to be added becomes the index of the amount of contrast induced and therefore of the brightness difference between the screen *S* and the colored paper for the degree of illumination used, and is kept constant during the remainder of the work. In order to be able to find out what gray differs as much in brightness from the screen as does the colored paper, a disc made up of variable proportions of white and black is substituted for the colored stimulus behind the screen; and a sector made up of variable proportions of the same white and black, for the colored sector on the measuring disc. The white and black are then varied in proportions which are carefully kept equal for stimulus and sector until a match is obtained. When this match is gotten the relative amounts of white and black either in the stimulus disc or the substitute sector on the measuring disc should give the gray value of the color for the illumination employed. That is, there is obviously only one gray that could be used in place of the contrast stimulus and in the substitute sector in the measuring disc which would satisfy the conditions of the experiment, and that is a gray having the luminosity or white-black value of the color originally used. It is scarcely necessary to caution that throughout the determination the light falling on the screen, the stimulus and the measuring disc must be kept constant. Care must be taken also in this case not to take the fixation so far from the stimulus-opening that a color difference is present between stimulus and measuring disc.

If the determinations are wanted in photometric units they can be gotten by the equality of brightness method from the grays which have been equated with the colors. A convenient instrument to use for this purpose is the Sharp-Millar portable photometer with its scale calibrated to give results in terms of candle-power per sq. in. The calibration can be readily made by the experimenter from a magnesium oxide surface with a known amount of light falling on it. If the light incident on the grays is daylight, the same difficulty will be experienced with the Sharp-Millar photometer, however, which was mentioned earlier in the paper, namely, the two halves of the photometer field will differ in color quality due to the fact that the carbon standard in this instrument gives a light rich in the long wave-lengths.

[1] In discussing the application of this method to the determination of the brightness of colored papers, reference should probably be given to the discussion of the Talbot-Plateau law in the paper preceding this. (See Ferree and Rand, 'A Preliminary Discussion of the Deficiencies of the Method of Flicker for the Photometry of Lights of Different Color,' *Psychol. Rev.*, 1915, 22, pp. 149–150.)

A PRELIMINARY STUDY OF TONAL VOLUME[1]

BY G. J. RICH, A.M.

It is a matter of common observation that high tones are, in general, small, sharp or pointed, while low tones are large, massive or voluminous. This fact is generally admitted by psychologists, though some, like Sully,[2] doubt whether the ear can distinguish 'degree of extensive magnitude.' But when we turn to the systematic treatment of tonal volume, we find marked disagreement. According to one view, it is merely a matter of association; low tones are associated to large instruments and to gross movements of the throat. This is the position of Stumpf[3] in his early writings. Ebbinghaus[4] and Wundt,[5] while they do not ascribe the extensiveness of tones to association, still regard it as only a metaphorical characterization of the sensations, and not as an attribute. Another view is that volume or extensity is an attribute of tonal sensations, distinct from pitch. This is the view held by Titchener,[6] Stumpf[7] (in his later works), James,[8] and Calkins.[9] Dunlap[10] admits an extensive attribute, but identifies it with pitch. Lehmann[11]

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[1] From the psychological laboratory of Cornell University.

[2] J. Sully, 'Outlines of Psychology,' 1889, 129.

[3] C. Stumpf, 'Ueber den psychologischen Ursprung der Raumvorstellung,' 1873, 298 ff. 'Tonpsychologie,' I, 1883, 207 ff.

[4] H. Ebbinghaus, 'Grundzüge d. Psy.,' I, 1902, 277 f., 423 f.; 1905, 294 f., 445 f.; 1911, 296 f., 458.

[5] W. Wundt, 'Physiol. Psy.,' II, 1910, 78.

[6] E. B. Titchener, 'Feeling and Attention,' 1908, 13 ff. 'A Text Book of Psychology,' 1911, 94 f.

[7] C. Stumpf, 'Tonpsychologie,' II., 1890, 56 ff., 535 ff. Ber. d. 6 Kongr. f. exper. Psychol., 1914, 300.

[8] W. James, *Mind*, XII., 1877, 1.

[9] M. W. Calkins, 'An Introduction to Psychology,' 1905, 54, 93. 'A First Book in Psychology,' 1912, 43.

[10] K. Dunlap, 'Extensity and Pitch,' *Psych. Rev.*, XII., 1905, 287 ff. 'A System of Psychology,' 1912, 215.

[11] A. Lehman, 'Folia Neuro-biologica,' IV., 1910, 119 f.

and Waetzmann[1] believe not only that extensity is intrinsic to tones, but also that it has a physiological equivalent in the area of the basilar membrane set in vibration by tonal stimuli.

The work here reported attempts to put the question to the test of experiment. The means used was the determination of the differential limen for judgments of volume at different points of the tonal scale. If volume is an attribute of tone, we may expect the judgments to be made with comparative ease and to be fairly consistent. If, on the other hand, it is due to association, we may expect large variations in the judgments. If pitch and volume are separate attributes we may expect, by the criterion of independent variability, that they have different limens, and that the course of the limens will be different. If they are two names of one and the same attribute, as Dunlap asserts, they will have the same limen.

Observers, Apparatus and Method.—The observers in the experiment were Mr. H. G. Bishop (B.), assistant in the department of psychology; Mr. F. L. Dimmick (D.), graduate student in psychology; and Mr. W. Feller (F.), undergraduate with no previous experience. B. and D. had had several years of training in psychological observation and in music; F. was entirely untrained in observation and was also unmusical. All of the observers knew in general what the problem was; they knew, that is, that it bore upon the determination of difference limens for volume; and B. and D. knew something of the theories of volume. F. was employed only in the preliminary part of the work.

The instruments used were four Stern variators and a piston whistle. They were blown from a compressed-air system which was kept at a pressure of 10.5 mm. of mercury. This pressure, which was of course too high for the apparatus, was reduced by a system of valves. It was practically constant during the experiment. The lowest variator (100–150 vs.) was blown at a pressure of 1.5 cm. of water (read while blowing); the next (150–300), at 2.2 cm.; the third

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[1] E. Waetzmann, 'Folia Neuro-biologica,' VI., 1912, 24 ff.

(300–600), at 3.3 cm.; the fourth (650–1,200), at 5.7 cm.; and the whistle at a pressure of 27.9 cm. of water. The whistle is fitted with micrometer adjustment of pipe-length, but not of mouth-width; this was, however, kept constant during the experiment. It gives tones above 1,000 vs. The variators were tuned by comparison with standard tuning forks; they were set accurately within one vibration. The whistle was calibrated by means of Marbe's *Sprachmelodie-apparat.* Both variators and whistle were standardized for 20° C. During the experimental work the room-temperature never deviated more than 4° from this value; the maximal temperature-error was therefore 0.8 per cent.

In the final series, the limens were found for standard tones of 110, 200, 400, 800, and 1,600 vs. by the method of constant stimuli. Series of seven comparison-stimuli were used, the differences being for the various standards 3, 5, 8, 15, and 35 respectively. Fifty series were taken with every standard, twenty-five in each time order. The order of the comparison-stimuli within the series was determined by chance; but it, as well as the order of the two time-relations, was kept the same for all standards. In giving the stimuli, E was guided by a pendulum beating forty single swings in the minute. He gave two 'ready' signals, one swing apart, and the first tone one swing (1.5 sec.) later. The second tone followed after two swings (3.0 sec.). The duration of each tone was approximately one half-swing of the pendulum.

The observers were instructed to give judgments of the 'size' or 'volume' of the two tones, irrespective of pitch or intensity. They were to judge the second tone in terms of the first, as larger (greater), same (equal), or smaller (less). They were not further instructed as regards a criterion of volume. They sat with back turned to the apparatus, and three to four feet distant from it. D. sat just inside a dark-room, as he reported that he could observe more easily in the dark.

Fifteen series were taken in an hour's work, five at each one of three standards. The order in which the various standards were used was so chosen that the number of series for all standards was approximately the same. The effect of practice was therefore sensibly the same for all five limens. No introspection was required during the series, but at the end of every hour's work the observers were asked for total introspections, and especially concerning their criteria of judgment.

At the very beginning of the experiment, a few practice-series were run through. B. and D. were able to judge volume immediately, and required only a single hour of practice; F. required some four hours before he could give consistent judgments. At first, he tended to confuse volume with intensity.

A preliminary determination was made of the upper limens at 100, 200, 400, and 800 vs. Fifty series were taken in every case; but the precautions regarding standardization of apparatus, etc., taken for the later work were not here observed. The results are given only for comparison. They are especially subject to errors due to unequal practice. Two sets of introspective series were also taken during this preliminary work. In addition, a very rough determination was made of the pitch limen. This merely served to show that the observers were not judging pitch; the limens were everywhere much smaller for pitch than for volume. Otherwise, the results are too rough to be of value.

TABLE I

PRELIMINARY RESULTS

Standard	Obs. B			Obs. D			Obs. F		
	D. L.	*h*	Rel. D. L.	D. L.	*h*	Rel. D. L.	D. L.	*h*	Rel. D. L.
100	7.711	.3130	.0771	6.279	.2371	.0628	3.501	.2216	.0350
200	11.705	.2203	.0585	8.012	.2421	.0401	5.765	.2027	.0283
400	18.040	.1080	.0451	12.770	.1156	.0319	9.084	.1316	.0227
800	41.163	.0730	.0514	28.583	.0676	.0356	23.288	.0887	.0292

The limens for volume were all computed from the raw data by the $\phi(\gamma)$ hypothesis. Urban's tables were used, and the numerical work was checked. In working up the crude results all judgments of 'doubtful' were considered as equal cases.

Results.—The numerical results are shown in the accompanying tables. Table I. gives the results of the preliminary series. Under each observer, the first column gives the computed limen (all upper limens); the second column the measure of precision, *h*, for the determination; and the third column the relative difference limen, obtained by dividing the difference limen proper by the standard stimulus.

Table II. gives the results of the main experiment. The upper and lower limens are computed separately, and are given with their measures of precision. The intervals of uncertainty are also given.

In Table III. the means of the upper and lower limens

TABLE II

UPPER AND LOWER LIMENS AND INTERVALS OF UNCERTAINTY

Standard	Limen	Obs. B			Obs. D		
		D. L.	*h*	Inter. of Uncertainty	D. L.	*h*	Inter. of Uncertainty
110	Lower	4.500	.2272	8.667	8.574	.1349	14.072
	Upper	4.167	.3530		5.498	.2344	
200	Lower	8.458	.4199	17.422	11.109	.1635	21.027
	Upper	8.964	.2197		9.918	.1645	
400	Lower	12.187	.2171	26.187	12.661	.0891	27.542
	Upper	14.000	.2065		14.881	.1300	
800	Lower	23.551	.0689	50.252	24.799	.0637	48.541
	Upper	26.701	.0928		23.742	.0752	
1,600	Lower	64.340	.0415	122.980	62.760	.0274	117.050
	Upper	58.640	.0373		54.290	.0366	
6,400[1]	Lower	229.950	.0065	502.800	149.010	.0059	360.440
	Upper	272.850	.0056		211.430	.0050	

TABLE III

MEAN DIFFERENCE LIMENS

Standards	Obs. B		Obs. D	
	D. L.	Rel. D. L.	D. L.	Rel. D. L.
110..........	4.333	.0394	7.036	.0637
200..........	8.213	.0411	10.513	.0526
400..........	13.093	.0327	13.776	.0344
800..........	25.126	.0314	24.270	.0304
1,600..........	61.490	.0384	58.520	.0366
6,400[2]..........	251.400	.0393	180.220	.0282

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[1] These limens were determined after the main work had been completed. They are thus not comparable (on account of increase of practice) with the other values of the table. The stimuli were given by an Edelmann-Galton whistle, calibrated by Kundt's method; the comparison-stimuli differed by 100 vs.

[2] See preceding footnote.

are given. The relative difference limens calculated from these means are also given.

Turning now to the introspective results, we present sample introspections to show the criteria used by the different observers, and the changes which these criteria underwent. At the beginning of the work, B.'s main reliance was upon imitative kinæsthesis. On the second day he says: "My criterion of difference is largely kinæsthesis. I do not judge much by vision. Difference is in mouth kinæsthesis; volume equal if I can sing the two tones without modifying mouth kinæsthesis. Also body kinæsthesis. Sometimes small tones seem as if body were contracted, pulled up tight. When there is less of this, it is a larger tone." A little later, in an introspective series: "The first tone was localized in tongue and mouth, with merest trace of kinæsthesis in throat. It was spread vaguely throughout the mouth and not localized certainly. The second tone was felt as localized in all of the chest muscles. The whole chest felt big and roomy." Still later: "I do not think criterion is uniform. I do not always judge by the same standard, but I do judge the same thing. . . . When I am not able to avoid pitch, it too is judged in kinæsthetic terms; so that the whole thing really becomes a judgment of kinæsthesis against kinæsthesis. . . . I am not sure that all judgments are of the kinæsthetic sort. Many of them are more vague, so vague that I don't know much about them. These are the ones, apparently, that are used when there is no trouble to distinguish between pitch and volume." Here he begins to drop secondary criteria. He never gets completely rid of them, and descriptions of localization in mouth, throat, etc., run all through his reports. But he is of a predominantly kinæsthetic type, and judges pitch in the same way. This is shown in the last report quoted, and again a little later: "I don't know whether the difference is inherent in the sounds themselves or not. I know perfectly well what volume is, and it is immediate with the tone. I can't hear a tone without having a setting for it. I have this for both pitch and volume." In the last introspective series, just before the last quantitative work, he finds a number of cases where the judgment comes immediately, without secondary criteria, though the kinæsthetic judgments are still the most frequent. In the final series, he begins to talk of kinæsthetic set. "The criterion most of the time is a particular kinæsthetic set. I am not sure whether it is there all of the time. I don't think I ever judge volume without having some kinæsthesis; the amount of it varies. Sometimes I seem to take the thing almost visually in terms of eye-rotation. I think this kinæsthesis is only image, though I am not sure." "By repeated practice the kinæsthesis I had isn't so elaborate. A little tag of it seems to carry the judgment. Sometimes I judge automatically."

D. relies less upon secondary criteria. At first he says: "I try to judge by the bigness of the thing, the roominess. . . . The larger tones seem to have a greater diffuseness. It seems as if the smaller ones fill space more compactly, what they fill." Visual imagery comes in very often. In an introspective series he says: "I visualized the last one as larger. . . . I think of them as bands of grays, both about the same width, the smaller one shorter than the other. . . . There is also a kinæsthetic component. I seem to repeat the sounds over again in my throat. . . . Also, I have a tendency to want to spread out my hands to demonstrate the size of the larger." Later on the secondary criteria tend to drop out, and the judgment becomes more direct. "There is a difference in size there. It is awfully hard to describe. I do not think it is a *meaning* applied to the thing. It isn't anything you read into the tone. I mean by that, that I don't get this idea of size from anything else, any imagery, any muscular movement. The size of the tone seems to be a part of the tone itself, just as much as the pitch is. I don't think I am judging pitch. I am not sure what my criteria are. In fact, I feel that I am judging a quality of the tone itself. I hear the difference in volume, just as I hear the difference in pitch." Again: "I think this is an actual difference in size. That is the thing that strikes me most definitely. It is actually larger. It is a different kind of size from visual size. . . . Space is so visual to me that, when I open my eyes, it seems a sort of folly to me; but when I close my eyes I feel the massiveness of the tones." Towards the end of the work, D. tends to report almost entirely in terms of attribute. "I was judging independently of pitch, although I can't help but notice the pitch. I think of the tone in this analytic way when I am trying to get at the volume." "The volume seems to be something different from the pitch. The large tones are roomier, bigger." Towards the end: "If I were to analyze the tone I should give it the three qualities of pitch, vowel, and volume. The volume is the hardest to judge, but I think I am doing it without at all mixing it with the other two. The difference between the volume of two tones is not the same, qualitatively, as between the pitch; that is, tones that are alike in pitch are practically identical, but, for volume, they seem to fall into groups, within which there is considerable variation in pitch and some in vowel quality. In judging, I try to group them in proper groups. I do have some kinæsthesis of the throat, but I cannot say that it plays any part in the judgment."

F. talks more of the meaning of volume than of his actual criteria. At first he says: "The biggest tones seem to fill me up more, to come from a bigger vessel." "Difference of volume is bigness of sound. It seems to take up more space, to fill me up more. It is an effect similar to an odor in filling the lungs." "Volume is the bigness of the vessel the tone comes out of; the way I hear it, whether it seems to go through me, to have a filling effect; the capacity of the sound." Later: "I am so used to some of the sounds now that they don't appeal to me as they used, in respect to volume. They don't have the filling effect on me that they had. Now I use as criterion the size of the vessel emitting the sound, and I seem to imagine the sound visually, as spread out behind me." "I noticed on several occasions that the volume of the sound appeared to be distinct from the pitch. I noticed both, but I could imagine the pitch as being emitted by the instrument, and the volume appearing like an enveloping atmosphere or medium. While the pitches were different, the mantle of sound around them seemed to be of the same bigness. In most cases I can't seem to help noticing the pitch. I try not to use it as a criterion for volume judgments, but cannot say whether I do or not."

Our observers are evidently not judging pitch. For one thing, they are able to distinguish introspectively between pitch and volume. Moreover, the limen for pitch is in every case much lower than that for volume. F. admits, it is true, that he sometimes uses pitch as a guide for judgments of volume; and his volume-limen is, in fact, lower than that of the other observers. But even so he is judging something more than pitch. For the other observers there is no doubt.

We may next inquire whether the observers were giving judgments of an attribute of the tone itself, or of something associated to the tone. D. is positive that volume is as much a part of the tone as is pitch. What secondary criteria he had came in only incidentally, and were not essential to the judgment. B. has more secondary criteria; and it might be thought that he is judging in terms of associated kinæsthetic images. But then he judges pitch in these same terms, and pitch is undeniably an attribute. Besides, with practice his judgments of volume become more automatic. F.'s reports are almost wholly in terms of meaning; and it is difficult to say what, psychologically, he was observing. He observed, as we have said, only for a short time.

The numerical results show a high degree of consistency. In the preliminary work, B. had everywhere the highest limen, D. the next highest, and F. the lowest. In the main series, D.'s limens are practically unchanged, though they are slightly higher than before in three cases. But B.'s values are much lower than in the preliminary series, and come very close to those of D. The change is evidently due to practice. The discrepancy between the final figures for B. and D. at 110 and 200 is probably due to imperfection of the apparatus; D. found these low tones weak, noisy, and hard to judge. In particular, D.'s lower limen at 110, which is entirely disproportionate to the upper limen, must probably be referred to this imperfection of the apparatus.

The value of the relative difference limen is fairly constant for the whole range. In the results for B. and F., it is as nearly constant as the ordinary limen for intensity under Weber's law. The figures for D. show the same trend, save where the results are vitiated by the observer's difficulty with the lower variators. This course is entirely different from that of the limen of pitch.[1] If, then, we accept independent variability as the criterion of an attribute, we have a strong case, so far as the present work goes, for the differentiation of pitch and tonal extensity.

Conclusions.—(1) Judgments of tonal volume can be made with ease, and with as great consistency as is usual for attributive judgments.

(2) The judgments may be made on an attributive basis. After secondary criteria have been eliminated, by practice, they are as immediate as judgments of pitch.

(3) Within the limits of our experiment, the relative difference limen of tonal volume is approximately constant.

(4) This limen is different from that of pitch, both in magnitude and in course.

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[1] See collected results in Titchener, 'Experimental Psychology,' Vol. 2, Part 2, 237 f.

OUTLINE AS A CONDITION OF ATTENTION

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Observations have recently been made in the psychological laboratory of Cornell University, concerning the effect of form, or outline, upon clearness. It was found by Meads[1] that a definitely outlined light, in the form of a cross, had a greater power to attract attention than a light, of the same objective intensity, which appeared merely as a "spot" that faded off gradually into its black background. It was found, further, that the light in the form of a cross could be considerably lessened in intensity and still attract as high a degree of attention as the formless spot. As a criterion of the ability of the stimulus to attract attention, introspective judgments of clearness were used; so that the statement, that the cross attracted more attention than the spot, is to be taken as meaning that the subjects judged it to be clearer. Meads's work, then, may be regarded as experimentally establishing the fact that definiteness of outline is a condition of attention, in the sense that a visual stimulus with definite outline is judged clearer than one without it.

The present study, though dealing with the same problem as that worked on by Meads, was made by a purely objective method of measuring degree of attention, one that does not require introspections of clearness on the part of the subject. I have recently described this method in detail, and given, as it seems to me, an elaborate demonstration of its validity.[2] This method seems of sufficient importance to justify testing it in every possible way, and adapting it to as many types of

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[1] 'Form *vs.* Intensity as a Determinant of Attention, Minor Studies from the Psychological Laboratory of Cornell University,' communicated by E. B. Titchener and H. P. Weld, *Amer. J. of Psychol.*, 1915, 150–151.

[2] 'The Measurement of Attention,' *Psychol. Monog.*, 1914, Vol. XVII., 5, pp. 158.

problems as possible. The problem of the effect of outline is not only an interesting one in itself, but seemed better than any other for determining the degree of correspondence between clearness as judged introspectively and degree of attention as measured objectively. On the whole, the results obtained by Meads, though reported as a minor contribution, probably form the best introspective basis at hand for testing the validity of any objective method of measuring degree of attention; and it was on this account that I undertook the present investigation. My object was not so much to study the effect of outline upon degree of attention as to ascertain whether the objective method, to which I have referred, can be given a further, introspective, justification, by determining whether it leads to the same conclusions as are reached by means of introspection on the part of trained observers. Pillsbury writes that ". . . the only test of a method is its accomplishment. . . . Any method that gives results must be kept."[1] It will be seen from the data below, that both the objective measurements here presented and the systematic introspections reported by Meads result in the same conclusion, namely, that definiteness of outline is a condition of attention.

The objective method used is one employing reaction times. It consists in first obtaining a series of reaction times with a regularly repeated preparatory interval of 2 secs., and then in obtaining a series of reaction times with a set of irregular preparatory intervals of widely differing lengths. The reciprocal of the absolute prolongation produced by the irregular preparatory intervals is taken as the measure of the degree of attention given to the stimulus, or rather to the change of stimulus, to which the subject reacts. The theory of the method is similar to that of the well-known distraction methods that have been proposed for the measurement of attention. The unfavorable preparatory intervals are regarded somewhat as a distracting influence, though in view of the fact that they merely constitute an obstacle to a high

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[1] 'The Function and Test of Definition and Method in Psychology,' *Science*, Vol. XLI., 1915, **1054**, 375.

degree of attention, without causing a division or tearing apart of attention, it seems preferable to speak of them as a *detracting* influence. The absolute prolongation in reaction time which is produced by the use of the irregular preparatory intervals may be spoken of as the absolute detraction effect. I have previously shown that this absolute detraction effect varies inversely as the degree of attention given to the change in stimulus to which the reaction occurs.[1] The reciprocal of the absolute detraction effect may therefore be taken as a measure of the degree of attention.

Now a reaction is always a reaction to a change, and the attention measured by the method just described is, therefore, always the attention to some change. Hence, to study outline as a condition of attention, it is necessary to determine whether a variation in the definiteness of outline of a stimulus causes a variation in the degree of attention to some change in that stimulus. If the degree of attention becomes greater as the stimulus becomes more definite in outline, we may expect that, at the same time, the degree of attention to a given change in the stimulus will likewise become greater.[2] The change used, in most of the work, and in all that reported in Tables I. to IV., was a slight decrease in intensity.

My procedure for studying the effect of definiteness of outline as a condition of attention, then, was as follows: Two visual stimuli were used, both of the same objective intensity. One was a definitely outlined square of light, the other, a formless spot. With each of these stimuli, reaction times to a slight decrease in intensity were obtained, both with regularly repeated 2 sec. preparatory intervals and with a set of preparatory intervals varying from 4 to 20 secs. and mixed in irregular order. The reciprocal of the absolute prolongation in reaction time produced by the irregular preparatory intervals is taken as a measure of the degree of attention to the decrease in intensity. If the degree of attention thus found is greater with the definitely outlined

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[1] *Op. cit.*, Chap. III.

[2] See Wirth, *Philos. Stud.*, Vol. XX., 1902, 625–635. Also Woodrow, *op. cit.*, 118.

square than with the spot, it means that definiteness of outline is a condition of attention.

The intensity of the stimulus used was quite weak, since it was found that the difference in clearness between the spot and square was then much more pronounced than when the intensity was great. The change in intensity, it is important to note, was produced at the source of illumination, a stereopticon lamp, and therefore affected the entire square and the entire spot. As some of the subjects could notice a much smaller change in intensity than others, different sized decreases were used with different subjects. In each case, preliminary trials were made to determine the size of decrease just great enough to be always promptly noticed, even in the case of the spot, with irregular preparatory intervals. When the size of decrease suitable for a given subject was once determined, it was not thereafter changed. It was found that as the size of the change in intensity was increased, the effect of outline became less important. However, even with the subjects who had the finest discrimination, subjects Ll and Ww, the largest size of change used, that with subject Bn, was still affected by outline.

Before deciding to use a decrease in intensity as the change in stimulus to which the subject should react, I made preliminary trials, with several of the subjects, in which I used a slight increase, and also the complete cessation and the simple occurrence of the light. With some subjects, provided a light of weak intensity was used, the degree of attention to any of the four types of change was markedly affected by definiteness of outline. However, in the case of the mere occurrence or the complete cessation of the stimulus, or even with a very large increase or decrease in intensity, unless the stimulus was very weak, the effect of outline upon the clearness of the change was comparatively slight. This was determined not only by introspection but by a number of measurements of the degree of attention. When the mere occurrence of the light was used as the reaction stimulus, even with a weak light, one of the two subjects tried showed absolutely no effect as the result of outline, while the other showed a marked effect. The subject who showed no effect gave the introspection that he reacted before he became definitely aware of the outline, he merely knew that "something had happened out there, and apprehended it as a definite square or as a spot only after reacting." This introspection would suggest that an awareness of the form of the outline is first necessary if it is to exert a favorable influence on the degree of attention. Further investigation, however, with very weak intensities of stimulus, and with more subjects, would be necessary to establish this point.

The subjects were carefully instructed to make every reaction as quick as possible. Only with such instructions is there any guarantee of the soundness of the method used, for then, only, is the time required for the reaction an adequate measure of the subject's efficiency. As the stimuli were always in view, no special fixation point was provided. The subjects merely fixated as nearly as possible the center of the stimulus, whether square or spot.

In all, seven subjects were used. With two of these, a series of 10 measurements were made, for both square and spot. With the others, as many measurements were made as their time permitted. At each sitting, 50 reactions were taken with both the square and the spot, 25 with a regularly repeated interval of 2 secs., and 25 with irregular preparatory intervals. The order in which the square and spot were used was changed each day. The set of 25 irregular preparatory intervals was the same as I have used in previous work, namely, 4, 16, 20, 4, 8, 20, 12, 16, 8, 4, 12, 4, 20, 8, 12, 4, 16, 20, 8, 16, 12, 12, 8, 16 and 20 secs. The order of the first few intervals was changed occasionally with those subjects who were used oftener than two or three times.

Details of Apparatus.—The lights used as stimuli were projected from a stereopticon onto a ground glass partition 180 cm. square, built into the wall separating the subject's room from the experimenter's room, both of which rooms were dark-rooms. The only illumination in the experimenter's room was a small shaded lamp, by which to read the chronoscope. The light from this lamp was prevented from falling onto the projection glass by a curtain, which entirely screened off the experimenter's end of the room. Between this curtain and the projection glass, was placed the stereopticon, the objective of which came about 75 cm. behind the glass. The stereopticon was illuminated by a frosted-tip, 40-watt, Mazda, tungsten lamp, in series with another

40-watt Mazda, on a 115-volt D.C. circuit. The intensity of this light was very considerably reduced by means of a yellow and a blue-green sheet of gelatine, placed in the path of the projected rays. The voltage for the lamp circuit was supplied from the university power-house. A Weston voltmeter was kept constantly connected with the circuit terminals, and placed right behind the chronoscope, where the experimenter could watch it. Most of the time the voltage was fairly steady. On certain days it was not, however, and work was given up on those days. Throughout the great majority of sittings, there was not a variation of more than 2 to 4 volts, and no sudden jumps.

In order to obtain a spot of light without definite outline, the stereopticon objective was thrown completely out of focus, and no slide put in the slide-holder. The subject's room was dimly illuminated by a 10-watt frosted Mazda placed about 5 meters behind, and 2 meters above, the subject's head. This dim illumination was used to make it easier to secure perfect retinal adaptation, as it is well known that a very long and variable time is required for complete adaptation to darkness. With this illumination of the subject's room, the spot of light appeared somewhat yellowish green, but very poorly saturated. It differed but little in color tone from the rest of the ground-glass plate which formed its background. It is difficult to give the dimensions of the spot, as it shaded off so gradually that it was impossible to tell where it ended. Its just visible edge was about 18 cm. in diameter. It was just noticeably less bright at 4 cm. from the center than at the center.

The square of light was obtained merely by lowering a dark gray paper screen on the opposite side of the projection plate from the subject. In the center of this screen was cut a square 7 x 7 cm. The change from spot to definitely outlined square was made, then, simply by cutting off the edges of the spot. The reason for using a square, instead of following Meads in the use of a cross, is that a square was found to present a surface of more uniform brightness than a cross of the same area. Along the border of the square one could discern a narrow contrast rim, 2 or 3 mm. wide, of heightened intensity. The brightness of the screen itself was so chosen that, upon being lowered into position, it did not change the apparent intensity of the background.

The decrease in brightness to which the subjects reacted was produced by closing a circuit in parallel with the stereopticon lamp. This circuit contained a series of four Mazda lamps, which were not the same for all subjects. By varying the resistance of these lamps, the amount of decrease in the intensity of the stimulus could be varied. As already stated, a decrease in intensity which seemed just sufficient to be always promptly noticed by the subject was chosen by preliminary trials with each subject.

The circuit in parallel with the stereopticon lamp was closed simultaneously with the separate chronoscope circuit, by means of a double key. The chronoscope, a Hipp's, was arranged in series with a storage battery of about 12 volts, and controlled before and after each sitting by the Wundt fall-hammer, set for a time of 201 σ. The warning signal used was the click of an electric sound-hammer. The duration of the preparatory intervals was judged by the experimenter with the aid of a head telephone receiver, connected with an electric metronome, beating seconds, in an adjoining room.

The results for each subject are given in Tables I. and II., and a summary of the results for all subjects in Table III. The reaction times obtained with the definitely outlined square are given in the columns headed 'Square,' and those with the indefinitely outlined spot in the columns headed 'Spot.' The columns headed '2 secs.' and those headed 'Irreg.' give the average reaction times with regularly repeated 2 sec. preparatory intervals and with the set of irregular preparatory intervals. The difference in the average reaction time with the 2 sec. intervals and with the irregular intervals is given in the columns headed $1/A$ and $1/A'$. Since this difference varies inversely with the degree of attention, A stands for the degree of attention given the decrease in intensity in the case of the square, and A' for the same in the case of the spot. The figures in italics are the mean variations of the averages above them.

A study of Tables I. to III. shows that, in every case, there was a marked prolongation in reaction time produced by the irregular preparatory intervals, but that this prolongation was always greater with the spot than with the square. Now the prolongation produced by the irregular preparatory intervals varies inversely as the degree of attention. Therefore, since at every sitting this prolongation is greater with the spot than with the square, it follows that the degree of attention to the change is less in the case of the spot than in the case of the square. The difference between the two cases in the amount of prolongation produced by the irregular intervals indicates the difference in the degree of attention. This difference is given in the columns headed $1/A' - 1/A$. In Table III., I have added another column, headed $(A - A')/A$, which, upon the assumption that the measurements of degree of attention made by the present method may be regarded as absolute measurements, gives the percentage of decrease in degree of attention produced by using the spot in place of the square.

The correctness of the general conclusion that the degree of attention to the change in intensity is less when it occurs in the spot than when in the square, is strongly confirmed by the subjects' introspections. These introspections were very carefully noted throughout the work, as it was mainly the possibility of obtaining a good introspective confirmation of the method of measuring attention here used that led to the

TABLE I

THE EFFECT OF DEFINITENESS OF OUTLINE UPON DEGREE OF ATTENTION TO A SLIGHT CHANGE IN INTENSITY

Subj.	Square			Spot			1/A′ − 1/A
	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A′	
Ww.	276	349	73	297	417	120	47
	33	*57*		*34*	*70*		
"	267	342	75	290	395	105	30
	42	*45*		*44*	*42*		
"	263	352	89	283	400	117	28
	32	*34*		*36*	*50*		
"	248	329	81	256	360	104	23
	18	*46*		*19*	*36*		
"	257	327	70	257	368	111	41
	19	*27*		*19*	*35*		
"	265	331	66	274	378	104	38
	18	*27*		*24*	*33*		
"	263	328	65	252	347	95	30
	29	*20*		*27*	*31*		
"	255	339	84	278	386	108	24
	38	*20*		*17*	*46*		
"	256	337	81	259	368	109	28
	32	*50*		*21*	*47*		
"	254	338	84	253	368	115	31
	30	*26*		*36*	*39*		
Av.	**260**	**337**	**77**	**271**	**379**	**108**	**31**
Av. M. V.	*31*	*35*		*28*	*43*		
Ll.	282	385	103	292	436	144	41
	19	*32*		*26*	*52*		
"	268	363	95	294	429	135	40
	18	*36*		*26*	*61*		
"	274	359	85	317	479	162	77
	17	*28*		*26*	*49*		
"	286	366	80	281	394	113	33
	14	*40*		*18*	*61*		
"	272	347	75	286	422	136	61
	20	*39*		*22*	*53*		
"	278	372	94	284	446	162	68
	16	*34*		*20*	*68*		
"	277	374	97	278	431	153	56
	19	*33*		*23*	*66*		
"	276	374	98	279	422	143	45
	18	*26*		*22*	*74*		
"	275	383	108	283	424	141	33
	18	*31*		*30*	*53*		
"	274	374	100	291	429	138	38
	26	*38*		*22*	*45*		
Av.	**276**	**370**	**94**	**288**	**431**	**143**	**49**
Av. M. V.	*19*	*34*		*24*	*58*		

TABLE II

A CONTINUATION OF TABLE I

Subj.	Square			Spot			1/*A*′—1/*A*
	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*′	
Fs.	304	393	89	311	486	175	86
	12	*32*		*26*	*62*		
"	282	385	103	292	436	144	41
	19	*32*		*20*	*42*		
Av.	**293**	**389**	**96**	**302**	**461**	**159**	**63**
Av. M. V.	*16*	*32*		*23*	*52*		
St.	280	413	133	296	479	183	50
	29	*47*		*35*	*61*		
"	295	388	93	303	441	138	45
	30	*42*		*38*	*98*		
"	297	385	88	305	427	122	34
	34	*33*		*39*	*58*		
"	288	379	91	317	465	148	59
	29	*41*		*38*	*68*		
Av.	**290**	**391**	**101**	**305**	**453**	**148**	**47**
Av. M. V.	*31*	*41*		*38*	*71*		
Bs.	263	385	122	282	474	192	70
	20	*34*		*24*	*54*		
"	259	413	154	264	485	221	67
	22	*27*		*38*	*49*		
"	232	370	138	255	438	183	45
	21	*38*		*27*	*34*		
"	247	373	126	268	449	181	55
	22	*35*		*25*	*50*		
"	235	369	134	255	435	180	46
	23	*25*		*26*	*56*		
"	274	385	111	288	477	189	78
	29	*37*		*28*	*68*		
"	249	356	107	256	435	179	72
	27	*39*		*29*	*59*		
Av.	**251**	**378**	**127**	**267**	**456**	**189**	**62**
Av. M. V.	*23*	*34*		*28*	*53*		
Dg.	295	426	131	322	517	195	64
	35	*59*		*36*	*48*		
"	282	407	125	299	495	196	71
	20	*43*		*28*	*60*		
Av.	**289**	**417**	**128**	**310**	**506**	**196**	**68**
Av. M. V.	*28*	*51*		*32*	*54*		
Bn.	332	460	128	374	597	223	95
	33	*50*		*55*	*103*		
"	375	478	103	410	604	194	91
	36	*48*		*58*	*85*		
"	333	490	157	396	626	230	73
	28	*62*		*70*	*98*		
Av.	**347**	**476**	**129**	**393**	**609**	**216**	**87**
Av. M. V.	*32*	*53*		*61*	*95*		

TABLE III

A SUMMARY OF TABLES I AND II

Subj.	Square			Spot			$1/A'-1/A$	$\frac{A-A'}{A}$
	2 Secs.	Irreg.	$1/A$	2 Secs.	Irreg.	$1/A'$		
Ww	260	337	77	271	379	108	31	.28
	31	*35*		*28*	*43*			
Ll	276	370	94	288	431	143	49	·34
	19	*34*		*24*	*58*			
Fs	293	389	96	302	461	159	63	·39
	16	*32*		*23*	*52*			
St	290	391	101	305	453	148	47	·32
	31	*41*		*38*	*71*			
Bs	251	378	127	267	456	189	62	·33
	23	*34*		*28*	*53*			
Dg	289	417	128	310	506	196	68	·35
	28	*51*		*32*	*54*			
Bn	347	476	129	393	609	216	87	.40
	32	*53*		*61*	*95*			

present investigation. All the subjects, without exception, gave the introspection that the change was clearer and easier to notice when it occurred in the square. The subjects were all just as sure that the change in the square was clearer than the change in the spot, as they were that the square itself was clearer than the spot.

The following are a few typical introspections:

Ll: "The square stays in view better, and the effort of concentration is less. After working with the spot, I always see the square come with a sigh of relief. Always tired when through with the spot, but not after the square. Winking didn't bother in the case of the square, but was necessary with the spot to relieve strain of concentration. The change in the square is certainly clearer than that in the spot."

Bs: "The square is much clearer—easier to see. The change is easier to notice in the case of the square—no doubt about it. The change is so much more distinct in the case of the square that it gives me more of a chance to think about reacting quickly."

Bn: "Change twice as easy to notice with square as with spot."

St: "There is no doubt about it" (that the change in the square is clearer than that in the spot).

Dg: "Square is of course more distinct and somewhat brighter, but it is also clearer, that is, makes more of an impression on you. The change is much clearer in the square."

In addition to such introspections as those noted above, systematic observations were made by myself and my assistant[1] on the effect of varying the absolute intensity of the light and also of varying the size of change in intensity of stimulus. We both observed that, with increase in the intensity of the stimulus, there occurred a marked decrease in the effect of outline upon the clearness of the stimulus, and, similarly, with increase in the size of the change in intensity, there occurred a marked decrease in the effect of outline upon the clearness of the change. The correctness of these observations was checked by objective measurements of degree of attention made with a high intensity of stimulus and a very large decrease in intensity. These measurements showed a high degree of attention to the change in intensity, in both the square and the spot, but no reliable difference between the two.

In interpreting these observations on the effect produced by variation in intensity and in size of change upon the importance of definiteness of outline, it should be remembered that just as the degree of attention to a simple stimulus is conditioned objectively by the intensity of the stimulus, so is the degree of attention to a change in intensity of stimulus conditioned by the size of the change.[2] Now, since the effect of outline becomes less marked as the size of change or the absolute intensity is increased, it is evident that the effect of outline as a condition of attention becomes less as the degree of attention affected becomes greater. For example, if the square and the spot both have a very great brightness, the degree of attention which each attracts is high, and there is then little, if any, difference in clearness between them. If, on the other hand, they are both very dim, the square is much the clearer. Similarly, as regards the effect of outline on the degree of attention to a change in

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[1] Assistance in this study, as well as in several others, was made possible by aid received from the research fund of the University of Minnesota.

[2] Woodrow, *op. cit.*, 114.

intensity, if the change is large, it attracts an equally high degree of attention in both the square and the spot, but if it is small, it attracts much more attention in the square.

The fact that the effect of outline upon degree of attention becomes less important with increase in the degree of attention otherwise conditioned, is merely an illustration of the general law, that "the absolute effect of a given detractor of attention varies inversely as the degree of attention upon which the detractor acts."[1] The detractor of attention, in the present instance, is a given degree of indefiniteness of outline. Another well-known illustration of this law lies in the fact that an "attention wave" is easily discernible only in the case of a very weak stimulus, such as produced by the rotating Masson's disk, in which case the degree of clearness is also very low because of the insufficiency in the objective condition of intensity. Here we are evidently dealing with some detracting influence, of a physiological nature, which has a marked effect when the degree of attention affected is low, but little or none when it is high. I have elsewhere[2] described in detail a number of other illustrations of this law, using irregular preparatory intervals as the detracting influence.

One might ask why the simple reaction times with the regularly repeated 2 sec. intervals could not be used alone as a measure of degree of attention. Perhaps they could, in the present instance, but they would not serve as a very sensitive measure and one could not be certain that the difference in simple reaction time between the reactions to the change in the square and those to the change in the spot was due to the difference in degree of attention. The reaction process is complex, and variation in simple reaction time may be the result of variation in other factors than attention. Difference in the prolongation produced by the irregular preparatory intervals, however, can be due only to difference in the degree of attention in the two cases, since this prolongation is due solely to the effect of the set of irregular

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[1] *Op. cit.*, 99.

[2] *Op. cit.*, 157.

intervals as a detractor of attention.[1] Moreover, by using the reciprocal of the absolute prolongation in reaction time produced by the irregular intervals, we obtain a measurement which is comparable with other measurements of the degree of attention, made at other times and under other circumstances.

To make an accurate comparison of the absolute effect of outline upon two different individuals, it would be necessary first to equalize their degree of attention in the case of the square. This could be done directly, by varying the size of the change to which they were to react, until the prolongation produced by unfavorable intervals was the same for both. This would consume a great deal of time, but by using several different sized changes, one could easily calculate the sizes of change needed to make the degree of attention of the two individuals the same. After having equalized the degree of attention for the two individuals to a change occurring in the square, we could then measure the degree of their attention to the same change occurring in the spot, and thus determine whether the effect of outline as a condition of attention was the same for both. No accurate measurement of individual differences has been attempted by the method just described, but there would be no great difficulty in making such. The data, as obtained, indicate marked individual differences—but it is certain that these would be greatly lessened if the subjects were made equal in degree of attention, since, in general, there is evident a considerable degree of correlation between the reciprocal of the degree of attention to the change in the square ($1/A$) and the unfavorable effect of indefiniteness of outline upon the degree of attention ($1/A' - 1/A$).

The fact, that a fixed change in the intensity of a stimulus attracts more attention when the stimulus is definitely outlined than when it is not, can be due only to the favorable effect of definiteness of outline, as such, upon the degree of attention. It is true that the square had a slightly greater average apparent intensity than the spot, presumably as the

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[1] *Op. cit.*, 49–63 and 114.

result of contrast. This fact, however, would not account for the greater clearness of the change in the case of the square, for the effect of contrast is to decrease the noticeability of a slight change in intensity. Anyone may readily convince himself that this is the case, by observing a very small change in the intensity of a light when it is matched with its background in brightness, and then observing this same change when the background is either markedly lighter or darker. In the latter case, this change is less clear.[1] After all, so long as enough contrast to give a definite outline is present, variation in degree of contrast has very little effect upon the clearness of a given relative change in intensity.

In order to determine beyond question, however, whether the greater degree of attention to the square was due to any increase in apparent intensity apart from its greater clearness, an additional experiment was made. In this experiment, by using a background considerably brighter than the stimulus, the square was rendered definitely less in apparent intensity than the spot. Nevertheless, the square was definite in outline and was judged unquestionably clearer than the spot. This may seem to run counter to the statement that intensity is an important condition of attention. As a matter of fact, it merely illuminates the meaning of that statement. In the case of light, not intensity, but difference in intensity, is the real, intensive, condition of attention.[2]

An experiment was conducted, then, in just the same way as the work already described, except that the definitely outlined square was surrounded by a brighter background, and therefore rendered slightly, but unquestionably, less in apparent intensity than the spot. The definiteness of outline of the square was judged by the two subjects used in

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[1] This seems to be due to the fact that the effect of contrast, relative to the difference in intensity of the contrasting surfaces, is greatest when the difference is very small. See Lehmann, 'Ueber die Anwendung der mittleren Abstufungen auf den Lichtsinn,' *Philos. Stud.*, 1886, Vol. III., 516–528.

[2] I have found by systematic introspection on the part of myself and subject Ll that the increase in clearness of the stimulus, with increase in the difference in intensity between the stimulus and its background, is marked when the difference is slight but becomes much less noticeable after a considerable difference has once been reached. This holds, no matter whether the background is lighter or darker than the stimulus.

this experiment to be about equal to that of the square with the dark background used in the work already presented. The same source of illumination and the same physical decrease in intensity were used in this experiment as in the previous work with these subjects, reported in Table I. The data are given in Table IV., where the headings of the columns have the same significance as in the preceding tables.

TABLE IV

THE EFFECT, UPON THE DEGREE OF ATTENTION TO A SLIGHT DECREASE IN INTENSITY, OF DEFINITENESS OF OUTLINE PRODUCED BY SURROUNDING THE STIMULUS WITH A BRIGHTER BACKGROUND

Subj.	Square			Spot			1/*A*′—1/*A*
	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*′	
Ww............	299	376	77	300	416	116	39
	38	*36*		*20*	*63*		
“	302	382	80	312	404	92	12
	32	*36*		*33*	*33*		
“	290	367	77	308	398	90	13
	29	*32*		*40*	*25*		
“	295	379	84	294	394	100	16
	26	*38*		*22*	*34*		
Av.............	**297**	**376**	**79**	**304**	**403**	**99**	**20**
Av. M. V.......	*31*	*36*		*29*	*39*		
Ll.............	297	407	110	304	484	180	70
	18	*37*		*25*	*36*		
“.............	301	381	80	297	426	129	49
	21	*46*		*22*	*46*		
“.............	263	369	106	285	420	135	29
	18	*38*		*23*	*22*		
“.............	265	371	106	283	397	114	8
	23	*26*		*16*	*25*		
“.............	306	378	72	306	415	109	35
	17	*25*		*22*	*41*		
Av.............	**286**	**381**	**95**	**295**	**428**	**133**	**38**
Av. M. V.......	*19*	*34*		*22*	*34*		

From a comparison of the values of 1/*A* in Tables I. and IV., it is evident that the degree of attention to the change in intensity in the case of the square was affected very little, if at all, by using a bright background in place of a dark one. For subject Ww, with the dark background (Table I.), the average value of 1/*A* is 77, and with the light background (Table IV.), 79; while for subject Ll, the corresponding values are 94 and 95. In the case of the spot, some improvement in the degree of attention is to be noted in

Table IV., as the value $1/A'$, for subject Ww, changes from 108 in Table I. to 99 in Table IV., while for subject Ll, its value changes from 143 to 133. Since the reactions with the spot given in Tables I. and IV. were obtained under identical conditions, the difference is due either to 'accidental variation,' or, since the results of Table IV. were obtained after those of Table I., to practice. While I have never been able to obtain any convincing evidence that the detracting effect of unfavorable preparatory intervals wears off with practice, there is no reason why practice might not lessen the effectiveness of such a detractor of attention as indefiniteness of outline.

From the data of Tables I. and IV., the conclusion follows, that the greater clearness of the change in the square is not due to the effect of contrast on the apparent intensity of the square, but to the greater *clearness* of the square, which in turn is due to the definite outline. Definiteness of outline means that there is a definite setting off of the stimulus from its background, and it is this fact alone that gives the square the greater clearness. Definiteness of outline, though it may involve brightness contrast, is thus a condition of attention apart from the effect of contrast on apparent intensity.

Conclusions

1. The effect of outline upon the degree of attention given to a visual stimulus may be measured by measuring its effect upon the degree of attention to a small change in that stimulus, preferably a small decrease in intensity. The degree of attention to any change which may be used as a reaction stimulus is measured by the reciprocal of the absolute prolongation in reaction time produced by using a set of unfavorable preparatory intervals in place of 2 sec. preparatory intervals.

2. Measurements made by this method show that definiteness of outline is a condition of attention. This statement means that the reaction time to a slight decrease in the intensity of a definitely outlined square is less affected by

unfavorable preparatory intervals than is the reaction time to the same decrease in intensity in an indefinitely outlined spot.

3. The results obtained by the purely objective method described above agree with the results of systematic introspection on the part of trained observers.

4. The effect of definiteness of outline as a condition of attention is not due to the effect of contrast on apparent brightness. The essential condition upon which the favorableness of outline depends is merely that the stimulus shall differ sharply from its background. It may be either less intense or more intense.

5. The effect of definiteness of outline upon clearness decreases with increase in the absolute difference between the intensity of the stimulus and that of its background.

6. The effect of definiteness of outline upon the clearness of a change in intensity decreases with increase in the size of the change.

Conclusions 5 and 6 are merely particular instances of the general law that the absolute detraction effect exerted by a given detractor of attention varies inversely as the degree of attention detracted from. In the present instance, the detractor is a given degree of indefiniteness of outline, the effect of which becomes small when the degree of attention otherwise conditioned is high. The degree of attention tends to be high, in the case of attention to a stimulus, when the difference in intensity between stimulus and background is large, and in the case of attention to a change in stimulus, when the size of the change is large.

A GRADED SERIES OF GEOMETRICAL PUZZLES

BY GRACE HELEN KENT

This series of puzzles is offered as one unit of a group of non-verbal tests to be used for measuring the capabilities of defective children. The work was commenced in the School of Pedagogy of New York University. The general trend of the undertaking has been influenced by three months' experience in the use of the Binet-Simon measuring scale (Goddard's translation) in testing children of the ungraded classes of the public schools of New York and Jersey City. The majority of the children examined were high-grade defectives, but the group included some middle- and low-grade imbeciles and also a few mentally normal children who had been placed in the special classes because of nervous disorders. Nearly all were children of foreign-born parents.

My experience in the use of the Binet test is too limited to justify a general criticism of the method, but I find it necessary to mention four fundamental difficulties which I encountered: (1) The scoring of results is arbitrary, and depends too much upon the personal equation of the examiner. (2) There is little or no uniformity in the tests for children of different levels of intelligence. (3) The test depends too much upon the subject's command of the language in which it is given to be valid for children of immigrants. (4) The subject matter of the test is too far removed from the interests of children to hold their attention, and it frequently requires much urging to induce them to coöperate.

Yerkes[1] has called attention to the lack of uniformity and the inaccuracy of the system of scoring results, and has indicated a way of overcoming these difficulties. He has also shown that the disadvantage which the test imposes upon children of non-English-speaking parents is smaller than

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[1] 'The Point Scale: a New Method for Measuring Mental Capacity,' *Boston Medical and Surgical Journal*, December 3, 1914.

might be expected and that it is comparatively constant for children of different ages. In testing children of immigrants by groups, it might be possible to make allowance in the final scoring of results for this disadvantage; but if children are to be tested as individual cases this would not be a safe plan to follow, inasmuch as the deficiency in the language might be so marked as to entirely invalidate the results for the particular child in question.

The fourth objection, the problem of interest, is in my opinion the most vital of all. The purpose of the test is not to determine the child's willingness to answer certain questions, but to determine his ability to answer them. If his coöperation is given unwillingly, the results are misleading. It is plain that Binet recognized this factor, for he called attention to the need of leading the child to regard the test as a game. But this does not depend wholly upon the attitude of the examiner. Some children, especially high-grade defectives, are extremely sensitive to any reminder of their deficiencies. Such a child may be irritated by a question that he cannot answer, and he is liable to feel humiliated when required to answer one that is entirely too easy for him. As it is impossible to determine his level without asking at least a few questions above and below it, the test is something of an ordeal. Young children are sometimes painfully shy in the presence of a stranger, and it requires no small effort to induce them to speak a single word. Such a child may be pleased when a picture is shown, but he becomes self-conscious as soon as he is requested to describe it. He may name a single object, then wait for the question "And what else do you see?" before he ventures to name another. It is possible that a child's willingness or unwillingness to talk to a stranger may be in itself of some significance as an index to his intelligence; but the Binet test is inadequate as a means of determining the exact degree of communicativeness, while it is of course an unnecessarily elaborate method of ascertaining that a child is or is not talkative.

Even the highly practised observer finds it difficult to give scrupulous coöperation in a psychological experiment that involves distasteful tasks. And in order to obtain full coöperation from immature, defective, insane, or otherwise irresponsible subjects, it is important to offer something that will stimulate a genuine desire to make a good record.

These considerations have led me to undertake the development of a system of non-verbal tests. In attempting this I do not wish to discount the importance of any verbal tests that have been found serviceable in the diagnosis and classification of defectives. In general these tests of action are intended to supplement rather than supplant such verbal tests as are fairly applicable to the subject. It would seem that they should be especially helpful in the study of children of foreign parentage, and possibly in the examination of immigrants at Ellis Island. In the selection of test methods I have adopted provisionally the following rules:

1. Each test is to call for a motor reaction from the subject, rather than for a verbal response. The test itself is to be essentially mental, not motor; but it is to be a test of the subject's ability to perform a certain act which calls for some mental effort, not his ability to answer certain questions.

2. Each test is to require the minimum of verbal explanation on the part of the examiner, so that the subject's attention will not be seriously taxed in merely comprehending the requirements. The instructions are to be so simple that they may be given satisfactorily through an interpreter.

3. Each test must possess sufficient intrinsic interest for the subject to hold his attention and to command his spontaneous coöperation. The performance of the act must possess a certain dignity, so that the subject will not consider it an insult to his intelligence. The accomplishment of the task is to be, from the subject's point of view, an end in itself. The aim is not merely to devise tasks that can be presented attractively by a kindergartener; it is rather to offer something which is in its very nature a game, and so good a game as not to suffer total shipwreck in the hands of the ultra-scientific examiner.

4. Each task is to be capable of being graded, so as to be adaptable to the ability of any subject. The method

must admit of being varied widely in difficulty without affecting the essential nature of the test.

5. Each task must admit of being evaluated by a system that will give partial credit for partial success. The results are to be scored either by a sliding scale or by empirically determined steps.

The unit here presented is well adapted for use among deaf-mutes or foreigners having no knowledge of the language of the examiner, because the instructions may be given wholly by gesture. There are, of course, many similar puzzles already in use; but in view of the present demand for graded tests, it seems probable that this set of puzzles will meet a real need.

The series includes twenty-four puzzles, consisting of rectangular or triangular pieces of wood. For convenience I have arranged the rectangular and the triangular puzzles independently, in parallel series (see plate). Each puzzle when solved forms a square, and is solved by fitting the pieces into a frame ten centimeters square. The puzzles were cut from wood 3/16 inch in thickness. The frame was made by attaching strips of wood 1/16 inch in thickness to the margins of a square of wood, leaving a depression ten centimeters square. The absolute thickness of the wood is presumably of no importance, but it is important that the puzzle pieces should be thicker than the border of the frame, so that any piece which has been placed in the frame may still be easily accessible to the fingers. If desired, cardboard may be used instead of wood. A full set of puzzles, including the frame, may be made very easily by pasting together several thicknesses of light cardboard that can be cut with scissors.

In the presentation of the puzzles to the subject it is well to alternate between rectangles and triangles, because this tends to reduce the effect of practice. The time occupied by the solution of each puzzle is measured by a stop watch. In order to prevent the test from consuming more time than can reasonably be allowed for it, and also to keep a child from becoming unduly fatigued by the effort to solve a puzzle

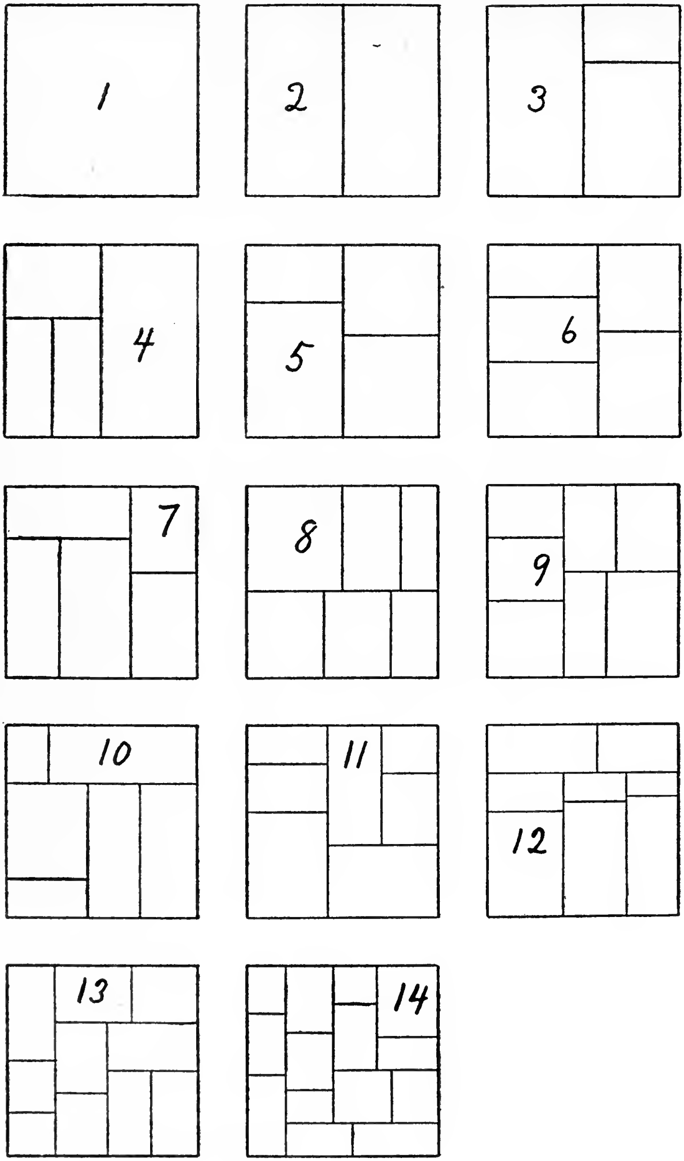

Fig. 1.

that is hopelessly beyond him, I have found it necessary to set an arbitrary time limit. When the gradation of the puzzles has been more thoroughly tested, it may be possible to fix the time limit for each puzzle according to its difficulty. For the present I allow two minutes for each puzzle, and at the close of that period I interrupt the subject and record that attempt as a failure.

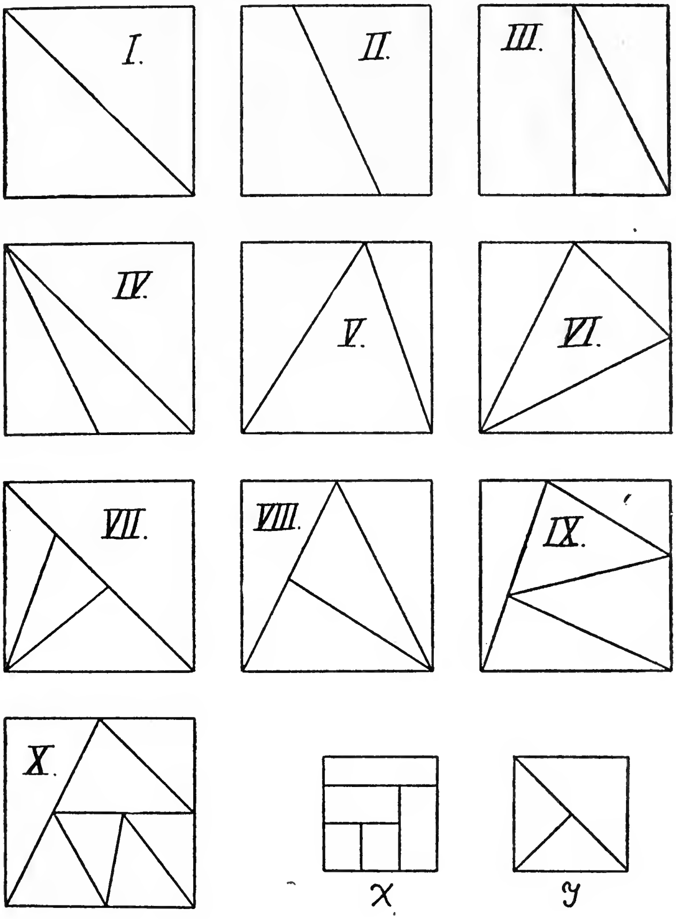

Fig. 2.

The four puzzles consisting of less than three pieces are designed primarily for infants or subjects of very low-grade intelligence, and these need not be given to a subject of fair ability. In a routine test I usually begin with 3, 4, or III., and give the puzzles as nearly in the order of difficulty as the alternation between rectangles and triangles permits, until the subject has failed to solve (within the time limit) at least one puzzle of each kind.

The arrangement of the puzzles is somewhat tentative, being based upon results obtained from but twenty-five subjects. In the development of the series preliminary tests have been made upon over one hundred subjects, including sixty-nine defective children. But the results of these tests were obtained under somewhat irregular conditions, and it seems hardly advisable to attempt to make them presentable. I have therefore disregarded them and have made a special series of tests upon twenty-five normal subjects, using the first twenty-five persons who were by any chance available. They range in age from six to seventy-five, and in education from illiteracy to university training.

The results of this series of tests are shown in tabular form. As far as possible, the arrangement of the puzzles is based on the median time record of the twenty-five subjects. The puzzles which were solved within the two-minutes time limit by less than half of the subjects are arranged according to the number of failures which they elicited.

In the early preliminary tests I used no frame, but requested the subject to make a square of the pieces. But the results were very variable, and some of the younger children failed to understand what was meant by a square. The use of the frame is recommended for various reasons, especially because it lowers the scale as a whole. In the grading of a mechanical test it is the lower end of the scale that presents the greatest difficulty. It is well to make the test qualitatively as simple as possible, and then to bring it to any desired degree of difficulty by quantitative differences.

Other things being equal, the difficulty of a puzzle depends upon the number of pieces. But a puzzle made of triangles is far more difficult than one consisting of the same number of rectangles, and there are indications that the two types differ in kind as well as in degree. The correlation between

TABLE I

Rectangular Puzzles				Triangular Puzzles			
Number of Puzzle	Minimal Time Record in Seconds	Median Time Record in Seconds	Failures, Per Cent.	Number of Puzzle	Minimal Time Record in Seconds	Median Time Record in Seconds	Failures, Per Cent.
1	1	2	0	I.	2	5	0
2	2	3	0	II.	2	5	0
3	3	7	0	III.	3	9	4
4	6	10	0	IV.	5	11	0
5	5	11	0	V.	8	16	0
6	7	18	0	VI.	12	42	16
7	10	22	4	VII.	27	—	68
8	10	23	0	VIII.	49	—	76
9	10	28	20	IX.	46	—	88
10	12	31	4	X.	—	—	100
11	20	65	24				
12	26	—	64				
13	—	—	100				
14	—	—	100				

them is apparently rather weak, and it is an open question whether they should be treated as one test or as two tests.

Puzzles 8, 9 and II. are somewhat superfluous, and need hardly be considered as belonging to the series. They are included on the ground that it is frequently convenient to have an extra puzzle which may be used as a control test. In case a child solves puzzle I. very promptly, apparently by chance, it is well to give puzzle II. Puzzles 8 and 9 consist of the same number of pieces respectively as 10 and 11, but the former are simpler of construction. If a subject is able to solve 10 and 11, it is a waste of time to require him first to solve 8 and 9. But if he fails to solve 10, it is only fair to permit him to try the intervening puzzles, so as to insure that his limit has been reached.

In the preliminary tests thirty-eight puzzles have been tried, and those which yielded the most variable results have been gradually weeded out. The figures *x* and *y* represent two puzzles that proved to be exceptionally unsatisfactory. Both gave a very low minimal time record but a very high percentage of failures, as compared with other puzzles of approximately similar difficulty. This indicates that the element of chance is an important factor in the solution. Puzzle *x* is constructed upon a definite plan, a plan which the subject may grasp at sight, or which he may fail to grasp after several minutes of hard work. Puzzle *y* is apparently very simple, but many subjects are misled by the not unnatural assumption that a right angle properly belongs in a corner of the frame. These two puzzles, however, are useful when there is occasion to give the instructions by gesture. As they are quite unlike any puzzles of the series, they may be used freely by way of illustration.

The puzzles which yield the most uniform results are those which call for a patient and systematic use of the trial and error method. Of course the element of chance cannot be eliminated, but it can be greatly reduced by avoiding any obvious principle of construction. In the rectangular puzzles the pieces should vary in both dimensions, and in the triangular puzzles it is well to depend mainly upon oblique or scalene triangles.

The puzzles at the lower end of the scale have seen considerable use in the tests made upon defective children, and it is significant that puzzles 1 and 2 are the only ones which have met with universal success. I have observed several children work very patiently for the full two minutes with puzzles I. and II., without success. It would seem that a two-piece puzzle must certainly be solved by chance within the time limit, but there are exceptions to this. These puzzles should be tested by using them with normal infants. I have as yet had access to only two infant subjects, one of twenty-three months and one of twenty months. The older child was successful with puzzles 1 and 2, but could not be induced to give sufficient attention to any others. The younger child was successful only with puzzle 1, which is nothing but a form board. Of course the failure to gain the coöperation of so young a child must be scored against the resources of the examiner in arousing the interest of the child, rather than against the child's ability.

I have not determined at what age a child may be expected to show interest in the puzzles, but I have found it very well developed in two children of six years. Older children are fascinated by them, and it is surprising to see a healthy boy so absorbed in any sedentary occupation. Some of my adult subjects have manifested an interest almost equal to that of children.

Even the defective children showed considerable spontaneity in their reactions to the puzzle test, and I made a practice of interspersing the puzzles among the questions of the Binet test, so as to make the test period as a whole more attractive. I have rarely found a child who would voluntarily abandon the attempt to solve a puzzle. Most children are reluctant to give up when time is called, and I have frequently been moved to promise another trial at the close of the test. A boy of twelve once remained more than an hour, working at the puzzles with absorbing interest and taking little notice of three persons who were watching him. His efforts were remarkably unintelligent, and there was no observable tendency to profit by experience. Because of his great persistency he finally succeeded in solving several of the moderately difficult puzzles; but if requested to try again one which he had just solved, he was as helpless as before.

The subject's mode of reaction is frequently more instructive than the time record, and it should always be observed. I have not made a practice of recording false moves, because I have not found any satisfactory way of classifying them; but I record under 'Remarks' anything noteworthy in the subject's method of working.

The puzzles should be kept behind a screen, and the subject should not be allowed to know how many there are. It is well to explain in advance that nobody is expected to be successful with every puzzle, so that he will not be disconcerted by his first failure. If he drops out in the early part of the series, he should be spared the annoyance of knowing how much he is leaving undone.

The significance of this test as a means of measuring intelligence remains to be seen. But inasmuch as geometrical puzzles are actually being used for clinical purposes, it seems worth while to develop a more systematic method of using them, so as to determine their possibilities and their limitations. The ability to grasp the relation between area and form is a highly specialized function, and its correlation with general intelligence may be comparatively weak. One of my subjects, a man of national reputation in his own field, spent nearly three minutes solving a puzzle which has since been solved in twenty-seven seconds by a child of six years. This puzzle (figure *x*) has since been dropped from the series, but the fact remains that subjects of unquestioned intelligence vary very widely in their possession of the ability to solve puzzles of this type.

If mechanical tests are to be useful for determining intelligence, it is of the utmost importance to have available more and yet more tasks which differ essentially in kind and which call into play widely different types of mental activity. Among the units of such a group there should be pairs which show a strong negative correlation, and the tests should be so varied as to give any subject a fair opportunity to show what kind of ability he may possess.

There will be no attempt at present to standardize this test on a large scale, because it is not certain that it is worthy of being standardized. The present need is for intensive rather than extensive study of test methods. A large number of methods must be tried out in order to find even a few that will be successful, and it is a work of years to find a sufficient number and variety of tasks each of which shall meet all the practical requirements. Standardization, of course, does not render valid a method that is fundamentally defective, but it is all too likely to prolong the life of a criterion which would far better be permitted to die a natural death.

I am offering this unit for what it is worth, possibly a little prematurely, in the hope that its use by others will bring to light its possibilities for improvement. Criticism both of the general plan and of this particular unit will be greatly appreciated.

RELATIVE VALUES OF POINT-SCALE AND YEAR-SCALE MEASUREMENTS OF ONE THOUSAND MINOR DELINQUENTS[1]

BY THOMAS H. HAINES, M.D., Ph.D.

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In the mental examination of adolescent delinquents, whether in psychological clinics in connection with juvenile courts or in surveys of the populations of reform schools, it has been generally recognized that the Binet-Simon tests are at their best for younger children, and that they afford practically no information as to the variations in mental endowment and character structure of persons who measure above twelve years in mental development. The work of the inventors of this scale and most of those who have sought to restandardize these tests and to make more accurate year scales on the Binet-Simon plan, has been generally confined to testing of children in the primary and grammar grades. Binet and Simon set apart no tests for eleven, thirteen, or fourteen years. The fifteen-year and adult tests remain very poorly standardized.

It is evident that tests devised for and applied to weeding out the feeble-minded from school populations where the oldest children are thirteen or fourteen, are poorly adapted for studying mental make-up of juvenile court cases ranging in age from nine to nineteen, and the majority of whom are adolescents of fifteen, sixteen and seventeen years. It is important to know of these cases whether they are feeble-minded, and, if not of such low mentality as to be unable to manage themselves after the stress of adolescence, what are the peculiarities of mental organization. In other words, the psychologist is called upon for a psychological explanation of the delinquency. The Binet-Simon tests are not accurate

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[1] Contributed from the Bureau of Juvenile Research, Columbus, O.

in setting apart the feeble-minded among adolescent delinquents. This is the primary reason for such very different percentages of feeble-minded among juvenile delinquents, obtained by different examiners at different places. Large numbers of our subjects yield Binet-Simon results of more than eleven but under twelve. Other large numbers test twelve flat. Are we to hew rigidly to the line of twelve years? Are all morons who do not make a score of more than twelve years? A glance at our tabulated year-scale ratings of these thousand cases shows at once how a few tenths of a year difference in the upper limit assigned for moronity will make a difference of twenty in the percentage of morons among these offenders. The percentage of moronity, however, is not the important matter to the juvenile court judge or the superintendent of the reform school. In any case feeble-mindedness is a practical concept, like insanity. It is not rigidly definable. One cannot hew accurately to the line in the one case any more than in the other. Society does not wish to confine all psychoses in hospitals for the insane, nor all morons in institutions for feeble-minded. It is only when the mental perversions or the mental deficiences underlie anti-social tendencies, and make the individual a danger or unwarranted expense to society, that he is to be made a custodial case.

What is needed in the practical dealing with the individual delinquent is accurate diagnosis and prognosis. Mental measurements, as accurate as possible, of a variety of mental performances, so these may be compared with average performances of other persons of the same age, sex, and social status, as the individual in question, are the necessary bases of such diagnosis and prognosis. It is important that the same mental performances should be measured by the same standards in all subjects in order that such comparisons may be made. After a number of seemingly normal persons at various stages of mental development have been tested, the average performance, in a given test by a given age, sex, and class, should serve as a unit of measurement of the performance of any person of the same age, sex, and class.

One can state the percentage values of his performances in all the various mental functions covered by the test. By this means a conspectus of the mental efficiency of the individual is afforded. The examiner can then see at a glance what and how great are his points of strength and weakness. Thus will come out reasons for his failure in his given environment (diagnosis) and ground for recommendations as to his future (prognosis and treatment).

The turn given to the Binet-Simon tests by Yerkes and Bridges[1] wherein they select nineteen Binet tests and add one from Wyatt, and devise a system of scoring according to the value of the performance, is founded upon true psychological insight. It provides for a fairly representative mental assessment of the capacities of each individual, and secures the means of comparing his various performances with what might reasonably be expected of a person of his age, sex, and opportunities in life.

In each of the thousand mental examinations of delinquent minors reported herewith, both a point-scale and a year-scale examination were made. The point-scale examinations were conducted throughout in conformity with the Yerkes instructions. The first seven hundred and eighty-six cases were measured by the Binet-Simon year-scale as modified by Goddard.[2] Reference for instructions was made also to the Binet-Simon reports of the tests and instructions.[3] It is so obvious from our own work that many of the Binet tests are misplaced, are too easy or too difficult for the years wherein they are placed, that we determined upon a revised year scale. The remaining two hundred and fourteen of the subjects here reported were measured by our revised year scale, as well as by the point scale. The fact of the necessity for revision of the Binet-Simon scale is neatly demonstrated by

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[1] 'A Point Scale for Measuring Mental Ability,' Yerkes, Bridges and Hardwick. Warwick & York, 1915, pp. 168.

[2] 'The Binet-Simon Measuring Scale for Intelligence,' *The Training School*, Jan., 1910, Revised 1911, Henry H. Goddard.

[3] 'A Method of Measuring the Intelligence of Young Children,' Alfred Binet and Th. Simon. Translated by Clara Harrison Town. 'Mentally Defective Children,' Alfred Binet and Th. Simon. Trans. by W. B. Drummond, London, 1914.

Thorndike.[1] He works out medians and averages for the different chronological years, from the results of Goddard, Dougherty, Terman and Childs, Strong, and Brigham. From about nine years to fourteen years the Binet age is lower than the chronological age, and these differences grow larger as age increases. By Binet measurements ten-year-olds are rated 9.8; eleven-year-olds 10.6; twelve-year-olds about 10.9; thirteen-year-olds about 11.4; and fourteen-year-olds about 11.9 years. Stern had gathered evidence on the same point at an earlier date. German and English children show the same defects in the Binet-Simon scale as do American children.[2]

Terman[3] has led out in the most decided revisions and extension of the Binet-Simon year scale. His suggestions were accepted, in very large measure, as to changes in the standards of the Binet tests, and for new tests for the adolescent period. The Knox line test standards, as revised by Pintner,[4] were adopted as follows: V. years, one of *X* and *Y*, correct; VI., one of *B*, *C*, and *D*, correct; VII., two of *B*, *C*, and *D*, correct; and X., one of *E*, *F*, and *G*, correct. These tests commend themselves, because they serve as measurements of a species of practical logic, and have no dependence upon formal education. They test attention (concentration and scope) and the capacity to get and hold a *plan* (*constructive imagination*). They also test capacity to execute the plan—*control of voluntary movements*—or manual dexterity.

When applying the Binet tests, as revised by Goddard, we made very little use of the 'XV.-year' tests, and none of the 'Adult' tests. 'Interpretation of Pictures' was scored in routine. 'Changing Clock Hands' and 'Opposites' were

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[1] 'The Significance of the Binet Mental Tests,' Edward L. Thorndike, Ph.D., *The Psy. Clinic*, VIII., Dec. 15, 1914.

[2] 'The Psychological Methods of Testing Intelligence,' Wilhelm Stern. Translated by G. M. Whipple. Warwick & York, Baltimore, 1914, p. 48.

[3] See *Journal of Educational Psychology*, 1912 (III.). The writer is greatly indebted to Professor Terman for copies of 'Record Blanks' and a 'Guide for the Use of the Stanford Revision and Extension of the Binet-Simon Measuring Scales of Intelligence.'

[4] 'The Standardization of Knox's Cube Test,' Rudolph Pintner, *The Psychological Review*, Sept. 1915, pp. 377–401.

sometimes used. Successes in any of these were commonly reckoned at 0.4 of a year each. They were thus added to the lower attainments. If the score thus mounted a fraction of a year above XII. it was generally designated merely 12.0 +. In the use of the revised year scale, the score was regularly counted, two tenths of a year for each test passed beyond the basal year, up to and including twelve years, and then four tenths of a year for each test passed. On this revised scale we had five tests for each year, III. to XII. inclusive, and five each for XIV., XVI., and XVIII.

Table I. presents in parallel columns and in year groups, the point-scale and year-scale ratings of six hundred and seventy-one delinquent boys at the Boys' Industrial School near Lancaster, Ohio. The scores are arranged within the year groups in the order of point-scale attainments. The chronological age in each case is that of the nearest birthday on the day of the examination. Thus, the XIV.-year column comprises all those between thirteen years and six months, and fourteen years and six months when examined. In general, the examinations were made soon after admission. Those year-scale ratings made by our revised year scale are marked with an asterisk. All year-scale ratings not so marked are measured by the Goddard revision of the Binet-Simon scale.

Table II. presents, in exactly the same way, the results from mental examinations of three hundred and twenty-nine delinquent girls at the Girls' Industrial Home, near Delaware, Ohio. The examinations were made, in considerable part, at both institutions, by the writer. About one half of the boys were examined by Miss Emilie M. Dietz, and about one third of the girls by Miss Alida C. Bowler. The high order of intelligence and conscientious devotion to the work contributed by each of these coworkers is gratefully acknowledged.

The most liberal criteria of feeble-mindedness accepted by so-called Binet examiners are these: All persons who are four years behind their chronological ages in mental development, and all persons who are sixteen or more, and are less than

TABLE I

POINT-SCALE SCORES OF SIX HUNDRED AND SEVENTY-ONE DELINQUENT BOYS, ARRANGED IN YEAR GROUPS IN THE ORDER OF THE SCORES, WITH THE YEAR-SCALE SCORES OF EACH INDIVIDUAL IMMEDIATELY TO THE RIGHT OF HIS P.S. SCORE

Year-scale scores, marked with asterisk (*) preceding, are by the revised year scale of the Bureau of Juvenile Research. Others by Goddard's revision of the Binet-Simon scale. The Yerkes-Bridges *averages* per year groups of Cambridge children of English-speaking parents are indicated thus (″) in the columns; P.S. scores, 20% below average, thus (=); 25% below average, thus (–); and 30% below average, thus (×).

10 Yrs.		11 Yrs.		12 Yrs.		13 Yrs.		14 Yrs.		15 Yrs.		16 Yrs.		17 Yrs.		18 Yrs.		19 Yrs.	
P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.
28	6.6	35	7.6	42	7.2	32	7.5	31	7.6	32	8.0	32	8.5	50	9.8	39	8.8	–25%–	
×30%×		38	8.2	42	8.6	40	7.4	41	8.0	35	7.8	33	6.6	50	*10.0	41	9.0	68	10.8
–25%–		×30%×		42	*8.6	41	8.6	41	8.8	36	7.8	46	8.4	51	8.6	56	9.4	68	10.8
		44	8.1	45	8.7	42	8.6	41	9.0	41	8.6	46	9.6	51	9.2	56	9.6	=20%=	
48	8.0	44	8.4	48	9.6	42	9.5	42	*8.4	45	8.4	47	9.0	51	9.8	56	9.6	78	11.7
=20%=		44	9.2	51	9.8	43	7.6	42	8.4	45	9.2	51	9.4	52	9.9	57	9.7	″Av″	
50	9.0	–25%–		×30%×		44	8.4	43	8.6	46	9.5	51	9.9	52	10.0	57	10.2		
51	9.7	49	8.6	55	10.0	44	*9.0	45	9.4	48	9.4	52	9.1	53	10.8	58	10.0		
53	10.0	49	9.0	56	*9.2	46	8.2	46	8.7	49	9.4	53	8.4	54	9.2	59	*8.8		
54	9.4	50	9.2	56	9.2	47	8.8	46	9.1	50	*7.8	53	9.4	54	9.5	59	9.4		
56	9.8	51	8.6	57	*9.4	48	9.8	47	*9.4	51	9.8	53	9.6	54	10.1	61	10.2		
59	9.2	=20%=		57	10.4	49	9.0	47	9.6	53	9.4	58	10.0	55	8.4	×30%×			
61	9.2	54	9.0	–25%–		51	9.4	48	9.6	53	9.4	×30%×		55	9.2	62	9.6		
61	9.8	55	10.1	58	9.8	51	9.8	49	9.4	53	*10.0	59	10.6	55	9.3	62	10.0		
″Av″		56	9.0	58	10.0	52	8.8	49	10.0	54	8.9	60	9.6	55	10.0	64	9.2		
69	10.8	56	10.0	58	10.0	53	9.8	51	9.6	54	9.0	60	10.4	56	9.8	–25%–			
70	10.6	58	10.1	60	9.8	54	8.6	51	9.8	54	9.2	61	10.3	56	9.9	67	11.0		
		59	9.4	60	10.2	54	10.0	52	9.2	54	9.4	62	9.3	56	10.6	68	10.4		
		60	10.3	61	10.8	54	10.2	52	9.2	54	10.3	62	9.4	57	9.3	68	10.4		
		61	10.2	=20%=		×30%×		53	9.6	55	8.5	62	10.2	57	9.4	68	10.6		
		61	10.5	62	10.0	59	9.6	53	9.8	55	9.0	62	10.2	57	9.6	68	10.8		
		62	10.5	63	9.6	59	10.1	55	9.0	55	9.2	62	10.4	57	*10.6	68	10.9		
		″Av″		63	10.1	59	10.2	55	10.1	55	9.9	–25%–		58	9.7	68	11.1		
		65	10.4	63	10.4	59	10.3	56	9.6	56	10.0	63	9.5	58	9.7	69	10.6		
		65	10.5	65	9.6	–52%–		×30%×		57	9.2	63	10.0	58	9.8	69	11.0		
		66	10.7	65	11.0	61	9.4	57	9.4	57	9.6	63	10.2	58	10.4	69	*11.8		
		67	9.8	66	10.0	61	*10.0	57	9.5	57	9.6	63	10.6	58	10.9	70	10.8		
		71	10.7	66	10.2	61	10.1	58	9.2	57	9.6	63	10.7	59	11.0	70	11.0		
		73	10.6	67	10.5	63	9.6	58	9.6	57	9.8	63	10.9	×30%×		70	11.4		
		74	10.7	67	11.0	=20%=		58	*10.0	×30%×		64	*10.0	62	10.0	70	*12.0		
				69	*11.6	64	10.7	58	*11.4	58	10.0	64	*10.0	62	10.4	=20%=			
				71	10.4	64	10.7	59	10.0	59	10.4	64	10.6	62	10.8	71	10.7		
				71	11.2	65	10.8	59	10.2	59	10.6	64	10.6	63	10.1	71	10.8		
				71	11.4	65	*11.0	60	9.4	59	10.7	65	10.0	63	11.0	72	10.7		
				72	10.8	66	10.1	60	9.9	60	9.8	65	11.2	64	9.6	72	11.4		
				75	10.8	68	10.7	60	10.6	60	10.3	65	11.2	64	10.2	74	11.2		
				76	10.2	68	10.8	–25%–		60	10.8	66	10.2	64	10.4	75	11.2		
				76	11.7	68	*11.0	61	10.4	61	9.8	66	10.9	64	10.4	75	11.3		
				″Av″		69	10.4	61	10.8	61	9.8	67	10.5	64	10.6	75	11.7		
				77	10.6	70	10.6	62	9.8	61	10.0	67	11.2	64	10.6	77	11.6		
						70	10.7	62	10.0	61	10.2	=20%=		64	10.6	77	12.0+		
						70	11.0	62	10.0	–25%–		68	10.4	64	10.7	78	11.2		
						70	11.0	63	9.8	62	10.0	68	10.6	64	10.9	78	11.6		

TABLE I—*Continued*

10 Yrs.		11 Yrs.		12 Yrs.		13 Yrs.		14 Yrs.		15 Yrs.		16 Yrs.		17 Yrs.		18 Yrs.		19 Yrs.	
P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.
						70	11.0	64	10.2	62	10.0	68	10.5	−25%−		79	11.5		
						72	10.4	64	10.6	62	10.4	68	10.9	65	10.6	82	11.0		
						72	10.8	64	*10.6	63	9.7	69	10.2	66	9.6	82	11.5		
						73	11.3	64	*10.6	63	9.9	69	10.3	66	9.6	82	11.8		
						73	11.5	64	*10.8	63	10.0	69	10.4	66	10.4	83	11.0		
						73	11.6	=20%=		63	10.0	69	10.8	67	9.8	83	12.0+		
						74	11.2	65	10.7	63	10.2	70	10.3	67	9.8	85	11.2		
						75	11.0	66	9.8	63	*10.4	70	10.6	67	10.5	85	11.7		
						75	11.6	66	9.8	63	10.9	70	10.7	68	9.7	85	12.0		
						78	11.5	66	11.3	64	9.2	70	11.0	68	10.4	86	11.6		
						78	*12.2	67	10.2	64	10.1	71	9.8	68	10.4	86	12.0		
						"Av"		67	10.4	64	10.4	71	10.4	=20%=		"Av"			
						79	11.0	67	10.4	65	10.0	71	10.4	69	10.4	88	11.8		
						79	11.6	67	10.6	65	10.0	71	10.6	69	10.9	88	12.0+		
						81	11.2	68	9.8	=20%=		72	10.8	69	11.1	89	11.8		
						81	11.4	68	10.2	66	9.8	72	11.0	69	11.7	89	*14.4		
						81	11.4	68	10.4	66	10.0	72	11.0	70	10.5	90	11.6		
						86	11.7	69	11.2	66	10.4	72	11.0	70	11.2	90	12.0+		
						93	12.0+	69	11.6	66	10.4	72	11.7	70	11.2	90	12.0+		
								69	*12.4	66	10.4	73	11.0	71	10.7	90	12.0+		
								70	10.4	66	10.8	73	11.0	71	11.0	91	*16.4		
								70	10.8	67	9.8	73	11.6	71	11.4	92	12.0		
								70	11.4	67	10.6	74	10.4	72	10.4	92	12.0+		
								71	10.2	68	*9.8	74	10.6	72	11.0	93	12.0+		
								71	11.0	68	10.4	74	10.6	72	11.0	94	12.0+		
								71	11.0	68	10.4	74	11.2	72	11.2	96	12.0		
								71	11.4	68	10.4	75	11.2	72	11.2	97	12.0+		
								72	10.6	68	10.6	75	11.5	72	11.6	97	12.0+		
								73	11.2	68	10.6	75	12.0	73	11.6				
								74	11.0	69	9.7	76	11.4	74	11.1				
								74	11.2	69	10.1	76	11.0	74	11.2				
								76	10.4	69	10.7	77	11.4	75	11.0				
								76	10.6	69	10.8	77	11.7	75	11.6				
								76	11.1	69	11.0	78	11.2	76	10.4				
								77	11.7	69	11.1	78	11.8	76	11.1				
								77	12.0	71	9.8	79	10.6	76	11.4				
								78	11.4	71	10.8	79	10.6	76	11.4				
								79	11.2	72	10.2	79	12.0	76	*11.6				
								79	11.2	72	10.6	79	12.0	76	11.6				
								80	10.6	72	11.0	80	11.2	76	11.6				
								80	10.8	73	10.4	80	11.4	76	11.6				
								80	11.4	73	10.9	80	11.6	76	11.6				
								80	11.4	73	11.0	81	11.2	76	11.7				
								"Av"		73	11.1	81	11.2	77	11.5				
								82	11.4	73	11.2	81	11.4	77	11.6				
								83	11.2	73	*11.4	81	11.6	77	11.7				
								83	11.2	73	11.5	82	11.6	78	10.8				
								83	11.6	74	11.6	82	11.7	78	11.2				
								83	11.6	75	10.6	83	11.0	78	11.6				
								83	*13.2	75	11.0	83	11.6	78	*12.8				
								85	11.9	75	*12.6	83	12.0	79	10.9				
								87	11.4	76	10.8	83	12.0	79	11.0				
								88	12.0+	76	11.2	83	12.0	79	11.2				
								92	*14.0	76	11.6	83	12.0+	79	11.4				

TABLE I—*Continued*

10 Yrs.		11 Yrs.		12 Yrs.		13 Yrs.		14 Yrs.		15 Yrs.		16 Yrs.		17 Yrs.		18 Yrs.		19 Yrs.	
P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.
								93	12.0+	76	11.7	83	*12.8	79	11.4				
										77	11.2	"Av"		79	11.6				
										77	11.2	84	11.4	80	11.6				
										77	11.4	84	12.0+	80	11.6				
										78	11.2	84	12.0+	80	11.9				
										78	11.6	85	11.4	80	*12.8				
										78	*12.8	85	12.0	81	11.4				
										79	10.8	85	12.4	82	11.0				
										79	10.8	85	12.4	82	11.4				
										79	11.2	86	11.4	82	11.8				
										79	11.4	86	11.6	82	11.9				
										79	11.6	86	11.8	82	12.0				
										80	11.4	87	12.0	82	12.0+				
										80	11.4	87	12.0+	83	11.9				
										80	11.6	89	12.0	83	12.0				
										80	12.0	90	12.0	84	11.4				
										81	11.0	90	12.0+	84	11.6				
										81	11.4	91	12.0	84	12.0+				
										81	11.4	92	11.4	85	12.0				
										81	11.4			85	12.0				
										81	11.7			85	12.0+				
										"Av"				"Av"					
										82	11.2			86	11.4				
										82	11.6			88	11.8				
										82	11.9			88	11.9				
										82	12.0			89	12.0+				
										83	11.2			89	*14.8				
										83	12.0			90	12.0				
										84	11.2			90	12.0+				
										84	11.5			90	12.0+				
										84	12.0+			91	12.0				
										84	12.0+			91	12.0+				
										86	11.6			91	*13.4				
										87	11.6			93	11.6				
										88	11.6								
										88	11.6								
										88	12.0								
										88	12.0+								
										88	12.0+								
										89	11.4								
										89	11.6								
										91	11.6								
										94	12.0+								

twelve in mental development, are classed as definitely feeble-minded. The rigid application of these criteria of feeble-mindedness shows five hundred and seventy of these thousand delinquents to be feeble-minded. Had we examined the last two hundred and fourteen also by the Goddard revision of the Binet scale, we should probably have a yet larger percentage of feeble-minded. The actual numbers

TABLE II

POINT-SCALE SCORES OF THREE HUNDRED AND TWENTY-NINE DELINQUENT GIRLS, ARRANGED IN YEAR GROUPS IN THE ORDER OF THE SCORES, WITH THE YEAR-SCALE SCORES OF EACH INDIVIDUAL IMMEDIATELY TO THE RIGHT OF HER P.S. SCORE

Year-scale scores, marked with asterisk (*) preceding, are by the revised year scale of the Bureau of Juvenile Research. Others by Goddard's revision of the Binet-Simon scale. The Yerkes-Bridges *averages* per year groups of Cambridge children of English-speaking parents are indicated thus (″) in the columns; P.S. scores, 20% below average, thus (=); 25% below average, thus (–); and 30% below average, thus (×).

10 Yrs.	11 Yrs.	12 Yrs.	13 Yrs.	14 Yrs.	15 Yrs.	16 Yrs.	17 Yrs.	18 Yrs.	19 Yrs.
P.S. Y.S.	P.S. Y.S.	P,S. Y.S.	P.S. Y.S.	P.S. Y.S.	P.S, Y.S.	P.S, Y.S.	P.S. Y.S.	P.S. Y.S.	P.S. Y.S.
39 7.6	=20%=	36 7.2	45 *8.4	38 9.2	36 *7.6	20 *5.8	35 *7.4	47 8.0	47 *8.6
×30%×	53 *9.2	×30%×	50 *9.0	40 *7.8	38 7.9	38 *6.8	47 *8.4	47 *8.8	54 9.3
	59 *9.0	−25%−	×30%×	43 *7.8	38 8.8	38 *8.0	49 *9.2	52 *9.2	58 9.7
	″Av″	=20%=	58 9.4	44 *9.4	45 *9.6	40 *6.8	50 8.8	55 9.7	×30%×
		65 *10.4	58 *10.0	45 *8.0	48 9.0	41 8.9	50 9.0	55 9.9	
		73 11.0	59 9.8	47 *7.8	48 *9.2	46 *8.8	50 9.2	56 9.3	
		″Av″	59 *10.0	48 9.0	49 *9.8	48 9.1	51 *8.6	58 *9.6	
		80 *11.2	−25%−	51 8.3	50 *8.4	49 *8.4	54 *9.1	58 9.7	
			60 9.4	52 8.8	50 9.7	50 *9.6	55 9.7	59 9.8	
			60 *10.6	54 *9.2	53 *9.6	51 9.0	55 *9.0	×30%×	
			61 *9.4	55 9.4	55 9.5	54 9.4	57 *8.8	65 *11.2	
			=20%=	56 *9.6	56 *10.8	56 10.2	57 9.8	−25%−	
			64 *9.6	56 *10.0	57 9.5	×30%×	58 9.6	66 *11.0	
			64 *12.0	×30%×	57 * 9.8	60 *10.4	58 10.3	67 *10.6	
			65 *11.6	58 *10.8	×30%×	61 9.5	59 *9.2	67 *12.0	
			71 11.3	59 *9.4	58 *10.4	61 *10.0	×30%×	68 *11.2	
			73 *12.0	60 *10.6	60 9.7	61 *10.4	61 10.0	68 *11.4	
			76 *10.6	−25%−	60 *9.8	62 9.5	62 *9.6	70 *11.2	
			″Av″	61 9.0	60 10.5	62 9.9	62 *9.6	=20%=	
			82 12.0+	61 11.0	61 10.0	62 10.7	63 9.2	71 *10.2	
			84 12.0+	61 11.1	61 10.2	−25%−	64 9.8	71 *11.8	
				62 *10.0	61 10.6	63 10.1	64 *10.0	72 *12.0	
				63 *10.6	−25%−	63 *10.6	64 11.5	73 11.5	
				63 11.0	62 *9.4	64 10.8	−25%−	79 *13.4	
				=20%=	62 10.1	64 *12.8	65 10.6	80 *11.8	
				67 *9.6	63 *9.8	65 11.0	66 9.8	81 12.0+	
				67 *10.8	63 10.1	65 *12.2	66 *11.0	83 *12.0	
				68 10.5	63 10.2	66 *11.0	66 *11.2	83 *12.8	
				68 *11.0	63 *10.8	=20%=	67 *10.6	85 *13.4	
				68 11.3	63 10.8	68 *10.2	67 11.0	86 12.0	
				70 *11.4	64 *10.2	68 10.8	68 *10.2	86 12.0	
				72 9.8	64 *11.2	68 11.0	68 *12.2	87 *13.2	
				72 11.0	65 9.6	69 10.0	=20%=	″Av″	
				72 *12.8	65 *10.6	69 *10.6	69 *11.0	89 *14.6	
				74 *10.6	65 11.2	71 11.2	71 *11.0	90 12.0	
				74 11.0	=20%=	72 11.0	71 *11.4	90 *14.8	
				75 *10.8	66 10.7	72 11.5	73 *11.2	92 12.8	
				75 *11.2	67 *10.2	73 11.1	75 11.5		
				76 *13.2	67 *11.2	73 *12.1	75 *11.6		
				77 *11.6	68 *10.6	74 *10.8	75 *12.1		
				78 11.2	69 10.2	74 11.4	75 *13.4		
				79 *12.2	69 10.4	74 11.4	76 *10.8		
				79 *12.8	69 *10.8	74 *12.4	76 *11.6		

Table II—*Continued*

10 Yrs.		11 Yrs.		12 Yrs.		13 Yrs.		14 Yrs.		15 Yrs.		16 Yrs.		17 Yrs.		18 Yrs.		19 Yrs.	
P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.	P.S.	Y.S.
								80	11.5	70	11.1	76	11.2	77	11.6				
								"Av"		70	*12.4	76	*12.0	77	11.7				
								83	*12.8	73	11.1	77	12.0	77	12.0				
								83	*14.0	73	11.2	77	12.0+	78	11.8				
								84	12.0+	73	11.5	77	*12.1	78	*12.2				
								86	12.0+	73	*11.6	77	*12.6	79	*13.8				
										73	11.7	78	11.7	80	*11.8				
										73	*12.0	78	11.8	80	12.0				
										73	*12.0	79	11.4	80	*12.6				
										74	*11.9	79	11.4	80	*12.6				
										74	12.0	79	12.0	81	11.3				
										75	*10.8	79	*14.0	81	12.0				
										76	12.0+	80	11.3	81	12.0+				
										77	11.1	80	*11.8	81	12.0+				
										77	11.7	80	12.0	81	*13.4				
										77	12.0	80	12.0+	83	12.0+				
										77	*12.4	81	11.1	83	*14.4				
										78	11.0	81	*13.4	84	12.1				
										78	*12.6	82	11.8	85	*14.0				
										79	*14.2	82	12.0	"Av"					
										80	*12.8	82	12.0+	86	12.0+				
										80	*13.2	82	12.0+	86	12.0+				
										81	11.5	82	*13.2	87	*13.8				
										81	12.0+	83	11.8	88	*13.6				
										81	*12.2	83	*12.0	88	*14.0				
										"Av"		"Av"		90	11.0				
										82	*12.4	84	*11.2	90	11.4				
										83	12.4	84	11.7	94	12.0+				
										83	*13.8	85	11.7	97	*15.2				
										84	*12.0	85	12.0+						
										84	*12.6	85	12.0+						
										85	*14.4	85	*13.2						
										87	12.0	86	*16.0						
										88	12.0+	87	12.0+						
										88	12.0+	87	12.0+						
										88	*12.8	90	12.0+						
										89	*14.6	90	*13.2						
										95	*14.6	90	*13.4						
												91	12.0+						
												94	*14.0						

thus classed as feeble-minded in each year and sex group, together with percentages of feeble-minded in each group, are given in Table V. Earlier examinations of delinquents at these same institutions are stated by Goddard[1] to have yielded seventy per cent. feeble-minded. By the same table several other reform schools have been found to have eighty per cent. and more, of feeble-minded. No one can doubt

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[1] 'Feeble-Mindedness; Its Causes and Consequences,' Henry Herbert Goddard, Ph.D., New York, 1914, p. 9.

the average mental caliber of the population of one reform school differs widely from that of another, and that the mentality of the same school will differ from one year to another. But such a general prevalence of feeble-mindedness among delinquents, almost identifying delinquency with feeble-mindedness, certainly demands the application of some critical judgment, and serious inquiry as to what is really meant by feeble-mindedness. The showing already made by Thorndike and Stern, as to the inaccuracy of Binet ratings of mentality above nine years, and the question as to the meaning of *twelve-year mentality* in an adult, whether or not this is the safely practical line of demarcation between the feeble-minded and those who can efficiently manage themselves, have likewise prepared us to be suspicious of such large percentages of *feeble-minded* among delinquents.

TABLE III

GIVES NUMBERS, AVERAGES AND MEDIANS FOR EACH YEAR GROUP OF (1) BOYS, (2) GIRLS, AND (3) BOYS AND GIRLS, AND (4) NUMBERS AND AVERAGES FOR YEAR GROUPS OF THE YERKES-BRIDGES CHILDREN OF ENGLISH-SPEAKING PARENTAGE

Age	Ohio Delinquent Males			Ohio Delinquent Females			Ohio Delinquents Male and Female			Yerkes-Bridges Male and Female English	
	No.	Aver.	Median	No.	Aver.	Median	No.	Aver.	Median	No.	Aver.
4	...	...	...	...	...	...	...	...	...	3	17
5	...	...	...	...	...	...	...	...	...	28	22
6	...	...	...	...	...	...	...	...	...	55	29
7	...	...	...	...	...	...	...	...	...	48	35
8	...	...	...	...	...	...	...	...	...	47	41
9	...	...	...	...	...	...	...	...	...	43	56
10	12	55.8	55.0	1	39.0	39.0	13	54.5	54.0	53	62
11	26	56.4	57.0	2	56.0	56.0	28	56.4	57.0	55	65
12	36	61.4	62.5	4	63.5	69.0	40	61.6	63.0	40	77
13	58	62.7	64.5	17	64.1	61.0	75	63.0	64.0	43	79
14	94	64.7	65.5	45	64.5	67.0	139	64.6	66.0	37	81
15	136	68.2	68.0	77	68.3	69.0	213	68.2	69.0	16	82
16	112	71.4	71.0	79	71.1	74.0	191	71.3	73.0	...	...
17	127	71.0	72.0	68	71.0	74.0	195	71.0	72.0	...	...
18	67	75.0	75.0	33	71.4	71.0	100	73.8	72.5	...	...
19	3	71.3	68.0	3	53.0	54.0	6	62.2	63.0	...	...
Totals	671	...	...	329	...	...	1,000	...	...	468	...

Turning now to the consideration of the point scale readings of these delinquents, let us consider first the averages and medians of their point-scale scores for year and sex groups, and the distribution of the cases above and below these, and the relations of these averages to the averages of Cambridge school children. These averages and medians are presented in Table III.

In the years directly comparable with the Yerkes-Bridges results (10 to 15 inclusive) the averages of the delinquents are, for boys and girls alike, considerably lower than the averages for Cambridge school children. Assuming norms of 84, 86, 88, and 88, for the years 16, 17, 18, and 19, respectively, as seems reasonable from Yerkes's report of examinations of mill operatives,[1] we find that these years' averages are likewise considerably lower for the delinquents. Taking the averages of boys and girls together, the year groups of delinquents from 10 to 19 inclusive, show retardations, year by year, of 7½, 9, 15, 16, 16½, 14, 12½, 15, 14, and 26 points. In other words, the 10-year-olds score points for a trifle under nine; the eleven, for about nine; the twelve, for about ten; the thirteen, for ten and one third; the fourteen, for eleven; the fifteen, for eleven and one third; the sixteen and seventeen, for eleven and one half; the eighteen, for eleven and three fourths years. This signifies that our group of delinquents is loaded with feeble-minded, considerably beyond the load carried by the average school population.

The comparisons of boys and girls do not result in any decided superiority for either sex at any age. This also holds in spite of the fact that nearly three times as many boys as girls are committed by the same courts every year. By averages, the girls have a slight superiority at twelve and thirteen, and the boys at eighteen. The figures are very nearly the same for years fourteen to seventeen, inclusive, and for eleven. Groups are too small, for years ten and nineteen, to signify anything. The medians suffice only to show there is nothing significant in these differences in averages for the sex groups. Another piece of evidence indicating the weighting or loading of these delinquent

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[1] See p. 90 of 'A Point Scale for Measuring Intelligence.' Twenty-five mill operatives of ages from 17 to 27, average 88.3 points.

groups with the feeble-minded, emerges from a comparison of the numbers of boys and girls among delinquents, and in the Yerkes-Bridges findings, who are more than 25 per cent. above, and more than 25 per cent. below the averages for their particular year groups. The material for this comparison is presented in Table IV.[1]

While the Cambridge school children from English-speaking homes yield the same numbers (46), 25 per cent. below average, and 25 per cent. above average, the delinquent minors under consideration yield 102 25 per cent. or more below the averages of their respective year groups, and only 76 who are 25 per cent. or more above the averages for their groups. Further, the six eighteen-year-olds are all low and yield an average quite out of the line of progression of the averages of the earlier year groups. Taking the seventeen-year average as appropriate for 18 years, these figures change to 75 and 104.

These two lines of evidence from the side of point-scale scores—lower year group averages than those of Yerkes and Bridges, and the much larger numbers of scores 25 per cent. below the averages for their groups than scores 25 per cent. above same—taken in connection with the Binet findings, certainly leave no room for doubt that the groups under consideration are overloaded with individuals of poor mental endowment. These reform-school populations are *selected.* Among them, *lower than average* mental equipment occurs much more frequently than in unselected groups.

In order to assess the point-scale findings in such a population we should compare the scores with averages from typical unselected groups. Such a group we have in the four hundred and sixty-eight Cambridge school children from homes where English is spoken. The question immediately arises, when we set about such an assessment of the mental ability of any given individual, What amount of retardation signifies a mental deficiency such as will make it impossible for the individual to manage himself within the lines of propriety as a self-sustaining member of society?

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[1] See 'A Point Scale for Measuring Mental Ability,' pp. 56 and 57.

TABLE IV

GIVES, UNDER AGE AND SEX, POINT-SCALE SCORES OF ALL INDIVIDUALS OF CAMBRIDGE SCHOOL CHILDREN, AND OF DELINQUENT MINORS, WHICH ARE 25% OR MORE ABOVE AND 25% OR MORE BELOW THE AVERAGES OF THEIR RESPECTIVE AGE GROUPS

Age.	4		5		6		7		8		9		10		11		12		13		14		15		16		17		18		19		Totals
Sex.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	
English-speaking Cambridge children 25% or more *above* the average.[1]	23	..	26	30	36	38	41	47	57	50	70	70	84	77	84	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	46
	..	..	31	34	38	39	41	48	58	52	71	74	..	77	85	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
	..	..	..	35	38	44	42	48	..	56	..	77	..	79	89	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
	..	..	..	..	39	44	42	49	..	57	..	..	..	80	93	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
	..	..	..	..	39	..	43	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
	..	..	..	..	47	..	45	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
English-speaking Cambridge children 25% or more *below* the average.[1]	12	..	11	11	11	13	14	21	21	15	32	21	44	39	45	41	53	..	..	38	61	54	..	55	..	..	..	..	..	..	..	..	46
	..	..	15	13	15	15	17	23	30	25	..	38	48	40	48	..	56	..	..	46	..	..	..	..	..	..	..	..	..	..	..	..	
	..	..	..	18	21	21	20	25	30	25	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
	..	..	..	..	..	22	23	..	31	29	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
	..	..	..	..	..	..	23	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
Delinquent minors 25% or more *above* the averages for delinquents.	..	..	..	..	..	..	..	..	..	..	..	..	69	..	71	..	77	80	79	82	82	80	86	87	90	90	89	90	93	..	78	..	76
	..	..	..	..	..	..	..	..	..	..	..	..	70	..	73	..	..	..	79	84	83	83	87	88	90	90	89	90	94	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	74	..	..	..	81	..	83	83	88	88	91	90	90	94	96	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	81	..	83	84	88	88	92	91	90	97	97	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	81	..	83	..	88	89	..	94	90	..	97	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	86	..	83	..	88	95	..	..	91	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	93	..	85	..	88	..	..	..	91	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	87	..	89	..	..	..	91	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	88	..	89	..	..	..	93	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	92	..	91	..	..	..	..	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	93	..	94	..	..	..	..	..	..	..	..	..	

TABLE IV—*Continued*

Age. . .	4		5		6		7		8		9		10		11		12		13		14		15		16		17		18		19		Totals
Sex . . .	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	
Delinquent minors 25% or more *below* the averages for delinquents.	..	..	..	..	..	..	..	..	..	..	..	..	28	39	35	..	42	36	32	45	31	38	32	36	32	20	50	35	39	47	..	..	102
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	38	..	42	..	40	..	41	40	35	38	33	38	50	47	41	47	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	42	..	41	..	41	43	36	38	46	38	51	49	..	52	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	45	..	42	..	41	44	41	45	46	40	51	50	..	55	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	42	..	42	45	45	48	47	41	51	50	..	55	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	43	..	42	47	45	48	51	46	52	50	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	44	..	43	48	46	50	51	48	52	51	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	44	..	45	..	48	50	52	49	53	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	46	..	46	..	49	..	53	50	..	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	47	..	46	..	50	..	53	51	..	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	47	..	51	..	53	..	..	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	47	..	..	..	..	..	..	..	..	..	..	..	
	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	48	..	..	..	..	..	..	..	..	..	..	..	

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[1] Figures taken from tables on pp. 56 and 57 of 'A Point Scale for Measuring Mental Ability.'

The *coefficient of mental ability*, obtained by dividing a given score by the average for the age and sex group to which the individual belongs, affords the readiest means of stating the mental status of an individual by the point-scale method. The question then is, What coefficient of mental ability (C.M.A.) signifies such mental deficiency as makes it inevitable that the individual will be an expense to others, and a social offender, if allowed at large?

We have referred above to mental traits not comprised in the Yerkes-Bridges point scale, which are determining factors in efficiency and in safeguarding against social offenses. Attachment to persons, ability to foresee the approvals and disapprovals of others, and the impingement of such motives upon behavior, are examples of such factors. However, the point scale gives us a preliminary survey of mental ability, alike for all, and we must try out some C.M.A. as a limit for inefficient mentality. The limit commonly used in the method of right and wrong cases, of seventy-five per cent., is suggested. When absolute chance (an unknown number of unknown factors) prevails, as in flipping pennies, fifty per cent. of each of two possible results ensues. When one constantly operating cause prevails, one hundred per cent. of the looked-for result ensues. Seventy-five per cent. of cases of a given result indicate the elimination of pure chance, and the operation of some fixed operating factors under the conditions prevailing, even though they may be undiscovered. So, until practical experience proves us wrong, we may assume that individuals scoring seventy-five per cent. or less of the average scores of their age groups, are so poorly endowed that they are to be classed as feeble-minded. We name that figure, however, with full consciousness that this practical concept (feeble-mindedness) must be marked off by the point scale, through the practical demonstration of what hundreds of individuals, who prove themselves socially inadequate, do and have scored, by this Yerkes-Bridges point scale, and by yet more thorough and adequate point scales for measuring intelligence. Seventy-five per cent. is merely proposed that we may see how it will work. We give, in addition, in Tables I., II., and V., the 70 per cent. and 80 per cent. levels.

TABLE V

SEX AND AGE GROUPS OF DELINQUENT MINORS TESTED. NUMBERS OF EACH GROUP SCORING (1) AT OR ABOVE YERKES-BRIDGES AVERAGES, (2) 20% OR MORE BELOW THESE AVERAGES, (3) 25% BELOW, AND (4) 30% BELOW. (5) NUMBERS OF FEEBLE-MINDED BY THE YEAR-SCALE MEASUREMENT, AND OF THESE (6) THE NUMBERS WHO SCORE ABOVE YERKES-BRIDGES AVERAGES BY P.S. AND (7) NUMBERS WHO SCORE BY P.S. HIGHER THAN 75% OF YERKES-BRIDGES AVERAGES. (8) NUMBERS OF THOSE WHO ARE NOT FEEBLE-MINDED BY YEAR SCALE, AND YET SCORE LESS THAN 75% OF YERKES-BRIDGES AVERAGES

Yerkes-Bridges Averages to 15, and Norms Assumed from Scores of Mill Operatives, 16 to 19 Years	(62)		(65)		(77)		(79)		(81)		(82)		(84)		(86)		(88)		(88)		Totals	Per Cent.
Age	10		11		12		13		14		15		16		17		18		19			
Sex	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.		
Numbers of individuals examined	12	1	26	2	36	4	58	17	94	45	136	77	112	79	127	68	67	33	3	3	1000	
1. Numbers at or *above* Yerkes-Bridges averages	2	0	7	0	1	1	7	2	11	4	21	12	17	14	12	9	16	4	0	0	140	14.0
2. Numbers *20% below* Yerkes-Bridges averages	2	1	9	0	17	1	27	9	46	22	55	33	38	26	52	30	28	16	2	3	417	41.7
3. Numbers *25% below* Yerkes-Bridges averages	1	1	5	0	11	1	23	6	35	16	40	21	21	19	42	22	14	10	0	3	291	29.1
4. Numbers *30% below* Yerkes-Bridges averages	1	1	2	0	6	1	19	2	24	13	29	14	12	12	28	15	11	9	0	3	202	20.2
5. Numbers feeble-minded by year scales	0	0	0	0	1	1	12	2	37	18	86	39	93	50	110	41	48	19	3	3	563	56.3
6. Numbers feeble-minded by year scales, and above Yerkes-Bridges averages	0	0	0	0	0	0	0	0	0	0	0	0	6	3	4	2	2	0	0	0	17	1.7
7. Numbers feeble-minded by year scales and less than 25% below Yerkes-Bridges averages	0	0	0	0	0	0	0	0	7	4	48	18	72	31	68	22	34	9	3	0	316	31.6
8. Numbers 25% or more below Yerkes-Bridges averages, and not feeble-minded by year scales	1	1	5	0	10	0	11	4	4	2	0	0	0	0	0	0	0	0	0	0	38	3.8

On the whole, a percentage of average mental attainment would seem a much more definite and satisfactory means of stating the mental development of an individual than is that of the years of childhood to which his mentality is equivalent. It gets over the marked unfairness to an adolescent whose four years of retardation are by no means an equivalent to the four years backwardness of an eleven-year-old who tests seven. On the other hand, this latter method has a practical intelligibility which commends it as a means of classifying and conceiving the mental status of imbeciles and low-grade morons, which will probably lead to its retention for a time even in connection with point-scale measurements.

Further, it is yet to be shown by practical studies, how much the actual place of the growing individual in the physical developmental process has to do with his prognosis. It may readily be that twenty-five per cent. deficiency of a seventeen-year-old means an unrecoverable retardation, permanently unfitting him for self management, while in a ten-year-old perhaps a thirty per cent. retardation of mental equipment may be recoverable, because of the greater growth of structure and consequent development of functional capacity, which may lie before him. Certainly *physiological age* must be taken into consideration in making diagnoses and prognoses of boys and girls from eleven to fifteen years of age. The child whose growth has been checked by unfavorable vital conditions has doubtless a fair prognosis when these conditions are made good, even though the mental showing makes him out feeble-minded. Again, in our American cosmopolitan, urban populations, racial antecedents are important factors in determining mental growth. These must be studied and given weight in making prognoses.

Table V. exhibits the incidence of feeble-mindedness, judged by year-scale standards. It will be observed that while five hundred and sixty-three of the thousand delinquents are either four years retarded mentally, or, if sixteen or above, test less than twelve mentally by the Binet or revised year-scale, three hundred and sixteen of these make scores, by the point scale, which are seventy-five per cent. or more of the Yerkes-Bridges averages for their respective ages. Seventeen of those who are feeble-minded, as judged by these year-scale standards, are even at or above the Yerkes-Bridges averages. This certainly directs suspicion at the year-scale methods of measuring the mental equipment of these adolescents. These seventeen cases all occur among sixteen-, seventeen- and eighteen-year-olds. Point-scale testing serves at least to put over into the class of "doubtfuls" more than half of the feeble-minded delinquents of the reform schools, as these have been previously rated by some examiners. Most likely they are dull and not interested in subjects of formal education.

By assuming a limit of seventy-five per cent. of average performances, as marking off the feeble-minded, we find two hundred and ninety-one, or twenty-nine per cent. of the one thousand subjects, who are feeble-minded by point-scale measurements. Of these, there are thirty-eight—all less than fifteen years of age—who do not fall four years below chronological age in mentality, as measured by year-scales. The place of incidence of this difference indicates that four years retardation is more than twenty-five per cent. retardation in late childhood and pubescence. This it is in fact. Twenty-five per cent. subtracted from any average score in this period gives more than the score of four years earlier. The twenty-five per cent. retardation turned into years, varies from one to three and a half. There are, thus, two hundred and fifty-three, or twenty-five per cent. of all subjects, who are both twenty-five per cent. retarded and either four years retarded or under twelve years mentally if actually sixteen or more years of age. A very large percentage of this group will no doubt prove definitely and permanently feeble-minded by the practical tests of life. They *may* all prove so. Some few outside of this twenty-five per cent. will probably prove feeble-minded by practical testing out. One must, by all means, be conservative in declaring individuals feeble-minded. Great injury is done to the individual, to society, and to this field of practical psychology, by calling feeble-minded any individual who later proves himself capable of earning his own living and managing himself in an inoffensive manner.

The point scale has furnished an expeditious means of marking off the group among juvenile offenders (relatively much smaller than figures current from many quarters), in which will be found nearly all of those who will prove to be feeble-minded. It prepares the way for psychological clinicians to arrive quickly at close quarters with the problems of the relationship of feeble-mindedness with delinquency, and to help judges and other arms of the social service to deal with the individual offender on the basis of his actual mental capacity, his potentialities for social living, the real stuff that is in him for the making of personality.

The fact that there are so many school children (forty-three in four hundred and sixty-eight) who are twenty-five per cent. or more below the averages for their groups, makes one cautious about expecting all who are twenty-five per cent. retarded to prove feeble-minded. The conditions of retardation and the state of physical development must be weighed in each case.

That only fourteen per cent. (one hundred and forty) of these individuals measure at or above the averages for their age groups, indicates again the heavy incidence of mental abnormality. In connection with the above facts and discussion, this also indicates that over fifty per cent. of the minor delinquents are retarded, peculiar, or subnormal, children and adolescents, who present puzzling problems to the medico-social service. To call them feeble-minded is simply to add to the cloudiness and confusion of the situation, and to stultify this branch of medico-psychological work.

The relationship of the *four years retardation* and the *twenty-five per cent. retardation* to each other, and the distribution of these thousand delinquents with respect to both of these standards and to the average attainments of Cambridge school children are exhibited in Fig. 1. The upper curve is that of average attainment. The next below at the left is that of twenty-five per cent. below average attainment.

The lowest at left is that of four years below average attainment. These last two cross at 13.7 years. For younger children four years retardation is greater than twenty-five per cent. Children older than 13.7 who are twenty-five per cent. retarded are more than four years retarded.

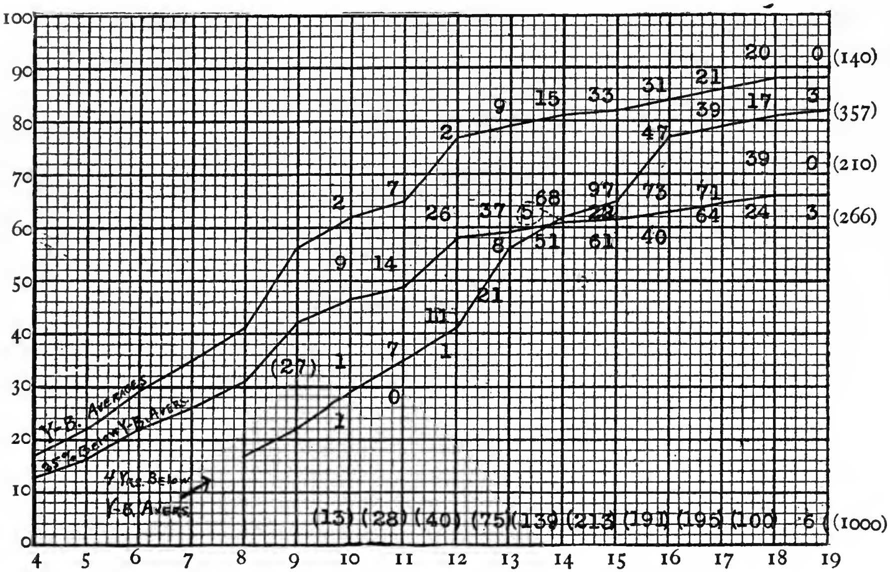

FIG. 1. Ordinates = points of score. Abscissae = age in years.

Upper curve shows the Yerkes-Bridges *average* attainment by years, of children of English-speaking parents.

The curve starting at left next below this, shows the *twenty-five per cent. below average* Y.-B. attainment of children of English-speaking parents.

The curve starting lowest at the left shows the *four years below average* Y.-B. attainments of children of English-speaking parents.

The actual numbers in each group are inserted of those (1) above average (140); (2) retarded less than four years and less than twenty-five per cent. (357); (3) retarded less than four years *but* more than twenty-five per cent. (27); (4) retarded more than four years *but* less than twenty-five per cent. (210); and (5) retarded more than four years and more than twenty-five per cent. (266).

There are fourteen per cent. (140) above average, as noted above; thirty-five and seven tenths per cent. (357) are under average but are neither twenty-five per cent. nor four years retarded; twenty-six and six tenths per cent. (266) are both four years and twenty-five per cent. retarded. Twenty-one per cent. (210) who are fourteen or more in actual age, test less than twenty-five per cent. retarded but more than four years retarded. This group is a substantial part of the approximately thirty-one per cent. of the thousand (316) who are mentally deficient according to year-scale standards, but are not so if we consider seventy-five per cent. of average point-scale performance as passing for normal. The ten per cent. have slipped over by virtue of the more accurate measuring of the point-scale.

The group of children of thirteen years or less who are not four years retarded but are more than twenty-five per cent. below the average, is small. There are twenty-seven such. This is seventeen per cent. of the total number examined of thirteen years or less. It is therefore about as important a group as that just considered which constitutes twenty-five per cent. of those fourteen or more years of age.

For the present it is important to use the year-scale in addition to the point-scale for children of thirteen years and less. Such children constitute less than one sixth of our reform school population. For the remaining five sixths, of fourteen years or older, we can see no advantage to be derived from a year-scale measurement. For these the point scale is quicker and more informing.

A direct comparison of the results of year-scale (Binet-Simon) and point-scale measurements of five hundred and seventy-one boys is afforded by the two parts of Table VI. The upper part groups the boys by chronological age, and then by retardation, and advancement according to point-scale measurement, within these year groups. All whose mental measurements fall within one half year of the assigned chronological year are classed "at age." Those whose mental age is from 0.6 to 1.5 years lower than assigned age are classed as "testing one year below age." The numbers of individual subjects are given for each of these subgroups. So also are given percentages of the total number in each chronological age group. The same arrangement of numbers and percentages of subgroups of Binet-Simon measurements is given in the lower part of the figure.

It must be borne in mind that we have of necessity ex-

TABLE VI

ABOVE—RELATIONS OF POINT-SCALE MEASUREMENTS OF 571 DELINQUENT BOYS, WHO TEST LESS THAN 12 YRS. BINET-SIMON, TO THEIR CHRONOLOGICAL AGES.

BELOW—RELATIONS OF BINET-SIMON MEASUREMENTS OF THE SAME 571 BOYS TO THEIR CHRONOLOGICAL AGES.

PERCENTAGES AND NUMBERS GIVEN FOR "AT AGE" AND EACH YEAR OF RETARDATION AND ADVANCEMENT.

Point Scale

Chronological Age	Testing Above Age				Testing at Age	Testing Below Age										Totals
	4 Yrs. 3.6–4.5	3 Yrs. 2.6–3.5	2 Yrs. 1.6–2.5	1 Yr. 0.6–1.5	0.5— to 0.5+	1 Yr. 0.6–1.5	2 Yrs. 1.6–2.5	3 Yrs. 2.6–3.5	4 Yrs. 3.6–4.5	5 Yrs. 4.6–5.5	6 Yrs. 5.6–6.5	7 Yrs. 6.6–7.5	8 Yrs. 7.6–8.5	9 Yrs. 8.6–9.5	10 Yrs. 9.6–10.5	
10	...	...	....	17% (2)	25% (3)	50% (6)	....	....	8% (1)	....	....	....	....	....	....	12
11	...	...	....	8% (2)	19% (5)	19% (5)	35% (9)	15% (4)	4% (1)	....	....	....	....	....	....	26
12	...	...	....	....	25% (8)	19% (6)	22% (7)	25% (8)	9% (3)	....	....	....	....	....	....	32
13	2% (1)	....	....	6% (3)	6% (3)	25% (13)	14% (7)	13% (7)	17% (9)	15% (8)	2% (1)	....	....	....	....	52
14	1% (1)	1% (1)	1% (1)	6% (5)	5% (4)	4% (3)	18% (15)	18% (15)	13% (11)	20% (16)	11% (9)	1% (1)	....	....	....	82
15	...	4% (5)	2% (2)	2% (2)	3% (4)	7% (8)	6% (7)	17% (21)	20% (24)	16% (19)	17% (21)	3% (4)	2% (2)	1% (1)	....	120
16	...	1% (1)	....	3% (3)	4% (4)	2% (2)	8% (7)	4% (4)	24% (22)	23% (21)	17% (15)	8% (7)	3% (3)	2% (2)	....	91
17	...	...	....	2% (2)	1% (1)	3% (3)	4% (4)	4% (4)	8% (9)	24% (26)	24% (26)	6% (6)	24% (25)	....	....	106
18	...	...	....	....	6% (3)	6% (3)	....	9% (4)	....	6% (3)	19% (9)	28% (13)	9% (4)	13% (6)	4% (2)	47
19	...	...	....	....	....	....	....	....	....	....	33% (1)	....	67% (2)	....	....	3
Totals	2	7	3	19	35	49	56	67	80	93	82	31	36	9	2	571

Binet-Simon Scale

Chronological Age	Testing Above Age				Testing at Age	Testing Below Age										Totals
	4 Yrs. 3.6–4.5	3 Yrs. 2.6–3.5	2 Yrs. 1.6–2.5	1 Yr. 0.6–1.5	0.5— to 0.5+	1 Yr. 0.6–1.5	2 Yrs. 1.6–2.5	3 Yrs. 2.6–3.5	4 Yrs. 3.6–4.5	5 Yrs. 4.6–5.5	6 Yrs. 5.6–6.5	7 Yrs. 6.6–7.5	8 Yrs. 7.6–8.5	9 Yrs. 8.6–9.5	10 Yrs. 9.6–10.5	
10	...	...	....	17% (2)	33% (4)	33% (4)	8% (1)	8% (1)	....	....	....	....	....	....	....	12
11	...	...	....	....	27% (7)	27% (7)	31% (8)	15% (4)	....	....	....	....	....	....	....	26
12	...	...	....	....	3% (1)	28% (9)	56% (18)	9% (3)	....	3% (1)	....	....	....	....	....	32
13	...	...	....	....	....	11% (6)	35% (18)	29% (15)	15% (8)	8% (4)	2% (1)	....	....	....	....	52
14	...	...	....	....	....	....	6% (5)	34% (28)	40% (33)	6% (13)	4% (3)	....	....	....	....	82
15	...	...	....	....	....	....	....	14% (17)	34% (41)	33% (40)	15% (18)	3% (4)	....	....	....	120
16	...	...	....	....	....	....	....	....	13% (12)	51% (46)	25% (23)	8% (7)	2% (2)	1% (1)	....	91
17	...	...	....	....	....	....	....	....	....	21% (22)	41% (44)	30% (32)	7% (7)	1% (1)	....	106
18	...	...	....	....	....	....	....	....	....	....	23% (11)	45% (21)	21% (10)	11% (5)	....	47
19	...	...	....	....	....	....	....	....	....	....	....	33% (1)	67% (2)	....	....	3
Totals	...	...	....	2	12	26	50	68	94	126	100	65	21	7	....	571

cluded from this comparison all those subjects who graded twelve years or twelve plus by the Binet-Simon scale. We did not carry these subjects to the limits of their functional capacities. It would manifestly be unfair to compare such figures with the point-scale limits attained by these same individuals. The only effect of the elimination of these data from the table is to make the population under consideration appear more feeble-minded than they are. The two mental measurements of the same five hundred and seventy-one boys are classified in relation to chronological ages in the two parts of the tables.

By the Binet-Simon measurement twelve test "at age" and two one year advanced. By the point-scale measurement thirty-five test "at age," nineteen "one year advanced," three "two years advanced," seven "three years advanced," and two "four years advanced." For those who are retarded, the Binet-Simon scale makes the retardation much greater than the point scale. The absence of "at-age" subjects, measured by the Binet-Simon scale after twelve years, and the regularity of the increase of both minimal retardation and of median positions of retardation, after twelve years, is striking. It is evident the point-scale makes more distinctions and finer differentiations in regard to mental ability than the Binet-Simon scale. The same group of one hundred and six seventeen-year-olds who are measured as five years or more retarded by the Binet year-scale, are distributed from "one year advanced" by the point-scale. There are twenty-three point-scale measurements, of seventeen-year-olds, of less than five years' retardation. If this wider distribution indicates more accurate mental measurement, and we find no facts or arguments to contradict that assumption, we evidently have a better measuring instrument in the point-scale. These differences in point-scale scores point directly to differences in mental capacity. By referring to the individual tests where the differences occur, the examiner can quickly ascertain the species of mental capacity in which a given subject is strong and weak.

Table VII. affords a view of a yet closer juxtaposition of

TABLE VII

RELATIONS BY YEAR STEPS OF THE BINET-SIMON MEASUREMENTS OF THE 571 BOYS OF TABLE VI. TO THE POINT-SCALE MEASUREMENTS OF THE SAME. NUMBERS OF BOYS AND PERCENTAGES GIVEN FOR EACH YEAR OF RETARDATION AND ADVANCEMENT AND AT AGE

Point-Scale Age	Binet *Above* P. S. Age		At Age. Binet Within 0.5 Yr. of P. S. Age	Binet *Below* P. S. Age							Totals
	2 Yrs. 1.6 to 2.5	1 Yr. 0.6 to 1.5		1 Yr. 0.6 to 1.5	2 Yrs. 1.6 to 2.5	3 Yrs. 2.6 to 3.5	4 Yrs. 3.6 to 4.5	5 Yrs. 4.6 to 5.5	6 Yrs. 5.6 to 6.5	7 Yrs. 6.6 to 7.5	
6	25% (1)	75% (3)									4
7		80% (5)	20% (1)								6
8		54% (22)	44% (18)	5% (1)							41
9	3% (3)	51% (57)	44% (49)	2% (2)							111
10		17% (11)	71% (45)	11% (7)							63
11			42% (63)	56% (83)	2% (3)						149
12			38% (39)	59% (60)	3% (3)						102
13				19% (6)	81% (25)						31
14					38% (5)	62% (8)					13
15						36% (8)	64% (14)				22
16							20% (2)	60% (6)	20% (2)		10
17								50% (4)	50% (4)		8
18									91% (10)	9% (1)	11
Totals.........	4	98	215	159	36	16	16	10	16	1	571

point-scale and Binet year-scale measurements. The Binet ratings of the same five hundred and seventy-one boys of Table VI. are distributed in regard to the point-scale ratings of the same boys. For instance, if a boy scores fifty-six points, the average for nine years, by point-scale, and scores eight and six tenths Binet, he is sorted out as a nine-year-old (P.S.) who tests "at age" by the Binet year-scale. If another boy tested nine years by point-scale and ten and eight tenths Binet, he is classed as a nine-year-old (P.S.) who is two years advanced by Binet measurements. The point-scale scores are reckoned in each case, as of the nearest year. Fifty-six is the score for nine years, and sixty-two for ten years. Fifty-seven, fifty-eight and fifty-nine were reckoned as nine years, and sixty and sixty-one as ten years. The differences between point-scale and Binet ages were always taken in actual tenths of years. For instance, a score (P.S.) of fifty-eight is equivalent to nine and thirty-three hundredths years. A Binet age ten and eight-tenths years shows an advance of one and forty-seven hundredths years, and is classed as "one year advanced,"—within the limits 0.6 to 1.5 year ahead of point-scale age.

The striking feature of this exhibition (Table VII.) is the fact of change of relationship of the two scales at ten years. At mentalities of ten and below, as estimated by the point-scale, the Binet scale measures a majority of the subjects higher than the point-scale. On the other hand, from mentalities of eleven years and more, the Binet estimates depart by wider and wider differences in the other direction. Subjects testing eleven and above by the point-scale are estimated lower by the Binet scale. Further, they are all estimated about the same by the Binet scale, for as the point-scale age advances, the Binet retardation over point-scale ratings advances. This progression in the amount of retardation of Binet rating as compared with the point-scale rating, and the yearly increase of the retardation with the years' advances of mentality as estimated by the point-scale, is striking evidence of the lack of flexibility and adaptability in the Binet scale for these finer distinctions in regard to mental capacity. It is in close conformity with the breakdown of the Binet scale as shown in the lower part of Table VI.

The swing across the year-scale standard at ten years is in close conformity with the criticism of the Binet scale, as made by Thorndike and Stern, to which we have already referred. Thorndike finds the Binet ratings of normal children as measured by five investigators to fall more and more behind the real ages from eight and nine tenths years onward to thirteen and eight tenths years, inclusive. Similar results are indicated by Miss Hardwick's exhibition of Dr.

Goddard's data.[1] By her showing eleven years is the point where normal children measured by the Binet scale first show a preponderance of retarded individuals. With twelve-year-olds the median position is at two years retardation, with thirteen-year-olds at three years retardation, with fourteen-year-olds at four years, and with fifteen-year-olds at six years. Our criticisms of the Binet-Simon measuring scale are quite in line with these investigators. Our data reënforce the need of a more accurate means of measuring mental capacity, and augment the arguments and demonstrations already made for the point-scale, that it is more accurate as well as more expeditious, and is constantly perfecting itself as more data are accumulated.

That it is a relatively simple matter to make a year scale which measures mental ability more accurately is shown by Table VIII., where the year-scale findings from the examinations by our revised scale of one hundred and seventy-four delinquent girls are correlated with the point-scale results from the same girls. The method of making estimates is exactly as outlined above for the construction of Table VII.

By these results, it is evident the revised year-scale measurements of mental ages, from seven to thirteen years, inclusive, are very close to those of the point scale. In nearly every one of these point-scale years the median year-scale rating falls within the "at age" group, that is, within six months either way of the point-scale rating. There are only eight cases out of these one hundred and thirty-nine (seven to thirteen years, P.S., inclusive) whose year-scale estimates scatter two years from the point-scale ratings. This, compared with the showing of the Binet ratings of boys of Table VII., is good evidence that progress has been made in this revision, in replacing tests, for the years up to thirteen, in accordance with the facts of mental development. This statement is not based solely upon these comparisons of Binet and revised year-scale findings, with the point-scale findings; it is based upon these comparisons,

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[1] See Table *A*, p. 40, 'A Point Scale for Measuring Intelligence,' Yerkes, Bridges & Hardwick.

TABLE VIII

RELATIONS OF THE REVISED YEAR-SCALE (BUREAU OF JUVENILE RESEARCH) MEASUREMENTS TO THE POINT-SCALE AGES OF 174 DELINQUENT GIRLS, STATED IN NUMBERS OF INDIVIDUALS IN EACH "AT AGE" AND "RETARDED" AND "ADVANCED" YEAR, AND PERCENTAGES OF THE TOTAL FOR EACH POINT-SCALE YEAR OF AGE

Point Scale Age	Revised Year Scale *Above* Point Scale		Y.S. at Age with P.S.	Revised Year Scale *Below* Point Scale					Totals
	2 Yrs. 1.6–2.5	1 Yr. .6–1.5	0.5— to 0.5+	1 Yr. .6–1.5	2 Yrs. 1.6–2.5	3 Yrs. 2.6–3.5	4 Yrs. 3.6–4.5	5 Yrs. 4.6–5.5	
5	....	1	....	....	....	....	....	....	1
6	....	....	....	....	....	....	....	....	....
7	....	....	75% 3	25% 1	....	....	....	....	4
8	....	23% 3	62% 8	15% 2	....	....	....	....	13
9	....	36% 9	60% 15	4% 1	....	....	....	....	25
10	....	17% 3	59% 10	24% 4	....	....	....	....	17
11	2% 1	12% 5	46% 19	37% 15	2% 1	....	....	....	41
12	4% 1	12% 3	46% 12	38% 10	....	....	....	....	26
13	....	23% 3	23% 3	23% 3	31% 4	....	....	....	13
14	....	....	....	75% 3	25% 1	....	....	....	4
15	....	....	....	22% 2	22% 2	56% 5	....	....	9
16	....	....	....	....	29% 2	43% 3	14% 1	14% 1	7
17	....	....	....	33% 1	....	....	67% 2	....	3
18	....	....	....	....	....	45% 5	27% 3	27% 3	11
Totals..	2	27	70	41	11	13	6	4	174

taken in connection with the findings of Hardwick and Thorndike. They find the Binet scale underestimates normal children above nine years. We find the Binet scale underestimates mixed groups of normal and subnormal as measured by the point scale. Thus the point-scale findings are shown to be in close conformity with the facts of mental development. The revised year-scale findings, being in much closer harmony with the point-scale findings, are therefore in closer conformity with the facts of mental development than those of the Binet scale.

For the years beyond thirteen, where all agree there is very little development, from year to year, of the kinds of mental functions measured by any of these scales, the small number of cases already examined (thirty-three) indicate likewise a closer approximation to the point-scale findings. It will be recalled that Yerkes and Bridges give us no norms beyond fifteen, and for fifteen the number of cases averaged is small. We cannot argue much about the fine distinctions between mental capacities of these delinquent adolescents until we have large accumulations of data concerning the mental capacities of non-delinquent adolescents having similar educational disadvantages.

An outline of the revised year scale is presented herewith. As stated above, our indebtedness to Terman is very large. We adhered to the five-tests-to-a-year plan of Binet, but adopted the fourteen, sixteen and eighteen year tests and many changes of place, within the scale, of Binet-Simon tests. With the recognition that our advanced tests need more advancing to make them conform to the facts of development, we present the outline of our test sheet from which the results of Table VIII. were obtained.

YEAR III

1. Pictures. Enumeration.
2. Names key, penny, closed knife, watch, pencil.
3. Gives last name.
4. Points to nose, eyes, mouth, hair.
5. Gives sex. (Note form of question.)

YEAR IV

1. Compares lines.
2. Copies square.
3. Counts four pennies.
4. Repeats three digits. (1 of 3. Order correct.)
5. Comprehension. First degree. (2 of 3.) "What ought you to do?"
    (*a*) "When you are sleepy?"
    (*b*) "When you are cold?"
    (*c*) "When you are hungry?"

YEAR V

1. Comparison of weights. (Point Scale No. 3. 2 of 3.)
2. Definitions. (Use, or better. 3 of 4.)
3. Æsthetic comparisons. (No error.)
4. Three commissions. (No error, order correct.)
5. Cube test. (One line correct of X and Y.)

Year VI

1. Mutilated pictures. (3 of 4.)
2. Counts 13 pennies. (1 of 2 trials without error.)
3. Coins. (3 of 4 presented in this order; penny, nickel, dime, quarter.)
4. Right and left. (3 of 3, or 5 of 6.)
5. Cube test. (1 correct, of lines *B*, *C*, and *D*.)

Year VII

1. Repeats five digits. (1 of 3. Order correct. Read 1 per second.)
2. Pictures, description or better.
3. Gives differences. (2 of 3.)
4. Ties bow knot. (Model shown 60 seconds.)
5. Cube test. (2 correct of *B*, *C*, and *D*.)

Year VIII

1. Copies diamond. (Pen. 2 of 3.)
2. Counts 20–0. (40 seconds. 1 error allowed.)
3. Comprehension. (Third degree. 2 of 3.)
   "What's the thing for you to do,"
   (*a*) "When you have broken something which belongs to someone else?"
   (*b*) "When you notice on your way to school that you are in danger of being tardy?"
   (*c*) "If a playmate hits you without meaning to do it?"
4. Repeats three digits backwards. (1 of 3. Read 1 per second.)
5. Ball and field. (Inferior plan.)

Year IX

1. Definitions, superior to use. (3 of 5. "Thing" as genus counts plus.)
2. Suggestion. (2 of last 3 "equal" or "left greater.")
3. Three words in one sentence, or two coördinate clauses.
4. Gives date. Error of three days in date allowed. Day of the week, the month and year exact.
5. Stamps. (Gives total value. Second trial if individual values are known.)

Year X

1. Weights. (6, 9, 12, 15, 18 gm. No illustration.)
2. Repeat six digits. (1 of 3. Order correct. Repeat 1 per second.)
3. Names months.
4. Makes change. (2 of 3. Coins not used. 10 − 6, 15 − 12, 25 − 4 .)
5. Cube test. (1 correct of lines *E*, *F*, and *G*.)

Year XI

1. Designs. (One correct, other half-correct.)
2. Absurdities. (4 of 5. P. S. No. 17.)
3. Sixty words. (Illustrate with star, man, hat, and candy.)
4. Comprehension. (3 of 4. Point Scale No. 15.)
5. Repeats 20–22 syllables. (1 of 3 correct, or 2 with 1 error each.)
   (*a*) "The apple tree makes a cool pleasant shade on the ground where the children are playing."
   (*b*) "It is nearly half-past one o'clock; the house is very quiet and the cat has gone to sleep."

(*c*) "In summer the days are very warm and fine; in winter it snows and I am cold."

YEAR XII

1. Defines, (2 of 3) obedience, charity, justice.
2. Disarranged sentences. (2 of 3. 1 minute each.)
3. Ball and field. (Superior plan.)
4. Repeats five digits backwards. (1 of 3. Read 1 per second.)
5. Similarities—three things. (3 of 5.) "In what way are the following alike":
   (*a*) Snake, cow, sparrow.
   (*b*) Book, teacher, newspaper.
   (*c*) Wool, cotton, leather.
   (*d*) Knife-blade, penny, piece of wire.
   (*e*) Rose, potato, tree.

YEAR XIV

1. Pictures. Interpretation.
2. Repeats seven digits.
3. Reverses hands of clock. 6:22 = 7:03 = (1 of 2. 3 minutes error allowed.)
4. Induction test. 1 — 2 — 3 — 4 — 5 — 6 —. (Gets rule by sixth folding.)
5. Problem of enclosed boxes. (2 of 3.)

YEAR XVI

1. Vocabulary. (60–10,800.)
2. Fables. (3½ credits.)
3. President and King. (Power, accession, tenure. 2 of 3.)
4. Arithmetical reasoning. (2 of 3. 1 minute each.)
5. Difference between abstract pairs. (3 real contrasts out of 5.)

YEAR XVIII

1. Vocabulary. (75–13,500.)
2. Code. Writes "Come quickly." (2 errors allowed.)
3. Repeats eight digits. (1 of 3. Order correct. Read 1 per second.)
4. Paper-cutting test. (Draws, folds and locates holes.) Give before XIV., No. 4.
5. Ingenuity test. (2 of 3. Five minutes each.)
   (*a*) 3 and 5-pint vessel to get 7 pints. (Begin with 5-pint.)
   (*b*) 5 and 7-pint vessel to get 8 pints. (Begin with 5-pint.)
   (*c*) 4 and 9-pint vessel to get 7 pints. (Begin with 4-pint.)

SUMMARY

1. By Binet-Simon scale measurements, fifty-six per cent. of one thousand juvenile delinquents are feeble-minded. Such results and declarations are stultifying the work of clinical psychologists.

2. By Yerkes-Bridges point-scale measurements, chronological year groups of the same juvenile delinquents yield considerably lower averages than Cambridge school children.

3. They also show larger numbers who fall twenty-five

per cent. below their own averages, than twenty-five per cent. above the same averages.

4. As compared with the averages of Cambridge school children, these delinquents yield only about twelve per cent. at or above the normal standards.

5. About twenty-five per cent. are twenty-five per cent. below the normal standards, or have coefficients of mental ability of 0.75 or less. Such a standard eliminates about half of the feeble-minded group set apart by uncritical application of Binet standards. There is no more reason for considering these three hundred delinquents feeble-minded, than there is to denominate large numbers of productive laborers in the community high-grade morons. These delinquents test slightly under twelve years through inefficiency of the Binet scale.

6. By comparison of the Binet and point-scale ratings of five hundred and seventy-one boys with each other, the point scale is shown to be a much finer measuring instrument. By this scale one can point out much more definitely the mental capabilities which are above and below normal. It is the rational procedure for beginning an analysis of character. Upon this basis one can more safely make recommendations for treatment and prognosticate the future of the individual.

7. The Binet scale, compared with the point-year scale, breaks down in the examination of all persons of mental ages over ten years, very much as other investigators have shown it does with normal children. It underestimates mentality more and more from ten years onward. It likewise overestimates below eight years.

8. A revised year-scale adopted largely from Terman, corrects these defects of the Binet scale, to a great degree, for mentalities up to fourteen years. For fourteen, sixteen and eighteen years our tests need toning down. They are too advanced for the years to which they are assigned.

9. For these finer differentiations of mental capacity, and especially the traits and functions characteristic of the period of adolescence, the development of the point-scale offers a procedure at once more efficient and more pliable.

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THE CONSTANT ERROR OF TOUCH LOCALIZATION

BY SHEPHERD IVORY FRANZ

In the consideration of the ability of localization on the skin of points stimulated by light touches, by pressures, by pain stimuli, and by temperature stimuli, the conclusions of Ponzo[1] have been widely quoted and accepted.[2] The conclusions set forth by Ponzo in his various articles are, as I have shown in a previous publication,[3] open to question in certain particulars, and it is with regard to his conclusions respecting the constant error that the present paper is presented. It is convenient to deal with these matters under the two headings of method and facts.

Method.—The usual method of determining the constant error in experiments of this character is familiar to all who have investigated these matters. It does not need to detain us long at this time. Briefly the constant error is found in the following manner: The tendency to the localization of points in a special direction is determined by referring the localizations to predetermined axes of ordinates which meet at the point of reference or origin of coördinates, which in touch localization tests is the point stimulated. The points localized to the right and those to the left of the vertical axis are considered algebraic opposites, and similarly those

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[1] M. Ponzo, 'Recherches sur la localisation des sensations tactiles et des sensations dolorifiques,' *Arch. ital. de biol.*, 1911, 55, 1–14.

[2] See, for example, the reviews of H. D. Cook in *Psychol. Bull.*, 1913, 10, 258–261; 1914, 11, 238–241.

[3] S. I. Franz, 'The Accuracy of Localization of Touch Stimuli on Different Bodily Segments,' *Psychol. Rev.*, 1913, 20, 107–128.

above and those below the horizontal axis. The averages of the algebraic sums will give the location of the average tendency, or the average constant error.[1]

Ponzo's method of determining the constant error is, superficially, one of great simplicity, and of apparent value. Briefly in relation to the localization of the skin sensations it is as follows: Either before or after the performance of a series of tests a primary axis is drawn through the point of stimulation to represent the supposed direction of the nerve or other anatomical segmental axis; through the point two perpendicular lines are drawn in such a fashion that each cuts the primary axis at an angle of 45 degrees; the right angles which include the primary axis are then considered by Ponzo to be boundaries to those points which are in the direction of the primary axis and, consequently, to be in the direction of the anatomical part. All localizations falling within these boundaries are considered to be "in the direction of the primary axis."

A brief consideration will show that by the use of Ponzo's method conclusions may be reached which are neither accurate nor representative of the actual conditions. The extreme case of the points of localization falling close to one of the boundaries or axes of reference might show the constant error falling within the direction of the primary axis, and with a slight shifting of the points the constant error may be found to be in a direction at right angles to the primary axis. Ponzo's method breaks down in these extreme cases, and in cases which are not extremes. If the latter can be shown, we should conclude that the method is not sufficiently exact, and that conclusions based upon the method should not be accepted until they have been confirmed by facts obtained by further and more exact methods. Two illustrations will suffice to indicate the discrepancies in the conclusions to be drawn from certain results when the two methods of determining the constant error are used. The results which give these illustrations are shown in Figs. 1 and 2.

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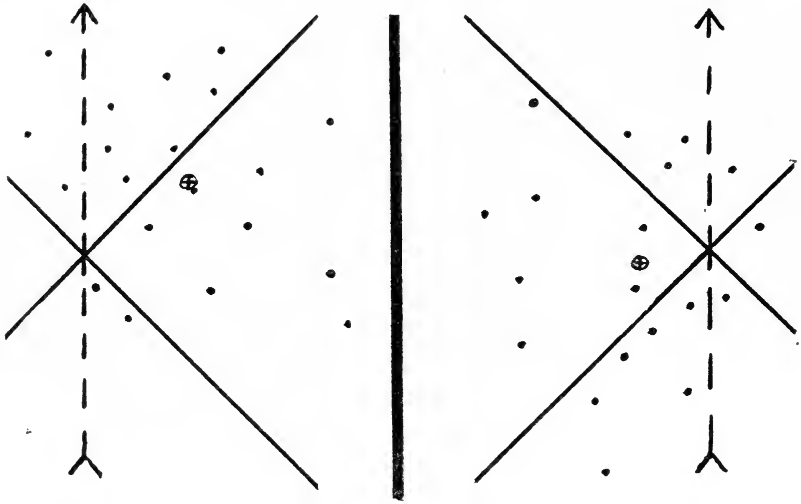
[1] These matters are so elementary that at first sight it seems superfluous to mention them, but the acceptance of Ponzo's conclusions, without due criticism, would indicate the necessity for repetition.

They show the localizations of a subject when two points on the mid-body lines were stimulated.



FIGS. 1 AND 2. Results of experiments on the localization of tactile stimuli. The points stimulated are at the intersections of the lines. The dotted lines show the principal axes, full lines, the general directions according to Ponzo. Each localization is shown as a point, the location of the average constant error as an encircled cross. Twenty tests in each experiment. For further details see texts.

In these diagrams the primary axis is shown by a broken line (with arrow markings), and Ponzo's constant error indicators are shown as unbroken lines. Each point of localization is shown as a dot, there being twenty in each illustration. Both experiments show according to Ponzo's method a superiority of localization in the direction of the primary axis. In Fig. 1 the number of points in the space 315° to 45° is 10, and in the corresponding space, 135° to 225°, 2; the numbers in the perpendicular spaces, 45° to 135° and 225° to 315° being 8 and 0 respectively. In Fig. 2 the points in the principal directions, 315° to 45° and 135° to 225°, are 5 and 7 respectively, while in the perpendicular spaces, 45° to 135° and 225° to 315°, there are 1 and 7 respectively. In each of these experiments, therefore, there are 12 points in the direction of the principal axis and 8 points not in that direction. It will thus be seen that there is an apparent constant error (by Ponzo's method) in the direction of the principal axis, the number of such localizations being fifty per cent. more than in the other direction.

When the results in these two experiments are calculated by the usual method of determining the constant error the supposed tendency to locate along the principal axis is not found to be present. Thus, the results in Fig. 1 show an average constant error toward the right of 15.2 mm. and an upward tendency of 10.6 mm. The results in Fig. 2 show an average constant error of 11.2 mm. to the left and 1.9 mm. downward. The localizations of these average constant errors are shown by small encircled crosses.

It will be seen, therefore, that although by Ponzo's method the constant error in these two examples is the same, this error when determined by the usual method differs in the two cases. In the first example it is toward the right, in the second it is toward the left. In the first example the constant error is at an angle of 55.5 degrees from the upper part of the principal line, in the second it is at an angle of 260 degrees.[1] In each case the "angular" error does not lie within the angular limits which Ponzo sets as the direction or axis of the anatomical part.

An objection may be raised that the illustrated examples are isolated and extremes, and that they do not fairly represent the conditions with which Ponzo, in his work, and I, in my former paper, dealt. In a brief space it is not possible to discuss in detail the results which Ponzo has reported and illustrated, but examples in which similar, but perhaps not as extensive, differences are to be found are given in Ponzo's publications. At the same time it may be stated that the results on the individual points which were grouped in my previous article show at times as great deviations as in the two examples given here. Because of these variations in a number of tests on different days the average constant error for one point or for an anatomical segment is not large. Furthermore it should be stated that if the method of Ponzo

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[1] Both roughly measured by a protractor showing angular degrees, and using the vertical axis above the stimulated point as zero degrees.

breaks down in cases of this character, which I cannot consider to be extremes, it is a method upon which little or no reliance can be placed, especially when broad generalizations are to follow from the account of the results.

Facts.—A question immediately arises regarding the accuracy of Ponzo's statement of the conditions encountered in localization tests when the adequacy of his method of dealing with the observations is disputed. Are suppositious general tendencies toward localizations along certain bodily axes, or along the courses of nerves, dependent upon his erroneous method of calculation of the constant error, or are they found to exist when the usual and more accurate constant error method is employed? An answer to this question is given in part in my previous paper, although in that article the results of the constant errors of localizations of touch stimuli for individual points were not given in full. The fact that my results on the individual points were not reported has led some to believe that my groupings into larger anatomical segments have concealed or balanced the constant errors, and that the apparent discrepancies between Ponzo's and my conclusions would disappear if I had dealt with the individual points. A further consideration of the groupings and the results from them will, however, serve to show that such is not entirely, if at all, the case. If similar groupings are made for all subjects the results should be directly comparable, regardless of the kinds of groupings, and it would make no difference how large or how small a grouping is made. That the constant errors are not the same in the two subjects, *A* and *C*, whose results were published, is evident from inspection.[1] At the same time it must be observed that with the groupings in the two series of the same subject, *A*, the constant errors were not always the same. Other results to which I may refer are those for the soles of the feet of these two subjects, on each of which anatomical part only one point of stimulation was used. The results of the two series of tests on subject *A* do not correspond, although the results with subject *C* have a fairly close correspondence with

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[1] See Table V., page 120 of my previous article, *op. cit.*

the results of the first series on subject *A*.[1] In this connection attention may be called to the results obtained by another method of constant error estimation which I employed and which gave inconstant results. When the subject localized the first stimulation this localization was taken as the second point of stimulation, the second localization was taken as the third point of stimulation, etc. Subject *A* by this method, starting from a point on the mid-line of the body and on a level with the nipples, reached in 10 tests on one day "a point 8.5 cm. from the mid-line and 17 cm. below the first stimulation. . . . In a second experiment on a subsequent day in 10 tests the point was reached on the left 10 cm. from the mid-line and 16.5 cm. below the first stimulation. In a third series, a point was reached after 10 tests, 11 cm. from the mid-line, and 9 cm. below the first stimulus."[2] In relation to the primary points the general tendencies in these three tests are indicated by the angular relations of the average constant errors to the point on the mid-line. These are respectively 153, 148, and 129 degrees.[3] The approximate amount of the angle of the ribs with the sternum at the original point of stimulation is approximately 85 degrees. These results tend to confirm what I shall now endeavor to show more accurately, namely, that the errors of localization of stimuli to definite points in certain anatomical segments

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[1] It would not be justifiable to make further comparisons of supposedly similar points on different subjects at this time, for it is not possible to determine that the points were in all cases anatomically identically located. The subjects are no longer available (artists' models make up part of our city-to-city floating population) and anatomical comparisons cannot be made. The reason why this comparison cannot be made from my available records is because I selected for my former work points which were at definite distances from certain anatomical landmarks, such as the axilla, the umbilicus, the nipples, etc., but with the differences in lengths and girths of the bodies the points did not always correspond with respect to other bony landmarks and with the supposed courses of nerves. Especially in relation to the stimulated points on the thorax are these differences of importance, for a variation of one or two centimeters in the length of the chest would shift the location of a point from an intercostal space to a supracostal area, or vice versa. The recalculation of the individual points would not, therefore, be of any advantage towards the solution of the problem.

[2] *Op. cit.*, p. 121.

[3] Protractor measurements.

do not correspond with the direction of the nerves, as Ponzo has contended.

Ponzo's[1] conclusions on this point refer especially to the localization of pressure and pain stimuli in the intercostal spaces. He has reported for three subjects results of localization of stimuli in the fifth, sixth, seventh, and eighth intercostal spaces, which are interpreted to indicate a localization in the direction of the intercostal spaces, with the further tendency to locate toward the end branches of the nerve which innervates the skin covering such a space. He subsequently added the conclusion that the errors of localization of sensations of warmth and of cold on other bodily parts correspond with the general direction of the superficial nerve trunks.[2] The value of these conclusions, if they should be found to be correct, is very great for anatomical workers, but greater for clinicians who, by the application of a few tests to show the constant error in the localization of pressures and temperature stimuli, would be enabled to discover in the living subject the course of the cutaneous nerves in different portions of the body.

The present series of tests were made upon an artists' model, Miss W., who is about 20 years of age.[3] Special attention was paid to certain areas of the body in which the directions of the nerves are very constant, in order that the constant error tendencies might be considered in relation to the courses of these nerves. The areas which were investigated are as follows: (*A*) A point in the axillary line overlying the seventh intercostal space when the arm is lying fairly close to the body; (*B*) a point near the mid-line of the chest at the junction of the fourth intercostal space with the sternum; (*C*) a point on the mid-body line, at the level of the nipples; (*D*) a point on the anterior part of the thigh,

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[1] Ponzo, M., 'Observations sur la direction des erreurs de localization dans les espaces intercostaux,' *Arch. ital. de biol.*, 1911, 56, 193–201.

[2] Ponzo, M., 'Studio della localizazione delle sensationi termiche caldo e freddo,' *Riv. di psicol.*, 1913, 9, 393–415.

[3] In most of the tests I have had the assistance of Dr. Mildred E. Scheetz, who also in my absence carried out some of the tests which are here reported. The results obtained by her did not differ from those carried out by me more than those obtained by me on one day differed from those obtained on a subsequent day.

about half way from the knee and about midway between the middle and internal branches of the anterior cutaneous nerve of the thigh (branches of the femoral or anterior crural); and (*E*) a corresponding point on the lateral portion of the thigh, which is supposedly supplied by the lateral femoral cutaneous nerve. All of the nerves supplying these parts are relatively constant in direction, although in the living subject it is not possible to determine their exact courses.[1] Dissection shows them to be about as constant in location as any other nerves of the body, and they were selected instead of arm nerves because of the presumably lesser experience the subject would have in locating stimuli along their courses. It should not be supposed, however, that the location of skin areas in relation to the underlying nerves can be determined exactly, and that the selected points are definitely related to the supposed underlying nerve trunks. With respect to the points at the intercostal spaces, and *pari passu* the remark applies to the other locations, a slight amount of movement of the arm or a slight twist of the thorax will throw the superficial point out of its definite relation to the intercostal space, bringing it above a rib.[2]

The stimuli were given by means of a short brush, stiffer than one which I have previously used for light touch tests, and it is certain that not only was the superficial skin stimulated but also the underlying tissues. This would give results relating to the compound of touch and pressure stimuli, which corresponds well with the conditions in the work of Ponzo. Twenty tests were made at each point on one day, and the averages of the tests are considered here. At the same time it is possible to combine the tests on different

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[1] The courses of these nerves may be learned from almost any good text-book of anatomy.

[2] This is a point which must be considered in dealing with the relations of localizations to underlying nerves. What for example shall we consider the nerve supply to the skin overlying an intercostal space to be? Is it from the nerve emerging in the intercostal space which the skin covers when the arm is held close to the side of the body? Or, is it the nerve of the space above, which the skin covers when the arm is raised above the head or in some cases when the arm is at a right angle to the axis of the trunk? In the present work the superficial assumptions of Ponzo have been followed, but not necessarily accepted.

days into suitable groups referring to the same point, and to make comparisons of the results.

Seventh intercostal space.—Two series of experiments were made on the left side, and one on the right, in the seventh intercostal space where the space meets the mid-axillary line. Twenty tests were made in each experiment. The results of the calculation of the constant errors in these experiments are as follows: On the left, 25.3 mm. upwards, 3.1 mm. toward the back of the body; and 4.0 mm. downwards, and 1.9 mm. toward the back of the body; on the right, 7.5 mm. upwards, and 14.1 mm. toward the front of the body. It will be noted that the error away from the axillary line, toward the front or back, differs in the experiments on the right and left sides, and it will also be noted that the errors in the two series on the left side differ with respect to the location of the stimulated point in relation to the upper and lower (cephalad and caudad) portions of the body. It is apparent that by the usual methods of determining the constant tendency there is not much agreement among the three experiments. With reference to the angles of the average constant error localizations in relation to the mid-axillary line it may also be said that in the three experiments the localizations are different. They are respectively 7°, 155°, and 61.5°.

At the times the tests were made the directions of the intercostal spaces in relation to the mid-axillary line were determined by laying a finger upon the skin overlying the space and after having pressed the finger into the space as closely as it would fit the outlines were marked upon the skin with a grease pencil. The center line between the two lines which were marked was taken to represent the direction of the space, and its angular direction in relation to the mid-axillary line was determined, using the portion of the latter above the point as zero degrees. In the three experiments which have been described the directions which were determined were greatly different, for they were 120°, 137°, and 122° respectively. At first glance there appears the probability of great error in the making of these measurements, for it seems improbable that on the same subject the angle of the ribs should differ by as much as these measurements differ. To test the matter the subject was at a subsequent session measured a number of times, both by me and by an assistant. The grease pencil marks were obliterated after each record was taken and new marks were made for each determination. The results of the measurements are as follows: Right side, with the arm close to the body, 119° and 118°, with the arm abducted and held above the head, 114° and 131°, left side, with the arm at the side of the body, 130° and 120°, with the arm raised above the head, 128° and 125°. These measurements which were taken with extra care do not differ from those taken three months previously more than do the earlier corresponding measurements from one another. In other words it appears that the angular relation of the seventh intercostal space to the mid-axillary line is not constant, or that the errors of measurement are large. The former is what might be expected from what we know of the rib movements in connection with the acts of respiration. At the same time it should also be noted that the angular directions of the intercostal spaces vary according to the position of the arms. This is shown by the series of figures just given and is further demonstrated by the results of a series of measurements on other subjects on whom the angular measurements were taken by Dr. Scheetz. These measures are given in Table I.

This table shows clearly the variations in accordance with the positions of the arms, the averages for all completed series (two each on *A* and *B*, and one each on *C* and *D*) being 95° when the arm is held close to the body, 111° when the hand is placed upon the opposite shoulder, and 118° when the arm is raised above the head. These variations, it will be noted, are greater than those found with the subject on whom the present tests were made, and may partly be accounted for by differences in the shapes of thorax, and in the amounts of subcutaneous fat in the four subjects.

Assuming, however, that the angular measures first taken on our present subject (120°, 133°, and 122°) are sufficiently exact, we may examine the results of the localizations by the method of Ponzo. When we draw the bounding lines to indicate the directions we find for the three series the following: I., In the direction of the space, 5 localizations, away from the direction of the space, 15 localizations; II., in the direction of the space, 8, away from the direction, 12; III., in the direction, 8, away from the direction, 12. In no case, therefore, did the subject show a constant tendency to localization in the direction of the space, as has been contended.

TABLE I

ANGULAR DIRECTIONS OF THE SEVENTH INTERCOSTAL SPACES IN RELATION TO THE MID-AXILLARY LINES

Asterisks denote that measurements were not made in these cases.

Subjects	Sides of Body	Arm Near Body	Hand on Opposite Shoulder	Arm above Head
A..........	R.	91	112	112
	L.	108	122	135
B..........	R.	109	120	121
	L.	78	117	106
C..........	R.	89	114	127
	L.	*	103	104
D..........	L.	95	83	107
E..........	R.	126	*	*
	L.	118	*	*

Fourth Intercostal Space.—One series of twenty experiments on a point in this space, on the right side four centimeters from the mid-line, gave an average constant error 11.8 mm. downwards, and 14.1 mm. toward the mid-line of the body. The angular relation of the space to the mid-body line was found to be approximately 90°. The angular location of the constant error is, therefore, within the boundaries set by Ponzo (45° to 135°). At a subsequent session measurements of the angular relation of the space to the mid-body line gave 78° on the right side, and 87° on the left side. In another subject the angular relation was 86°. It will be seen, therefore, that the error is close to the lower boundary of the angular limits, although within them.

When measured in relation to the boundary lines it was found that 14 of the localizations were within the boundaries and 6 outside, although it should also be mentioned that only two of the localizations were between 0° and 90°, and the remaining 18 were in the space bounded by the 90° and 180° lines.

Mid-body Line at the Level of the Nipples.—Two tests of twenty experiments each were made with the right hand for localization and one similar test with the left hand for localization. These three tests show respectively the following constant errors: I., 7.8 mm. downwards, 15.2 mm. toward the left side; II., 0.9 mm. toward the head, 6.1 mm. toward the left; III., 6 mm. toward the head, 3.7 mm. toward the left. The results in the three series correspond as far as left-sided localization is concerned, but do not correspond with respect to cephalad or caudad localizations. Since we have no definite knowledge regarding the course of the nerves at this point beyond the general belief that the skin is probably innervated almost equally from both sides of the spinal cord, the results are negative. It should be said, however, that if we believe in an equilateral innervation there is no apparent reason for the left-sided localization constant error.

Anterior Thigh.—The location of the stimulated point has already been mentioned. The results of 200 experiments, 20 on each leg on each of five days, are given in the accompanying table (Table II.) which shows the average errors of localization, the constant errors centralwards or distalwards, and to the right or to the left. The results of the calculation of the same results by the rougher method of angular limits are also shown in Table III. It will be seen that the constant errors in the five series do not correspond. In one test the average constant error lies to the left, in another test it lies to the right; in one it is toward the abdomen, in another it is toward the foot. At the same time it will be observed that there are great variations in the localizations in relation to the angular measurements, and that, on the assumption of a direction of nerve from 355° to 175°, the constant error is located within the angular limits in only seven of the ten experiments. The totals of the 100 experiments on the right and the 100 experiments on the left show that the general direction of localization is in the nerve direction, if we can use the method of angular limits for the determination of the constant error. This tendency to location in the supposed direction of the nerve is, however, not very large, for there is only a superiority of 50 per cent. in the localizations in the direction as compared with those at right angle to the direction (viz., 120 : 80).

TABLE II

AVERAGE AND CONSTANT ERRORS ON THE RIGHT ANTERIOR THIGH.
20 EXPERIMENTS ON EACH DAY

Constant errors calculated by the usual method.

	Serial Days					Averages
	1	2	3	4	5	
Av. error..........	24.3	17.5	12.0	20.0	17.0	18.2
C.E. ⇅	↑19.1	↑ 10.5	↓6.3	↓13.8	↓1.6	↑ 4.0
C.E. ⇄	0	0	←1.3	→5.3	→9.7	→2.8
C.E. degrees.......	0	0	168.5	159.0	99.0	35.0

AVERAGE AND CONSTANT ERRORS ON THE LEFT ANTERIOR THIGH.
20 EXPERIMENTS ON EACH DAY

Av. error..........	17.8	13.0	12.5	14.0	25.0	16.5
C.E. ⇅	↑ 3.3	↓2.8	↓4.8	↓ 2.7	↑4.0	↓ 1.5
C.E. ⇄	←2.8	→1.7	←2.1	←7.5	→0.2	←2.1
C.E. degrees.......	320.0	149.0	203.5	250.0	3.0	235.0

TABLE III

CONSTANT ERRORS DETERMINED BY METHOD OF ANGULAR LIMITS

Right Side

Serial Days	Direction of Nerve	Opposite Direction
1	13	7
2	11	9
3	14	6
4	10	10
5	15	5
Totals....................	63	37

Left Side

1	10	10
2	8	12
3	12	8
4	15	5
5	12	8
Totals....................	57	43

TABLE IV

AVERAGE AND CONSTANT ERRORS ON THE RIGHT LATERAL THIGH.
20 EXPERIMENTS ON EACH DAY

Constant errors calculated by the usual method.

	Serial Days					Averages
	1	2	3	4	5	
Av. error..........	17.0	13.3	14.0	33.8	54.5	26.5
C.E. ⇅	↑3.8	↓2.8	↑8.5	↓5.2	↑10.3	↑2.9
C.E. ⇄	→9.1	←3.5	→3.7	→34.1	→50.6	→18.8
C.E. degrees.......	67.0	231.5	24.0	98.5	78.5	82.0

AVERAGE AND CONSTANT ERRORS ON THE LEFT LATERAL THIGH.
20 EXPERIMENTS ON EACH DAY

Av. error..........	22.8	14.8	16.0	23.3	28.3	21.0
C.E. ⇅	↓15.3	↑2.8	↑2.8	↑5.9	↑4.2	↑0.1
C.E. ⇄	←12.3	←7.5	←5.7	←8.8	←22.8	←13.4
C.E. degrees.......	219.0	290.5	297.0	287.0	280.0	270.0

TABLE V

CONSTANT ERRORS DETERMINED BY METHOD OF ANGULAR LIMITS

When a point has been on a line it has been calculated as half each way.

Right Side

Serial Days	Direction of Nerve	Opposite Direction
1	12	8
2	9.5	10.5
3	10.5	9.5
4	0	20
5	0	20
Totals....................	32	68

Left Side

Serial Days	Direction of Nerve	Opposite Direction
1	10.5	9.5
2	5.5	14.5
3	7	13
4	3.5	16.5
5	5	15
Totals....................	31.5	68.5

Right Lateral Thigh.—The nerve supplying this portion of the body is very constant in direction in cadavers, following an almost straight line from the crest of the ileum to the middle of the knee, in other words from 0° to 180°. The results of five tests on different days on the two legs on the selected points, 200 experiments in all, are given in Table IV., and the results of the calculation by the method of angular limits are given in Table V.

In these tables it will be observed that the constant errors in the individual tests differ greatly. In some there is an apparent tendency to locate toward the pelvis, in others toward the knee; in one there is a tendency to locate toward the dorsal surface and in the other nine toward the anterior surface of the leg. The angular relations of the constant error determinations to the supposed nerve course in these ten series greatly differ, ranging from 24° to 231° on the right leg, and from 219° to 297° on the left leg. Within the artificial angular limits there is only one constant error direction on the right and none on the left side, four on the right and five on the left being outside of the supposed angular directions.

In only three tests is there a predominance of localizations in the direction of the artificially set angular limits, and in the other seven tests there is a superiority of localization in the spaces at right angle to the artificially set directions. For the total of ten tests, therefore, there is a superiority in direction away from the space amounting to over 100 per cent.

Summary.—The method of calculation of constant error by angular limits is too inexact to warrant its use except as a possible qualitative method for preliminary work. The constant error tendency in the five locations selected for the present series of tests is in only one place in the supposed direction of the innervating nerve, whether this tendency be measured by the usual and more exact means or by the method of angular limits used by Ponzo. Conclusions regarding the constancy of direction of constant errors must be radically changed to conform to the facts.

One possible objection may be raised to the present series of results. This is that they have been obtained with only one subject. This objection loses its force in view of the widespread generalizations which have been made, for it becomes apparent that if the conclusions set forth by the Italian investigator are not applicable to other than his own subjects they cannot be accepted as general. I am not willing, however, to admit that Ponzo's conclusions have been founded upon the utilization of the best methods, and in fact it is amply demonstrated that the method employed by him is so faulty that its use is unjustified.

THE COMPLICATION EXPERIMENT AND THE AFTER IMAGE

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I

The career of the 'Complication Experiment' has been so long and varied that the new investigator needs to spend a large portion of his time in repeating and confirming the results of earlier observers. To do this in the present instance with anything like adequacy, we have, for example, made over twenty-three hundred separate determinations. None of these was used in the material presented. In this case, the repetition of earlier investigations has been particularly interesting. In the development of the problem, one point after another has been discovered, yet new variations and new factors continue to appear. The following paper does not attempt an historical survey of the field. Such work has been performed with relative completeness by Sanford,[1] and more recently by Burrow.[2] Moreover, notwithstanding Dunlap's remark concerning the 'remnants of the complication-problem,' we have not convinced ourselves that sufficient work has yet been done to write its final history.

Disregarding the chronological order, the systematic history of the problem falls roughly into three divisions, the period of discovery and collection of data, the period of experimental modification of objective conditions, and finally that of specific directions to the observer. The first period begins with the discovery by Bessel, of the note in the Greenwich Observatory records, and again with Wundt's introduction of the problem into the psychological laboratory.

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[1] Sanford, E. C., 'The Personal Equation,' *Amer. Jour. of Psychol.*, Vol. 2, pp. 3, 271, 403.

[2] Burrow, N. T., 'The Determination of the Position of a Momentary Impression, etc.,' *Psychol. Rev. Mon. Suppl.*, Vol. 11, No. 47.

The second is again begun by both Bessel and Wundt when the first mentioned changed the rate of the clock to test the effects on the observations, and the second, complicated the experiment by using the pendulum in place of the uniform movement of the clock hand. In the psychological study of this problem, as against its astronomical study, it is probable that Angell and Pierce[1] are the first investigators to give specific directions to their observers. Typical of the further development of this change in the method of experimentation is the 'inversion of the complication experiment' introduced by Stevens,[2] the rigid set of directions given his subjects by Dunlap,[3] to control eye movements, and the analysis of the methods of observation by Guenther,[4] and Dunlap.[5]

In the following pages, the results of some experiments with Dunlap's 'exact fixation' method, using practically an exact duplicate of Geiger's apparatus,[6] are presented. After practice with this method, our observers settle down to one or the other form of error with surprising steadiness. Our introspections show that the after image is a recognizable influence in making the determinations. By a slight modification in the apparatus which shortens the after flash of the pointer, the loss of this factor can be made to appear in the results. As Dunlap has shown the necessity of eliminating eye movements, we see here that a further condition for accurate results is some adequate visual cue to indicate the pointer position at the time of the click. The determinations that follow the selection of any single factor will be regular. Particularly the perception that follows the selection of the "pointer interruption" point on the scale is very accurate.

The most complete summary since Geiger, of the numerous

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[1] Angell, J. R., and Pierce, A. H., 'Experimental Research on the Phenomena of Attention,' *Amer. Jour. of Psychol.*, Vol. 4, pp. 520–541.

[2] Stevens, H. C., 'A Simple Complication Pendulum for Qualitative Work,' *Amer. Jour. of Psychol.*, Vol. 15, p. 581.

[3] Dunlap, Knight, 'The Complication Experiment and Related Phenomena,' *Psychol. Rev.*, Vol. 17, pp. 157–191.

[4] Guenther, F., 'Reacktionsversuche bei Durchgangsbeobachtungen,' *Psychol. Stud.*, Bd. 7, S. 232 f.

[5] *Op. cit.*, p. 168 f.

[6] Geiger, M., 'Neue Complicationsversuche,' *Phil. Stud.*, Bd. 18, S. 347 ff.

factors involved in the judgment of coincidence, under its many and varied experimental conditions, has been given by Burrow. The point with which we are to deal first is treated by Dunlap.[1] He found that at the moment of the determination of coincidence the pointer did not blur, but remained frequently very distinct as if stationary. There could evidently be but one explanation for this,—the eye was making a short excursion with the pointer before settling down. The general observation of eye movement was noted by several other observers previous to this. Dunlap cast about for means to aid in maintaining this fixation point at the moment of coincidence. He finally used a screen of mosquito netting placed in front of the dial. By this means he was able to determine whether the eyes moved or not at the moment of alignment. If the threads of the net remained clear, it might be assumed that the fixation was adequate. His results indicate that inadequate fixation is perhaps the most potent factor in the production of errors. Other factors are still operative, but as he shows in a series of supplementary experiments, their effect is minimized when exact fixation is maintained. In taking records, he approached the assumed point of coincidence from both directions. This was done to equalize the influence of the two modes of approach, since Burrow had previously shown that they affect the errors differently.

II

After testing these results carefully, we decided to use one method of approach and to begin a practice series.[2] In this we hoped to discover more definitely the relation of the various factors involved. In place of the bell on the older apparatus, we used the click of a telephone receiver. This modification has been made in several recent experiments. Our scale, at first, was marked off with one-degree spaces; from series '11' on, a dial with scale marks two degrees apart and of equal length, was used. The five-degree divisions on

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[1] *Op. cit.*, p. 164.

[2] Special mention is due Miss Mary Hill, since her continued assistance made possible the collection of a large portion of this data.

the first scale were longer than the degree marks, while the ten-degree divisions had marks approximately one centimeter in length and were numbered. The radius of the dial to the inner ends of the scale marks was 26 cm. The time of revolution was 1.2 sec. or $3\frac{1}{3}\sigma$ per degree. This rate was used throughout the following experiments.

The directions given all observers are as follows: "Fixate first the point at the top of the dial, marked 360." O. was warned to maintain exact fixation at all times and at the preliminary points to use only peripheral vision in determining tentative coincidences. "By successive fixations locate the point of coincidence." As far as possible, this was to be done by movement to the left only, that is, he was not to go 'negative' of the point of actual coincidence, though O. did not know, of course, whether he did this or not. By such successive approximations, he was expected to establish coincidence.[1]

In the first part of the experiment, we used only the left upper quadrant of the dial. Later we used the whole left half. All determinations were given in haphazard order. The points of actual coincidence were made out in advance and changed frequently so that O. would have no external cues to aid him. Fixation points are always to the right and above the sound in the upper quadrant. In the lower this is changed. Three different persons learned to manage the experiment, so that the position cue, or other cues, from E. might be made inoperative.

In the preliminary series mentioned above, we used over thirty different subjects. Only four subjects began the practice group arranged here; three others were added later. Subject I. knew the literature of the experiment thoroughly, while subject III. was unfamiliar with the nature of the experiments. Subjects II. and IV. were instructors in psychology. Subject III. did not take part in the preliminary experiments mentioned above.

The tables that follow are practically self-explanatory. The first column gives the number of the series; '1,' '2,' etc.,

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[1] See Geiger's 'reflective type,' *op. cit.*, p. 378.

each constitutes a day's work for that subject. The columns headed 'Positive,' 'Negative,' 'Coin' give the number of errors and of successes. The column marked 'Average' gives the average of all determinations without regard for sign; the next column is the mean variation of this average. Columns headed 'Pos. Average,' and 'Neg. Average' give the average error for the positive and negative errors respectively. The final column is the difference between the sum of the positive and the sum of the negative errors, divided by the total number of determinations, together with the sign of the preponderating error. All results are given in degrees. Numbers in brackets are estimated.

TABLE I

SUBJECT III

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1	14	5	0	6.74	3.59	7.50	4.60	+4.31
2	15	4	2	3.05	1.87	3.60	2.50	+2.09
3	16	3	2	3.14	2.10	3.43	3.66	+2.09
4	12	2	8	1.91	1.73	2.83	4.00	+1.18
5	18	3	0	2.90	1.70	3.23	1.00	+2.62
6	15	3	3	2.14	1.23	2.53	2.33	+1.47
7	9	4	9	1.45	1.31	2.11	3.25	+0.27
8	14	5	2	3.04	1.88	2.71	5.20	+0.57
9	8	11	3	5.02	2.10	7.28	5.40	−0.18
Total...	121	40	29	3.18	2.10	3.91	2.45	+1.46

In series '5,' O. remarked that he never went below the click, if he could help it, because the 'after whirl' bothered him. About the middle of series '6,' the experimenter asked O. if he were satisfying himself that the points selected were actual coincidences. O. thought that he was staying a little positive. 'If he went farther he was bothered by the after-image streak.' At no time did the experimenter discuss the point; O. was merely urged from this time to make his determinations as accurate as possible. In series '8' and '9' the complete half circle was first used. Previous to this, determinations were in the left upper quadrant.

After three days' practice, in which subject I. was allowed to know the type of error she had been making and to study a few cases of actual coincidence, she tried again. The results this time were but slightly better than before. This subject was familiar with the discussions of theory and was particularly interested in the various theories of apperception, etc., offered to explain the source of error. Her introspections clung to questions of priority of sound or of pointer in attention, the 'pulling' influence of the sound in the negative direction, and the tendency of the eyes to follow the pointer. If at any time during the 'practice with knowledge' work she was urged to go on to where the sound 'pulled' her, she found a point that was always more correct than the first point selected. During the remainder of this phase of the practice and the work of the table below, she developed a habit of finding this first assumed point; stopping a moment, then moving to a second satisfactory determination. If she went where the sound seemed to pull her, the results were always better than otherwise. Subject I. failed decisively in finding that the pointer was a part of consciousness of position at all. She would regularly reply to questions intended to elicit descriptions of the pointer, etc., 'Of course I know where it is but I don't see it; the *sound* is at the place I select finally.'

TABLE II

SUBJECT I

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1	10	10	1	10.71	6.43	9.60	12.90	− 1.57
2	9	9	3	6.52	5.17	5.00	10.22	− 2.23
3	14	5	2	7.52	4.78	10.71	3.40	+ 5.90
4	19	1	0	13.90	6.42	14.47	3.00	+13.57
5	17	1	1	12.00	6.63	12.76	11.00	+10.84
6	18	1	1	10.40	5.06	11.22	6.00	+ 9.80
7	20	1	0	16.85	8.16	17.65	1.00	+16.85
8	13	4	3	8.30	4.53	10.38	7.75	+ 5.20
9	16	3	2	9.09	5.45	11.12	4.25	+ 7.85
10	19	1	1	13.71	7.19	13.78	6.00	+13.14
Total...	155	36	14	10.92	5.98	12.60	8.6	+ 7.91

We now made a change in the directions to O. Subject I. was asked to find, by the usual method, the place on the dial where her vision of the dial was *interrupted* by the pointer *when* the click sounded. The results follow in Table III.

TABLE III

SUBJECT I. TO FIND 'INTERRUPTION'

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
19	9	10	2	3.64	2.23	4.22	3.80	0.00
20	6	11	1	3.83	3.09	3.16	4.54	−1.72
21	5	3	9	1.30	1.40	2.80	2.66	+0.35
22	4	3	3	2.70	2.10	4.00	3.66	+0.50
Total...	24	27	15	2.86	[2.20]	3.54	3.66	−0.31

Subject I. still felt the 'pull' and the 'push' of sound and of pointer, but she was now looking for a *definite visual occurrence* at the time of the click and succeeded in eliminating the effects of the previous adjustment to a large extent. At this point, however, no introspections from 'O.' enabled us to find the reasons for such steadiness of the positive error in the previous experiments.

TABLE IV

SUBJECT II

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1	7	14	0	8.09	2.88	7.71	8.28	− 2.95
2	5	15	1	6.85	3.67	8.00	6.93	− 3.05
3	5	15	0	14.40	6.85	12.60	15.00	− 8.10
4	5	13	3	6.04	3.48	4.80	7.92	− 3.61
5	8	11	1	6.35	3.82	6.00	7.18	− 1.55
6	5	14	2	8.61	6.26	2.20	12.17	− 7.51
7	2	18	1	7.14	3.71	2.00	8.11	− 6.76
8	0	19	1	10.15	4.48	....	10.68	−10.15
9	0	19	1	12.80	6.20	....	13.47	−12.80
10	5	12	4	7.33	5.61	5.80	10.50	− 4.57
Total...	42	150	14	8.73	4.79	5.6	10.2	− 6.13

Subject II. was familiar with the experiment. He knew the results of the various methods of observation, yet here we see him passing over all the other possible 'cues,' to what must be the final adjustment among all. The effort most prominent in O.'s mind was fixation; together with this was attention on the pointer except when the sound failed to coalesce with pointer. The important point in introspection was the difficulty connected with the long spread of the pointer at these rapid rates.

At the close of this series, three of the observers attempted to examine the peripheral, or objective factors more closely. All observers were allowed to know the direction of their error. We may quote certain typical statements from the laboratory notes of subject II. These are from the practice series with knowledge.

Coincidence is at 328; O.'s final determination, 327; tentative fixation points, 360, 340, 330, 335, 325, 327. O. reports: "Trouble in seeing the pointer; can't distinguish between pointer and black streak, yet have a distinct feeling that there is a black streak below 320,—no streak above 330." Several days later, O. wrote: "Felt able to distinguish the pointer disturbing fixation, from the positive after image,—it is a difficult task to combine pointer and click even when the pointer is seen clearly." On this day, all determinations, after the first, were either positive or correct; the first was negative three degrees.

The time relation is now the problem presenting the most difficulty for this subject. O. felt that he could seldom satisfactorily combine the two elements into one percept. There was always either a space or a distinct sense of time between. After the difficulty was noticed a few times as a temporal judgment, this seemed to disappear, or, in the later trials, to dissolve into the space relation. When pointer and click are held apart by the space that separates them it is almost impossible to determine which position is the point of coincidence. Sometimes actual coincidence proves to be just between the two points selected; sometimes it is a few degrees below the lower one; more often, with subject II., actual coincidence is above either of the assumed positions. If actual coincidence is 320, the two separate assumed coincidences may be 317 and 310. In this case, such conditions seem to be explained by the click in relation to one or the other end of the 'blur' or near the vanishing point of the positive after image.

Subject IV. shows the more usual distribution of equal numbers of positive and negative errors. (In an earlier series, he had been regularly positive.) In this group of determinations, Subject IV. made shifts in fixation that amounted to approach from both directions. Toward the close, the experimenter also discovered that O. was not attempting to make determinations more accurate than the five-degree divisions. Both conditions operate to produce the equal distribution of positive and negative errors.

Series '7' to '12' inclusive were spent in studying the factors involved, so far as the subject could introspectively ascertain them, as he made the determinations. In series '12' a black screen with white scale marks and the pointer with white tip were used.

TABLE V

SUBJECT IV

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1	3	14	2	4.94	3.52	5.00	5.64	−3.37
2	3	13	4	3.85	2.73	2.00	5.46	−3.25
3	13	5	2	4.75	2.87	6.00	3.40	+3.05
4	8	8	4	4.10	2.91	5.75	4.50	−0.50
5	7	10	4	2.43	1.58	2.43	3.40	−0.81
6	14	6	1	5.09	2.73	5.00	6.16	+1.57
Total...	48	56	17	4.18	[2.72]	4.64	5.07	−0.59
7	2	2	1	5.00	1.80	5.50	7.00	−0.60
8	7	1	3	2.45	1.59	3.57	2.00	+2.09
9	7	3	2	2.50	1.33	2.86	3.33	+0.83
10	0	6	3	1.22	0.86	....	1.83	−1.22
11	5	2	1	3.35	1.62	3.00	6.00	+0.35
12	4	5	0	2.88	1.65	3.75	2.20	+0.44
Total...	25	19	10	2.70	[1.47]	3.44	3.15	+0.48

Subject IV. found difficulty with the after image from the beginning. Often a fixation point would be selected that seemed confused with the image, and O. would change to a safer position. The change in these instances increased the error to a more positive one, if O. had been proceeding cautiously. O.'s criterion was 'to make the pointer slap the mark'; it would seem to bend if he felt he had reached a negative position. Sometimes he attempted to select the 'blackest' part of the streak; in other instances, the best position was where 'the scale seemed to brighten up.' The 'streak' that subject IV. is attempting to analyze was often 20 degrees in length. More commonly it was from five to ten degrees shorter.

Subjects III. and IV. report this disturbing influence of the after image constantly. Subject I. makes no report on it; she is concerned with the 'pull' that the sound has. Subject II. was still uncertain whether he had been led to make negative errors through attention to the sound,[1] or because of the failure to distinguish between the after image and the 'blur' of the pointer, or possibly because of some unnoticed movement of the eyes downward.

In the practice experiments, I (subject II.) gave special attention to the difference between after image and pointer. At the same time I attempted to get the most advanced moment of interruption. The results are massed together in Table VI. below. In series 21, I used the black screen and pointer with white tip.

As can be seen, I succeeded in eliminating the preponderance of negative errors fairly well, and in reducing the general average (see Table IV. above). My tendency proved to be one that led me to select the scale position that was free of the pointer at the time of click. (Prior entry of sound?) The tendency was eliminated by closer attention, not to my fixation, but to the *features of the dial at the fixation point*, so that I might recognize the *first* change or 'interruption' of vision due to the passage of the pointer. Under these conditions the blur can seldom be seen. To my astonishment, the sound still did not seem to occur at this first 'interruption' point, it continued to stand at the 'scale position that was free of the pointer'; caught, of course, with peripheral vision at a later revolution of the pointer. Experience showed me, however, that if I did take this advanced

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[1] Titchener, E. B., 'The Psychology of Feeling and Attention,' p. 253 f.

point,[1] I would be more accurate than by using the other methods. As a rule, blur, after images, angles of the pointer, and other features of the dial are eliminated by the successful attainment of this form of fixation. Squinting, closing one eye, etc., aid at times in this elimination. There is, naturally, no peripheral vision, or a very limited field, during this manner of fixation, hence the procedure becomes one of alternation between 'fixation with peripheral vision' and 'fixation to catch the visual interruption.' The black dial with white scale marks, and a white tipped pointer, give the most easily seen 'interruption,' so far found. We have not yet had time to test this thoroughly.

TABLE VI

SUBJECT II

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
11 to 20.	58	72	23	5.03	....	5.82	6.01	−0.62
21......	8	11	4	2.21	1.84	3.87	1.91	+0.43

We have quite accidentally found four subjects whose results fall in separate classes. Subject I. makes the positive error. Subject II. emphasizes the negative error. Subject III. is positive, but his error is so small that it perhaps comes within the limit of accuracy set by the scale marks, or that set by the daily disposition and accidental combinations of attention. The error made by subject IV. is slightly larger, but the distribution is nearly even. All four were using the same apparatus and presumably following the same set of directions. After studying the features of the moving pointer as they appear on the dial, and then placing emphasis in the directions on a point overlooked previously, the visual interruption, the results of all four subjects' determinations are brought into closer correspondence.

It seems evident from the above material, and other work since Geiger's article is corroborative, that such factors as eye movements, after images, the 'blur' of the pointer, the

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[1] See Dunlap, *op. cit.*, p. 168.

ease or difficulty experienced in finding the 'pointer interruption' position, and other positional factors, such as 'Umkehr' points, angles made by the pointer (as it changes direction across the field of vision), etc., are essential elements in irregularities of error of the grosser sort. With this method also, to attain subjective satisfaction is always a matter of great difficulty. It can be kept only for very short periods of time, and is largely a matter of preference. The usual judgment is: "This is the best I have found; might find a better if I watched longer."

The existence of a temporal 'indifference-zone' seems fairly certain. Under usual conditions the 'blur' and after image streak are distinctly noticeable. At the ends of, or within, this region of the streak, the 'indifference-zone' may vary without conscious change. The assumed coincidence will depend on the momentarily important factors, and any theory O. has concerning actual coincidence within this constellation of elements. The larger errors ought to show a regular distribution around some point other than the actual coincidence, indicating that one of the factors is exercising a primary influence in the judgments. The latter result appears especially well in our experiments.

III

All of our observers, except subject I., mentioned the uneasiness attending determinations, on account of the after image. We, therefore, attempted to plan an arrangement of apparatus that would either eliminate this factor or at least reduce its effect on the observations.

We used in place of the pointer a revolving disk that covered the inner surface of the dial. This disk barely touched the inner ends of the scale marks. It had on the circumference, at the edge, a black spot, approximately the size of the black tip of the pointer. The actual size of the spot was 8 mm. along the radius of the disk and 5 mm. wide. Such an arrangement still leaves the blur of the moving spot,[1] and, as experiment shows, if fixation shifts to the scale marks,

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[1] See Dunlap, *op. cit.*, p. 170.

often gives an after image. Sometimes it appears as long as that produced by the pointer, but usually the short 'blur' is all that can be noticed.

TABLE VII

(*A*) DISK

Subject V

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*b*	6	3	1	5.3	2.96	5.16	7.33	+0.80
2*a*	8	2	0	3.8	2.16	3.75	4.00	+2.20
3*b*	2	8	0	4.6	1.6	5.0	4.50	−2.60
4*a*	6	2	2	4.8	3.3	6.83	3.50	+3.40
5*b*	1	6	3	3.9	3.68	2.0	6.16	−3.50
6*a*	2	8	0	3.6	1.68	3.5	3.62	−2.20
7*b*	2	7	1	2.8	1.56	1.5	3.57	−2.20
Total...	27	36	7	4.1	2.5	4.55	4.55	−0.57

(*B*) POINTER

1*a*	0	10	0	12.5	6.8	....	12.50	−12.5
2*b*	1	8	1	3.9	2.54	3.0	4.50	− 3.6
3*a*	0	10	0	9.1	2.1	....	9.10	− 9.1
4*b*	1	9	0	7.2	3.64	2.0	7.77	− 6.8
5*a*	0	10	0	10.9	3.1	....	10.9	−10.9
6*b*	1	9	0	9.8	5.0	1.0	9.6	− 9.6
7*a*	0	10	0	15.1	4.48	....	15.1	−15.1
Total...	3	66	1	10.07	4.95	2.33	10.57	− 9.87

(*C*) DISK

8*a*	8	1	1	2.9	1.70	3.37	2.0	+2.5
9*b*	9	1	0	7.4	4.18	8.00	2.0	+7.0
10*a*	9	0	1	4.6	2.92	5.11	....	+4.6
11*b*	7	2	1	4.5	2.6	5.28	4.0	+2.9
12*a*	7	3	0	5.2	1.44	5.42	4.6	+2.4
Total...	40	7	3	4.86	2.57	5.42	3.7	+3.82

(*D*) POINTER

8*b*	5	4	1	2.5	1.1	2.8	2.7	+0.3
9*a*	6	3	1	7.5	4.1	6.5	12.0	+0.3
10*b*	9	1	0	6.4	2.7	6.4	6.0	+5.2
11*a*	8	1	1	4.0	2.2	4.5	4.0	+3.2
12*b*	2	7	1	3.9	2.3	2.0	7.0	−3.1
Total...	30	16	4	4.82	3.0	4.96	5.7	+1.14

Directions to the observers remained as before. New subjects were not cautioned concerning the possibility of after images, of the use of an 'interruption' point, etc. Subjects I. and II. of the previous group acted as subjects here. Subjects V., VI., and VII. were new. None of the three knew the purpose of the experiment and only one had seen the apparatus before. Twenty determinations were taken a day. Ten of the twenty were with the pointer and ten with the disk. If we began with the pointer one day, the next began with the disk. The entire left half of the dial was used. Actual coincidence was given in haphazard order. The arrangement of tables is the same as above.

Table VII. represents the first work subject V. had with the apparatus. He was unacquainted with the previous theories and developed his own statements of method. During the first day's work, O. remarked: "When I started I put coincidences in the middle of the long streak; now I am putting them at the lower point, because it is the click that calls my attention to the place and the impulse to follow the pointer would carry me beyond." O. studied the possibility that the 'shadow' might be behind the pointer, but decided that this was not the case here—he could see the pointer up to the click. In series '3,' O. explains his theory still further. "I push around to the lower end of the image, since I seem to 'spot' the click all right, and the after *image spreads out afterward*," *i. e.*, after the click is heard, and as a consequence of the continued movement of the pointer. His theory is not so applicable to the spot on the moving disk, but O. fails to think of this.

O. continued using this procedure until the eighth day. The disk came first this time. O. reported that the disk seemed much easier. The 'spot' on the disk was more easily seen than the black tip of the pointer. Characteristic remarks in series '8' and '9' are: "Didn't notice the after image today:" "Simply tried to pick out where it (coincidence) was and called it." "It depends on where you fixate where the streak appears." O. was not certain whether he had not been using something like this with the disk up to this time. The pointer now hits out the sound for O.

The difference between results with pointer and those with disk is striking. Even with the negative influence of the pointer (which O. consciously used in the first seven) the determinations with the disk are easily within the 'indifference-zone.' 66% of all errors are smaller than the general average, which is 4.1. On the other hand the errors with the pointer are almost all negative and 56% of the errors are smaller than the general average, 10.07. After O. changed his theory and sought to find the place 'where the pointer or spot on the disk hit out the sound,' the difference becomes less. Practically the same factors are now operative in both forms of the apparatus. With either theory the results with the revolving disk are within the temporal 'indifference-zone.' It is possible that with such a large field revolving before the eyes, they will be pulled in the direction of the movement of this field. To avoid this, the fixation point with disk always included a definite mark on the dial, causing frequent after images. Further, O. never fails to report a blur of the spot on the revolving disk. The distinct-

TABLE VIII

(*A*) DISK

Subject I

Series	Number of			Average	M. V,	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*a*	5	4	1	4.6	3.62	3.2	7.5	− 1.4
2*b*	5	5	0	6.7	4.24	6.8	6.6	+ 0.1
3*a*	4	5	1	6.4	3.80	7.0	7.2	− 0.8
4*b*	10	0	0	10.6	5.32	10.6	....	+10.6*
5*a*	2	8	0	5.9	2.08	7.0	5.6	− 3.1
6*b*	3	6	1	4.8	3.00	6.6	4.6	− 0.8
Total...	29	28	3	6.5	3.91	7.5	6.1	+ 0.76

(*B*) POINTER

Series	Pos.	Neg.	Coin	Average	M. V,	Pos. Average	Neg. Average	Abs. Average
1*b*	7	3	0	9.0	5.8	10.14	6.3	+ 5.2
2*a*	5	5	0	6.3	3.5	5.60	7.0	− 0.7
3*b*	8	2	0	7.0	4.4	7.87	3.5	+ 5.6
4*a*	9	1	0	6.3	2.8	6.55	4.0	+ 5.5
5*b*	5	5	0	7.8	3.4	10.00	5.6	+ 2.2
6*a*	5	4	1	7.6	5.0	9.40	7.2	+ 1.8
Total...	39	20	1	7.33	4.17	8.15	6.1	+ 3.26

ness with which the spot can be made to stand out clearly by the least eye movement, makes this differential easy for introspection.

Table VIII. is the work of subject I. It will be remembered that this observer regularly made positive errors in the earlier experiments and found it very difficult to overcome the tendency. She had not worked with the apparatus for six weeks. With the disk she makes a set of determinations that are equally positive and negative. The positive tendency still remains in the determinations made with the pointer. It appeared once in the use of the disk, series '4b' (* in table). My notes here say: "It seems absolutely impossible for O. to get any results that are satisfactory to her with the disk today." The notation 'lost' appears five times in the series of ten, and O. could not work into her disk method. The pointer procedure, which also was unusually positive today, persisted with the disk. The determinations with the pointer seemed fair to O., but with the disk she did not obtain subjective satisfaction in any case. The 'indifference-zone' is relatively steady.

TABLE IX

(*A*) DISK

Subject II

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*a*	1	9	0	8.6	3.88	2.0	9.33	− 8.2
2*b*	1	9	0	6.3	3.62	6.0	6.33	− 5.1
3*a*	3	7	0	7.5	4.40	4.3	8.71	− 4.7
4*b*	2	7	1	5.5	4.10	3.5	6.85	− 4.1
5*a*	0	10	0	9.5	4.80	....	9.50	− 9.0
6*b*	4	6	0	8.2	5.04	9.2	7.50	− 0.8*
Total...	11	48	1	7.6	4.47	6.0	8.12	− 5.4

(*B*) POINTER

1*b*	3	5	2	2.9	1.92	4.33	3.2	− 0.3
2*a*	4	6	0	6.3	5.13	6.00	6.5	− 1.5
3*b*	9	1	0	12.4	5.00	13.22	5.0	+11.4
4*a*	9	1	0	9.0	5.60	9.77	2.0	+ 8.6
5*b*	9	1	0	7.8	5.72	8.55	1.0	+ 7.6
6*a*	8	2	0	7.7	4.04	8.50	4.5	+ 5.9
Total...	42	16	2	7.68	5.32	9.26	4.5	+ 5.28

In the middle of '*6b*' (* in table) with the disk, O. accidentally saw that the last was a few degrees negative. This threw him off entirely in the remainder of the series. The results of Table IX. are poorer than usual since O. had spent the week just preceding with the oculist, and his eyes were not yet in shape to maintain the necessary fixation.

Subject II., the writer, was regularly negative in the earlier experiments. When it came to the use of the pointer in this experiment, O. endeavored to catch the *first* 'interruption point.' Since this was the first work with the disk, there was no way of knowing how to use it. The main difficulty noted by introspection, was the inability to close the space between pointer and click. With the disk, O. found it easier to select the 'interruption-nothing' sequence; while with the pointer, he sought continually to avoid the after image. As results indicate, both attempts were successful. However, the failure to close the 'space' gap reversed the form of the errors. With this 'time' or 'space' gap unclosed, certainty in the judgment of visual 'interruption' tends to produce positive errors with the pointer. Especially is this true if O. is consciously avoiding the after image and as our procedure was arranged, approaching actual coincidence from the positive error side. Introspectively, this 'space' gap with the disk, was clearest when pointer and click were together and this was followed by a blank in the field of vision. This is the 'interruption-nothing' sequence. This proved to be the negative error.

If we examine the individual determinations in this set of records, we find that there is a very irregular distribution curve. With the pointer, twelve of the sixty are errors of 16 degrees and above. With the disk, the error distribution is regular, though the number of determinations is small. The distribution in O.'s previous work was regular, indicating that unfavorable conditions existed here.

In the time available, subject VII., Table XI., did not succeed in obtaining regularity of control. The records still show the differential effects of the disk and pointer. For example, if we compare the results of the first five series

TABLE X

(*A*) DISK

Subject VI

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*b*	1	9	0	6.4	4.28	1.0	7.00	− 6.2
2*a*	0	10	0	17.7	4.60	....	17.70	−17.7
3*b*	0	9	1	17.6	8.60	....	19.55	−17.6
4*a*	1	9	0	19.0	7.20	1.0	21.00	−18.8
5*b*	0	9	1	10.2	7.28	....	11.33	−10.2
6*a*	0	10	0	12.3	5.64	....	12.30	−12.3
Total...	2	56	2	13.86	7.47	1.0	14.82	−13.8

(*B*) POINTER

1*a*	1	7	2	5.8	4.6	2.0	8.0	− 5.4
2*b*	0	10	0	25.7	5.5	....	25.7	−25.7
3*a*	0	10	0	25.0	5.8	....	25.0	−25.0
4*b*	0	10	0	10.3	6.4	....	10.3	−10.3
5*a*	0	10	0	23.8	8.8	....	23.8	−23.8
6*b*	0	8	2	10.1	6.7	....	12.6	−10.1
Total...	1	55	4	16.78	8.93	2.0	18.27	−16.71

TABLE XI

(*A*) DISK

Subject VII

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*b*	9	1	0	34.0	13.4	37.6	1.0	+33.8
2*a*	4	6	0	16.9	6.5	16.0	17.5	− 4.1
3*b*	7	3	0	7.3	3.9	8.4	4.6	+ 4.5
4*a*	6	2	2	10.7	8.8	14.0	11.5	+ 5.9
5*b*	4	4	2	5.9	3.9	5.5	9.2	− 1.5
6*a*	2	8	0	8.1	4.7	10.0	7.5	− 4.0
Total...	32	24	4	13.78	10.2	18.54	10.0	+ 5.78

(*B*) POINTER

1*a*	9	1	0	25.9	11.48	27.55	11.00	+23.7
2*b*	6	3	1	7.5	3.6	8.33	8.33	+ 2.5
3*a*	7	2	1	7.6	5.4	9.14	6.00	+ 5.2
4*b*	8	2	0	9.0	6.8	9.75	6.00	+ 6.6
5*a*	5	4	1	6.2	3.6	9.00	4.25	+ 2.8
6*b*	0	9	1	15.2	7.4	....	16.87	−15.2
Total...	35	21	4	11.9	8.17	13.86	10.9	+ 4.26

with the pointer, with the last five of the disk, we obtain for the pointer group a general average of 11.24 degrees and for the disk group, one of 8.15 degrees. Corresponding absolute averages are + 8.16 and + 0.18.

Subject VI. (Table X.) presents an interesting set of determinations, since he obviously used the 'simple reaction' method in the major portion of his judgments. The average falls on both the median and the mode, and he is consistently negative throughout. His procedure was to move from the first fixation point to others while listening for the sound. When he reached a point where he heard sound and *then* saw pointer, he stopped. The effect of the sound and pointer rhythms, as Dunlap has shown,[1] is to make this interval shorter than the normal 'simple reaction' time. Subject III. also used the sound rhythm, but he *saw* the pointer and click together. His characteristic method was to press the thumb and finger together at the click.

In Subject VI.'s results, we may still note the effect of the disk in removing the, in this case, negative influence of the after image.

The 'indifference-zone' remains a distinct and relatively important element. The varying prominence of adventitious conditions and of other factors noted by previous investigators, is also indicated. The full significance of each element cannot yet be made out. Otherwise the differential introduced by pointer *vs.* disk runs through the experiments here presented. The clear-cut way in which the determinations vary between disk and pointer and finally reverse as Subject V. changes his theory, is still demonstrable in the records of the other subjects, although complicating conditions are more numerous. We can almost measure the 'indifference-zone' in the average of the general averages of Table VII. *A*, *C*, and *D*. It is tempting to offer the difference (5.48) between this and 10.07, the general average for the pointer, given in Table VII. B., as the measure of the influence of the after image.

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[1] Dunlap, *op. cit.*, p. 172 f.

IV

In the practice series, the regularity of results found by Burrow is again demonstrated. The importance of 'exact fixation,' emphasized by Dunlap, is, we believe, thoroughly corroborated by our data. The after image reappears as a factor in the determinations. The *visual* mental content is complex. In our preliminary experiments, we find evidence for nearly all of the factors mentioned by other investigators. Not all of our subjects make negative errors with the slow rates; nor did we succeed in producing consistent negative or positive errors with attention on sound or pointer respectively.[1]

What are some of the factors *seen* at the time of the click, or near the moment of judging coincidence? With the rapid rate used in these experiments, several phenomena may be noted. These vary with the locus of fixation. In following the pointer accurately, a spot that is clearly defined in shape, etc., is seen.[2] In 'exact fixation,' two elements are usually present, the 'blur' and the positive after image. Both depend on the rate of the moving pointer for their length and upon the illumination for their intensity. To obtain the blur no movement of the eye must take place. The appearance of the after image depends on the direction of attention. Unless one is looking for it, consciousness will not take account of its position and size. Our directions to the observer necessitate a distribution of attention over an arc of varying length; and, within the general field of vision, portions of this arc are seen in direct vision, while other parts appear only in peripheral vision. Along this arc, the blur will maintain a regular position at the point of fixation, and the after image will appear indifferently on either side of this point. Attention will note parts of this image.

With very slight eye movements this range is increased enormously. The distance from the point of fixation to which the 'pulses of sensation' extend and the number of their repetitions, seem to depend on the rate of the moving

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[1] Stevens, *op. cit.*

[2] Dunlap, *op. cit.*, p. 181 f.

stimulus and its intensity.[1] Our observers speak frequently of seeing the pointer near the center of the entire disk. The change in angle it makes from the vertical at the lowest point to vertical again is confusing. Subjects trained to make determinations in the upper left quadrant immediately made greater errors when the lower left was added.

A third phenomenon is observed, when, in addition to 'exact fixation,' the subject attempts to limit attention to the 'interruption' of his vision of some seen object at the point of fixation, *e. g.*, the inner end of the scale mark, a defect in the cardboard material of the dial, etc. The tip of the pointer passing 'interrupts' this view. This seems a probable interpretation of the effects obtained by Dunlap in using a white mosquito netting to maintain fixation. Dunlap thus distinguishes by implication between the 'blur' and the 'after image.' He has called our attention to the use of this in detecting eye movements.[2]

Geiger undoubtedly has in mind the entire complex in his attempt to measure the influence of eye movements on the quality of the error.[3] Geiger finds that the greater length of the after image accompanies the upward movement of the pointer, and this condition indicates a greater eye-strain, which is favorable to more negative or less positive amounts in the error on the ascending side. Dunlap speaks of an after image, other than the 'blur,' only in connection with the 'pointer pursuit' method.[4] In our experiments, the 'interruption' was first consciously used by subjects III. and IV. I noticed subject III. squinting and frequently using one eye in making his determinations.[5] When questioned, he replied that he could see the place where the pointer hits the mark better. Subject IV. described it as 'slapping the mark.' It was through an attempt to verify these statements introspectively, that I discovered the great discrepancy between

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[1] McDougall, W., 'The Sensations Excited by a Momentary Stimulation of the Eye,' *Brit. Jour. of Psychol.*, Vol. 1, p. 87 f.

[2] *Op. cit.*, pp. 158 and 168.

[3] *Op. cit.*, S. 426.

[4] *Op. cit.*, p. 184.

[5] Dunlap, *op. cit.*, p. 168.

my subjectively satisfactory determinations and the 'feeling' that actual coincidence gives (p. 108). The use of this factor with our method, reverses Burrow's procedure. He literally moved coincidence to the stationary mark, while here a series of stationary marks were tested and rejected till 'satisfactory coincidence' is found. *Such a visual cue is no longer a moving stimulus.*

In this paper, we have emphasized certain visual factors that enter into the determinations of simultaneity of two disparate stimuli under the conditions set by Wundt's form of apparatus. It is true that these are objective, or mere conditioning elements, and not a part of the fundamental problem set the observer. However, they are a part of the complex constellation of factors and when the final perception is formed, must either be involved as analyzable elements of that complex, or as non-conscious conditions affecting it. In general, a number of factors will be given each time, and the mind has set for it the problem of combining these in the most satisfactory manner. What that final perception will be depends on the factor or factors that are most prominent during that particular judgment.

All the factors that different observers have discovered may not be operative in the formation of any single perception; under the usual conditions of the experiment there are always several present. These are to be united into a perception that has for its central unity the sound and the pointer. Certain main variations have been worked out. For example, the sound 'floats' just as soon as the subject relaxes his fixation. It floats, of course, in one or the other direction according to the prominence of some phase of the attention process. This attention process, I believe, is primarily conditioned by the changing and unstable relations that appear in the field of vision, such as we have mentioned above. The habit character of this perception is excellently illustrated thus: If one has practiced making determinations with a certain quadrant, change to another is immediately productive of uncertainty in the judgments and distinct increase in the error. The effect of different directions of approach also illustrates the point. The experiment offers an especially difficult problem to perception.

Summary

1. Using a rigid method of observation, we see that practice tends to establish a regular distribution curve, which curve exhibits a mode in one or the other form of error. The inference that this is due to the observer's preference for some particular criterion, rather than to some new element in the attention process, seems warranted by the introspections.

2. Results with a revolving disk that tends to destroy the after image and localize the visual factors, differ materially from those with the usual form of rotating pointer. These differences seem closely connected with introspectively observable changes in the length and intensity of the after image.

3. With the 'interruption point' as the 'position' to be united with 'click,' subjective satisfaction is more difficult to obtain but when obtained, is much more accurate than when other criteria are used.

A COMPARISON OF THE FACTORS INVOLVED IN THE MAZE LEARNING OF HUMAN ADULTS AND CHILDREN

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In an account published a year ago, one of the writers of the present paper reported an investigation of the human learning process in the maze.[2] In a previous article, Hicks and Carr had attempted a correlation of the reactions of the white rat, the human adult and the child.[3] A comparison of the two papers suggested to us the desirability of additional investigation in the field of correlation, for the following reasons: The human mazes used in the two investigations were radically different, the one employed by Hicks and Carr being the more simple. Inasmuch as much emphasis was placed upon the fact, in the first report referred to, that the type of learning curve is conditioned largely by the pattern of the maze itself, we assumed that with a maze somewhat more difficult than that used by Hicks and Carr, different results might be found. Our assumption proved to be entirely legitimate, as the two sets of curves obtained present a considerable amount of dissimilarity. In the second place, it was hoped to obtain somewhat more extensive data than those reported in either of the papers. While results upon the additional data obtained were not all illuminating, several items were recorded that we think merit discussion, and some criticisms of the maze experiment suggested.

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[1] The maze tests reported in this article were conducted jointly by both experimenters. The paper was written by Dr. Perrin, who assumes responsibility for the views herein contained.

[2] Perrin, F. A. C., 'An Experimental and Introspective Study of the Human Learning Process in the Maze,' *Psychological Monog.*, 1914, Vol. XVI, No. 70.

[3] Hicks, Vinnie C., and Carr, H. A., 'Human Reactions in a Maze,' *Jour. of Animal Behavior*, 1912, Vol. 2, No. 2.

A pencil maze was used in the present investigation. The pattern, drawn in correct proportions, with the numbers and letters used to designate the various segments, is given in Fig. 1. The grooves, constituting the paths, were of the uniform width of 1 centimeter; the area bounded by the paths 22–1, 3, K, and 19 was 46 by 30 centimeters. The true path measured 210.8 centimeters in its entire distance, and the cul-de-sac segments totaled a length 117.3 centimeters, thus making a total of 328.1.

The maze was constructed by cutting a sequence of grooves through a board 1 centimeter in thickness, and by fastening this with screws to a second board, of the same thickness and size, that served as a base and prevented warping. Between the two a sheet of zinc was inserted, to serve as a floor for the maze. The pencil or tracer was made of hard rubber, rounded at the end, with an aluminum straight handle. It was about 7 millimeters in diameter—so that when held in the proper position in the path, it was not necessarily in contact with either side of the passage.

We had two objects in mind in designing the maze. The main interest was to present a task of considerable, but not extreme, difficulty to the two sets of learners. This was accomplished (1) by a sufficient number of paths and cul-de-sacs—22 segments of the former, 10 of the latter; (2) by cul-de-sacs of varying complexity, and of somewhat different types; (3) by the element of similarity in maze formations that had proved to be a source of confusion and difficulty in our previous experiment. This third element in design also served to accomplish our second purpose, which was to afford opportunity for analyzing and discriminating, as well as for memorizing. We hoped, by the introduction of this situation, to obtain data on possible differences in mental adjustment made by the children and adults. The maze proved to be of about the degree of difficulty desired. The second consideration is discussed in detail in an ensuing section.

Fifteen children and about 25 adults learned the maze. Our results are based upon the records of 10 of the former and 14 of the latter. The ages of the children are given in

Table II. They averaged 11.6 years. They were, with two exceptions, brothers or sisters of students in the university, and we were careful to select those who seemed to be thoroughly normal. The adults with one exception were recruited from the undergraduate ranks of the university, and the exception, *Hv*, was a teacher in the public schools. The adults should average about 21 years of age. The distribution by sex was as follows: children, 4 girls and 6 boys; adults, 7 and 7.

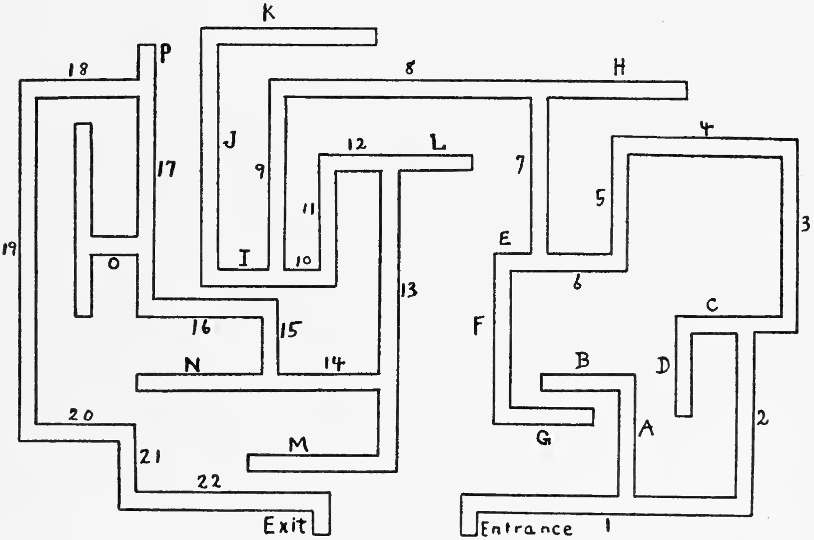

Fig. 1. Diagram of the Maze.

One child refused to apply himself to the task with what we considered to be a normal degree of interest, and never technically learned the maze. His record was accordingly discarded, as were the records of those who were subjected to some unavoidable irregularities during the course of the experiment. We knew the adult learners personally and we were careful to select them on the basis of normality. Both children and adults regarded the performance as a puzzle, and most of them, the children in particular, asked for more mazes at the end of the tests.

The trials were made in sequence. The subjects were given all the time between trials they desired for rest, physical and mental recreation. We recorded, for each trial, (1) time, (2) number of errors, (3) complete route traversed. One experimenter recorded the route, while the other watched the time and checked off the errors. The learners were permitted and encouraged to make oral comments during the trials. These comments were found during the previous experiment not to be a distraction when they were made spontaneously. They were always interesting and expressed personal reactions more definitely than did their more formal replies to questions. In the questions, asked after each trial, they were instructed to tell us, "how they learned it; what places were difficult, and why; what segments they recognized," etc. Formal introspections on imagery were neither attempted nor desired.

In studying the results obtained, we made comparisons from as many different points of view as the data afforded, in the hope that at least certain aspects of the learning would disclose differences between the two groups and suggest correlations. The following represent the various aspects of the learning activity available: (1) quantitative results—totals and averages of number of trials, time, error and distance records, and rate; (2) the distribution throughout the trials of these measurements; (3) knowledge of the maze, at various stages of the learning, and the development of mental reaction to the maze.

I. The Maze Test and its Criteria of Efficiency

There have been, as yet, no satisfactory criteria established for measuring the ability of an animal or human learner in a maze. In fact, the different procedures followed by Yerkes, Watson, Carr and others, concerned not only with the kind of measurement to be recorded, but with the mathematical treatment of this measurement, have made comparisons impossible. The original controversy centered about the relative values of the time and error records; at Watson's laboratory, recent attention has been given to the path record. Vinnie C. Hicks discussed the problem in 1911 in an interesting critical paper.[1] Her conclusion favored the time curve as the best single criterion; not, however, because of its theoretical importance alone, but because "it will yield the best results at the hands of different investigators."[2] Her final conclusion spoke for a combination curve, constructed from both time and error data.

The present writers are inclined to think very strongly that the diversity of opinion in the evaluation of different results is due to the absence of any accurate definition and criticism of the maze experiment itself, as it is ordinarily conducted. Carr emphasized the difficulty of interpreting the learning curve in general, in a sentence by no means conservative: "As yet practically nothing is known of the real meaning of a learning curve and the interrelations of its several features."[3] Certainly, a specific experiment, purporting to call into function any type of intelligence, should be analyzed rather carefully before its results consciously or unconsciously influence our theory of comparative intelligence. Three criticisms rather insistently presented themselves in our study of the results of this experiment, and while they have not, in our opinion, vitiated our conclusions, they have made the reservations which we have attached to them seem advisable.

In the first place, we believe we are justified in raising the question of whether the maze is a fair test of any type of learning ability, inasmuch as it places a rather definite premium upon purely chance discovery. Then, we are not convinced that the different experimenters have emphasized or even recorded all of the significant data. Lastly, we find in the results of the experiment under discussion a noticeable absence of correlation of the various records made by the same individual. These differences must be explained before the respective records are evaluated.

1. Without question, the factor of accidental discovery of correct turns plays an important rôle in the learning of a

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[1] Hicks, Vinnie C., 'The Relative Value of the Different Curves of Learning,' *Jour. of Animal Behavior*, 1911, Vol. 1, No. 2.

[2] Hicks, *op. cit.*, p. 156.

[3] Hicks and Carr, *op. cit.*, p. 123.

maze, at least, with human beings. This is, of course, assumed to be one of the conditions of the experiment. But, unlike most human learning problems, the possibility of logical anticipations of most probable solutions is precluded, during the traversing of any passage for the first time. As the learning process of the human in the maze involves conscious analysis, it is to a certain extent a matter of pure luck whether either *A* or *B*, two subjects equally well equipped, is going to have the better record. This is the frequent testimony of the subjects themselves, and their scores seem to give some credence to their claim. One may not necessarily argue the point from a comparison of the first trial with the ensuing trials. As a matter of fact, the values for the initial trials average practically the same for both the good and the bad learners. Chance may operate during the later stages of the learning: *A* and *B* may both, through a slip of memory or through faulty analysis, find themselves in a new cul-de-sac, let us say in trial No. 10. It will then be partly a chance matter which one will be able to extricate himself in the less disastrous manner. Practically, however, we do not need to assume that the passage is being explored for the first time in order to make allowance for the chance factor. The nature of the situation is such that a subject may be in a passage which he has traversed many times before, and may not recognize the maze formations.

We have concluded in our final statement that chance is not the most determining factor in maze learning. This conclusion is based upon the average results obtained in this experiment. But we are convinced that intelligence does labor under a severe handicap in its attempt to function in the maze. While it is not presumed that the introspective reports furnish anything like an adequate account of the intelligent process, we have found too much variation in the records of the learners using the same method to explain the results on any other grounds. Our chief criticism is, not that chance plays a determining rôle, but that it is a factor which has not been measured, and probably fluctuates. The problem awaits a careful experimental test, in which a number of subjects will learn a sufficient number of different mazes to permit an estimate of the amount of individual consistency. Assuming that other factors are controlled, any lack of individual consistency may be taken as a rough index of fortuity.

2. Our criticism of the importance to be attached to the data usually recorded and discussed, hinges upon the use made of that data. Mrs. Hicks opens her paper with the following statement: "There are three possible quantitative criteria which can be used in representing the learning process of an animal in the maze, viz., the number of errors, the time of the run, and the total distance traversed."[1] For gross purposes of comparison, these data naturally represent the total showing made by the maze learner. That is, by quoting one or some of these total records made, we may label one learner as 'better' or 'worse' than another. But if the maze experiment is worth while conducting at all, our first concern certainly should be to analyze in more detail the learning process itself; and if this is a legitimate aim, the three kinds of data referred to are insufficient. With the knowledge of what the process really represents, individual or group comparisons would be more in order. It is quite true that our learners were given an ideal of efficiency comparable to that held out to the beginner in stenography, or in any practical accomplishment. Both speed and accuracy were emphasized: he was asked to learn the maze in the 'smallest possible time with the fewest number of mistakes, in the long run.' But both standards are definitely used in measuring the accomplishments of of the stenographer, and they purport to measure nothing but the accuracy and speed with which a habit is acquired. One does not take either his isolated time or error curve alone and proceed to establish that as a criterion of efficiency in stenography, and neither does one take the combined standard as an index of intelligence, unless the term is restricted to 'stenographic intelligence.' If we are to rest content, in a similar manner, with measuring the relative ability of the different types to learn a maze, surely a composite time and error curve,

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[1] Hicks, *op. cit.*

such as that suggested by Mrs. Hicks, is required. But we are not sure that such a set of records, even though they were furnished by an inclusive variety of animal and human types, would be of much value to comparative psychology. Their significance, we believe, would be found in their contribution to the problem of comparative intelligence. If we are seeking to evaluate maze records from this point of view, more data is required: we should want to know, for instance, the actual route traversed by the learner. It is conceivable that two subjects may total the same time, error and distance results, and yet one of them show more retracing in the same segments than the other. This might possibly be taken to mean, when the two routes were analyzed, that he made use of greater intelligence.

3. The data afforded by the present experiment by no means made us feel at ease regarding the rôle of chance, nor did it cover completely the point referred to in the preceding paragraph, but it did disclose some of the difficulties we encountered in attempting to analyze and evaluate the respective total time, error and path records. In Table III. the subjects of both groups are assigned places in the columns according to their decreasing order of merit. Some of the interpretations made are as follows: The learner who is thoroughly "good," without qualification or doubt, will be able to show a record indicating relatively few trials, few errors, a small amount of total time, and a small amount of total path traversed. Such a record was made by *B*, *Hv*, and *Hg*. As opposed to this, the poor record will show a large number of trials, much time spent, many errors, and a vast amount of wandering in the maze paths. *McS* and *Lev* have such records. Another point should be noted. The good learner tends to work at a relatively slow rate. That is, he makes fewer errors per minute and covers less path per minute. It does not seem quite so clear, however, that the poor subject necessarily worked at the higher rate, although this is clearly the tendency.

After disposing of the unambiguous good and bad records, how shall we rank the majority of our learners, whose results place them in fluctuating positions in the scale? One may tend to take a large number of trials, but may be able to show a fair time, error and path record, as did *D*. *Hl*, by way of variation, found 42 trials necessary for his learning, and his time record places him as the sixteenth—out of the 24—in order of excellence. But in the matter of errors and excess distance traversed, he stands in the fourth place from the best. Again, we find this record, by *Hz:* a large number of trials, many errors, much path covered, but relatively little time consumed. With *H*, the net result was a poor error score, a large number of trials; but his time and path records were fair. The table shows that such fluctuations were the rule rather than the exception. The reader, after noting *K*'s record in all columns, would probably rank him as inferior to *R;* but if the time record is to be considered alone, *K* is better by four places.

We should expect, on a priori grounds, such variations, due in part to accident, and in part to differences in character and habits among different individuals. The man of the street distinguishes between the careful, plodding individual and the brilliant though erratic one. Unfortunately, neither he nor the professional psychologist have carried the analysis further. Rather pronounced personal idiosyncrasies, suggesting something of the nature of the ones just mentioned, seem to be reflected in our record sheets. Hence our difficulty in evaluating these records in terms of intelligence. *D* extended the number of his trials willfully—his aim, each time, was to "hurry through and start all over again." *McS* showed no lack of intelligence in her comments. In fact, she is decidedly of the brilliant type. But she lacked patience, and at times made the most rapid movements of any of our subjects, often hurdling out of the run-way altogether. She explained that she does everything that way. *W* is inclined to think things over before acting; *Hy* is noted for being impulsive. Other personal traits seemed to have influenced the different curves.

While we are unable to correlate efficiency in the maze with such temperamental traits or with intelligence in any exact manner, a few general tendencies were noted. The individual with good general intelligence—our statement is based upon personal estimate rather than upon scholarship—is not necessarily a good maze learner. This seems to hold true especially in the case of the brilliant type: they are mentally alert and competent to profit by past experience in the maze, but they have a tendency to resort too much to a trial and error method. The maze taxes patience and the quick thinker frets about the slow order of things. The individual too much given to analyzing and theorizing is not necessarily proficient in the maze. Too much time is lost in exploring and testing out theories. Intelligence defeats its own purpose. It seems to be just as obvious, however, that stupidity is represented by poor maze records. We experimented with several subjects who are considered to be somewhat backward, and found a general tendency towards inferiority. The mastery of a maze is a learning process calling for intelligence; but human intelligence is complex and aims at different accomplishments, that is, more freedom and variation in the control of the environment. In terms of the maze situation, the learner can deliberately adopt one of several methods of attack, with the consequence that his different values registered do not parallel.

II. Quantitative Results

Our investigation, as originally stated, had for its purpose a comparison of the records of the child and the adult. The numerical data at hand, giving the individual error, time and distance values by trials, invite numerous comparisons. Some of these may be of significance, some may not. Group comparisons, based upon the individual's order of excellence, as shown in Table III., are not clearly suggested; we had not assumed that age differences would be so clearly pronounced as to permit a seried order of excellence, the line of demarcation of efficiency coinciding with the age line. Even in this table, however, the tendency for the children to predominate towards the inferior end of the series is seen. The method of group averages points to more definite group distinctions.

TABLE I

RECORDS OF ADULTS

Name	No. of Trials	Errors		Time		Excess Distance			Av. No. Errors per Minute	Av. Distance per Minute
		Total	Av.	Total	Av.	C. D. S.	True Path	Total		
						4,453.4				
H.	41	728	17.7	54′ 12″	1′ 19″	4,453.3	3,483.7	7,892.1	13.4	145.6
W.	25	333	13.3	67′ 47″	2′ 42″	3,821.3	4,292.8	8,114.1	4.9	119.7
Mac.	33	453	13.7	27′ 10″	49″	4,499.7	3,045.2	7,544.9	16.6	277.6
McG.	31	528	17.0	107′	3′ 27″	5,808.3	4,947.2	10,755.5	4.9	100.5
L.	41	335	8.2	49′ 53″	1′ 13″	6,015	7,751.7	11,766.7	6.7	235.9
R.	48	553	11.5	111′ 30″	2′ 19″	6,419.6	3,157.9	9,577.5	4.9	85.8
Hv.	24	217	9.0	48′ 7″	2′	2,663.6	1,415.6	4,079.2	4.5	84.7
Hy.	32	711	22.2	108′ 28″	3′ 23″	8,849.1	16,226.4	25,075.5	6.5	231.1
F.	46	839	18.2	74′ 19″	1′ 36″	7,738.7	5,693.4	13,432.1	11.2	180.7
Wd.	47	473	10.6	63′ 16″	1′ 20″	10,703.3	5,591.6	16,294.9	7.4	257.5
Rh.	35	532	15.2	70′ 58″	2′ 1″	5,658.2	5,135.2	9,793.4	7.4	137.9
D.	69	538	7.7	64′ 30″	56″	5,835.6	3,024.7	8,860.3	8.3	137.3
Hg.	27	213	7.8	42′ 16″	1′ 33″	2,450.8	2,141.6	4,592.4	5	108.6
Hp.	27	375	13.9	58′ 51″	2′ 10″	3,707.6	3,354.5	7,062.1	6.3	120.
Total	526	6,828	186	948′ 17″	26′ 48″	78,624.2	66,216.5	144,840.7	108	2,222.9
Average	37.57	487.7	13.2	67′ 44″	1′ 54″	5,616.0	4,729.7	10,345.7	7.7	158.77

TABLE II

RECORDS OF CHILDREN

Name	Age	No. of Trials	Errors		Time		Excess Distance			Av. No. Errors per Minute	Av. Distance per Minute
			Total	Av.	Total	Av.	C. D. S.	True Path	Total		
Hl.	10	42	254	6.0	70′ 45″	1′ 41″	2,771.2	1,870.2	4,641.4	3.5	65.6
McS.	14	89	1,559	17.5	113′ 35″	1′ 16″	14,992.5	12,883.6	27,876.1	13.7	245.4
Lin.	10	38	1,089	28.6	97′ 36″	2′ 34″	9,908.4	7,499.7	17,408.1	11.1	178.3
B.	10	20	254	12.7	41′ 50″	2′ 5″	2,290.	1,697.2	3,987.2	6.	95.3
N.	13	49	599	12.2	55′ 25″	1′ 8″	8,554.8	3,917.	12,471.8	10.8	225.
T.	12	40	520	13.	58′	1′ 27″	5,873.6	6,211.6	12,085.2	8.9	208.3
J.	13	50	828	16.5	64′ 4″	1′ 16″	10,742.6	6,317.7	17,060.3	12.9	266.2
Hz.	12	40	516	12.9	49′ 3″	1′ 13″	5,201.8	3,795.	8,996.8	10.5	183.4
Lev.	11	54	2,171	40.2	173′ 3″	3′ 12″	20,272.7	14,963.1	35,235.8	12.5	203.6
K.	11	58	1,331	22.9	90′ 58″	1′ 34″	13,243.	6,614.2	19,857.2	14.6	218.2
Total	116	480	9,121	182.5	814′ 19″	17′ 26″	93,850.6	65,769.9	159,619.9	104.5	1,889.3
Average	11.6	48	912.1	18.25	81′ 26″	1′ 44″	9,385.06	6,576.93	15,961.9	10.45	188.93

Such distinctions are, first of all, open to the criticism that necessarily applies when small groups are used. As a matter of fact, the small number of subjects is limited practically to the younger group; a large number of adult subjects have learned the maze in connection with courses in experimental psychology, and their results lead us to believe that our older group is representative.

All comparisons of the scores of the two groups (Tables I. and II.) indicate superiority on the part of the adults. That such would be the result was obvious to the experimenters, observing behavior, before the numerical records were tabulated.

TABLE III

CHILDREN AND ADULTS—DESCENDING ORDER OF EXCELLENCE

Children are indicated by heavy-faced type

	No. of Trials		Total Errors		Total Time		Total Distance Traversed		Errors per Minute		Distance per Minute	
1	**McS**	89	**Lev**	2,171	**Lev**	173′ 3″	**Lev**	35,235.8	Mac	16.6	Mac	277.6
2	D	69	**McS**	1,559	**McS**	113′ 35″	**McS**	27,876.1	**K**	14.6	**J**	266.2
3	**K**	58	**K**	1,331	R	111′ 30″	Hy	25,075.5	**McS**	13.7	Wd	257.5
4	**Lev**	54	**Lin**	1,089	Hy	108′ 28″	**K**	19,857.2	H	13.4	**McS**	245.4
5	**J**	50	F	839	McG	107′	**Lin**	17,408.1	**J**	12.9	L	235.9
6	**N**	49	**J**	828	**Lin**	97′ 36″	**J**	17,060.3	**Lev**	12.5	Hy	231.1
7	R	48	H	728	**K**	90′ 58″	Wd	16,294.9	F	11.2	**N**	225.
8	Wd	47	Hy	711	F	74′ 19″	F	13,432.1	**Lin**	11.1	**K**	218.2
9	F	46	**N**	599	Rh	70′ 58″	**N**	12,471.8	**N**	10.8	**T**	203.8
10	**Hl**	42	R	553	**Hl**	70′ 45″	**T**	12,058.2	**Hz**	10.5	**Lev**	203.6
11	H-L	41	D	538	W	67′ 47″	L	11,766.7	**T**	8.9	**Hz**	183.4
12			Rh	532	D	64′ 30″	McG	10,755.5	D	8.3	**Lin**	178.3
13	**T-Hz**	40	McG	528	**J**	64′ 4″	Rh	9,793.4	Wd-Rh	7.4	H	145.6
14			**T**	520	Wd	63′ 16″	R	9,577.5	L	6.7	F	180.7
15	**Lin**	38	**Hz**	516	Hp	58′ 51″	**Hz**	8,996.8	Hy	6.5	Rh	137.9
16	Rh	35	Wd	473	**T**	58′	D	8,860.3	Hp	6.3	D	137.3
17	Mac	33	Mac	453	**N**	55′ 25″	W	8,114.1	**B**	6.	Hp	120.
18	Hy	32	Hp	375	H	54′ 12″	H	7,892.1	Hg	5.	W	119.7
19	McG	31	L	335	L	49′ 53″	Mac	7,544.9			Hg	108.6
20	Hg-Hp	27	W	333	**Hz**	43′ 3″	Hp	7,062.1	R-W-McG	4.9	McG	100.5
21			**Hl-B**	254	Hv	48′ 7″	**Hl**	4,641.4			**B**	95.3
22	W	25			Hg	42′ 16″	Hg	4,592.4	Hv	4.5	R	85.8
23	Hv	24	Hv	217	**B**	41′ 50″	Hv	4,079.2	**Hl**	3.5	Hv	84.7
24	**B**	20	Hg	213	Mac	27′ 10″	**B**	3,987.2			**Hl**	65.6

1. *The Number of Trials.*—A correlation exists between excellence—as measured by total gross values—and the number of trials. The children averaged a greater number of trials by a rough ratio of 4 to 3. Eight of the 14 adults learned

the maze in 35 trials or less. While one child mastered the maze in 20 trials, her nearest competitor took 38 trials. The poorest record made by a child exceeded the second poorest by 31; in the case of the adults, the difference was also 31. Granting that the child's record was more of an abnormality than that of the worst adult offender, and subtracting 30 from the sum totaled by the children, their average would, even at that, be 45.

TABLE IV

THE PERCENTAGE DISTRIBUTION OF ERRORS, TIME AND DISTANCE

Trs.	Average Per Cent. of Errors		Average Per Cent. of Time		Average Per Cent. of Distance	
	Adults	Children	Adults	Children	Adults	Children
1	22.59	19.64	21.84	21.82	25.98	20.65
2	13.26	10.04	11.85	7.40	13.62	10.48
3	9.57	16.2	9.12	13.94	10.13	18.
4	7.79	9.99	3.69	7.70	4.73	10.44
5	4.81	7.86	4.49	5.80	6.02	7.60
	58.82	63.73	50.99	56.66	60.48	67.17
6	3.9	8.41	4.04	3.87	4.45	5.55
7	3.27	3.67	2.62	2.75	2.80	4.07
8	3.10	2.40	3.23	4.33	2.52	6.28
9	1.64	1.74	1.94	1.42	1.16	1.74
10	4.27	2.08	3.06	1.87	3.73	2.
	16.18	18.30	14.89	14.24	14.66	19.64
11	3.14	1.88	2.35	1.17	2.80	2.53
12	2.62	2.88	2.12	1.41	2.13	1.93
13	1.34	1.17	1.66	.97	.89	1.07
14	1.90	1.32	1.92	1.22	2.01	1.09
15	2.08	.60	1.94	1.17	1.60	.54
	11.08	7.85	9.99	5.94	8.43	7.16
16	1.70	1.82	1.81	1.59	1.12	1.61
17	1.63	.35	1.99	.72	1.91	.33
18	1.44	.5	1.48	.66	1.05	.23
19	1.26	.26	1.23	.65	1.19	.41
20	1.40	.36	1.62	.68	1.30	.38
Total for first 20 trs......	92.71	93.17	84.00	81.14	90.14	97.13

2. *Time, Error and Path.*—The error record of the adult again indicates superiority by a considerable margin. The average variation is much greater in the case of the children, due to the contributions of 4 of the 10; but the child with the fifth worst record lacks only 11 errors of reaching the poorest

record made in the adult group. Time and distance show a corresponding balance in favor of the older learner. This balance is the more noticeable with path measurements. Two children exceeded the poorest path record made by any adult. The ratios between the groups for errors, time and distance respectively stand at 1.8, 1.2, and 1.5.

3. *Rate*.—The rate record is suggestive, and partially explanatory of the poorer showing made by the children. Their average time per trial is 1 minute, 44 seconds; for the older learner, 1 minute, 54 seconds—a difference of an amount of time sufficient to permit one familiar with the maze to make the journey from entrance to exit. The child averaged 10.45 errors per minute, while the adult averaged 7.2. A corresponding record is found in the distance results: the child traversed 188.93 centimeters per minute, the adult 158 centimeters. Table III. shows this segregation of the children towards the faster end; and it also shows the tendency towards a correlation between efficiency and a relatively slow rate. The parallel is not exact: *Mac*, with an error record only fair and a path record somewhat better, heads the rate list by virtue of her proportionately lower time record. The case of *J* is somewhat similar. Other variations are represented at both ends of the rate column, but those whose total records were good in all three respects are found to have slow rate scores.

We see nothing in the mathematics of the maze situation to indicate that the efficient learner necessarily makes fewer errors per unit of time, or covers less distance. It is of course possible that of two subjects, one may make twice the number of errors per minute made by the other, and yet equal him in the total errors made; or one of the two may traverse twice as much space as the other per unit of time, both covering the same total amount. *Mac* and *Wd* respectively approximate the first situation; *Mac* and *H* the second. *Mac's* record however seems to be something of an anomaly. The tendency is towards the correlation suggested. Now it is significant that of the six columns given in Table III., only the scores of the last—the amount of space traversed per minute—are under the direct control of the learner. He cannot will to excel in any measurement but he can move his pencil slowly or rapidly, as he chooses. It seems that his chances of a good total record are better if he chooses to proceed somewhat cautiously. If chance were the sole factor, the likelihood of attaining the exit in any trial would increase as the speed of the subject is increased, and as the ratio between errors and time would be constant, a premium would be placed upon speed. This would presumably be the situation in the case of a purely mechanical device, such as a rolling ball, capable of travelling and turning or retracing at random in the maze paths. Success would be, with such a device, purely chance success. Our situation is that in which the learners are intelligent performers, and while some deliberately assumed

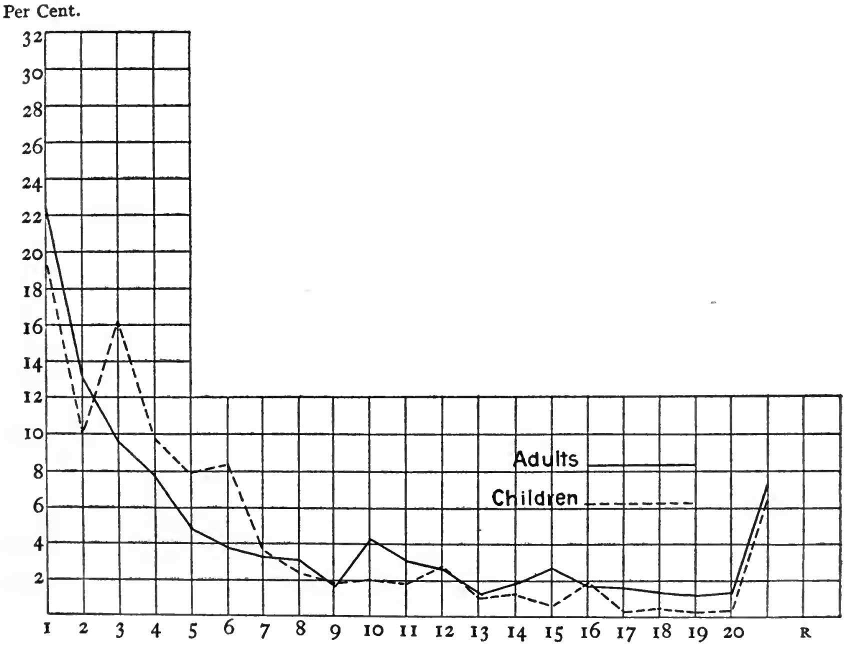

Fig. 2. Percentage Distribution Curve of Errors.

the chance attitude, and relied upon speed, they were as a type unsuccessful. One might consider it advisable to make the first trial a chance matter, and to assume a more rational attitude in the ensuing trials, after the general topography had been learned, but in practice this plan was not attempted. If intelligence, and not chance alone, functions in the maze learning process, evidently our respective groups either differed in intelligence—maze intelligence—or that function operates more efficiently if given time for the fixing of associations, for memorizing, for analyzing, etc. Our alternatives take the following form: (1) As the two groups differ in age and maturity, they presumably differ in intelligence. The older learners are thinking as they are traversing the maze paths; consequently they tend to go somewhat slowly. The maze does not call into play ideational reactions from the children, and having little to think about they naturally increase their speed. If this explanation should hold as stated, it would follow that the child would have little to gain by adopting a slower rate. Their time record would simply be extended. (2) It is conceivable that the child and the adult have practically the same intellectual equipment for the maze. A child of 10 may certainly make a creditable showing in any

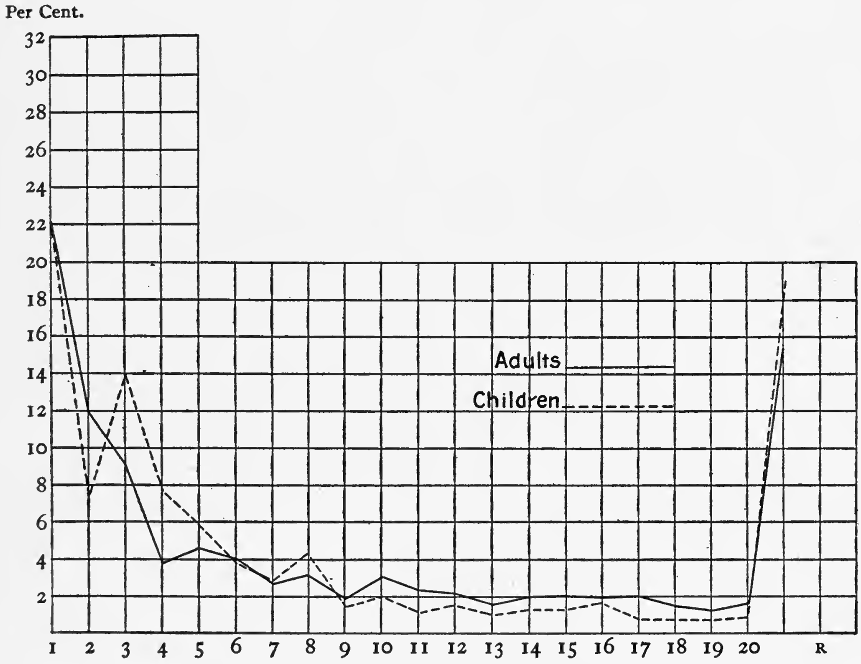

FIG. 3. Percentage Distribution Curve of Time.

isolated test similar to the exercise of any function called for by the maze. But the child lacks the patience, the power of continued application, of the adult. He is more nervous, becomes fatigued the more readily, etc. If this alternative states the psychology of the situation the child would profit by an effort of will leading to more cautious movements in the maze. One may of course push the analysis a bit further and conceive that intelligence operates by choosing the more logical method, and that the child has less ability than the adult to exercise wisely potentialities which they both have in common.

4. *The Distribution of Time, Errors and Distance.*—Table IV. gives the average per cent of error, time and excess distance results for each of the first 20 trials and for the remaining trials considered as a unit. For convenience in ascertaining larger units of distribution, the 20 trials are divided into 4 arbitrary groups of 5 each. Our mathematical treatment of these results is more simple than that made by Carr, as we have assumed that the direct statement of results itself affords a more logical basis for conclusions. One explanation should be given: Carr deals with excess values—scores made in addition to those necessarily registered when the maze is traversed in the shortest possible time without errors. All errors are excess, as is all distance exceeding the length of the true path, and all time over the shortest time made by the individual subject. We preferred to give absolute time results, inasmuch as non-excess time varies with the individual and is probably not accurately represented by his shortest time made. We followed Carr's procedure in subtracting the length of the true path from the path record of each trial, for each individual. While excess distance is by no means confined to cul-de-sacs, all distance exceeding the length of the true path is excess.

The curves given in Figs. 2, 3 and 4, based upon Table IV, show the following characteristics: (1) The adult curve tends to exhibit a more regular and more pronounced slope during the first 10 trials than that of the children. The children's curve is lower in the second trial, but rises above the adults' in the third and shows a more pronounced series of steeples for the following part of the period, particularly in errors and distance. Values are eliminated early in the series, in both curves, and as the trials are prolonged, the percentage of values yet to be scored tends to be the same for children and adults. All curves from the tenth to the end show general similarity. Those of the children extend considerably further from the line of abscissæ, due to the greater number of trials necessary for their learning. (3) After the period of about the twentieth trial, the percentage of time yet to be disposed of is greater than the percentage of errors and path. The adults have more distance than the children to cover after this trial, but about the same percentage of errors.

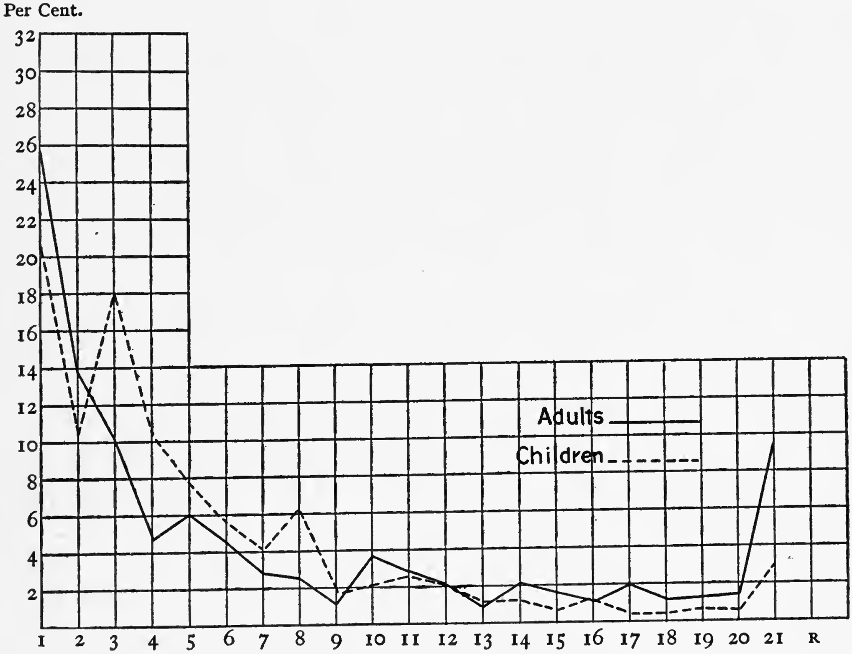

FIG. 4. Percentage Distribution Curve of Excess Distance.

In order to ascertain more clearly the characteristics of good and bad learning as reflected in the distribution of results, we compared the averages of 5 of the best learners, considered as a group, with 5 of the poorest. The first group was made up of *Hv*, *Hg*, *Hp*, *Mac* and *B;* the second, of *Hy*, *McS*, *Lev*, *Lin* and *K*. It will be noticed that 4 adults and

1 child constituted the first group, and that the ratio is reversed in the second.

A detailed statement of the results of this comparison is not necessary, but, stated in terms of the composite curves, it discloses in brief the following: (1) Both the "good" and the "bad" complete the first trial with about the same time and error results—28–9 per cent and 24 per cent respectively. The poorer subjects average more path than the 24 per cent path distance made by the superior group. (2) A somewhat curious phenomenon is found in trial No. 2. The good learners made a relatively poor showing in this trial, in all three values, by a negative margin of about 10 per cent. (3) During the period represented by the first 10 trials, the curves of the better subjects show a more uniform and pronounced slope, while the other curves display more and higher steeples. For these 10 trials however, the curves average about the same height above the base line. (4) From the tenth trial to the end both sets of curves are similar, for the reasons mentioned in our comparison of the curves of adults and children. The good subjects averaged 26.2 trials, the poorer ones 54.2.

It might appear that in comparing the beginning values for the two sets of records the difference in the number of trials should be taken into consideration. The first fifth of the better group would include their first five trials, while the corresponding fifth for the other would extend to trial No. 11. Thus it would seemingly be misleading to compare the second trial of the good with the second of the inferior. But in both sets of curves, the values for the first 10 trials are practically the same; and our notes on introspection and behavior indicate that the respective first 10 trials are comparable. The learning is practically completed by this time, and new factors account for the extension of the poorer set of curves. Hence, we are unable to agree with Carr's argument that the extent of the initial slope is dependent upon the number of trials in the curve.

The individual variations found in the groups again suggest caution, but certain points of consistency justify an interpretation of the results of the comparison. (1) It is obvious that the percentage showing of the first trial is not to be taken as an index of future developments. The trial does disclose the fact that for both sets of subjects more time is eliminated than errors or path. This is merely indicative of the hesitancy with which all learners inaugurate their attempts, before the nature of the experience is known.

(2 and 3) The period included by the first 10 trials represents the critical stage of the learning. Our notes on behavior and our introspective reports, together with the fact that the curves show their greatest points of difference during this period, establish this fact. An analysis of this part of the curve shows two things: (*a*) Bad records tend to occur in individual trials, followed by better trials: that is, the curve takes the form of a series of steeples, rather than that of a high plateau. This holds for all of our records examined, including those of the group under discussion. In the case of the subject with a poor total record, the steeples are higher and more irregular than those found in the good curves. (*b*) Since two of the better group make a creditable showing in trial 2, we may not attach undue importance to the results of this trial, considered by itself. But it is significant that all of the good subjects did relatively poorly in this trial. The net result in practically every case is that the good subjects average a larger per cent of all values in the first two trials than the poorer subjects, for these same trials. All subjects tend to show a slump somewhere in the series, and this tends to come earlier for the better subjects. This, in conjunction with the steeple phenomenon, results in a more uniform and sloping descending curve for the superior subjects in the first 10 trials.

These differences in curve characteristics admit of an explanation in terms of intelligence. Intelligence asserts itself in two forms: (*a*) in the elaboration of schemes of control, or methods of attack—the mature learner is more likely to attempt such expedients as sticking to one side of the path. Such schemes may or may not prove to be well-advised. (*b*) The more intelligent subject does more thinking in the shape of analyzing maze experiences. He does more discriminating and memorizing, and is more capable in the matter of recognizing maze segments. Moreover, this greater ideational activity begins in the first trial.

The motive in this first trial is the same with all—to attain the exit. Intelligence does not seem to assist in this immediate end, if our conclusion regarding the numerical significance of the first trial holds. But it does function in acquiring a more accurate knowledge of the maze.[1] Two subjects may thus complete trial 1 with the same quantitative results, but with a difference in the amount of retained experience to be applied to the ensuing trials.

It might seem that if this were true the better equipped subject should expedite matters in the second trial, but such does not seem to be the situation. Our explanation takes into account the fact that the intellectually weaker learner is more likely to follow in an unthinking way the route traversed in trial 1, in his second attempt. He does not realize the extent to which he has been retracting from blind endings. The better subject has realized this, and is consequently more uncertain in his second trial. The result is more analyzing in trial 2, more exploring, and a temporary poor showing. He begins his attempt to establish the details of the true path earlier in the series of trials than does the subject who resorts to a less rational method.

It should be remembered that a subject, in any prolonged trial, traces back and forth over the maze segments many times. The first trial, or any similar trial, is thus equivalent to several later in the series. In this initial extended exploration, the learner is not only elaborating his concept of the route, but he is also establishing habits. The less alert subject during his beginning trials tends to build up more false ideas about the maze, and to establish habits for cul-de-sac segments, incorporating them into the true path. In the course of time these are forced upon his attention. The result is the necessity for the reconstruction of ideas and for the breaking up of inadequate habits.

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[1] See page 26.

The weaker subject thus not only finds analysis more difficult, but he is handicapped by being forced to "start all over again," to use a quotation which more definitely expresses his difficulty. This ordeal is passed through in trials 3–10. That the quantitative expressions of this period take the form of irregularities rather than that of a uniform high score of errors, time and distance is another matter. Explanations of steeple phenomenon were attempted in the writer's monograph article. As far as the child is concerned, a prolonged trial during this period is usually explained by him on the grounds that he was "lost." This means less ability to recognize. Further analysis discloses the fact that the learning takes place in terms of the acquisition of the ability to recognize and anticipate cul-de-sac openings, rather than the cul-de-sacs themselves. Either a good or a bad learner, at any stage of the process, may become lost; but the better learner is more quick to reestablish his orientation, after he regains the true path.

Several modifications should be appended to our explanation. We wish particularly to refer to the chance element, criticized in our preliminary statement. That the better records of the adult, generally speaking, are to be accounted for in terms of intelligence is a conclusion supported by our various data, but the handicap of the rational element is emphasized in the results of the first trial, and in the lack of consistency found in many of the trials that can not be explained on other grounds. Superior reasoning does not facilitate the attainment of the exit in the first trial, as it should do if it were an unhampered functioning agent. Practically all of the subjects expressed surprise when the exit was reached for the first time, thus indicating the haphazard nature of this trial. Then we must make due allowance for a chance discovery of the true path early in the first trial. The good reasoner would of course follow this in the second and succeeding trials. *McS* and *Hv*—about our poorest and best subjects respectively—both made exceptionally good initial records. *Hv* was the more successful in this trial, but her total success is out of all proportions to the accomplishments of *McS*. Hence we assume that *Hv* made better use of the knowledge acquired in this first trial, as well as in the ensuing ones. Her first drawing (section III.) was infinitely superior to that of *McS*. *Hv* was, incidentally, our most mature and experienced subject. We conclude that both were favored by chance at the start, but that the intelligent factor operated in *Hv's* favor as the trials progressed. But we find that many of our subjects who to all appearances reasoned well in the maze, did poorly at the start so that they were evidently working under a greater chance handicap than *Hv*, unless we wish to credit her with more than exceptional intellectual ability in the maze. This we are unwilling to do.[1]

Differences in fatigue, temperament, general emotional tone, entered into this period to some extent, although we show reasons later for thinking that they were more influential towards the close of the series.

Again we should modify the implications of our explanation with the statement that a bright child may show evidence of as successful an intellectual performance as that evinced by the normal adult. In a mental test employing the maze subject *B* certainly would qualify as an adult.

More uniformity is found in the trials from the tenth on, as result (4) stated. The important turns of the true path had been learned by this trial, and the rest of the effort was centered upon minor difficulties. By the stage of about the twentieth trial, all subjects were definitely motivated by a

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[1] The writer, in his monograph study of the maze process, contributed one of the many criticisms directed against Thorndike's principle that a pronounced slope of the learning curve indicates intelligence. We held that the slope of the maze curve is determined by the pattern of the maze. Carr objected to Thorndike on the grounds that the number of trials conditions the apparent slope of the maze curve. That criticism has already been stated and discussed. Our curves for the maze described in this paper show general points of similarity, as contrasted to curves from other mazes, hence our first point seems to be substantiated. In a corresponding manner, the curves for any given problem are characteristic (compare Ruger's puzzle curves). But within the curves of a given maze Thorndike's principle seems to apply. Assuming that the reasoner could master the maze—or the puzzle, for that matter—in the first trial, his curve would drop to the base line. Purely accidental discovery in the first trial would not produce this curve unless retention were perfect in ensuing trials, but that would presuppose the rational factor in a high degree of efficiency.

special desire—to run the 5 consecutive trials without error. An error in the fifth trial, following 4 perfect trials, meant a series of at least 5 more. In its intelligent aspects, this was a task of practically equal difficulty for both children and adults. But the greater fatigue shown by the children, the more obvious signs of nervousness on their part, began to tell and contributed largely to their added trials.

5. *Error and Distance Comparison.*—The ratio between the number of errors and the distance traversed was a matter of interest in the light of some conclusions made by Carr. In his error and distance comparison, it was found that his groups, human and animal, eliminated distance more than errors, but that the period during which the elimination took place varied with the groups. For the adult humans, it was in the first half of the learning; for the animals, in the second half. The children occupied a mean. This was interpreted as a quantitative representation of the intelligent factor: the humans, more particularly the adults, learned in time to recognize cul-de-sac entrances, after turning into them, without the necessity of traversing the entire cul-de-sac distance. By the method of scoring, this behavior would enroll as many errors but less distance. Neither our ratios nor our notes on behavior indicate that such a numerical index of intelligence is to be found in our results. In the curve of the adults, the path record gains on the error record 2 per cent in the first quarter, loses this amount in the second, and is behind about the same amount at the end of 20 trials. For the children, more distance than errors is eliminated in the first quarter by 4 per cent, in the second quarter by 1 per cent., and at the end of the last trial, by 4 per cent. The greater elimination of distance is only obvious in the case of the children, and not to the extent to be regarded as significant.

Particularly noticeable was the behavior tendency of all individuals invariably to go to the end of every cul-de-sac entered. This may be explained either upon the grounds that we employed a pencil maze, not one through which the subjects walked, or that our maze was more difficult by way of homogeneity and number of passages than Carr's. The learning process in the pencil maze goes on essentially in terms of the anticipation of turns to be avoided, as the subject is going through the maze, rather than in terms of the recognition of cul-de-sacs once entered. Such reports as the following were consistently given to us: "I learned not to turn down at that point, but to keep straight ahead." With the anticipatory idea in mind a few centimeters ahead of the turn in question, the learner was able to avoid it. The subject as a rule did not recognize that he was off the true path until he had reached the end of the cul-de-sac; or if the premonition of danger was given to him, he usually decided to "go ahead anyhow" for purposes of verification. By comparing the diagrams of the two mazes, one may understand the difference in behavior found by Carr. The point at least would make one hesitate before assuming the maze learning process is the same with any type of maze.

6. *Time.*—The curves demonstrate the noticeable extent to which time is eliminated more slowly than errors and distance, for both groups. From the twentieth trial to the end the child and the adult had respectively 18 and 16 per cent of their time yet to spend in making less than 10 per cent of the errors. This indicates a common element in the learning process rather than a group distinction. The maze was practically mastered by all by the twenty-fifth trial. The concern of the learners from this period on, was to make the 5 trials in succession without errors. A mistake in the fifth trial without errors meant 5 more trials. Hence extreme caution—*i. e.*, a slow rate—was universally in evidence in the latter trials.

III. Introspective Contributions

Our attempt to obtain and record an introspective account of the learning process necessarily involved a rehearsal on the part of the subject of his intelligent processes after each trial, and the attendant danger of learning between trials. Since this danger could not be avoided, we endeavored to make it a common factor with all the learners by making the introspections as systematic and uniform as possible. As a matter of fact, the end of the trial generally found the subject interested in his performance and more than willing to discuss it. The danger could not have been avoided merely by dispensing with this data.

The aspect of the subjective learning process most easily obtained is the learner's progressive knowledge of maze paths and cul-de-sacs. This was given to us by the subject primarily through a series of drawings or diagrams, supplemented by oral explanations. Drawings were asked for after the first and last trials, and after intermediate trials in which acquisition of new points was reported. At least 5 drawings were made by each subject. We had given consideration to the possibility that a mere difference in the ability to draw might unconsciously affect any conclusions suggested after comparing the two sets of drawings, but since the diagrams were explained and defects obviously due to lack of ability with the pencil corrected, this difficulty was obviated.

While the drawings were sufficiently clear, the matter of passing adequate and fair judgment upon them did prove to be rather difficult. Some included too many path-ways, some not enough, and the proportions varied independently of the accuracy with which the sequence of paths was indicated. We attempted a double scoring method, that omitted the evaluation of proportions and a consideration of cul-de-sac paths. These cul-de-sac passages were hazily drawn by all of the subjects, and that fact in itself indicates that the thinking was essentially concerned with the true path and the location of the "bad places," or false turns—a point that receives ample substantiation in their verbal statements. The subjects were scored, (1) upon the number of segments correctly included, (2) upon imaginary segments believed to belong to the route.

The results of the first and final drawings are recorded. The intermediate diagrams for each subject seemed to be of a degree of excellence represented by a mean between his initial and final attempts. Our method gave us a comparison between the two groups that certainly would be in harmony with the verdict of anyone who should examine all of the drawings. The adult disclosed a superior knowledge of the maze, and also showed a greater tendency to incorporate paths not actually belonging to the maze. Out of the 22 segments of the path, the adults averaged at the end of Trial 1, 8.9 paths drawn correctly, 10.3 excess; the children were able to draw 6.1 paths, and included only 3.2 unnecessary runways. These proportional amounts of actual and excess paths gradually approached the final drawings, which show less dissimilarity. At the final trial (that is, for each subject) the adult included 17.5 out of the 22 paths, and retained 4.3 imaginary paths; the child's results were 18.2 and 2.9. *Hp*, an adult, was scored 7 on his final drawing. As the adult's drawing judged to be the next poorest received a grade of 17, and as the child's final drawing ranking lowest was graded as 16, we would, by omitting this one case, judge the two sets of final diagrams to be about of the same merit, or rather, rank those of the adult as slightly superior. We were unable to decide in favor of either group in the matter of proportions. The adults showed a greater tendency to recognize all the turns of the maze as being right-angled turns, to estimate the size of the maze and the lengths of its various path-ways more correctly.

A tendency towards correlation between accurate drawings and good maze records is in evidence, but it is by no means unambiguous. *Hv* made by far the best drawings, *Hp* made the poorest, of any of the subjects. *McS* made miserable diagrams, and those of *J*, *K*, *Hy* and *Rh* were only slightly better. *B*, an excellent subject, was only mediocre in this respect; *Hg* was somewhat better. We had demonstrated before that only an uncertain positive relationship obtains between ability to traverse a maze route and ability to describe it, either verbally or graphically. One may complete the learning and be unable to describe more than the general course of the route.[1]

With the greater knowledge of the maze displayed by the adult there was also sufficient evidence of more ideation—more thinking. This does not necessarily mean that the adult

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[1] Perrin, *op. cit.*, p. 28.

paid closer attention to his task. The distinction is rather to be found in the more pronounced tendencies towards theories and interpretations—probably represented by the greater number of superfluous pathways noted in the drawings. Reports of schemes and methods came predominately from the adults—plans to "wiggle" the pencil, to follow one side of the path at a time, to explore systematically, to count sections of the path. More ideas and suggestions, whether legitimate or not, seemed to occur to the adult than to the child during the trial.

Although the child and the adult represent different stages of intellectual equipment, these degrees are separated by a fluctuating line of demarcation. Subject *B* has already been mentioned as a child who gave all indications of mature thinking. The maze experience is unique in its presupposition of any kind of technical knowledge. A few elementary spatial concepts, such as those of a "T" turn or an "L" turn, and a primitive knowledge of spatial relationships on a plane, constitute all of the capital stock of information necessary. And these concepts may be acquired for practical purposes in the first trial. We see no reason for assuming that differences in mere knowledge background should count in favor of the adult. If the knowledge factor is reasonably well equated, our distinction remains to be expressed in terms of associational and retentive activities, of a general rather than a specific sort, inasmuch as the pencil maze presented a novel experience to all of the learners. Distinctions in these abstractions, found between the human ages of 10 and 21, are to be made only with considerable reservations.

IV. Methods of Attack

Two methods of attack were fairly well defined among our learners. They represent the extreme types of such behavior as is under the control of the learner, and while intermediate stages were described, the majority of the subjects may be practically classified as falling into one of the two groups. The terms "rational" and "trial-and-error" have recently come in for some long-needed criticism, and we feel that they are inadequate and misleading for our classification, but our two methods approximate something like the antithesis connoted by these phrases. A subject may avowedly and deliberately rely upon chance in his effort to discover the correct sequence of maze paths. He will hurry through the path-ways, turning at random where opportunity affords. He will make no special effort towards analyzing. As the result of his random endeavor, success will eventually come, and the correct turn will tend to "stick" in his mind. Even then, he may not exert himself in any attempt to remember the turn, but will trust to a repetition of the experience to guide him the next time. As opposed to this, we find the studious, analytical type. We certainly should not say that the first type exhibits a non-rational behavior; neither is it implied that the learner of the second type does not consciously fall back upon the element of chance. All testify to the "trial and error" nature of the process. The distinction is made more difficult by the appearance of the subject who chooses to work rapidly in the maze, gambling upon the greater likelihood of welcome discoveries through rapid excess activity, but attempting as much analyzing as his behavior will permit.

No consistent correlation holds between method and the age of the learner. It should be stated that in our preliminary directions we urged the utmost freedom in the matter of method, suggesting only that the learners employ the tactics which they think will produce the best ultimate results, and notifying them of the three records to be kept. The greater proportion of both groups were decidedly deliberate. The children certainly were not open to criticism on the charge of slip-shod method. *B*, *N*, *T*, *Hl* and *Hz* were thoroughly studious and conscientious. *Lev*, *Lin* and *K* were difficult to classify. *J* attempted the combination of rapid movements with seemingly intense mental application. *McS* consistently sacrificed everything to speed, and represents the extreme of the trial and error type. The adults do not all fit themselves readily into our scheme of classification. *W*, *Hv*, *R*, *L*, *McG* and *Wd* all studied their problem. *H* and *Hg* did so most of the time, but were subject to aberrations towards the end of the series. *Mac* worked rapidly but carefully. *Hy* was somewhat erratic, generally tending to emphasize random try-outs. No case similar to that of *McS* was found among our adults, although an identical instance was furnished by R. B. O., reported in our previous paper.

That a rational method is necessary for efficient maze learning is obvious. Such an intellectual attempt may easily defeat its own purpose if it runs to over-speculation. It then leads to excessive deliberate exploration and to highly elaborated concepts of maze formations that do not exist. The tendency, by no means confined to the laity, to disparage the performance of the adult in mental tests, finds some support, but more adverse criticism in our conclusions. If theorizing is disastrous in the maze, practical thinking counts largely towards success, and other things being reasonably equated, we should decidedly credit the adult with the more average ability of this sort.

V. Fatigue, Motor Stability and Emotion

Our statement has so far explained the superior excellence of the adult from the point of view of more efficient intelligence. A group of non-intellectual factors however contributed to the juvenile inferiority. Possibly they influenced total results as much as rational activities. These factors were more obvious than tangible, but we have conveniently referred to them under three arbitrary headings: (1) Although the children displayed an amount of effort towards the solution of an exacting task that almost amounted to heroism, their patience began to be considerably strained by the fifteenth or twentieth trial, and signs of greater fatigue were unmistakable. As the number of their trials was greater, this may be taken as either cause or effect. But by trial 20 they had the route practically under as good control as the adults, and they therefore took more trials in eliminating minor errors, the nature of which they perfectly well understood. Hence we conclude that lack of patience, due mostly to fatigue, prolonged the trials. (2) The test was after all of the nature of a class-room exercise, and our adults were selected from a somewhat restricted group—with one exception they were university students, hardened to the routine of laboratory habits. It was unfortunate but unavoidable that the trials were made in sequence. These facts would suggest the possibility of the superior organization of general habits on the part of the adult. (3) Emotional tone seemed to vary with the success of the learner, irrespective of his age. Interest fell when success was slow in coming, and something almost amounting to elation marked the discovery of correct reactions. The adult seemed to suffer from the effects of discouraging results as much as the child; both assumed with equal emphasis that their mentality was being examined, and were correspondingly sensitive in the matter of their scores. Although indications pointed to more similarity than difference between the groups in this respect, we are not prepared to assert positively that an intrinsic distinction is not to be found.

VI. Summary of Results and Conclusions

The maze experiment affords opportunity for a comparison of the learning curves of different human and animal types, but it is open to serious criticism in that it places a premium upon purely chance discovery—a factor which, whatever its importance may be, has never been measured.

A standardization of the technique of conducting the maze experiment, and a more detailed study of the significance of its learning curve, are needed before its value in comparative psychology is established. It is suggested that a study of the actual route traversed by the learner may yield valuable results, and that a combination time and error curve should be employed in evaluating quantitative records.

On the basis of total error, time and distance results, and the number of trials, the adult of 21 is superior to the child of 11 in the pencil maze. Only loose correlations are found in these criteria, in the records of our learners, hence our standards of excellence are not definitely established.

The factors counting for the superiority of the adult are more effective intelligence and more motor stability.

A graphic representation of intelligence is found in the initial part of the learning curve; the effect of fatigue and motor control are found mainly in the last part of the curve. The intelligent learner tends, (1) to make a relatively poor record in the first two trials, (2) to eliminate values gradually, the result being a curve which shows a relatively more pronounced initial slope and absence of steeples, when compared with the curve of the child. Fatigue shows its effects more towards the end of the learning by extending the number of the trials, after the maze has been actually learned. This prolongs the curve, especially that of the child. The crucial part of the learning for both the child and the adult is represented by the first 10–15 trials; the ensuing trials represent the effort of the learner to perfect the route.

The quantitative differences in the two sets of records are substantiated by the introspective and observational data, which indicate the adult's superior knowledge of the maze, his more efficient regulation of the rate of movement, and evidence of less repetition of maze path-ways.

All distinctions found to hold between the child and the adult are extremely relative. Some of the children seem to compare in every respect with the normal, average adult; and the reverse is found to be true, especially when the records of adults obtained in a previous experiment are examined.

THE EFFECT OF UNIFORM AND NON-UNIFORM ILLUMINATION UPON ATTENTION AND REACTION-TIMES, WITH ESPECIAL REFERENCE TO STREET ILLUMINATION

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A. Introduction

The problems of street illumination have not been approached hitherto from a psychological standpoint. The principal considerations have been photometric and æsthetic, but it is evident that these are insufficient. Mere ability to discern surface irregularities, vehicles, pedestrians or obstacles on the street or sidewalk does not insure safety unless the reaction to these stimuli is normal. If, under a given system of illumination, the reactions of the driver or pedestrian are retarded and the degree of attention diminished, the danger is manifest. From casual observation it would appear that some such factor is present. Many automobilists speak of feeling "sleepier" under certain varieties of street-lighting. Moreover on theoretical grounds the monotony of some lighting systems might be expected to influence the mental state.

During the summer of 1914 the writer was connected with an experimental study of street illumination conducted by the Street Lighting Committee of the National Electric Light Association on Intervale Avenue, New York City. It was found feasible to test the attention and reaction-times of a subject walking on the street under various conditions of illumination. The results thus obtained proved rather significant and the following winter the problem was reduced to laboratory form and experiments conducted in the Harvard Psychological Laboratory. The following article thus comprises two parts, an account of the experiments performed upon the street and of the supplementary experiments in the laboratory.[1]

B. EXPERIMENTS ON THE STREET

I. *Equipment*

The following experiments were conducted at Intervale Avenue, Bronx, N. Y. Two sections of the avenue, each a third of a mile in length were equipped for experimental purposes. In one section series circuits fed lamps mounted at the curb 14 ft. above the street, staggered at 50 ft. intervals, with an alternative system of centrally hung lamps 24 ft. above the street at 150 ft. intervals. In the other section the lamps were suspended from span wires and could be moved to any position transverse the street, or from a height of 18 to 30 ft. Provisions were made for mounting series lamps at 75 ft. intervals with two series circuits feeding alternate lamps. The circuits were operated from tub transformers in a temporary sub-station supplied by a 2,200 volt circuit from the Hunt's Point sub-station of the New York Edison Company. The street was 60 ft. wide from curb to curb and paved with Belgium block. Dwelling houses, small apartments and a few stores lined the sides. The street was reasonably free of traffic during the evening.

II. *Method.*

Certain members of the Street Lighting Committee conducted experiments designed to bring out the ability to discern surface irregularities and obstacles in the street under various conditions of illumination. The writer confined himself to attention, reaction times and motor coördination. The efficiency of the eye, while undoubtedly of importance where a person is exposed to street illumination for a prolonged period, as in the case of the motorman, was not studied. It was deemed advisable to restrict the experiments to relatively short periods of continuous exposure to a given illuminating system—the condition encountered in the case of the average pedestrian.

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[1] The writer expresses his obligations to Professors Hugo Münsterberg and Herbert S. Langfeld and to Messrs. Preston S. Miller and W. F. Little, of the Electrical Testing Laboratories, New York City.

To measure the quickness of response to unexpected stimuli and the ability to make a quick association and carry out the appropriate movement, the auditory choice reaction was adopted. Auditory rather than visual stimuli were used because they left the subject free to look down the street as he would normally. It was desired to reproduce as closely as possible the situation that is met in practical conditions, and with red and green lamps for stimuli the subject would tend to keep his eyes fixed on the point from which the stimulus was to come rather than directed down the street. Two electric bells of distinctly different pitch were connected in parallel. In series with them were two presselles at the end of 10 ft. of lamp cord, a signal magnet and dry cells. The magnet and a 100vd. electric tuning fork recorded on a kymograph. The whole apparatus was mounted rigidly and clamped to a table built on the truck of a baby carriage. The subject walked down the traffic line midway between the center of the street and the curb, holding a presselle in each hand. The experimenter followed at a distance of about 8 ft. wheeling the apparatus. The subject was instructed to look down the street as he would under ordinary conditions, and when he heard the "cow-bell" to release the right hand and when he heard the "door-bell" to release the left. Trials for the right and left hands were given in irregular orders and at irregular intervals without warning. Right and left reactions registered similarly and were averaged together, approximately the same number of both being given in a series. Incorrect reactions were few (not more than 5 per cent.). The subject reported mistakes and such reactions were omitted from the results. The subject walked continually except when it was necessary to stop a moment to shift the drum of the kymograph. The subject always passed through the complete distance between lamps or multiples thereof, depending on the spacing, so that reactions were made at approximately all degrees of illumination under a given system. Fifteen to twenty reactions constituted a series according to the capacity of the drum, *i. e.*, from 60 to 80 reactions for a subject on a given comparison.

A form of short exposure test was adopted as a measure of attention. A box was constructed 30 × 30 × 30 cm. with a 15 cm. tapering extension on the top through which the subject could look at a field 45 cm. from the eyes. The interior was illuminated by two battery lamps screened from the eyes of the subject and controlled by a switch and an opening at the bottom of one side of the box allowed cardboard material to be inserted. This tachistoscope was clamped on the baby-carriage. Simple geometrical figures such as a square, circle, trapezoid, etc., constituted the material. It was desired to test not merely memory for a series of separate figures, but rather the ability to analyze them out of a complex group, which necessitated a high degree of attention. Accordingly five were drawn in outline with ink on light gray cardboard 30 × 30 cm. so that four of them touched at the center of the card, above, below, right and left, and the fifth was concentric with the field. The subject placed his eyes at the opening and one second after the signal "ready" the field was exposed for one second by closing and opening the switch in time with the swings of a half-second pendulum attached to the side of the tachistoscope. Immediately after the exposure the subject drew upon a pad as many as possible of the figures in the correct position.

Exposures were made at intervals of 15 ft. along the traffic line, a new permutation of objects being shown each trial. When walking between trials the subject kept his eyes directed down the street as he would normally. In scoring the results a figure in the correct position was counted 2, and a figure merely disoriented or displaced less than a quadrant from the correct position counted 1. The benumbing or quickening effect of a given illuminating system on the mental setting would presumably not disappear in the few seconds that elapsed while the subject was lowering his head to the opening of the tachistoscope and awaiting the exposure after the signal "ready." The gray cardboard was used as it afforded less contrast with the street surface upon which the subject had been gazing a moment before. Ten exposures usually constituted a series, in some cases twenty—*i. e.*, forty or eighty trials on a given comparison. In some instances several subjects (never more than five) were used at the same time, each one stepping aside to make his drawing while the next one observed the figures. Care was taken that no exposure should be made while there was any distracting motion among the subjects.

As a measure of motor coördination a "three-hole" test was employed. A board 20 × 20 × 1 cm. had three 12 mm. holes bored in it at the vertices of an equilateral triangle 10 cm. on a side. A brass plate covered the bottom of the board and was connected in series with a signal magnet and dry cells. A tapping stylus formed the other pole of the circuit and contacts made at the bottom of the holes were registered by the signal magnet on the kymograph. The subject was instructed to insert the stylus in each hole successively as rapidly as possible, being sure to touch the brass plate at the bottom of each hole before passing to the next. The results were thus reduced to terms of speed. The test was always performed under a lamp where the holes could be plainly seen, the inside of them being painted black to render them more visible against the white board. The actual illumination of the board was not of importance as long as the holes were clearly visible and there was no distracting reflection of light from the board. The subject tapped for 30 seconds from the signal "ready—go" until the signal "stop." Four or five trials were given after each choice reaction series. Between trials the subject looked down the street.

The main interest of the experiment was in the comparative results under any two systems of lighting. With each subject two equipments were used on the same evening, so that variables such as temperature, weather and physiological condition were fairly constant. The tests were always performed in both time orders—equipment *A* followed by equipment *B* and then after an interval of 10 or 15 minutes, *B* followed by *A*. The average of the two series on the first equipment was compared with that of the two series on the second. A short practice series was given each subject before proceeding to the street for the first series on a given method. The subject always walked for 5 minutes on a given street before being tested.

For all the above experiments only short sections of the two streets were used. This was because one street passed through a square with cross traffic and a number of brightly lighted stores. Two short sections were selected which were fairly similar in conditions along the side. In one case there was a small garage and in the other a delicatessen store on the side of the street used and these were frequently closed during the later hours. Precautions were taken against undue distraction in all the experiments. No trials were made when a vehicle of any sort was passing or when there was any unusual disturbance on the sidewalk. Conversation during a series was avoided. In only two or three cases were the subjects at all aware of the trend of previous results. All possibility of suggestion was avoided. The subjects were employes of the Electrical Testing Laboratories of New York City, and of the New York Edison Company, the majority of them between twenty and thirty years of age. The experiments were performed between 8 P.M. and midnight.

III. *General Results*

Experiments carried on by the above methods indicate that of the illuminating factors influencing the attention and mental reactions, uniformity and non-uniformity of illumination are paramount. The main thesis that seems established by the experiments on the street is that under a distinctly non-uniform illumination (with a mean variation in foot-candles of upwards of 50 per cent.), the attention of an individual walking through the street during a period of 20 to 30 minutes is at a higher level and his reaction time is quicker than when he is walking under a distinctly uniform illumination during a similar period.

Table I gives the results of all the experiments in which a markedly uniform system of lighting was compared with a markedly non-uniform. In the first column are given the two equipments compared on a given evening. Nitrogen gas filled Mazda lamps were always used. To produce uniform illumination, 80 candle power lamps in Holophane units with refractors were mounted 14 ft. above the curb, staggered at 50 ft. spacing, or 250 c.p. lamps in Holophane units with refractors were mounted 24 ft. above the center of the street at 150 ft. spacing. The non-uniform illumination was usually produced by 250 c.p. lamps in diffusing globes (the Edison AB unit) centrally hung 24 ft. above the street at 150 ft. spacing, or by 400 c.p. lamps in Edison units mounted 30 ft. above the street 7½ ft. out from the curb and staggered at 300 ft. intervals. In a few cases bare lamps were used. In the first column of the table the first figure indicates the mounting and the second the spacing followed by the type of unit and the candle power. The next two columns indicate roughly the difference in uniformity of the two equipments under comparison. Measurements of horizontal and vertical illumination were made with portable photometers at thirty stations on a typical block of the street between two lamps for each of the equipments used. The mean variation of these thirty measurements expressed as a per cent. of the mean illumination represents roughly the degree of uniformity of a given system. In the table the per cent. mean variation of the non-uniform street is divided by that of the uniform that was compared with it for both horizontal and vertical illumination. The larger figure in the table thus indicates a greater difference in uniformity between the two equipments compared. The remaining colums give the results of the individual series on each of the three methods. The names of the subjects are not given because, with a few exceptions, the same subject did not participate on more than one evening and consequently stress is to be laid not on individual differences but on general tendencies. The three columns under each group give respectively the per cent. of superiority of the average results of a subject under the non-uniform equipment, the difference between his averages on the two equipments divided by the probable error of difference, and the mean variation (in per cent.) of the results under the uniform lighting divided by that under the non-uniform. The mean variation

Table I

Equipment				Difference in Uniformity		Choice Reaction			Tachistoscope			Motor Coördination		
Mount	Space	Type of Unit	C.P.	Horizontal	Vertical	% Superiority Nonuniform	Difference / P.E.	M.V. Uniform / M.V. Nonuniform	% Superiority Nonuniform	Difference / P.E.	M.V. Uniform / M.V. Nonuniform	% Superiority Nonuniform	Difference / P.E.	M.V. Uniform / M.V. Nonuniform
24	150	Edison	250	2.32	1.25	30.0	5.5	1.07	13.6	2.2	1.52	12.1	4.6	1.49
14	50	Holophane	80	...	...	28.4	7.4	.88	− .7	.2	1.86	5.3	1.6	3.46
..	..	....	..	...	...	9.2	2.8	.86	5.1	.7	1.18	20.0	7.7	2.02
..	..	....	..	...	...	6.4	3.2	.75	28.0	3.4	2.20	5.4	1.7	1.62
..	..	....	..	...	...	9.7	2.6	.76	5.7	1.0	1.51	5.5	1.1	.40
..	..	....	..	...	...	....	...	...	....	...	...	7.1	1.4	1.07
..	..	....	..	...	...	....	...	...	....	...	...	− .7	.1	1.10
..	..	....	..	...	...	....	...	...	....	...	...	− 8.1	1.0	.56
..	..	....	..	...	...	....	...	...	....	...	...	10.1	1.7	1.09
..	..	....	..	...	...	....	...	...	....	...	...	− 5.6	1.1	1.57
30	300	Edison	400	3.06	2.09	46.2	6.4	1.23	13.1	1.9	1.13	2.0	1.3	1.14
14	50	Holophane	80	...	...	11.7	3.5	1.79	9.6	1.8	.74	− .4	.3	1.98
..	..	....	..	...	...	16.8	2.5	.69	3.9	.6	1.06	4.4	1.5	2.08
..	..	....	..	...	...	10.7	3.1	1.25	− 5.8	.8	1.15	....	...	...
..	..	....	..	...	...	....	...	...	14.1	3.9	1.01	....	...	...
..	..	....	..	...	...	....	...	...	− 3.8	.5	.82	....	...	...
..	..	....	..	...	...	....	...	...	5.7	.9	1.08	....	...	...
..	..	....	..	...	...	....	...	...	1.0	.2	2.02	....	...	...
..	..	....	..	...	...	....	...	...	11.0	1.5	.94	....	...	...
..	..	....	..	...	...	....	...	...	16.8	9.9	2.00	....	...	...
..	..	....	..	...	...	....	...	...	10.3	2.9	1.70	....	...	...
..	..	....	..	...	...	....	...	...	2.0	.2	1.90	....	...	...
..	..	....	..	...	...	....	...	...	14.9	1.9	1.20	....	...	...
..	..	....	..	...	...	....	...	...	16.8	2.0	2.04	....	...	...
..	..	....	..	...	...	....	...	...	− 2.7	.5	1.27	....	...	...

Equipment				Difference in Uniformity		Choice Reaction			Tachistoscope			Motor Coördination		
Mount	Space	Type of Unit	C.P.	Horizontal	Vertical	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P,E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform
24	150	Edison	250	1.32	1.25	15.1	2.8	.76	− 8.6	1.5	1.26	5.5	1.6	1.42
24	150	Holophane	250	...	...	8.5	2.3	1.05	7.2	1.1	.78	3.4	2.4	1.03
..	..	....	..	...	...	15.4	3.9	1.03	1.8	.3	1.11	5.0	3.1	1.05
..	..	....	..	...	...	13.2	3.5	1.01	13.3	2.3	1.54	1.0	.9	1.00
..	..	....	..	...	...	15.1	5.3	1.25	9.2	1.7	.90	6.3	1.1	.68
..	..	....	..	...	...	14.1	3.4	1.27	20.2	3.2	1.22	1.3	.6	2.16
..	..	....	..	...	...	....	...	...	17.5	2.1	1.73	....	...	...
..	..	....	..	...	...	....	...	...	6.8	1.1	1.34	....	...	...
30	300	Edison	400	1.74	2.10	....	...	...	....	...	...	4.0	1.9	.89
24	150	Holophane	250	...	...	....	...	...	....	...	...	1.2	.4	2.63
..	..	....	..	...	...	....	...	...	....	...	...	− 5.3	2.2	2.08
24	150	Edison	400	1.96	1.34	32.3	6.5	.71	5.1	7.7	.73	....	...	...
14	50	Holophane	80	...	...	....	...	...	....	...	...	....	...	...
24	150	Edison	250	1.23	.90	10.8	2.3	.64	....	...	...	....	...	...
14	50	Bare	80	...	...	....	...	...	....	...	...	....	...	...
24	150	Bare	250	1.18	.96	18.2	3.6	.66	....	...	...	....	...	...
14	50	Bare	80	...	...	....	...	...	....	...	...	....	...	...
24	300	Holophane	250	2.16	2.08	17.0	3.9	.98	12.5	2.3	1.34	....	...	...
14	50	Holophane	80	...	...	...	...	...	7.0	.8	1.01	....	...	...
Average	..	....	..	...	...	17.0	3.9	.98	8.1	2.0	1.32	3.6	1.8	1.48

of almost any series of tasks performed under properly controlled conditions is regarded by many as an index of attenttion, the higher mean variation indicating the lower degree of attention.[1] Thus a larger figure in the table might indicate a higher degree of attention under the non-uniform system.

The table shows that in every case the auditory choice reaction time is shorter under the non-uniform equipment and on the average is 17 per cent. shorter. The differences are all greater than twice the probable error of difference and average nearly four times as great. The differences between the per cent. mean variation under uniform and non-uniform lighting is small. Attention, as indicated by a tachistoscopic test, is superior under non-uniform illumination in 26 out of the 31 series. The average superiority, including the negative values, is 8 per cent. The differences are on the average twice the probable error of difference. The mean variation is considerably greater, about 30 per cent. on the average, under the uniform condition. In motor coördination three fourths of the series show greater efficiency under non-uniform lighting, with an average superiority, including negative values, of 3.6 per cent. The differences are on the average nearly twice the probable error of difference. In most cases the mean variation is greater under the uniform conditions—48 per cent. on the average.

The above results are more briefly indicated in Table II. The successive columns give the number of series performed under each of the three methods, the per cent. of that number in which the non-uniform series is superior, the average superiority of the non-uniform, the per cent. of series in which the greater mean variation (*i. e.*, lower degree of attention) is shown under the uniform lighting and the average of the quotients obtained by dividing the mean variation of the uniform by that of the non-uniform. The general tendency in favor of a quicker reaction and coördination and a higher degree of attention under the non-uniform equipments is evident.[2]

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[1] Compare Pillsbury, "Attention," p. 89ff.

[2] Simple auditory reactions were tried with two subjects and yielded results similar to those with choice reaction. These two types were correlated later in the laboratory work.

TABLE II

	No. of Series	% of Series in Which Non-uniform Superior	Average Superiority Non-uniform, %	% of Series in Which Uniform Had Greater M.V.	Av. $\frac{\text{M.V. Uniform}}{\text{M.V. Non-uniform}}$
Choice reaction	18	100	17.0	50	.98
Tachistoscope	31	84	8.1	81	1.32
Motor coördination	22	77	3.6	82	1.48

IV. *Checks*

A number of other variables that might have been instrumental in producing the above results were ruled out by suitable check experiments.

As a check on the possible influence of the nature of the street in distracting elements tests were made on a few subjects with identical equipments on the two streets. The average differences were 6 per cent. and 2 per cent. on reactions and on the tachistoscope respectively, insignificant in comparison with the averages of Table I. Moreover the uniform equipment was sometimes on the north street and sometimes on the south street with little difference in results. Further, the same street was occasionally employed for all four series on a given evening. Here the sidewalk, traffic and window lighting conditions were constant, the only variable being the illuminating conditions. The average superiority of the non-uniform equipments under such conditions differs only slightly from the averages of Table I.

As to the type of illuminant used and the possible effect of Edision *vs.* Holophane units, some reaction series were made, using bare lamps in one or both streets. A street equipped with Edison units, compared with one identical but minus the globes yields slight differences in reaction. But if the Edison equipment is compared with bare lamps with refractors giving uniform illumination, or if two systems of bare lamps are used, one with refractors, the usual differences occur.

As to the intensity of illumination, the photometric data shows that in approximately half of the comparisons the mean intensity of the non-uniform system was greater than that of the uniform and in the other half the reverse was true. The experimental results favored the non-uniform lighting under both conditions.

The time error was checked as above indicated by working on both equipments on a given evening in both time orders. Further, the uniform equipment was tested on the first series in about half of the comparisons.

As to the possible influence of number of lamps, cross street location, height or spacing, 250 c.p. lamps in Edison units were centrally hung 24 ft. high at 150 ft. spacing and compared with similar lamps in Holophane units with refractors having exactly the same mounting. The results are included in Table I. in the third horizontal group—6 series in choice reaction and motor coördination and 8 on the tachistoscope. In a few of these series moreover the two equipments were on the same street. In this rather crucial check it is evident that the usual tendency is manifest to about the usual degree.

Finally the question might be raised as to whether the superior results under non-uniform illumination are not due to a sort of voluntary alertness produced by the difficulty in seeing objects in the dark areas, that is, whether the results are not due to the dimness of the lighting rather than the non-uniformity. As a partial answer one subject was tested in reaction times on a uniformly lighted street run at the usual 7.5 amperes and then at 6.1 amperes. The reactions were 16 per cent. quicker under the former condition. This would indicate that the dimness factor was not responsible for the results of Table I.

V. *Degrees of Non-uniformity*

The above results point rather strongly to the superiority of non-uniform illumination of the types studied in its effect upon attention and reactions of the pedestrian. Whether the degree of attention varies with the degree of non-uniformity is doubtful. In the present study there were two non-uniform equipments that were used for the most part, with

150 ft. and 300 ft. spacing, the latter of which was more non-uniform in both horizontal and vertical illumination. Both of these equipments were frequently compared with a uniform system which might be regarded as the standard. The 300 ft. spacing shows a greater average superiority over the uniform system in choice reaction than does the 150 ft. spacing, but in motor coördination and attention this condition is reversed. A few tests were made comparing directly the two non-uniform equipments on the same evening with the same subjects. The 300 ft. system was slightly superior by all three methods.

This problem of degrees of uniformity could be answered only by exhaustive investigation. Even if the degree of attention were found to be a positive function of the degree of non-uniformity, it is probable that beyond a certain point the dimly lighted stretches of street would be so broad that the effect would amount to uniformity and produce retardation.[1]

C. EXPERIMENTS IN THE LABORATORY.

In transferring the problem to the laboratory there were five questions in mind:

1. How far do results obtained with the factor of uniformity and non-uniformity of illumination controlled in the laboratory substantiate in general the results obtained on the street?

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[1] The results of the experiments carried on at the same time by other investigators to determine the revealing qualities of the different equipments for surface irregularities and obstacles in the street, tend somewhat in the same direction as the above results, rather than in the opposite direction. Targets of wood 2 inches thick or of galvanized iron plate the size of the surface of a paving block, painted gray and sanded were planted systematically through the two streets. Subjects walked along the traffic line at their normal rate, recording on a pad all the targets they saw and their location. It was found in general that these targets were perceived somewhat more readily on the street with the large illuminants widely spaced than on that uniformly lighted by small illuminants closer together. Furthermore with the equipments studied the targets were found almost as successfully in the darker areas between lamps as in the lighter areas. In other tests sections of stove-pipe one foot high, painted gray and sanded, were planted at various positions and the subjects in an automobile rode through the streets at constant rate with a device which recorded the maximum distance at which each target was seen. Results thus obtained tend in general in the same direction as those with the surface targets. The above results are not as yet published and no conclusions should be drawn from them, but their general tendency is of interest in the present connection.

2. How far do other kinds of reaction correlate with auditory reactions under the various illuminating conditions?

3. What are the effects of different degrees of non-uniformity?

4. Can the results in favor of non-uniform illumination on the street be attributed to greater voluntary effort in watching for obstacles in the dark regions between lamps, *i. e.*, does voluntary attention to one stimulus quicken reaction to another?

5. Can the results be attributed predominantly to the changing illumination through which the subject walks or to the light and dark patches on the street surface?

I. *Equipment*

The experiments were performed in an interior room with black walls, indirectly illuminated by light reflected from the white ceiling. The source was about 1.5 meters from the center of one end of the room directly above the head of the subject, and consisted of a 100-watt tungsten lamp in a box 60 cm. square and 45 cm. deep suspended 60 cm. from the ceiling. The subject sat alone, the experimenter and all the apparatus being in an adjoining room in front of the subject.

With the pedestrian walking on a non-uniformly lighted street there are two possible factors which may influence his mental state—the varying illumination intensity through which he is passing and the alternating light and dark regions on the street surface before him. To reproduce the first of these a 110-point circular rheostat was placed in series with the lamp illuminating the room. An alternating-current motor, geared down by belts and speed reducers, was arranged to drive a crank. One end of the crank was connected by a shaft with the margin of a wheel of greater radius, so that the latter oscillated through 160 degrees. This was belt-geared to a disc attached to the axle of the rheostat. Thus, when the motor was in operation the illumination in the room where the subject sat was slowly dimmed to a definite point (not, however, to the point where the color of the light changed perceptibly), then increased and this process continued. These conditions roughly parallelled those to which the individual on the street is subjected as he walks along, the brightest point in the cycle in the laboratory corresponding to the position under a street lamp, the darkest part of the cycle to the position midway between lamps and the intervening points in the cycle to the intermediate positions on the street. The speed reducers were combined to produce complete cycles of 25, 55 or 85 seconds.

To reproduce the second factor, the shadows on the street surface, a black box with light interior 115×35×20 cm. with one of the 115×20 edges open was mounted vertically on a table at the subject's right with the open edge facing the left at an angle of 60 degrees to the wall. In strips attached to the edges of this side a piece of cardboard 115×20 cm. with 17×5 cm. holes in it 5 cm. apart could be moved up and down. In the center of the opposite side of the box was a 25–watt tungsten lamp. Thus a slightly fan-shaped array of shadows was cast on the wall directly in front of the subject. The cardboard was weighted at the bottom and attached at the top to a cord which ran through pulleys over the partition into the adjoining room. The cord could be attached to the crank which actuated the rheostat and the shadows thus be made to move up and down at various rates through a distance of 40 cm. This roughly reproduced the slowly changing effect of the light and dark patches on the street surface as the subject walks along under non-uniform lighting.

To parallel the uniform conditions on the street, the room was left at its normal condition with the lamp at maximum intensity. The average brightness on the black wall in front of the subject was then .119 meter candles.[1] When the light was dimmed to the lowest point of the cycle the average brightness was .037 m.c. With the shadows the average figure (averaging maxima and minima of the shadows and of the light spaces between them) was 1.38 m.c. When the shadow equipment was compared with a uniform, the card

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[1] Photometric measurements were made over an area of approximately one square meter with a Lummer-Brodhun photometer, using an amyl-acetate lamp as a standard.

in the latter case was drawn up above the box and the light in the box shunted through the rheostat and held constant at a point which gave approximately the same average brightness on the wall. A point was found on the rheostat which gave a brightness of 1.42 m.c. as compared with the 1.38 m.c. for the shadow equipment run at the full current.

II. *Method.*

Experiments were made under these conditions upon various sorts of reaction times and upon attention as involved in a tachistoscopic test similar to that employed in the street experiments. For the latter purpose a portion of the wall between the subject and the experimenter was replaced by a piece of black cardboard 66 × 100 cm. in the center of which was a piece of ground glass 10 cm. square. Behind this was a rack into which paper or cardboard material could be inserted. Arrangements of geometrical figures in combinations of five, quite similar to those described above (p. 158) were employed. The figures were 4 sq. cm. in area, drawn with a soft pencil on white linen paper. When placed in the rack pressing against the ground glass and illuminated by a lamp 20 cm. distant they were plainly visible to the subject on the other side of the glass. The exposures of one second were made by covering the end of one of the speed reducers with a wooden disc with a sector of proper magnitude removed and arranging a contact of spring brass to press on the disc. The spring contact and the speed reducer were connected to two poles of the lamp circuit which was thus closed during a part of the revolution of the reducer. A telegraph key in the circuit enabled the experimenter to make the exposures whenever he wished. After the exposure the subject reproduced the figures on a small square of paper on the table at his side and placed his drawing in a drawer which the experimenter pushed through the wall for the purpose and which was then withdrawn. In scoring, a figure in the correct position counted 2 and a figure displaced less than a quadrant or disoriented counted 1. A different permutation of objects was shown each trial during the first series on two lighting equipments in one time order, and then the same material repeated on the two equipments in the opposite time order. That is, each pattern was used once on each of the two equipments under comparison, thus eliminating any error due to degrees of difficulty in the material.

Auditory choice reactions with the feet were sometimes intermixed with the short-exposure tests and sometimes used alone. Keys were made of two pieces of wood 20×10×1 cm. hinged at one of the longer edges, with brass strips for contacts at the outer edges and held apart by light springs. Each foot rested on one of these keys, the normal weight of the foot being sufficient to close the circuit. The stimuli consisted of a small electric gong and a sounder made by removing the bell from a larger gong so that the armature struck the magnets. The time was measured with two electric vernier chronoscopes. By the use of a double-throw switch, the two chronoscopes could be operated through the same connections and thus could be used in quick succession and counted simultaneously. The counting was facilitated by giving the second stimulus on the 10th, 15th, etc., swing of the first. The chronoscopes were always callibrated at the beginning of the hour. For simple reactions a telegraph key on the table beside the subject was connected in parallel with the foot contacts.

For the stimulus for a simple visual reaction a battery lamp on a frame, so hinged that it could be swung near the ground glass before the eyes of the subject, was placed in parallel with one of the bells by means of a double-throw switch, and a piece of cardboard with a 6 mm. round hole placed between it and the glass. The subject thus received a small patch of light as a stimulus.

In conducting the experiment, two equipments were always employed on a given hour in both time orders, *i. e.*, uniform, non-uniform, non-uniform, uniform, or the reverse. The subject sat passively under the given equipment for approximately 3 minutes prior to each series. On a given comparison half the subjects began with the uniform and half with the non-uniform equipment, and when the same subject worked two different hours on a given comparison, the second hour began with the equipment the opposite of that of the first hour.

The instructions for the short-exposure test (after a preliminary explanation of the material and observation of a sample exposure) were, "Reproduce as many of the figures as possible in the correct position." Those for the choice reaction were "When you hear the bell lift the right foot and when you hear the sounder lift the left." When the reactions were intermixed with the short-exposures the instructions were a combination of these two: "When you hear the bell lift the right foot, when you hear the sounder lift the left, and when you see the pattern reproduce as many of the figures as possible in the correct position." For the simple reactions the subject was instructed: "Motor reaction to the bell" or, "Motor reaction to the light." The instructions were repeated before each of the four series during the hour. At the beginning of the hour three patterns and ten reactions of the sort to be used were given for warming up, and the first two reactions of the four series were omitted from the results.

The number of trials varied according to the combination of tests used on a given hour. The tachistoscope tests were always given in series of 40, *i. e.*, 20 on each equipment. When choice reactions were used in connection with the tachistoscope there were 80 trials, 40 with the short exposures intermixed and 40 without. With simple reactions it was possible to give 240 per hour, usually 120 auditory and 120 visual.

The reactions and exposures were given at irregular intervals. The order for the choice reactions was determined by shuffling cards containing the same number of right and left at the beginning of each series. When the reactions were intermixed with the tachistoscope, cards marked "T" were intermixed with the others before shuffling.

The subjects were graduate students in the Harvard Laboratory and a few undergraduates doing advanced work in psychology. None of them knew the trend of the results of the experiments on the street. Ten subjects participated at different times.

III. *General Results*

Various combinations of uniform and non-uniform illuminating conditions were studied comparatively and the results tend in general to substantiate those obtained on the street. This can best be shown by averaging together all those series in which a distinctly non-uniform condition was compared with a uniform. Table III summarizes the larger part of such results. The Roman numerals at the left indicate the equipments compared and are as follows:

I. Non-uniform......26 sec. dimmer.
Uniform..........Dark wall.
II. Non-uniform......26 sec. dimmer—shadows stationary.
Uniform..........Dark wall.
III. Non-uniform......26 sec. dimmer—26 sec. shadows.
Uniform.........Dark wall.
IV. Non-uniform......26 sec. dimmer—26 sec. shadows.
Uniform..........Light wall.
V. Non-uniform......55 sec. dimmer.
Uniform.........Dark wall.
VI. Non-uniform......55 sec. dimmer—shadows stationary.
Uniform..........Light wall.
VII. Non-uniform......55 sec. dimmer—55 sec. shadows move.
Uniform..........Dark wall.
VIII. Non-uniform......85 sec. shadows.
Uniform..........Light wall.
IX. Non-uniform......85 sec. dimmer—85 sec. shadows.
Uniform..........Light wall.
X. Non-uniform......Shadows stationary.
Uniform..........Light wall.
XI. Non-uniform......25 mm. shadows stationary.
Uniform..........Light wall.

The first column under each heading gives the average per cent. of superiority of the non-uniform equipment (*i. e.*, shorter reaction time or higher score on the tachistoscope, per cents. reckoned in terms of the smaller of the two averages), for the given comparison. Below this figure is in each instance the total number of subjects who yield differences in favor of the non-uniform equipment followed by the total number of subjects who participated on that comparison. The second column gives the average obtained by dividing the differences by the probable error for the various subjects,

TABLE III

	Tachistoscope			Auditory Choice With			Auditory Choice Without			Simple Auditory			Simple Visual		
	% Superiority Non-uniform	$\frac{\text{Difference}}{\text{P.E.}}$	$\frac{\text{M.V. Uniform}}{\text{M.V. Non-uniform}}$	% Superiority Non-uniform	$\frac{\text{Difference}}{\text{P.E.}}$	$\frac{\text{M.V. Uniform}}{\text{M.V. Non-uniform}}$	% Superiority Non-uniform	$\frac{\text{Difference}}{\text{P.E.}}$	$\frac{\text{M.V. Uniform}}{\text{M.V. Non-uniform}}$	% Superiority Non-uniform	$\frac{\text{Difference}}{\text{P.E.}}$	$\frac{\text{M.V. Uniform}}{\text{M.V. Non-uniform}}$	% Superiority Non-uniform	$\frac{\text{Difference}}{\text{P.E.}}$	$\frac{\text{M.V. Uniform}}{\text{M.V. Non-uniform}}$
I........	2.16 8/11	1.27 ...	.99 ...	.94 6/11	1.63 ...	1.00 ...	1.35 8/11	1.60 ...	.96 ...	.90 3/5	1.46 ...	1.04 ...	7.07 4/5	3.16 ...	1.07 ...
II.......	−2.50 2/6	2.59 ...	1.04 ...	9.05 5/6	3.03 ...	1.14 ...	6.86 5/6	2.51 ...	1.06 ...	... ...	... ...	... ...	....	... ...	... ...
III......	−2.30 4/8	1.22 ...	1.14 ...	2.21 5/8	1.55 ...	1.10 ...	2.28 3/8	2.53 ...	.95 ...	... ...	... ...	... ...	....	... ...	... ...
IV......	....	... ...	... ...	... ...	... ...	... ...	... ...	... ...	... ...	5.75 2/2	2.15 ...	1.00 ...	2.70 1/2	3.20 ...	1.30 ...
V.......	5.79 7/11	1.62 ...	1.08 ...	.76 8/11	1.55 ...	.95 ...	6.36 6/8	1.87 ...	1.12 ...	... ...	... ...	... ...	....	... ...	... ...
VI.......	....	... ...	... ...	... ...	... ...	... ...	... ...	... ...	... ...	5.36 5/6	2.22 ...	.95 ...	6.46 5/6	3.88 ...	1.20 ...
VII.....	....	... ...	... ...	... ...	... ...	... ...	2.12 4/5	1.45 ...	1.11 ...	5.01 5/5	1.27 ...	1.15 ...	....	... ...	... ...
VIII....	....	... ...	... ...	... ...	... ...	... ...	2.86 4/6	1.64 ...	1.02 ...	8.65 4/6	3.26 ...	1.04 ...	....	... ...	... ...
IX......	....	... ...	... ...	... ...	... ...	...	3.35 3/4	1.56 ...	.92 ...	5.55 7/8	2.49 ...	1.11 ...	−6.10 1/4	2.68 ...	.98 ...
X.......	4.64 4/9	1.58 ...	.95 ...	2.37 2/4	.91 ...	1.68 ...	.83 7/14	1.42 ...	1.10 ...	7.99 9/10	2.80 ...	1.15 ...	....	... ...	... ...
XI......	.52 2/4	1.02 ...	.93 ...	... ...	... ...	... ...	... ...	... ...	... ...	3.35 ...	1.53 3/4	.95 ...	....	... ...	... ...
Average .	1.91 27/49 55%	1.52	1.01	2.50 26/40 65%	1.75	1.09	2.88 40/62 65%	1.72	1.04	5.76 38/46 82%	1.73	1.07	3.24 11/17 65%	3.30	1.12 ... ...

and the third gives the average of the figures obtained by dividing the per cent. mean variation of the uniform series by that of the non-uniform for each subject. To illustrate: the figures at the beginning of the table indicate that for the tachistoscopic tests on the comparison denoted as I., of the 11 subjects, 8 yield differences in favor of the non-uniform equipment with an average superiority of the non-uniform (including negative values) of 2.16 per cent. The average of the 11 differences divided by their probable error is 1.27. If the per cent. mean variation of the results on the uniform equipment is divided by that for the non-uniform the average of these figures is .99.

The summaries at the bottom of the columns give in the first row the weighted averages of all the first rows above, obtained by multiplying each average by the number of series involved and dividing the sum of these by the total number of series. In the second row of the summary are the sums of the corresponding figures above, *i. e.*, the number of series yielding differences in favor of the non-uniform, the total number of series and the per cent. which the former is of the latter.

From the averages it is evident that there is a general tendency toward greater efficiency under non-uniform illumination. Simple auditory reaction time is superior under such lighting in 82 per cent. of the series an average of 5.8 per cent. Simple visual reaction time is quicker in 65 per cent. of the series, an average of 3.2 per cent. Auditory choice reactions with tachistoscopic trials intermixed yield similar results in 65 per cent. of the series with an average superiority of 2.5 per cent. Auditory choice reactions without the tachistoscopic trials tend in the same direction in 65 per cent. of the series, an average of 2.8 per cent. And the tachistoscopic experiments tend likewise in favor of non-uniform illumination in 55 per cent. of the cases with an average superiority of the non-uniform of 1.9 per cent. The differences in general are between 1.5 and 2 times the probable error of difference and in the case of the visual reactions over 3 times. The mean variations do not show as marked a tendency, although they average in each method slightly in favor of the non-uniform condition. It might be noted that the individual averages in the simple reactions are based on a larger number of trials and the differences prove greater.

At the end of each hour the subjects were asked whether they had any introspection as to their comparative mental state under the two systems of illumination. In a large majority of the series (83 per cent.) the subject had nothing of interest to report. The usual statement was: "Felt no difference under the two systems"; or "Think I did as well under one system as the other." It is evident that an individual may be influenced by the illuminating conditions without being aware of it. However in the cases in which introspection was given there is no unanimous tendency. Only two of the subjects are at all consistent in their reports, the others feeling more efficient sometimes under uniform and sometimes under non-uniform lighting. There is no general tendency in the introspective as in the objective results. However the two correlate in about two thirds of the individual cases in which introspection is given—a report that the subject is less efficient under a given system corresponding to a slower reaction time. There are no marked individual differences, *i. e.*, subjects who are consistently superior under uniform or non-uniform conditions. On the whole, considering the few cases in which introspection was forthcoming and its variable character, the principal stress must be placed on the objective results. It is evident that a subjective estimate of the mental state under various conditions of illumination is sometimes unreliable, and furthermore such illuminating conditions frequently influence the reactions without producing any conscious effect at all.

Thus the results obtained in the laboratory tend to corroborate those found on the street although not as marked. Reactions are quicker and a higher degree of attention is maintained under non-uniform illumination. It would seem that the results could be attributed solely to the non-uniformity factor either in the form of the increasing and decreasing intensity or of the shadows of the visual field. External factors such as traffic and extraneous light from store windows were not present in the laboratory. Nor could anything be attributed to the type of lamp or the number and mounting of lamps. Further, when a shadow equipment was used, the brightness of the wall in the corresponding uniform equipment was in some series equated to the average brightness of the former. The results are evidently due to the uniformity factor.

IV. *Correlation of Other Reactions with Auditory Choice*

In the experiments on the street auditory choice reactions were used almost entirely. From Table III, it is evident that in the laboratory simple auditory reactions as well as auditory choice yield greater efficiency under the non-uniform illumination. Data is available for 20 series in which the two were measured on the same hour with the same subjects. If the individual per cents. of superiority of the non-uniform equipment are ranked for the simple and the choice reactions, the two correlate by the method of rank differences to give a coefficient of .42 ± .13. It is to be noted further that simple visual reactions also tend on the average in the same direction as the others. Scarcely enough series are available in which auditory and visual were measured on the same hour to compute correlation on the basis of individual averages, but the general tendency is manifest.

V. *Degrees of Uniformity*

Given the general superiority of non-uniform illumination, the question arises as to various degrees of non-uniformity. In the laboratory as on the street no conclusive answer can be given. In Table III. are a few cases in which results are available under two comparisons that differ merely in their temporal aspect. Combinations I. and V. compare the 26 and 55 sec. dimmer respectively with the constant conditions. The 55 sec. appears considerably superior in tachistoscope experiments and in auditory choice reactions alone, and slightly inferior in auditory reactions intermixed with the tachistoscope. Combinations III. and VII. compare 26 and 55 sec. cycles of the dimmer and shadows moving together, with the constant conditions. The average difference in auditory choice reaction is slight, but a larger per cent. of the series yield superiority of the non-uniform system in the case of the 55 sec. cycle. The 85 sec. cycle (IX.) of dimmer and shadows, compared however with the light wall equipment, is superior to either the 26 or the 55 sec. cycles. One might also note the comparative results under combinations II. and III. in both forms of choice reaction. With the 25 sec. dimmer and stationary shadows the score is high, but when the shadows move in 26 sec. cycle as well as the dimmer, the score is low. Evidently the rapidly moving shadows offer a distraction.

Some series were performed to test this problem directly. The subjects were given simple auditory and visual reactions under the dimmer moving at three rates on a given hour with the time-error properly controlled and at another time under the shadows moving at three rates. The results indicate that the slower rates are more advantageous with the shadows and the more rapid rates with the dimmer for auditory reactions, while visual reactions are facilitated by the more rapid rates with both.

The slower rates with the dimmer and shadows correspond roughly to the more non-uniform conditions on the street, *i. e.*, to a system in which the subject walking along is less frequently under a lamp and in which the light and dark patches on the street are broader and hence produce a less rapid change in the visual field. There are slight indications in the results on the street of greater efficiency in auditory reactions the more non-uniform the lighting. In the laboratory from the standpoint of the changes in the visual field, auditory reaction is likewise quicker the more non-uniform the system, but from the standpoint of change in illumination intensity this is not the case.

On the whole no definite conclusions can be drawn upon the present problem. It seems best to conclude on the basis of both street and laboratory experiments that within reasonable limits no marked differences in efficiency are found under various degrees of non-uniformity of illumination, but that the important point is the general superiority of the various non-uniform equipments to the uniform.

VI. *Voluntary Alertness*

The question was raised in the street experiments as to whether the superiority of reaction under non-uniform illumination was not due to a voluntary alertness produced by the necessity of watching more carefully for obstacles in the darker regions between lamps. It was endeavored in the laboratory to answer this question as to whether voluntary attention to one stimulus under the various illuminating conditions would quicken reaction to another. For this purpose auditory choice reactions were given when the subject's attention was somewhat focused on the patch where the tachistoscopic material was to appear, followed by auditory choice reactions with no exposures intermixed. Series were performed on five different illumination comparisons, numbers I., II., III., V. and X. (see p. 173). 80 trials were given on a comparison. The results for the uniform and the non-uniform equipments were then evaluated separately and the efficiency of auditory choice reactions presented alone compared with that of such reactions with tachistoscopic trials intermixed. The former were found to be superior with the majority of the subjects under all the lighting equipments thus investigated. Considering the totals under the five uniform equipments, 76 per cent. of the series yielded a superiority of the reactions presented without the tachistoscope, with an average superiority (including negative cases) of 5.2 per cent. and the average difference 2.44 the probable error of difference. Considering the totals under the five non-uniform equipments, 76 per cent. of the series likewise yielded a superiority of the reactions presented without the tachistoscope with an average superiority of 7.9 per cent. and an average difference 2.6 the probable error. Evidently voluntary alertness to one stimulus under the various illuminating conditions does not quicken reaction to another stimulus.

VII. *Shadows vs. Changing Illumination Intensity*

Finally there remains the question as to whether the effect of non-uniformity on the street is due primarily to the changing illumination through which the subject passes as he walks along the street or to the light and dark patches on the street surface. One series was performed to test this directly, auditory choice and simple reactions being given under the 55 sec. dimmer and under the stationary shadows. Choice reactions proved quicker under the dimmer with 3 of the 5 subjects, with an average superiority of 9 per cent. and simple reaction was quicker for 3 subjects under the shadows with the average difference small. Nothing in the introspection throws light on the matter.

In Table III. if we consider the simple auditory reactions we find the highest comparative efficiency for the non-uniform lighting on comparisons VIII. and X. in which the shadows are alone at the 85 sec. cycle and stationary respectively, but the results with a combination of the shadows with the dimmer are not greatly inferior to these. On choice reactions without the tachistoscope the highest scores are for II. and V. which involve the 26 sec. dimmer with stationary shadows and the 55 sec. dimmer respectively. In these cases, however, the uniform system used involves the dark wall with a possibility of the influence of the brightness variable. Of the others the highest score is for the 85 sec. dimmer with the shadows moving (IX.), which is superior to that for the shadows alone either stationary (X.) or in an 85 sec. cycle (VIII.). Choice reactions intermixed with the short exposures show the highest score for II. a combination of the 26 sec. dimmer and stationary shadows and the next highest for X., the stationary shadows. The tachistoscope shows a high score for both V. and X., the 55 sec. dimmer and the stationary shadows. On the whole, non-uniform illumination produced by the dimmer seems slightly superior to that produced by the shadows in its effect upon choice reaction, while the result is reversed with simple reaction, and a combination of dimmer and shadows seems in general to produce better results then either alone. Presumably the effect of non-uniform street illumination upon attention and reactions is due to both factors, the changing illumination intensity through which the individual passes and the dark regions on the street surface.

D. CONCLUSIONS

The results of experiments upon subjects walking under actual conditions of street illumination indicate rather consistently that the auditory choice reaction time is quicker, the attention more alert and the efficiency in motor coördination greater under a distinctly non-uniform illumination than under a distinctly uniform. There is no other variable that follows as directly with the results as does the uniformity variable. Sometimes the non-uniform street has more distracting elements in the form of people on the sidewalk or extraneous light from stores, sometimes less; sometimes its average illumination intensity is greater, sometimes less; sometimes it has a different type of illuminating unit, sometimes the same; sometimes it constitutes the first of the series, sometimes the second; sometimes it has fewer lamps with different mounting and spacing, sometimes the same number of lamps with the same mounting and spacing. But under all these conditions it shows a superiority over the uniformly lighted street. When the uniformity factor is a variable in the comparative study of two equipments, there is a marked difference in results, and when it is a constant the difference is slight. Under a distinctly non-uniform street illumination as compared with a uniform, the subjects show in 77 per cent. of the series a superiority in motor coördination with an average (including negative values) of 3.6 per cent. superiority; in the tachistoscopic test they are superior in 84 per cent. of the series with an average (including negative values) of 8 per cent.; and in auditory choice reaction every series yields a quicker reaction time with an average of 17 per cent. superiority.

With the problem transferred to the laboratory and non-uniformity produced by slow rhythmic increasing and decreasing of the illumination intensity of the room, or by shadows to break the monotony of the dark wall—conditions designed to parallel respectively the changing illumination intensity through which the individual walks on a non-uniformly lighted street, and the shadows on the street surface—the results tend in general to corroborate those just mentioned. Further, simple visual and auditory reactions correlate with choice reactions in this tendency. The superiority of reactions under non-uniform lighting on the street cannot be attributed to a voluntary alertness produced by the necessity of watching more carefully for obstacles in the darker regions between lamps, for the experiments show that voluntary attention to an expected stimulus under such illuminating conditions does not quicken reaction to another stimulus. Within reasonable limits no definite relation exists between degree of efficiency in reaction and degree of non-uniformity in illumination, the important point being the general superiority of the various degrees of non-uniformity to the uniformity. The superior effect of the non-uniform street lighting seems due to both the changing illumination intensity through which the individual walks and the light and dark regions on the street surface.

Finally the practical implications of the experiment should be borne in mind. Safety on the street depends largely on the ability to apprehend a dangerous situation and carry out quickly the proper psychomotor response. If external conditions produce a lower degree of attention and a retardation of reaction, the danger of such conditions is manifest. The above experiments show that uniform street illumination as compared with a moderately non-uniform, produces such a state in the case of the pedestrian walking through the street. The work in the laboratory with various degrees of non-uniformity would seem to indicate a similar condition in the case of more rapid movement through the street such as that of the driver or chauffeur. Driver and pedestrian alike must make quick decisions and reactions and a difference in reaction time of a few hundredths of a second may avert or precipitate an accident. The conclusion thus seems warranted that, ceteris paribus, non-uniform illumination is more conducive than uniform to safety on the street.

DISCUSSION

TONAL VOLUME AND PITCH

In J. G. Rich's paper: *A Preliminary Study of Tonal Volume* (this JOURNAL, February), he makes the assumption that "If they (pitch and volume), are two names of one and the same attribute, as Dunlap asserts, they will have the same limen." I have already pointed out (in my *System of Psychology*, pp. 143–4), that we may reasonably expect that the opposite principle will hold. It is very possible that the pitch-discrimination of the unmusical person, and sometimes of the musical person where the differences are large, is a pure extensity judgment, *i. e.*, based on the extent of the stimulation in the hair-cell series; but that the discrimination of the musical person, especially where the differences are small, is primarily a local-sign judgment, *i. e.*, based on the differences in the hair-cells at which the region of stimulation ends. Mr. Rich's results may therefore be more reasonably interpreted as supporting the extensity theory than as against it, although I can not as yet see that these results have any certain bearing on the theory. There are so many possibilities as to the contential nature of the judgment-reactions in these experiments that it seems hardly worth while speculating about them.

I had hoped that we would be able to exclude the extensity theory, as the frequency theory ('telephone theory') is so much simpler; but on the contrary the extensity theory becomes more and more plausible. The most vital objection to it has just been removed by the epoch-making work of Hardesty (*American Journal of Anatomy*, 1915, Vol. 18, 471–514), who has shown that a structure such as the tectorial membrane may be affected by vibrations of different periods in just the way indicated by the psychological facts; the slower rates affecting larger, the faster, smaller extents of the tectorial membrane, these extents coterminous at the 'vestibular' end.

KNIGHT DUNLAP

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THE HIPP CHRONOSCOPE: ITS USE AND ADJUSTMENT

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In an article entitled 'The Hipp Chronoscope without Armature Springs,'[1] Knight Dunlap describes an ingenious means of getting accurate time records with the Hipp chronoscope. The present paper is the report of a rigid test of the method, carried on during a period of six months. The writers feel that a detailed account of the tests and of the arrangement of the apparatus for them will be appreciated by owners of the Hipp chronoscope. Special attention will be given to the actual chronoscope adjustments required.

The method as described by Dunlap is essentially as follows: Current is taken from the ordinary commercial circuit (direct current), and passed through a bank of incandescent lamps (three 16-c.p. 60-watt, arranged in parallel). The two terminals are connected by a shunt circuit containing a rheostat. Two circuits are then taken from this source, one to each pair of the electromagnets of the chronoscope. The springs attached to the chronoscope armature are removed and the clutch shaft is so balanced that the shaft will stay in clutch as well as out of clutch. This is done by increasing the size of the counterweight previously used for balancing.[2] The upper magnet circuit includes the stimulus

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[1] *Brit. J. of Psychol.*, 1912, 5, 1–7.

[2] The chronoscope might well be modified so as to make this change in the counterweight unnecessary. Dunlap states that such a modification is being worked out by him, but those who own the instrument in its present form will have to resort to a change in the counterweight.

key and the lower one the reaction key. When reaction time is to be measured, both circuits are closed and the armature is held against the upper magnets. Since both magnets are acting, the armature might be drawn to the upper or lower set of magnets depending on which circuit is closed first, that is, the clock might or might not be recording. The proper position of the armature, namely, against the upper magnets, may be obtained by opening the reaction key (controlling the circuit through the lower magnets) for a moment, thus allowing the upper magnets to draw the armature up. When in this position, the lower circuit may again be closed without changing the position of the armature. When the stimulus is given, the upper circuit is broken and the lower magnets pull the armature down, throwing the clutch into the moving gears and putting the chronoscope hands in motion. The circuit through the upper magnets is again closed (after an interval ranging from 20 to 73 sigma). This does not change the position of the armature which is against the lower magnets, since the upper magnets cannot overcome the force of attraction of the lower magnets, the two currents being equal. The subject reacts by opening the circuit through the lower magnets, thus allowing the upper magnets to draw the armature up. This movement throws the shaft out of clutch, and the hands stop. The current through the upper magnets thus takes the place of the armature springs.

Dunlap suggests the use of a simple relay for closing the upper circuit, about 50 sigma after it is opened. Thus arranged, the key, which gives the stimulus and opens the circuit through the upper magnets of the chronoscope, may be made to open (or close) the circuit through the electromagnets of a relay. The armature of the relay when released (or attracted, if the relay circuit is closed), will again make the circuit through the upper magnets. This set of magnets is now ready to draw the chronoscope armature up as soon as it is released from the lower pair of magnets. The arrangement of the two circuits may be represented schematically as in Fig. 1. Dunlap used a Wundt hammer for testing the chronoscope and hence the falling hammer broke and made the different circuits instead of the experimenter and subject.

As Dunlap states, the method does away with one great source of variation, namely, that due to the effect of temperature changes and age upon the springs. It also permits a relatively large variation in the current strength, if the mechanism is properly adjusted. The matter of current strength is the more important factor since it would make unnecessary the use of storage or gravity batteries and eliminate the constant control testing previously made necessary by the slight changes in the current.

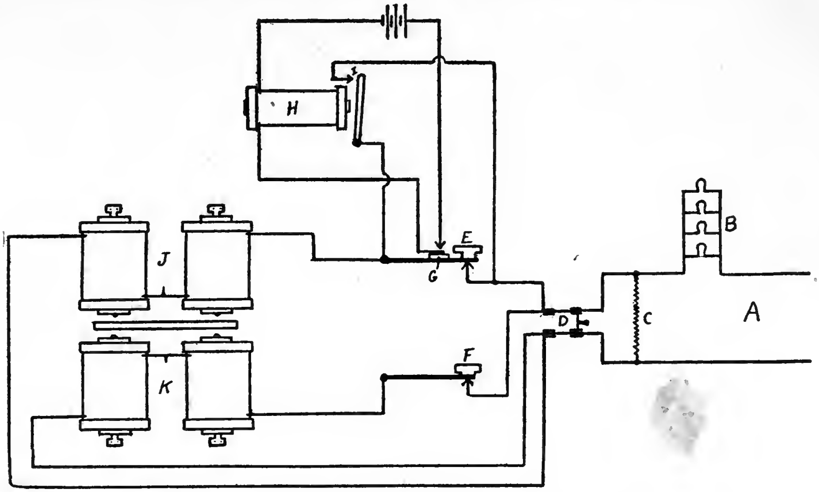

Fig. 1. Schematic Arrangement of the Chronoscope Circuits. *A.* Source of current supply from the ordinary light circuit. *B.* Lamp resistance, which reduces the 116-volt circuit to the desired strength. *C.* Shunt circuit, connecting the two sides of the circuit, and acting as a kind of safety valve for the escape of current beyond the desired strength. *D.* Switch connecting the chronoscope circuits with the main circuit. *E.* Experimenter's key, inserted in the circuit containing the upper magnets of the chronoscope. Stimulus is given by release of this key. *F.* Subject's key, inserted in the circuit containing the lower magnets of the chronoscope. Reaction is made by release of this key. *G.* Insulating block to keep the relay circuit insulated from the chronoscope circuit. *H.* Relay to close the chronoscope circuit through contact *I*, after the stimulus is given. The breaking of the chronoscope circuit makes the circuit through the relay magnet *H*, which in turn by reacting on the armature closes the upper magnet circuit after a brief interval. *J.* Upper magnets of the chronoscope. *K.* Lower magnets of the chronoscope.

The value of Dunlap's method depends upon the following assumptions, the first three of which were clearly stated by him:

1. That the two sets of electro-magnets are nearly alike in windings and cores.
2. That the current through the two sets of magnets is equal.
3. That the chronoscope is properly adjusted.
4. That the current acts upon the two magnets for approximately the same length of time, or that this difference in time has no appreciable effect.

Upon the first and the fourth depends the absence or equality of the latent times of the two magnets, and the equality of the retardation due to the magnetization of the magnet cores. By the common method of using the chronoscope with its springs, the latent time of the one set of magnets was offset by the action of the spring against it. This spring could be adjusted for a given current strength. But any changes in the tension of the spring or in the strength of the current would change the time of chronoscope, until a new adjustment should be made. Changes in the current strength in the one set of magnets according to Dunlap's method are accompanied by corresponding changes in the other. And the force is not exerted against any tension since the armature is balanced and the one magnet never moves it until the other magnet has released it.

Upon the first three conditions and especially upon the third (as will later appear) depends the equality in the actual force exerted by the two sets of magnets.

These four conditions will be discussed in detail.

1. The Equality of Windings and Cores in the Two Sets of Magnets

The equality of the windings and cores is assumed in the original construction of the chronoscope. The resistance offered by the two sets of magnets in the chronoscope in this work was found to be exactly the same. The following seems to be a good test for the equality of the magnet cores, on the assumption that any differences would appear in the different amounts of residual magnetism of the two sets of cores. The armature is first perfectly balanced by adjusting the counterweight, after the apparatus has stood idle for several days to insure absence of residual magnetism. Equal currents are then passed through both sets of magnets and at various intervals the balance is again tested. If the cores are alike, the balance should not be disturbed, but if one has a greater tendency to retain magnetism than the other, then the balance will be disturbed. This test made upon our chronoscope showed that when the current passed through both sets of magnets for two minutes the armature came to a position of rest against the lower magnets. This would indicate greater residual magnetism in the cores of the lower magnets. This conclusion was supported by the fact that the balance could be restored by reversing the current through the lower magnets for about one second. Reversing the current through the upper magnets did not restore the balance.

The influence of this factor upon accurate time records is shown by the following readings:

Normal reading	200.1	sigma	A.D.	*0.4*
Current through upper magnets only for 10 min.	200.2	"	"	*0.6*
Current through lower magnets only for 10 min.	201.0	"	"	*0.4*

When the current passed through the upper magnets alone for 10 minutes, the average for 10 readings was 200.2 sigma, and when passed through the lower magnets for 10 minutes the average was 201.0 sigma. This difference of 0.8 sigma is practically covered by the probable error of the difference (0.6). It is clear that for short periods of time small differences in the cores need not be considered.[1]

2. The Equality of the Currents in the Two Sets of Magnets

The arrangement of the circuits as reported by Dunlap insures the equality of the two currents provided there is no great difference of resistance in the two circuits. Both circuits are taken from the same current source. The shunt

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[1] It might be well to mention that the two sets of magnets on our chronoscope were taken from two chronoscopes of the same pattern. One of the original pair had been rewound to correct errors arising from the common method of using the instrument.

circuit containing a rheostat set for a constant resistance as mentioned above, serves to reduce the effect of changes upon the chronoscope circuit of changes in the current source.

Conditions 1 and 2 being fulfilled, the latent time of the two magnets is no longer a factor to be reckoned with. That is, the latent time of the one would be balanced by the latent time of the other. A weak current would make a long latent time for each set of magnets, and a strong current would make a short latent time for each set. In the use of the springs, however, the spring tension would be constant as compared with the latent times which vary with the current strength.

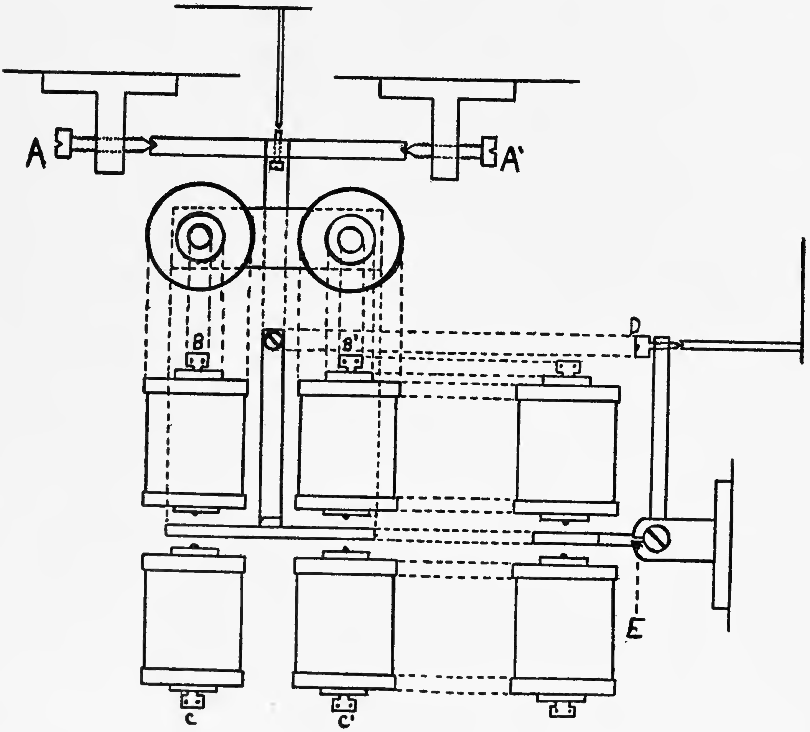

Fig. 2. Front, Top and Side Views of the Mechanism of the Chronoscope. *AA'*. Bearings of Armature Lever. *BB'* and *CC'*. Pins passing through the cores of the magnets and adjustable in position. *D*. Bearing of clutch shaft. This adjustment must be used to compensate for adjustment of pins *BB'* and *CC'*. *E*. Showing how bearing pins of the armature lever *A* and *A'* are kept in adjustment.

3. THE PROPER ADJUSTMENT OF THE CHRONOSCOPE

After the first two sets of conditions were fulfilled and the chronoscope was apparently adjusted, it was found that, when both circuits were closed, the upper magnets always drew the armature away from the lower ones. Then it was noted that the armature did not move equally close to the upper and lower magnets, and that this was just as important a factor as the strength of the current in the two magnets.

This distance it will be seen by referring to Fig. 2 is regulated by the pins *BB′* and *CC′* which pass through and project from the face of the core of each magnet. An attempt to vary the position of these pins showed at once the extreme delicacy of this adjustment and its importance for the proper regulation of the chronoscope. No matter how exactly the two currents may be adjusted, a change in the position of these pins, almost too small to be perceived by the unaided eye, will affect the time, especially where there is any variation in the currents. This will be demonstrated later. Two other adjustments are possible, the importance of which are likely to be overlooked; first, the two screws forming the bearings of the lever, one arm of which is the armature (represented in Fig. 2 by *A* and *A′*); second, the screw (marked *D* in the figure), which regulates the movement of the clutch shaft with relation to the armature lever.

A. The Adjustment of Screws A and A′

Allowing the screws *A* and *A′* to work loose from the vibration of the apparatus caused a change in the average time record of about 6 sigma as shown from the figures in Table I. Each figure is an average of 10 records and is accompanied by its average deviation. In the chronoscope used in this work, these screws gradually worked loose,

TABLE I

	Av.	A.D.
Screws *AA′* properly adjusted	200.0	*0.8*
" " loosened	194.3	*1.1*
" " readjusted	200.3	*0.7*

although they are supposed to be firmly held in position by the split socket. (See *E* in Fig. 2.) When properly adjusted these screws were permanently fastened by pinching the sides of the socket more closely together and covering the screw with shellac.

B. The Effect of Changing the Position of Pins BB′ and CC′

The effect of changing the pins *BB′* and *CC′* may be seen from Table II. In our instrument these pins also were somewhat loose and seemed likely to change their position. After adjustment they were shellaced to prevent movement. The figures in the table represent averages of 10 reactions. They are followed by their average deviations. The changes in the position of the pins are expressed in terms of fractions of a turn of the screw which is the upper part of the pin. An inspection of these figures will show how great a change in the time a slight change in the position of these pins makes.

TABLE II

	Av.	A.D.
Before changing position of pins	200.3	*0.7*
Lowering *C* and *C′* 1/8 turn	202.3	*0.4*
Raising *C* and *C′* 1/8 turn	200.7	*0.8*
" *B* " *B′* 1/6 "	199.5	*0.8*
" *C* only 1/12 "	198.3	*0.7*
" *C* and *C′* 1/12 "	198.1	*0.7*

C. The Adjustment of Screw D

The adjustment of screw *D* must follow that of the pins *BB′* and *CC′*, since by that means the clutch can be properly meshed, after the range of movement has been limited by the pins.

D. A Proposed Method of Testing the Accuracy of Adjustment

The assumption made by Dunlap is that if the chronoscope is properly adjusted, ordinary changes in the current will not affect the time. This being theoretically true, it follows that any changes in the current strength which are accompanied by a variation in the time will indicate that the instrument is not properly adjusted. And further, the nature of the required adjustment can be determined from the direction of the time variation. Hence it occurred to the writers that changes in time as a result of controlled variation of the current strength might serve as a measure of accuracy of chronoscope adjustment.[1] Table III. shows the records obtained by applying this test.

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[1] This test is based on the law that the attracting or repelling force exerted by a magnet at any point in the magnetic field is inversely proportional to the square of the distance from the pole. Now if the pins *BB′* and *CC′* are properly set, when the armature is against the pins of the lower magnets, it will be the same distance from the poles of the upper magnet as it will be from the poles of the lower magnets when it is against the pins of the upper. Therefore, according to the law just stated, the force exerted upon the armature by either set of magnets will be equal. And further the time of movement of the armature will be the same in the one direction as in the other.

Now suppose that the pins were so set that when the armature is down, it is farther from the poles of the upper magnet than it is from the poles of the lower magnets when it is against the pins in the upper magnet. This would make a difference in the relative force exerted by each set of magnets inversely proportional to the squares of the distances from each. In order to give a concrete case, let us suppose that this difference was 0.1 mm., allowing 2 mm. for the thickness of the armature and supposing the total distance that it could move to be 1 mm. The distance of the center of the armature from the two poles when in the two positions against the pins would be 2.2 mm. and 2.1 mm. respectively. This allows for the projection of the pins past the poles of the magnets, a minimum distance of 0.1 mm. Then according to the law of inverse squares, the force of the two poles would be as 1 is to 1.0974.

$$\frac{\text{Distance when at pins } BB' \text{ to lower poles}}{\text{Distance when at pins } CC' \text{ to upper poles}} = \frac{(2.2)^2}{(2.1)^2} = \frac{4.84}{4.41} = \frac{1.0974}{1}$$

Knowing the relative force exerted upon the armature at each position, by the application of the law of acceleration, we can ascertain what difference in time such a difference in force would make. We can also determine the differences when different strengths of current are used. Without going into details to determine the force of the magnets, let us suppose for our purpose that the forces with the different currents used are such as to cause accelerations of 4, 6, and 8 cm. per sigma.

Since with the assumed difference of movement the relative force of the magnets upon the armature in the two positions is as 1 to 1.0974, the relative accelerations of the armature with the three currents would be:

	Upward movement	Downward movement
Weak current strength	4	4.3896
Medium current strength	6	6.5844
Strong current strength	8	8.7792

Applying the formula for acceleration (which is that the distance is equal to one half of the acceleration multiplied by the square of the time, Dis. $= \frac{1}{2} at^2$) we obtain the times for the different strengths of current as follows, keeping in mind that the total distance to be moved is 1 mm.

TABLE III

Changes in Adjustment	Resistance					
	1 Lamp		2 Lamps		3 Lamps	
	Av.	A.D.	Av.	A.D.	Av.	A.D.
Original reading	202.8	*1.3*	200.3	*0.7*	199.0	*0.6*
Lowering *CC′* 1/8 turn	205.0	*1.4*	202.3	*0.4*	196.5	*1.4*
Raising *CC′* 1/8 turn and Raising *BB′* 1/12 turn	202.9	*1.2*	200.7	*0.8*	197.3	*0.8*
Raising *B* only 1/12 turn	201.6	*1.0*	200.8	*0.2*	197.5	*0.5*
Raising *BB′* 1/12 turn	200.5	*1.1*	199.5	*0.7*	197.5	*1.5*
Raising *C* only 1/12 turn	197.3	*0.9*	198.3	*0.7*	196.4	*1.0*
Raising *CC′* 1/12 turn	196.1	*0.9*	198.1	*0.7*	196.4	*0.8*

Each figure in this table is the average of 10 trials. The first column shows the changes that were made in the position of the pins. The next three columns show the

	Upward Movement	Downward Movement	Difference
Weak current	$1.0 = 2t^2$ $t = 0.707$ sec.	$1.0 = 2.1948t^2$ $t = 0.675$ sec.	0.032 sec.
Medium current	$1.0 = 3t^2$ $t = 0.577$ sec.	$1.0 = 3.2922t^2$ $t = 0.551$ sec.	0.026 “
Strong current	$1.0 = 4t^2$ $t = 0.500$ sec.	$1.0 = 4.3896t^2$ $t = 0.477$ sec.	0.023 “

The difference between the time of the upward and downward movements for the weak, medium and strong currents is respectively 0.032, 0.026 and 0.023 seconds. The upward movement is slower than the downward movement in every case. But the stronger the current the less the difference. This shows the advantage of a fairly strong current to escape the effects of slight misadjustment. If we work out the time values for a greater difference in adjustment, *e. g.*, 0.3 mm. instead of 0.1 mm., we get the following:

	Upward Movement	Downward Movement	Difference
Weak current	0.707 sec.	0.618 sec.	0.089 sec.
Medium current	0.577 “	0.505 “	0.072 “
Strong current	0.500 “	0.437 “	0.063 “

This shows that the greater the misadjustment, the greater will be the divergence of the readings with the different current strengths.

Consider now the records of actual chronoscope tests after the adjustments had been made as accurately as possible. The three current strengths consisted of the regular light circuit with one 16-c.p. lamp in circuit (the weakest current), two 16-c.p. lamps in parallel (the medium strength current), and three 16-c.p. lamps in parallel (the strongest current). The weak current gave a reading of the chronoscope of 202.8 sigma as an average of 10 readings; the medium current gave a reading of 200.3 sigma, and the strong current gave a reading of 199.0 sigma. This shows that the pins readings with one, two and three lamp resistances. The changes in the position of the screws *BB′* and *CC′* indicated by the fraction of a turn, are only approximate, although the extent of the movement was judged as accurately as possible by the eye. A comparison of the readings in the three columns shows that the times were closer together or farther apart as a result of the change in position of the pins.

It will be noted that in the final record there is still a difference of about 2 sigma between the two lamp reading and the one and three lamp reading, also that the general average is somewhat below 200 sigma which is the standard time. This was found to be due to the fact that the changes in the position of the pins required a compensating adjustment of the counterweight. When the armature had again been perfectly balanced, the following readings were obtained:

1 Lamp[1]		2 Lamps		3 Lamps	
Av. 198.4	A.D. *0.4*	Av. 200.1	A.D. *0.3*	Av. 198.8	A.D. *0.6*

The above readings are about as close as one can get with the present means of adjusting the counterweight and

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were now so set as to give the same situation as shown in the theoretical case above, *i. e.*, that when down the armature was farther from the poles of the upper magnets than it was from the poles of the lower magnets when up. Experimenting confirmed this, for lowering the pins in the lower magnets, thus increasing the difference still more, gave the following readings: weak current 205.0 sigma, medium current 202.0 sigma, and strong current 196.0 sigma. Raising the lower pins again gave readings similar to those obtained in the first instance. A complete series of such readings taken as adjustments were made is shown in Table III. It will be seen that changes in the positions of the pins were so slight that accurate adjustments would be impossible without the aid of some test such as this.

To use the test for accuracy of adjustment, one need not follow the theory of the matter. All that is needed after the currents in the electro-magnets have been made equal, is to alter the current strengths, affecting both circuits alike and take readings with each change. If the readings differ, the pins of one pair of magnets may be changed. If this makes the readings more diverse, it will be seen that the wrong movement was made and the succeeding adjustments can then be made in the right direction.

[1] Here it will be noted that the one-lamp reading has dropped slightly below the two-lamp reading, leaving the time longer for the two-lamp resistance than for either one or three lamps. The same thing may be noted in the last two changes in Table III., where only one pin was changed at a time. The writers are unable to account for this change unless it be due to slight differences in the position of *C* with relation to *C′* or *B* with relation to *B′*. As it occurred only when the three readings came very close together, it does not reduce the accuracy of the method, and has only a theoretical interest.

moving the pins. A more severe test of the adjustment of the chronoscope was made by changing the resistance at each reading over a range of from one to three lamps, in a continuous series of 25 readings. The results of the test are shown in Table IV. The first and third columns represent the number of lamps resistance, and the second and fourth columns the individual readings of the chronoscope. Below is the average of these readings with its average deviation. The changes of current in this test are greater than the fluctuations of an ordinary commercial circuit. The results of this test seem to give ample proof of the value of Dunlap's method of using the Hipp chronoscope.

TABLE IV

Resistance	Readings	Resistance	Readings
3	198	2	200
2	198	3	199
1	197	2	200
2	200	1	196
3	199	2	201
2	200	1	197
1	198	3	199
3	198	2	200
2	199	1	196
1	197	3	199
2	200	1	196
3	198	3	198
		1	196
			Av. 198.4 A.D. *1.2*

4. THAT THE CURRENT ACTS UPON THE TWO SETS OF MAGNETS FOR APPROXIMATELY THE SAME LENGTH OF TIME, OR THAT THIS DIFFERENCE HAS NO APPRECIABLE EFFECT

After the chronoscope had been adjusted by the method described under 3, the current was sent through each set of magnets separately and for periods varying in length. When the current was allowed to flow continuously through the upper magnet only, for ten minutes, there was no change in the standard time. When the same procedure was followed for the lower magnets the time was increased about 0.8 sigma with a P. E. of 0.6.

This shows that a cumulative difference of 10 minutes or less between the times the current acts upon the two sets of magnets makes no difference in the chronoscope record. In the ordinary reaction time procedure, such a cumulative difference would not occur, on account of the intervals between measurements.

THE WUNDT HAMMER AS A CONTROL INSTRUMENT

The foregoing tests were made with the Cattell gravity chronometer, which has been described in several reports.[1] However, as most laboratories are equipped with the Wundt hammer, a number of readings were taken to determine the reliability of this instrument for control purposes. In order to adapt it to Dunlap's method, the hammer was used as follows: The electro-magnet of the hammer was controlled by a separate circuit. The circuit containing the upper magnets of the chronoscope passed through the core of this electro-magnet to the arm of the hammer. An additional binding post was attached to the hammer at its fulcrum. This circuit was broken as soon as the hammer head was released from the core of the magnet, and the chronoscope began to record at this point. The upper key on the Wundt hammer was set so as to be *made* about the middle of the hammer's fall, and served to remake the circuit through the upper magnets as required by Dunlap's method. This key was changed to a mercury contact key on account of the uncertainty of action of the original contact. The circuit through the lower chronoscope magnets included the break key at the base of the hammer. The readings taken with the instrument thus arranged are given in Table V.

The apparatus is not as satisfactory as the more cumbersome types of control, but with care might be used as a test for ordinary reaction times. This method of wiring which would eliminate the inaccuracies attributed to the upper contact of the hammer probably accounts for an accuracy greater than that usually reported with the use of this apparatus.

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[1] Cattell's original description of the apparatus appeared in *Memoirs of the Nat. Acad. of Sciences*, 7, 1893.

TABLE V

	Av.	A.D.
1st 10 readings	199.8	*1.4*
2d " "	198.8	*1.2*
3d " "	198.2	*0.8*
4th " "	199.3	*1.1*
5th " "	198.5	*1.1*
6th " "	197.6	*0.6*
7th " "	198.0	*0.6*
8th " "	199.0	*1.4*
9th " "	197.1	*0.3*
10th " "	197.9	*0.3*
	198.3	*0.7*

A TEST FOR PERMANENCE OF ADJUSTMENT

After several months' use of this apparatus by a beginning class in experimental psychology and for research purposes (during which time the apparatus was moved from room to room) it was again tested for accuracy. At various intervals it had been tested with the Wundt hammer, but no changes in adjustment were made. This final test was made with the Cattell gravity chronometer in the same manner as the earlier tests. The first ten readings taken were as follows:

201	198
200	200
200	199
200	199
198	200

Av. 199.5 A.D. *0.8*

CONCLUSION

This test of the Hipp chronoscope according to the method proposed by Dunlap seems to justify the following conclusions:

1. The method is more accurate than that with the spring adjustment and removes the necessity of accurately controlled current source, when the four conditions stated at the beginning of the paper have been fulfilled.

2. The equality of magnet cores which is one of the above conditions, may be tested by finding whether the balance of

the armature is disturbed after the current has passed through *both* sets of magnets for a considerable length of time.

3. Any change in time due to a variation in current strength will indicate a lack of proper adjustment, and the manner in which the time changes will indicate the nature of the adjustment required.

4. The Wundt hammer may be adapted by a change of wiring to serve as a fairly accurate control when the chronoscope is used in this manner.

5. No chronoscope which has given good records with springs, should be used for Dunlap's method until by proper adjustment it can withstand such a test as that outlined in this paper.

THE MÜLLER-LYER ILLUSION WITH CHILDREN AND ADULTS

BY RUDOLF PINTNER AND MARGARET M. ANDERSON

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The well-known illusion first described by Müller-Lyer was shown by the writers to children of different ages, varying from six up to fourteen. The original purpose of the work was to discover whether such an illusion could not be utilized as a suggestion test that might find its place among other mental tests. Work already done by Binet[1] and by Seashore and Williams[2] with this illusion showed some difference in the responses of children and adults. Rivers[3] found that children were more influenced by the illusion than men to the extent of 3 mm., with a standard of 75 mm., though the judgments of women varied very slightly from those of children. In the work of van Biervliet[4] the influence on children seems to be much greater since it is about twice as much as with adults, and they show about twice the mean variation. Winch[5] reports a greater degree of suggestibility to the vertical-horizontal illusion among children than among adults. None of these writers had tested large numbers of children at each age and, though all report a decrease in the amount of the illusion with an increase in age, their results scarcely show how this decrease of the illusion takes place from year to year. It was felt by the writers that if this

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[1] Binet, A., 'La Mesure des Illusions Visuelles chez les Enfants,' *Revue Philosophique*, Vol. XL., 1895, p. 11.

[2] Seashore and Williams, 'An Illusion of Length,' *Psychological Review*, VII., 1900, p. 592, and 'Visual Perception of Interrupted Linear Distances,' Univ. of Iowa Studies in Psychology, Vol. II., 1899, p. 5.

[3] Rivers, W. H. R., 'Observations on the Senses of the Todas,' *British Journal of Psychology*, I., 1904–5, p. 321.

[4] Biervliet, J. J. van, 'Nouvelles Mesures des Illusions Visuelles chez les Adultes et les Enfants,' *Revue Philosophique*, Vol. 41, p. 169.

[5] Winch, W. H., 'The Vertical-Horizontal Illusion in School Children,' *British Journal of Psychology*, Vol. 2, p. 220.

decrease were marked and if the average deviation at each age were relatively small, some correlation between the amount of the illusion and general intelligence might be discovered, so that we might have a suggestion test, easy to apply and definite in interpretation. It might be said at the outset that these hopes of the writers were not realized as the results came to hand.

The Method of Procedure.—The apparatus employed followed the usual type used to measure this illusion. On a base-board two pieces of tin were fastened so that the one moved easily under the other which was stationary. The stationary piece had fastened on it a piece of paper with the line having the acute angles and the sliding piece had a similar paper with the line having the obtuse angles at the farther end. This is almost exactly the same as the apparatus used by Rivers.[1] The length of the stationary line was 12.7 cm., having acute angles of 35 degrees. The length of the movable line was 16.4 cm., with the oblique lines making angles of 146 degrees. The thickness of the lines was about 2.5 mm. On the reverse side of the base-board an aperture was cut large enough to allow a millimeter scale to be affixed by means of which the amount of the illusion could be readily determined.

Each observer was given six trials, three with the movable slide extended as far as possible and three with the slide pushed in as far as possible. In the one case the movable line was very obviously too long and in the other very obviously too short. The illusion board was given to the observer and he was told to push in the movable slide until the two lines looked equal. He was shown in each case how the apparatus worked. In the second three trials he was told to pull the slide out. The children were told to 'make the two lines look the same' or 'look equal.' None of the children were found to be familiar with the illusion. Among the adults those who were found to be familiar with it were told to make the two lines look the same and not to try to overcome the illusion by means of estimating how much

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[1] *Op. cit.*

shorter the one ought to look in order to be equal to the other. In all, 250 children, varying in age from six to fourteen, and 28 adults acted as observers.

In regard to the behavior of the observers it was noted that, on the whole, the adults took a much longer time than the children to make their judgments. They showed greater care and deliberation. Many of the children came to a decision very rapidly. Among some of the younger ones, notably the six-year-olds, it is questionable whether they really understood what was required. This rapidity and flightiness of judgment shown by the children is reflected in the larger average deviation of the younger children, as will be noted below.

The Results.—The first set of three judgments made with the movable line extended, obviously longer than the standard line, has been called the First Series. The second set of three judgments where the comparison line is obviously shorter than the standard line has been called the Second Series. The difference between these two series is so great and so constant at all ages that it seems best to deal with them separately rather than with an average of the two series. The average amount of illusion of the three trials and the average deviation for each individual for each series was computed. These figures obviously cannot be given here. Since our main interest lies with the change in the amount of the illusion at each age, it will suffice to give the average amount of illusion at each age.

The amount of illusion in all cases, except where percentage of illusion is given, is measured in millimeters, showing the excess or positive amount added to the variable line by the observer. There are a few cases in which the observer made the variable line shorter than the standard. This error occurs very seldom and seems to be more common among younger children. Since it does not appear constantly in any three or six judgments of the individual, it is very probably due to inattention or lack of comprehension on the part of the observer. This error appeared very seldom among the adults.

TABLE I

FIRST SERIES

1 2 3 6 7 8 9

Age	No. of Observers	Averages of Judgments			Average of the Three Judgments	Average Deviation	General Average Deviation	Percentage of Illusion
		1st	2d	3d				
6	14	21.2	18.0	17.6	18.79	3.53	6.7	14.8
7	24	17.6	17.6	20.4	19.90	4.96	9.0	15.7
8	38	16.4	17.2	19.1	17.60	3.72	8.7	13.8
9	32	18.7	18.6	19.3	18.80	3.20	7.2	14.8
10	34	17.3	16.9	18.6	17.81	2.88	4.9	14.0
11	37	14.7	18.1	17.7	17.55	3.87	7.7	13.8
12	34	15.7	17.2	18.0	17.20	3.44	6.4	13.5
13	19	17.8	17.4	18.0	18.20	2.78	7.5	14.3
14	18	15.0	13.1	14.2	13.29	3.10	6.9	10.5
Adult	28	16.1	16.8	16.8	15.40	2.54	7.8	12.1
F. M.	13	20.6	16.1	17.9	18.15	4.55	4.5	14.3

Table I. shows the results for the First Series. The first column gives the age of the observer; the second the number of observers tested at each age; the next three columns give the average amount of illusion for the first, second, and third trials respectively; the sixth column shows the general average for each age of the individual averages for the three judgments; the seventh column shows the average of the average deviations of the three trials for each individual; the eighth column gives the general average deviation, *i. e.*, the deviations of the averages of the three trials for each individual from the general average for all the observers of the same age; and the last column gives the percentage of illusion calculated from the average of the three judgments. The percentage of illusion is on the whole less in these experiments than the percentages reported by Rivers[1] and by Seashore and Williams.[1]

Table II. gives the corresponding results for the Second Series, in which the comparison line was pulled out until it appeared equal to the standard.

Curves I., II., III., and IV. show the average amount of illusion at each age for each series for the first, second, third, and average judgments respectively. A glance at these curves shows that in general the illusion tends to decrease

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[1] *Op. cit.*

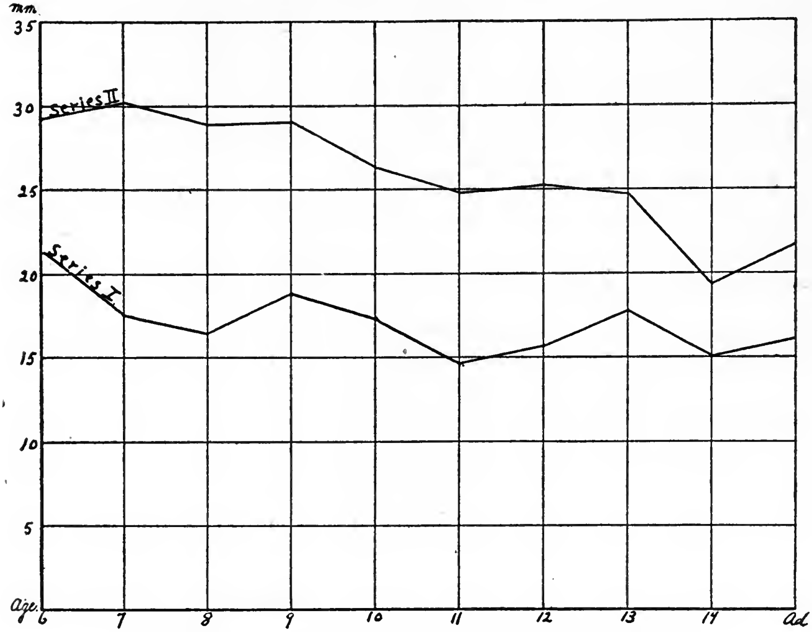
with increasing age. Some of the curves show a fairly uniform decrease, but every now and then there are drops and rises in the curves that spoil the general symmetry of the whole. The most noticeable is the drop at age fourteen on almost every curve. Why there should be this decrease in the amount of the illusion at fourteen does not seem explicable. There appeared to be nothing exceptional in the group of fourteen-year-olds to mark them off in any way from the other children or from the adults. It is true that the number of fourteen-year-olds was not quite as large as in most of the other ages, although we have as many at this age as at ages six and thirteen.

TABLE II

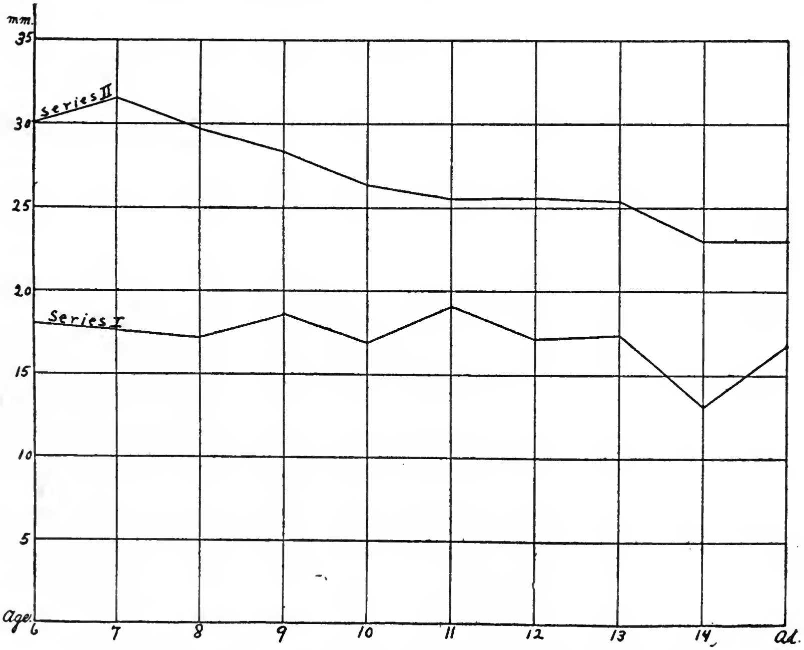
SECOND SERIES

Age	No. of Observers	Averages of Judgments			Average of the Three Judgments	Average Deviation	General Average Deviation	Percentage of Illusion
		1st	2d	3d				
6	14	29.3	30.0	34.4	31.07	3.71	7.3	24.5
7	24	30.2	31.5	32.0	32.70	3.24	7.1	25.7
8	38	28.9	29.7	29.2	30.40	3.48	8.5	23.9
9	32	29.0	28.4	28.8	28.56	3.02	7.4	22.5
10	34	26.4	26.4	27.0	26.80	1.92	5.3	21.1
11	37	24.9	25.6	24.9	25.45	2.79	7.4	20.0
12	34	25.2	25.6	25.2	24.88	2.51	5.9	19.6
13	18	24.8	25.4	26.3	25.50	2.16	5.5	20.1
14	18	19.4	23.0	23.7	22.50	2.30	5.5	17.7
Adult	28	21.9	23.0	22.6	22.40	1.54	6.8	17.6
F. M.	13	26.3	24.2	21.8	24.10	3.24	6.7	19.0

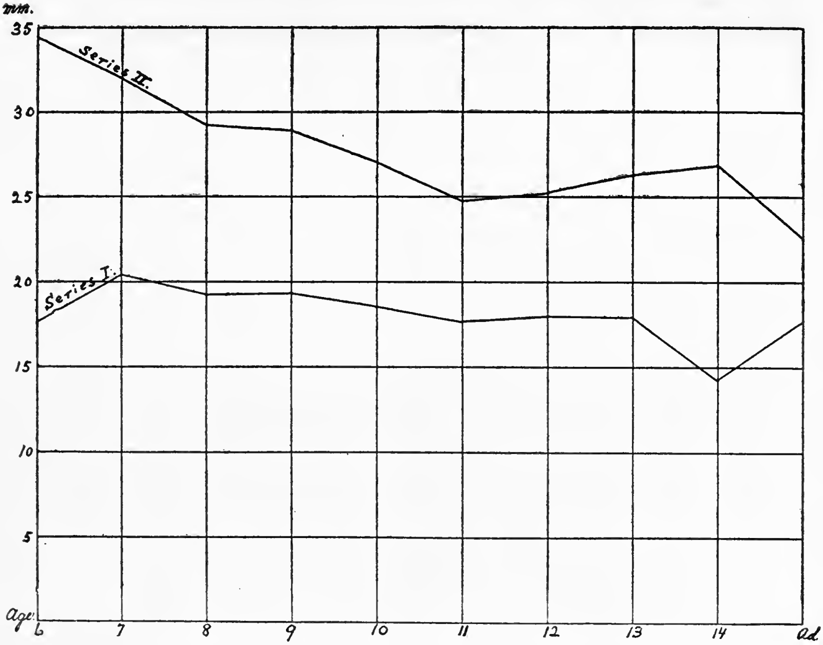
It is obvious from these curves that the difference between the amount of illusion at any two ages is never sufficient to mark off one age from another, so as to make this illusion useful as a test. And this is more obvious when we examine the figures in Tables I. and II. a little more carefully. The general averages for the three judgments fluctuate between 15 mm. and 20 mm. for the First Series (neglecting the drop to 13 mm. at age fourteen), and between 22 mm. and 32 mm. for the Second Series. In the First Series there is a difference of 5 mm. distributed over ten age groups and in the Second Series a range of 10 mm. distributed over ten age groups. In the last series this difference is indeed fairly well marked



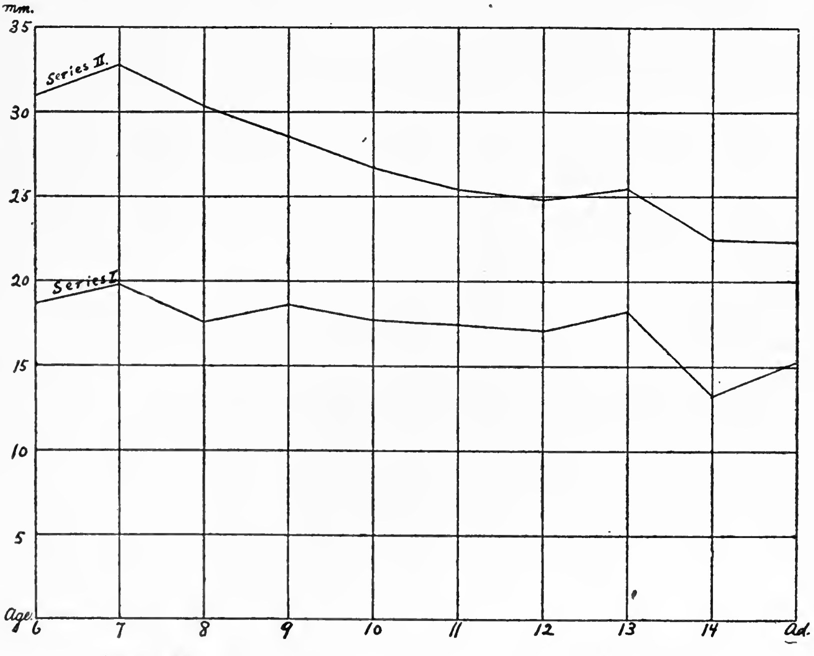
Curve I. Average Amount of Illusion for First Judgment.



Curve II. Average Amount of Illusion for Second Judgment.



CURVE III. Average Amount of Illusion for Third Judgment.



CURVE IV. Average Amount of Illusion for the Three Judgments.

and yet any hope of seeing a constant tendency in the amount of the illusion at any one age is marred by an inspection of the general average deviations in the eighth column. These large average deviations at almost every age show that there is little uniformity in the amount of the illusion to which an individual may be subject according to his age. There are many adults who are more subject to the illusion than many of the younger children. If we are to believe that this illusion decreases owing to the individual's increasing experience at estimating distance, it seems hard to understand these discrepancies although in a very general way this may be correct, since there undoubtedly is a constant tendency to decrease present. Again it would seem as if there is only a very slight increase in the uniformity of judgment in the individual as he grows older. The average fluctuation of the individual in his six judgments is shown in the seventh column under average deviation. On the whole it would seem that as the child grows older he becomes more uniform in his attitude, that is to say, he varies less from one judgment to the other. As we have noted above, the larger amount of the individual deviation in the lower ages corresponds well with the rapidity and flightiness of the child's behavior in making his judgments.

Both the tables and the curves show very markedly the great difference between the first and second series. This same difference appears in the results of Rivers.[1] Binet[1] calls this an error due to expectation, and in conformity with the more general phenomena of contrast. Binet's variable line, however, was the line enclosed by the acute angles and the method of presentation was radically different from ours.

It seems to us that the general tendency of the observer is to continue pulling out the slide until the comparison line looks almost longer than the standard. For most observers there is undoubtedly quite a considerable field within which the two lines are equal. Beginning with a much shorter line he does not stop when he first reaches the field, but tends to go on a little further to see if this is just right, and

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[1] *Op. cit.*

indeed often until the comparison line looks slightly too long. This was seen from the behavior of many observers who pulled out the slide and then pushed it slightly back. The reverse conditions would hold for the First Series, beginning with the long line.

TABLE III

TABLE OF DIFFERENCES

Age	No. of Observers	Differences Between the Two Series			Difference of Averages
		1st	2d	3d	
6	14	8.1	12.0	16.8	12.28
7	24	12.6	13.9	11.6	12.80
8	38	12.5	12.6	10.1	12.80
9	32	10.3	9.8	10.5	9.76
10	34	9.1	9.5	8.4	8.99
11	37	10.2	7.5	7.2	7.90
12	34	9.5	8.4	7.2	7.68
13	19	7.0	8.0	8.3	7.30
14	18	4.4	9.9	9.5	9.21
Adult	28	5.8	6.2	5.8	7.00

The difference between the first and second series at each age is shown in Table III. In the columns headed first, second, and third judgments are shown the differences between the average judgment for the First and Second Series at each age. In the last column the differences between the averages of the two sets of judgments are given. These differences show the excess in millimeters of the Second Series over the First. It is interesting to note how uniformly this difference decreases from age to age. Undoubtedly the factors, whatever they may be, that are at work in making us pull out the standard line too far in the Second Series, are not nearly so effective as we grow older. Not only does the uniformity in judgment become greater in the three trials of each series but also in the two series compared with each other.

Thirteen feeble-minded individuals, all of whom had been rated about nine years mentally according to the Binet Scale, were also tested with this illusion. The results are shown in the tables on the line marked F. M. There is nothing very exceptional in the results. Their average judgment on the First Series corresponds to the judgments of the children, since it is larger than the average judgment of the adults. This is true also of their judgments in the Second Series. The individual average deviation is large, particularly in the First Series, indicating a great fluctuation in individual judgments. If this is an indication of fluctuation in attention, it would be very appropriate with this group. The general average deviation is very much like the average deviations for normal individuals. It indicates, as in the normals, wide fluctuations between the individuals of the group.

Conclusions.—The Müller-Lyer illusion cannot be used as a test for suggestion which will correlate with general intelligence as measured by increase in chronological age. With increasing age there is a gradual decrease of illusion along with a gradual decrease in individual fluctuation. Neither of these decreases is uniform. At each age we have a large general average deviation, which indicates a great fluctuation between the individuals of each age group. We may deduce from this that the decrease in the illusion is due to the individual's increasing experience in judging distances as he grows older, but this can only be true in the most general sense. Or we may agree with Binet[1] when he says that the illusion is "innate" or "physiological," since the differences in the amount of the illusion between one age and any other age are not very great, and since the individuals in each group vary so much. Rivers[2] did not find Englishmen superior to savages (Todas and Murray Islanders) in this illusion, in spite of the fact that university students would have naturally had more experience in judging distances, especially in dealing with geometrical figures.

Considering the illusion 'innate' or 'physiological' and therefore more or less equally potent to all observers, the slightly greater amount of illusion with younger children might be explained as a matter of attention. They are not able to attend so well to the task; they are more careless and therefore make larger errors. This would be borne out by

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[1] *Op. cit.*

[2] *Op. cit.*

the larger average deviations of individual judgments in the lower ages. In this case we would have to suppose that the savages tested by Rivers were equal to civilized individuals, as far as attention is concerned. The emphasis we have laid upon attention would correspond well to Smith's[1] suggestion of some slight correlation between accuracy of work and smallness of illusion. Furthermore he looks upon individual variability as an index of the general factor of attention, and as we have noted above, the individual variability was greater among children than adults. Very probably all three factors of contrast, experience, and attention are operative in producing the illusion and in increasing the extent of the illusion with children.

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[1] Smith, W. G., 'A Study of Some Correlations of the Müller-Lyer Visual Illusion and Allied Phenomena,' *British Journal of Psychology*, Vol. 2, p. 16.

REPORT OF PSYCHOLOGICAL TESTS AT REED COLLEGE

BY ELEANOR ROWLAND AND GLADYS LOWDEN

Reed College

It was the purpose of those conducting this investigation to discover if there were some practical and valuable grouping of psychological tests for college students which would give an index of relative mental ability. Our problem was to find if any group of tests showed correlation with the office grades in college subjects, granting the assumption that grades in college subjects are an indication of the mental ability of the student. The tests were carried out individually on all the students in Reed College, over a space of three years. Three classes received the tests during the sophomore year, and one during the freshman year.

A group of experiments was selected for trial which were standard tests for measuring the processes of memory, association, attention, suggestion, imagination and judgment. Both rote memory and logical memory were tested. For the former a series of 42 cards was used for auditory-visual-articulatory presentation, and the experiment was performed and evaluated according to Whipple's 'Manual of Mental and Physical Tests,' pp. 364–66. The logical memory test consisted of the reading twice to the subject of a story of ten logically related incidents, after which he was to write this story in the form in which it was read. As it contained 166 words, it is evident that it could be remembered only by meaning, that is by the dependence of one point on the preceding.

Complete success in the logical memory meant 10 points. Failure in one complete section of the ten meant a deduction of 1. Failure in one half a section meant a deduction of 0.5. Attention was measured by three different tests. For studying the range of visual attention, cardboards were prepared each containing 10 unrelated objects. Each card was exposed six seconds, after which the subject was to repeat as many objects as could be remembered. In order to measure the degree of attention, cancellation blanks were used: first, where a single letter (a) was cancelled; second, four letters, (q, r, s, t); third, words containing both *a* and *t*. The test of crossing out assigned letter or letters or words from printed sheets demands maximal attention inasmuch as any reduction of attention is reflected at once in the speed or accuracy of the work done. A similar test eliminating the motor factor was given in asking the subject to count the number of o's in three paragraphs of prose without pointing. There were 53 o's in the paragraph.

The result for attention span was the average number of objects recalled from the seven exposures.

The three cancellation tests and counting o's were figured according to Whipple's 'Manual,' p. 261, on the basis of speed divided by accuracy where accuracy equals number cancelled over number present.

Uncontrolled association was tested in the usual way by giving the subject a test word after which he was to write as rapidly as possible sixty-six words suggested. The standard whole-part, species-genus, and opposite lists comprised the material for measuring controlled association. Simple addition, subtraction and multiplication were introduced to test even more strictly controlled association.

Uncontrolled association and controlled association by whole-parts, species-genus and opposites was scored on the basis of speed only. Hence shortest time indicates greatest success. In case of opposites, the final figure was the average time for three lists. Speed was divided by accuracy to give the figure for computation tests, exactly as in the cancellation tests.

Imagination was tested by creative ability in linguistic invention. Both Miss Sharp's method[1] of incorporating into sentences three nouns and verbs, and also Binet's method of partially written sentences to be completed were tried.[2]

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[1] *A. J. P.*, 10, 1899, 329–391.

[2] Whipple's 'Manual,' p. 438.

For the linguistic invention, two sets of nouns and two of verbs were given. The final score was the average number of sentences for nouns and the average for verbs. Care was taken to strike out such sentences as repeated the thought with a change of tense or number. Sentence completion was recorded on the basis of speed. Hence shortest time was the greatest success.

In the first arrangement of tests were included but two of suggestion: progressive lines as first used by Binet, and suggestion by the illusion of heat, according to Whipple, p. 424.

The procedure for computing results in the line length test was according to Binet, 'La Suggestibilite,' p. 102.

Ten trials were given in the heat test, five with each hand, and record was made of the number of times heat was felt. Hence 0 was success; 10 failure.

Ten syllogisms, some true, some false, were used to test judgment. We also included a quick judgment test devised by Munsterberg, and described in 'Psychology and Industrial Efficiency,' p. 87.

In the winter of 1912–13 this grouping of psychology tests was tried out on 54 students of Reed College, none of whom had had experimental psychology, thereby eliminating the practice element. Each student was tested separately, and with careful planning on the part of the experimenter, the set could be completed within two hours.

When the scoring of each individual test was completed relative lists of the standing of each subject in each test were made in which the upper one half were considered high and lower one half low. For the purposes of correlation, credits in college courses earned by each student for the semester in which the tests were taken were secured from the college office, and arranged in a relative list in which the upper one half were considered high and the lower one half low. With the material in this form it was possible to work out the coefficient of correlation figures by substitution in the formula of Mr. G. Udney Yule.[1] The coefficient of corre-

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[1] Jr. Royal Statistical Soc., Vol. LXXV., Part VI., pp. 579–642.

lation is called ω, and with the use of the four-fold table

$$\begin{array}{|c|c|} a & b \\ \hline c & d \end{array}$$

ω is equal to $(1 - \sqrt{K})/(1 + \sqrt{K})$ where $K = bc/ad$. The error of this coefficient is equal to

$$\frac{1 - \omega^2}{4}\sqrt{\frac{1}{a} + \frac{1}{b} + \frac{1}{c} + \frac{1}{d}}.$$

Of course, the smaller the number of cases the greater the probability of error and for a coefficient figure to be really significant according to this formula it must be two or more times as great as the coefficient of error.

With but 54 cases it was to be expected that the percentage of error would be high, but after correlating each individual test with the credits for the semester and the tests as a whole with the credits for the semester, the results were sufficiently gratifying to warrant further investigation.

Some changes, however, were made in the grouping of the tests as a result of the first year's investigation. Uncontrolled association was omitted as taking too much time, and likewise for the reason that it seemed to be measuring the speed of handwriting rather than rapidity of association. It was felt likewise that the value of a free association test lies in its use as an indication of the type of association rather than its speed alone. Computation was omitted also because it seemed to take a disproportionate amount of time. Sentence completion was not repeated because of the low correlation and because of the impossibility of giving credit for the quality of sentences written as well as for the time required to complete them. Five tests were added. A new cause-effect association test, in which 24 sets of paired words connected by cause and effect were read at the beginning of the tests. At the completion of the tests, the first word for each pair was read and a record made of the number of second words, correctly recalled. The greater the number of correct associations the greater the success.

The attention tests showed good correlation: hence a dot-counting test devised by Binet and described in Whipple's 'Manual,' p. 270, wherein efficiency was equal to the average speed divided by the average accuracy was added.

Progressive weights after Binet[1] and comparison blocks after Gilbert[2] were added to the two suggestion tests.

To test mechanical ability the Healey puzzle box was introduced. The shortest time required for opening it constituted the greatest success.

With these revisions the group has been given to 195 more subjects, so that the results for 249 cases were available. On the basis of related lists for the 249 cases, correlation figures were worked out between grades in each mental test and credits in college courses for the semester in which the tests were taken with the result that the q, r, s, t cancellation test received the highest correlation .18 ± .06.

Next to the q, r, s, t test, the association test by opposites received the highest correlation .16 ± .06. At the first glance it seemed mere chance that determined the greater correlation of the lists of opposites, but closer analysis revealed a probable reason for this. It will be remembered that there were three lists of opposites and on each list there were twice as many words as on the whole-part, and the genus-species lists. Hence the chance for error was reduced by the wider range of the test.

Both logical memory and the judgment test (syllogisms) received a correlation of .13 with a percentage of error of .06.

In fact it was noticeable that the highest correlations between individual tests and college credits were for a difficult attention test, the most comprehensive association test, and the two tests requiring logical thought. No other correlations were significant. However, after all, we had no expectation of finding one single test of some one process which could be used as an indication of mental capacity. We hoped rather for a group of tests which would measure mental capacity in the one respect of correlating with office

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[1] Whipple's 'Manual,' p. 410.

[2] *Ibid.*, p. 405.

grades. Hence the next step was an attempt to establish a correlation between some group of tests and the credits. Inasmuch as the q, r, s, t, opposites, logical memory, and judgment (syllogism) tests individually showed the highest correlation, it was most natural to group these four tests and correlate with grades. The resulting figure was .31 with a probable error of .06, showing an increase of .13 for the group correlation over the highest correlation figure for any individual test. Several other combinations of tests were tried, but the one combination raising the figure still higher was formed by adding to the four highest tests, the rote memory, and the attention by cancellation of words with a and t which resulted in a coefficient of correlation of .37 ± .06.

During the process of correlating tests with credits as a whole, it was suggested that some interesting results might be reached by correlating tests with high grade of work in individual courses.

Mathematics and English were selected for the purpose, inasmuch as a larger number of students were enrolled in these courses, thereby reducing the percentage of error for correlations depending on these credits. The significant correlation between tests and credits in Mathematics was for the q, r, s, t, opposites, logical memory, and judgment group, = .25 ± .10. However, it is to be noted that the percentage of error is higher owing to the fact that of the 249 who had taken tests, credits in mathematics were available for but 86.

More material was available for English courses, there being 211 cases. Different combinations showed the highest correlation for English to be between English and association by opposites, = .26 ± .06.

We also worked out some correlations on the basis of sex. The results were as follows: a correlation of .20 between the men and superiority in the syllogism test, and .24 in resistance to suggestion in the line-length test. There was a correlation of .25 between women and superiority in cancellation of a, .38 in cancellation of q, r, s, t and .30 in counting o: that is in the principal attention control tests. The error was .06 in all.

It has been suggested that in a two-hour group of tests, fatigue may tend to render unreliable the results. No doubt this is true in individual cases, but in a group study of so many cases the factor of fatigue becomes unimportant. In an investigation of this kind it is always desirable to have but one experimenter, but owing to the number of cases and the length of time, twelve students of experimental psychology assisted in conducting the tests.

One might also question the assumed value of college grades, but at the present time, they are the standard measurement of ability of students in college classes.

However, in spite of difficulties, the correlation figures are sufficiently high to indicate the desirability of conducting further investigations in the line of mental tests for normal adults.

There seems to be little doubt that the revised list of tests did make a selection of the better students in Reed College. With information from this, and with further revisions, it is hoped to increase the value of this list and to interpret the results with regard to information about the individual student, rather than about the group alone. The latter is all that has been possible so far.

THE INFLUENCE OF THE EMOTIONS ON RESPIRATION

BY ANTOINETTE FELEKY

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The experimental investigation here reported concerned itself with the respiratory changes which accompany the six primary emotions, pleasure, pain, anger, disgust, wonder, fear, as well as laughter and hatred. The respiratory changes studied are (1) duration of inspiration as compared with that of expiration, (2) changes in the depth or amplitude, (3) and changes in the amount of work accomplished per unit of time.

INTRODUCTION

This work grew out of a series of experiments begun in September, 1911, and continued until January, 1915, with a view of recording the influence of thoughts and feelings on the facial organs, known as "the muscles of expression." Over one thousand photographs of one individual, A. F., were taken for this purpose and studied. Samples of these were published in a previous article.[1] Although the camera recorded a great many interesting and valuable facts, many of which were interpreted with considerable success by the 100 judges, thus verifying the mental states revived by A. F. and recorded by the camera, we felt a need of supplementing this knowledge by recording other expressive movements. In order to get good results, we made our task as simple as possible, and concentrated all our efforts in recording and interpreting the respiratory movements only. For this purpose, and as a preliminary study, we had A. F. revive the various thoughts and feelings while the breathing movements were recorded.

The stimuli were the same as those used in posing before the camera: reciting poetry and prose; multiplying and other

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[1] *Psy. Review*, Jan., 1914.

mental activities; reviving bodily movements of the emotions; sense stimuli, taste (sour, sweet, bitter), smell, sight, hearing (noise and harmony); feelings of strain, relaxation, fatigue, as well as the various sensations of pain (head, back, crushed fingers, toes, etc.).

We also studied the respiratory changes which accompany the various movements of the head, arms, upper portions of the body. In fact every conceivable movement was studied and recorded—chewing, swallowing, sneezing, coughing, yawning, sighing, sobbing, crying, speaking, singing and even dancing.

Many of the breathing records were genuine and spontaneous. For instance, while working one may naturally yawn or sigh from fatigue, swallow saliva, sneeze, cough, and make other movements such as 'stop to think,' 'stop to look,' which leave their stamp on the revolving drum of the smoked kymograph. The records of the spontaneous and genuine movements do not differ from the records of the assumed or revived movements. This fact is of great importance, for it seems to prove that the revived feelings express themselves as clearly in the respiratory muscles as they do in the facial muscles. This fact is also of great importance for us, as our experiment greatly depends upon the revived or imaginary stimuli. The reviving of habits of feelings and emotions need be no stranger than reviving the habits of walking, talking, writing, adding, multiplying. It is to be remembered that the newborn infant's experiences are first those of feelings of comfort and discomfort, of crying, sneezing, coughing, smiling, etc. In fact, almost all the primary emotions are experienced long before he attempts to walk.

After having obtained over 100 records of A. F., we experimented with four other subjects, graduate students in psychology (two men and two women), also as a preliminary study, in order to evolve a working method for our experiment. For a better understanding of the breathing curves, we recorded simultaneously the respiratory and facial movements (in the case of A. F. only).

From the results obtained, we concluded: First, that the respiratory muscles speak as clear a language as do the muscles of the larynx (voice), or 'the muscles of expression' (facial organs), but that the three supplement each other, and each must be interpreted. For example, in laughing, crying, yawning, wonder and disgust, the accompanying facial expressions are characteristic; the respiratory movements are modified, and record differently in each case; the vocal cords are thrown into characteristic vibrations. Second, that the feelings and emotions are principally reflected in the 'muscles of expression' and that of respiration. Third, that the emotions are intense feelings, and may be accounted for by the summation of stimuli.

The above suggestions and conclusions are not new. The physiological and psychological investigations on our subject need not be discussed here. However, we may draw attention to the following important facts: Charles Bell, the discoverer of Bell's law, Bell's palsy (facial paralysis), points out the fact that the act of respiration is not limited to the trunk, but includes also the nose, mouth, the windpipe, the throat, the lips, so that the air may be admitted through them in respiration with a freedom corresponding with the increased action of the chest. He proves that the organ of breathing, in its association with the heart, is the instrument of expression, and is the part of the frame by the action of which the emotions are developed and made visible to us. Sudden changes of color in the countenance denote disturbances in the heart's action; labored, irregular breathing of the chest, extending to the neck and face, mark corresponding interruptions to the action of the respiratory organ; and both give rise to the variety of expression, which man interprets as a natural language. Bell not only shows the natural association between the muscles of the face and that of respiration, but he also gives experimental and clinical evidence to prove that in facial paralysis, the muscles of the

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[1] "When a horse has run and pants and breathes hard, the nostrils are alternately dilated and contracted, while the chest rises and falls. So in man, excited by exercise or passion, the shoulders are raised at each inspiration, the muscles of the neck and throat are violently drawn, and the lips and nostrils move in time with the general action." ('The Anatomy of Expression,' by Chas. Bell, p. 7, second ed. also eighth ed.)

face are powerless, and the countenance acquires a characteristic look from the absence of all expression.

Darwin[1] points out that one of the most important facts brought out by Bell is that the muscles round the eyes are involuntarily contracted during violent respiratory effort, and that this fact aids to throw a great deal of light on many of the important expressions in man.

More recent investigations, including those of Mosso, Sherrington, Crile, and Cannon, seem but to strengthen and uphold Bell's conclusions, which are also in accord with our own views. For our purpose, we may merely state that Mosso[2] gives evidence and concludes that "the emotions are principally reflected in the muscles of the face and that of respiration." Bell[3] as well as Mosso draw attention to the fact that the human countenance performs many functions: we find "the organs of mastication, of breathing, of natural voice and speech, and of expression. The face serves for the lowest animal enjoyment, and reflects the highest and most refined emotions."

Cannon[4] states that the differential features of emotions are not to be traced to the viscera, and that this view is also in accord with the experimental results of Sherrington.

Crile[5] states with Sherrington that "the environment drives the brain, the brain drives the various organs of the body." He adds that each separate motor action has its own brain pattern, which is adapted for but one type of motion, and that "the specific stimuli of the innumerable ceptors play each upon its own brain pattern only. In addition each brain pattern can react to stimuli applied only within certain limits."

In regard to the psychological investigations, we must be brief. Both Stevens[6] and Shepard[7] publish experiments on organic changes and feeling and attention, and both review

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[1] 'Expression of the Emotions in Man and Animals,' Chas. Darwin, p. 2, 1873.

[2] 'Fear,' Angelo Mosso, p. 164, 1896.

[3] *Op. cit.*, p. 5 and 6.

[4] 'Bodily Changes in Pain, Hunger, Fear, and Rage,' Cannon, p. 280, 1915.

[5] 'The Origin and Nature of the Emotions,' S. W. Crile, p. 90, 1915.

[6] 'Study of Attention,' H. C. Stevens, *Am. Jour. of Psy.*, 1905.

[7] 'Organic Changes and Feeling,' J. F. Shepard, *Am. Jour. of Psy.*, 1906.

the writings on this subject. Stevens concludes that the psychophysical processes of sensation are different with the visual, auditory, and tactual. Shepard[1] studied Stevens's records and states that as far as any conclusion can be drawn from them, the differences are due to changes in breathing.

Our own observations seem to show that we may not only have visual, auditory, and tactual attention, but we may also have pleasant, fearful, angry, etc., attention, and when the intensity of the feeling is increased, we experience the various emotions. We may say with Wundt[2] that intense feelings are emotions. "As a result of the summation and alternation of successive stimuli there is in emotion not only an intensification of the effect of the heart, blood-vessels, and respiration, but the external muscles are always affected in an unmistakable manner. .Strong movements of the mimetic muscles appear at first, then movements of the arms and of the whole body (pantomimetic movements). In case of stronger emotions there may be still more extensive disturbances. . . ."

As each emotion has a characteristic expression, we may expect to find corresponding changes in the respiratory apparatus. This is the object of the following experiment.

Apparatus and Method

Records were taken of the respiratory movements by means of Ellis's pneumograph, a closed tube distended by a spiral spring and fastened around the chest of the subject. Inspiration changes the air pressure in the tube, and this change is transmitted to a Marey tambour which writes the respiratory movements on the smoked surface attached to a drum of Porter's kymograph. Time in seconds was recorded by means of a Jacquet chronometer.

In order to simplify measurements, Benussi's[3] method of tracing each record on unit paper was employed. As milli-

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[1] 'The Change of Heart Rate with Attention,' Billings and Shepard, *Psy. Review*, 1910.

[2] 'Outlines of Psy.,' 3d ed., 1907, W. Wundt.

[3] *Archiv. Gesamte Psy.*, Jan., 1914, p. 244. Article "Die Atmungs symptome der Lüge," by V. Bennussi.

meter paper was not available, the unit paper, having 1/20th of an inch to a unit, was used. The beginning and ending of each inspiration and expiration was projected on that straight line which conformed to the horizontal position of the writing pen.

There were six subjects: A. F., who had training in posing for the various graded emotions with or without the camera, and five other women who had no such training. Among the latter were three actresses, one singer and one school teacher.

The subjects were seated in a comfortable position beside a table upon which stood the registering instruments. In order not to distract the attention of the subject, the instruments were hidden from view by a screen.

The questions asked and instructions given to the subjects were as follows: "Have you ever found yourself smiling or laughing at some experience which had previously made you smile or laugh?" This question was answered affirmatively by all. "Now recall an experience which gave you pleasure and try gradually to increase this pleasure to your utmost capacity, then suddenly think of something funny and pleasurable, and begin to laugh." A similar question was asked in regard to the emotion of anger. The subject was then told to revive the emotion and gradually to increase the intensity and finally to say the words, 'I hate you,' at the same time to try as much as possible to feel this hatred. The same directions were given in the case of the emotions of pain, disgust, wonder and fear. The last four were merely increased in intensity without a change from pleasure to laughter, or from anger to hatred. Thus the revival of the six emotions, pleasure, pain, anger, wonder, fear, and disgust, which the writer considers the primary emotions, were employed.[1]

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[1] The seven primary emotions named by McDougall include fear, disgust, wonder and anger. He adds the emotions of subjection, elation and the tender emotion, and states that "from these seven primary emotions together with feelings of pleasure and pain (and perhaps also feelings of excitement and of depression) are compounded all, or almost all, the affective states that are popularly recognized as emotions." As our objectivized emotions (photographs) seem to show some overlapping in this classification, we therefore substituted pleasure and pain, which have characteristic expressive movements, for elation, subjection and the tender emotion. In fact the

In order to make the situation as natural as possible, we had A. F., the trained subject, assume the various emotions together with the other subjects.[1] This helped to do away with some self-consciousness, which was very noticeable. This was more apparent in the school-teacher, who had no musical or dramatic training, than in the singer and the actresses, although all seemed self-conscious until A. F. worked with them. Attention must be drawn to the fact that the intensifying of the emotions in this manner is arbitrary, and not the natural mode of expressing them. Each situation will bring along with it a certain degree of emotion, but in order to study the various intensities of the different emotions pure and simple this seemed the most practicable way.

The first record was simply taken to orient the subject to his task. Enlarged photographs of A. F. of the various degrees of pleasure culminating in laughter were shown to the five subjects. A normal curve was taken for some seconds, then one photograph was shown at a time. The subjects all smiled when shown the smiling expression and increased their smiling, corresponding to the intensity found in the photos. The breathing records also showed corresponding changes. Then the different emotions were revived.

It was to be expected that the effect upon the respiratory organs, produced by varying the intensity of the different emotions, would not turn out as perfectly as desired. Then again, the various emotions are inhibited more by some individuals than by others.

One subject mentioned that she seldom is provoked to anger and seldom hates. The only person this subject ever hated was an uncle who defrauded her of an inheritance. Two other subjects were under the impression that laughing aloud is not proper, therefore they always suppressed it, or

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parental instinct not only excites the tender emotions, but also all the emotions which we consider are the primary emotions.

[1] It is well known that just as a frightened animal may communicate fear to another animal, so may one individual spread joy or fear to another. Thus our stimuli consisted not only in the revived emotional experiences of the subject, but also the bodily expressions of the emotions in another individual.

laughed inwardly. Thus taking into consideration the difficulties encountered, and for the sake of uniformity, we have eliminated in our measurement all the curves with the exception of the five middle ones, which would give us only the medium degree of each emotion. In the case of wonder, where the curves are characteristic, one respiratory phase, which includes an inspiration and an expiration, was all that was necessary for measurement. The downward stroke corresponds to inspiration and the upward stroke to expiration.

The following figures show the character of respiration during normal breathing, and the medium degree of the emotions of pleasure, pain, anger, disgust, wonder, fear, laughter and hatred. The amount of respiratory work accomplished per unit of time was found by dividing the amplitude, which is both force and distance, by the time taken for each respiratory phase. Thus the sum of the five middle amplitudes or inspirations was taken and divided by the time taken by five inspirations and five expirations. In the case of wonder only one amplitude was measured.

Column I. gives the results for the relation between inspiration and expiration. When I. : E. as 3 : 3, or 4 : 4, or 5 : 5, etc., we get a ratio 1.–. When the ratio is less than 1.– the time for inspiration is less than the time for expiration. When the ratio is greater than 1.– then the time for inspiration is greater than that of expiration. The inspiratory pause was included as a part of inspiration, as may be seen in the characteristic curve of 'wonder'; and the expiratory pause was considered as belonging to expiration, as shown in the breathing curves of laughter and hatred. Only the beginning of each inspiration and expiration was marked off for measurement. That is the end of the first inspiration including what is considered a pause (if one exists) was of course the beginning of the expiration; and the end of the expiration was the beginning of inspiration.

We may summarize in Table II. and Table III. the average results and the mean variations of the respiratory changes, and the rate of work per second during normal breathing and the medium degree of the six primary emotions. Laughter and hatred are tabulated separately (Table III.).

Table I

1. Normal

	I./E.	Depth	Work per Sec. in Units Where 1 Unit Equals 1/20 of an Inch
Subject A. F.	.74	12.7	5.18
" F. M.	.81	14.24	4.73
" M. M.	.83	14.2	4.08
" D. L. E.	.79	11.7	4.09
" L. B.	.75	15.	5.76
" I. M.	.91	15.5	4.20
Average	.805	13.89	4.67

2. During Disgust

	I./E.	Depth	Work per Sec.
Subject A. F.	.85	7.9	4.44
" F. M.	1.24	6.7	4.21
" M. M.	.66	17.	5.70
" D. L. E.	1.07	21.9	5.01
" L. B.	.75	24.2	9.75
" I. M.	1.93	14.2	6.48
Average	1.08	15.31	5.93

3. During Wonder

	I./E.	Depth	Work per Sec.
Subject A. F.	3.5	47.	7.66
" F. M.	2.45	40.5	6.15
" M. M.	3.	34.	6.85
" D. L. E.	2.25	33.	4.19
" L. B.	1.64	45.	8.68
" I. M.	2.14	32.	4.79
Average	2.49	38.58	6.39

4. During Pain

	I./E.	Depth	Work per Sec.
Subject A. F.	2.	26.5	8.16
" F. M.	1.7	22.4	3.74
" M. M.	1.12	29.5	5.70
" D. L. E.	1.71	37.7	6.92
" L. B.	.78	27.8	14.62
" I. M.	1.97	34.	9.03
Average	1.546	29.65	8.028

5. During Pleasure

	I./E.	Depth	Work per Sec.
Subject A. F.	1.05	24.	9.42
" F. M.	1.33	30.1	9.85
" M. M.	1.10	24.1	6.15
" D. L. E.	1.26	34.2	7.56
" L. B.	1.20	21.8	12.33
" I. M.	.73	29.4	11.47
Average	1.11	27.26	9.465

6. During Anger

	I./E.	Depth	Work per Sec.
Subject A. F.	1.87	36.2	17.32
" F. M.	1.36	25.5	10.82
" M. M.	.84	32.3	16.69
" D. L. E.	1.72	38.1	15.13
" L. B.	.52	18.1	9.41
" I. M.	2.57	40.5	10.43
Average	1.48	31.78	13.30

7. During Fear

	I./E.	Depth	Work per Sec.
Subject A. F.	3.31	22.3	15.68
" F. M.	2.19	17.15	10.14
" M. M.	2.55	41.8	23.20
" D. L. E.	3.93	25.5	10.58
" L. B.	1.59	52.	13.70
" I. M.	2.39	39.8	14.12
Average	2.66	33.19	14.57

8. During Laughter

	I./E.	Depth	Work per Sec.
Subject A. F.	.22	34.	11.58
" F. M.	.39	22.6	7.31
" M. M.	.34	21.65	11.66
" D. L. E.	.22	30.6	9.
" L. B.	.36	20.	4.47
" I. M.	.27	30.5	6.47
Average	.30	26.55	8.415

9. During Hatred

	I./E.	Depth	Work per Sec.
Subject A. F.	.275	35.	4.67
" F. M.	.745	45.25	9.28
" M. M.	.99	30.08	16.25
" D. L. E.	.45	37.8	9.6
" L. B.	.49	10.4	.88
" I. M.	.14	12.	4.095
Average	.515	28.42	7.462

Table II

	I./E. (in Units)	Depth (in Units)	Rate of Work per Sec. (1 Unit = 1/20 of an Inch)
Normal			
av.	.805	13.89	4.67
m.v.	.045	2.	.55
Disgust			
av.	1.08	15.31	5.93
m.v.	.33	5.71	1.45
Wonder			
av.	2.49	38.58	6.39
m.v.	.50	5.58	1.34
Pain			
av.	1.546	29.65	8.028
m.v.	.398	4.14	2.57
Pleasure			
av.	1.11	27.26	9.465
m.v.	.15	3.96	1.75
Anger			
av.	1.48	31.78	13.30
m.v.	.57	6.65	3.08
Fear			
av.	2.66	33.19	14.57
m.v.	.64	11.44	3.24

Table III

	I./E. (in Units)	Depth (in Units)	Rate of Work per Sec. (1 Unit = 1/20 of an Inch)
Laughter			
av.	.30	26.55	8.415
m.v.	.06	5.14	2.33
Hate			
av.	.515	28.42	7.462
m.v.	.24	11.48	4.25

The above record gives I. : E. : : 4 : 5(.805) during normal breathing. This shows rather clear results when we consider that the mean variation is .045; and that the ratios obtained by various investigators vary from 2 : 3, 3 : 4, 4 : 5, or 6 : 7. (That is the time for inspiration is less than the time for expiration.) The average depth during normal breathing is 13.89 (m.v. 2.–); and the rate of work per second is 4.67 units (m.v. .55). Fig. 1 gives A. F.'s and L. B.'s breathing curve during normal respiration. In the latter, the first five curves were measured; in the former, the last five curves.

In disgust the average ratio is 1.08 (m.v. .33). Subjects A. F., M. M., L. B. give less time for inspiration than expiration, while the other three subjects give more time for inspiration than for expiration. The average depth is 15.31 (m.v. 5.71). In order to explain this irregularity, we will introduce

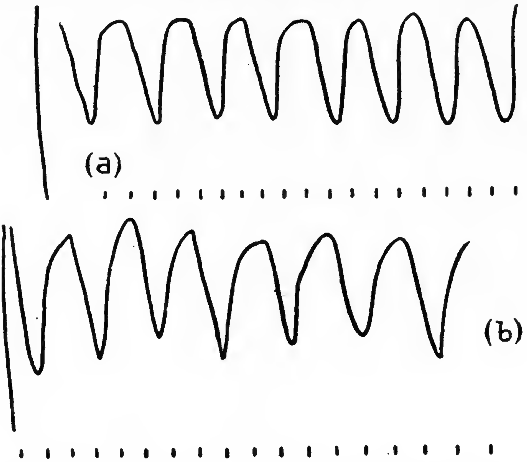

Fig. 1. (*a*) Subj. A. F.—I. : E. = .74 units; depth, 12.7;[1] rate of work per sec. 5.18 units. (*b*) Subj. L. B.—I. : E. = .75 units; depth, 15.–; rate of work per sec. 5.76 units.

the breathing curves as shown in Fig. 2. It seems as though the subjects A. F. and F. M. inhibit their breathing to a much greater extent than the others. The nose is very largely a respiratory organ; and in disgust is often slightly contracted as partly to close the passage (Bell; Darwin). Then again the terms which express immediate personal attraction or repulsion are derived for the most part from the sense of smell and taste. To loathe is much the same as to be nauseated at something. Disgust is a strong term for personal repugnance, and even in its objective manifestation centers about the curl of the nostrils and of the mouth.[2] Then there

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[1] We can in calculations convert 12.7 units (depth) into amount of air breathed in, which, in the case of A. F. is 216.44 c.c. These figures were obtained by breathing into a standard water spirometer at various levels, not exceeding 40 c.i., while a pneumograph was strapped around the chest, and a record was obtained upon a smoked surface of a drum. These records were then measured by means of the unit paper. We thus calculated that one unit equals 1.04 c.i. or 17.9425 c.c. This is the average of 96 trials with levels ranging from 10 to 40 c.i. A. F.'s vital capacity was found to be 143 c.i. or 2,343.34 c.c.

[2] 'Psy.,' Dewey, p. 257.

are various ways of exhibiting disgust, as shown by Darwin.[1] If the guttural sound 'ugh' is employed, as in the case of A. F. and F. M., the breathing is inhibited, and thus the

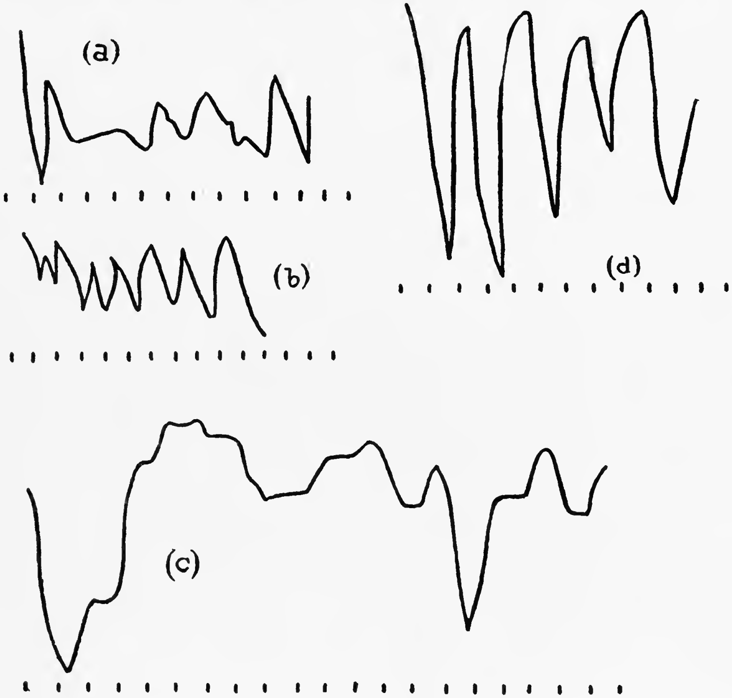

Fig. 2. (*a*) Subj. A. F.—I. : E. = −.85 units; depth, 7.9; rate of work per sec. 4.44 units. (*b*) Subj. F. M.—I. : E. = 1.24 units; depth, 6.7; rate of work per sec. 4.21 units. (*c*) Subj. M. M.—I. : E. = .66 units; depth, 17.–; rate of work per sec. 5.70 units. (*d*) Subj. L. B.—I. : E. = .75 units; depth, 24.2; rate of work per sec. 9.75 units.

rate of work. M. M. gives first a deep inspiration followed by a long irregular expiration and then inhibited respiration. A breathing curve similar to this, but a little more regular is given in Plate II.; and seems to be a typical curve for the 'emotion of disgust.' That is, there is first a quick deep inspiration and then expiration followed by inhibited respiration.

Fig. 3 gives the characteristic breathing curve of 'wonder' with its decided inspiratory pause. The average ratio for the six subjects is 2.40 (inspiratory pause is included as

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[1] 'The Expression of the Emotions in Man and Animals,' C. Darwin, p. 258.

belonging to inspiration), with a m.v. −.50. This ratio does not differ greatly from the av. ratio of fear 2.60 (av. m.v. −.64) as shown in the records and in Fig. 4. In 'fear,' we find a gradual lengthening of the curve in some cases, while in others there is a slight inspiratory pause. L. B.'s fear breathing curve is somewhat similar to her 'wonder' breathing curve as shown in Fig. 3, with the difference that 'wonder' has the greater inspiratory pause. Fear with a decided inspiratory pause is a complex emotion which may

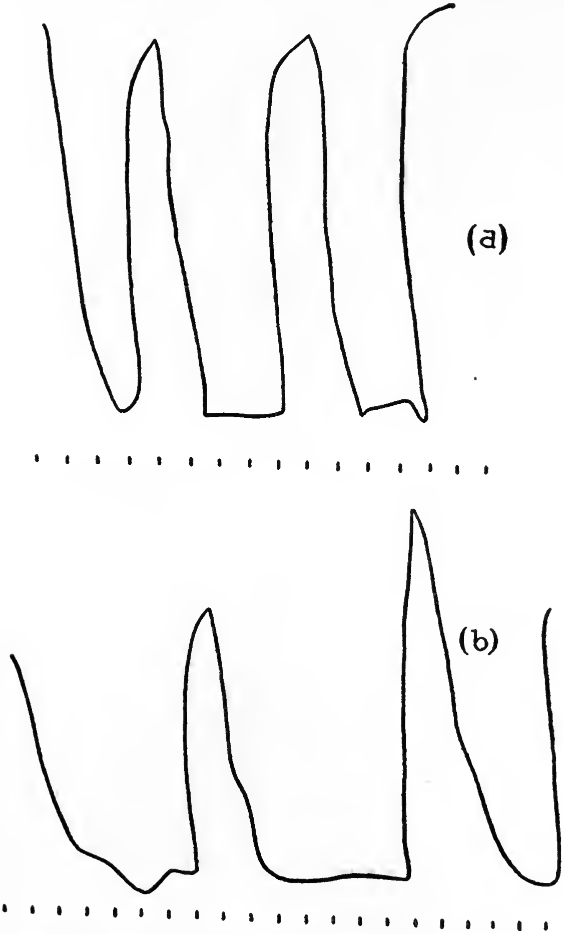

Fig. 3. Breathing Curve During Wonder. (*a*) Subj. L. B.—I. : E. = −1.64 units; depth 45 units; rate of work per sec. 8.68 units. (*b*) Subj. D. L'E.—I. : E. = −2.25 units; depth 33 units; rate of work per sec. 4.19 units.

be either fear and shock, fear and wonder, or fear and surprise. (The breathing curve of 'surprise' is similar to that of wonder.) However, the rate of work differs in the two emotions. There is much less respiratory work done during 'wonder' than during 'fear,' but if the inspiratory pause is introduced into the 'fear' breathing curve, the rate of work per sec. will be equivalent to that of 'wonder.'

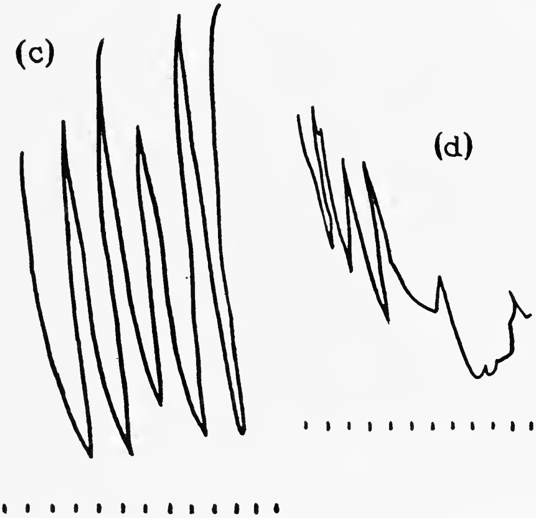

Fig. 4. Breathing Curve During Fear. (*c*) Subj. M. M.—I. : E. = 2.55 units; depth 41.8 units; rate of work per sec. 23.20 units. (*d*) Subj. F. M.—I. : E. = 2.19 units; depth 17.15 units; rate of work per sec. 10.14 units.

In 'pain,' the average ratio is 1.546 (m.v. −.398). Five subjects give more time for inspiration than expiration, while one subject gives less time. Subj. F. M. does 3.74 units of work per sec., while L. B. does 14.62 units per sec. The rate of work depends not only upon the amplitude, but also upon the respiratory pause. Fig. 5 shows the inspiratory pause in F. M.'s pain breathing curve which lessens the rate of work per sec. I. M.'s curve shows five respirations in 21 sec., while L. B.'s curve shows five respirations in about 10 sec. Thus it seems that the rate of work also depends upon the rapidity of the breathing phases.

In "pleasure" the average ratio is 1.11 (m.v. −.15).

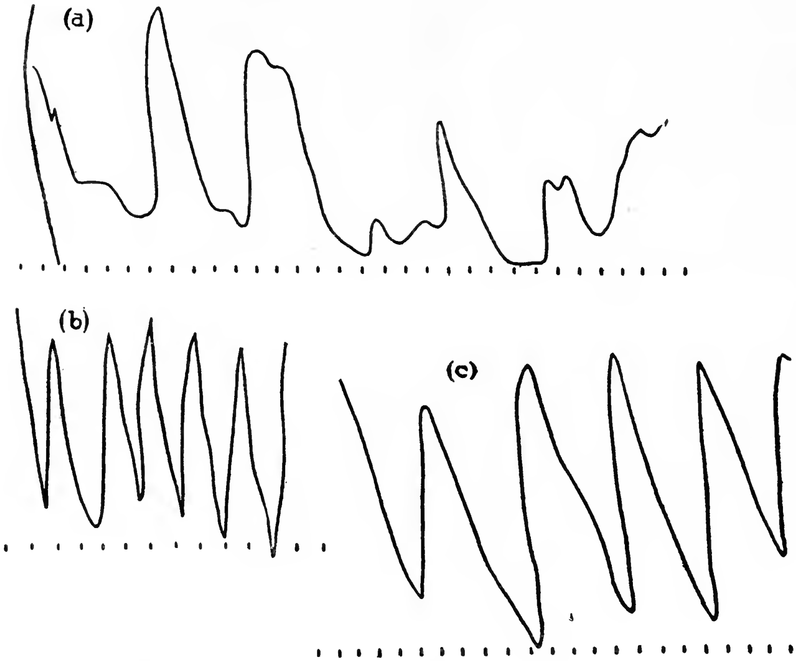

Fig. 5. Breathing Curve During the Emotion of 'Pain.' (*a*) Subj. F. M.—I. : E. = −1.7; depth 22.4; rate of work per sec. 3.74 units. (*b*) Subj. L. B.—I. : E. = −.78; depth 27.8; rate of work per sec. 14.62 units. (*c*) Subj. I. M.—I. : E. = −1.97; depth 34.–; rate of work per sec. 9.03 units.

Fig. 6. Breathing Curve During the Emotion of Pleasure. (*a*) Subj. D. L'E.—I. : E. = 1.26; depth = 34.2; rate of work per sec. = 7.55. (*b*) Subj. L. B.—I. : E. = 1.20; depth = 29.4; rate of work per sec. = 12.33.

As in pain, five subjects give more time to inspiration than to expiration, while subject I. M. gives more time to expiration. Fig. 6 shows D. L'E.'s curve: five respirations in about 15 seconds (rate of work per sec. 7.56); and L. B.'s curve: five respirations in about 9 seconds (rate of work per sec. 12.33).

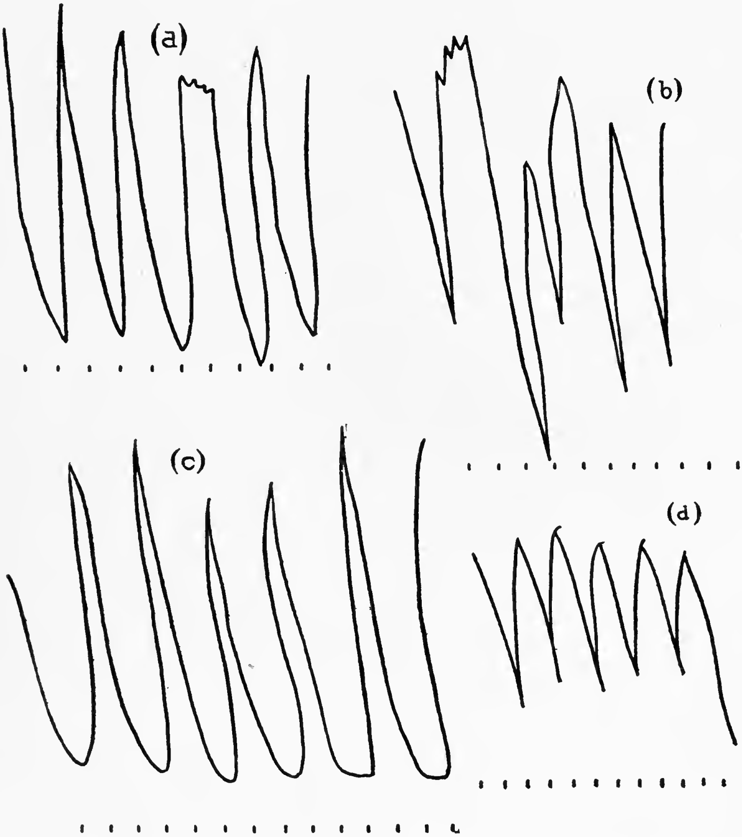

FIG. 7. Breathing Curve During the Emotion of Anger. (*a*) Subj. A. F.—I. : E. = 1.87; depth = 36.2; work per sec. = 17.32 units. (*b*) Subj. M. M.—I. : E. = .84; depth = 32.3; work per sec. = 10.82 units. (*c*) Subj. D. L'E.—I. : E. = 1.72; depth = 38.1; work per sec. = 15.13 units. (*d*) Subj. L. B.—I. : E. = .52; depth = 18.1; work per sec. = 9.41 units.

In 'anger' four subjects give more time to inspiration, while two subjects give more time to expiration. Av. ratio = 1.48 (m.v. −.57). The rate of work varies from 9.41 per sec. to 17.32 units per sec. This irregularity is caused by a slight inspiratory or expiratory pause due to swallowing or clenching the fists; to lack of uniformity in the amplitude; and to the breath rate, as shown in the records and in Fig. 7.

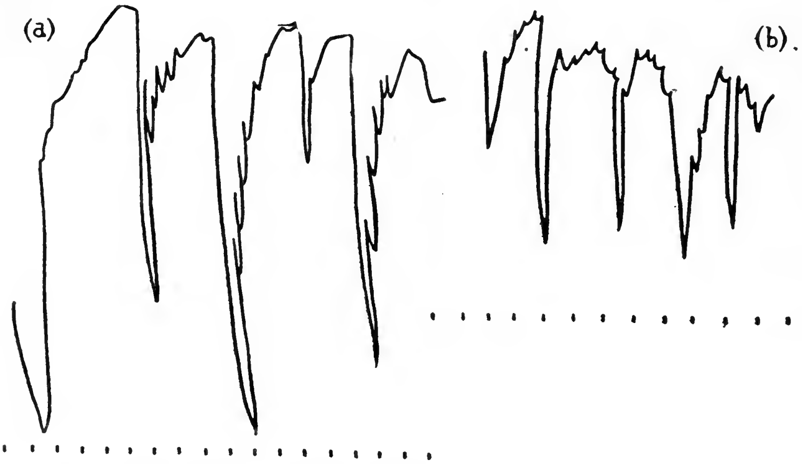

FIG. 8. Breathing Curve During the Emotion of Laughter. (*a*) Subj. D. L'E.—I. : E. = .22; depth = 30.6 units; rate of work per sec. = 9.- units. (*b*) Subj. M. M.—I. : E. = .34; depth = 21.65 units; rate of work per sec. = 11.66 units.

Both laughter and hatred have characteristic breathing curves as shown in Fig. 8 and Fig. 9, as well as in Plates IV. and V. In laughter I. : E. = −.30 units (m.v. −.06); and in hatred I. : E. = −.515 (m.v. −.24). It seems that the subjects were a little more successful in the expression of laughter than in the expression of hatred. However, both show a decided respiratory pause.

SUMMARY

In normal breathing, the average time of the inspiration as compared with that of expiration is about as 4 : 5 (.805). That is, the time for inspiration is less than the time for expiration. This is also true for laughter (.30) and hatred (.515). In disgust (1.08); pleasure (1.11); anger (1.48); pain (1.546); wonder (2.49); and fear (2.66), the average time for inspiration is greater than the time for expiration.

When we compare the average rate of respiratory work per second for the six subjects with each individual record, we find great variability, due, perhaps, to the lack of uniform stimuli, to the lack of uniform intensity; as well as individual

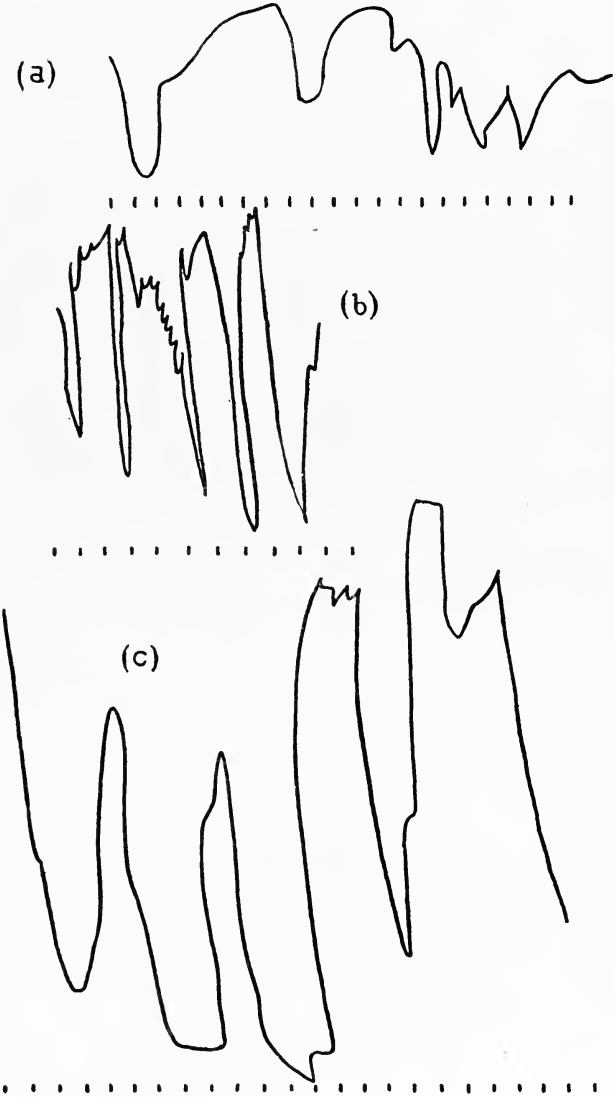

FIG. 9. Breathing Curve During the Emotion of Hate. (*a*) Subj. L. B.—I. : E. = .49 units; depth = 10.4 units; work per sec. = .88 units. (*b*) Subj. M. M.—I. : E. = .99 units; depth = 30.08 units; work per sec. = 16.25 units. (*c*) Sub D. L'E.—I. : E. = .45 units; depth = 37.80 units; work per sec. = 9.60 units.

differences. Although irregularities are found, there seems to be a tendency to do much less work in wonder and disgust than in anger and fear; and less in pain than in pleasure. These facts do not contradict the data of ordinary observation. James has pointed out that 'fear has bodily expressions of an energetic kind, and stands, beside lust and anger, as one of the three most exciting emotions of which our nature is susceptible.'[1] In wonder, we have an inspiratory pause which lessens the rate of work per sec.; while in hate we have the expiratory pause which also lessens the rate of work per sec. In disgust, the rate of work depends upon the depth of the first inspiration followed by an expiration, and the great inhibition of the following respirations. It is interesting to note that pleasure and pain appear side by side in order of work done per second.

Check No. 1.—As great irregularities were found in the depth of the curve, we have arbitrarily calculated the rate of work per second with an amplitude of one unit. Tables IV. and V. allow a comparison of results for the six subjects in order of respiratory work done per sec. with the original average, and when calculated with a depth of one unit.

TABLE IV

		Av. Depth	Av. Work per Sec.
1	Fear	33.19	14.57
2	Anger	31.78	13.30
3	Pleasure	27.26	9.465
	(Laughter)	(26.55)	(8.42)
4	Pain	29.65	8.028
	(Hate)	(28.42)	(7.46)
5	Wonder	38.58	6.39
6	Disgust	15.31	5.93
7	Normal	13.89	4.67

Thus we find more work accomplished during 'fear' and 'anger' than during 'wonder' and 'disgust'; and more work done during pleasure than during pain. However, the 'normal' and 'disgust' rates have changed positions. The 'normal' holds the seventh rank in Table IV., while it has

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[1] Wm. James, "Principles of Psychology," Vol. II., p. 415.

the fifth position in Table V. Disgust, which has the sixth position in Table IV., occupies the third position in Table V. All that we can say is that when we calculate the rate of work with an amplitude of one unit, we ignore the form of the curve. Both the 'normal' and 'disgust' breathing curves possess characteristic forms, as may be seen in Figs. 1 and 4, as well as in Plate II.

TABLE V

	Depth Taken Arbitrarily	Rate of Work per Sec.
Fear	1 unit	.4389 units
Anger	1 "	.4185 "
Disgust	1 "	.3873 "
Pleasure	1 "	.3475 "
Normal	1 "	.3363 "
(Laughter)	(1 unit)	(.3167)
Pain	1 "	.2707 "
(Hate)	(1 ")	(.2625)
Wonder	1 "	.1656 "

The 'fear,' 'anger,' 'pleasure,' 'pain,' breathing curves have similar forms, and for that reason did not change their positions in order of work done per second, but were merely displaced by the 'normal' and 'disgust' breathing curves. This also holds true for 'wonder,' 'hate,' and 'laughter' which are similar in respect to having either an inspiratory or expiratory pause which lessens the rate of work per second.

Check No. 2.—In order to partly check up these results, we had A. F. pose before the camera, while the breathing movements were recorded on the revolving drum of Porter's kymograph and the time recorded by Jacquet's chronometer.

The directions given were the same as those mentioned in experiment: "Look at a stated point, then look quickly into the camera and express the various degrees of (1) pleasure culminating in laughter, (2) anger culminating in hatred, and the various degrees of disgust, pain, wonder and fear." As soon as A. F. heard the second click of the camera, the second degree was assumed. The following photographs, breathing curves, and figures speak for themselves much better than any discussion might do.

Unfortunately all the scales are not as perfect as desired, due to fatigue and other disturbances. The error of execution may also be due 'partly to an error of movement;[1] and partly also to an error in the intention'[2] (quoted from 'Elements of Exper. Phonetics,' Edw. W. Scripture, p. 202). Nevertheless, we note that, in the medium degree, more work is done during fear and anger than during pleasure and pain or 'wonder' and 'disgust.' However, although pleasure and pain appear side by side in order of work done per sec., more work is done during 'pain' than during 'pleasure'; and more work is done during disgust than during wonder. These facts do not only contradict our former statement in regard to order of work done per second, but also contradict A. F.'s own record, as may be seen in the following tables.

TABLE VI

SUBJECT A. F. WITH PHOTOGRAPHS (MEDIUM DEGREE)

	I./E.	Depth	Rate of Work per Sec.
Photo 40, Wonder	2.6	10.–	1.77
" 46, Disgust	.63	11.54	7.74
" 58, Pleasure	.93	22.16	10.08
" 52, Pain	1.27	23.1	12.06
" 64, Anger	1.19	22.25	13.68
" 70, Fear	2.16	23.88	14.97

TABLE VII

SUBJECT A. F. WITHOUT PHOTOGRAPHS (MEDIUM DEGREE—SEE ALSO TABLE I)

	I./E.	Depth	Rate of Work per Sec.
Disgust	.85	7.90	4.44
Wonder	3.50	47.–	7.66
Pain	2.–	26.50	8.16
Pleasure	1.05	24.–	9.42
Anger	1.87	36.20	17.32
Fear	3.31	22.30	15.68

It is self-evident that a 'wonder' breathing curve which gives I. : E. = 2.6 and depth of 10 units will give different results when calculating the respiratory work done per sec.,

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[1] Fullerton and Cattell, 'On the Perception of Small Differences,' 65, Philadelphia, 1802.

[2] Woodworth, 'Accuracy of Voluntary Movement,' *Psy. Rev.* Monogr. Suppl. III., No. 2, 71.

than the ‘wonder’ breathing curve which gives I. : E. = 3.50 units and a depth of 47 units. The same may be said of the breathing curve of ‘disgust.’ All this merely emphasizes the fact that we may expect to find as great a mean variation around the subject’s own average, as around the average of a great number of observers. We may, therefore, for the present, consider our first conclusions as valid. (See Tables II. and IV.) And, as there is uncertainty and lack of uniformity is there need of further experiments, with many trials for each subject.

It is interesting to note that in the small space, known as the face, we find assembled almost all the senses of the body, for the special senses are certainly present. Without words we may read the emotions of wonder, disgust, pleasure, pain, anger and fear. We also see the natural association between the muscles of respiration and that of the face (muscles of expression), for the mouth and nose are the respiratory organs (air passages) which carry the air to the closed cavity, known as the chest. We may safely say with Bell and Mosso thae the emotions are principally reflected in the muscles of the face and that of respiration. The various emotions have characteristic expressions, and thus naturally we should expect corresponding changes in the respiratory apparatus.

PLATE I

37 38 39 40 41 42

	I./E.	Depth	Work per Sec.
Attention (37)	1.25	19.5	9.74
Wonder (38)	3.55	20.–	6.3
“ (39)	20.33	25.	4.98
“ (40)	2.6	10	1.77
“ (41)	6.8	21.–	3.24
“ (42)	6.44	27.–	4.83

PLATE II

43 44 45 46 47 48

	I./E.	Depth	Rate of Work per Sec.
Attention (43)	1.01	18.5	11.81
Disgust (44)	.79	15.3	8.79
“ (45)	.67	13.	8.03
“ (46)	.63	11.54	7.74
“ (47)	.44	15.58	6.93
“ (48)	.08	31.5	5.55

PLATE I.

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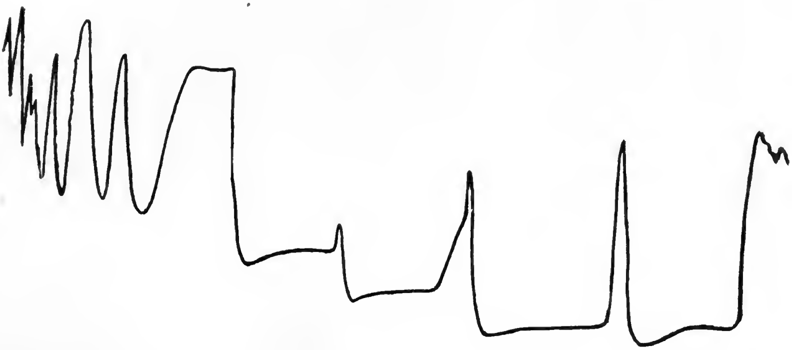

PLATE 2.

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PLATE 3.

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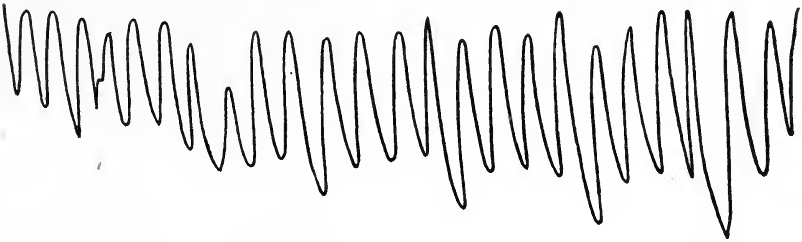

PLATE 4.


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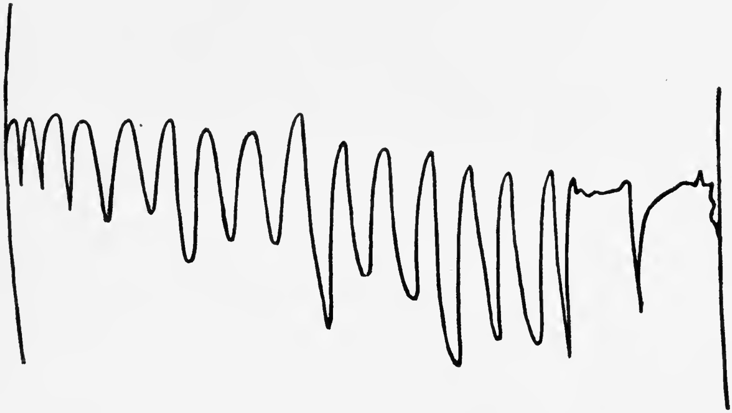

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PLATE 15.


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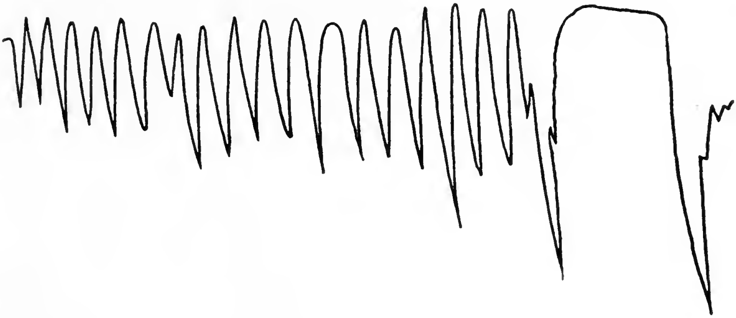

PLATE 6.


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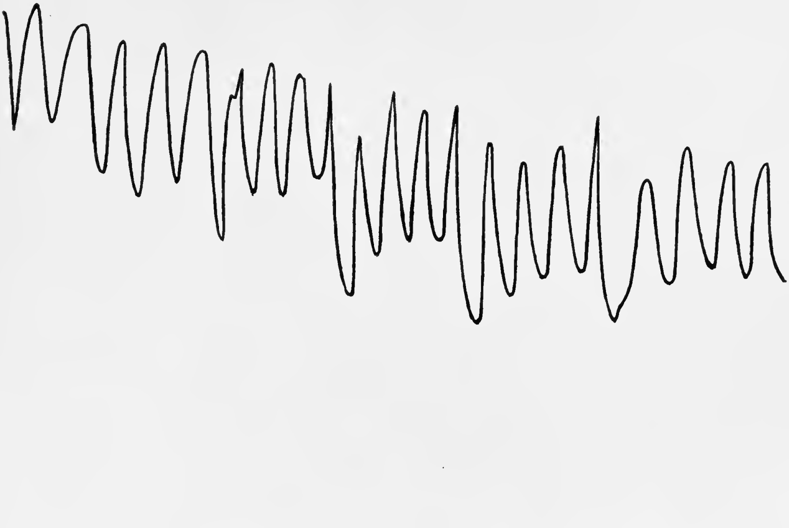

PLATE III

49 50 51 52 53 54

	I./E.	Depth	Rate of Work per Sec.
Attention (49)	.93	15.87	9.19
Pain (50)	.89	17.63	10.19
“ (51)	1.40	21.25	10.56
“ (52)	1.27	23.1	12.06
“ (53)	1.08	24.63	13.17
“ (54)	1.06	25.33	11.46

PLATE IV

55 56 57 58 59 60

	I./E.	Depth	Rate of Work per Sec.
Attention (55)	1.–	11.–	5.50
Pleasure (56)	1.08	12.–	5.58
“ (57)	1.28	16.–	7.27
“ (58)	.93	22.16	10.08
“ (59)	.94	23.66	10.81
Laughter (60)	.18	19.75	4.98

PLATE V

61 62 63 64 65 66

	I./E.	Depth	Rate of Work per Sec.
Attention (61)	1.50	16.75	10.70
Anger (62)	1.34	15.16	10.48
“ (63)	1.32	19.12	12.39
“ (64)	1.19	22.25	13.68
“ (65)	1.12	28.–	18.66
Hatred (66)	.19	41.5	5.64

PLATE VI

67 68 69 70 71 72

	I./E.	Depth	Rate of Work per Sec.
Attention (67)	.73	17.75	8.62
Fear 1° (68)	1.43	23.	11.87
“ 2° (69)	2.12	23.63	14.45
“ 3° (70)	2.16	23.88	14.97
“ 4° (71)	2.26	24.25	13.47
“ 5° (72)	1.69	21.375	10.16

A GRADED SERIES OF COLORED PICTURE PUZZLES

BY GRACE HELEN KENT

This test is offered as the second unit of a system of non-verbal tests, the purpose of which has already been stated.[1]

The picture puzzle is well adapted to the general requirements, because it is a game which appeals strongly to persons of all ages. The interest which many adults manifest in difficult jig-saw puzzles is well known. I have seen a boy of three years work very diligently to solve a puzzle made from a highly colored picture, and in the series of tests here reported I have used two subjects only five years of age.

I have found picture puzzles useful in the study of insane subjects, especially advanced cases of dementia præcox. The puzzles had not been standardized, and were used as a means of gaining the coöperation of unresponsive patients or as a reward for the performance of a less attractive task rather than for purposes of systematic observation.

In the spring of 1915 I attempted to use picture puzzles in the study of defective children, but with little success. The puzzles which had been so popular among insane patients proved to be far too difficult for children of low grade intelligence, and I was unable to find pictures from which puzzles sufficiently simple could be made. The difficulty of obtaining suitable pictures is a surprisingly serious objection to the use of picture puzzles as a test method.

The test may be made indefinitely difficult, either by cutting the picture into many small pieces or by imposing a time limit. But it cannot be made indefinitely easy without increasing the possibility of solution by chance. If, therefore, a series of puzzles is to be graded in difficulty, the plan of construction should be formulated with reference to the lower end of the scale.

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[1] 'A Graded Series of Geometrical Puzzles,' JOURNAL OF EXPERIMENTAL PSYCHOLOGY, Vol. I., p. 40.

In the course of many preliminary experiments with puzzles more or less unsuitable, I have found that the following conditions are favorable to prompt solution:

1. The coloring of the picture should be moderately strong. There should be sharp contrasts and clear-cut lines, mainly diagonal.

2. The subject of the picture should be simple, preferably a single figure which contrasts strongly with a comparatively plain background. It should represent something that a very young child will instantly recognize. The ideal picture for the simplest puzzles would be a human face, a human figure in ordinary clothing, a cat, a dog, or a horse.

3. Each piece of the puzzle should be of such size as to be easily handled.

I have not been able to find pictures that satisfy these requirements. Postal cards, unfortunately, are much too small to be serviceable. I have spent many days in New York art shops, and have looked over toy picture books by the score, but have failed to find a single picture perfectly adapted to the purpose. The most satisfactory pictures I have seen were taken from advertisement calendars and cannot be duplicated. It will probably be necessary, before the test can be perfected, to have pictures made for the purpose; but much further experimentation will be needed before suitable pictures can be designed. For the present it seems best to use inexpensive pictures, so as to make the test available to any one who may care to give it a trial.

This series is made wholly from pictures sold by one or both of the following companies: The Perry Pictures Company, Malden, Mass.; and G. P. Brown and Company, Beverly, Mass. I visited the headquarters of both companies, so as to have an opportunity to examine every colored picture on their lists, and selected about one hundred pictures which possessed no very obvious defects. The more unsuitable of these have been eliminated by two independent series of tests[1] and the number has been reduced to twenty.

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[1] In making the preliminary tests I have been assisted by Celia H. Sheldon, and I gratefully acknowledge my indebtedness to her.

Few of them represent scenes familiar to the average American child, and they are far from satisfactory. They are offered mainly on the ground that they are obtainable in any quantity at a cost of two or three cents apiece.

Each puzzle is made by cutting the picture or pictures into horizontal strips. In this system of tests it is my aim to keep different variables separate, as far as possible, and to deal with one at a time. Irregularities of form have no place in this test, which is primarily a test of color matching. Apart from theoretical considerations, I have empirical evidence that such irregularities tend to make a picture puzzle more difficult as well as more complex. In my early experiments upon insane subjects I cut many pictures irregularly, in some cases following the lines of the picture; but I found that these puzzles required more time for solution than those which were cut into plain strips or blocks.

The pictures are of approximately the same size, about fifteen by twenty centimeters. The width of the strips varies for different puzzles, but all the strips of a given puzzle are of equal width. The pictures should be mounted on moderately heavy cardboard. This may be done very easily by laying a sheet of rubber tissue between the picture and the cardboard and then passing a warm flatiron over the picture, protecting it from direct contact by a sheet of tissue paper. All the borders of the pictures should be trimmed. Detailed instructions for cutting each picture are given in the table.

The more difficult puzzles consist of two or three similar pictures, but any of these may be used singly if desired. The simplest puzzles of the series consist of three pieces each. The three two-piece puzzles are not intended for general use, and for this reason they are not numbered as a part of the series. They may be given to a subject of low grade intelligence who is unable to solve any of the three-piece puzzles. In view of the very considerable possibility of solution by chance, the subject should be required to solve all three of the two-piece puzzles in order to receive credit for any.

Because of the unfamiliarity of the scenes represented in the pictures, it is well to show the subject a whole picture before giving the puzzle pieces. I do not require the subject to look at the picture for a specified number of seconds, but am careful to be sure that he gives attention to it. The pieces are presented in a pile, each piece with the picture side up but with about half the pieces reversed end for end. The subject is thus required to turn some pieces around, but not to turn them over. The pieces should be mixed with some care and then they may be numbered on the back, so that they can be given to all subjects in the same order. The time required for the solution of a puzzle is measured by a stop watch and recorded in seconds.

Number of Puzzle	Name of Picture	Catalogue Number, Perry Pictures Co., Malden, Mass.	Catalogue Number, G. P. Brown & Co., Beverly, Mass.	Number of Strips	Distance of First Cut from Top of Picture, in Millimeters	Width of Each Strip, in Centimeters	Time Records of 29 Subjects, in Seconds		
							Minimum	Median	Maximum
A	Napoleon	......	91	2	0	10			
B	Gopher	9,308	6,308	2	0	10			
C	European Squirrel	9,296	6,296	2	0	10			
1	Scarlet Ibis	9,322	......	3	15	6	3	6	12
2	Gray Rabbit	9,174	6,174	3	0	5	3	6	15
3	American Blue-Jay	9,011	6,011	3	5	6	3	7	27
4	See! The Conquering Hero Comes	......	13	4	0	5	5	10	35
5	Domestic Horse	9,494	6,494	3	0	5	6	17	64
	Domestic Cow	9,487	......	3	3	5			
6	Baby Stuart	......	4	3	10	6	6	16	104
	Miss Simplicity	......	14	3	20	6			
7	Zebra	9,456	6,456	4	0	4.5	11	23	94
	Giraffe	9,447	6,447	4	18	4.5			
8	Sandhill Crane	9,224	6,224	4	2	5	13	60	206
	Clapper Rail	9,307	6,307	4	2	5			
	Bartramian Sandpiper	9,147	6,147	4	2	5			
9	Forests	9,273	......	5	0	4	23	52	218
	The Three Birches	......	35	5	0	4			
10	Just a Little More	......	103	6	8	3	69	187	504
	Helping Mother	......	102	6	8	3			

The puzzles are numbered approximately in the order of difficulty, as indicated by the results of preliminary tests.

An additional series of tests has been made upon thirty-two subjects, but three of the records are omitted because of errors. The table shows the extreme and median time records obtained from twenty-nine subjects.

When the test is used for the study of individual cases it will probably be necessary to set an arbitrary time limit. For the convenience of the examiner, I recommend that one minute be allowed for each puzzle, regardless of its difficulty. This is a liberal allowance for the puzzles at the lower end of the scale, while it is short enough to insure that the last puzzle will be solved only by a subject of exceptional ability in this line of work. It is not necessary to interrupt the subject at the expiration of the time, nor to let him know that he has failed. It is better to give him a little assistance, so as to enable him to finish without undue loss of time.

A SPECTROSCOPIC APPARATUS FOR THE INVESTIGATION OF THE COLOR SENSITIVITY OF THE RETINA, CENTRAL AND PERIPHERAL

BY C. E. FERREE AND GERTRUDE RAND

Bryn Mawr College

The need has long been felt in physiological and psychological optics for better methods of specifying and standardizing the stimulus. In an adequate specification and standardization of the stimulus for work in these fields, three points are involved. Means must be had (1) of securing a small range of wave-lengths and of stimulating with them any part of the retina; (2) of determining accurately the values of the wave-lengths employed; and (3) of specifying the amounts of light used. In the present series of papers apparatus will be described with which it is, comparatively speaking, easily and conveniently possible (1) to stimulate any part of the retina with the light of the spectrum and to control as desired the conditions of preëxposure and surrounding field; (2) to control the amounts of light used within the small gradations needed for threshold and just noticeable difference determinations; and (3) to specify in C.G.S. units in any case in which it is wished the amount of light used. This work was begun and announced three years ago as the logical completion of work published at that time[1] and has

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[1] See Rand, G., 'The Factors that Influence the Sensitivity of the Retina to Color: A Quantitative Study and Methods of Standardizing,' *Psychol. Rev. Monog.*, 1913, *15*, No. 1, 166 pp.; and Ferree, C. E., and Rand, G., 'A Note on the Determination of the Retina's Sensitivity to Colored Light in Terms of Radiometric Units,' *Amer. Journ. of Psychol.*, 1912, XXIII., pp. 328–332.

If more time has been taken to accomplish what we have here to present than seems justifiable, we beg to point out that the work has been in many respects pioneer in character and that the difficulties on the radiometric side had not yet been overcome by the physicist at the time we began our work. We have also been greatly diverted by the pressure of other work.

The apparatus has been greatly simplified from its original construction. As it involved the construction of a new type of campimeter, spectroscope, apparatus for the regulation of the amount of light used applicable to the spectroscope employed, and radiometric apparatus non-selective in its response to wave-length and sufficiently sensitive for the purpose in hand. The apparatus included under the first three of the above classes was constructed under direction by our department mechanician and should be within the technical capabilities of any good college mechanician. The description of this apparatus will form the subject matter of the present paper. For the radiometric apparatus we are indebted to Dr. W. W. Coblentz, radiometric specialist for the Bureau of Standards. Believing that the sensitivity of the thermopile could be increased by enlarging the area of the receiving surface and wanting to measure for at least one color all the light energy falling on the stimulus-opening in our campimeter screen rather than attempt to calculate it from one or more linear elements in this surface, we took up with Dr. Coblentz the feasibility of constructing surface thermopiles. He was asked to construct for us a surface thermopile having a receiving surface 16 mm. square, a trifle larger than the opening in our campimeter screen.[1] By means of this thermopile and one of the linear type we have been able to measure the light at three places—at the analyzing slit, at the campimeter opening, and at the eye. The use that is made of these measurements in the color investigation will be stated in a later paper.

Description of Apparatus

For the sake of convenience the apparatus may be described under three headings: (*a*) an apparatus for getting

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now stands it is comparatively simple in form; it is not difficult to operate, and is, we believe, within the financial and technical possibilities of any laboratory in which serious work is being done in the optics of color. There is no work known to us in the investigation of color sensitivity in which light of a standard quality and intensity is needed to which this or a similar apparatus can not be adapted. On the spectroscopic side at least we hope it will prove sufficiently feasible and practicable to render undesirable the use of colored papers and filters in much of the work that is now being done in the optics of color.

[1] See Coblentz, W. W., 'Instruments and Methods Used in Radiometry'—II., *Bulletin of the Bureau of Standards*, 1912, IX., p. 22, ff.

the light stimulus of spectrum purity and of exposing different parts of the retina to this stimulus under any conditions of surrounding field, preëxposure, etc., that may be desired; (*b*) an apparatus for varying the intensity of the stimulus within the limits needed for threshold and j.n.d. work; and (*c*) an apparatus for measuring the intensity of the stimulus in terms of common or C.G.S. units.

1. An apparatus for getting a light stimulus of spectrum purity and of exposing the different parts of the retina to this stimulus under any conditions of surrounding field, preëxposure, etc., that may be desired.

To design an apparatus which will combine all the above features and which can besides be easily and conveniently operated is a task which has presented considerable difficulty.[1] Dreher[2] and Abney,[3] for example, have each described apparatus devised to accomplish a part of what is outlined above, but the apparatus of neither would at all serve the purpose that has impelled us to take up anew the problem of the construction of apparatus.

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[1] A statement of the points needed by an apparatus by means of which all the factors influencing the sensitivity of the retina to colored light may be controlled and which will have besides a wide range of serviceability is, in the belief of the writers, as follows. (1) It must provide an accurate means of separating out the desired ranges of wave-lengths throughout the spectrum. (2) Means must be provided for directing these wave-lengths to any part of the retina that is desired. (3) Provision must be had for controlling the brightness of the field surrounding the color and the brightness of the preëxposure. (4) The apparatus must be available for use in a light as well as in a dark room, else (*a*) the influence of the brightness of the surrounding field and of the preëxposure upon the response of the retina to the different colors can not be eliminated from the investigation when wanted; and (*b*) a large part of the work in the optics of color can not be done, namely, the investigations of response under different degrees of illumination. (5) A method of presenting the light to the eye must be had which will give the effect of a surface or field variable in shape and size that may be imaged on the retina. (6) The method of presentation must be such as to allow of no admixture of light from the room with the range of wave-lengths needed for the stimulus, and it must be as little as possible wasteful of light, else a sufficient range of intensity can not be had. (7) A beam of colored light should be provided of such intensity that its energy value per unit of cross section can be specified when desired at the point of entrance of the light into the eye.

[2] Dreher, E., 'Methodische Untersuchung der Farbentonänderungen homogener Lichter bei zunehmend indirektem Sehen und veränderter Intensität,' *Z. f. Sinnesphysiol.*, 1912, *46*, pp. 1–82.

[3] Abney, W. deW., 'The Sensitiveness of the Retina to Light and Color,' *Philos. Trans. of the Roy. Soc. of London*, 1897, Ser. A, *190*, pp. 155–195.

A schematic representation of the apparatus under this heading is shown in the diagram in Fig. 1. It consists in

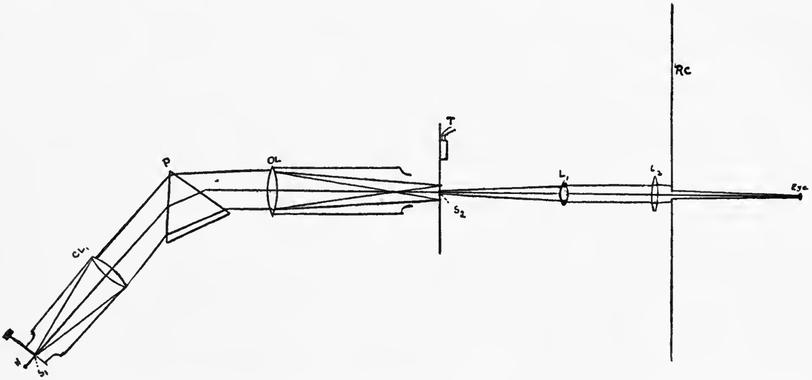

Fig. 1. Showing the Path of the Light from the Source to the Eye.

general of light source (*N*), spectroscope (*Sp*), micrometer slit (S_2) for separating out the wave-lengths needed for the colored stimulus, rotary campimeter (*RC*), collimating lens (L_1) for rendering parallel the beam of light emerging from the analyzing slit (S_2), and focusing lens (L_2) for bringing this beam to a focus on the pupil and for shifting the image formed to follow the pupil as the eye takes fixation at different degrees of excentricity.

It is scarcely needful to call to mind that one of the chief difficulties in devising a peripheral vision spectroscope has been to secure a means of throwing the colored light on the excentric portions of the retina. Dreher, for example, has tried to do this with limited success by holding the eye stationary and by reflecting the light to the different portions of the retina by a system of mirrors. By this means so much light is lost by reflection that the device would be useless for our purpose because we wish to be able to radiometer the light which finally reaches the eye. Moreover the difficulty in making the adjustments and the impossibility of making fine adjustments even for the different portions of the retina in the same meridian render the apparatus practically useless for extended work. Also the ordinary campimeter precautions with regard to the control of the brightness of the surrounding field and preëxposure can not be taken nor can the apparatus be used in an illuminated room without the admixture of a great deal of light from the room with the wave-lengths selected for the stimulus. So far as we know, no attempt has yet been made to adapt the spectroscope to the campimeter, at least not in such a way that any meridian of the retina can be worked in easily and conveniently from center to periphery and an accurate control be had at every step of the brightness of the field surrounding the stimulus and the preëxposure.[1]

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[1] The hollow hemisphere, for example, used by Abney can scarcely be considered a campimeter for it was not designed for exercising any control over the brightness of the surrounding field. The apparatus was designed solely for dark-room work and was called by him a perimeter. The practical impossibility of uniformly controlling the brightness of the field surrounding the stimulus by means of an opaque campimeter screen hemispherical in shape was shown by one of the writers (Ferree) in a previous article. ('A Note on the Rotary Campimeter,' *Psych. Rev.*, 1913, *20*, pp. 373–378.)

Abney describes two devices for exposing the peripheral retina to the light of the spectrum. Both are for use in the dark room. The first consists of a perimeter, the graduated arm of which is painted white, and the following auxiliary apparatus. Below the eye is placed "a small mirror on a ball and socket joint which by means of an arm causes a beam of light falling on it to be cast in any direction that is desired." The monochromatic light for the stimulus is gotten by means of an instrument which he calls his "color patch apparatus." The light passes from this apparatus to the mirror on the ball and socket joint which reflects it to the different points on the arm of the perimeter. In this way patches of color which serve as the stimuli for the eye in the color investigation are formed at different degrees of excentricity on the perimeter arm. That is, with regard to the method of securing the stimulation of the peripheral retina, this apparatus is of the usual type. The eye is held stationary and the retina is stimulated from center to periphery in a given direction by causing the colored stimulus to pass from the center to the periphery of the field of vision in the opposite direction. In his second apparatus, also called a perimeter by him, a hollow white hemisphere made of papier mâché is employed instead of a rotating arm. In the center of this surface is a circular aperture 1½ in. in diameter to admit the stimulus light. To give the effect of a colored surface to this aperture when illuminated from behind with colored light, it was filled in with glass ground on both sides. In order that the shape and size of the stimulus may be varied, additional special apertures are provided which are placed as desired in the path of light immediately behind the fundamental aperture. As before, the color patch apparatus is used as the source of the stimulus light. The method of securing the excentric stimulation of the retina, however, is just the reverse of that described before. That is, the colored stimulus is kept in a fixed position and the eye follows a phosphorescent point which is moved from the center to the periphery of the hemisphere in the meridian in which the investigation is being made.

Neither of Abney's devices would have answered our purposes for the following reasons. (1) Neither is adapted for use in a light room. (2) Provisions for the

One of the difficulties encountered in attempting to do this is that when the eye is turned to take an excentric fixation, the pupil is rotated out of the beam of light. To overcome this, we have taken advantage of the simple fact that when the axis of a double convex lens is displaced from the axis of the beam of light, the image of the light is displaced in the same direction in which the lens is moved. Therefore, by moving the focusing lens (L_2) an appropriate amount in the direction the eye is moved in taking its excentric fixation, the beam of light can always be kept focused on the pupil, while no light energy is lost in the operation, at least not enough to be detected either by the eye or by the thermopiles, linear or surface, that we are now using. That is, in the apparatus shown in the diagram, the lens (L_2) which focuses the light on the eye, is mounted on a rack and pinion operating in the line of the fixation-arm of the campimeter. When a given meridian is to be investigated, the fixation-arm is turned into this meridian and the light is always kept focused on the pupil of the eye by means of fine changes in the position of the lens made possible by the rack and pinion adjustment.[1]

control of surrounding field and preëxposure are lacking. And (3) the loss of light is so great and the method of presentation is such that the intensity of the light entering the eye could not be measured.

[1] One of the objects in devising this means of presenting the stimulus light to the eye has been to get an apparatus that could be used in a well illuminated room, for only under these conditions, as has been shown in previous articles (see 'A Note on the Determination of the Retina's Sensitivity to Colored Light in Terms of Radiometric Units,' *Amer. Jour. of Psychol.*, 1912, *23*, pp. 331–332; 'The Effect of Changes in the General Illumination of the Retina upon its Sensitivity to Color,' *Psychol. Rev.*, 1912, *19*, pp. 463–490; 'The Factors that Influence the Sensitivity of the Retina to Color; A Quantitative Study and Methods of Standardizing,' *Psychol. Rev.* Monog., March, 1913, pp. 135–166) can the influence of the brightness of the surrounding field and of the preëxposure be eliminated from the investigation. Obviously a serious difficulty in working in an illuminated room is to get a method of presenting the stimulus to the eye that will give the effect of a colored field and prevent the admixture of light from the room with the wave-lengths which are needed for the investigation. The following methods may be used in dark-room work to get the effect of a colored field or surface to image on the retina. (1) Light of the desired range of wave-lengths is allowed to fall on a diffusely reflecting surface which is non-selective in reflection of wave-length. The standard reflecting surface for purposes of this kind is prepared from magnesium oxide deposited from the burning metal. Such a surface properly prepared has a high reflection coefficient approximately 90 per cent., and possesses

The Source of Light.—The requirements of a source for our purpose are (*a*) that it should give a light in which all the wave-lengths are represented in sufficient strength both for the needs of the investigation of color sensitivity and for radiometric standardization; and (*b*) that the light emitted shall be as constant as possible in intensity and spectro-radiometric composition. After trying many light sources we have finally adopted as best for our purpose the Nernst filament. This filament, after having been properly seasoned,

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the additional advantage that it can easily be kept fresh. And (2) light of the desired composition is allowed to fall on a diffusely transmitting surface also non-selective, or approximately so, in its transmission of wave-length. These methods of getting the effect of a colored surface or field while serviceable within limits for dark-room work, have the following objections for our purpose. (*a*) Both kinds of surface would reflect the white light of the room—the magnesium oxide surface, for example, would reflect approximately 90 per cent. of the light falling on it. This effect, however, is reduced to a minimum by using a double convex lens in the manner employed by us. For when a parallel beam of light falls on the lens and the eye is placed at the focus, the lens fills uniformly with light, and, so far as we are able to determine, white light is not reflected to the eye from the surface of the lens in appreciable amounts under the conditions obtaining in our work. That is, the lens is placed 2.5 cm. behind the stimulus-opening and the whole spectroscope and lens system are enclosed up to the campimeter screen in a light-tight compartment. Under these conditions by exercising a little care in the selection of the position of the apparatus with reference to the distribution of light in the room, so little light is reflected from the lens to the eye that the presence of the lens can not be detected when the eye is in position before the stimulus-opening and no colored light is coming from the spectroscope. In other words, not enough white light is reflected from the lens to render it visible to the eye. (*b*) Both of these methods are very wasteful of light. That is, there is not only a comparatively high percentage of absorption of light by the types of diffusing media, but only a small percentage of the light coming from any point or unit of the surface of these media enters the pupil of the eye. Thus there is not the possibility of getting the wide range of intensities of light needed to give the apparatus maximum serviceability for the investigation of color sensitivity. Furthermore, we are desirous of being able to specify in radiometric units the amount of light per unit area in the cross section of the beam of stimulus light at its entrance into the eye. This would be a very difficult task indeed with either of the two methods of presenting the stimulus light to the eye mentioned above. Its accomplishment, however, is not at all difficult by the method we have adopted. That is, instead of the light spreading from the stimulus-opening as if emanating from a source, it is concentrated into an image on the pupil of the eye, an image in this case of the analyzing slit. (S_2, see pp. 250, 261 and Figs. 2 and 3.) The amount of energy concentrated into this image can easily be determined by direct measurement with our radiometric apparatus for any of the wave-lengths used by us as stimuli, and the amount per unit area be estimated; further, if an artificial pupil were used, or the substitute to be described in a later paper, see *Psychological Review*, 1916, September number, the total amount of light entering the eye could be determined in C.G.S. units.

gives a light which changes comparatively little through long intervals of time. It has the advantage, moreover, that its shape well adapts it for use with the slit of the spectroscope, *i. e.*, the shape is such as to make it possible to utilize for the illumination of the face of the prism a relatively large proportion of the light emitted. Also by increasing the length of the filament and collimator slit, it is possible to increase in direct ratio the amount of light falling on the face of the prism. When in use, in order that no light shall be lost in passing through one or more condensing lenses, the filament is mounted as closely as possible to the jaws of the slit. It is shown in position for work at (*N*) in Figs. 2 and 3. The filament is mounted in a lamp socket fastened to an upright

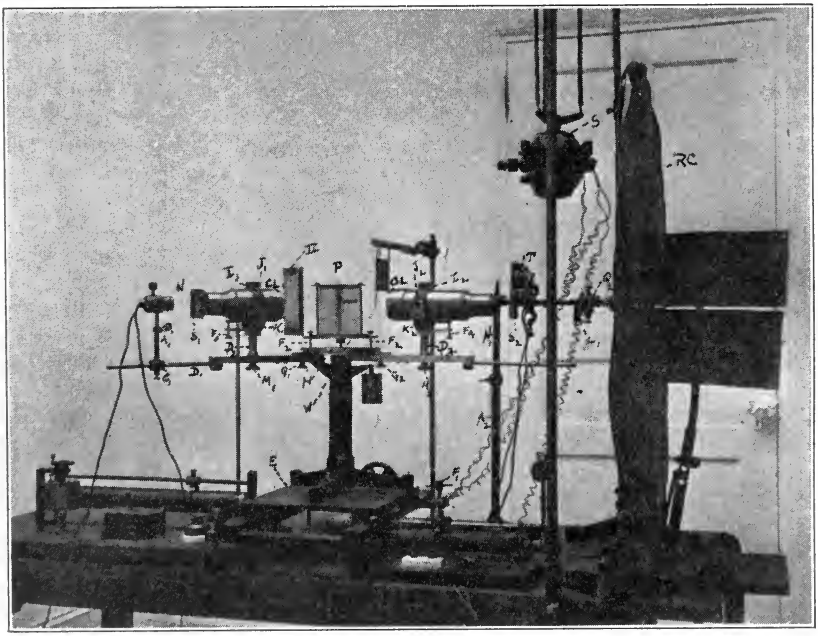

Fig. 2. Showing the Apparatus Grouped for Work—light source, spectroscope, thermopile, and rotary campimeter with its auxiliary lens system. In order that all stray light either from the general illumination of the room or by reflections from the Nernst glower be eliminated, the spectroscope and lens system are, when in use, enclosed from just in front of the Nernst to the campimeter screen in a light-tight compartment.

(A_1). In order that the height of the filament shall be adjustable this upright consists of a short rod sliding in a tube fitted with collar and set screw (B) which permits of a movement of the socket up and down, also to right and left. The upright (A_1) is fastened to the horizontal support (D_1) extending out from the collimator arm of the spectroscope by a clamp and set screw (C_1) which permits of the motion of the socket to right and left and to and from the collimator slit. The Nernst filament used by us is designed to be operated at 110 volts on a D.C. current, and has a carrying capacity rated at 1.2 amperes. When operated at its maximum capacity on a direct current, we have found, however, that the life of the filament is short. Satisfactory results have been obtained by us only when it is operated at or below 0.6 amperes. In series with the ballast, therefore, which is ordinarily used for the reduction of the current from the line and to compensate for the change in the resistance of the

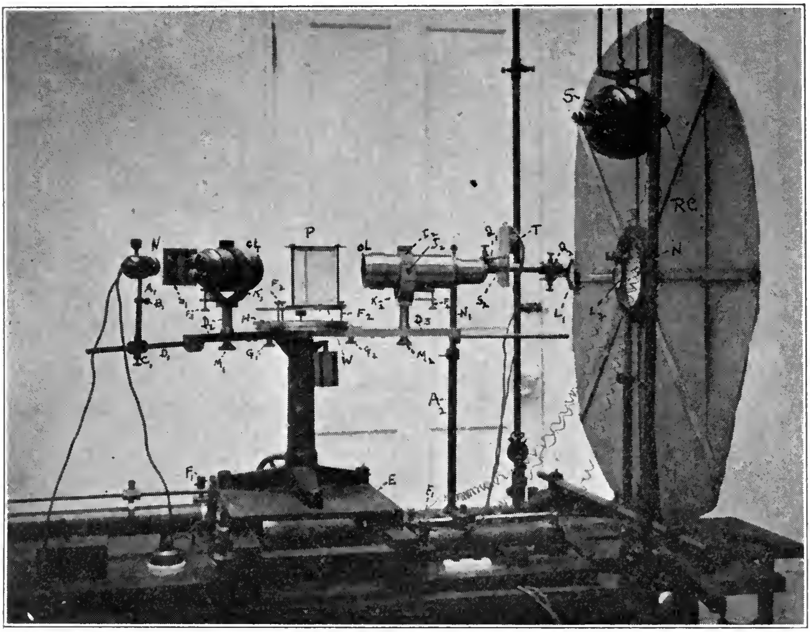

FIG. 3. Showing a Second View of the Apparatus Grouped for Work.

Nernst material as its temperature varies, we have found it necessary to use additional resistance. This resistance is needed not only to cut down the current to the desired value, but to correct for fluctuations in the line. Two coils are employed, one coarse and one fine. The former is used to cut down the current to approximately the desired value, and the latter to correct for the fluctuations in the line. Both are in the form of adjustable rheostats. The second is of special construction to give the small changes needed. It consists of a cylindrical coil of wire wound on an insulated core of brass tubing and is operated by a screw motion in such a way as to give the effect of a slider on a single wire. This rheostat is described in greater detail on p. 275. It is not on the market but can readily be constructed to order by a laboratory mechanician. The finely graduated control afforded by it not only makes it possible to correct for fluctuations in the line, as is stated above, but also to compensate for the slow decrease in the carrying capacity of the Nernst material with use. The current consumed is measured by a Weston ammeter graduated to 0.02 amperes. Operated with this type of control the light flux obtained from the Nernst may be kept constant within the limit of change that can be detected either by the radiometer or the eye.

The Spectroscope.—In addition to the usual features attaching to a good spectroscope, our instrument was designed especially to meet the following needs. (1) To answer all the purposes for which a source of colored light may be used in the investigation of color sensitivity, a wide range of intensity is needed. In order too that an adequate radiometric standardization be made, it is especially desirable that light of high intensity be available. (2) If the spectroscope is to be used in conjunction with a campimeter, it is necessary that the objective arm remain in a fixed relation to the stimulus-opening of the campimeter screen and that some convenient and accurate means be had of changing the wave-lengths without changing the position of the objective. The first of these needs was met by employing a collimator slit long enough to admit the amount of light needed, and a prism and lenses large enough to take care properly of the amount of light admitted. The second point has been met by us in the two following ways, one of which is technically more correct perhaps than the other. (*a*) The spectroscope was mounted on a stationary base supported by levelling screws. In fixed relation to this base a slit (S_2) was permanently mounted to separate out the wave-lengths which are to fall on the stimulus-opening in the campimeter screen. In order to be able to change the wave-length of the light falling on the slit (S_2) the stationary base carries a track along which the whole spectroscope may be shifted by very small amounts. This movement is made by means of a screw of fine thread turned by a wheel $4\frac{1}{2}$ inches in diameter. This track and the base of the spectroscope carry a Vernier scale graduated to 1/50 of a mm. by means of which any previous setting may be accurately reproduced, and in terms of the readings of which the spectroscope may be calibrated in wave-length. This method of changing wave-length not only provides abundantly for small changes in wave-length but it obviates any necessity for readjustment of the collimator arm which would be the case were the wave-lengths changed, for example, by rotating the prism. In case the wave-length is changed by this device, the prism is set for minimum deviation for the D-line, and this adjustment is kept throughout. An adjustment which gives minimum deviation for the D-line alone is generally considered by spectroscopists to be adequate for the work in the visible spectrum when the light is passed through only one prism. Kayser, for example, says:[1] "Bei den Apparaten mit nur einem Prisma verzichtet man für gewöhnlich darauf, alle Wellenlängen unter dem Minimum zu beobachten, sondern stellt das Prisma fest auf, so dass etwa die D-Linien unter dem Minimum durchgehen. Die Dispersion ist bei einem Prisma so gering, dass dies gewöhnlich genügt. Das ist aber nicht mehr der Fall, wenn man über die Grenzen des sichtbaren Spectrums hinausgeht, und so hat vielleicht zuerst Langley bei seiner Untersuchung des ultrarothen Theiles

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[1] 'Handbuch der Spectroscopie,' Bd. I., p. 510.

des Sonnenspectrums an einem Apparat mit einem Prisma eine Vorrichtung beschrieben, welche automatisch das Minimum erhält, und welche wegen ihrer Einfachheit seitdem oft verwandt wird." (*b*) In order that minimum deviation may be had automatically for all wave-lengths falling on our analyzing slit, one of our spectroscopes was built with a minimum deviation attachment.[1] A schematic representation of this attachment and the prism in position for use is shown in Fig. 4. In this figure, *P* represents the prism so placed on the prism table that its refracting angle is bisected by one of the radii of the table; *PB* and *PA* represent respec-

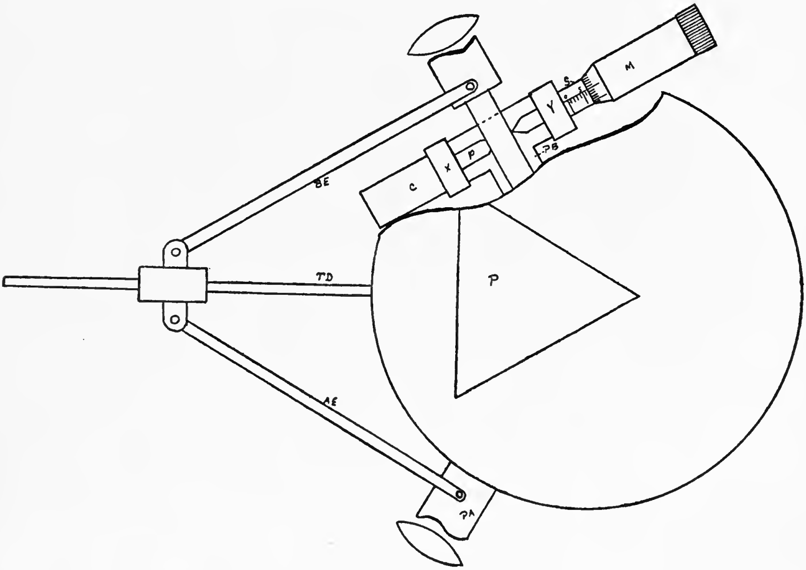

FIG. 4. Showing the Prism Table with Its Auxiliary Minimum Deviation Attachment.

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[1] This attachment is a modified form of a device apparently used first by A. Cornu in 1873. He does not describe the apparatus, however, until ten years later ('Sur un spectroscope à grande dispersion,' *J. de Phys.*, 1883 (2), *2*, pp. 53–57). The first suggestion of such a device seems to have been made by Mascart ('Recherches sur le spectre solaire ultra-violet, et sur la détermination des longueurs d'onde,' *Ann. école norm.*, 1864, *1*, pp. 219–272). See also S. P. Langley, 'The Selective Absorption of Solar Energy,' *Amer. J. of Sc.*, 1883 (3), *25*, pp. 169–176, *Philos. Mag.*, 1883 (5), *15*, pp. 153–183, *Ann. Chim. et Phys.*, 1883 (5), *29*, pp. 497–542, *Wied. Ann.*, 1883, *19*, pp. 226–244, 384–400; B. B. Donath, 'Bolometrische Untersuchungen über Absorptensspectra fluorescerender Substanzen und ätherischer Oele,' *Wied. Ann.*, 1896, *58*, pp. 608–661; and F. L. O. Wadsworth, 'Fixed-arm Spectroscopes,' *Philos. Mag.*, 1894 (5), *38*, pp. 337–351.

tively the collimator and objective arms which are fastened to the stem of the spectroscope independent of the prism table; *TD* represents an arm fastened to the prism table in such a position as to be continuous with the radius of the table which bisects the refracting angle of the prism; *AE* and *BE* represent two rods of equal length which connect *PA* and *PB* at points equidistant from the center of the table to a collar which is free to play back and forth along the arm *TD*. *M* is a micrometer screw with a graduated cylindrical head, which is used to move the collimator arm through the small angles needed to give the change of wave-length. Opposed to this screw is a plunger *p* working against the spring in the case *C*. When the screw advances it moves the collimator arm forward and when it recedes the collimator arm is made to follow it by the push of the plunger *p*. The screw and plunger are supported by a curved arm coming off from the stem of the spectroscope, which can be clamped rigidly in any position which may be desired. This arm ends in two right-angled extensions, one of which carries the screw and the other the plunger. Between *X* and *Y*, the vertical arms of these extensions, the collimator arm is moved to give the change of wave-length. The micrometer screw passes through a fixed sleeve *S* graduated in fortieths of an inch. As the screw advances the cylindrical head telescopes on the sleeve *S*, one of the graduated spaces being traversed with each complete turn of the screw. The forward end of the cylindrical head is bevelled and on the circumference of the bevelled edge is a scale of equal divisions graduated in twenty-fifths of an inch. By means of this scale and the scale on the sleeve *S*, the advancement of the screw can be read in thousandths of an inch.

That minimum deviation is given automatically by this device to all the wave-lengths falling on the analyzing slit may be understood from the following considerations. In attaining minimum deviation the incident and emergent rays make equal angles with the normal to the refracting faces of the prism, therefore equal angles with the refracting faces themselves. When an adjustment is made for minimum deviation for the D-line of the spectrum, for example, and the wave-length is changed by moving either the collimator or objective arms, the prism must be turned through half the angle through which the collimator or objective arm is moved if the wave-length traversing the axis of the objective tube is to be deviated the minimum amount. That is, for the prism to be set for minimum deviation the line bisecting the refracting angle of the prism will also bisect the angle made by the incident and emergent rays; hence if in changing the wave-length, the angle between the incident and emergent rays be changed a given amount by a movement of the collimator or objective arm, the prism must be moved through half that angle in order that the line which bisects its refracting angle will also bisect the angle made by the incident and emergent rays. The special purpose of the attachment described above, therefore, is to turn the prism through half the angle traversed by the collimator arm in changing the wave-length. This is attained by placing the prism on the table so that the radial arm *PD* bisects the refracting angle, and by connecting the moveable collar on the arm *PD* with the collimator and objective arms at points equidistant from the center of the prism table by rods of equal length (*AE* and *BE* of Fig. 4). Then when an adjustment is made for the D-line, and the collimator arm is moved through the angle needed to change the wave-length which falls on the analyzing slit, the arm (*PD*) turns through half that angle and takes up a position midway between the collimator and objective arms, and the part of the spectrum which falls on the analyzing slit is deviated the minimum amount in passing through the prism.[1]

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[1] A constant deviation prism may be used also for getting automatically minimum deviation for all parts of the spectrum. Such a prism may be constructed by setting two 30° prisms against the faces of a right-angled totally reflecting prism; or the prism may be made in one piece in such a way that the four vertical faces enclose four angles of 90°, 75°, 135°, and 60° respectively. In using this type of prism the collimator and objective tubes are set permanently at right-angles to each other. When this is done and the prism is rotated about a vertical axis, the different portions of the spectrum are thrown on to the analyzing slit and the portion that enters it at any moment, will traverse the system at minimum deviation.

In the early part of our work we used a constant deviation prism of the Cassie

Following is a description of the parts and mounting of the spectroscope. The collimator slit (S_1) is 12 mm. long. Its width is adjusted by means of a micrometer screw fitted with a head graduated to read thousandths of a mm. The collimator lens (*CL*) is a Zeiss triple achromat, 180 mm. focal length, 60 mm. diameter; the objective lens (*OL*) is also a Zeiss triple achromat, 240 mm. focal length, 60 mm. in diameter. The prism (*P*) is 100 mm. high and has a refracting base of 85 mm. and a refracting angle of 60°. Owing to the large size required, a liquid (CS_2) prism has thus far been used. With the exercise of a reasonable amount of caution to keep the CS_2 free from impurities, this prism has given very good satisfaction. At present we see no good reason why its use should not be continued. The analyzing slit (S_2) is made adjustable in length. The range attainable varies from 0 mm. to 12 mm. A slit adjustable in length is employed in order that the amount of light entering the eye may be made independent of the natural pupillary aperture. (See 'A Substitute for an Artificial Pupil,' *Psy. Review*, 1916, —, September number.) So far it has been mounted on an independent base screwed to the table in a fixed relation to the base of the spectroscope and the stimulus-opening in the campimeter. If desired this base might be made a continuation of the base of the spectroscope. In order that the distance of this slit from the objective lens (*OL*) may be adjusted for the different focal distances for the different wave-lengths, the frame in which the slit is mounted is furnished with a rack and pinion adjustment.

In Figs. 2 and 3 is shown the mounting of the spectroscope. At *E* may be seen the platform and track on which moves the carriage bearing the spectroscope. The platform is supported on four upright somewhat pointed screws (F_1) which serve the triple purpose of leveling the apparatus, of

type which not only gives automatically minimum deviation but also the effect of a train of prisms; *i. e.*, the arrangement is such that the light is passed back and forth through the prism a number of times before it finally enters the objective tube. (Cassie, W., 'Multiple Transmission Fixed-arm Spectroscopes,' *Philos. Mag.*, 1902, Ser. 6, *3*, 449-457.) This prism was constructed to special order of the dimensions needed for our apparatus.

allowing an adjustment of 4 inches in height, and of preventing chance shifting of the position of the apparatus on the table.[1] To this movable platform is securely fastened the heavy stem and tripod base on which the spectroscope is mounted. The prism table of the spectroscope is shown at *W*. It has two adjustments. (*a*) With its supporting table it can be rotated in the horizontal plane to provide for the adjustment of the collimator and objective systems at the angle of minimum deviation and to allow for change of wave-length by the rotation of the prism if that should be desired. And (*b*) it is furnished with three leveling screws (F_2) by means of which it can be accurately leveled. The collimator and objective tubes are mounted on two horizontal arms (D_2 and D_3) which rotate about the upright stem on specially prepared collars. D_2 furnishes the support for the light source and the collimator tube, and D_3 for the objective tube. In order that these arms when adjusted shall be

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[1] In the construction of the carriage and track great care was taken that there should be no play or free motion of the parts. This was necessary in order that for a given reading of the Vernier scale on the carriage and platform the axis of the objective should always sustain the same relation to the slit (S_2)—in other words that for a given reading the same wave-lengths should always fall on the slit.

The calibration of this Vernier scale was accomplished by means of a Hilger direct vision spectrometer which has a scale reflected across the upper half of the spectrum. Since wave-lengths can not be read directly from this type of instrument, a supplementary chart must be made in which are given the values of the different scale divisions in terms of wave-length. This was done as follows. Twenty-six points in this scale were identified with the bright lines given by potassium, strontium and cadmium arcs and by the mercury tube. The wave-lengths of these bright lines were obtained from Hagenbach and Konen's 'Atlas of Emission Spectra,' and from these values the curve of wave-lengths was plotted for the entire spectrum. For the calibration of the Vernier scale the Hilger spectrometer was then mounted behind the stimulus-opening in the campimeter screen, and the wave-lengths coming through for any given reading of the Vernier scale were determined.

The calibration of the graduated scale on the fixed sleeve and cylinder head of the minimum deviation attachment (p. 258–260) may be accomplished in a similar way.

In case the minimum deviation attachment is used and the changes of wave-length are produced by it, such precision in the construction of the carriage and track as is described above is of course not necessary. That is, the carriage and track, while devised in our particular apparatus primarily for changing the wave-length, is very serviceable for other purposes which do not require such careful construction. For example, we have found this feature to be very useful in lining up the instrument with other apparatus, especially when the work requires the objective arm to be fixed as is needed in our adaptation of the spectroscope to the campimeter.

held rigidly in place, they are firmly clamped to the supporting table by means of the metal pieces (G_1 and G_2) one end of each of which is milled down to fit in a groove in this table (H) and the other is clamped respectively to the arms (D_2 and D_3). The collimator and objective lenses are each mounted in brass telescope tubing provided with a rack and pinion for the adjustment of its length. In order that the axes of these tubes may be in the same horizontal plane the following provisions for leveling are made. The tubes fitted about one-fourth of their length from the larger end with collars (I_1 and I_2), are swung on trunions (J_1 and J_2) in U-shaped housings (K_1 and K_2) supported by small vertical pillars coming up from the horizontal arms (D_2 and D_3). The smaller end of each tube rests on a leveling screw (F_3 and F_4) threaded in a short horizontal stem coming out from the housings (K_1 and K_2), by means of the adjustment of which the axis of the tube may be brought into the proper position. When this position is attained provisions are made for clamping the tube firmly in place. In order that room may be allowed between the prism and the collimator and objective tubes for the introduction of apparatus for reducing the intensity of light or for any other purpose that may be desired, the vertical pillars carrying the tubes run in slots 10 cm. long cut in the horizontal arms (D_2 and D_3). When adjusted to the proper position the pillars are firmly clamped to the horizontal arms by means of the milled nuts (M_1 and M_2). The analyzing slit to separate out the wavelengths to be used for the colored stimulus is shown at S_2. The length of this slit may be made to vary from 0 mm. to 12 mm. Its width is adjustable by means of a micrometer screw fitted with a head graduated to read to thousandths of a mm. This slit is mounted vertically in an oblong brass frame, 12 cm. long and 6.5 cm. broad, carrying on each side a groove. In this groove slides a narrow brass plate on which is mounted a linear thermopile (T) with its receiving surface facing in the direction of the slit through a circular opening in the plate. During the color observation the plate and thermopile are raised out of the path of the beam of light and clamped. For the energy measurements they are lowered to the level of the slit. In order to focus the different wave-lengths of the spectrum on the analyzing slit, a micrometer adjustment is provided to regulate the distance from the objective lens. The frame for thermopile and slit and the micrometer attachment are carried by an upright (A_2) which is mounted to one side of the path of the beam of light on a heavy independent tripod base screwed to the table. In order that there may be an adjustment of height, the upright consists of a tube 34 cm. long fitted at its upper end with a collar and set screw into which slides a steel rod (N_1) 38 cm. in length. The micrometer adjustment consists of a tube 30 cm. long and 15 mm. in diameter along the axis of which runs a finely threaded screw fitted with a milled head. In this tube is a beveled slot 5 mm. wide and 22.5 cm. long exposing the threaded screw. Telescoping the tube are two sections of tubing of larger bore (Q_1 and Q_2), 4.5 cm. in length, to the inner surfaces of which are screwed threaded pieces which extend down into the slot and engage the micrometer screw. As the micrometer screw is turned, these tubular sections move slowly along the screw. They are each fitted with collar and set screw which hold the horizontal rods supporting respectively the framework for the thermopile and the holder for the collimating lens (L_1). The collimating lens (L_1) is inserted in the path of the beam of light in order that the light emerging from the analyzing slit (S_2) may enter parallel the focusing lens (L_2) in front of the stimulus-opening in the campimeter screen. This lens has a diameter of 40 mm. and a focal length of 140 mm. Obviously if it is to act as a collimator for all the waves of light emerging from the slit (S_2), its distance from the slit must always be kept equal to its focal length. That is, when the distance of the slit from the objective lens is changed to accommodate for the difference in focus for the different wave-lengths of light, the distance of the lens (L_1) must also be changed by an equal amount and in the same direction. This is accomplished by the micrometer adjustment described above. That is, the tubular sections which carry respectively the framework for the slit and the holder for the lens are both operated by the same micrometer screw, hence any movement of the slit is accompanied by a similar movement of the lens. An adjustment of the distance of the lens for the slit, once made, need not, therefore, ever be disturbed in the process of accommodating for the difference in focus for the different wave-lengths of the spectrum.

In order that all stray light either from the general illumination of the room or by reflections from the Nernst glower be eliminated, the spectroscope and lens system are enclosed from just in front of the Nernst to the campimeter screen in a light-tight compartment. So far we have found it most convenient to make this compartment of light-proofed fabric. That is, the compartment must be made large enough to enclose all of the auxiliary reduction and adjusting apparatus, the thermopile, etc., and must permit of easy entrance for the purpose of making the adjustments and settings required. The construction of a compartment having these requirements we found could be most simply and feasibly accomplished by employing, as stated above, light-proofed fabric. There are so many ways in which such a compartment can be constructed that space will not be taken here for a description of the compartment we have used. It will be sufficient to say that adequate care has been taken to exclude any stray light that might find its way into the stimulus by means of reflections and refractions within the system.

It is well known to spectroscopists that if some portion of the spectrum obtained by a single spectroscope be examined by a second instrument of good resolving power, frequently more than one band will be obtained. For example, if a spectroscope is so adjusted that only a narrow region of the red falls on the objective slit and the light emerging from this slit is passed through a second spectroscope, it may be found that the second resolution gives one or more comparatively faint bands in some other part of the spectrum. In most cases where a good instrument is used this degree of impurity would probably not appreciably change either the radiometric or optical results. However, we have endeavored to devise means whereby light of such a degree of purity may be obtained that the second resolution shows only the one band. Our first step in this direction was (*a*) to eliminate as far as possible all stray light and internal reflections. Stray light was eliminated by carefully light-proofing the housing of the apparatus. Also the base of the prism and every other surface which might either admit or reflect extraneous light into the path of the refracted beam was blacked. Some of the harmful sources of internal reflection were found to be the bounding surfaces of the objective lens, the prism faces and the analyzing slit, principally the surfaces of the lens.[1] For example, in looking into the forward end of the objective tube a number of small images of the spectrum can be seen graded in size and nearly in line but projected to different distances. These images show of course that light is being reflected back and forth in the optical system. The amount of this reflection can be lessened to a considerable extent by reducing the area (cutting out the edges) of the lens and prism surface exposed to the beam of light. However, since this method of reducing the amount of internal reflection lessens also the intensity of light, only a limited use was made of it. If care is not taken to prevent it, light from neighboring regions of the spectrum will also be reflected back into the objective tube from the surfaces on either side of the analyzing slit. These surfaces should, therefore, be very carefully blacked. Some good perhaps is accomplished by slanting them slightly so that the edges of the slit point in the direction from which the light is coming. The effect of this is to reflect out of the system all light but that which passes through the slit. The edges of the slit also require careful attention if reflections are to be avoided. The knife-edge should not be obtained by a steeply pitched bevel on either side. We have found that very satisfactory edges may be made from sections of Gillette razor blades. (*b*) A minimum deviation device was attached to our spectroscope (see pp. 258–260). This was found to add a great deal to the

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[1] The surfaces of this lens being concave on the side opposite to which the light enters the lens tend to reflect the parallel waves toward the axis of the beam, and thus again to mix the light which had been resolved by the prism.

purity of the spectrum.[1] But (*c*) the simplest and most effective device proved to be the use of thin gelatine filters especially selected with reference to the bands that were to be eliminated. The use of these filters in fact is enough to give the desired purity even when no other precautions are taken. Moreover, we were able to select these filters so that there was so little absorption of the light that we wished to use as to be of no consequence for the purposes of our work. They were mounted in an especially constructed holder between the objective tube of the spectroscope and the analyzing slit. A similar result could be accomplished by using two spectroscopes in series.[1] But the expense and inconvenience of doing this make it undesirable in work of the type we are doing, more especially when the desired purity can be obtained by the simple means described above with probably a negligible reduction of the intensity of the useful light.

APPARATUS FOR VARYING THE INTENSITY OF THE STIMULUS LIGHT

In designing an apparatus which will be at all broadly serviceable in the investigation of color sensitivity, due

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[1] Our attention was called to the effect of the minimum deviation by the observation that when the prism was adjusted for minimum deviation for the D-line and the wave-length was changed by shifting the spectroscope by means of the driving screw shown in Fig. 2, the farther the D-line was from the analyzing-slit, the greater were the number of bands which appeared in the field when the light emerging from the slit was examined by means of a second spectroscope. Without the use of the gelatines or a second spectroscope, however, we have not been able to get rid entirely of the bands mentioned above.

[1] It is obvious that an advantage is gained here for purity over and above the increase in resolving power given by two prisms. Increase in resolving power is usually gotten by using a train of prisms in a single spectroscope or a single prism through which the light is passed more than once. (The Cassie prism used earlier in our work is of this latter type, see this paper, footnote, p. 260.) But while increasing the resolving power increases the purity of the spectrum (Schuster, 'Theory of Optics,' p. 163 expresses the dependence of purity on resolving power by the formula $P = pR$, where P represents purity; R, resolving power; and p, a factor which is a function of the slit-width), the impurities caused by internal reflections are clearly of a kind that can never be eliminated entirely by an increase of the resolving power of the prism or train of prisms. This, so far as we know, can be done only in ways similar to those described above. The use of a second spectroscope, however, besides being much more expensive and more inconvenient to manipulate, causes a greater decrease

attention should be paid to adequate means of varying intensity. This has become of especial importance because of the growing recognition of the usefulness of the threshold and just noticeable difference determinations as the basis of comparative work. We have designed, therefore, apparatus to produce both gross and fine changes in the amount of light employed, more especially the small gradations needed for threshold and just noticeable difference determinations. This apparatus is shown in Fig. 5. It consists

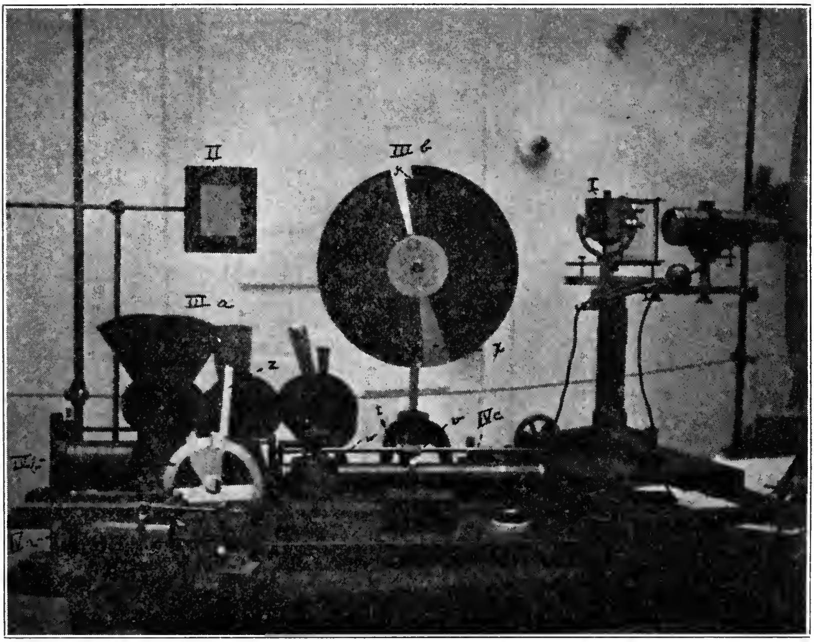

Fig. 5. Showing Auxiliary Apparatus Designed for the Reduction of the Intensity of Light—specially constructed resistance coils, coarse grating, aluminum sectored discs with Vernier protractor, and collimator slits including just noticeable difference slit.[1]

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in the intensity of light used as stimulus than do the filters mentioned above, which when properly selected may produce practically no change in a given region of the spectrum and absorb heavily in other regions.

[1] In III. *b* the disc made up of a single 15° sector (*x*) is shown bright side before to make it distinguishable from the remainder of the compounded disc. When in use it is placed behind the other discs and the black side is turned towards the focusing lens just behind the stimulus-opening.

of (1) two types of collimator slit, one of which is especially devised for just noticeable difference work; (2) coarse gratings designed to give gross variations in the intensity of light; (3) sectored discs suitable for threshold and just noticeable difference work in which all of the changes from 0°–348.75° total aperture may be made, and a protractor with Vernier scale to permit of close reading of the discs especially when the aperture is small; and (4) special resistance coils designed to vary the intensity of light at the source.

Collimator Slits.—One of these slits is of the usual type having carefully beveled knife-edged jaws 12 mm. long, with a micrometer adjustment of width graduated to thousandths of a mm. Such a slit may be used (*a*) to make a reduction of the spectrum as a whole or of any part, and the reduction may be computed directly from the slit width provided the source is uniformly luminous over the surface exposed; or (*b*) one part of the spectrum may be made in turn to sustain, within limits and under the conditions mentioned above, any ratio that may be desired to any other part of the spectrum, providing the original intensities from which the reductions are made, are known. This slit, however, is not adapted to just noticeable difference determinations for a given color or range of wave-lengths. The second slit (shown at *I*, Fig. 5) is especially designed for just noticeable difference work. This slit is constructed so that its upper and lower halves are independently variable in width. It was designed especially for some new methods we are using for a quantitative comparative determination of the retina's inertia to the different wave-lengths of light in which just noticeable differences are employed. In the short exposures used in tracing the sensation from the threshold to its maximum, it is obvious that the sectored disc could not be employed in making the variations needed for just noticeable differences. While designed to meet this special need we have found it to be a very convenient means of making the variations needed for much of the general work in just noticeable difference determinations. The slit is formed by three knife-edged jaws. That is, one jaw of the slit is made in one piece and is 12 mm. long; the other jaw is made in two pieces, each 6 mm. long. One edge, the upper for the upper jaw and the lower for the lower jaw, is beveled to fit into a dovetailed guide cut in the frame. The other edge of each jaw is held in place by a slender close fitting key of appropriate length. The jaw, 12 mm. long, is stationary and the other two jaws are made to move away from it by two independent micrometer screws operated by drum heads graduated to thousandths of a millimeter. In operation the source is adjusted so that one edge of its equally luminous surface is flush with the stationary jaw and the other jaws are pulled away from it exposing as desired different widths of this surface. In a just noticeable difference series one half of the slit is held constant and the other is varied to give the just noticeable difference. When the width of slit needed for this is obtained, that half of the slit is held constant and the other is varied, and so on until the series is completed.

The Coarse Grating.—This device is serviceable for gross reductions of the spectrum as a whole. It is shown at II. in Figs. 5 and 2. The grating consists of an exposed photographic plate ruled on a dividing engine with lines 60 to the centimeter. The gelatine side of the plate is covered with a thin glass plate and the whole is mounted in a brass frame supported by a slender rod running parallel to the lines of the grating. This rod fits into a collar furnished with a set screw so that the grating may be rotated any amount that is desired about the axis of the rod. When interposed in the path of the light this grating allows the maximum amount of light to pass through when it is perpendicular to the path. As it is rotated, less and less light gets through the open spaces afforded by the lines, the amount depending upon the angle of rotation. If it were wanted to use this grating as a more precise instrument of reduction, it would be comparatively simple to add a graduated scale so that any previous setting might be reproduced, and to calibrate the scale so that the amount of reduction might be read directly from it. So far in our work we have not felt the need to do this as the grating has been used only in making gross reductions in the amount of light employed, the finer changes being made either by the slit or by the sectored discs. This grating may be mounted anywhere in the path of the light from the source to the campimeter screen. Thus far we have found it most convenient to insert it between the collimator and the prism (see Fig. 2).

The Sectored Discs and Vernier Protractor.—Probably the most convenient and widely applicable apparatus for varying the intensity of light by known amounts is the sectored disc. A strong objection to the use of the sectored disc when fine changes are needed such as are required, for example, in threshold and just noticeable difference work, is the difficulty of getting and of measuring accurately sufficiently small amounts of change. Such discs are ordinarily constructed with two or more open sectors, and a change in one is multiplied as many times as there are open sectors. Moreover, an error made in the measurement of one sector is multiplied by the number of open sectors. This latter difficulty becomes especially significant in working with intensities at or near the threshold when a small error may represent a high percentage of the total open sector. We have sought to overcome these difficulties in two ways. (1) Our discs for a low total aperture are so constructed that one sector may be varied at a time. And (2) a special protractor has been designed fitted with a movable arm carrying a knife edge and Vernier scale graduated to read to 1/60 of a degree.

Two sets of discs were made in all—one for simple reductions and threshold work, and the other for just noticeable difference determinations. The discs were cut from hard sheet aluminum No. 20 B. & S. gauge, 0.9 mm. thick; and are for the first set 19.5 cm. in radius. In order to give a wide range of change a number of discs are required. For example, a variation of total range of open sector from 0°–348.75° is obtained in the first set of discs by using four pairs of two-sector discs and one pair of one-sector discs with a counter-balancing weight. In the first pair the breadth of each of the two open sectors is 90°, and the range of variation of total open sector is from 0°–180°; in the second pair the breadth of each of the two sectors is 45° and the range of variation of total open aperture is from 180°–270°; in the third pair the breadth of each of the two open sectors is 22.5° and the range of total aperture is 270°–315°; in the fourth pair the breadth of each of the two sectors is 11.25° and the range of total aperture is 315°–337.5°; and in the fifth pair the breadth of the single sector is 11.25° and the range of total aperture is from 337.5°–348.75°. In order that very small apertures may be obtained or that small variations of aperture may be had, when each open sector is 15° or less, a small single sector furnished with counterbalancing weight was provided in addition to the five pairs of discs. When this is used with the pair of discs having 90° sectors to cover one of the open sectors, the total aperture may be varied from 0° through 15°. This single sector may also be used in connection with any of the other pairs of discs to aid in making smaller variations in the total aperture than is obtainable with the pair of discs alone. That is, as one of the open sectors is opened, the other may be closed by any desired amount less than 15° by means of this single sector. Following this principle broader single sectors may be constructed to permit of smaller variations when the total aperture is still larger. We have found, however, that the 15° sector satisfies the need for the values of total aperture for which small changes are significant.[1]

In order to make the first set of discs serve also for just noticeable difference determinations, it was necessary to supplement each of the pairs of discs in this set with single discs of the same breadth of sector and of lesser radius.

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[1] In the construction of these discs a solid disc with a radius of 19.5 cm. was first cut from a sheet of aluminum. The open sectors were then cut into these discs of the breadth desired to a depth of 13.5 cm. This left a small solid disc of 6 cm. radius to support the pairs of sectors. As was stated above in order to guarantee symmetry of rotation in case of the single sectors, a counterbalancing weight was fastened on the opposite side from the sector 6 cm. from the center of rotation. This weight was in the form of a small lead disc soldered to the inside supporting disc in line with the radius which just bisects the sector. It is scarcely needful to say that great accuracy is demanded in the cutting of the discs. This accuracy was obtained by means of a special cutter designed for cutting with accuracy straight and curved edges in metal.

These discs were 17 cm. in radius and the open sectors were cut in from the outer edge to a depth of 11 cm. A margin of 2.5 cm. was thus left between the edges of the two sets of discs. In making the just noticeable difference determination the two sets of discs are adjusted so that the edge of the inner disc just bisects the stimulus-opening. The openings of the two sets are then varied independently as may be required, it being necessary of course always to make the open sectors of the outer discs the larger. The sectored discs are shown at III. *a* in Fig. 5. The method of using them is further illustrated at III. *b*. Here the discs are shown mounted on an electric color mixer and are set for a just noticeable difference determination low in the intensity scale. Two of the set of larger discs each having two 90° sectors are mounted to give two open sectors one of which is closed by means of the single 15° sector shown at *x*, leaving a total aperture of 10°. In front of these is mounted the disc of smaller radius with the edge of one of its sectors shown at *y* projecting 4° into the 10° opening reducing it to 6°. When the disc so compounded is adjusted in front of the stimulus-opening so that the edge of the disc of shorter radius just bisects this opening and the disc is rotated at the fusion rate, the two halves of the opening are illuminated with light of intensities proportional respectively to 10 and 6. By using the discs of different breadths of sector, similar variations can be achieved over a range 0°–347.75° open sector.

The special protractor by means of which the width of the open sectors may be read to 1/60 of a degree is shown at Z. This protractor is provided with a 180° arc of 10 cm. inside radius, and an arm 27 cm. long which rotates about a central collar closely fitting the chuck of the motor. The 180° arc is graduated to 1/4 degrees and the movable arm carries a Vernier scale graduated to 1/60 degrees. The movable arm carries a beveled slot also of a 10 cm. radius of curvature into which the 180° arc fits. To insure accuracy of setting this movable arm is provided with finely beveled straight edges. When making a measurement of open sector, one of these edges is set flush with one of the edges of the sector and the reading made. It is then rotated until the same edge is flush with the other edge of the sector, and the difference between the readings is taken as the value of the sector. We have thought it necessary to stress the accuracy with which these measurements must be made because the threshold value of sensation is frequently obtained for the intensities of light employed by us with a total aperture of 1/15°. With such small apertures it is obvious that accuracy of measurement becomes of prime importance. When used in connection with the spectroscopic apparatus described in a preceding section to determine the threshold and just noticeable differences in sensation, the sectored discs are interposed in the path of the parallel beam of light just behind the lens L_2 (see Figs. 2 and 3). As is shown in this figure in order to eliminate as far as possible all vibrations and consequent displacements of the edges of the discs from the desired alignment with the beam of light, the discs are mounted on a motor (*S*) suspended by springs. With discs so designed and used, and with the proper standardization of the factors which influence the response of the eye, determinations may be made having a very high degree of reproducibility.

Special Resistance Coils.—Special resistance coils have been devised which serve the following purposes: (1) to give the fine changes of resistance needed to compensate for fluctuations of voltage in the lighting circuit which otherwise might produce troublesome variations in the flux of light from the Nernst filament; (2) to produce changes in the intensity of the spectrum given by the filament;[1] and (3) to make possible the fine changes in the speed of rotation of the discs that are so frequently required in work in the optics of color. These coils are of two general types. (*a*) Coils which give fine changes over a narrow range; and (*b*) coils which will permit of fine changes over a wide range. Coils of the first type are shown in Fig. 5 at IV. *a* and *b*; a coil of the second type at IV. *c*. The resistances of the first type consist of one coil and are constructed to give the effect of a

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[1] It was recognized of course in using the resistance for this purpose that a change in the amount of current by which the Nernst is operated changes the spectro-radiometric composition of the light.

contact sliding along a single wire. The effect is produced by winding the coil of wire of the desired size and resistance on a hollow brass cylinder insulated with micanite, and by turning this coil by means of a screw motion under a U-shaped contact. In this way the contact is made to travel along the entire length of the wire, and the fineness of change is limited only by the size and coefficient of resistance of the wire. In one example of this general type of resistance shown in Fig. 5 (IV. *a*), the contact is kept stationary and the cylinder is mounted on a rod as its axis, threaded at both ends. As this rod operated by a milled head turns, the cylinder rotates and slowly advances, so that the contact travels continuously over the whole length of the wire. In a second example of this type, shown at IV. *b*, the contact slowly advances as the cylinder is rotated, and passes continuously over the whole length of the wire. This effect is accomplished as follows. The cylinder is mounted on a horizontal rod turned by a geared wheel 5 cm. in diameter. The contact is mounted on a threaded rod which is turned by a geared wheel of the same diameter as the wheel which turns the cylinder. As the latter wheel turns, its teeth engage the teeth of the wheel which rotates the rod on which the anchor piece of the contact is threaded, and the contact advances along the rod at a rate which keeps it continuously in touch with the wire throughout its whole length. The coil which we use to control the amount of current operating the Nernst filament is made of No. 26 wire, 47.8 ft. long, and has a resistance of 55 ohms.

The resistance of the second general type consists of two coils in series, one designed to make gross changes and one to add fine changes to it for any given setting of the contact. The wire of the first coil is wound on a long section of brass tubing insulated by micanite. This tube was given a stationary mounting and the changes in resistance are produced by a sliding contact shown at *v*. The supplementary coil was wound on a brass drum shown at *w*, insulated by micanite and mounted on a vertical threaded rod operated by a small wheel. As this wheel is turned the drum rotates and slowly changes its level bringing every point on the wire successively in touch with a stationary contact. The connections are so made in this rheostat that by means of a double-pole switch shown at *t*, the poles of the large coil can be reversed and a high rate of speed can be instantly changed to a low rate of speed, and *vice versa*. In the rheostat shown in Fig. 5, the large coil is made of Advance wire No. 26, 139 ft. long, and has a resistance of 160 ohms. The small supplementary coil is made of wire of the same composition, No. 26, 17.4 ft. long, and has a resistance of 20 ohms. By means of these two coils changes of resistance amounting to very small fractions of an ohm can be made and the speed of rotation of motors of the type constructed by the C. H. Stoelting Co., for example, can be controlled to fractions of a revolution per second. We have found such control to be very useful in the general field of work in which impressions are to be given to the eye in succession, and especially necessary in the studies that we have made of the factors that influence the results obtained by the method of flicker for the photometry of lights of different color. It is obvious that some such control must be had if the phenomena produced by changing the rate at which impressions are given to the eye are to be studied in satisfactory detail.

THE ROTARY CAMPIMETER

In a previous paper[1] a rotary campimeter was described especially devised for use with pigment papers. At that time it was stated that this apparatus had also been adapted for use with a spectroscope, and that a description of it would be given in a later paper. The apparatus we shall here describe has been in use, therefore, for three years and its feasibility has been tested for that length of time both in the research and drill work of the laboratory.

The object of the rotary campimeter is to add to the vertical campimeter the rotary features of the perimeter, and thus to allow investigation of every possible meridian of the retina

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[1] C. E. Ferree, 'Description of a Rotary Campimeter,' *American Jour. of Psychol.*, 1912, *23*, pp. 449–453.

with as much ease and precision as was possible with the old form of campimeter in the nasal meridian only, or at most in the nasal and temporal meridians. As designed for use with the spectroscope this apparatus consists of two parts with appropriate supports and accessories: campimeter screen and lens to focus the light on the pupil of the eye and to shift the image to follow the pupil as it takes an excentric fixation; and attachment to line up the eye with the stimulus-opening. The campimeter screen rotates on a brass collar around a circular support. The stimulus is exposed through an opening in the center of the campimeter screen. Behind this opening mounted in a smaller brass collar is the focusing lens (L_2). This lens is carried on a rack and pinion which moves its center back and forth along a line which passes through the center of the stimulus opening and which contains all the fixation points on the arm (I–I').

Fig. 6 shows the skeleton apparatus. It consists of the following parts: supporting base, frame for campimeter screen,

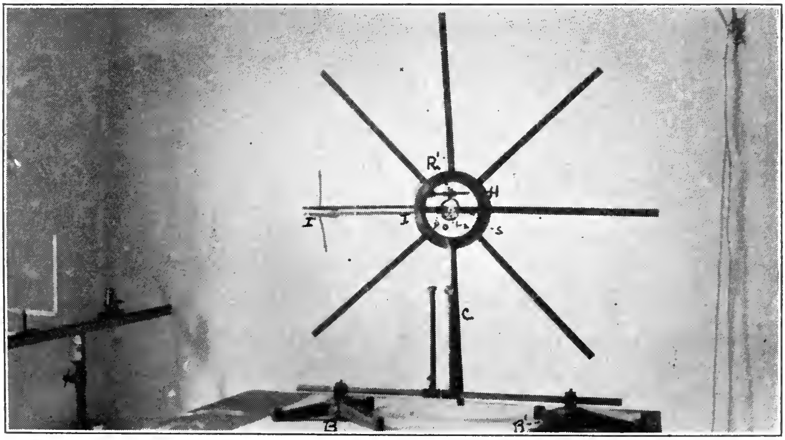

Fig. 6. Showing the Rotary Campimeter, the Lens for Focusing the Light on the Eye, and the Rack and Pinion used to shift the focal point as the eye changes its fixation.

rack and pinion adjustment and support for the focusing lens, and attachment for lining up the eye with the stimulus-opening. The supporting base consists of a horizontal steel bar, 83 cm. long, supported by two tripod rests (*B* and *B'*). To this bar is clamped an upright (*c*) which serves as a support for the framework of the campimeter screen. In order that the distance of the upright of the screen above the table may be adjustable, this upright consists of a steel tube 27 cm. long and 15 cm. in diameter furnished at its upper end with a collar and set screw into which fits a rod 20 cm. long, to which the framework of the campimeter screen is attached. The framework of the campimeter screen consists of a stationary brass ring about which rotates a larger brass collar (*H*), 20 cm. in diameter.[1]

The back circumference of collar (*H*) is graduated from 0°–360°. To this collar are fastened the radiating arms. There are eight of these arms, one for each 45° mark on the graduated collar. They are made of steel and are 2 cm. broad and 40 cm. long. The eighth arm (*I–I'*) differs from the other seven. It forms a right angle, one side of which lies in the plane of the background, and the other in front of this plane. The part in the plane of the background is 60 cm. long and the part at right angles to this plane is 28 cm. long. The arm is graduated from 18°–57° along the section that lies in the plane of the background, and from 57°–92° along the section at right angles. The graduations are based on the arc of a circle of 25 cm. radius. The rack and pinion adjustment (*R'*) which carries the focusing lens is attached to the rotating collar. Thus when the arm carrying the fixation points is rotated into any given meridian, the rack and pinion adjustment is also rotated so that the line of motion of the center of the lens always contains the fixation points. The focusing lens (L_2) is a double convex lens 50

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[1] This ring was made large in diameter for two reasons. (*a*) The ring had to be made thick in order to give sufficient rigidity to support the campimeter screen and to furnish the proper attachment for the rotary collar. Had the circumferance been made small, the effect of the ring would have been that of a short tube. If the stimulus were viewed through a short tube, an induction factor would have been involved which would have been difficult if not impossible to standardize. The opening of the ring was, therefore, made considerably larger than any stimulus we wished to use in order to avoid the introduction of this factor. (*b*) The large circumference of the ring makes the apparatus available for investigating the effect upon sensitivity of varying the size of the stimulus.

mm. in diameter and with a focal length of 275 mm. It moves in a plane 2.5 cm. behind the stimulus-opening, hence the parallel rays of light entering it from the collimating lens (L_1) are brought to a focus on the pupil of the eye when in position 25 cm. behind the stimulus-opening. To the eye at this point, the lens, or as much of it as is visible through the stimulus-opening, is seen uniformly filled with light. That is, it is a well-known fact in physiological optics that when parallel rays of light are focused on the pupil of the eye, or more accurately perhaps, at the optic center of the refracting mechanism, by means of a double convex lens, the lens is seen by the eye as if uniformly filled with light. As the eye takes the different fixation points on the arm (*I–I′*) the light is kept focused on the pupil by slightly displacing by means of the rack and pinion adjustment the center of the lens in the direction in which the eye is turned in taking the new fixation.[1] The adjustment is quickly and easily made. In fact the rotary campimeter adapted to the spectroscope in the manner described in this paper presents little if any more difficulty of operation than it does when pigment papers are used as stimuli.

The device for lining up the eye with the stimulus-opening is also attached to the rotary collar (*H*) and is constructed as follows. A cross piece (*S*) 16 cm. long and 1.8 cm. wide is fastened by a screw with a milled head to one side of the collar (*H*) and is supported by a pin on the opposite side. A pin is used instead of a second fastening in order that the device may be conveniently and quickly turned out of the path of light when not in use. In the center of the cross piece (*S*) is a circular opening (*O*) 15 mm. wide. When in position immediately behind the opening in the campimeter screen which admits the stimulus light, the center of this opening lies in a line perpendicular to the opening in the screen at its central point. To the cross piece (*S*) 3 cm. to

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[1] In taking a new fixation the pupil is seen to turn from under the colored image and to the observer the stimulus-opening is no longer filled with colored light. As the center of the lens is shifted in the appropriate direction, however, the image is seen to travel towards the pupil, and when it falls full upon it, the observer again sees the stimulus-opening filled with light.

the side of the opening (*O*) is fastened at right angles an arm 12 cm. long and 1 cm. wide, terminating in a disc (*P*) 2 cm. in diameter. Three centimeters from its outer end this arm is bent at right angles so that the disc lies directly behind the opening (*O*) and in a plane parallel to that opening. The size and position of the stimulus-opening, the opening (*O*), and the disc (*P*), and their distances from each other sustain such relations that when the eye is in position 25 cm. behind the stimulus-opening with the center of the image of that opening on the fovea and the line of regard normal to the plane of the opening, the edge of the opening (*O*) is just contained within the stimulus-opening and the edge of the disc is just contained within the opening *O*. That is, in effect the device is a peep sight arrangement, and the alignment described above is possible only when the eye is at the center of curvature from which the fixation points on the arm (*I–I′*) are determined. As stated above, this attachment is fastened to the collar (*H*) by means of a screw with a milled head, so that after the alignment is made it can be readily turned out of the road and clamped. In order that the distance of the eye may be adjusted at the same time as its alignment is made with the stimulus-opening, a measuring device 25 cm. long is provided. This device consists of a slender brass rod fitted at either end with two short right angled arms 5 mm. in length. On the end of one of these arms is a ring which is just larger than the stimulus-opening, and on the other is a brass disc of the same diameter as the ring, provided at its center with a pupillary aperture. In adjusting the distance of the eye the disc is rested lightly against the forward surface of the eyeball and the ring against the campimeter screen concentric with the stimulus-opening. When the position of the eye is once determined, a mouthboard is adjusted and clamped in position so that the observer's teeth fit into impressions previously made and hardened in wax. This fixes the relation of the observer's eye to the campimeter system. All that is needed, therefore, at subsequent sittings to bring the eye into this relation is again to fit the teeth into the impressions on the mouthboard.

In order to facilitate excentric fixation in the nasal and temporal meridians, the head should be turned in adjusting the mouthboard 45° nasalwards or temporalwards as the case may be. With the head so placed, the eye can swing easily from the stimulus-opening to a fixation point whose excentricity exceeds 90°.

The front view of the campimeter in readiness for use may be seen in Fig. 2 of our former paper; a back view is given in Fig. 3 of this paper. A cardboard background has been fastened to the steel arms by means of metal fasteners pushed through holes in the steel arms and clinched. Since the background is fastened to the arms attached to the brass collar (*H*), a circular gap is left at its center. This gap is filled by a disc (*N*) shown in Fig. 3, which has been fastened to the arms just outside of the collar (*H*). The disc is 27 cm. in diameter and contains the stimulus-opening (*O*), the size of which may be varied to accord with the purpose of the investigation. In order to complete the graduations on the fixation arm to the stimulus-opening, disc (*N*) is graduated from 0°–18°. A background, 40 cm. in height, is fastened to the extension arms (*I′*). This background for screen and extension arm may be covered with whatever standard paper or surface that is desired.[1] The graduations from 0°–92° are pricked in this covering at points determined by the markings on the back of the disc (*N*) and the arm (*I–I′*). These constitute the fixation points.

The method of using this apparatus is as follows. The eye of the observer is lined up with the stimulus-opening by means of the attachment described above at a point 25 cm. from the campimeter screen; and the position of his mouthboard is adjusted. With his eye in this position the lens should fill uniformly with light whenever the image of the analyzing slit falls on the pupil. Before the color observations are begun this is tested out by taking a number of

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[1] In all tests of the relative and absolute sensitivity of the retina this screen should be made of a gray of the brightness of the color to be used. No departure from this rule should be permitted in tests of sensitivity unless it is for the purpose of determining the effect of different screens on sensitivity, or of using this effect as a means of varying sensitivity.

fixation points from 0° to the periphery of the field of vision and adjusting the focusing lens in each case to bring the image of the slit full upon the pupil of the eye. In making the color observation the unused eye is covered with a bandage. The arm (*I–I′*) is turned into the meridian to be investigated, the position being determined by the graduations on the collar (*H*). The experimenter inserts a card which we shall call the preëxposure card, between the eye and the stimulus-opening as near to the opening as possible while the observer takes the fixation required. In all investigations of relative and absolute sensitivity, this card should be made of a gray of the brightness of the color to be used. At a signal given by the observer the preëxposure card is withdrawn, the eye is exposed to the stimulus for the required length of time and the card is replaced in the path of the stimulus light. The observer is required to rest the eye after each observation. Further provisions against fatigue are made by frequent and regular intervals of rest.

It is often desirable to have an equation representing what is sensed at a given point in the peripheral retina in terms of what is sensed in the central retina. In this way a representation may be had for comparative purposes of the color tone, brightness and saturation for light of a given intensity and range of wave-length.[1] In the campimeter devised for use with pigment papers provisions were made for this. That is, it was possible to rotate a small disc on which the representation might be made, just behind the fixation point for all positions of the point from 0°–90° in any meridian. This disc was rotated by a small motor the shank of which protruded through a slit 8 mm. wide running the full length of the fixation arm (*I–I′*). For a further description of this motor, its supports, adjustments, etc., and the method of making the match of the sensations aroused in central and peripheral retina, see the former

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[1] In making a comparative study of the sensitivity of the different parts of the retina, such an investigation is a valuable supplement to a survey of sensitivity made on the basis of threshold and just noticeable difference determinations. The two methods are needed in fact to give a complete representation of the sensitivity of the retina in its quantitative and qualitative aspects.

article, p. 452. In the rotary campimeter devised for use with the light of the spectrum no similar provision has as yet been made. The difficulties appending the attempt to get a spectrum light variable at will in intensity, color, and brightness which can be easily and conveniently presented to the eye at any point of any meridian of the field of vision are obvious at a glance. Dreher, for example,[1] was able to make a match of the sensations aroused at the center and periphery of the retina without very inconvenient changes in his apparatus for one point only in the peripheral retina. An attempt is being made to adapt our present apparatus so that this match can be accomplished conveniently for any point in the peripheral retina, but as yet nothing definite can be promised.

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[1] Dreher, *loc. cit.*

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THE FACULTY OF ATTENTION

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In order to justify the designation of attention as a faculty, it must be possible to demonstrate that the degree of attention of any individual at a given time depends upon certain factors that remain constant for that individual, no matter what the particular case of attention in question may be, that is, in spite of endless variation in the specific form or content of the mental processes involved. The problem is to determine whether there are conditions of attention which remain constant in all cases of attention of a given individual.

Different individuals, under apparently the same conditions, show differences in degree of attention. The individual, himself, that is, is one of the most important conditions. Of course, for the 'individual,' we may some day be able to substitute a more definite condition or list of conditions. What this list would include is at present not known, but merely as a suggestion of the meaning of these general, individual conditions, we might surmise such things as the general cortical structure or certain features thereof; the number of association fibers; the frontal lobes[1] or some function thereof; 'mental constitution'[2] or organization; or 'intellective energy.'[3] Whatever goes into the list of conditions which make individuals necessarily different with respect to degree of attention in spite of all that can be done to make

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[1] Wundt, 'Physiologische Psychologie,' 6th ed., 1908, Vol. I., 378.

[2] Binet, 'Attention et adaptation,' *Anneé psychol.*, 1899, 6, 395.

[3] Hart and Spearman, 'General Ability, Its Existence and Nature,' *Brit. Journ. of Psychol.*, 1912, 5, 70.

the conditions of attention the same for them, or to allow for unavoidable inequality in the conditions, may be called the general, individual conditions. The question of a faculty of attention is merely the question whether there are any such general conditions. For any case of attention, we may write the perfectly general formula,

$$A = f(c_1, c_2, c_3, \ldots, c_n),$$

in which A represents degree of attention in any case with any individual, and $c_1, c_2, c_3, \ldots, c_n$, conditions of attention which vary in different cases of attention of the same individual. The question now is, are we justified, as long as we are dealing with the same individual, in writing this formula as

$$A = f(k, c_1, c_2, c_3, \ldots, c_n),$$

in which k is a constant which varies with the individual, but not with different cases of attention of the same individual.

In view of the great complexity of the conditions of attention and the apparently uncontrollable nature of many of them, the problem might at first thought seem impossible of direct solution. There would be no difficulty if all the c factors could be kept constant for a number of individuals; for, with these factors constant, any differences in A for different individuals would necessarily be due to differences in a factor, k. It is of course impossible to keep all the c factors constant. Most of them can not be controlled. Many, however, may be ruled out in any particular case as of negligible importance. For example, knowledge of chemistry may be a condition of attention to some topic of chemistry, but may be regarded as not affecting the degree of attention given to a tennis ball served by one's opponent. Under the particular conditions of the method of measuring attention employed in the present work, many of the conditions of attention may be regarded as ruled out. Many more may be kept approximately constant. It may be possible, consequently, to arrange a case of attention such that if the differences between individuals are due to the more or less specific c factors, they must be due chiefly to variations in one, or one very narrow set, of *c* factors, which one or set may be altogether different in another case of attention. Now if it is possible to arrange several such cases of attention, and if we find the same individual differences in all of the several cases, these individual differences cannot reasonably be supposed to be due to differences in the *c* factors; for the latter, if to any degree specific, should show little or no correlation, certainly not perfect correlation, and individual differences due to the *c* factors should therefore vary with the use of different *c* factors. It will be shown that the conditions of attention may be controlled experimentally in such a way that, in spite of variation in certain *c* factors, and consequent effect upon the absolute degree of attention of the subjects, the individual differences in degree of attention remain relatively the same. Evidently, then, the individual differences are due to variations in a general factor, *k*, which varies only with the individual.

To make sure that the conditions necessary for this direct demonstration of the *k* factor were obtained, it is necessary to examine carefully the four different cases of attention used in the present research. Three of the cases are those of attention to a touch, sound and light, each of moderately strong intensity, with the purpose of reacting as quickly as possible. The fourth case of attention is that which occurs in a choice reaction experiment, in which the subject reacts with his right hand, if a light already visible suddenly becomes brighter, and with his left, if it becomes less bright. If the large differences found to exist between the individuals are not due to differences in an individual constant *k*, they must be due to inequality for the different individuals in some of the *c* factors. While it is impossible to discuss in detail all the *c* factors, a careful consideration of the more obviously important ones will be sufficient to make it clear that the individual differences could not have been due to inequality in any other *c* factors than those which varied in the four different cases; and the data, showing that the individual differences remain the same in these four cases, eliminate even these factors as the cause of the individual differences.

In each of the four specific cases of attention used, it is evident that all the objective conditions could be kept constant for all individuals. This includes such factors as intensity, mode and quality of stimulus, the nature of the warning signal, and the duration of the preparatory intervals. With careful instructions, such factors as zeal, interest and purpose may be regarded as constant in all four cases, and as differing between different individuals only in so far as they are general, individual factors.

Familiarity may perhaps be regarded as constant for all subjects, since every one may be regarded as maximally familiar with touch, sound and light stimuli; but in any event, this factor is not pertinent, since results already published show that degree of attention, under the circumstances of the present measurements, does not vary with practice.[1] This conclusion is confirmed by the data presented below (in Table I.). Now, if a person shows the same degree of attention the first few times he is measured as at the end of a long series of measurements, familiarity with the conditions is evidently a negligible factor.

Marked variations in sensory sensitivity of different individuals would no doubt affect the degree of attention in any of the four cases used. I have already conducted a study of means whereby this factor may be excluded. The conclusion was reached, that by the use of reactions to a *change* in intensity, attention can be measured independently of wide variations in sensory sensitivity.[2] This method was not used in the present instance, however, since it seemed highly probable that, with moderately strong intensities of stimuli, as long as fairly normal sensitivity was secured, slight variations in sensitivity would not seriously affect the results. That this surmise was correct is proved by the results obtained which show that the effect of mode of stimulus may, without serious error, be regarded as a constant one in the subjects used.

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[1] Woodrow, 'The Measurement of Attention,' *Psychol. Monog.*, 1914, Vol. XVII., 5, 122–142.

[2] *Op. cit.*, Chap. IV.

Another set of specific factors which should be considered are differences which, in spite of careful and identical instructions, may have existed in the direction of attention. In all cases, the instructions were to react as quickly as possible, and to take the fingers off the key as quickly as possible, when the stimulus occurred. It is evident, however, that considerable room for variations in attentional attitude is left by any instructions. Attention can be more or less unequaly directed to the stimulus, the reaction movement and the idea of reacting as quickly as possible. These variations probably do constitute slight and variable sources of error in the measurements made, but it is quite certain that they are of no considerable importance. I made a long series of trials, several years ago, with myself and one other subject, in which I studied the effect of directing attention on some days as much as possible towards the light used as stimulus and on other days as much as possible towards the reaction movement. In all cases, every reaction was made as quickly as possible. I could find no reliable difference in the degree of attention or even in the simple reaction time, and concluded, in agreement with Ach, that the only very fundamental distinction in reactions from the point of view of reaction time is that which divides them into those which occur with the endeavor to react as quickly as possible and those which occur without such endeavor.[1] As experimental evidence of the negligible importance of variations in direction of attention, may be cited the results presented below[2] which show that about the same individual differences in degree of attention exist with a choice reaction as with a simple reaction. Since the whole attitude in a choice reaction is different from that in a simple reaction, it seems hardly probable that the individual differences in attentional attitude would remain the same in both cases.

Preparedness of motor centers for action, which some writers, in spite of severe criticism,[3] emphasize so strongly as

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[1] *Op. cit.*, 21.

[2] See Table VI.

[3] Moore, T. V., 'A Study in Reaction Time and Movement,' *Psych. Rev. Monog. Sup.*, 1904, Vol. VI., 1, 62–72. Pillsbury, 'The Place of Movement in Consciousness,' *Psychol. Rev.*, 1911, 83–99.

a condition of attention, might be alleged as the factor which determines individual differences in the present work; and it might be argued, further, that this capacity for motor adjustment was specific, that is, that it determines the ranking of attentions in the cases here in question, where attention is given for the purpose of reacting, but might not do so in other cases. In the present work, which measures attention as it occurs in conjunction with the purpose of reacting as quickly as possible, the factor of preparedness for action is conspicuously present. Indeed, to say that the stimulus meets with a high degree of preparedness to react is here taken to mean that it meets with a high degree of attention. But in what does the preparation to react consist? Does it consist in preparation of the *motor* centers or in a general cortical adjustment? That the motor centers are not actually innervated during the preparatory interval is evident from the fact that, with practiced subjects, the hand rests quietly on the reaction key, without even an increase in muscular tonus. The preparation for a given act does not consist, then, in innervation of the motor centers which control that act. To postulate any sort of 'openness' or preparedness other than actual innervation is to engage in mere speculation, to raise many problems and to solve none. That the preparatory state involves contraction of other muscles than those to be used in the reaction movement, such as those involved in breathing and in the adjustment of the sense organ for the proper reception of the stimulus, is known; but these adjustments are involved in any act of attention about as much as in the cases used in the present work. Moreover, they vary in each of the four cases used. They are not the same in a reaction to sound as in a reaction to light. It is evident, consequently, that the individual differences in attention found in the present investigation can not be due to some specific capacity for keeping 'open' certain motor centers.

I have elsewhere discussed at length what we must suppose to be the nature of the preparation to react if we are to explain the leading facts of reaction.[1] The conclusion reached, from

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[1] 'Reactions to the Cessation of Stimuli and Their Nervous Mechanism,' *Psychol. Rev.*, 1915, 445–452.

the study of both beginning and cessation reactions, was that the preparation to react must consist in a widespread system of adjustments affecting the cerebral cortex as a whole. As a secondary effect of this adjustment, certain adjustments of the sense organs, the muscles concerned in breathing and other muscles may occur, but not any actual innervation of the motor response for which preparation is made. Preparation for an act, then, does not mean, primarily, a preparation of the motor area which controls that act. It is a disposition of the whole cortical system, that is, it is 'pre-motor.' One hardly needs to raise the question whether the adjustment of the nervous system in preparation for a reaction is a condition or a result of attention. The adjustment is the physiological parallel of attention.

From this point of view, we may state the problem of a faculty of attention in physiological terms. The act of attention involves a widespread cortical adjustment. Is there any general factor that comes in as a constant which helps to determine the efficiency of the adjustment in spite of variation in its type? That the conditions of the present research are satisfactory for a study of this problem is evident, for each of the four cases used clearly involves a widely different type of adjustment. The adjustment varies in pattern with the mode of stimulus to which the reaction is to occur, and is different in a simple reaction from what it is in a choice reaction. If, in spite of this variation in type of cortical adjustment, we find the same individual differences in its efficiency in all the four cases used, it must be concluded that these differences are general, individual differences.

Any individual differences in such specific factors as muscular strength or the nature of the reaction movement would not at all affect such measurements as the present ones, since they consist, not in reaction times, but in differences between two sets of reaction times of the same individual. Such individual peculiarities would, therefore, constitute a constant which would not affect a difference in reaction times any more than would a constant error in the chronoscope.

Without further discussion, it may be said that all the *c* factors were the same for all subjects, unless the subjects differed in specific attention capacity in the four cases of attention used. The primary object of the present investigation is just to determine whether individuals do differ in specific attention capacities, that is, whether an individual may have good attention in the case of one type of mental process, and a relatively inferior one in another case, all conditions other than the type of mental process remaining constant. Of course, if other conditions of attention than the type of mental process were changed, and changed unequally for different individuals, in passing from one case to another, we would not expect a given individual to maintain the same standing in different cases. But it might be supposed, for example, that an individual would have good attention for a light stimulus and poor attention for a touch stimulus, even though no other differences in the conditions of attention existed in the two cases than that one was attention to light and the other attention to touch.

Since three of the four cases of attention used in the present investigation were produced by the use of three different modes of stimulus, the investigation may in large part be regarded as a study of mode of stimulus as a condition of attention. The case of attention with choice reactions was not studied so intensively, and only with ten of the twelve subjects. Consequently, the results with the three modes of stimuli will be presented first, and those with choice reactions afterwards, as supplementary data.

It has long been established that reaction time varies with the mode of stimulus, that at moderately strong intensities of stimulus the reaction time for sound is slightly longer than for touch and shorter than for light. Various explanations have been offered. The commonest view has been that the variation in reaction time with mode of stimulus is due to variation in the latent period of sensory stimulation. I have recently pointed out, however, that this view is untenable. Reaction time to the cessation of a stimulus equals that to its beginning, no matter what the mode of stimulus; and the latent period of stimulation cannot account for the fact that *both* the beginning and the cessation reactions are shorter in the case of touch and sound than in the case of light. For this reason, as well as others, I have urged that the only acceptable explanation of the difference in reaction time with difference in mode of stimulus is to be found in the rôle played by the processes of the central nervous system, especially those which may be presumed to form the physiological aspect of attention. In confirmation of this position, I referred to unpublished results which showed that the degree of attention attracted by a light stimulus was less than that attracted by either a sound or touch stimulus.[1] These results are herewith presented.

The method used for measuring attention may be designated a *detraction* method, since it takes as the measure of degree of attention the effect produced by a detractor. A detractor is any condition or circumstance unfavorable to a high degree of attention. The detraction effect here used as the measure of degree of attention is that produced in reaction time by a set of unfavorable preparatory intervals, a set of irregularly mixed intervals varying in length from 4 to 20 seconds. The average reaction time with such a set of intervals is invariably greater than with a regularly repeated preparatory interval of 2 seconds; and the difference in reaction time obtained with the two sorts of preparatory intervals is a measure of the detraction effect. That this detraction effect is due solely to the fact that the unfavorable intervals allow a lesser degree of adaptation of attention to the reaction, is not only highly plausible from the absence of other explanation, but is a proposition which I have been able abundantly to confirm by showing that whenever the degree of attention was varied by varying any of its determinants, the magnitude of the detraction effect likewise varied.

That the detraction effect increases whenever the degree of attention decreases, has been shown by variation in a number of conditions of attention.[2] Thus, a decrease in the

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[1] 'Reactions to the Cessation of Stimuli and Their Nervous Mechanism,' *Psychol. Rev.*, 1915, 444.

[2] Woodrow, 'The Measurement of Attention,' *Psychol. Monog.*, 1914, Vol. XVII., 5, pp. 158.

intensity of the stimulus, the most important of the objective conditions of attention, causes a marked increase in the prolongation in reaction time produced by the unfavorable intervals. Similarly, a decrease in the size of the change in intensity, in the case of reactions to a change, causes an increase in the detraction effect. An increase is likewise produced by ordinary distractors, such as unpleasant electrical shocks given the subject at the same time that he is reacting; by requiring the subject to carry on simultaneously some other task; or by having him imbibe of alcoholic liquor. The detraction effect is very much greater in children than in adults, a point on which additional data is presented below. In view of these results, there seems little room for doubt that degree of attention is measured by the reciprocal of the absolute detraction effect. Moreover, in a special study of 'Outline as a Condition of Attention,'[2] I have shown that the conclusions arrived at by the use of the detraction method completely correspond with those reached by means of introspections of clearness by practiced subjects.

The conditions of attention, to the study of which I have so far applied this method, it will be observed, are among those which were already best established. Such conditions were taken up first, as my interest was mainly in the method. The results, however, have been so uniformly corroborative of the validity of the method employed, that it seems safe to undertake now the study of conditions which it has hitherto been impossible to study for the lack of a suitable method. The effect of mode of stimulus has been taken up first, because of its bearing upon the question of a single faculty of attention, one of the most fundamental psychological questions now pressing for solution.

The intensities of stimuli used were probably about what are ordinarily regarded as moderate. They were not measured, but were kept as constant as possible throughout the work. It is known that, as long as the intensity of stimulus remains moderately strong, slight variations in intensity produce no reliable difference in reaction time; and when there

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[1] This Journal, Vol. I, 1916, 23–40.

is no difference in reaction time, all conditions remaining the same, there is in all probability no difference in the degree of attention. Any variations that could have occurred in the intensity of either the sound or light stimuli must have been very slight indeed, as such were never detected. The sound stimulus consisted in the click of an electric sound hammer (Wundt's), and the light stimulus in the flash of a Geissler's tube. As the electric current was supplied in both cases from a storage battery of 12 volts, there was no difficulty in maintaining a constant intensity. In the case of the touch stimulus, however, some difficulty was experienced. This stimulus consisted in an electric current, interrupted 250 times per second, led off from the secondary coil of an inductorium, to two large test-tubes filled with salt water, in each of which the subject placed a finger of his left hand. The interruptions of the current were made by means of an electric tuning-fork with platinum contact, and the intensity of the current varied considerably with the adjustment of the fork. While necessary readjustments of the fork were made with the greatest care, and at each sitting the subjects were asked whether the current seemed the same as usual, there is no doubt, especially when variations in the bodily resistance are considered, that only an approximate constancy was maintained.

As an index of the intensities of the stimuli used, the average reaction time with the regularly repeated 2 seconds interval, for the four quickest adults, may be cited. The average for touch was 125 σ; for sound, 144 σ; and for light, 175 σ. These times are perhaps rather long for practiced subjects, but this is readily accounted for by the fact that the subjects had to react first to one kind of stimulus and then to another, and first with regular preparatory intervals and then with irregular. This variation in the type of their task no doubt tended to obliterate the increase in speed that would normally result from doing the same thing day after day.

With each mode of stimulus, 25 reactions with a regularly repeated preparatory interval of 2 seconds were taken first and then 25 with irregular preparatory intervals. The 50 reactions required about 20 minutes. Then the experiment was repeated with a different stimulus. At one sitting of an hour, 150 reactions were taken, making three separate measurements of attention. Not more than two of these measurements were the measurements at present under discussion, that is, measurements of degree of attention in the case of reactions to moderately strong touch, sound and light stimuli. This is due to the fact that work was carried on at the same time on the degree of attention with choice reactions and with a weak light stimulus. The results with choice reactions will be presented later. Those with the weak light merely confirmed others already published, and will not be presented. The order in which the different measurements were made varied each day, the object being to take each kind of reaction about equally often at the beginning, middle, and end of the hour and to have each kind of reaction preceded in turn by each of the others. This constant change in each day's program undoubtedly accounts for a good deal of the variability of the measurements made on different days, and makes it impossible to reckon with any reliability to what extent the variation from day to day in the measurements obtained represents a true variation in degree of attention. Measurements with different modes were taken either on different days or at different places on the same day's program, and consequently it is not surprising that no reliable correlation was found to exist in their variation. The measurements with each mode may be safely regarded as independent series of measurements.

Twelve subjects were used. Seven of these were adults; four, subjects *Ws*, *Hl*, *Ss* and *Sy*, were boys of twelve to fifteen years of age, and one, subject *Je*, was a boy of eight years. Of the adults, four had had previous work in reaction time. These were subjects *St*, *Ww*, *Sl*, and *Sm*. An endeavor was made to obtain nine or ten measurements of degree of attention with each subject with each mode of stimulus. With two subjects, however, a smaller number of measurements had to suffice, while with two other subjects twelve measurements were made.

The reaction times were measured by a Hipp's chronoscope, controlled by a Wundt's fall-hammer. It seems unnecessary to describe the apparatus in detail, as it was in general like that already described in other work, and followed in the main the conventional reaction time arrangement. The warning signal was in all cases a click of a small sound hammer. The interval between warning signal and stimulus was judged by means of a head-receiver connected with an electric metronome beating seconds. The 2 seconds intervals were given as accurately as possible, and the whole series of reactions run off regularly and quickly. The experimental work occupied the whole of one summer, the total number of reactions here reported being 19,350. An assistant acted as experimenter in about one third of the work and also computed most of the averages and mean variations.[1]

The results with the different modes of stimuli are presented in Tables I. and II. Table I. gives the results of each measurement with each subject. As already explained, all three modes were never used at the same sitting, so that the three measurements given on the same line of the table were not made during the same hour. The measurements with each mode, however, are listed in the order in which they were obtained. The average reaction times with the 2 seconds intervals and with the irregular intervals are given in σ, and the absolute mean variation of each average given immediately below it in italics. The difference in the reaction time obtained with the two kinds of preparatory intervals is given in the columns headed $1/A$. This heading signifies that the greater this difference the less the degree of attention, A standing for degree of attention. Table II. is a summary of Table I.

Tables I. and II. establish certain conclusions beyond question. For every individual, the prolongation in reaction time produced by the irregular intervals, that is, the magnitude $1/A$, is greater with light than with either sound or touch. Since this prolongation varies inversely with the

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[1] Assistance in this work was secured through a grant from the Research Fund of the University of Minnesota.

Table I

Measurements of Attention with Different Modes of Stimuli

N, for each average, = 25. Total No. of reactions = 17,250.

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*
St	119	169	50	124	163	39	166	219	53
	7	*10*		*11*	*26*		*15*	*18*	
"	114	163	49	150	198	48	164	210	46
	7	*12*		*10*	*28*		*27*	*18*	
"	111	160	49	154	206	52	162	209	47
	8	*19*		*17*	*30*		*18*	*20*	
"	123	168	45	142	174	32	174	216	42
	10	*13*		*11*	*16*		*17*	*15*	
"	125	174	49	155	202	47	164	219	55
	8	*17*		*13*	*29*		*15*	*17*	
"	117	159	42	148	210	62	183	226	43
	6	*8*		*13*	*30*		*22*	*22*	
"	128	175	47	157	216	59	172	221	49
	9	*14*		*9*	*25*		*21*	*13*	
"	138	170	32	152	200	48	189	248	59
	9	*8*		*17*	*20*		*14*	*21*	
"	133	171	38	171	202	31	177	229	52
	10	*8*		*13*	*17*		*18*	*18*	
"	146	176	30	180	237	57	202	244	42
	9	*11*		*13*	*14*		*23*	*14*	
"	149	180	31	165	217	52	191	244	53
	10	*12*		*12*	*12*		*13*	*13*	
"	138	183	45	170	215	45	182	247	65
	11	*15*		*15*	*16*		*18*	*18*	
Av.	**128**	**170**	**42**	**155**	**203**	**48**	**177**	**228**	**51**
Av. M. V.	*9*	*12*		*13*	*22*		*18*	*17*	
Ww	120	182	62	138	197	59	162	228	66
	13	*18*		*19*	*25*		*18*	*21*	
"	112	178	66	131	208	77	172	237	65
	13	*21*		*11*	*25*		*14*	*18*	
"	115	154	39	120	197	77	185	254	69
	16	*24*		*21*	*17*		*13*	*36*	
"	131	174	43	140	188	48	169	231	62
	11	*14*		*20*	*26*		*16*	*24*	
"	115	151	36	142	177	35	166	246	80
	10	*16*		*14*	*15*		*15*	*22*	
"	118	169	51	132	167	35	163	235	72
	17	*13*		*17*	*15*		*13*	*18*	
"	119	155	36	136	185	49	162	231	69
	9	*13*		*16*	*10*		*15*	*18*	
"	105	142	37	141	176	35	163	241	78
	7	*12*		*8*	*24*		*10*	*19*	
"	111	149	38	130	199	69	166	248	82
	12	*15*		*10*	*20*		*10*	*21*	
"	110	151	41	128	194	66	165	228	63
	11	*14*		*12*	*18*		*12*	*19*	
Av.	**116**	**161**	**45**	**134**	**189**	**55**	**167**	**238**	**71**
Av. M. V.	*12*	*17*		*15*	*20*		*14*	*22*	

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*
Ll	123	168	45	149	192	43	177	212	35
	24	*19*		*23*	*15*		*9*	*34*	
“	128	178	50	140	176	36	166	215	49
	15	*17*		*8*	*26*		*17*	*14*	
“	135	171	36	144	186	42	172	222	50
	6	*10*		*10*	*39*		*10*	*23*	
“	125	165	40	137	181	44	169	221	52
	21	*22*		*13*	*24*		*10*	*29*	
“	127	177	50	141	185	44	178	226	48
	12	*25*		*16*	*25*		*16*	*10*	
“	128	183	55	146	180	34	163	231	68
	22	*23*		*7*	*26*		*15*	*22*	
“	120	182	62	152	195	43	172	225	54
	23	*16*		*25*	*16*		*12*	*23*	
“	140	167	27	136	170	34	159	217	58
	13	*14*		*12*	*10*		*9*	*26*	
“	113	161	48	130	172	42	171	225	54
	9	*13*		*11*	*25*		*9*	*14*	
“	121	158	37	138	175	37	172	227	55
	10	*12*		*12*	*17*		*12*	*15*	
Av.	**126**	**171**	**45**	**141**	**181**	**40**	**170**	**222**	**52**
Av. . M. V.	*16*	*17*		*14*	*22*		*12*	*21*	
Ht	145	215	70	146	204	58	194	258	64
	23	*26*		*18*	*18*		*23*	*38*	
“	165	227	62	181	254	73	212	281	69
	19	*22*		*26*	*24*		*38*	*25*	
“	180	241	61	194	254	60	220	278	58
	23	*21*		*11*	*34*		*17*	*17*	
“	165	209	44	211	273	62	197	244	47
	13	*27*		*19*	*19*		*22*	*26*	
“	151	202	51	169	219	50	207	280	73
	23	*23*		*22*	*33*		*20*	*38*	
“	167	226	59	154	214	60	182	246	64
	13	*21*		*14*	*16*		*26*	*35*	
“	132	190	58	155	199	44	207	266	59
	12	*15*		*16*	*15*		*16*	*25*	
“	165	199	34	185	221	36	187	237	50
	18	*14*		*16*	*17*		*18*	*10*	
“	139	181	42	150	198	48	206	273	67
	8	*11*		*12*	*18*		*11*	*25*	
Av.	**157**	**210**	**53**	**171**	**226**	**55**	**201**	**262**	**61**
Av. M. V.	*16*	*20*		*17*	*22*		*21*	*26*	
Bs	139	216	77	154	211	57	196	287	91
	24	*28*		*18*	*28*		*15*	*25*	
“	135	208	73	138	211	73	210	278	68
	12	*20*		*25*	*28*		*13*	*25*	
“	126	184	58	146	211	65	188	258	70
	8	*10*		*13*	*26*		*10*	*18*	
“	127	182	55	125	180	55	182	256	74
	10	*12*		*14*	*17*		*12*	*20*	
“	129	187	58	160	215	55	182	260	88
	10	*18*		*15*	*26*		*17*	*20*	
“	131	185	54	148	218	70	171	249	78
	12	*17*		*9*	*18*		*12*	*22*	
“	122	194	72	139	222	83	168	262	94
	12	*21*		*16*	*23*		*12*	*21*	
Av.	**130**	**194**	64	**144**	**209**	65	**185**	**264**	**79**
Av. M. V.	*13*	*18*		*16*	*24*		*13*	*23*	

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*
Bt	151	227	76	149	228	79	212	279	67
	20	*31*		*18*	*48*		*37*	*33*	
“	169	219	50	189	253	64	209	308	99
	21	*21*		*22*	*38*		*32*	*35*	
“	169	233	64	176	233	57	221	286	65
	12	*26*		*25*	*23*		*24*	*26*	
“	194	241	47	166	217	51	209	287	78
	33	*24*		*10*	*22*		*22*	*35*	
“	167	224	57	162	243	81	190	278	88
	10	*31*		*14*	*29*		*25*	*29*	
“	178	233	55	166	211	45	195	294	99
	18	*26*		*13*	*18*		*15*	*34*	
“	149	235	86	151	217	66	212	288	76
	15	*25*		*12*	*26*		*30*	*30*	
“	154	207	53	153	234	81	205	261	56
	15	*18*		*11*	*21*		*11*	*15*	
“	151	207	54	163	236	73	216	274	58
	20	*14*		*10*	*16*		*15*	*26*	
“	149	197	48	159	226	67	208	296	88
	9	*13*		*16*	*20*		*14*	*23*	
Av.	**163**	**222**	**59**	**165**	**231**	**66**	**208**	**285**	**77**
Av. M. V.	*17*	*26*		*15*	*26*		*23*	*29*	
Lm	144	214	70	148	237	89	209	304	95
	12	*31*		*13*	*19*		*25*	*26*	
“	146	233	87	170	248	78	230	328	98
	13	*30*		*14*	*24*		*23*	*42*	
“	166	239	73	173	282	109	224	321	97
	16	*17*		*28*	*45*		*22*	*32*	
“	151	247	96	186	281	95	240	330	90
	25	*34*		*29*	*36*		*14*	*26*	
“	144	222	78	168	260	92	230	323	93
	21	*26*		*21*	*29*		*19*	*28*	
“	172	259	87	198	290	92	209	301	92
	27	*53*		*23*	*30*		*14*	*27*	
“	157	254	97	184	279	95	240	328	88
	15	*28*		*25*	*49*		*26*	*42*	
“	153	247	94	185	282	97	210	310	100
	28	*18*		*21*	*44*		*16*	*26*	
“	145	230	85	178	269	91	217	323	106
	11	*20*		*31*	*41*		*17*	*30*	
“	152	242	90	172	278	106	178	279	101
	14	*27*		*21*	*27*		*32*	*24*	
“	151	255	104	172	272	100	210	314	104
	26	*23*		*18*	*28*		*21*	*23*	
“	127	210	83	174	260	86	226	327	101
	12	*17*		*26*	*24*		*35*	*29*	
Av.	**150**	**237**	**87**	**175**	**269**	**94**	**219**	**316**	**97**
Av. M. V.	*18*	*27*		*23*	*33*		*22*	*30*	

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*
Ws	161	248	87	180	278	98	223	341	118
	27	*52*		*12*	*46*		*40*	*73*	
“	154	251	97	178	228	50	224	306	82
	22	*41*		*25*	*25*		*22*	*33*	
“	170	230	60	156	238	82	197	303	106
	26	*30*		*18*	*33*		*15*	*44*	
“	151	235	84	145	204	59	214	304	90
	14	*43*		*19*	*29*		*26*	*41*	
“	146	227	81	148	221	73	195	278	83
	29	*44*		*15*	*45*		*27*	*28*	
“	122	205	83	151	232	81	219	302	83
	19	*26*		*18*	*45*		*25*	*35*	
“	134	208	74	142	223	81	219	304	85
	15	*31*		*16*	*35*		*32*	*40*	
“	130	231	101	138	240	102	198	306	108
	9	*34*		*17*	*43*		*16*	*44*	
“	133	244	111	137	246	109	202	318	116
	13	*48*		*14*	*35*		*18*	*29*	
“	130	206	76	134	218	84	213	306	93
	15	*26*		*19*	*27*		*31*	*39*	
Av.	**143**	**228**	**85**	**151**	**233**	**82**	**210**	**306**	**96**
Av. M. V.	*19*	*38*		*17*	*36*		*25*	*41*	
Ls	163	277	114	162	249	87	198	308	110
	25	*37*		*26*	*30*		*23*	*44*	
“	151	253	102	158	264	106	244	365	121
	14	*36*		*27*	*41*		*48*	*42*	
“	172	277	105	159	280	121	195	329	134
	21	*56*		*16*	*42*		*25*	*40*	
“	184	259	75	185	289	104	239	390	151
	27	*26*		*25*	*53*		*23*	*55*	
“	166	254	88	180	276	96	224	337	113
	10	*27*		*25*	*46*		*34*	*44*	
“	172	252	80	172	255	83	227	354	127
	15	*24*		*24*	*25*		*48*	*42*	
“	176	244	68	192	319	127	249	375	126
	22	*24*		*30*	*36*		*35*	*52*	
“	192	250	58	184	259	75	231	359	128
	24	*34*		*28*	*32*		*20*	*23*	
“	180	293	113	175	304	129	200	309	109
	12	*48*		*23*	*50*		*20*	*26*	
“	157	251	94	153	267	114	214	322	108
	12	*33*		*15*	*31*		*22*	*34*	
Av.	**171**	**261**	**90**	**172**	**276**	**104**	**222**	**345**	**123**
Av. M. V.	*18*	*35*		*24*	*39*		*30*	*40*	
Je	253	392	139	268	461	193	285	462	177
	34	*43*		*39*	*74*		*39*	*115*	
“	229	397	168	259	423	164	256	451	195
	24	*68*		*18*	*37*		*44*	*62*	
“	200	372	172	203	361	158	242	459	217
	26	*54*		*34*	*53*		*45*	*63*	
“	198	312	114	225	402	177	269	440	171
	15	*58*		*28*	*41*		*48*	*47*	
“	196	338	142	214	395	181	289	435	146
	19	*47*		*29*	*53*		*45*	*77*	
Av.	**215**	**362**	**147**	**233**	**408**	**175**	**268**	**449**	**181**
Av. M. V.	*23*	*54*		*29*	*51*		*44*	*73*	

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg	1/*A*	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*
Hl..............	164	275	111	157	274	117	211	331	120
	14	*42*		*19*	*54*		*25*	*44*	
".............	156	232	76	164	282	118	210	316	106
	10	*26*		*21*	*59*		*12*	*45*	
".............	154	260	106	174	254	80	199	294	95
	11	*38*		*27*	*48*		*27*	*38*	
".............	173	245	72	161	228	67	214	321	107
	21	*31*		*27*	*30*		*14*	*46*	
".............	160	287	127	177	326	149	234	350	116
	14	*47*		*27*	*90*		*26*	*48*	
".............	195	300	105	161	291	130	229	361	132
	22	*50*		*26*	*70*		*24*	*52*	
".............	156	264	108	167	252	85	226	388	162
	13	*30*		*16*	*69*		*35*	*52*	
".............	166	249	83	188	324	136	233	399	166
	15	*35*		*19*	*33*		*18*	*41*	
".............	187	264	77	163	319	156	215	387	172
	17	*31*		*20*	*38*		*22*	*44*	
Av..............	**168**	**265**	**97**	**168**	**283**	**115**	**219**	**350**	**131**
Av. M. V........	*15*	*36*		*22*	*55*		*23*	*46*	
Sy..............	220	336	116	186	327	141	255	386	131
	29	*46*		*17*	*43*		*27*	*60*	
".............	192	313	121	208	334	126	228	385	157
	24	*50*		*16*	*49*		*23*	*85*	
".............	212	340	128	233	432	199	258	409	151
	56	*60*		*31*	*53*		*38*	*64*	
".............	156	258	102	191	330	139	131	387	156
	13	*34*		*23*	*46*		*28*	*53*	
".............	158	316	158	187	335	148	222	370	148
	18	*38*		*18*	*22*		*26*	*48*	
".............	158	269	111	182	325	143	232	351	119
	17	*43*		*20*	*37*		*29*	*36*	
".............	161	281	120	192	309	117	216	381	165
	15	*40*		*18*	*38*		*17*	*42*	
".............	191	343	152	185	332	147	215	368	153
	25	*23*		*22*	*53*		*22*	*39*	
".............	158	255	97	191	331	140	240	383	143
	15	*38*		*13*	*41*		*33*	*37*	
".............	165	298	133	189	323	134	225	373	148
	19	*39*		*19*	*43*		*21*	*40*	
Av..............	**177**	**301**	**124**	**194**	**337**	**143**	**232**	**379**	**147**
Av. M. V........	*23*	*41*		*20*	*43*		*26*	*50*	

TABLE II

A SUMMARY OF TABLE I

Subj.		Touch			Sound			Light		
		2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*
St....	Av........	128	170	42	155	203	48	177	228	51
	" M. V..	*9*	*12*		*13*	*22*		*18*	*17*	
Ll....	"	126	171	45	141	181	40	170	222	52
	" "	*16*	*17*		*14*	*22*		*12*	*21*	
Ww...	"	116	161	45	134	189	55	167	238	71
	" "	*12*	*17*		*15*	*20*		*14*	*22*	
Ht....	"	157	210	53	171	226	55	201	262	61
	" "	*16*	*20*		*17*	*22*		*21*	*26*	
Bs....	"	130	194	64	144	209	65	185	264	79
	" "	*13*	*18*		*16*	*24*		*13*	*23*	
Bt....	"	163	222	59	165	231	66	208	285	77
	" "	*17*	*26*		*15*	*26*		*23*	*29*	
Ws...	"	143	228	85	151	233	82	210	306	96
	" "	*19*	*38*		*17*	*36*		*25*	*41*	
Lm...	"	150	237	87	175	269	94	219	316	97
	" "	*18*	*27*		*23*	*33*		*22*	*30*	
Ls....	"	171	261	90	172	276	104	222	345	123
	" "	*18*	*35*		*24*	*39*		*30*	*40*	
Hl....	"	168	265	97	168	283	115	219	350	131
	" "	*15*	*36*		*22*	*55*		*23*	*46*	
Sy....	"	177	301	124	194	337	143	232	379	147
	" "	*23*	*41*		*20*	*43*		*26*	*50*	
Je....	"	215	362	147	233	408	175	268	449	181
	" "	*23*	*54*		*29*	*51*		*44*	*73*	

degree of attention, it follows that the degree of attention of all the subjects was less in the case of reactions to light than in the case of reactions to sound or touch. We may conclude, consequently, that just as we say that the reaction time to light is ordinarily longer than to sound or touch, so may we say that the degree of attention given to a light stimulus is ordinarily less than that given to a sound or a touch stimulus.

As regards the difference in degree of attention between touch and sound, the data are somewhat less uniform, but clearly show a general tendency for the degree of attention to touch to exceed that to sound. The average per cent. by which 1/*A* with sound exceeds 1/*A* with touch is almost as great as that by which 1/*A* with light exceeds 1/*A* with sound. There are, however, two individuals, subjects *Ll*, and *Ws*, with which degree of attention, as measured, is greater with sound than with touch. Two exceptions out of twelve is not sufficient to invalidate the conclusion that, as a general rule, attention to touch is better than to sound. By saying this is a general rule, it is meant that, if all errors of measurement and ignored causes of variability were ruled out, there would be no exceptions. The two exceptions may be regarded as natural in view of the variability of the measurements.[1] Two undoubted sources of this variability, which were not eliminated, were the variations, already referred to, in the intensity of the electric current used as the touch stimulus, and variations in the sensitivity of the sense organs. None of the subjects had any marked abnormalities of sensory acuity, but that differences great enough to affect the outcome of the measurements may have existed is rather probable. Subject *Ll*, one of the two who showed a better attention to sound than to touch, was known to have exceptionally acute auditory sensitivity, both from tests with the Lehmann acoumeter and from general observation, while, on the other hand, she frequently complained of the weakness of the electric current, and makes the statement that her fingers often become rather numb because, according to her physician, of poor circulation. For the other exceptional subject, *Ws*, who showed a value of $1/A$ of 83 with sound and 85 with touch, no explanatory data were obtained.

The preceding data show that the result obtained from the measurement of the degree of an individual's attention by the method here employed depends upon the mode of stimulus. Measurements made with different modes of stimuli are made, therefore, as it were, upon different scales. The question now arises, whether, after determining the relation between the three scales, and allowing for this difference in scale, a person's attention would be found to be equally good in all three cases. Of course, the allowance must be made in the same way for all individuals and must be based on the ascertained relationship between the three scales. If, after making such allowance, we find the degree of attention for each individual to be the same, we have definite proof of a common factor of attention, that is, of general conditions which remain the same for a given individual in spite of the variation in mode.

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[1] See Tables III. and IV.

Instead of attacking the problem by the method here proposed, that is, by making an allowance for the effect of mode, many psychologists would no doubt be inclined to use the method of correlation. The problem of a faculty of attention may be clearly stated in terms of correlation. If the correlation of degree of attention as involved in one type of mental activity, *e. g.*, in a touch reaction, with degree of attention in a different type, *e. g.*, in a sound reaction, were found to be perfect, we would have quite conclusive evidence that the degree of attention of any individual was determined in the two cases by the same factors. Stated in terms of rank, it is the problem whether each individual maintains the same rank in different cases of attention in spite of variation in the specific psycho-physical constitution of the different cases, *e. g.*, in spite of variation in mode of stimulus.

The present investigation, however, was not planned with the idea of treating the data by the method of correlation. Main reliance was placed on the method already mentioned, which is explained in detail below. It was thought preferable to use a relatively small number of subjects and make as accurate measurements as possible with each subject, repeating the measurements a sufficient number of times to obtain fairly reliable averages. Five youthful subjects were used in addition to the adults in order to secure a greater variation in degree of attention than would be found in even a very large group composed exclusively of normal adults. It did not seem particularly important to what the differences in attention were due, whether to growth or other factors. The important question was whether the individual differences in degree of attention would be found always the same. The research as planned, then, is somewhat unsuited to the application of correlational methods, both because of the limited number of cases and the fact that different ages were intentionally included. For those who are accustomed to correlational methods, however, it may be interesting to note the coefficients of correlation which may be obtained between the values of $1/A$ for sound, touch and light. The Pearson coefficients

$$\left(r = \frac{\Sigma(xy)}{n\sigma_x\sigma_y}\right)$$

are in all cases practically 1.00, and remain so when calculated for age constant, by the formula,[1]

$$r_{12.3} = \frac{r_{12} - r_{13} \cdot r_{23}}{\sqrt{(1 - r_{13}^2)\,(1 - r_{23}^2)}}.$$

The correlations are as follows:

r (sound and light) = 1.00; age constant = 1.00;
r (sound and touch) = .99; " " = .99;
r (touch and light) = .98; " " = .96.

The correlation, in the group used, between age and the reciprocal of degree of attention ($1/A$), in the case of touch was – .78; of sound, – .78; and of light, – .76. The fact that the correction for difference in age leaves the correlations of the values of $1/A$ for the three different modes practically unchanged, shows that the high correlations obtained are not due to difference in age. As regards the number of individuals, it is certain that the correlation obtained would remain approximately the same no matter what the number of subjects used, as it will be shown below that the degree of attention of any individual, no matter what his age, is the same, within the limits of the inaccuracy of measurement, in the case of all three modes, when reduced to the same scale.

The method used for equating the measurements made with the different modes is simple. It may be seen from Table III. that the average value of $1/A$ for the entire group of individuals is 78.2 for touch, 86.8 for sound, and 97.2 for light, that is, that the value of $1/A$ for the group is 11 per cent. greater for sound than for touch and 12 per cent. greater for light than for sound. These percentages for the group very nearly coincide with the average of the percentages in the case of each individual; and, though they show a large variation among the different individuals, there is no general tendency for them either to increase or decrease with increase in the value of $1/A$. It will, then, be a very close approxi-

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[1] Yule, 'An Introduction to the Theory of Statistics,' 1911, 247.

TABLE III

THE OBTAINED AVERAGE VALUES OF THE RECIPROCAL OF DEGREE OF ATTENTION WITH MEASURES OF THEIR UNRELIABILITY

$1/A$ = average value of reciprocal of degree of attention.
N = No. of measurements.
S.D. = standard deviation of the distribution.
σ_m = standard error of the mean.

Subj.	Touch			Sound			Light			N
	$1/A$	S.D.	σ_m	$1/A$	S.D.	σ_m	$1/A$	S.D.	σ_m	
St.........	42	7	2.1	48	9	2.7	51	7	2.0	12
Ll.........	45	10	3.1	40	4	1.2	52	8	2.5	10
Ww........	45	12	3.7	55	16	5.1	71	7	2.2	10
Ht.........	53	11	3.6	55	10	3.5	61	8	2.7	9
Bs.........	64	9	3.4	65	10	3.7	79	9	3.5	7
Bt.........	59	12	4.0	66	15	4.7	77	15	4.8	10
Ws........	85	14	4.4	82	17	5.5	96	14	4.3	10
Lm........	87	9	3.0	94	8	2.6	97	5	1.7	12
Ls.........	90	18	5.6	104	18	5.6	123	13	4.1	10
Hl.........	97	18	6.1	115	29	9.9	131	27	9.1	9
Sy.........	124	19	5.9	143	21	6.5	147	13	4.0	10
Je.........	147	21	9.4	175	13	5.7	181	23	10.1	5
Av........	78.2			86.8			97.2			

mation to the truth, to assume that $1/A$ tends in general, with the subjects used, to be 11 per cent. greater in the case of sound than in the case of touch and 12 per cent. greater in the case of light than in the case of sound.

The correction for mode has been made, therefore, on the basis of the following equations:

$$\frac{1/A\ \text{(sound)}}{1/A\ \text{(touch)}} = \frac{86.8}{78.2} = 1.11 \text{ and } \frac{1/A\ \text{(sound)}}{1/A\ \text{(light)}} = \frac{86.8}{97.2} = .89.$$

The results with sound have been kept as a standard and those with touch and light reduced to this standard by multiplying the final values of $1/A$ by 1.11 in the case of touch and by .89 in the case of light. Another way of putting this is to say that the results for each individual with any one mode have been divided by the average for all individuals, and thus reduced to the same scale; only in order to keep the results with sound as a standard, the results have then been multiplied by 86.8, the average result with sound. This procedure is similar to that which would be used in reducing to the same basis measurements made by different types of thermometers, providing the latter had the same zero point.

The results are presented in Tables III. and IV. It will be observed in Table IV. that, after allowance for the effect of mode has been made, there is a striking degree of similarity in the absolute values of $1/A$ for a given individual with the three different modes. The results for the three modes are not identical. It would be nothing short of a miracle if they were, in view of the variability of the measurements, which the original data, presented in Table I., show to be considerable. The question arises, therefore, whether the results obtained with the different modes of stimuli agree, after correcting for mode, as closely as could be expected, in view of the variability of the measurements compared, even if their true value were identical. To determine this, the standard error of each average (σ_m) has been calculated and a comparison made, in Table V., of the correspondence between the variation found in the averages of each individual with the variation that would be expected in view of the standard errors, provided the variation were due solely to

TABLE IV

THE DEVIATIONS OF THE OBTAINED AVERAGES FROM THE MOST PROBABLE TRUE AVERAGES, AFTER REDUCTION OF ALL THE OBTAINED VALUES TO A COMMON SCALE

The deviations are expressed as fractions of the most probable standard errors (Av. σ_m) of the most probable true values (Av. $1/A$), in the columns headed d/σ_m.

Subj.	Touch			Sound			Light			Av. $1/A$	Av. σ_m
	$1/A$	σ_m	d/σ_m	$1/A$	σ_m	d/σ_m	$1/A$	σ_m	d/σ_m		
St.....	47	2.3	0.00	48	2.7	+0.44	46	1.8	−0.44	47	2.3
Ll.....	50	3.4	+2.17	40	1.2	−2.17	46	2.2	+0.44	45	2.3
Ww...	50	4.1	−1.68	55	5.1	−0.27	63	2.0	+1.89	56	3.7
Ht....	59	4.0	+0.91	55	3.5	−0.30	54	2.4	−0.61	56	3.3
Bs....	71	3.8	+0.57	65	3.7	−1.14	71	3.1	+0.57	69	3.5
Bt.....	66	4.4	−0.23	66	4.7	−0.23	69	3.9	+0.47	67	4.3
Ws....	94	4.9	+1.49	82	5.5	−1.06	86	3.8	−0.21	87	4.7
Lm....	97	3.3	+1.60	94	2.6	+0.40	87	1.5	−2.40	93	2.5
Ls....	100	6.2	−0.96	104	5.6	−0.19	110	3.7	+0.96	105	5.2
Hl....	108	6.8	−0.60	115	9.9	+0.24	117	8.1	+0.48	113	8.3
Sy....	138	6.5	0.00	143	6.5	+0.91	131	3.6	−1.27	138	5.5
Je.....	163	10.4	−0.48	175	5.7	+0.95	162	9.0	−0.60	167	8.4

errors of sampling. Since a number of measurements of 1/*A* were made for each individual with each mode, the data permit the calculation of the standard error for each average value of 1/*A* given in Table III. By the standard error of the average[1] is meant the standard deviation of the distribution (S. D., Table III.) divided by the square root of the number of cases (*N*, Table III.).

TABLE V

A COMPARISON OF THE OBTAINED DISTRIBUTION OF THE VALUES OF 1/*A* WITH THE DISTRIBUTION TO BE EXPECTED IF THE VARIATION IN THESE VALUES WITH MODE OF STIMULUS (AFTER REDUCTION TO A COMMON SCALE) WERE DUE TO CHANCE

d/σ_m	Expected	Obtained
0.00 to ± 0.25	20%	22.2%
± 0.25 " ± 0.53	20	22.2
± 0.53 " ± 0.84	20	13.9
± 0.84 " ± 1.28	20	22.2
± 1.28 " ± 2.58	19	19.4
± 2.58 " ± ∞	1	0.0

In reducing the measurements made with different modes to a single scale (Table IV.), the standard errors of the averages have been treated like the averages, that is, each average and its standard error has been multiplied by 1.11 in the case of touch and by .89 in that of light. This treatment gives the same standard error as would multiplying each of the original measurements of 1/*A* for touch by 1.11 and for light by .89 and calculating the standard errors anew; so that no new assumption is made in correcting the standard errors of the averages in the same way as the averages. If we assume that, in the measurements with touch, sound and light, we are really measuring the same general power of attention under three different conditions, it is evident that the most probable true value of this power of attention for a given individual is the average of the values obtained in the three cases. This average is given in Table IV. in the column

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[1] Called by Thorndike the mean square deviation of the probable divergence of the true from the obtained average ($\sigma_{\text{t. av.} - \text{obt. av.}}$), 'Mental and Social Measurements,' 2d ed., 1913, 189.

headed "Av. $1/A$." Similarly, the most probable standard error of an average obtained for this true value, from the same number of measurements as used in the case of each mode, is the average of the standard errors in the three cases. For example, in the case of subject *St*, the three averages obtained for $1/A$, after eliminating the effect of mode, are given in Table IV. as 47, 48 and 46. The most probable true value of $1/A$, then, is 47. The standard errors of these averages, each based on twelve measurements, are 2.3, 2.7 and 1.8, respectively. The most probable standard error, then, for an average of twelve measurements[1] is the average of 2.3, 2.7 and 1.8, or 2.3. This most probable standard error is given in Table IV. in the column headed 'Av. σ_m.'

Now if the standard error of a mean of 12 measurements is 2.3, this implies that, if we determined independently a number of means each based on 12 measurements, these would tend to vary in such a way that 20 per cent. of them would not deviate from the average of all by more than $.025 \times 2.3$, 40 per cent. by more than 0.53×2.3, 60 per cent. by more than 0.84×2.3, etc.[2] It is easy to determine, therefore, whether the three obtained values of $1/A$ deviate from the most probable true value more than should be expected if the deviations were due merely to sampling. To determine this, the deviation of each average is expressed as a fraction of the most probable standard error (Table IV., columns headed d/σ_m). Altogether, there are thirty-six cases. It is true that these thirty-six cases are obtained by combining three cases only from each of twelve subjects. The deviations for each subject, however, are reckoned with respect to his own average in terms of the corresponding standard error. It is quite permissible, consequently, to group all cases together, to see to what extent the distribution of the obtained means corresponds with the distribution to be expected solely on account of the unreliability of the means.

Table V. shows that the distribution of the individual

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[1] For the formula for calculating the standard error for the total number of measurements, that is, in the illustration, 36, see Yule, *op. cit.*, 142.

[2] See Thorndike, *op. cit.*, 198, or W. F. Sheppard, *Biometrika*, 1903, Vol. II., 182.

means from the most probable true means corresponds as closely as could be expected with the most probable distribution of these means if their variation with mode of stimulus were due to chance. The three average values of $1/A$ obtained with each individual with different modes of stimuli do not vary from each other, or from their own average, more than they would because of errors of sampling. In the case of each individual, a true value of $1/A$ may be assumed, namely, the most probable true value, and in no case will any of the obtained values differ from this assumed true value more than would be expected from the standard errors of the obtained means. All the means obtained with any individual may, then, be regarded as measurements of the same magnitude. Since the magnitude in question varies as the reciprocal of the degree of attention, each individual may be said to show the same degree of attention in all three cases, after proper allowance has been made for the specific factor of sensory mode.

As supplementary proof of the conclusions drawn from Tables III.–V., it may be mentioned that in no case does the difference in the mean values of $1/A$ obtained with any two different modes of stimulus, after reduction to a common scale, exceed three times the standard error of the difference.[1] Further, in no case does the deviation of any mean from the most probable true mean exceed three times the most probable standard error.

The preceding tables have shown that the degree of attention of an individual is affected by the mode of stimulus to which he attends, but that when the effect of mode is allowed for, the individual differences in degree of attention remain the same. The data demonstrate, therefore, that an individual has a certain general ability to attend which remains the same, relative to other individuals' abilities, in spite of variation in the type of mental process concerned. It might be objected that the preceding data show merely that an individual's attention is conditioned by general factors which remain constant in spite of variation in the mental content,

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[1] For formula, see Yule, *op. cit.*, 342.

but that these general factors would not remain constant with variation in the form of mental process. To test the validity of this objection, degree of attention was measured in the case of ten of the objects, as it occurred in a choice reaction—a form of mental activity quite different from the simple reaction, and one which, according to the results obtained, conditions a quite different degree of attention than does the form of activity involved in the simple reaction.

The stimulus for the choice reactions consisted in an electric lamp placed behind an aperture in a dark box, and arranged so that its brightness could be made to either increase or decrease as the chronoscope circuit was closed. The lamp in the dark box, a 40-watt Mazda, was placed in series with two other 40-watt Mazda lamps, the latter two lamps being connected in parallel. The circuits were so arranged that as the switch in the chronoscope circuit was closed, either a 150-watt lamp was thrown in parallel with the pair of 40-watt lamps, thereby decreasing the resistance in the stimulus circuit, or one of the two 40-watt lamps in parallel was cut out, thereby increasing the resistance. When the resistance was decreased, the lamp in the dark box suddenly increased in brightness, and when the resistance was increased, the lamp decreased in brightness. The increase and decrease were about equally large changes. With the Mazda lamps used, these changes occurred with great suddenness, though of course there must have been some slight lag in both the increase and decrease. Since, however, both the increase and decrease were used equally often with both the regularly repeated and the irregularly mixed intervals, any lag in the change in brightness would be no drawback to the arrangement as regards the purpose of the present work, which uses merely the *difference* in reaction time with the two sets of preparatory intervals.

The sequence of increases and decreases in brightness was a chance one, and different at each measurement. It was the same, however, for both the regular and irregular intervals of any one measurement. Two reaction keys were employed, one for the right hand and one for the left. At the warning signal, the subject had to press down on both keys. Then, if the stimulus increased in brightness, he reacted by removing the fingers of the right hand while keeping those of the left pressed down, while if the stimulus decreased in brightness, he reacted by raising the fingers of the left hand while keeping those of the right upon the key.

TABLE VI

A COMPARISON OF DEGREE OF ATTENTION IN THE CASE OF CHOICE REACTIONS WITH THAT IN THE CASE OF SIMPLE REACTIONS

Subj.	Choice				Av. 1/*A* × 1.6 (Simple)	Av. 1/*A* (Simple)
	N	2 Secs.	Irreg.	1/*A* (Choice)		
St..........	6	239	307	68	75	47
		56	*57*			
Ll..........	3	238	301	63	72	45
		48	*53*			
Ww.........	4	296	387	91	90	56
		45	*68*			
Ht..........	4	273	358	85	90	56
		43	*59*			
Bs..........	4	302	437	135	110	69
		47	*70*			
Bt..........	2	345	461	116	107	67
		48	*61*			
Ws.........	5	353	496	143	139	89
		66	*71*			
Lm.........	5	332	492	160	149	93
		63	*75*			
Ls..........	4	322	496	174	168	105
		48	*91*			
Sy..........	5	436	642	206	221	138
		68	*79*			

The results are given in Table VI., the headings of which have the same significance as in Table I. For comparison of the degree of attention in choice reactions with that in the other cases already reported, in which simple reactions were employed, the average value of 1/*A* for the three other cases is given for each individual in the column headed 'Av. 1/*A* (Simple).' This is the value which is given in Table IV. as 'Av. 1/*A*.' The average value of 1/*A* for the group is 1.6 as large for choice reactions as for the average of the other cases. Consequently, to facilitate the comparison of the individual differences in the case of choice reactions with those in the case of simple reactions, the average value in the latter cases has been multiplied throughout by 1.6, and the result given in the column headed 'Av. $1/A \times 1.6$ (Simple).' As the number of measurements with choice reactions made with each individual is small, in some cases as low as two, only the average values are given, as in Table II. Both the averages and the mean variations in italics below them are the mean values obtained for a series of 25 reactions such as were taken in each measurement. In column N is given the number of measurements. The total number of reactions represented is 2,100.

The significance of Table VI. is made clear by a comparison of the column '$1/A$ (Choice)' with the column 'Av. $1/A \times 1.6$ (Simple).' While the two columns fall far short of being identical, there is nevertheless a very marked correspondence in the values obtained for each individual. On account of the small number of measurements made with the choice reactions, it does not seem worth while to make such comparisons in the present instance as in the case of the results obtained with the three modes of stimulus and simple reactions. Without any such comparison, it is yet perfectly obvious that the individual differences which exist with simple reactions remain substantially the same with choice reactions. It must be concluded that the individual differences in degree of attention obtained in the present work are not dependent upon the specific form of the mental process involved. These individual differences remain substantially the same in spite of variation in either the form or content of the mental processes, and must therefore be regarded as in all probability due to general conditions, which affect any act of attention of a given individual.

The preceding data establish the fact that there are general, individual conditions of attention. This, of course, does not mean that there are not also very many more or less specific conditions, conditions which apply only in one or a few cases of attention. In fact, two of these conditions, mode of stimulus and form of mental activity, are brought out by the above data. The specific conditions used in the present work are controllable, laboratory conditions. The data show that it has been possible to keep these specific conditions the same for all subjects. Under ordinary circumstances, this would not be possible. If situations were used in which a certain body of knowledge or certain special aptitudes were conditions of attention, it would be impossible to demonstrate the existence of the general, individual factor—at any rate, with the same definiteness and precision as in the present investigation. One person, for example, might be able to give much better attention to an orchestral symphony than another, but the latter individual might give much better attention to a debate on socialism. This is due to the fact, that, in these cases of attention, the specific conditions are not the same, and cannot be made the same, for all individuals. When there is variation, from one case of attention to another, in the degree to which different individuals are favored with respect to the specific conditions, there will naturally be found variation in the ranking of the individuals. On the other hand, the present investigation shows that the individual differences in degree of attention persist unchanged in spite of variation in the specific conditions, when the latter are rendered equal for all subjects. Thus, as the result of the control of conditions in the laboratory, it is possible to demonstrate the existence of general factors. While these are not so obvious in ordinary life, the variety of cases employed in the present investigation is sufficient to render it highly probable that these general conditions are really general, that is, that they are operative in any case of attention whatsoever, inside or outside of the laboratory, tending to make the attention of any individual uniformly better or worse than that of another, but ordinarily obscured by inequality for different individuals in the specific conditions of each case of attention.

The present investigation has resulted in the experimental demonstration—probably the first such—of a general capacity for attention on the part of any individual. At the same time, it has been emphasized that the general conditions are never operative alone, but always in conjunction with certain more or less specific conditions. There is a certain similarity in this position to Spearman's well-known theory of intelligence, called by him the theory of two factors.

Spearman has advanced the view " . . . that a person's success in any intellectual performance may be regarded as the joint product of two factors.

"The one is 'specific ability' for the performance in question with all its particular features."[1] The second is 'general ability.'[2] He writes that " . . . while the range of the specific factor is exceedingly narrow, that of the general factor is universal; and between these two there appears to be no intermediate."[3]

One might attempt to identify the individual's general capacity for attention with the factor called 'general ability,' and to analyze all the other conditions of attention into complexes of very specific elements. In such case, the present study might be regarded as merely a confirmation of Spearman's theory. The identification of general ability, or the essential factor therein, with capacity for attention has often been attempted,[4] but with questionable success. Spearman, himself, declines to admit such identification. Without doubt, the question of the identification of the general capacity for attention with general ability should be regarded as an open one. The data for a sure decision in the matter are as yet unobtained.

SUMMARY

The present research consists in measurements of the degree of attention of each of twelve subjects in each of the following four cases: simple reactions to touch; to sound; to light; and choice reactions to an increase or decrease in the intensity of light. The first three cases, each of which involves a different mode of stimulus, studied are especially

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[1] *Journ. of Abnorm. Psychol.*, 1914, 4, 219.

[2] *Op. cit.*, 221.

[3] *Op. cit.*, 221.

[4] Ferrier, 'The Functions of the Brain,' 2d ed., 1886; Binet, 'Attention et adaptation,' *Anneé psychol.*, 1899, 6, 393 and 395; Sollier, 'Psychologie de l'idiot et de l'imbecile,' 1901, 36–37 and 60–74; Wundt, 'Physiologische Psychologie,' 6th ed., 1908, Vol. I., 378–386; Burt, 'Experimental Tests of General Intelligence,' *Brit. Journ. of Psychol.*, 1909, 3, 166; Abelson, 'The Measurement of Ability of "Backward" Children,' *Brit. Journ. of Psychol.*, 1911, 4, 311.

intensively. Degree of attention is measured, not by the simple reaction time, but by the reciprocal of the prolongation in reaction time produced by the use of irregularly mixed preparatory intervals of widely differing lengths in place of a regularly repeated preparatory interval of two seconds.

The results lead to the conclusion that mode of stimulus is a condition of attention. At moderate intensities of stimulus, a higher degree of attention is secured with touch as a stimulus than with sound, and a higher degree with sound than with light. In the case of choice reactions, where the psychological processes involved are more complex than in the case of simple reactions, and where the subject does not know beforehand what change in stimulus to expect, degree of attention is lower than in the case of simple reactions.

In all four of the above cases, marked individual differences in degree of attention were found to exist, even among the adults. While these individual differences vary in absolute magnitude in the different cases, they remain relatively the same. If we think of the measurements made in each case as made on a different scale, we may say that the degree of attention of each individual persists unchanged in all cases. It follows that the degree of attention of any individual is conditioned in part by certain general conditions which remain constant while the type of the mental processes concerned is varied, and that it is to these general conditions, alone, that are due the individual differences in degree of attention found in the present investigation.

The variety of cases of attention used is sufficient to render it probable that these general conditions of attention are operative as a constant in all cases of attention of a given individual. Their influence is in most cases somewhat obscured because it is ordinarily impossible to obtain for different subjects sameness in the more or less specific conditions which in every case of attention act in conjunction with these general conditions. For any case of attention, then, we may write the equation,

$$A = (k,\ c_1,\ c_2,\ c_3,\ \ldots,\ c_n),$$

in which A is degree of attention, c_1, c_2, etc., are those conditions of attention which vary from one case of attention to another, while k is a factor which remains constant for a given individual, tending to make his attention uniformly better or worse than that of another. The presence of the factor k in this equation means that every individul has a certain power or faculty of attention, in the sense that the degree of his attention is determined in part by general conditions which remain effective in spite of variation in the specific type of the mental activity in question.

THE FACTORS AFFECTING A PERMANENT IMPRESSION DEVELOPED THROUGH REPETITION

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One of the most interesting psychological problems now confronting us is that concerned with the distribution of intervals of time between working periods. It is now recognized in certain industries that a man can do more work with less fatigue by working in spurts and then resting than by working at a slower but steady pace. Neither the psychologist nor the physiologist has had much to do with these industrial adaptations. But both knew enough in an academic way to have made some valuable prophecies in days gone by. Today both should strive for a clearer and better understanding of the principles underlying these problems, as through such knowledge should come many valuable contributions to industry, as well as to education.

The application to education looms much more important when we substitute the term 'presentation of stimuli' for 'working-period.' The whole problem of one-hour versus five-hour courses comes then under this heading. Likewise the problem of how long classes should be held is included here. And then again the manner of presentation of a subject is involved: Should a topic be taken up and finished and then another similarly treated, or should they be unfolded little by little over a considerable lapse of time. Many other such topics will occur to the reader, all of which hinge on this general subject of how stimulations can be best distributed so as to bring about the greatest permanent effect.

There are at least two general methods of approach to this field of investigation. We may first take up and study one factor, such as the interval between stimulations, and vary it alone while keeping all the other factors constant. This method undoubtedly gives us the most detailed information as to the value of the particular factor under study, but it often leads us to manufacture total situations far from what we should meet in every day life. And then when we have in this way discovered how each factor operates, this method will in all probability require further experimentation to discover how two or more factors will behave when united together. Over against this method is the second one where combinations of various factors as we find them in life are studied. Here we may discover just how the several factors may operate together but we may not be able to discover the laws of behavior of each one separately.

The writer believes both these lines of attack are needed. For that reason the experiment reported in the *Psychological Review* of March, 1914,[1] in which advertisements were shown at intervals of one month, has been repeated. In the new experiments different intervals of time between successive presentations have been employed. A number of interesting relations have been found through a comparison of the various details of the experiments. The writer believes that many more such relations are still to be discovered, possibly from the data now on hand, but more likely after more such experiments have been run.

The Experiment

The same set of advertisements was used here as in the experiment reported in the *Psychological Review* of March, 1914. (For a detailed description of the set see that article.) The 288 advertisements were so arranged in four groups (dummy magazines) that we had some firms advertising once, some twice, and some four times. And moreover, we had the firms divided so that an equal number used respectively quarter-page advertisements, half-page advertisements, and one-page advertisements.

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[1] E. K. Strong, Jr., 'The Effect of Size of Advertisements and Frequency of their Presentation,' *Psychol. Rev.*, 21, March, 1914.

Comparisons can consequently be made between any of the following propositions:

Firms	using	full pages	and advertising	4 times
"	"	"	"	2 "
"	"	"	"	1 time
"	"	half pages	"	4 times
"	"	"	"	2 "
"	"	"	"	1 time
"	"	quarter pages	"	4 times
"	"	"	"	2 "
"	"	"	"	1 time

In these experiments the sheets, on which all the advertisements were pasted, were given to the persons being tested and they were instructed to look them through at their leisure. It was suggested that they look them through in the same way that they turn the pages of an advertising section of any magazine, looking at what interested them and ignoring the rest. In each case they were timed by a stop-watch.

In what will be referred to as Experiment I. the four dummy magazines were looked through by the subjects at their leisure, *all at the same sitting.* Four weeks later they were tested by the recognition method as to what firms they had seen in the four dummy magazines.

Experiment II. differs from the above in that an interval of *one day* occurred between the perusal of each of the four dummy magazines. The test followed here also four weeks after the first set of advertisements was seen.

Experiment III. differs from Experiment II. in that an interval of *one week* intervened between each set of advertisements instead of one day.

Experiment IV. differs from the above (1) in that the interval of time between the four dummy magazines was *one month,* and (2) in that the *test occurred sixteen weeks* after the first set was seen. This is the experiment reported in the March, 1914, *Psychological Review.*

The number of individuals who served as subjects in these four experiments was as follows:

Experiment I. 6 men and 12 women, or a total of 18
“ II. 10 “ 15 “ “ 25
“ III. 12 “ 10 “ “ 22
“ IV. 10 “ 11 “ “ 21

(As there were 24 firms which advertised once in ¼-page, ½-page, and 1-page space, and 12 firms which advertised twice and 12 firms which advertised four times in each of these sized advertisements, the probable errors are determined on the basis of from 12 × 18 cases (the minimum) to 24 × 25 cases (the maximum). Such a large number of cases gives us very low probable errors, already indicated in the March, 1914, article. It has not seemed worth while to report the probable errors of Experiments I., II., and III. for this reason.)

Results

1. *The Results from Experiments I., II., III., and IV*

Table I. gives us the results from the first three experiments. They are also presented in Plate I., where the data for the three different sized advertisements are averaged together. The results show very clearly that an interval of one week between the successive presentations of a firm's advertisements gives a greater permanent impression at the end of the month than if the advertisements are seen on successive days at the commencement of the month, of if they are seen all at the same sitting.

At least four factors enter into this final determination. First, the length of time between seeing a particular advertisement and the day of the test. Second, the interval of time between the successive exposures, already pointed out. Third, the total number of advertisements seen at one time (42 pages of advertising seen at one time in Experiments II., III., and IV. and 168 pages in Experiment I.). And fourth, the effect of the size of the advertisement. Each of these factors will be discussed presently, but it is well to have them in mind in considering the gross results of these experiments.

In order now to compare the data in Table I., where the

TABLE I

SHOWING THE PER CENT. OF FIRMS REMEMBERED PER READER FOR EACH OF THE NINE COMBINATIONS OF SPACE AND FREQUENCY

Test made four weeks after first dummy magazine was seen.

1. *When the 4 Dummy Magazines Were Seen One Right After the Other (Experiment I.)*

Frequency	Size of Ads.			Av. 3 Sizes
	¼ Page	½ Page	1 Page	
1 time	3.4	5.0	10.1	6.2
2 times	4.6	8.2	15.9	9.6
4 times	4.8	12.4	16.6	11.3

2. *When the 4 Dummy Magazines Were Seen 1 Day Apart (Experiment II.)*

Frequency	Size of Ads.			Av. 3 Sizes
	¼ Page	½ Page	1 Page	
1 time	4.5	8.5	13.2	9.4
2 times	8.0	11.6	20.7	13.4
3 times	12.5	15.6	20.4	16.2

3. *When the 4 Dummy Magazines Were Seen 1 Week Apart (Experiment III.)*

Frequency	Size of Ads.			Av. 3 Sizes
	¼ Page	½ Page	1 Page	
1 time	3.7	9.8	19.9	11.1
2 times	9.0	13.7	23.9	15.5
3 times	10.5	26.8	29.2	22.2

test followed one month after the first advertisements were seen, with the data from Experiment IV., where the test followed four months after the first advertisements were seen, it is necessary to make allowance for the amount that would be normally forgotten in the interval between one month and four months. A separate series of three experiments were run for that purpose and the results are shown in Table II.

TABLE II

SHOWING THE EFFECT OF TIME UPON REMEMBERING ADVERTISEMENTS

The average firm was remembered immediately after by 50.5 per cent. of the readers.
" " " " " one month after by 14.9 per cent. " " "
" " " " " four months after by 8.6 per cent. " " "

Evidently, then, in the interval between one month and four months there has been a decrease in memory from 14.9 per cent. to 8.6 per cent. Expressed in a ratio the relative amounts remembered after one month and four months are 100: 58.

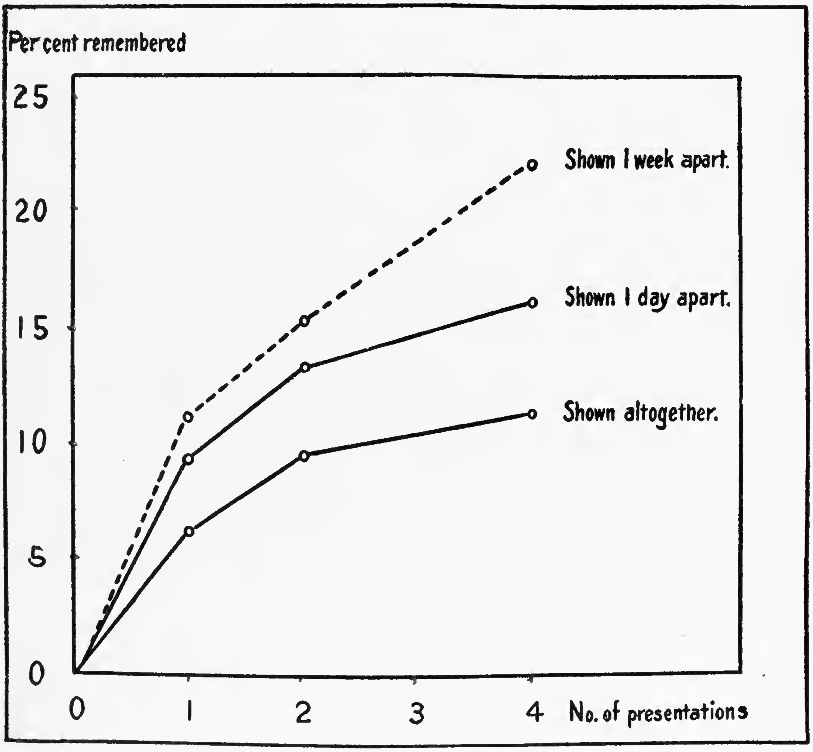

Plate I. Showing the per cent. of readers who remember a firm when one, two, and four advertisements are shown (1) altogether, (2) at intervals of 1 day, and (3) at intervals of 1 week: the test following 4 weeks after the first set was seen.

When the averages of the data in Table I. have been multiplied by this decimal (0.58) we have the figures as given in the first column of Table III.

This decimal (0.58) is apparently a trifle too high, for when the value of one presentation in Experiment III. as given in Table I. is multiplied by it, we get a value of 6.4. Now, if anything, this value in Experiment III. should be slightly less than that in Experiment IV. (*i. e.*, 6.1 per cent.).

In the former there were 15 concerns whose advertisements appeared only once, say on January 1, 21 other concerns whose advertisements appeared on January 8, 15 more on January 15, and 21 more on January 22. The lengths of time between seeing the advertisements and the test for these four groups were then, respectively, 16 weeks, 15 weeks, 14 weeks, and 13 weeks. Now in the latter the four groups were shown on January 1, January 29, February 26, and March 26. And the lengths of time between seeing the advertisements and the test for these groups were, respectively, 16 weeks, 12 weeks, 8 weeks, and 4 weeks. No other factor, besides this one of difference in interval of time, enters into the two experiments. Hence the figure in Experiment IV. should be higher than that in Experiment III. for concerns showing one advertisement, because there was a shorter interval of time on the

TABLE III

SHOWING THE PER CENT. OF FIRMS REMEMBERED PER READER FOR EACH OF THE NINE COMBINATIONS OF SPACE AND FREQUENCY

Test made 16 weeks after first dummy magazine was seen. (Data in Experiments I., II., and III. taken from Table I. and multiplied by the decimals 0.58, 0.47, or an average of the two.)

	Times Ads. Were Seen	Correction Decimals			Ratios
		× 0.58	× 0.47	Av. of Two	
1. When the four dummy magazines were seen one right after the other	1	3.6	3.0	3.3	1.00
	2	5.6	4.7	5.2	1.58
	4	6.5	5.4	6.0	1.82
2. When the four dummy magazines were seen 1 day apart	1	5.1	4.6	4.9	1.00
	2	7.8	6.6	7.2	1.47
	4	9.4	7.9	8.7	1.78
3. When the four dummy magazines were seen 1 week apart	1	6.4	5.4	5.9	1.00
	2	9.0	7.3	8.2	1.39
	4	12.8	10.9	11.9	2.02
No correction decimal, data actually obtained with 16 week interval.					
4. When the four dummy magazines were seen 1 month apart	1	6.1	6.1	6.1	1.00
	2	7.3	7.3	7.3	1.20
	4	9.2	9.2	9.2	1.51

average between the exposure and the test in Experiment IV. than in Experiment III. The average length of time between exposure and test per advertisement in the former was 9.7 weeks, while in the latter it was 14.4 weeks. Interpolating from our data in Table II. we should expect, if we express the percentage remembered in Experiment IV. as 100, that the percentage remembered in Experiment III. would be 88. This would mean that one presentation in Experiment III. should have a value of 5.4 (6.1 × 0.88) instead of 6.4.

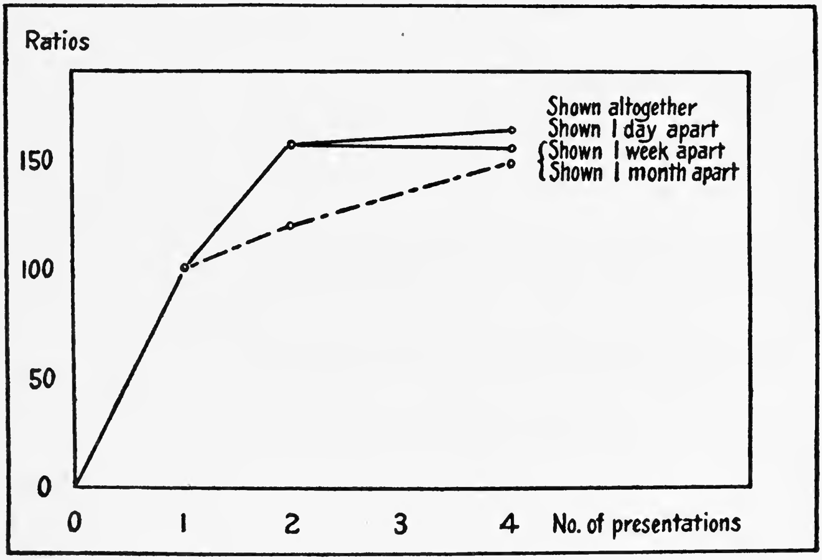

PLATE II. Showing the per cent. of readers who remember a firm when one, two, and four advertisements are shown (1) altogether, (2) at intervals of 1 day, (3) at intervals of 1 week, and (4) at intervals of 1 month: the test following 16 weeks after the first set was seen.

If 5.4 is the correct value then our correction decimal should not be 0.58 but rather 0.47. The latter decimal would give us the figures in the second column of Table III.

An average of the two sets of data thus worked out gives us the third column in Table III. These figures are plotted in Plate II. It is clear from the table and plate that a greater permanent impression is made four months later, when a firm advertises for four successive weeks and then stops, than if it advertises once a month during that period, or on four successive days, or advertises in four different magazines which are seen by a reader at the same time. It is also clear that the latter procedure is the least efficient of the four distributions. But from the data and the plate it is not at all clear whether advertising on four successive days or on four successive months will give the greater returns. The latter distribution is favored by the data here. But the probable errors of our determination are greater than the differences.

The writer was so certain that intervals of one day would be found superior to those of a few minutes, a week, or a month, that he stated at one time that "of all intervals between successive repetitions that of a day's length will give us our maximum results."[1] This conclusion was reached on the basis of the work of Starch, Pyle, Ebbinghaus, etc. But it is clear that, when there is a long interval of time between exposure and the final test, presentations on successive days are not so effective as when they occur at intervals of one week.

2. *The Effect of Different Intervals of Time between Seeing an Advertisement and its Recognition*

Table II. presents what data there are in existence on this particular point. The curve of forgetting for recognition memory (in the case of unconnected words) has already been shown to approximate the curve as given us by Ebbinghaus for recall memory.[2] We would naturally suppose then that the great bulk of the loss in what is remembered immediately after seeing the advertisements and one month later occurs during the first two days. If this is the case the amount forgotten in the interval between two days after the original presentation and the test bears a nearly constant ratio to the interval of time. Various calculations of the writer on this

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[1] E. K. Strong, Jr., 'Two Factors which Influence Economical Learning,' *Jour. Philos., Psychol. and Sci. Methods*, XI., Feb. 26, 1914.

[2] E. K. Strong, Jr., 'The Effect of Time-Interval upon Recognition Memory,' *Psychol. Rev.*, XX., Sept., 1913.

basis in different experiments have checked up with directly obtained data, as closely as could be expected.

3. *The Effect of Different Intervals of Time between the Successive Presentations of Advertisements when Tested at a Considerable Time Later*

If the data in Table III. are expressed in ratios in terms of the value of advertisements seen once, we have the ratios given in column four of that table. These are plotted in Plate III. With one exception, *i. e.*, the effect of four presentations a week apart, the curves show very clearly that the

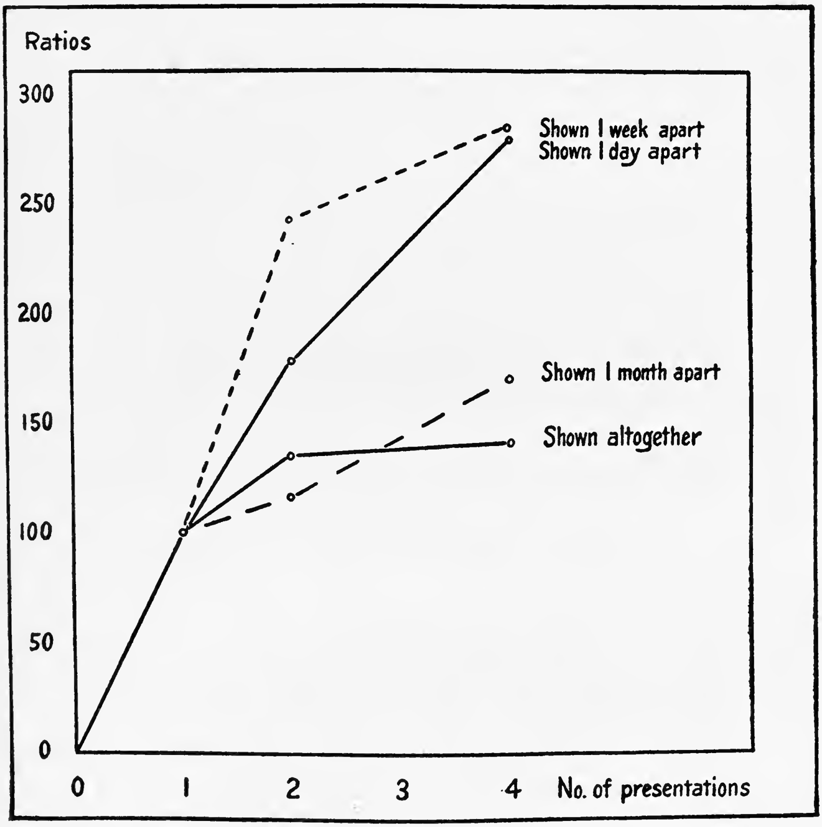

Plate III. Showing the cumulative effect of two and four repetitions over one with the four methods of distribution. The value of one presentation is in each case called 100 and the other values are expressed in ratios of this value.

shorter the interval of time between the successive presentations the greater is the cumulative effect.

But the relation between 'intervals of time' and 'permanent impression' from successive stimulations is not so simple as would appear from Plate III. In Plates IV., V., and VI. are shown the same relations as in Plate III. but here restricted, respectively, to ¼-page advertisements, ½-page advertisements and 1-page advertisements, instead of giving an average of all three sized advertisements.

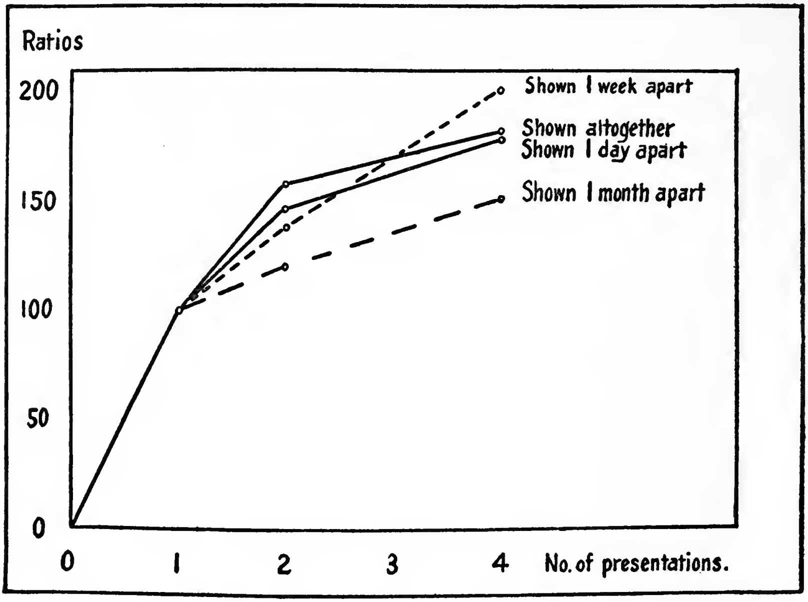

Plate IV. With ¼-page advertisements. Showing the cumulative effect of two and four repetitions over one with the four methods of distribution. The value of one presentation is in each case called 100 and the other values are expressed in ratios of this value.

The first fact that appears from a study of these plates is that a relatively greater effect is made by the first presentation of a large advertisement than by a small advertisement, as compared to the effect made by two or four presentations. Or possibly, the relation should be expressed as follows: A second presentation adds relatively more to the permanent impression when the advertisement is small than when it is large.

A second fact that appears is that repetition within a few minutes has relatively little effect upon ¼-page advertisements as compared with repetition at longer intervals of time, but that it has much greater effect with ½-page advertisements, and has the greatest effect of all our intervals of time upon 1-page advertisements. On the other hand, repetition at intervals of one month has (with one exception) the least effect of all the intervals regardless of the size of the advertisements.

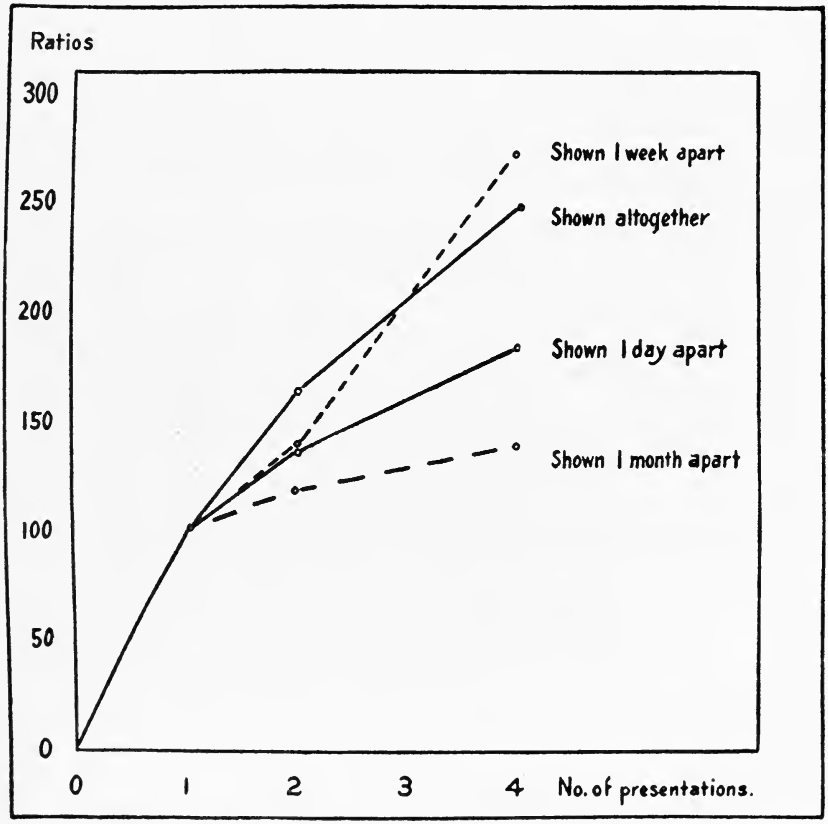

Plate V. With ½-page advertisements. Showing the cumulative effect of two and four repetitions over one with the four methods of distribution. The value of one presentation is in each case called 100 and the other values are expressed in ratios of this value.

Our general conclusion as to the effect of different intervals of time upon the permanent impression, *i. e.*, the shorter the interval of time between the successive presentations the greater is the cumulative effect, holds exactly for 1-page advertisements and nearly so for ½-page advertisements, but not at all for ¼-page advertisements.

The results obtained here support previous work as to the greater impression made by the first stimulation than by any succeeding one. But they do not entirely support Smith's[1] extreme statement that "the first repetition is undoubtedly the best, *i. e.*, more is learned by it than by any other repetition, or, in fact, by all the other repetitions put together." In his experiment, as in these, here, the subjects were not directed to learn but simply to notice what was presented to them. Smith's conclusion is correct as far as our data go with the full-page advertisements, for here three more repetitions have not doubled the effect of the first one. The same holds true in the case of ½-page advertisements when the intervals were either one month or one day apart, and in the case of ¼-page advertisements when the intervals were either

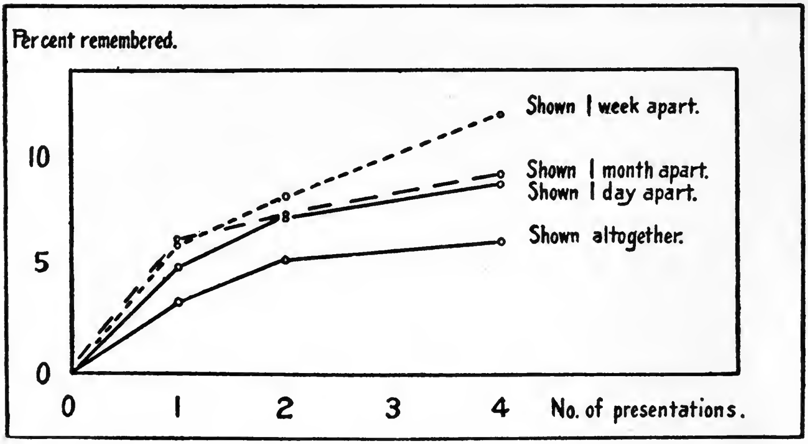

PLATE VI. With 1-page advertisements. Showing the cumulative effect of two and four repetitions over one with the four methods of distribution. The value of one presentation is in each case called 100 and the other values are expressed in ratios of this value.

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[1] W. G. Smith, 'The Place of Repetition in Memory,' *Psychol. Rev.*, III., 1896, 21–31.

one month or a few minutes apart. But in four cases, the three additional repetitions more than doubled the effect of the first repetition.

4. *The Effect of Seeing Varying Totals of Advertisements at One Time*

The writer has already shown at some length that as you increase the number of advertisements which are seen at any one time, you decrease the effectiveness of the permanent impression from any one of them.[1] "Increasing the number of advertising pages from 42 to 168 resulted in a decrease in the percentage remembered among the 42 pages from 14.9 per cent. to 7.8 per cent. That means that the situation in which but 42 pages are glanced at allows a 91 per cent. greater impression to be made (from identically the same advertisements) than that which can be made when 168 pages are glanced at."

5. *The Effect of Different Sized Advertisements upon the Permanent Impression*

If we average the effect of the one, two, and four presentations together, as given in Table I. and the last part of Table II. in the March, 1914, article, we have the following as the value of ¼-page, ½-page, and 1-page advertisements, respectively:

	¼-page	½-page	1-page
When shown all together	4.3	8.5	14.2
When shown 1 day apart	8.3	11.9	18.1
When shown 1 week apart	7.7	16.8	24.3
When shown 1 month apart	4.9	7.0	10.7
Average	6.3	11.1	16.8

If these are expressed in ratios, we have:

	¼-page	½-page	1-page
When shown all together	100	198	330
When shown 1 day apart	100	143	218
When shown 1 week apart	100	218	316
When shown 1 month apart	100	143	218
Average	100	176	267

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[1] *Op. cit.* (see note 2).

These ratios are higher than those previously reported in this particular experiment, which were 100: 141: 215, but are very similar to ratios obtained in other experiments, as given in the March, 1914, article, *i. e.*, 100: 166: 241 as the average of four experiments.

More recently the attention-value of various sized advertisements in the *Saturday Evening Post* has been ascertained.[1] In this case 285 individuals were canvassed and from this number 90 were found who had handled that week's edition. Of these 88 were tested by the recognition method as to what advertisements they had seen in the last number of the *Saturday Evening Post.* In Table IV. are given the results as they bear on the value of space. They are expressed in terms of ratios of the attention-value of a full-page. It is clear from this table (based on 113 advertisements) that attention-value does not vary directly with the size of the space, but does vary very closely with the square-root of the size of the space. The figures, indeed, correspond as closely as one could expect.

Table IV

The Attention-Value of Different Sized Advertisements in the *Saturday Evening Post*

Size of Advertisements	Supposed Value, *i. .e,* Percentage of Size of a 1-Page Ad.	Value as Obtained in this Experiment Expressed in Terms of a 1-Page Ad.	Theoretical Value, *i. e.*, Based on the Square Root Law
2 page	200%	149%	141%
1 page	100	100	100
½ page	50	81	71
2 cols. × 8″	31	70	56
2 " × 6½	25	57	50
2 " × 5	21	44	45
2 " × 4	17	41	40
1 " × 7	14	32	37
1 " × 5½	11	32	34
1 " × 4½	9	25	31
1 " × 3½	7	28	27
1 " × 2½	5	21	23
1 " × 1½	3	25	18

Generally speaking our previous conclusion is correct,

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[1] E. K. Strong, Jr., 'A Study of the *Saturday Evening Post,*' Association of National Advertising Managers' Research Bulletin No. 7, Mar., 1914.

i. e., 'that the attention-value of space increases approximately as the square-root of the increase in area and not directly with the increase in area.' This is exactly so in some cases: in others the increase in attention-value is in excess of the square-root ratio by as much as 25 to 30 per cent.

6. *Relative Strength of these Four Factors*

Undoubtedly the third factor—the effect of varying numbers of advertisements seen at one time—is the most important factor as affecting the total impression. An increase from 42 to 168 pages of advertising results in a decrease of 48 per cent. in the effectiveness of the impression from any one advertisement. The difference as shown in Plate I. between the data of Experiments I. and II. is due to this factor of seeing 42 advertisements at one time versus 168 advertisements at one time. It must be due to this factor, as the difference in the number of days between presentation and testing is too slight to account for any appreciable difference in the amount remembered. Now we have seen in Section 3 that repetition within a few minutes has a greater effect than at longer intervals with 1-page advertisements. If this is actually true, then it would seem that if we had shown the four dummy magazines in Experiment I. at intervals of one hour instead of one right after the other, there would have been a greater permanent impression made with the 1-page advertisements than by any of the other distributions used here. It is, however, possible that the greater cumulative effect from seeing the advertisements within a few minutes is due to the fact that the first impression made in Experiment I. was very slight. This conception is suggested by the fact that all the types of distribution have a greater cumulative effect upon small advertisements than large ones. If this latter conception is true then our above prophecy is not necessarily correct. The matter can only be settled by actual experimentation.

As was shown in the March, 1914, article, the same total amount of space is more effective when used in large amounts than when used in small amounts but more frequently.

Table V. shows this fact very clearly. Here the value of a ¼-page advertisement shown once is called 100. In all but two of the thirty-two cases the large space seen less often is more effective than the smaller space seen often.

Table V

Showing the Value of the Nine Combinations of ¼-page, ½-page, and 1-page Space Presented Once, Twice, and Four Times

Each value is stated in terms of the value of a ¼-page advertisement shown once.

Size of Advertisement	Number of Presentations	Shown Altogether	Shown One Day Apart	Shown One Week Apart	Shown One Month Apart
¼-page ad.....	1 time	100	100	100	100
¼-page ad.....	2 times	135	178	243	116
½-page ad.....	1 time	147	189	259	155
¼-page ad.....	4 times	141	278	284	171
½-page ad.....	2 times	241	258	370	184
1-page ad.....	1 time	297	293	538	229
½-page ad.....	4 times	365	347	724	216
1-page ad.....	2 times	468	460	646	276
1-page ad.....	4 times	488	452	789	342

Possibly the relationship is something as follows: When an advertisement is seen it arouses many associations more or less well established. A large advertisement on the average arouses many more such associations than a small advertisement. But the increase in number is never in proportion to the increase in space. All these associations tend to be welded into a complex giving us the new conception desired by the advertiser. We have then finally as resulting from the impression, a more or less clearly formed new conception—a sort of higher-order association formed from the old associations. The more associations which were aroused the better formed is the complex. When the second advertisement is seen it again arouses this complex and adds to it one or more new associations. But we must judge from our data that the higher-order association is more readily developed when the many old associations (with possibly some new ones) are all aroused at the same time than when some are aroused at one time and then later aroused again in a different setting. (It should be remembered here that the repetition consisted in these experiments not of the same original advertisements but of new ones which were more or less similar to the first advertisement displayed.) It is very likely true that in the case of the small advertisement, the total impression from the first advertisement varied much more from that of the second advertisement than when large space was employed. There would be in this case less overlapping of the associations in the repetition of the small advertisements and more emphasis upon new associations (new to the complex) than in the case of the large advertisements.

At first thought it would seem as though the results in Sections 5 and 6 could not be harmonized. First we see that attention-value of large advertisements is never proportional to their size. Hence the argument arises, use small space for effective advertising. Then we see in Section 6 that a full-page advertisement is more effective than four quarter-page advertisements. Now the argument must surely be, use large space for effective advertising. Can both be correct? To the writer it seems than common advertising practice supports both as correct. When the advertisement is expected practically to complete the sale, as in mail-order advertising, we find generally small space is employed. Mail-order houses are the only advertisers who are in a position really to know the effect of their advertising. The fact that they ordinarily use small space seems to demonstrate that small space is more efficient than large space. And such advertisers constantly affirm that this is so. Now, on the other hand, when it is not expected that the first advertisement will sell the goods but that only after many such advertisements have been run will the advertiser reap the effect of the advertising, as in advertising pianos, autos, etc., we find that large space is used almost altogether. It is doubtful if any such advertiser has ever been able really to demonstrate the value of this style of publicity. But certainly at the present time the majority of such campaigns are carried on in this way. We should conclude, then, by saying that in Section 5 is given the relative attention-value of advertisements of varied sizes when there is but one presentation, whereas in Section 6 there is given the relative attention-value of advertisements of various sizes when the factor of repetition is taken into account. In the first case the attention-value is measured after one repetition; in the second case after many repetitions.[1]

CONCLUSION

1. A greater permanent impression is made (*i. e.*, 16 weeks after the first presentation) when a firm advertises for four successive weeks and then stops, than if it advertises once a month during that period, or on four successive days, or advertises in four different magazines which are seen by a reader at the same time. The last form of distribution is the least efficient. The second and third are approximately equal.

2. The permanent impression, after the first two days, apparently dies out gradually and in a direct ratio to the total length of time which has elapsed.

3. It would seem that the greater the initial impression the less will be the added impression from successive repetitions.

4. It would also seem that the greater the initial impression (*i. e.*, one-page advertisements here) the more does repetition within a few minutes affect the total impression as compared with repetition at longer intervals such as a day or longer.

5. As you increase the number of advertisements which are seen at any one time you decrease the effectiveness of the permanent impression from any one of them.

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[1] The factors discussed in this paper are, of course, not the only ones that enter into the question as to what size of space will be most effective. Large space may be very ineffective from the advertising standpoint but very effective from the manufacturing standpoint. That is, it may cost more per dollar spent for advertisers to use large space than small space, but it may save far more through producing a larger total of sales, thus cutting down the cost of production. It is probable that economic factors, such as this, now dominate the reasons for selecting one size of advertisement over that of other sizes. But when small differences in the cost of salesmanship will seriously affect the entire business the psychological factors considered in this section are all important. See W. A. Shryer, 'Some Mail Order Weaknesses and their Cure, Advertising and Selling,' May, 1915.

6. The value of space in advertising as affecting permanent impressions increases approximately as the square-root of the increase in area, or sometimes at a somewhat greater rate.

7. The same total amount of space is more effective when used in large amounts less often, than when used in small amounts but more frequently.

INCIDENTAL PERCEPTION

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The aim of this paper is to study incidental perception of time, size and weight and to make some comparisons between incidental and purposive perception of time and size.

Experiment I

Forty men of Juniata College and Academy were each given three successive tests for estimating a minute interval of time. All, save a few day students, were tested on the same evening during study hours, when no chance was offered the prospective subjects to be apprised of the nature of the test by those already tested. In the first test the subject was told to read silently from a certain editorial page of the *Saturday Evening Post.* At the end of a minute he was interrupted by the request to guess how long he had been reading. Then he was given a poem entitled 'Two Chums,' and was told to read it carefully and to state when he had read one minute. Finally he was told to estimate, while unemployed, a minute from the signal 'go.' He was cautioned not to count and not to count his breaths. A stop watch was used in this and the following experiments. The writer was the experimenter.

The average time estimated in the first test was 1 min. 23 sec., M.V. 24.6 sec.; in the second test, the average was 44.3 sec., M.V. 12.6; and in the third, 37.9 sec., M.V. 15.7. The average error for the first test was 24.2 sec. and the constant error, + 22 sec. The highest estimate was 2 min. 30 sec.; lowest 45 sec. Nine estimated at 2 min., 6 at 1 min. 30 sec. Twelve estimated correctly; 25 estimated above a minute; 3, below. In the second test the A.E. was 19.8 and C.E. − 15.7. The highest estimate was at 90 sec.; lowest, 19. Two cases were correct, 5 too high, and 33 too low.

In the third test the A.E. was 23.8; C.E. −22.1. No cases were correct, 3 were too high, and 37 too low. Thirty-four made higher estimates when they were asked to estimate how long they had read (1st test) than they made when they were asked to designate when they had read for a minute (2d test); two made the same, four made lower estimates. Following are some random samplings of the three estimates.

Individual	1st Test	2d Test	3d Test
1	1 min. 23 sec.	35 sec.	17 sec.
2	2 min. 10 sec.	27 sec.	35 sec.
3	1 min. 15 sec.	65 sec.	37 sec.
4	1 min.	36 sec.	32 sec.
5	50 sec.	52 sec.	49 sec.
6	45 sec.	81 sec.	42 sec.
7	1 min. 15 sec.	27 sec.	40 sec.
8	1 min. 5 sec.	47 sec.	57 sec.

The large C.E.'s and the small difference between the C.E.'s and A.E.'s indicate strong overestimation and that nearly all errors made were in this direction. The variation among the three tests by the same performer as well as the variation among the performers in the same test was rather pronounced. The M.V.'s were about one third the averages. There was a slightly greater tendency to overestimation in incidental perception than in purposive perception, but the overestimation in the case of incidental perception of filled time and that of purposive perception of empty time were about the same. Although 37 of the 40 in the third test against 25 in the first test overestimated, the large number (13) who estimated at 1 minute may help explain this discrepancy.

The subjects were asked for introspections in the last test. Most said they could offer no explanation of how they estimated; a few gave brief introspections; "I measured in steps;" "I felt myself measuring" (29); "Just guessed" (39); "Felt rhythm of breathing" (57); "Grouped in series of 10" (38); "Allowed a little for grace" (43); "Imagined I heard and saw a watch" (22); Four imagined the second hand moving (29, 20, 32, 28). The numbers in parenthesis indicate the respective estimates. Apparently these devices were of no advantage.

EXPERIMENT II

Forty-six normal-school girls were given group tests. At the end of their logic courses the writer read from the introductory chapter of Creighton's 'Logic,' presumably to present some facts in logic. At the end of one minute he asked them to write down a 'precise estimate' of how long he had read. Then he resumed reading for another minute and again asked for their estimates. Then they were told to estimate the time between the signals "Go" and "Halt."

The average estimate for the first test was 1 min. 44.1 sec. with a M.V. of 44.5 sec.; second test, 1 min. 29.3 sec., M.V. 38.4; third test, 1 min. 39.2 sec., M.V. 42.6. The ratio of the highest variation to the lowest was practically 2 to 1 (first test 196–79 sec.; second 151–79 sec.; third 121–71 sec.). The M.V. is between ½ and ⅓ the average. Thus individual difference is tremendous and the women are a little more variable than were the men in Ex. I., though the tests are slightly different.

In the first test 7 estimated correctly, 8 underestimated and the rest, 31, overestimated a minute with a C.E. of + 39.8 sec. and A.E. of 46.2 sec. In the second test 2 estimated correctly, 16 underestimated and the remaining 28 overestimated, with a C.E. of + 30.8, A.E. 38.6. In the third test 4 estimated correctly, 10 underestimated and the remaining 32 overestimated. The C.E. + 39.1, A.E. 47.7.

Not one individual estimated the minute of one length in the three successive tests and only four got any two estimates the same. The correlation of first and second tests gives + .509, second and third + .338, first and third + .278. In the second test 34 gave smaller estimates than in first test; 10, larger; and 2, the same. In the third test 15 gave smaller estimates than they did in the second; 28 gave larger, and 3 same. Also 24 gave smaller estimates in the third test than in the first, 20 gave larger and 2 gave the same. These small correlations suggest a wide variation in the estimation of a minute of time under different conditions by each individual.

Therefore there are very wide individual differences as to the influence of different conditions in determining the estimates, and there is a strong tendency at overestimation of time. As was also shown by experiment I., the overestimation of incidental perception of time was slightly greater than it was by purposive perception during employment of the subject; but the incidental estimate in both experiments when the subject was unemployed was about the same as when estimating empty time purposely perceived.

Assuming that employment by listening to material read factors the same in estimating time as employment by reading, the C.E.'s by the women, Ex. II., show a much greater overestimation of a minute than by the men, Ex. I. This assumption, however, deserves experimental evidence.

Accidentally the writer, in testing 20 girls of a class of psychology with the same experiment, said, in giving the directions: "Estimate the time during which I was reading, in *minutes and seconds*." The influence of suggestion was rather pronounced. Out of the total 60 estimates all except one was above a minute and that one was just one minute. The records are given below in full along with twenty records randomly selected from those not exposed to the suggestion.

First Test		Second Test		Third Test	
With Suggestion	Without Suggestion	With Suggestion	Without Suggestion	With Suggestion	Without Suggestion
220	75	195	80	130	100
210	65	185	23	160	28
182	80	240	90	180	45
180	30	240	120	110	80
150	180	210	148	60	120
200	70	250	150	180	125
130	120	120	120	90	150
80	180	65	160	65	210
150	59	180	130	90	140
300	62	240	61	120	62
180	25	150	120	100	220
150	85	110	60	70	160
255	60	285	50	170	80
180	130	210	100	80	120
150	120	180	100	180	60
240	180	300	120	120	90
210	120	245	90	180	38
180	45	210	75	90	180
160	60	210	70	80	45
180	150	240	120	120	90
184.3	94.8	203.2	99.3	113.7	107.1

While the suggestion is not so pronounced for the third test the average estimate in the first and second tests is about twice as great with the suggestion as it is without the suggestion. These records emphasize how readily the results of an experiment may be vitiated by suggestion from the experimenter.

Experiment III

On Monday following a Saturday evening basket-ball game 100 students—68 men and 32 women—of Juniata College and Academy who had attended the game, were asked in their respective classes, at the same hour, to estimate how long they thought the teams had played before "time out" was called. At the time referred to a player had received a rather serious injury, 6 minutes 15 seconds, after the game had started. The average estimate by the hundred students was 11 min. 38 sec. By the men it was 10 min. 7 sec. with a M.V. of 3 min. 4 sec., an A.E. from the standard (6 min. 15 sec.) of 4 min. 28 sec. and a C.E. of + 3 min. 52 sec. Only 9 of the 68 men underestimated the time. By the women the average was 14 min. 54 sec. with M.V. of 7 min. 50 sec., an A.E. of 7 min. 46 sec. and a C.E. of + 6 min. 54 sec. Only 11 women underestimated and their estimate was at 5. All the other women estimated in whole minutes and all but 10 of the 32 estimated in some multiple of five; whereas 6 of the men estimated in minutes and seconds and only 25 of the 68 men estimated in some multiple of 5. The highest estimate by the men was 22 min.; the lowest, 45 sec. The highest estimate by the women was 25 min., the lowest, 5 min. Fifty-six per cent. of the men's estimates were between 7 and 13; 34 per cent. of the women's records were between these limits, and 62.5 per cent. of the women's exceeded the average of the men's. (Delayed report may have been responsible for some of the error.)

Thus by incidental perception 6 min. 15 sec. was overestimated by about 80 per cent. of the cases and more by women than by the men. Likewise the women varied more than did the men. For both, the general tendency at over-estimating time and the predominance of the men, in this respect, over the women is in line with the findings of Yerkes and F. M. Urban[1] in purposive perception and of Myers[2] in incidental perception of time. For example the writer found that of 70 school children, 36 boys estimated two and one half minutes during which they had been employed, at an average of 5.4 minutes and 34 girls at an average of 6.4 minutes.

Experiment IV (*a*)

In the center of each of two white cardboards 3 × 3 inches, a circle was drawn. One circle represented the outer edge of a cent, the other, that of a half dollar. Forty men and forty women of Juniata College and Academy were tested in their respective rooms during study hours in one evening. Mrs. Myers was experimenter for the women and the writer for the men. Two and three students were tested at the same time. One of the circles was presented to each subject who was asked to state on paper whether or not the circle appeared to be in the center of the square. After the answer was written the experimenter removed the circles from view and presented in a visiting way, some photographs of mutual interest to experimenter and subject, for 2 minutes. Then he asked the subjects to write down the number of the circle which they would select from a cardboard on which were drawn a large number of circles as equal in size to the circle previously presented. Among these were circles representing the exact size of the respective coins and the circles numbered from 9 to 29; no reference to coins was made. Then the other circle was presented to the subject, who was told he would have a few seconds to look at it when he would be asked to select this same circle from among those on the cardboard. One minute intervened between the time the circle was recovered and the time the cardboard of circles

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[1] R. M. Yerkes and F. M. Urban, 'Time Estimation in Its Relation to Sex, Ages and Psychological Rhythm,' *Harvard Studies*, II., 1906, pp. 405–30. See also Nichols Herbart, 'The Psychology of Time,' Amer. J. Psychol. 1890, 3, 453–529. He gives an exhaustive bibliography.

[2] Myers, Garry C., 'A Study in Incidental Memory,' *Archives of Psychology*, No. 26, Feb., 1913, pp. 92–93.

was presented. To one half of the subjects of each sex the cent circle was first presented and to the other half, the half-dollar circle. As the diameter of each successive circle increased by one millimeter, the difference between the numbers of the circles represents the difference in diameters of the circles. The records are given below.

CENT

Cent Circle Shown First

Men (20)	Women (20)	M. and W. (40)
A.E. 2.0	A.E. 1.9	2.0
C.E. −.6	C.E. −1.1	−.6
5 + cases	6 + cases	11 + cases
11 − cases	10 − cases	21 − cases
4 0 cases	4 0 cases	8 0 cases

Cent Circle Shown Second

Men (20)	Women (20)	M. and W. (40)
A.E. 2.0	A.E. 1.2	1.6
C.E. −1.2	C.E. −.2	−.7
3 + cases	6 + cases	9 + cases
13 − cases	7 − cases	20 − cases
4 0 cases	7 0 cases	11 0 cases

HALF-DOLLAR CIRCLE

Half-Dollar Circle Shown First

Men (20)	Women (20)	M. and W. (40)
A.E. 2.7	A.E. 2.0	2.7
C.E. −.7	C.E. −.2	−.5
9 + cases	8 + cases	17 + cases
10 − cases	10 − cases	20 − cases
1 0 case	2 0 cases	3 0 cases

Half-Dollar Circle Shown Second

Men (20)	Women (20)	M. and W. (40)
A.E. 2.6	A.E. 1.6	2.1
C.E. −1.4	C.E. −.8	−1.1
5 + cases	4 + cases	9 + cases
14 − cases	11 − cases	25 − cases
1 0 case	5 0 cases	6 0 cases

They should read: "The 20 men to whom the cent-circle was first presented made an average error of 2.0 mm. and a C.E. of −.6 in estimating the cent circle. The records for the 20 to whom the half-dollar circle was first shown were: A.E. 2.0 and C.E. −1.2." A glance at the C.E.'s and A.E.'s, for both sexes shows little or practically no advantage to the purposive perception. It must be remembered that the size of the circle shown first was incidentally perceived, whereas the second was purposely perceived. The women however seemed to gain more in the second test over the first than did the men, and they had a smaller A.E. than the men. The negative C.E. for all groups suggests that the sizes were underestimated. This does not agree with the findings of the writer in his 'Study of Incidental Memory' where records of tests made on several hundred subjects of various ages fall into two very distinct groups. The smaller coins, cent, nickle and dime, were almost universally underestimated whereas the larger coins—quarter, half-dollar, and dollar—were overestimated. These findings for which the writer had no satisfactory interpretation seem the more puzzling in the light of this newer data. Certainly values seem now to have played a rôle in the estimates of the sizes of the coins. It should be remembered that in the present test no reference was made to the circles representing coins when they were presented. Apparently the image of the circles first observed must have grown smaller during the interim between the perception of the circle and the selection of its equal from the cardboard. This is contrary to the central tendency of judgments as found by Hollingworth[1] and to the tendency for images to increase in size during a brief interim as found by Baldwin, Warren and Shaw.[2] The writer also found that the drawing of an estimated size of a dollar bill was almost always increased in size when corrected.[3] Evidently the standards for the measurement of a square and a circle are different. Baldwin suggests that 'circles tend to be measured by their radii but in the case of the squares the impression is that of area.'

The correlation between the estimate of the respective coins is for the first 20 women + .52, the second 20, + .65. First 20 men, + .76; second 20, + .50.

Of the women 17.5 per cent. said the circle was not in the

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[1] Hollingworth, H. L., 'Central Tendency of Judgment,' *Journal of Phil., Psychology and Scientific Methods*, 1915, 7, 461–69.

[2] Baldwin, Warren and Shaw, 'Memory for Square Sizes,' *Psychological Review*, 1895, 2, 236–244.

[3] 'Incidental Memory,' pp. 41.

center of the square cardboard while 57.5 per cent. of the men said it was not. Therefore men seem more open to suggestion here than the women. However, the probable difference in the emphasis and voice modulation between the experimenters in repeating the same statements verbatim could render this difference possible. The women on the whole do better than the men and they seem to improve in their purposive perception over their incidental perception much more than the men.

EXPERIMENT V

Forty-six college men and 20 college women, and 46 boys and 20 girls of the seventh and eighth grades were asked to estimate the weight of each of two discs, one of light wood, the other of aluminum, in terms of the commonly used coins. Each disc was made to balance, on fine scales, a new nickel. The wooden disc measured in diameter 43 millimeters and in thickness 10.5 millimeters, the metal one 55 millimeters and one millimeter, respectively. The discs were given to the subject who was told that each disc represented the exact weight of one of the coins (cent, nickel, dime, quarter, half dollar, or silver dollar) and he was asked to name the coin he judged to be represented by the respective discs. No time limit was set but the subject was not allowed to shift the discs from one hand to the other. Each subject was tested individually, and no chance was afforded the subject to know anything about the test before taking it. The results are given on the following page.

In the above table one reads "Of the 46 men 8 estimated the wooden disc at 50 cents and 2 the metal disc at 50 cents; 9 the wood disc at 25 cents and 15 the metal; 12 estimated the metal disc as heavier than the wooden disc. The boys have a strong central tendency for the wooden disc at the nickel (correct), and over half of them estimated the metal disc at the dime. The tendency for the same number of men is not so pronounced as that for the boys, and falls at the dime and quarter for the respective discs. Therefore the wooden disc is better judged than the metal disc. The same is true for the girls, but not for the women. However, the

WEIGHT OF DISCS IN TERMS OF COINS

	Men (46)		Women (20)	
	Wood Disc, Cases	Metal Disc, Cases	Wood Disc, Cases	Metal Disc, Cases
$1.00				1
.50	8	2	2	1
.25	9	15	3	3
.10	15	11	9	10
.05	11	13	4	2
.01	3	5	2	3

Metal heavier than wood	12	Metal heavier than wood	6
Metal lighter than wood	12	Metal lighter than wood	7
Metal equals wood	22	Metal equals wood	7

	Boys (46)		Girls (20)	
	Wood Disc, Cases	Metal Disc, Cases	Wood Disc, Cases	Metal Disc, Cases
$1.00	1			
.50		1		
.25	5	7	1	6
.10	10	27	10	5
.05	19	4	4	4
.01	11	7	5	5

Metal heavier than wood	20	Metal heavier than wood	6
Metal lighter than wood	19	Metal lighter than wood	10
Metal equals wood	7	Metal equals wood	4

Total, Both Sexes (132)

	Wood Disc, Cases	Metal Disc, Cases
$1.00	1	1
.50	10	4
.25	18	31
.10	44	53
.05	38	23
.01	21	20

Metal heavier than wood	44
Metal lighter than wood	48
Metal equals wood	40

number of women tested was only 20. The number of times each disc was estimated as equal to, greater, or less than the other is about the same. Grouping all the cases regardless of sex or age, there is a marked central tendency for each disc at the dime but more for the metal than for the wood. This means that most of the subjects estimated the discs as equivalent in weight to that of the dime. Apparently then the weight of the discs was underestimated. One may conclude perhaps that the weight of the dime is overestimated. This, however, would be contrary to the average estimation as found for the size of the dime in another study[1] where the size of all the smaller coins was underestimated. The surface area of the hand covered in judging the weight of the discs as compared with the surface covered by the respective coins is a disturbing factor, as well as the necessity of translating these unaccustomed objects into terms of the experienced objects (coins). There is no obvious sex difference but tremendous individual differences obtain.

Messenger[2] tested college students and other grown persons on their ability to estimate the number of one dollar bills and ten dollar bills respectively, that would equal the weight of a silver dollar. He found the average for all judgments 20.1 times, and the median 8 times the actual number required; Pierce,[3] an average of 15.8 times and median 4.3 times as great; MacDougall,[4] an average of 10.2 times and a median 1.3 times the actual number.

Conclusions

On the average one minute of time was overestimated by about one half. Six and a quarter minutes was doubled in the estimation of the women and overestimated by about one half by the men.

The estimation of a minute of filled time incidentally perceived was about the same as the estimation of empty time purposely perceived. There was, however, a slight gain for purposive perception of filled time over incidental perception of filled time, yet the gain is surprisingly small.

Wide individual differences obtain with a M.V. of from one fourth to almost one half the average. The women are slightly more variable than the men.

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[1] Myers, G. C., 'Incidental Memory,' p. 35.

[2] Messenger, J. F., 'A Note,' Science, 15, April 25, 1902.

[3] Pierce, A. H., 'Guess on the Relative Weights of Bill and Coins,' Science, 1902, 16, 474.

[4] MacDougall, R., 'Secondary Bias in Objective Judgment,' *Psychological Review*, 1906, 13, 97–120.

In estimating the sizes of the cent and the half dollar the coins were both underestimated and the estimation after incidental perception was almost as good as that after purposive perception. The women were slightly superior to the men.

The weight of the discs in terms of coins was underestimated. Wide individual differences obtain but there was no appreciable sex difference.

The slightest suggestions by the experimenter were tremendous factors in determining the observer's performance.

Much of what has been studied herein may be classed as incidental memory. Although the writer elsewhere emphasized the untrustworthiness of incidental memory in certain cases, there was no desire to minimize its general importance. Indeed, this study strongly emphasizes the place of incidental experiences in making up the total mental content.

THE CORRELATION BETWEEN THE SEX OF OBSERVERS AND THE SEX OF PICTURES RECOGNIZED[1]

BY YO SUGISAKI AND WARNER BROWN

In the course of an experiment conducted for an entirely different purpose a large number of trials were made of the ability to recognize 20 photographs once seen when mixed with an equal number of new ones. The pictures were observed at the rate of one each half-second and the test was made as soon as the 20 pictures could be shuffled up with 20 others. The pictures were of young men and women, cut from old college annuals, and were of uniform size and color. No picture was used more than once. At one sitting the first 10 of the 20 pictures were of men, at the next sitting the first 10 were of women. A tabulation of the data, each picture having been numbered according to the order of its presentation, shows that recognition is not seriously or systematically affected by the position of the picture in the series of 20.

The persons who acted as subjects were three men and three women who were college students of slight psychological training, and 15 Japanese children attending schools in Alameda and San Francisco, California, under Japanese teachers.

None of the subjects thought of the present aspect of the experiment, although remarks were occasionally made by and to the college students to the effect that pictures of one sex or the other were easier to recognize, and mild disputes arose over the matter.

The results show that 4 of the 6 college students recognize members of their own sex better than members of the opposite sex. The same statement holds true for 5 of the 6 Japanese girls and for 4 of the 9 Japanese boys. An inspection of the

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[1] From the Psychological Laboratory of the University of California.

tables (Tables I. and II.) shows that for college men and women of the same social status as the persons represented in the pictures it is easier for a subject to recognize a picture of one of his own sex than a picture of one of the opposite sex. The

TABLE I

COLLEGE STUDENTS

Subject	Recognized		Of a Possible Number for Each Sex
	Own Sex	Other Sex	
Miss Atkinson	151	133	190
" Collom	105	80	150
" Fisher	119	118	140
Total women	375	331	480
Mr. Hodgkin	169	147	180
" Clark	118	98	160
" McCoy	106	113	140
Total men	393	358	480

Total women's pictures recognized................. 733
Total men's " " 724

TABLE II

JAPANESE CHILDREN

Subject	Recognized		Of a Possible Number for Each Sex
	Own Sex	Other Sex	
Miss Fujiwi	172	142	210
" Takagi	134	131	180
" Ishibashi	180	163	220
" Morimoto	161	162	200
" Towata	149	139	200
" Sano	155	130	180
Total girls	951	867	1,190
Master Fukuzawa	170	159	210
" Tsuchiya	131	125	200
" Towata	114	101	170
" Miyanchi	114	110	200
" Yamagami	111	122	180
" Joseph	115	124	180
" Nakao	119	128	180
" Kiro	78	89	180
" Takeo	81	114	180
Total boys	1,033	1,072	1,680

Total women's pictures recognized................ 2023
Total men's " " 1900

pictures of neither sex possess any intrinsic advantage over those of the other; men's pictures are as easy to remember as women's in spite of the greater variety of dress and coiffure in the latter. But in the group of Japanese school children, who are of a different age and race from the persons represented in the pictures, the generalization does not hold. Women's pictures are more easily recognized by the girls, and also, though with many exceptions, by the boys. It may be noted that the work of this group of boys is distinctly inferior to that of the girls. On the surface of the figures, however, it appears that for these children there is an intrinsic advantage for the women's pictures, probably because of the greater variety of dress, etc., which overrides the possible preference of boys for men's pictures and is added to the actual preference of girls for women's pictures.

The characteristics of the pictures which led to recognition in this experiment have been carefully investigated through the observations of Miss Rose Wolf, a student with a good memory and thoroughly trained in psychological judgment. The observer accompanied each recognition with a statement of the basis on which it was made according to the captions of the following table (Table III.). No account was made of the failures to recognize nor of the cases of false recognition. It will be observed that this observer failed to recognize only 16 of the 380 pictures. All of these mistakes were made on pictures of men.

TABLE III

ANALYSIS OF RECOGNITION

	Men's Pictures	Women's Pictures
Decoration; clothing, manner of dressing the hair, etc.	46	102
Expression of face	82	40
Position; front, side, etc.	4	1
Resemblances to acquaintances	3	5
Expression and decoration combined	3	9
Position and expression combined	5	3
Decoration and position combined	1	0
Total	144	160
Total number of pictures shown	160	160

The table shows clearly that clothing, decoration and facial expression are the most important criteria of recognition. Clothing and decoration help most in the case of women; facial expression in the case of men. As this observer is a marked example of the preference for pictures of one's own sex, the figures are capable of interpretation on the supposition that she was more familiar with the details of dress and ornamentation in her own sex and hence employed this criterion to greater advantage there. On this supposition it is evident that members of either sex will have an advantage with pictures of their own sex on account of the factor of dress and ornamentation, assuming that other factors remain constant.

ON CERTAIN FLUCTUATIONS IN CEREBRAL FUNCTION IN APHASICS

BY SHEPHERD IVORY FRANZ

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The results of the experiments recently reported by Brown and Sherrington[1] on the reversibility of action of certain motor centers in the cerebral cortex have again called attention to certain fluctuations in the activity of the cerebral mechanisms. These authors have shown that if at different times a so-called flexion point be stimulated the resultant activity may change, and instead of the normal flexion there may be produced an extension. Observations of somewhat similar reaction reversals have been made in diseases of the nervous system in man which, while not as anatomically well correlated as those of Brown and Sherrington on the monkey, have also some importance in the general consideration of the modes of cerebral activity. It might be hastily concluded that acceptance of these facts would force us to return to an earlier conception of the method of cerebral function, but it is perhaps more correct to conclude that the facts show the inadequacy of our present-day widely accepted views and of the exactness of our knowledge of the cerebral activities and of the cortical interrelations.

By experiments on *Rhesus* monkeys I have also shown that in different brains and in the two hemispheres of the same brain there are variations in the extent and in the location of the motor areas for the limb segments.[2] This fact has a direct bearing upon the question of variations in functional activity of the cerebrum, similar to that of the reversibility of action which has been described by Brown and Sher-

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[1] Brown, T. G., and Sherrington, C. S., 'On the Instability of a Motor Point,' *Proceedings of the Royal Society*, 1912, B **85**, 250–277.

[2] Franz, S. I., 'On the Functions of the Cerebrum: Variations in Distribution of the Motor Centers,' *Psychological Monographs*, 1915, **19**, No. 1, 80–162.

rington, although it relates particularly to the variations in function of the similarly located collections of cells in the cerebral cortices of different animals.

In the course of several series of experiments with patients who were motor or sensory aphasics (using these convenient but misleading terms) variations in the daily capability to name certain objects, etc., were found. Since the observations indicate how fluctuating cerebral activity may be, they are here reported because they may be of interest in connection with other reports of variations in cerebral activity. Systematic observations have been made on four patients, who have been the subjects of an investigation of the rapidity, quantity, and character of speech reëducation.[1] All of the subjects showed variations such as will be described, but reports will be made at this time of only some of the variations in naming which have been shown by two of the subjects, G. and W. Summaries of the clinical histories of these two patients follow.

G., aged thirty-two, was a corporal in the Coast Artillery Corps. In 1907, after venereal infection he had intermittent headache and five years later confusion, with dulness and aphasia. He was unable to find words, but he understood simple orders. He was unable to write and did not talk voluntarily, and did not understand written questions. No paralysis of the vocal apparatus was discovered. The diagnosis was made of 'cerebral syphilis, tertiary, probably vascular, causing motor aphasia, agraphia, and partial apraxia.' The reason for the last (apraxia) is not given in the history. An examination two months later showed that some phrases and words could be said, such as the common 'yes, I know, but' in answer to questions, and in attempting to give the names of objects. He understood, as shown by his actions, some questions regarding himself and his family history, and he carried out simple acts at command, but not always. He was reported to be confused, but he played cards well, although he could not count and had to have another patient do the 'bidding' for him. Some slight

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[1] A detailed account of the results of these experiments will be published later.

paresis of the face was suspected. Six months subsequently a clinical note was written to the effect that the patient had written a letter which was quite intelligible, but a few days later when he was asked to write out a simple request for parole he could not do so. This story is repeated several times in the history, as is likewise that of his ability to talk fairly freely on the wards with other patients and with the nurses, but not with the physicians. It was noted that 'his spontaneous speech is good, and he is able to make his wants known.' In three months' daily examination of the patient I was unable to confirm the statement regarding his ability to write, and that regarding his ability to talk freely. He was able, by the use of simple words, by gestures, and by a few expressions, to make his wants known, and to tell about himself to a limited extent, but his conversation could not be considered to be free. He made gross mistakes in copying the alphabet, he could write only a few of the letters voluntarily, and he could not write the names of objects which were presented to him. He could read aloud words and simple sentences well, but he could not repeat the names of some of the simplest and most commonly used objects. During the period of three months he did not on any occasion speak as freely as W., the other patient who is reported here. At no time did he give evidence of being confused. It is unfortunate that at least a copy of the letter, which we now know would have been a very important document, was not kept, and that accurate records of his supposed 'free' conversations with nurses were not made. It is possible, but lacking the documentary evidence very uncertain, that we may have had in his case some of the more marked variations in cerebral activity and cerebral ability of which minor ones are reported in this article.

W., aged thirty, poorly educated, an infantry private, became paralyzed about eight years ago, when he was serving in the Philippines. An Army surgeon certified that his paralysis and aphasia were due to 'a gumma in the left cerebral hemisphere,' but the reason for this conclusion is not apparent. The Wassermann reaction with the blood serum has shown a 'trace' only once. Six years ago he could point out only a few objects which were named, but he understood simple questions. A clinical note written about a year later said that 'this patient is suffering from right hemiplegia with complete (*sic*) aphasia. He understands everything that is said to him; if his right hand was not paralyzed he could write his answers.' During the past five years it was repeatedly noted that he showed improvement in speech, but at the time the tests began there was a marked inability to name many objects, pictures, etc., which should have been familiar to him, he could not read the simplest words, and he could not write. Tests showed that he did not understand much of what was said to him, excepting simple sentences or clauses referring to his daily life and his wants. His spontaneous speech was made up of a number of names of objects which he saw almost daily, a favorite oath, and a few words indicating different activities, but he was able to repeat every name and simple phrase which was said to him. This patient appeared to be lazy or indifferent at times, but when his attention or interest appeared to wane he was stimulated to renewed activity. His efforts to learn were shown by his actions. He would repeat to himself a number of words in order to find the suitable name to speak, and he would clench his fist, wrinkle his brow, and show other evidences of mental effort.

In addition to teaching the names of objects of a familiar character the patients were also taught the names of ten ordinary colors (black, blue, brown, gray, green, pink, purple, red, white, and yellow) and ten shapes (circle, cross, diamond, heart, moon or crescent, oblong, oval, square, star, and triangle). The two series just mentioned were selected to obviate as far as possible special familiarity, although it was presumed that all subjects were familiar with the simple color names but not particularly familiar with the shape names. In the series with the different patients different procedures were used, but in all the name of each object, color, or shape, was given to the patient at least five times each day. In the object series with G. the names were given at least ten times each day. Each object, or color, or shape, was 'named' by the patient ten times each day. In the following account the serial days, omitting Sundays and holidays, are mentioned and the total number of times a particular stimulus has been named for the subject can easily be calculated.

In the 'object' series with G. the fluctuations in naming are very apparent. Some of these are as follows: *Hinge:* For four days this name could not be given, on the fifth day it was correctly named seven times (out of a total of ten), but on the following day there was a decrease to three, subsequent to which the course of learning was regular and progressive with only slight variations. *Straw:* This name was given once on each of the first two days, five times on the third day, once on the fourth, twice on the fifth, three times on the sixth, not at all on the seventh, and thereafter the acquisition of the name was rapid and regular. *Button:* This was one of the most difficult names for the patient to acquire. For seventeen days the name was not given correctly once, although he had heard it at least 170 times. On the eighteenth and nineteenth days the name was given correctly three and four times respectively, but on the five succeeding days it was not given once. *Cannon:* On the first day this object was correctly named five times, on the second day not once, and on the third day it was correctly given seven times. The complete disappearance of the name for this object on the second day is the more remarkable in view of his previous occupation, namely that of an artilleryman. The variations in naming ability described for these four objects can be paralleled by a number of others, all of which show during one or more days a considerable degree of ability to name the object followed by a more or less complete loss of this ability, and a very quick return to the former level.

In the shape series with W. similar variations were noted. *Oblong,* which was named on the average more than five times in ten from the eighth to the thirteenth days, could be named only once on the following two days, and after a period of mediocre ability there was a complete inability noted for the forty-fifth, forty-seventh and the forty-eighth days. Similarly with *square;* on the second say the name was given correctly five times in ten, but thereafter until the twenty-eighth day the name could scarcely be considered part of his usual vocabulary, since he used it correctly only twice on the fifth day, once on the ninth, and once each on the twenty-third, twenty-fourth, twenty-sixth, and twenty-seventh days. The naming of *triangle* also showed similar effects. Although difficult at first there was a gradual progression in learning until the seventeenth day, there being on the ten previous days an average of over five out of ten trials correct, with a maximum of nine on the sixteenth day. On the seventeenth day, however, the name could not be correctly given once in the ten trials, and on the following day it was given only once. During the succeeding eight days there was an increase in naming ability, average of over four in ten trials, which was followed by relative inability for two days, on each of which the name was used only once. On the following two days the name was correctly given nine times each. The subsequent period, although showing fluctuations, was one of gradual perfection. In this series also we have to note the fading away of a name for a period of one or more days followed by a return to a normal training level.

The 'color' series with W. also showed fluctuations worthy of note. *Black* on the third day and on the fourth day was named correctly ten times, on the fifth day, once, on the sixth day, seven times; on the seventh day, ten times. Here we find the almost complete dropping out of a name which appears to have been thoroughly learned. More remarkable, however, are the results for some of the later days. On the eighteenth day the name was correctly given five times, on the nineteenth day not once, on the twentieth day five times, and on the twenty-first day ten times. *Purple:* On the sixteenth day this name was given nine times; on the seventeenth day ten times, on the eighteenth day not once, on the nineteenth day five times and on the twentieth day seven times. The variations in the ability to name *blue* were even more marked. Some days it would range very high, to be followed by a number of days with no apparent ability. The weekly percentages of correct responses (sixty tests each week) give the plainest indication of the course of the fluctuations. During the first week there was fourteen per cent. correct; in the second week, thirty per cent.; in the third week, twenty-three per cent.; in the fourth week, fifteen per cent.; fifth week, four per cent.; sixth week, nothing; seventh week, ten per cent.; thereafter a gradual increase in ability. These three examples show sufficiently the irregularity from zero to almost complete accuracy, or from almost complete mastery of the name to no ability.

The three series of tests show the same condition, namely, a variation from day to day in the ability to name certain objects, etc., instead of a gradual increase with minor variations such as is to be expected in a normal training series. At the same time these variations run to extremes of full accuracy to no ability, or the reverse. It is to be noted that these variations do not correspond by days, for there may be a loss on one day for one name and on another day when that name is suddenly picked up there is a loss for another name. Nor do the losses occur only once in a series with each object. In a few cases there is an apparent high ability followed by a loss of this ability, and after some days with a re-acquisition a second loss or great decrease. The case of 'black' in the series with W. is most instructive, because from his actions it was apparent that he knew the difference between that stimulus and the other stimuli in the series, but the name could not 'break through.' On certain days the only names he could give for this color were 'dark' and 'pitch dark,' although in the periods of training which accompanied the naming periods each day he showed an appreciation of his inability to get this name and frequently repeated to himself the word 'black' a number of times in order to get it fixed. For brief periods in different series other similarly obsessive-like names were given by all subjects, but not to as great a degree as in the case of W. with black.

Variations of the character described are probably similar to those difficulties in finding (or saying) the exact word necessary for the expression of an idea which is sometimes experienced by normal people (assuming, of course, that not all of the naming difficulties are like those described by Freud and his followers). The inability of W. to give the name 'black' and his obsessive-like use of the terms 'dark' and 'pitch dark' are also like some experiences of normal people. The variations do not, I believe, resemble the 'attention' fluctuations which have been extensively studied by experimental psychologists, and which may have correlations with respiratory waves, for the subjects were allowed thirty seconds in which to give the name of a particular object, and in the 'color' series with W. when the wrong name was given he was told that it was wrong and he was permitted to give a second or a third or any number of names within the thirty-seconds period until he was satisfied that he had given the correct one. Nor are these results similar to the fluctuations which have been described by Küchler as being prominent in the naming ability of aphasics.[1] Küchler found that the addition of new objects and new words to a series interfered to a considerable degree with the percentages of correct answers for those things which had been in process of acquisition. This result is, however, only an example of exaggeration of the normal kind of disturbance in learning. In the 'color' and 'shape' series the same objects and the same number of objects were used each day, and the fluctuations which have been found can not, therefore, be due to newly added stimuli acting as interferences. In the 'object' series with G. the number of objects remained constant, but after the first six days when the name of an object had been learned a new object took the place of the old. Fluctuations due to the addition of new objects may have taken place, but in some of the examples which are noted above the great fluctuations occurred on days when no new stimuli were presented. At the same time it should be said that on the days when new objects were introduced into the series not only was there not always a decrease in ability to name the old, but in fact there was frequently a progressive increase.

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[1] Küchler, H., 'Ein Fall von Wiedererlangung der Sprache nach neun Jahre alter Aphasie,' *Prag. med. Wochensch.*, 1893, **18**, 507–508; 520–522; 535–536.

The present results are more like the fluctuations in motor power which have been observed in paralyzed limbs by Sternberg,[1] phenomena which I have had opportunity to observe in cases of cerebral hemiplegia. Stertz[2] has also recorded considerable variations in the time of reactions and in other motor responses of patients with cerebral diseases. The interesting patient who has been studied by Stern[3] should also be mentioned in this connection for he showed not only fluctuations in motor power and coördination but also in the sensory fields. In the last mentioned case, however, the fluctuations appeared to come suddenly and they may have been of the nature of 'petit mal' attacks, although an epileptic condition was not obviously present. In the course of writing his name this patient would begin to lose some of his ability and the illustrations which are given by Stern show how rapidly the variations appeared. In connection with the fluctuations which I have found in aphasics mention may be made of an interesting observation of Brown-Séquard. This observation has also great interest in connection with the question of the possibilities of reëducation of aphasics as well as with the general subject of cerebral function. Brown-Séquard mentions fluctuations in the ability of aphasics to speak and records as a fact that 'there are cases of aphasia where the diseased person has had the power of speech restored during delirium.'[4]

The results obtained with the aphasic patients cannot be

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[1] Sternberg, M., 'Ueber die Kraft der Hemiplegiker,' *Dtsch. Zsch. f. Nervenhk.*, 1908, **34**, 128–153.

[2] Stertz, G., 'Ueber periodische Schwankungen der Hirnfunktion,' *Arch. f. Psychiatr. u. Nervenkr.*, 1911, **44**, 199–155.

[3] Stern, R., 'Ueber periodische Schwankungen der Hirnrindenfunktion,' *Arch. f. Psychiatr. u. Nervenkr.*, 1895, **27**, 850–917.

[4] Quoted in the *London Medical Record*, 1874, **2**, 334–335, from a summary of a lecture published in *Nature*, from advanced sheets of the report in the *New York Tribune*. I have been unable to find a fuller account of this interesting lecture, unless a brief article published ten years later is the one which contains the facts referred to (Brown-Séquard, 'Persistance de la parole, dans le chant, dans les rêves et dans le delire, chez des aphasiques,' *Comptes Rendus de la Soc. de Biol.*, 1884, 8 sér., **1**, 256–257). In that article the author mentions a case of motor aphasia in which there was a considerable return of speech during delirium, the patient using words to describe his hallucinations during that added abnormal condition.

satisfactorily explained by themselves, but in connection with other experimental and clinical facts they indicate a degree of variability of cerebral functioning beyond that which we usually consider may be present.[1] Several explanations of the condition suggest themselves, the more probable being that we have in these cases examples of the variations in permeability of neuronic connections or in the degree of smoothening out of paths. That we may be dealing with a cerebral condition akin to that diseased state known by the French as 'intermittent claudication' is a possibility, but in view of our lack of knowledge regarding localized variations in blood supply in the cerebrum the explanation cannot be seriously considered at the present time. Whether or not the facts can be satisfactorily explained they point to a less exclusively machine-like view of the method of cerebral activities and in this way they lead to a better understanding of the activities of collections of cerebral cells which are commonly called centers.

[1] We cannot, of course, exclude the so-called lower centers when we deal with motor phenomena, or with behavior of the type under discussion.

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THE EXISTENCE AND FUNCTION OF INNER SPEECH IN THOUGHT PROCESSES

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The purpose of this experiment was to discover the functions of inner speech in the thought processes, particularly in the apprehension of meaning in reading. That is, Is inner speech an important aid either in the apprehension of meaning or as a vehicle of thought, or is it merely an incidental by-product of these processes? A considerable literature has been produced upon this subject but it is mostly introspective in character, very little having the stamp of objective certainty. By way of introduction it will be helpful to refer to two important papers that were among the first to appear upon the subject, those by Dr. S. S. Stricker and M. Paulham.[1]

In a small monograph, which appeared in 1880, Dr. Stricker, reporting principally his own introspections, says that he cannot call up images of any sort without making incipient speech movements corresponding to their verbal symbols. For example, he cannot imagine the sound of B without an incipient feeling in his lips nor think a tune without incipient singing movements in his larynx. He says he cannot understand any words without reproducing them in inner speech or think of father and mother at the same time because the accompanying speech movements must be made successively. After a long practice period he, however, learned to read a newspaper while repeating aloud a ballad of

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[1] For other literature on the subject see Pintner, *Inner speech during silent reading*, Psych. Rev., 22, 1913, 129f; and Wyczoikowski, idem, 326f.

Schiller, but in this case, he says the speech movements came in between the words of the ballad. Dr. Stricker tended to give his introspections objective value by questioning a hundred persons in regard to lip-feelings accompanying M, P, and B in silent thought, and found only one who did not experience them. He also questioned sixty persons in regard to inner speech accompanying silent reading and found only two who did not experience it.

In 1886 M. Paulham offered his own introspection in reply to those of Dr. Stricker. He said that he could sound the vowel A continuously and at the same time get images of all the other vowels. He can image the sound of any vowel without any motor accompaniment. Inner speech is an individual peculiarity. Thought may be represented by other forms of imagery, auditory, motor, visual, or even abstract representations which are unlike any sensible image. In fact, words may be understood without any images. All that is required is to prepare the proper reaction to which the word is a stimulus.

For Stricker inner speech is the indispensable vehicle of all thought; for Paulham it not only is useless but even does not exist. These two cases illustrate the confusion which results from the introspective method as well as the necessity of finding a method which will yield objective validity with reference to the phenomena in question.

To settle this confusion, experiments were begun with the view of getting objective evidence of the existence of inner speech. After devising several instruments for registering inner speech movements, I finally settled upon a speech receptor illustrated in Fig. 1, and believe it successfully registers speech movements when they occur. It registers only tongue movements, but it is very probable that tongue movements are made whenever speech movements of any sort occur. If this is the case, objective evidence of inner speech may be secured from registering tongue movements alone.

This instrument is made of a block of wood 5 × 2 × 1.5 cm. in size, one end of which is cut in the form of a mortice

3 × 2 × .5 cm. To this block two thin steel wires in the form of *a b c d e f g* (Fig. 1) are fastened, over which is stretched a condom of very thin rubber. This is fastened airtightly to the cut end of the block by a wrapping of rubber sticking tape. The wires and condom form a drum which is sensitive to the slightest pressure. A hole drilled through the long way of the block admits a pipe stem to which a rubber tube from a writing tambour is attached, thus forming an air passage between the tambour and the speech receptor. The drum as thus constructed is put into the mouth cavity, which it fills, and is held in place by closing the lips and teeth upon the morticed end of the block. Breathing or movements by the tongue produce changes in air pressure which can be registered.

The records were taken on smoked paper rotated by a kymograph. First a record was taken of the breathing when the subject was told not to think of anything. Above this a so-called silent reading curve was taken while the subject read a brief newspaper clipping. This was continued by a so-called writing curve taken while the subject wrote a review of the clipping. Above the writing curve a so-called whispering curve was taken while the subject reread the clipping in a whisper. Sometimes a so-called reading-aloud curve was taken above the whispering curve for the same passage. The curves arranged in this way are easily compared. If, then, we find that the silent reading curve is like the breathing curve, it may be taken as evidence that the subject does not make speech movements in reading. If, however, it is like the whispering curve, then it may be taken as evidence of inner speech, for, by analogy, something similar to the whispering occurs.

Since the speech receptor records not only the tongue movements but also the changes in air pressure due to breathing, some means had to be found for separating these two elements. To do this I took breathing curves from three other places, the nose, the chest, and the abdomen. For the chest and abdomen pneumographs were used. For the nose I used a glass tube held in one nostril by means of a rubber nipple. For the use of this instrument I am indebted to Professor Raymond Dodge, who suggested it. As illustrated in Fig. 2, one end of a small glass tube is pushed through the end of the nipple and is kept from slipping by wrapping the tube with rubber sticking tape underneath the nipple. The bulb of the nipple fits tightly into the opening of the nostril and holds the instrument firmly while in use. It does not interfere with breathing, since the subject has the free use of the other nostril. The other end of the glass tube allows a rubber tube connection with a writing tambour.

Each of these instruments was connected with its own writing tambour, the needles of which were sharpened in different ways so as to make the graphs distinguishable. The graphs, together with the time line, for which a Jacques chronograph was used, were all taken at the same time and in the order described above. The subject thus equipped with a speech receptor in his mouth, one pneumograph about his chest, another around his waist, a rubber nipple and glass tube in his nose sat comfortably at a table, read newspaper clippings silently, in whisper or aloud; wrote abstracts; counted mentally, in whisper or aloud; performed mental multiplication problems; or added columns of figures as directed by the experimenter. After each performance the subject's introspections were noted. These will be compared with the graphic records which we are now prepared to study in detail for each subject.

To begin with the curves secured from Mr. D. are very interesting. Record 1 shows the breathing (*B*), reading (*SR*), writing (*W*), and whispering (*Wh*) curves taken in connection with clipping No. 4. The breathing curve is at the bottom of the record. It is a series of regular hills and valleys. The steep slope of the hill is the graph for inhaling. The long slope may be analyzed into two steep slopes connected by a line relatively horizontal, which indicates not only that inhaling has a relatively longer duration than exhaling but is made up of two inhaling movements with a period of relative rest between them. Next to the breathing curve came the silent reading, the writing, and the whispering curves in order from bottom to top. The silent reading curve not only indicates deeper and faster breathing but shows numerous irregularities in the form of sharp peaks all along the line. It is improbable that this subject breathed in such a jerky fashion. These numerous sharp peaks almost certainly indicate that the subject spoke the words silently to himself as he read the clipping. This becomes all the more evident when the silent reading and whispering curves are compared, the only difference appearing to be in the magnitude of the tongue movements. The likeness between the silent reading and whispering curves is still more striking in Record 2[1] for which the subject read one word per second to the beats of a metronome. This procedure evidently exaggerated the tongue movements. In Record 3 the silent reading (*SR*), the whispering (*Wh*), and the reading-aloud (*RA*) curves for the same passage together with the time line and breathing curves are shown. There is a very general similarity between the reading-aloud and the whispering curves, showing a corresponding similarity in speech movements. The silent reading shows much more similarity to the whispering and reading aloud curves than it does to the breathing curve. Surely some element quite foreign to breathing comes in here. Comparison of the curves indicates that it must be inner speech. However, not to be too hasty in coming to this conclusion I took the records of individual words for the same passage, a part of which is shown in Record 4, in which the subject read one word every other second to the beat of the metronome. In the reading aloud curve I made a dot at the completion of each word so as to make clear the amount of graph due to each word. I then wrote underneath each division the corresponding words. The silent reading curve was taken under the same temporal conditions. Guided by the amount read, the word order, the time order, and the almost identical form of particular graphs such as those for 'juryroom,' 'general sessions,' etc., I wrote out the words for the silent reading curve. There is no doubt in this case that the subject used inner speech in his silent reading and

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[1] Records marked with a star are omitted.

that the speech movements were very similar to those made in reading aloud. If now we compare the silent reading curve of Record 4 with that of Record 3 for which the subject read normally we see that these are much more similar to each other and to the whispering and the reading-aloud curves than to the breathing curves. Undoubtedly the same cause explains the irregularities of both curves, that is, inner speech. Records 1, 2, 3 and 4 were taken with the speech receptor alone which registers the impressions from both the speech and the breathing processes. In Record 5 the breathing curves from the nose, chest and abdomen are shown in relation to the curve from the speech receptor. The breathing curves from the four receptors show about the same amount of regularity, the one from the mouth being slightly more varied than that from the nose and chest but no more than that from the abdomen. In the silent reading curves the breathing curves from the nose, chest, and abdomen are just as regular as in those below but the curve from the mouth is vastly different, having numerous peaks not contained in the other curves. If these peaks were due to breathing, they would appear in all the curves, for the air pressure within the nose, mouth, and lungs must be the same. In the whispering curves the breathing is more irregular than in silent reading, but the great peaks coming from the speech receptor are certainly not due to breathing, but to speech movements, which also account for the irregularities in the silent reading curve. With this I take it that the objective certainty of inner speech in silent reading and in writing in case of this subject is established.

After having tested my subjects for inner speech during silent reading and writing, I also took some graphs while the subjects performed mental multiplication problems; counted, mentally, aloud, and in whisper; and added columns of figures. In Record 6 a section of the graph for multiplying mentally 98 × 15 and 258 × 93 are shown. The graph marked *Wr* × 15 is the writing graph for the clipping used for Record 5. It shows inner speech very clearly. The graph for mental multiplication 98 × 15 shows inner speech for that process. But the 258 × 93 shows very little inner speech. What is interesting here is that the breathing becomes fast and very short. In Record 7* the graphs from the speech receptor are shown for breathing, mental counting, counting in whisper and counting aloud. The subject counted from one to ten to the beat of a metronome. No inner speech appears in the mental counting. But the graph shows that the subject counted with his breath, there being five peaks for the ten counts, one count for each exhale and inhale.

Reporting his introspections Mr. D., whose records we have been studying, says that he pronounces the words to himself in silent reading and in writing. He thinks he sometimes whispers them halfway. In mental multiplication and in addition he repeats the figures over to himself. In fact all his thought processes are carried on in the form of inner speech. When I called his attention to the mental counting, he admitted that he did it with his breath. His introspections agree with the graphic records with the possible exception of difficult problems in mental multiplication.

The next subject to be studied is Mr. E., whose records are very different from those of Mr. D. Record 8, which is typical for this subject, shows no difference between the silent reading and breathing curves, both being perfectly regular and continuous lines. The writing curve shows a disturbing element, probably a faint inner speech. In Record 9* the silent reading curve clearly shows the presence of inner speech, but none appears in the writing. Out of a dozen records taken from this subject, this is the only one that shows inner speech in silent reading. The subject, however, read apparently in his usual way and was surprised at the result when I showed him the record. Record 10 shows the breathing, the silent reading, the writing, and the whispering curves from the four receptors. The only difference between the breathing and silent reading curves is the slower breathing in the latter. Some inner speech appears in the writing as is indicated by the sharp peaks which are similar to those in the whispering curve.

Record 11* shows the curves for counting mentally, in whisper, and aloud from one to ten inclusive. The breathing for the mental counting is slightly more irregular than in the silent reading, but hardly enough to indicate inner speech. The great difference between this and the whispering and counting aloud curves makes this all the more probable. Record 12 shows the curves for addition and multiplication. Add 3 is the curve for a single column of figures, while Add 5 is the curve for adding a double column of figures at once. These columns were twenty-six digits high and contained no zeros. The subject had the card with the column before him and was told to add silently in his usual way. The use of a pencil was permitted, the subject writing down the result when the column was completed. It may be remarked that adding two long columns of figures at the same time is a very difficult process, much more so than the mental multiplication of three-place numbers. It would seem that in such a difficult mental process, inner speech would most certainly appear if it has any function at all, especially when the subject nears the upper end of the column. The curves, however, show no inner speech at any stage of the addition or mental multiplication processes, the curves being excellent specimens of pure breathing curves. The two lower graphs in Record 13 show two stages of the mental multiplication of a three-place number. The lower one is at the beginning while the upper one is near the end of the process. No inner speech appears. The third graph in Record 13 is the curve resulting when Mr. E. held the drum of the speech receptor between his lips while adding a single column of figures. I had noticed lip movements while Record 12 was taken and therefore took the lip graph. Mr. E. said that he always made these movements while adding but did not know whether they were of any use. They may result from his usual custom of adding aloud. Record 14 shows the curves for addition and mental multiplication from the four receptors. The absence of inner speech is conspicuous throughout.

Reporting his introspections, Mr. E. says that if he makes speech movements in silent reading he does not know it, but he experiences a peculiar tension about the throat and larynx, and in the back of the tongue in writing. In his best reading he hears the sounds in his mind. Particular writers have their peculiar forms of auditory rhythm. He has a tendency to pronounce difficult words, and feels peculiar twists in his mouth when writing certain words, *e. g.*, incompetent. He has vague visual images of the printed forms in reproducing a passage in writing but he cannot read them. In the complete memorizing of a passage, these visual images either become very distinct or disappear entirely. He says they aid him in memorizing and recalling, especially the particular part of the page on which the statement was made. In mental multiplication he visualizes the process as if he were doing it with pencil and paper. And in addition he visualizes the products as he goes up the column.

Mr. E.'s introspections nowhere contradict his graphic records. The peculiar twists and the peculiar sensations in the back of the tongue that he experiences when writing is very probably a faint form of inner speech, as the graphic records show. His records and introspections also show that mental processes need not be conducted by inner speech as is the case with Mr. D., but can just as well be carried on in other forms of imagery, for example, auditory and visual.

In the case of Mr. A. inner speech does not appear either in silent reading or in writing as is shown by Record 15*; or in mental counting as is shown in Record 16*; very slightly in mental multiplication, as is shown by the three graphs above 68 × 59 mentally in Record 17*; inner speech appears slightly in the addition of a single column, as per the graph marked Add 11, and very marked in the addition of a double column, as per the graphs above Add 6. The most marked feature of the graphs for these arithmetical processes is the change in breathing. When the curve at the bottom, which is for normal breathing, is compared with those above, this change becomes very conspicuous. Breathing becomes much deeper and faster in the mental multiplication, shorter and slower while adding the single column, deeper and faster again for the adding of the double column. This seems to show that Mr. A. breathes faster and deeper while performing difficult mental problems.

According to his introspections, Mr. A. pronounces the words to himself in silent reading, in writing, in mental multiplication, and in addition, but he does not do so in mental counting. In mental multiplication he visualized the process while adding the partial products. He has occasional visual images of the objects read about, and these, he says, aid him in recalling. The graphic records show inner speech only for difficult arithmetical processes, but none in the silent reading or writing. His report of inner speech for the latter may easily be mistaken for auditory imagery.

The graphs of Mr. C. show a little inner speech for silent reading as per Record 18* and for addition as per Record 19* but none for mental multiplication. An interesting feature in the latter is that he held his breath. He multiplied 37 × 45 correctly in 14 seconds. He failed in 243 × 458 but held his breath during the entire attempt. This is additional proof that he did not use inner speech which I believe impossible while holding the breath. Mr. C. does not think he used any imagery in the mental multiplication except possibly touch and kinæsthetic and a little visual while adding partial products. He says he has muscular feels of the digits up to 20 as if going up a stairway. In ordinary life he adds aloud and describes the forms of figures in the air with his fingers. He thinks the speech receptor inhibited his usual processes. At any rate he added the columns correctly after it was removed.

Mr. B. does not show any inner speech in silent reading but it does appear in writing as per Records 20*, 21*, and 22. For Record 21 he read German where the reading curve is little more irregular than in Record 20. It scarcely evidences inner speech except a faint tendency.

It occurred to me that the little waves appearing in the writing curves might be due to the pulse. Instead of the abdominal breathing curve, I therefore took a graph for the pulse which appears in Record 22. There appears, however, no correlation between the little hills in the curve from the speech receptor and the pulse beats. In the section shown in Record 22 forty-eight pulse beats are shown in forty-three seconds but sixty-seven little tongue movements are recorded, which shows clearly that the tongue movements are not due to pulse. Two other subjects tested for the same purpose showed similar results. Mr. B. showed no inner speech in the adding of a double column as per Record 23*; nor in mental counting, as per Record 24*; nor in the mental multiplication of 624 × 357, as per Record 25*. This was his first attempt at such a task which he found extremely difficult, but yet no inner speech appears in the graph.

According to his introspections taken after one performance, Mr. B. says he does not pronounce words in silent reading and in writing except in difficult cases. He gets the meaning direct from the visual form. In writing he believes the words come to his mind in their sound values. In adding he uses only auditory imagery. In mental multiplication he repeats the figures over to himself and visualizes the process as if done with pencil and paper. Records 20 to 25 are almost wholly in agreement with these introspections. For other performances, however, my notes state that he pronounces the words in reading, in writing, and in the arithmetical processes. Introspection thus yields contradictory reports. The graphic records, however, give important data for evaluating them.

Mrs. E., who was tested only for inner speech in reading and in writing, shows none in the former but it is quite marked in the latter. Reporting her introspections, she says that she pronounces words when writing but not when reading. This agrees with her graphic records.

According to Records 28* and 29, Mr. MC. does not use inner speech in silent reading. The little breaks in the writing curve indicate its presence there. This is more apparent in Record 29 than in 28. He uses it in a mild form in mental multiplication and in addition. In any case, the curve for these processes is more irregular than for the breathing alone. No inner speech appears in mental counting, which was probably done with the breath.

Reporting his introspections Mr. MC. says that in silent reading he hears the words in his mind, but he has a tendency to pronounce in writing. In mental counting he used auditory imagery and breathing. In mental multiplication and in addition he said the figures over to himself but does not think he made speech movements except near the end of the process. In adding the partial products in multiplication he used visual imagery. With respect to inner speech these observations agree with the graphic records. They also indicate the adequacy of visual and auditory imagery as vehicles of thought.

According to Records 33 and 34*, Mr. M. does not use inner speech in silent reading but he does in writing. The meaning of the curve for inner speech in addition and in mental multiplication as per Records 35* and 36*, is doubtful, although the breathing is more irregular and occasionally a sharp peak appears. The change in breathing during mental multiplication and addition is particularly evident in Record 37*. During the 69 × 58 mentally this subject breathed on an average once every 2.8 seconds. After doing 243 × 458 mentally, and adding two single columns and one double column, he breathed only once in 3.2 seconds.

Reporting his introspections Mr. M. says that he does not use inner speech in reading, but has a tendency to do so in writing. In the latter process he has visual images of the printed forms and reads from these images, which, however, soon fade away. In mental multiplication he tried to visualize the process but failed while adding the partial products. This failure in imagery prevented him from getting the correct results. In addition he thinks he said the figures over to himself. The graphic records confirm his introspections on writing and in silent reading. They neither contradict nor confirm them on the other processes.

Mr. Mt. has a smooth breathing curve but the reading and writing curves are closely similar to the whispering curve, indicating very clearly the presence of inner speech in these processes, as per Record 38. Record 39 shows that he has a very marked inner speech in the mental multiplication of 37 × 45; very much less in multiplying 4356 × 7189 with pencil and paper; very little inner speech in the beginning of the addition of a column of figures, but very much when he gets near the end. He also uses inner speech in mental counting as per Record 40*.

These records are in agreement with a theory that I had in mind while trying out these tests. After seeing that not all my subjects used inner speech in reading and in writing, which processes were highly automatized for all of them, they would have to do so when it came to a very difficult and new mental process, for example, the mental multiplication of a three-place by a three-place number, or the addition of a long double column. I supposed that when a subject came to the adding of the last partial product in the mental multiplication of three-place numbers, he could not keep the partials in mind and get the correct sum without continually repeating them over to himself. Such a task would be too difficult for visualization and therefore inner speech had to take on the function. With this in mind I arranged a series of tests having a graduated difficulty from simple to complex as follows: (1) Silent reading, (2) writing, (3) 37 × 45 mentally, (4) 258 × 463 mentally, (5) adding a single column, (6) adding a double column. I was, however, disappointed to find that most of my subjects gave not the least confirmation for my hypothesis. Inner speech was no more present for them in adding a double column than in silent reading. Record 39 is one of the few that confirm this hypothesis. It is interesting to notice that when Mr. Mt. multiplied 4356 × 7189 with pencil and paper, his inner speech almost disappeared. That is, when the process was conducted by perceptual symbols, the imagery dropped out, but when the perceptual symbols were removed, they were supplied by inner speech. If imagery has any use in the workings of thought, it seems reasonable that this should be its proper function.

Record 41* indicates that Dr. P. uses inner speech in silent reading and in writing. The interpretation of Record 42*, for which the four receptors were used, is more difficult. I experienced much difficulty in getting a good record from Dr. P. because the speech receptor irritated his tongue and made him swallow repeatedly, causing the needles from the tambours to make sudden jumps and spoil the graph. But Record 41* is in agreement with his introspection.

According to Record 43*, Mrs. R. uses no inner speech in silent reading, but it appears quite distinct in her writing curve. For Record 44 she seems to have used it in both processes. In Record 45, it is not clear whether she used inner speech or not. Inner speech may be of a character to appear only on certain occasions. However, if this were true, it would be rather a strange fact, especially so if it appeared and did not appear under the same circumstances, as in case of these records. Mrs. R. uses no inner speech in mental counting, as is indicated by Record 46*. The portions of the *MC* graph not underscored are the graphs for counting from one to ten to the beat of a metronome once per second. The absence of inner speech in these graphs is evident when they are compared with the *WC* (counting in whisper) and *AC* (counting aloud) graphs, which have characteristic forms. These forms are more evident in Records 47* and 48* for which the drum rotated more rapidly. The accidental character of inner speech in case of this subject also appears in Record 49 and 50 for mental multiplication. According to the former she uses more inner speech in 98 × 67 mentally than 37 × 45 mentally and more in 258 × 63 mentally than in 98 × 67 mentally. That is, her inner speech increases with the difficulty of the problem. But according to Record 50* it is doubtful whether any inner speech appears either in 37 × 45 mentally or in 258 × 463 mentally. If inner speech was necessary in Record 49* it is difficult to see why it was not so in Record 50.

Reporting her introspections Mrs. R. says that she does not pronounce words in silent reading but hears them in her mind. These images are more vivid in writing. She thinks she sometimes pronounces the words in both processes. In mental multiplication she thinks the spoken names for the figures and visualizes the process as if done on paper. She had no difficulty in visualizing while multiplying mentally two-place numbers, but could not do it in three-place numbers, and therefore failed to get the correct result. She must visualize the partial products in order to add them. So far as inner speech is concerned these observations do not contradict the graphic records. The more vivid auditory imagery in writing is possibly mistaken for inner speech.

It occurred to me that it might be interesting to discover whether the spoken names for the digits had a fixed graphic form. Records 51* and 52 are the results of this inquiry. In Record 51* the subject spoke one digit every 1.5 seconds. In Record 52 she counted according to a signal given by the experimenter. It shows the graph for each spoken digit in its clearest form. There is much variability in the graph for the same digit spoken at different times. For example, the 8's and 9's in Record 52 are so dissimilar that it is doubtful whether one of either pair could be identified by the other. Still taking the graphs for all the digits together there is a very general similarity, enough to indicate that the same cause produced them.

Record 53 shows the graphs from Mrs. R. for the same words which Mr. D. read for Record 4. If these two are compared, the only similarity seems to be that both graphs have numerous tall and steep peaks. It would be quite impossible to identify them as graphs for the same words. Although the graphs for the same words from the same subject are relatively similar, these two cases seem to show that they are relatively dissimilar for the same words from different subjects. If this would prove to be a general law, a graphic language, even for spoken words, would be an impossibility. They rather point to the possibility of graphic language for each individual, but in the opposite direction for the possibility of reading the silent thoughts of man by attaching a harness to his vocal organs.

According to Records 54, 55, 56* Dr. S. does not use inner speech in silent reading or in writing, or in mental counting, or generally in mental multiplication. Near the end of the last process, the graph takes on a new character as if the subject whispered softly. Whether he did so or whether his tongue trembled because of his mouth filling with saliva is difficult to say. It also might indicate inner speech during the adding of the last partial products. The curves for each of these processes is not smooth but made up of numerous little vibrations. If they were absent in the breathing curve, they might indicate a slight inner speech, but as it is, it may simply be the result of a tetanic effect in the tongue muscles.

Reporting his introspections, Dr. S. says that he gets the meaning of words direct from their visual form in print. He might make incipient speech movements. He repeats words over to himself in writing, spells out long words; judges grammatical form with motor imagery by pronouncing words. He has no auditory imagery. He thinks he pronounced in the double column addition. He generally adds by counting aloud. The mental multiplication of 243 × 458, which he did correctly, although it was his first attempt, he performed on the ends of his fingers. When adding the partial products he projected the digits on the wall. His remarks on incipient speech movements and pronouncing are hardly proven by his graphic record. His introspections are interesting in emphasizing the absence of auditory imagery and the vivid presence of kinaesthetic imagery.

According to Record 57* Mrs. W. breathes more irregularly in reading and writing than in pure breathing. Whether this augurs inner speech is difficult to say. It easily might do so. She probably uses inner speech in mental counting. According to her introspections she does not use inner speech in either reading or writing or have auditory images. She has neither visual or any other sort of images.

If we make a collective summary of our graphic records for the thirteen subjects tested, Table I is the result. The existence or non-existence of inner speech is indicated by a + or – sign respectively. If the introspection agrees with the graphic records, it is indicated by a like sign to the right. If the record or report is doubtful it is indicated by a ?.

Summarizing the results roughly, we may say one fourth of the subjects use inner speech in silent reading, three fourths use it in writing, one fifth use it in mental counting, one third use it in mental multiplication and one half use it in addition. The first conclusion that appears from this is that inner speech is not a universal but an individual trait.

+ = process in question is used.
− = " " " is not used.

Subject	Silent Reading		Writing		Mental Counting		Mental Multiplying		Addition	
	Graphic Record	Intro-spection	Graphic Record	Intro-spection	Graphic Record	Intro-spection	Graphic Record	Intro-spection	Graphic Record	Intro-spection
Mr. D.	+	+	+	+	+	+	+	+	+	+
Mr. E.	−	−	+	?	−	−	−	−	−	−
Mr. A.	−	+	−	+	−	−	+	+	+	+
Mr. B.	−	?	+	?	−	?	−	?	−	?
Mr. C.	+	−	−	−			−	−	+	−
Mrs. E.	−	−	+	+						
Mr. Mc.	−	−	+	−?	−	−	?	?	+	?
Mr. M.	−	−	+	+	−	−	?	−	?	+
Mr. Mt.	+	+	+	+	+	+	+	+	+	+
Dr. P.	+	++	+	+						
Mrs. R.	−	−	+	?	−	−	?	−	?	−
Dr. S.	−	−	−	?	−	−	?	−	−	+
Mrs. W.	−	−	−	−						
	4+ 9−	4+ 8− 1?	9+ 4−	6+ 2− 5?	2+ 7−	2+ 6− 1?	3+ 3− 4?	3+ 5− 2?	5+ 3− 2?	5+ 3− 2?

56 cases
34 agreements
5 disagreements
17 uncertain

A second conclusion that appears is that the graphic records upon inner speech are more often confirmed than not by introspection. Out of fifty-six cases appearing in the table, there are thirty-four agreements, 5 disagreements, and 17 uncertain. The chances are almost 2 to 1 that the introspections are correct. But if over one third of the introspections are wrong, it would be difficult to get scientific results from these alone. To separate the right and wrong cases, some objective tests are needed. The graphic records are of assistance here.

The discovery that inner speech is an individual trait does not tell us what its function is. If we could find a uniform set of circumstances in which it always appeared, we should be able to infer its function. But the facts seem to be that most people who use it at all, use it in every mental process. A few who do not use it in simple processes, use it in difficult processes. Many do not use it at all. The question is then:

Has inner speech any function in the thought processes for those who use it? I thought that this difficulty might be solved by eliminating inner speech in some processes for which it is ordinarily used and then check up the mental product. If the elimination of inner speech is found to deteriorate the mental product, we could hardly avoid the conclusion that it is of some use for the process tested. On the other hand, if it has no effect upon the mental product, then it would be of no use to the thought functions. With this hypothesis in mind I began an experiment for testing the function of inner speech.

The essence of my method was to have some subjects perform additions and read and write reviews of newspaper clippings while repeating aloud continuously and rapidly the sentence: "Jack and Jill ran down the hill." For purposes of comparison, each subject did a series of the same tests without distraction. I chose newspaper clippings because I thought they would come the nearest to be equally interesting and uniformly difficult for all my subjects. Each clipping was cut so as to have ten points, a point being considered as one complete idea, such as a sentence or clause intelligible by itself. Two hundred and ten of these clippings were cut, numbered on the back, and put into envelopes, each envelope containing eighteen clippings. Into each envelope were also put eighteen blank yellow sheets, and a score card with blanks for the following data: Number of reading, number of clipping, time of reading in minutes and seconds, and date. The envelopes were then handed to the subjects with the following directions written upon them:

1. Take *three* clippings from the envelope, beginning with the lowest number.

2. Place a watch before you and mark the time in minutes and seconds for beginning the reading.

3. At the set time turn over the first clipping and read it through once.

4. Mark the time of reading in minutes and seconds on the score card.

5. Note time again as in 2 and write on the yellow sheet all that you remember of the *sense* of the clipping.

6. Mark the time of writing on the score card.
7. Repeat 2, 3, 4, and 5 with the other two clippings.
8. Mark on the yellow sheet the number of each clipping reproduced.
9. Fill out the remainder of the score card for each clipping.
10. Replace the three clippings in the envelope.
11. Repeat the exercise daily until all the clippings are read, and then return to the experimenter.

These specific directions were given because each of the subjects did the work by himself at his own time and place. They were qualified to do this since they were either graduate students or instructors in psychology.

Eight subjects were used. Six did three envelopes each, two did four each, and one did two envelopes. As will appear later, the order of distraction was varied among the different subjects, some beginning and some ending with distraction. After completion the abstracts were graded from 0 to 10, according to the number of points correctly reproduced.

To give an idea of the character of the clippings a few examples are reproduced herewith. The short slanting lines or marks cutting the lines of print, divide the clippings and abstracts into points as defined above.

To be Done without Distraction

Clipping No. 3

"Doolittle.—What I'm complaining of is that he has made a gentleman out of me./ Who ever asked him to do that?/ I was happy,/ I was a free man./ I have gotten money out of pretty nearly everyone,/ just as I got it out of you, Henry Higgins,/ when I needed it./ Now I'm hedged in, bound hand and foot,/ and everyone gets money out of me./ You ought to congratulate yourself, says my lawyer."/

Abstract No. 3

Some man formerly a crook was satisfied in his trade because of his happy state. A gentleman he now is. In turn he gives money rather than take unearned money.

Reading, 26 sec. Writing, 1 min. 45 sec. Grade, 0.

Clipping No. 162

"Washington, Feb. 25.—/Representative Palmer of Pennsylvania and Representative Sherley of Kentucky conferred with President Wilson today/ on ways to forward the plan for repealing the clause in the Panama Canal act/ that exempts American coastwise shipping from payment of tolls./

"The President has been reluctant about sending a message on canal tolls to Congress,/ or putting into writing the full reasons for asking for the repeal of the exemption clause./ It is said now, however, that instead of sending a formal message to Congress on the tolls question,/ the President may send a letter to some influential member asking the repeal of the exemption clause./ The letter is to deal as frankly with the tolls question as the exigencies of the international situation will permit./

"A steering committee has been formed consisting of Messrs. Palmer, Sherley, Adomson, and Covington to canvass the House./"

Abstract No. 162

Rep. Palmer of Penn. and Rep. Sherley of Ky. called yesterday on Pres. Wilson to confer on the matter of abolishing coastwise trade. The President has been reluctant about sending any bill to Congress dealing with this quest,/ but it is suggested that he will send a letter to some influential member explaining the situation frankly/ and ask the repeal of the bill./

Reading, 25 sec. Writing, 2 min. 25 sec. Grade, 4.

Clipping No. 157

"Mr. Mitchell referred twice to the Police Lieutenant's dinner./ He spoke of Col. Goetals as the prospective head of the department,/ and said the Colonel most certainly would accept the office if he could have the power to remove from the department those men whom he considered dangerous to the city's welfare./

" 'I wish I had not been ill on the night of the Police Lieutenant's dinner,' he continued,/ 'because I wanted to go there to tell them just what I am going to tell you. To continue a man in the Police Department after he has been removed by the Commissioner is subversive of discipline/ and continues the "System," which has ruled the department as we know;/ the "System," that cohesive group which imposes itself on the department,/ compels honest men to stand by dishonest men,/ and so perverts sentiment that an Inspector under indictment can walk into a Police Lieutenant's dinner and be cheered for his refusal to answer questions put to him by the District Attorney./' "

Abstract No. 157

Mr. Mitchell referred twice to the Police Lieutenant's dinner./ He said Goetals would surely accept the position/ if he could have the power to remove those from the force he thought dangerous—/ I wish I had not been ill and could have been at the Police Lieutenant's dinner/ so that I could have told them just what I am going to tell you./

To continue a man in office after he has been dismissed by the Commission is subversive of all discipline/ and continues the system as we know./ It causes honest men to stand by dishonest men./ For a man to be cheered as he enters the dinner hall because he refuses to answer questions put to him by the district attorney is the condition of rule by a few known as the system./

Reading, 1 min. Writing, 4 min. 2 sec. Grade, 9.

WITH DISTRACTION

Clipping No. 34

"Our grandfathers did not give up $50 each for the privilege of spending New Year's Eve in crowded restaurants. Neither did they, at the stroke of twelve, put on caps of colored paper, ring cow bells, and throw confetti at strangers. Yet we, who ridicule their traditions and superstitions, can find, it seems, no more satisfactory way in which to speed the Old Year and welcome the New. Perhaps we might profitably follow their example."

Abstract No. 34

Our grandparents did not spend $50 each/ to spend New Year's Eve in a crowded restaurant./ Nor did they at the stroke of 12/ put on paper caps,/ ring cow bells,/ and throw confetti at strangers./ Yet we deride their superstitions./ We might well follow their customs in our New Year's observance./

Reading, 20 sec. Writing, 2 min. 50 sec. Grade, 8.

In summing up the data one envelope of eighteen clippings was treated as a unit. From the eighteen reading times, or writing times, or grades the median was taken and the average deviation was taken from the median. Under the circumstances there was necessarily considerable variability in the data, a situation that prevents the average from giving the middle measure as well as the median. However, either method would give the same conclusions. Table II. shows the collected results of the clipping tests. They appear in graphic form in Plate VII. The *C* lines which measure the subject's comprehension are unaffected by distraction in the case of Mr. A., Dr. P., and Mr. E. According to the graphic records Mr. A. and Mr. E. did not and Dr. P. did show inner speech in silent reading. Removing the distraction increases

TABLE II

FOR CLIPPING TESTS

Time = seconds. Figures in parentheses = order in which the envelopes were read.

	Without Distraction						With Distraction					
	Reading		Writing		Grade		Reading		Writing		Grade	
	Time	A. D.	Time	A. D.	Value	A. D.	Time	A. D.	Time	A. D.	Value	A. D.
Mr. A.	(1)[2]29	3.8	108	22	4	1.2	(2) 28	4.4	125	29.6	5	1.0
	(4) 29	5.5	115	23.5	4	1.0	(3) 27	3.8	131		4	1.0
Mr. B.	(3) 35	10.4	150	42.2	6.5	1.2	(1) 90	81.1	233	67.2	4	1.0
							(2) 80	21.1	240	49.3	5	1.2
Mr. D.	(3) 38	10.5	130	20.3	4	1.0	(1) 22	5.6	115	12.5	4	1.0
							(2) 40	9.1	140	20.4	5	1.0
Mr. E.	(1) 35	5.5	165	30.1	6.3	1.37	(3) 55	15.3	206	47.7	6	1.33
	(2) 33	8.0	178	32	6	1.0	(4) 40	8.5	150	26	6	1.0
Mr. M.	(3) 32	4.5	72	21	4	1.6	(1) 40	6.66	80	25	3	1.0
							(2) 30	5.3	60	35.6	2	1.33
Dr. P.	(1) 27	3.4	87	18	3.9	1	(2) 30	3.6	108	25.4	4	1
	(3) 29	3.5	120	20	4	1						
Mrs. R.	(3) 30	5.5	150	26.6	7	1.33	(1) 30	5.1	130	21.2	5	.82
							(2) 25	6.7	120	28.8	5	1.44
Mrs. W.	(1) 39	9.0	82	32	2	0.7	(2) 29	4.5	52	13	1	.5

comprehension 1½ points for Mr. B., ½ point for Mrs. R., and 2 points for Mr. M., although none of them showed any inner speech in the graphic records. It lowered comprehension 1 point for Mr. D., who showed inner speech in its most marked form. Putting on the distraction lowered comprehension for Mrs. W., but she showed no inner speech in the graphic record. Practice effect may account for part of the improvement made by Mrs. R., Mr. M. and Mr. B., but this is hardly an adequate explanation, because after removing the distraction, practice is much more rapid for Mr. B.; for Mrs. R. the curve rises from a level, and for Mr. M. it changes from a downward to an upward direction. These changes coming directly after removing the distraction can hardly be due to any other cause. In general the 'Jack and Jill' distraction does not decrease the efficiency of thought for one subject who does not use inner speech and for three subjects who use it. Out of four subjects who do not use inner speech the efficiency of thought is improved for three by removing the distraction and lowered for one by putting on the distraction. If the graphic records are to be relied upon for the existence of inner speech, and if inner speech has any function in the efficiency of thought, the results ought to be just the opposite. The question is then whether the speech receptor failed to record the existence of inner speech, or whether the 'Jack and Jill' distraction failed to eliminate inner speech, or whether it affected primarily the attention. The speech receptor might not have succeeded in all cases, but it certainly did in some, and in those the distraction did not affect comprehension. If the distraction did not entirely eliminate inner speech, it could hardly have failed to interfere with it. But those who use it most show no interference in comprehension. The probability is then that the distraction interfered in some cases with the attention. Since repeating aloud one sentence while reading another in print are both voluntary processes more or less conflicting, one could hardly avoid shifting the attention between the two. Under the condition a subject could not give his whole mind to the reading alone. Removing the distraction would enable him to do this, resulting in an improved comprehension. This seems to me the most probable explanation for the subjects who showed improved comprehension when the distraction was removed. But in any case the facts seem to show that in no case is inner speech indispensable to thought. In fact, I cannot see that it is of any use at all in the comprehension of meaning in reading.

Considering the differences between the last envelopes read with distraction and those read without distraction, we find that *removing* the distraction raises the median reading time 2 seconds for Mr. A., 5 seconds for Mrs. R., and 2 seconds for Mr. M. It lowers the median reading time 45 seconds for Mr. B., and 1 second for Dr. P. *Putting on* the distraction raises the median reading time 7 seconds for Mr. E., and lowers it 10 seconds for Mrs. W. We may say that the median reading time is unaffected, or at most only temporarily affected, by distraction for Mr. A., Dr. P., Mr. E., Mrs. R., Mr. M. and Mr. D. Of these four use inner speech and two do not. The only cases that call for explanation are those of Mr. B., Mr. E., and Mrs. W., neither of whom use inner speech. The drop of 45 seconds for Mr. B. is remarkable. If he used inner speech we might suppose that he repeated the words to be written during interruptions in the distraction, alternating between the two processes. His observation is that he found the distraction extremely difficult at first and possibly used inner speech in a few cases during interruptions, but he soon overcame this difficulty so that it did not bother him at all. These remarks together with his graphic records make it improbable that the drop in the reading time can be explained by the presence of inner speech. It seems very probable that the distraction badly disturbed his attention, making it necessary for him to reread many lines in order to get the thought. After the distraction was removed this rereading became unnecessary and therefore reduced his time. The same explanation would apply to Mr. E.'s case, but it would fail in Mrs. W.'s case for whom *putting on* distraction reduced the reading time ten seconds. For this case I have no explanation unless the distraction caused her to put forth much greater effort or else read rapidly and carelessly. Her poor comprehension during distraction favors the latter hypothesis. But in all cases the variations in the reading time are much more easily explained by effects on the attention than by interference with inner speech. That the latter does not account for anything here is proven by the fact that the median times of the four subjects who use inner speech are unaffected by the 'Jack and Jill' distraction, whereas the reading times of three out of four who do not use it, are affected by the distraction.

It may also be noticed that with the exception of two cases, those of Mr. B. and Mr. E., the curves for the reading times and grades run closely parallel, that is, they rise and fall in the same general direction. This means that the faster the subjects read, the poorer they comprehended; and the slower they read, the better they comprehended. Such a correlation is contrary to what most other investigators have found upon this relationship.

The 'Jack and Jill' distraction has its greatest effect upon writing time. *Removing* the distraction *lowers* the median writing time 16 seconds for Mr. A., 90 seconds for Mr. B., and 10 seconds for Mr. D., *raises* it 13 seconds for Dr. P., 30 seconds for Mrs. R., and 12 seconds for Mr. M. *Putting on* the distraction *lowers* the median writing time 15 seconds for Mr. E. and 30 seconds for Mrs. W. That is to say, *removing* the distraction *lowers* the writing time for two who use inner speech in writing and for one who does not use it, and *raises* the writing time for three who use it. Putting on the distraction *lowers* the writing time for two who do not use inner speech in writing. In other words, the distraction *raises* the median writing time for two who use inner speech in writing and for one who does not, and *lowers* the median writing time for three who use it and for two who do not. This statement is derived by considering the removal of the distraction as having opposite effects if those subjects who first read and wrote with distraction and then without, would have read and written in the reverse order. Inner speech is present and not present in cases where distraction has the same effect, or it is present in cases where distraction has opposite effects. If inner speech were important in the writing function for those who use it, distraction ought to raise their writing time very much, and have little effect upon those who do not use it. But according to the above facts, out of five subjects who use inner speech in writing, the 'Jack and Jill' distraction raises the median writing time for two and lowers it for three. This situation compels us to find some other explanation of the variation in the writing time. In the case of Mrs. R., Mr. M., Mr. D., and Mrs. W. the writing time and grade curves rise and fall together. This means that when the subjects remembered much, their writing time was high; and when they remembered little, their writing time was low. This is an intelligible relationship, for evidently it does not require so much time to write two lines as it does ten lines. The grade, however, does not explain the variation in the writing time for the other four subjects. In the case of Mr. E. and Dr. P., the *W* lines vary greatly while the *G* lines remain constant. For Mr. B. the *R* and *G* lines run in opposite directions. For Mr. A. they run both parallel and opposite. In the case of Mr. E. the first week of distraction raised the writing time 28 seconds, the second week lowered it 56 seconds. This suggests that the distraction temporarily disturbed his attention. His mind alternated between recalling the subject matter for writing and an effort to keep the distraction process going properly. During the second week the latter took care of itself, enabling him to give his whole mind to the recalling and writing. The same explanation would apply to the cases of Mr. A. and Mr. B. For Dr. P.'s case, where removing the distraction raised the writing time without raising the grade, I have no explanation. In general, however, I think the variation in the writing time must be sought in the variation of the attention and grade.

The general conclusion which the tests in connection with the newspaper clippings point to is that inner speech has no important function in comprehension in reading, and in writing, or in the rate of reading and writing.

The reading and writing tests were followed by a series of tests in addition. The series was made up of six sheets, each sheet containing sixteen single columns of twenty digits each, no columns having ones or zeros. The first and sixth sheets were done without distractions. The remainder were done with distraction, the same distraction as in the reading and writing tests. Only four subjects completed these tests, the results of which are shown in the curves of Plate VIII. The curves marked *S* represent the sum of errors for each sheet, those marked *C* represent the number of columns in each sheet added wrongly. The *S* and *C* curves therefore measure the same fact.

It will probably facilitate interpretation to make a table giving for each subject the average sum of errors made per sheet with distraction and without distraction; also the average number of columns per sheet added wrongly with distraction and without distraction. These are given in Table III.

TABLE III

	Without Distraction		With Distraction	
	S'	C'	S'	C'
Mr. D.	19	3	11.25	7
Mr. M.	74.5	12.5	47.50	12.25
Dr P.	1	1	10.50	2
Mrs. R.	17.25	5	41.75	5.75

S' = Average sum of errors per sheet.
C' = Average number of columns added wrongly per sheet.

We notice that two of the subjects add on the average more columns wrongly with distraction than without distraction. Distraction has no effect upon the number of columns added wrongly for the other two subjects. For two subjects the distraction increases the average sum of errors per sheet, and for the other two subjects it has the opposite effect. All of these subjects ordinarily use inner speech in addition. If inner speech is important for the addition process, the 'Jack and Jill' distraction ought to show its effects. It ought to increase the quantity of errors as well as the adding time for all the subjects. But this is not the case with either the errors or the time. If the series of tests with distraction had been prolonged, we undoubtedly would have gotten the ordinary practice curve in spite of the distraction, which would probably show itself in slower improvement and greater variability in the early stages than a series without distraction. Such results are even shown in this series in case of Mrs. R., whose curves are typical practice curves. In such a case we should probably interpret the variability in the curves as a function of the attention.

The writer wishes to express his sincere gratitude to Professors J. McK. Cattell and R. S. Woodworth, in whose laboratory this experiment was done, and also to each of his subjects, namely: Drs. Poffenberger, Strong, and Evans; Messrs. Anderson, Brimhall, De Bra, McCall, Martin, and May; and Mrs. Williamson and Mrs. Reed, all of whom assisted him at a great sacrifice of their time.

(H. B. Reed.)

PLATE I.

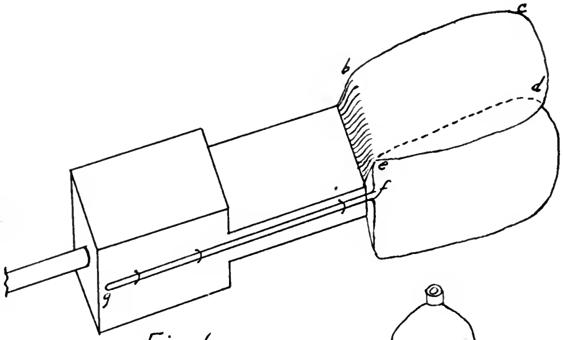


Fig. 1.
Speech Receptor.

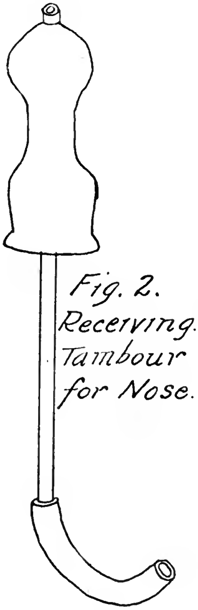
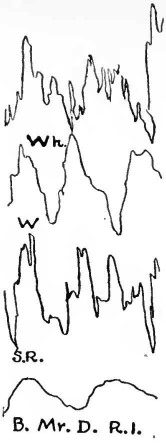
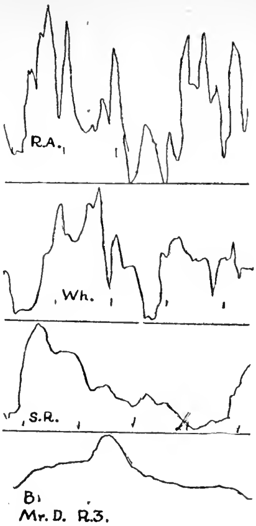
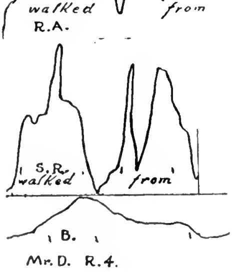


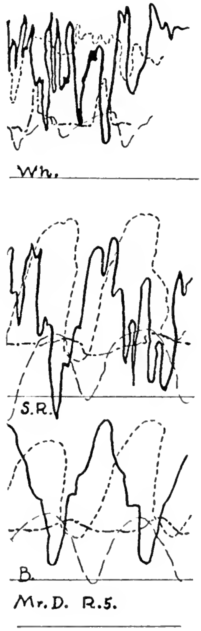
Fig. 2.
Receiving
Tambour
for Nose.

Fig. 1. Speech receptor.
Fig. 2. Receiving tambour for nose.





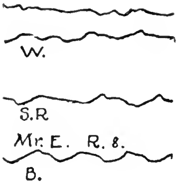


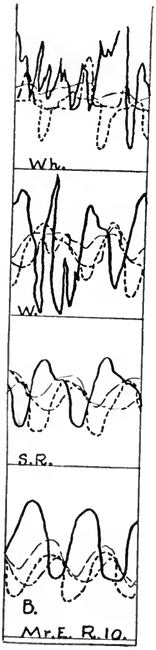


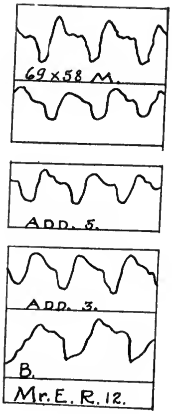
R. 1, *R.* 3, and *R.* 4, are tongue curves. In *R.* 5 the heavy continuous line represents the curve from the speech receptor. The line of short dashes represents the curve from the tambour in the nose. The line of long dashes represents the curve from the pneumograph around the chest. The line of dots and dashes represents the curve from pneumograph around the abdomen. The same is true of similar cuts.

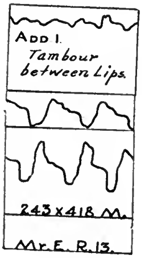
392²

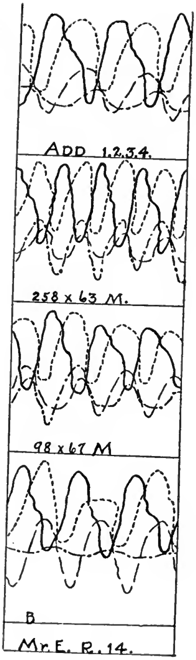
PLATE 3.

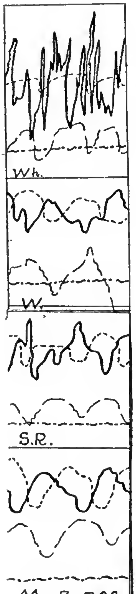

W.
S.R
Mr. E. R. 8.
B.


Wh.
W.
S.R.
B.
Mr. E. R. 10.

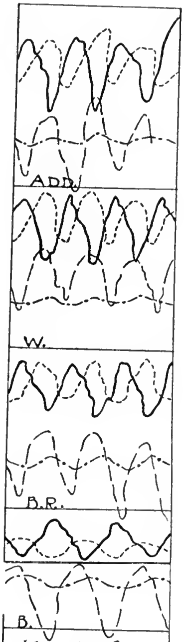

69 x 58 M.
Add. 5.
Add. 3.
B.
Mr. E. R. 12.


Add 1.
Tambour between Lips.
243 x 418 M.
Mr. E. R. 13.

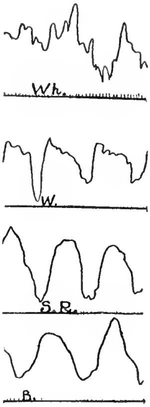

Add 1.2.3.4.
258 x 63 M.
98 x 67 M
B
Mr. E. R. 14.



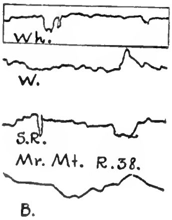
Mr. B. R22.



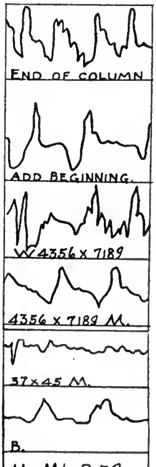
Mc. R.29.



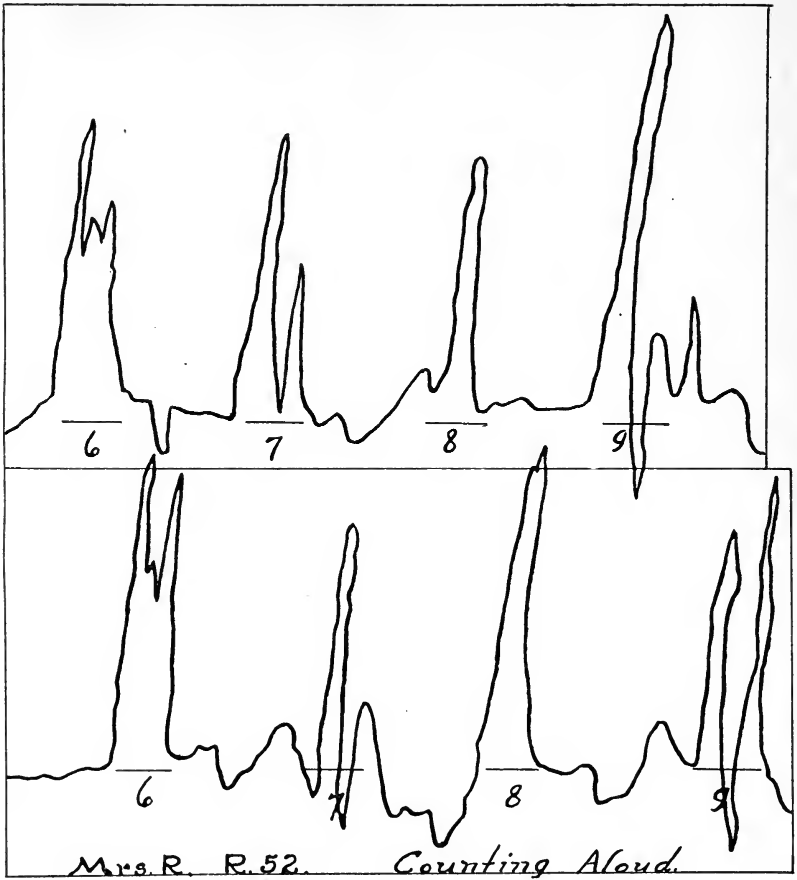
Mr. M. R.33.

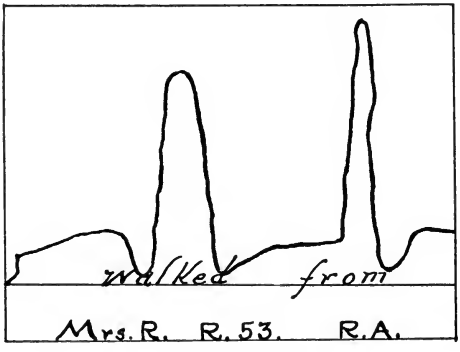


Mr. Mt. R.38.



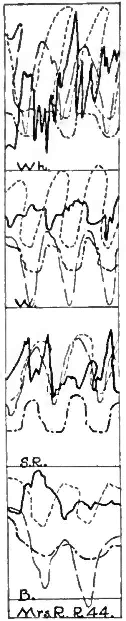
Mr. Mt. R.39.

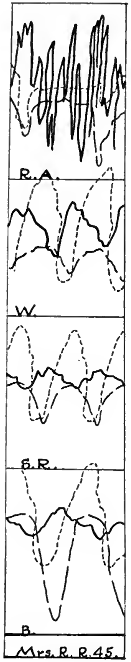

6
7
8
9
6
7
8
9
Mrs. R. R. 52. Counting Aloud.

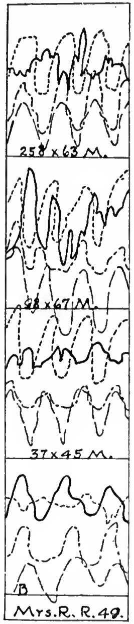

walked from
Mrs. R. R. 53. R.A.

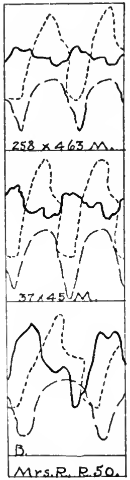
(H. B. Reed.)

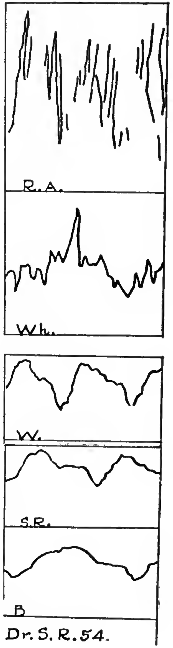
PLATE 6.











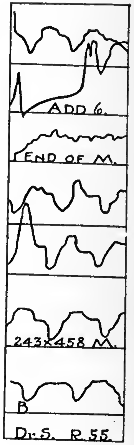


PLATE 7.

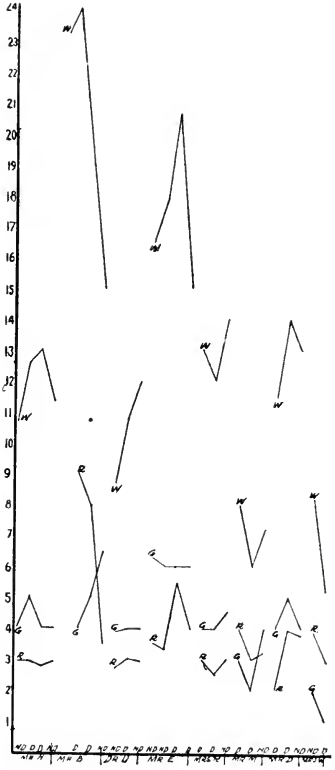


Fig. 4. Clipping tests: *W* = writing T per 18 clippings. *R* = reading T per 18 clippings. *G* = grade. *D* = with distraction. *ND* = no distraction.

1 cm. along vertical = 24 secs. for T lines
= 2.4 " for R and G lines

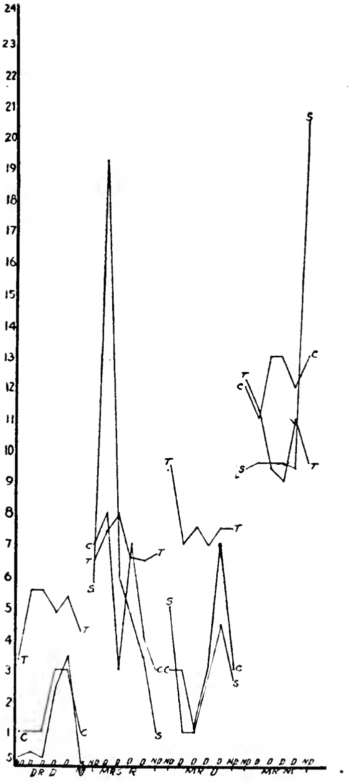


Fig. 5. Addition tests. *S* = sum of errors = 5. *T* = time per 16 columns. *C* = number of columns wrong. *D* = with distraction. *ND* = no distraction.

1 cm. along vertical = 2.4 mins. for T lines
= 12 " " S "
= 2.4 " " C "

THE MNEMONIC SPAN FOR VISUAL AND AUDITORY DIGITS[1]

BY ARTHUR I. GATES

A small amount of experience in testing immediate memory for digits led the writer to the belief that the quantity of material presented was an influential factor in determining an individual's capacity. Accordingly the following experiment, which aims to find the exact span of a number of individuals and to discover the effects of increasing the presented series beyond the span, was planned and carried out. The investigation has yielded some information with regard to the adequacy of single long series of items as a test for memory,[2] as well as some interesting results with regard to individual differences.

About 165 college students completed the tests, working in groups of from eight to fourteen persons, each group at a different hour, the earliest beginning at 8:10 a.m. and the latest at 5:10 p. m. The program covered two full days.

For auditory tests, which were always given first, the following series of digits were used: 9627, 41852, 736294, 8513627, 38471629, 529468371, 2574638197, 83519472631, 628194357283. By means of a silent pendulum, invisible to the subjects, the digits were spoken to them at the rate of one to each three fourths of a second, clearly and sharply, and without rhythm.

Similar series were prepared for visual tests. Black figures two and three fourths inches in height were mounted on white cards (two and a half by three and a half inches) and these cards were pasted on a narrow strip of gray cloth. The series were: 6283, 57294, 241738, 2170463, 27985543, 215903847, 5978024318, 57402623871, 183570467392. The strip could be folded in the hands, held before the subjects

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[1] From the Psychological Laboratory of the University of California No. XXIV.

[2] For a summary of methods of testing memory see A. Pohlmann, *Experimentelle Beiträge zur Lehre vom Gedächtniss*, Berlin, 1906.

and quickly displayed by drawing it taut with a snap. The total time of exposure in this case was the same as that for the auditory series, *i. e.*, three fourths of a second was allowed for each digit.

In both cases the subjects were called to attention just before each test was made. At the word "ready" the test was started. The instructions, to write down the numbers immediately after the exposure but to write nothing until the series had been taken from sight, were carefully followed. Ample time was allowed between tests for the subjects to write down the digits remembered and to prepare for the next test.

The results were scored by counting the number of digits which were correct in form and position. A digit was considered as being correctly placed if its position was correct when measured either from the left or the right of the series.

By the 'span' of memory is meant, in this paper, the longest series of digits which, after exposure, an individual can accurately reproduce entire. If a series of digits is exposed, of which the individual can reproduce only a part, the number reproduced is always spoken of as the 'number reproduced' or 'recalled,' etc. The terms 'exposure series' or 'presentation series' refer simply to the group of digits read or shown.

Difficulties were sometimes encountered in deciding what series should be taken as the span. For example, an individual may reproduce a series of seven digits, fail at eight, succeed at nine and fail thereafter. Another may reproduce seven, fail at eight and at nine, and succeed at ten. When there is but one failure immediately followed by a success, the latter is taken as the span. Thus in the first example the span would be nine. In case more than one failure followed a success, the number before the failures would be considered the span; thus in the second example seven digits would stand as the span.

THE AVERAGE SPAN OF VISUAL AND AUDITORY DIGITS

With the auditory method of presentation the average span was 7.666 digits. The same individuals reproduced on the average, 8.172 digits when these digits were presented visually. The mode is 8 digits with either method; a relatively greater number falling upon it in the case of the visual method. The tendency in the direction of a larger span in the case of visual presentation is shown by the table of distribution (Table I.). A greater number (14 more) surpass the modal span and a smaller number (19 less) fall below the mode.[1]

In the visual method, since mode and average nearly coincide and since the average span is above eight digits, the norm for adults of good training could with reasonable security be placed at eight digits. We can, more exactly, say that the normal individual should reproduce a series of at least eight digits when presented visually at the rate of one for each three fourths of a second.

The selection of the normal span in the case of auditory digits is less easy. The modal span is eight, but the average falls below that by one third of a digit. The number of individuals unable to span eight digits is greater by 7.2 per cent. than the number which exceeds the modal span. Although to set the normal span at eight digits would not be strictly correct, for practical purposes there is no choice other than eight digits for the norm.

Table I. shows that when the number of digits exposed surpasses the span, the distribution of individuals become mor extended. A greater number recall but very few (0, 1, 2, 3, or 4) digits and nearly as many recall very many (10, 11, or 12) digits. The curve of distribution in the cases where the series exposed is longer than the span, compared with the curve in the case where the series exposed equals the span, appears skewed toward the lower end.

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[1] The opportunity afforded the subjects to read the very short series of digits more than once in the visual series may explain, partly at least, the fact that a smaller number of individuals have short spans. Miss Ada Felt, who coöperated in the tests, gave her attention to noting the methods employed. She found that many subjects, unless warned, wrote down the short series before the exposure was complete. Since this practice was not permitted, some glanced aside and repeated the numbers to themselves and others reread the series one or more times. As the series became longer, all the subjects devoted their whole time to reading the series, generally completing them but once although the time per digit was the same as in the short series.

THE INFLUENCE OF PRESENTING A SERIES OF DIGITS WHICH EXCEEDS THE SPAN

If eight digits is the largest number that a particular individual can retain and reproduce correctly, what will be the result when nine, ten, or more digits are presented?

Table I. shows that an increase in the number of digits exposed has an unfavorable influence on immediate memory. In no group is the average number of digits remembered equal to the average span. In every case except one the difference amounts to more than two digits. In the single case which forms the exception, the difference is nearly two, and in other cases it is nearly three digits. The average reduction amounts to 25.9 per cent. with the visual method, and to 36.6 per cent. with the auditory method.

Table I., Section (5), shows that the facts found in the

TABLE I

SHOWING THE NUMBER OF INDIVIDUALS REPRODUCING VARIOUS NUMBERS OF DIGITS WHEN THE EXPOSURE SERIES WAS THE EXACT LENGTH OF THE SPAN, ONE DIGIT LONGER THAN THE SPAN, ETC.

Visual Series

No. Digits	Span, No. Inds.	Span + 1, No. Inds.	Span + 2, No. Inds.	Span + 3, No. Inds.	Span + 4, No. Inds.
12	3	0	0	0	1
11	4	0	1	1	1
10	13	1	6	2	2
9	35	8	7	8	6
8	63	14	20	15	15
7	29	37	27	32	17
6	15	44	42	34	22
5	2	32	20	21	11
4	0	13	14	14	11
3	0	5	5	9	7
2	0	5	10	4	4
1	0	1	2	1	5
0	0	0	2	2	5
Total No. inds.[1]	163	160	156	143	107

	At Span	At Span + 1	At Span + 2	At Span + 3	At Span + 4	
1. Av. span of inds.	8.172	8.10	8.03	7.84	7.54	
2. Av. No. digits recalled		5.98	5.94	5.90	5.51	
3. Diff. between span and av. No. recalled		2.12	2.09	1.94	2.03	
4. Diff. in per cent.		26.00	26.00	24.80	27.00	Av. 25.9
5. Mode	8	6	6	6	6	

Auditory Series

No. Digits	Span, No. Inds.	Span + 1, No. Inds.	Span + 2, No. Inds.	Span + 3, No. Inds.	Span + 4, No. Inds.
12	2	0	0	2	0
11	1	0	0	2	2
10	9	1	1	1	1
9	29	2	2	5	0
8	56	17	14	9	5
7	30	20	25	10	9
6	23	33	30	20	17
5	12	38	26	23	18
4	0	25	21	24	20
3	0	14	17	20	17
2	0	5	14	16	14
1	0	4	3	10	7
0	0	1	6	8	10
Total No. inds.[1]	162	160	159	150	121

	At Span	At Span + 1	At Span + 2	At Span + 3	At Span + 4	
1. Av. span of inds.	7.666	7.61	7.59	7.44	7.07	
2. Av. No. digits recalled		5.30	4.96	4.50	4.09	
3. Diff. between span and av. No. digits recalled		2.31	2.63	2.94	2.98	
4. Diff. in per cent.		30.40	33.30	39.50	42.10	Av. 36.6
5. Mode	8	5	5	4	4	

[1] When the series is increased beyond the span, certain individuals are eliminated from the group. Those whose span was 12 digits are eliminated from the group "Span + 1" because a series of 12 digits was the longest given. When we reach "Span + 2," those whose span was 11 and 12 digits are eliminated, and so on. The average span of the group becomes smaller, consequently, as the number of individuals decreases.

general averages are characteristic of the group showing the central tendency. In the case of visual memory, to increase the number of digits exposed beyond the span means to decrease the modal figure by 25.0 per cent. which is identical with the reduction in the average figures. The decrease is greater for auditory digits, a distinction which was also found in the average results.

Table II. gives the directions of change, without regard to the amount, as 'up,' 'down,' or 'the same.' 'The tabulations were obtained by noting the number of individuals who remembered more ('up'), less ('down'), or 'the same' number of digits when the length of the exposure series is increased.

The figures show that the distractions offered by too long a series either are not effective for all individuals or are not effective for some individuals at all times. On the average, however, 77.1 per cent. of the individuals, when the visual method is used, and 84.5 per cent. with the auditory method, fall below their span when the series exposed is longer than the span. That is to say, only one individual out of four, when the visual method of presentation is used, or one out of six with the auditory method, is able to equal or surpass his span. But one out of ten, with the visual method, or one out of eighteen with the auditory method, is able to surpass his span.

THE INFLUENCE OF THE AMOUNT BY WHICH THE NUMBER EXPOSED EXCEEDS THE SPAN

Table I., Sections (1), (2), (3), (4), gives the average decrease in the number of digits recalled as the series becomes

TABLE II

SHOWING THE ABSOLUTE AND RELATIVE NUMBER OF INDIVIDUALS WHO REMEMBER MORE, LESS, OR THE SAME NUMBER OF DIGITS AS THE EXPOSURE SERIES IS INCREASED

Visual Series

		Span + 1		Span + 2		Span + 3		Span + 4		Average
		No.	Per Cent.	No.	Per Cent.	No.	Per Cent.	No.	Per Cent.	Per Cent.
	"Up"	0	0.0	19	12.2	19	13.3	15	14.0	9.9
	"Same"	7	4.5	23	14.7	23	16.9	18	16.8	13.0
	"Down"	153	95.5	114	73.1	101	70.8	74	69.2	77.1
1. From Span to	Total	160	100.0	156	100.0	143	100.0	107	100.0	100.0
	"Up"	...	.....	67	43.0	59	41.2	50	46.7	43.6
	"Same"	...	.....	27	17.0	28	19.6	16	15.0	17.3
	"Down"	...	.....	62	40.0	56	39.2	41	38.3	39.1
2. From Span + 1 to	Total	...	.....	156	100.0	143	100.0	107	100.0	100.0
	"Up"	...	.....	...	.....	63	44.0	54	50.3	47.15
	"Same"	...	.....	...	.....	17	12.0	11	10.5	11.25
	"Down"	...	.....	...	.....	63	44.0	42	39.2	41.6
3. From Span + 2 to	Total	...	.....	...	.....	143	100.0	107	100.0	100.0
	"Up"	...	.....	...	.....	...	.....	47	44.0	
	"Same"	...	.....	...	.....	...	.....	19	17.7	
	"Down"	...	.....	...	.....	...	.....	41	38.3	
4. From Span + 3 to	Total	...	.....	...	.....	...	.....	107	100.0	

Auditory Series

		Span + 1		Span + 2		Span + 3		Span + 4		Average
		No.	Per Cent.	No.	Per Cent.	No.	Per Cent.	No.	Per Cent.	Per Cent.
	"Up"....	0	0.0	13	8.1	15	10.0	6	5.0	5.8
	"Same" .	8	5.0	27	16.9	11	7.0	12	10.0	9.7
	"Down".	154	95.0	120	75.0	124	83.0	103	85.0	84.5
1. From Span to........	Total....	162	100.0	160	100.0	150	100.0	121	100.0	100.0
	"Up"....	...	.....	59	37.0	52	34.6	31	25.9	32.5
	"Same" .	...	.....	33	20.6	17	11.3	11	9.1	13.7
	"Down".	...	.....	68	42.5	81	54.0	79	65.0	53.8
2. From Span + 1 to....	Total....	...	.....	160	100.0	150	99.9	121	100.0	100.0
	"Up"....	...	.....	...	.....	63	42.0	48	39.6	40.8
	"Same" .	...	.....	...	.....	20	13.0	17	14.0	13.5
	"Down".	...	.....	...	.....	67	45.0	56	46.4	45.7
3. From Span + 2 to....	Total....	...	.....	...	.....	150	100.0	121	100.0	100.0
	"Up"....	...	.....	...	.....	...	.....	50	41.3	
	"Same" .	...	.....	...	.....	...	.....	17	14.2	
	"Down".	...	.....	...	.....	...	.....	54	44.5	
4. From Span + 3 to....	Total....	...	.....	...	.....	...	.....	121	100.0	

longer. When the visual method of presentation is used, the amount by which the exposure series exceeds the span seems to have little effect, for only one series (Span + 4) shows a smaller number of digits recalled than at Span + 1. When the auditory method is employed the case is different. As the exposure series becomes longer, the difference between the span and the number recalled becomes greater.

The modal figures for the different visual series are identical, but in the auditory series, the modes for Span + 3 and Span + 4 are the smallest, indicating an unfavorable effect of very long exposure series.

Table II., Section (1), shows, that using the visual method of display, the number of individuals who are unable to equal their span becomes less as the exposure series becomes longer. But in the auditory series, the opposite is true. A striking feature in both cases is the relatively large number of subjects who go 'down' when the exposure series just exceeds the span (Span + 1) although the average number of digits recalled is as great in the auditory series and nearly as great in the visual series.

The effects of increasing the length of the exposure series can be seen from different points of view in Table II. by referring to Sections (2), (3), and (4) in which Span + 1, Span + 2, and Span + 3 are used as a basis of comparison. With the visual method, it is noticeable throughout that as the series becomes longer the number of individuals going 'down' becomes less, while in the auditory series the opposite is the case. The regularity with which these changes take place can be seen by glancing down the vertical columns or by scanning the table diagonally from left to right. In the auditory series the relations are quite regular, showing that the difficulty of recalling digits increases with fair uniformity as the exposure series are lengthened by uniform additions.

The results seem to justify the conclusion that as the series of digits given orally exceed the span by uniformly increasing amounts, conditions become more and more unfavorable for immediate memory. This rule, however, does not hold for the visual method of presentation. Perhaps a partial explanation of this difference lies in the fact that many individuals purposely neglected certain digits in the very long visual series and held to a number of digits at the beginning or end of the series to make up a number equal to that which they believed to be their limit. This could be more easily accomplished with the visual method since the series were exposed entire and because such a distracting influence as the reading of other digits was not present. Moreover since the visual series were given after the auditory series, many individuals, profiting by practise, were able to adjust themselves to the situation more effectively.

Types of Individuals

As soon as the span is passed most individuals are unable again to recall as many digits as at that point. Yet in a small number of cases, individuals succeed in equalling or surpassing their span.

To determine whether such results were caused by comparatively distinct types of individuals or whether they repre-

TABLE III

SHOWING THE PERCENTAGE OF INDIVIDUALS WHO EQUAL OR SURPASS THE SPAN AND THE NUMBER OF TIMES THIS IS DONE IN THE SERIES
SPAN + 1, + 2, + 3 AND + 4

Number of times (Visual Series)	Once	Twice	Thrice	Four Times	Once	Twice	Thrice	Four Times	Equal Once and Surpass Once	Equal Once and Surpass Twice	Equal Twice and Surpass Once	Equal Twice and Surpass Twice
Per cent. of inds......	17.80	12.30	0.00	0.00	8.40	1.87	0.93	0.00	8.40	2.80	2.80	0.93
			0.00				0.93			6.53		
							7.46					
(Auditory Series) Per cent. of inds......	18.30	8.26	0.82	1.65	9.15	2.50	0.00	0.00	5.00	2.50	3.30	0.00
			2.47				0.00			5.80		
							8.27					

sent merely exceptional performances by normal individuals, Table III. was compiled, showing the number of times that the subjects equalled or surpassed their span.[1]

Table III. shows that a comparatively large number of individuals either equal or exceed their span but once. Such performances can scarcely be considered those of a special type of individuals distinct from those who do not equal their span. There is, however, a smaller group who succeed in equalling or surpassing their span more frequently. If an individual accomplishes this feat three or four times out of four possible times, the performances seem to lie within the normal power of the individual. Consequently, this group, numbering about eight out of one hundred individuals, will be considered as forming a type comparatively distinct from the larger group whose members never or very seldom equal their span.[2]

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[1] This tabulation is based on the results of those individuals whose span was eight digits or less. The series Span + 1, + 2, + 3, + 4, only were used.

[2] Of course, there are intermediate stages between the two groups. Thorndike's objection to calling the extremes of such a distribution "types" is worth considering. See his *Educational Psychology*, Vol. III., Chap. 16. The term has been used here for want of a better one.

CORRELATION OF THE TESTS

For measuring the correlation of the two forms of tests, the following familiar formula was used:[3]

$$r = 1 - \frac{6\Sigma D^2}{n(n^2-1)}.$$

When the span is taken as the basis for measurement, a correlation of + 0.625 is found. This correlation is high enough to be significant.

Correlations were also computed for memorial efficiency in the two forms of test when the series of digits exposed were longer than the span. A sum was made of the total number of digits correctly recalled from the three longest exposure series (the series of 10, 11, and 12 digits) for those individuals whose spans were nine or less digits. The correlation was computed on the basis of these figures and a coefficient of + 0.225 was found. This correlation is small compared to that found when the computations are based on the span. Differences in method of presentation of the material, and perhaps differences in the form that practise-effects may take, account, in part, for the low correlation.

SUMMARY

1. The average span for college students is approximately 8.2 digits when the visual method of presentation is employed, and 7.7 digits with the auditory method.

2. An increase in the number of digits exposed beyond the number constituting the span results in a more extended distribution of individuals.

3. An increase in the number of digits exposed beyond the span decreases the number of digits that can be recalled.

(*a*) Under these conditions, the number of digits recalled is 25.9 per cent. less than the span when the visual method is used, and 36.6 per cent. less when the auditory method is employed.

(*b*) Consequently, in group-tests, if a single series of 10, 11, or 12 digits is used, we should add to the number recalled one fourth when the visual method is employed or one third

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[3] See Thorndike, E. L., 'Mental and Social Measurements,' 1913, p. 162.

with the auditory method, to obtain the average span of the group.

4. Two types of individuals appear when the exposure series is longer than the span.

(*a*) Most individuals are never or very seldom able to equal or surpass their span.

(*b*) A few individuals (about 8 per cent.) always or nearly always equal or surpass their span.

5. A correlation of + 0.625 is found for the individuals in the two tests when the span is taken as a basis of comparison; + 0.225 when the exposure series surpasses the span.

THE CORRELATIONS BETWEEN DIFFERENT MEMORIES[1]

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The purpose of the present investigation is to determine the correlations between immediate and mediate retention for auditory and visual presentation with various sorts of material.

Experiments and Results

The belief that retentiveness is a function dependent in large measure upon the plasticity of the nervous system, the fact that rather high correlations of abilities are shown by a study of results obtained in school subjects, and the common observation that memory efficiency among normal individduals is not highly specialized, led to the belief that we should find in the following study evidence indicating high correlations in memorization.

The materials selected for the tests include nonsense syllables of three letters; numbers of two digits; concrete nouns of four letters; descriptions of approximately one hundred and thirty-five words; narratives of approximately one hundred and fifty-five words; expositions of one hundred words for mediate retention; and expositions of two hundred words for immediate retention; memory for faces was tested by means of photos cut from college annuals. Except in the last mentioned tests visual and auditory presentations were given, reversing the alternatives in successive tests.

The observers selected to be tested include two professors, four graduate students and three seniors of the University of Wisconsin. Two of the graduate students and two of the seniors had had no previous experience in laboratory work.

In testing mediate and immediate retention for discon-

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[1] Grateful acknowledgment is due to Prof. V. A. C. Henmon, of the University of Wisconsin, for guidance and many hours of coöperation in this piece of work.

nected or non-significant materials, each series was placed on the Müller Kymograph drum 50 cm. in circumference, and revolving at the rate of fifteen seconds; the aperture of the screen permitted the observer, in the visual presentation, to see each unit of the respective series for ¾ seconds. In the mediate material the number of repetitions was registered automatically by the kymograph.

The tests were administered in hour periods, an hour usually sufficing for the mediate learning of one series of each type of disconnected material visually and a corresponding one auditorily; in the immediate material four successive series of each type were presented visually, and four auditorily. The variability in rapidity of learning connected material caused a little more irregularity in the amount of work accomplished per hour. The total time required for each observer varied from fourteen to eighteen hours. The order of presenting the materials as well as the order of the methods of presentation varied from day to day so that any effects of fatigue might thus be counterbalanced.

Results

Disconnected Material for Immediate Retention

The successive or memory-span method was used to determine retention with three series of 4, 5, 6, and 7 syllables; 4, 5, 6 and 7 numbers; and 6, 7, 8, and 9 nouns, for each method of presentation. The scores were expressed in per cent. as calculated from the relation of the number of units retained to the number of units presented. The per cent. rather than a single figure was used as the measure of retention because of the variations of each subject in his performances; not only did these vary from day to day, but the same set of series often showed peculiar fluctuations, so that it seemed quite impossible to indicate the memory-span accurately by a single figure.

In deducting for errors, omissions and substitutions each counted one unit; transpositions counted one half unit; errors of one letter in the nouns and syllables, one half unit;

errors of one digit in the numbers, one half unit; errors of two letters in nouns or syllables were rates as substitutions.

Connected Material for Immediate Retention

These selections included one description, one narration, and one exposition for each method of presentation. For the visual tests the observer was directed to read the selection silently, and rather slowly, that the results might be more comparable to the corresponding selections presented auditorily. Immediately after finishing the reading the subject wrote the substance of the selection.

In the auditory tests the experimenter read the selection slowly, and the observer immediately reproduced the substance in writing.

The variation in the rates of reading for the different subjects led to the adoption of the number of words retained per second of reading-time, as the basis for determining ranks. In checking the materials, all errors in the reproduction were crossed out, and credit was then given for the number of words correct, calculating the per cent. on the basis of the number of words in the original. The advisability of scoring by the number of ideas recalled was considered, but a careful study of several reproductions showed that for the immediate retention of shorter selections, at least, the number of words reproduced is probably as accurate an estimate of an observer's performance as is the number of ideas.

Disconnected Material for Mediate Retention

This material, as that for immediate memory, was presented on the kymograph. The subjects were ranked for each method of presentation according to the average number of repetitions required by each to learn four series of ten units of the different types of material. In allowing for errors, one repetition was added for each substitution, omission or addition; one half repetition was added for each transposition; and one half repetition for each error of one letter in the nouns and syllables, and of one digit in the numbers.

Connected Material for Mediate Retention

These selections included, as for the immediate memory tests, one description, one narration and one exposition for each method of presentation. In the visual tests the observers read and re-read the selections silently and by the whole method until they were memorized for a written reproduction; the time required was noted by the experimenter while the number of repetitions was recorded by the observer. In the auditory presentation the selection was repeatedly read by the experimenter until the observer signified that he had memorized the production. The scoring of this material presented great difficulty, and the results are not very satisfactory for comparative purposes. Despite the earnest cooperation of the subjects, and the careful instructions given them to learn the material for a written verbal reproduction, underlearning was the result in many cases. The number of errors made as well as the learning-time required by the different subjects were so variable as to make it manifestly impossible to use either time required, or errors made, alone, as a basis for ranking; a penalization for errors was clearly necessary. Equally difficult material was selected, and five of the subjects repeated part or all of the tests; a comparison of the first and second performance showed that an addition of ten seconds of time for each error would be sufficient on the average to account for the improvement shown, and this amount was therefore added to determine the ranks.

Memory for Faces

Three series of photos, two of women, and one of men, each containing approximately fifty faces, were selected as material; ten of the first series were exposed to the observer for thirty seconds and then mixed with the entire list; after an interval of five minutes the observer was directed to identify the ten faces previously exposed. In the second and third series the procedure was the same, except that the intervals were respectively ten and fifteen minutes.

In scoring, the number of faces correctly identified as compared with the number exposed gave the per cent. from which the ranks were determined.

RANKS

From the gross results discussed above, the ranks of the individuals in the various tests were determined. They appear in Table I. The ranks in immediate memory for nonsense syllables, numbers, and nouns are based on the per cent. retained. The ranks in immediate memory for prose are based on the number of words retained per second of reading time for the three types of prose combined. For reasons stated above, it was found to be impossible to obtain a fair ranking on a basis of the amount retained without reference to the time of reading. The rank for memory for faces is based on the scores for the three intervals combined. The ranks are determined directly from the results referred to above. The ranks for mediate memory for prose are based on the time for learning for the three prose selections plus ten seconds for each error.

TABLE I

RANKS

	Immediate Memory									Mediate Memory							
	Visual					Auditory				Visual				Auditory			
	Non-syl.	Numbers	Nouns	Prose	Faces	Syllable	Numbers	Nouns	Prose	Syllable	Numbers	Nouns	Prose	Syllable	Numbers	Nouns	Prose
A	6	3½	3	2	4	3½	5½	2	7	2	7	2	6	1	6½	2	3
B	1½	5	7	7	7	3½	4	7	4½	3	3½	1	5	3	3	4	5
C	5	9	5	3	1	7	9	6	1	5	2	4	2	6	2	5½	1
D	4	1½	1	1	2	1	1	1	6	1	3½	3	1	2	6½	1	6
E	8	8	4	6	4	5	7½	3	4½	6	8	7½	4	9	8	3	2
F	3	6	8	5	9	8	7½	9	2	9	9	8	7	8	9	9	7
G	7	1½	6	8	8	6	5½	4½	8	4	5½	6	9	4	5	5½	9
H	9	7	9	4	6	9	2½	8	3	7	1	5	3	5	1	7	4
I	1½	3½	2	9	4	2	2½	4½	9	8	5½	7½	8	7	4	8	8

CORRELATIONS

The coefficients of correlation, calculated by the formula

$$r = 1 - \frac{6\Sigma D^2}{n(n^2 - 1)}$$

from the above table of ranks, appear in Table II.

TABLE II

CORRELATIONS

		Immediate									Mediate								
		Visual					Auditory				Visual					Auditory			
		Syllable	Numbers	Nouns	Prose	Faces	Syllable	Numbers	Nouns	Prose	Syllable	Numbers	Nouns	Prose	Faces	Syllable	Numbers	Nouns	Prose
Immediate Visual	Syllables		.28	.25	−.24	−.01	.51	.27	−.01	−.13	−.01	−.14	.20	−.20	.06	−.03	−.17	−.37	0
	Numbers	.28		.44	−.07	−.02	.62	.62	.50	−.78	.08	−.10	.25	−.37	.61	−.20	.28	−.72	.11
	Nouns . .	.25	.44		.20	.70	.85	.28	.88	−.57	.45	.02	.22	.13	.25	−.28	.56	.08	.28
	Prose . . .	−.24	−.07	.20		.52	.02	.03	.31	.40	.47	.33	.51	.76	.45	−.09	.53	.56	.29
	Faces . . .	−.01	−.02	.70	.52		.43	.07	.60	.05	.40	.38	.33	.67	.15	.22	.51	.58	.35
Immediate Auditory	Syllables	.51	.62	.85	.02	.43		.52	.75	−.66	.56	−.08	.38	.03	.42	−.18	.58	−.19	.29
	Numbers	−.27	.62	.28	.03	.07	.52		.32	−.50	.45	.40	.37	.12	.48	.26	.24	−.39	.22
	Nouns . .	−.01	.50	.88	.31	.60	.75	.32		−.63	.67	−.13	.29	.18	.43	−.30	.81	.10	.30
	Prose . . .	−.13	−.78	−.57	.40	.05	−.66	−.50	−.63		−.25	.24	.08	.54	−.33	.10	−.21	.58	−.04
Mediate Visual	Syllables	−.01	.48	.45	.47	.40	.56	.45	.67	−.25		.26	.83	.35	.85	.03	.90	.16	.41
	Numbers	−.14	−.10	.02	.23	.38	−.08	.40	.40	−.13	.26		.53	.57	.36	.90	.08	.23	.23
	Nouns . .	.20	.25	.22	.51	.33	.38	.37	.29	.08	.83	.53		.45	.86	.34	.66	.37	.42
	Prose . . .	−.20	−.37	.13	.76	.67	−.03	.12	.18	.54	.35	.57	.45		.20	.25	.48	.67	.30
Mediate Auditory	Syllables	.06	.61	.25	.45	.15	.42	.48	.43	−.33	.85	.36	.86	.20		.23	.62	−.05	.35
	Numbers	−.03	−.20	−.28	−.09	.22	.19	.26	−.30	.14	.03	.90	.34	.25	.23		−.17	.28	.09
	Nouns . .	−.17	.28	.56	.53	.51	.58	.24	.81	−.21	.90	.08	.66	.48	.62	−.17		.38	.38
	Prose . . .	−.37	−.72	.08	.56	.58	−.19	−.39	.10	.58	.16	.23	.37	.67	−.05	.28	.38		.14
	Average.	0	.11	.28	.29	.35	.29	.22	.30	−.04	.41	.23	.42	.30	.35	.09	.38	.14	

With the small number of cases the probable errors of the coefficients are necessarily high, as can be determined by the formula,

$$P, E(r) = \pm .706 \frac{\sqrt{1 - r^2}}{\sqrt{n}}.$$

It was manifestly impossible to carry out such detailed experiments as these with the number of individuals necessary to bring the coefficients within the standard reliability limits, and the conclusions drawn in this paper are stated with an appreciation of this fact.

The table at first glance shows an astonishing confusion and variability. A closer inspection reveals, however, some interesting results. Taking each of the seventeen sets of correlations seriatim before considering the amalgamated correlations, it is at once evident that immediate retention of syllables presented visually shows no correlation worth noting with any other memory function, except that for immediate memory for syllables presented auditorily. It is then clear that immediate visual memory for syllables can not be used as a test of memorial efficiency. The immediate auditory memory for syllables shows higher correlations, but even it is an index of memorial efficiency only for disconnected materials.

Immediate visual memory for numbers correlates fairly well with immediate and mediate disconnected material, except in the case of mediate memory for numbers where curiously enough it is negative; the average correlation with prose is −.49. Immediate visual numbers correlate fairly well with disconnected materials, while the correlations with prose average −.18. Immediate auditory memory for numbers is then a better index of memorial efficiency than is immediate visual memory for numbers.

Immediate visual and immediate auditory memory for nouns shows higher average correlations than either the syllables or numbers, and are therefore a better memory test; even here, however, the correlation of the visual with prose is −.04, and of the auditory with prose −.01.

Immediate visual memory for prose shows a rather high correlation with mediate memory for all types of material except auditory numbers; the correlation is also high with faces and immediate memory for nouns and auditory prose, but low with all the numbers and syllables.

Immediate auditory prose correlates well with other types of prose, but shows very little correlation with other functions of memory here tested; the average correlation with the other tests is —.04.

Faces show a higher average correlation than any type of immediate disconnected material, and are apparently especially good as an index of ability to learn nouns, mediate prose, and immediate visual prose.

Mediate visual memory for syllables shows an average correlation of .41 with all the other memories tested, though its average correlation with prose is only .19. Mediate auditory syllables show a lower general correlation, and the average correlation with prose is only .07.

Mediate visual memory for nouns shows an average correlation with other memory functions of .42; the correlation with prose is .35. Mediate auditory memory for nouns shows an average correlation with other memories of .38; the correlation with prose is .30. The learning of visual nouns is then a fair index of general memory, and of ability to learn prose which constitutes much of our learning.

Mediate visual memory for numbers shows a general correlation of .23, and a correlation with prose of .32; the average correlation with immediate materials is low. Mediate auditory memory for numbers shows an average correlation of only .09; the correlation with prose is .15. Visual numbers are therefore a better memory test than auditory numbers.

Mediate visual memory for prose has a correlation of .30 with other materials; mediate auditory memory has a correlation of only .14. Each correlates loosely with all immediate materials except faces, and the other presentation of prose; the lowest correlation with mediate material is that with syllables.

An inspection of the average correlations shows that of disconnected materials, mediate visual nouns is the best index of general memory; mediate visual syllables is second best; mediate auditory nouns ranks third; mediate auditory syllables and faces rank together, fourth; mediate visual prose and immediate auditory nouns rank next; immediate syllables and immediate auditory prose follow. If both methods of presentation are employed for both types of learning, the numbers are the least accurate of the materials through which to determine efficiency in general while the prose is but little superior.

CORRELATIONS IN IMMEDIATE AND MEDIATE RETENTION WITH PRESENTATIONS POOLED

There is a correlation of —.24 between the average ranks of the observers in immediate memory for the three types of disconnected materials with both visual and auditory presentations, and the average ranks for mediate prose with both presentations.

There is a positive correlation of .33 between the immediate retention for disconnected materials and the mediate retention for disconnected materials and prose amalgamated.

The immediate disconnected materials and the mediate disconnected materials show a correlation of .50.

Immediate memory for the different types of materials and mediate memory for the same show a correlation of .52.

The ranks in retention for immediate connected material and mediate connected material show the high correlation of .73.

These results show that there is a rather high correlation between immediate retention and mediate retention. Any single test of immediate memory correlates but loosely with mediate memory, but when a sufficient number and variety of measurements are given the amalgamated rank is a fairly good index of ability to memorize. Tests for immediate memory of disconnected material do not correlate as highly with mediate memory of the connected and disconnected material, and are not as useful an index of learning capacity. This dependence of the correlation on the type of material is brought out strikingly when we find an inverse correlation between immediate memory for syllables, numbers and nouns, and the capacity to learn connected discourse. The immediate memory for syllables, numbers and nouns correlates rather highly with the ability to learn the same materials; the coefficient is .50.

The highest correlation of the five discussed is between the immediate and the mediate learning of prose, *i. e.*, a memory function which has run parallel with comprehension and apperception is closely correlated with verbal learning.

The general result is clear. When the statement is made that there is little or no correlation between the memory span and ability to learn, the proposition is true only when we compare immediate memory for disconnected material with the learning of connected discourse. The immediate memory for disconnected materials, such as syllables, numbers and nouns, is correlated highly with the ability to learn the same type of materials. The immediate retention of connected discourse is correlated very highly with the ability to learn such material.

Disconnected Materials and Prose

There is an inverse correlation of —.27 between immediate memory for syllables and prose, and an inverse correlation of —.48 between immediate memory for numbers and prose.

Mediate memory for syllables and prose shows a correlation of .11, while mediate memory for numbers and prose shows .27. The correlations are low for both, showing the dependence of the adult on meaning even in verbal learning.

The immediate memory for nouns and prose shows a correlation of —.23, while the mediate memory for the same shows a correlation of .36. We find a correlation of .33 between the mediate learning of disconnected materials amalgamated and the immediate and mediate retention of prose amalgamated.

The correlation between the learning of all the disconnected materials and all the connected materials is an inverse one of —.01.

The memory span for numbers, nonsense syllables and nouns, seems then to have a negative correlation to immediate memory for prose, the type of learning in which the adult is most interested. No amalgamation of immediate and mediate learning of disconnected materials seems to give an index to the ability to retain prose. The mediate learning of disconnected material is a far more accurate measure of the mastery of connected discourse, but this correlation is only .33.

CORRELATIONS IN MEDIATE AND IMMEDIATE MEMORY WITH VISUAL PRESENTATIONS AND WITH AUDITORY PRESENTATIONS

The highest of the correlations, .92, is between the auditory and visual presentation of mediate disconnected material. The corresponding correlation for immediate disconnected material is .82; for mediate connected .67; for immediate connected .40. From this it appears that the auditory and visual presentations of disconnected materials correlate more highly than the same presentations of connected materials; also that mediate learning has higher correlations than immediate.

The immediate disconnected material, when compared with immediate prose shows correlations varying from .05 to −.74; when compared with mediate prose the correlations vary from .07 to −.47.

The mediate and immediate learning of the same types of material shows correlations ranging from .42 to .73; the correlations between the connected materials are higher than those between the disconnected.

Correlations between the mediate learning of prose and the mediate learning of disconnected material show a range from .20 to .54.

The immediate learning of prose presented visually shows a correlation of .49 with the mediate learning of disconnected material presented visually, and a correlation of .60 with the mediate learning of disconnected material presented auditorily. The immediate learning of prose presented auditorily shows a correlation of −.16 with the mediate learning of disconnected material presented auditorily, and a correlation of −.08 with the mediate learning of disconnected material presented visually.

VISUAL AND AUDITORY PRESENTATION. THE LAST GENERAL PROBLEM ON WHICH THESE RESULTS THROW LIGHT, IS THE CORRELATION BETWEEN VISUAL AND AUDITORY PRESENTATIONS

The correlations are .80 for the immediate learning by the different presentations and .75 for the mediate learning; the correlations between the mediate and the immediate memory are about half as much, averaging .37. The correlations, therefore, between the different presentations within the same type of learning are positive and very high.

Relative Values of Visual and Auditory Presentations

A comparison of the immediate retention with visual presentation and the immediate retention with auditory presentation shows very slight differences. The average number of units retained out of 222 with disconnected materials were: Visual, 173.9; auditory, 175.8. It is noteworthy, however, that the memory span, or immediate memory for disconnected materials is 76.7 per cent. with auditory presentation and only 71.2 per cent. with visual. The average number of words per second of reading time retained in immediate memory for prose were, with visual presentation 1.35 and with auditory presentation 1.38.

The average number of repetitions for learning syllables, numbers, and nouns with visual presentation is 6.9, while with auditory presentation is 7.4. The mediate learning of prose with visual presentation gave an average time of 31.4 minutes, while for auditory presentation it was 32.5 minutes.

Individual Differences

Many striking individual differences are disclosed. The most noteworthy will be enumerated.

1. Contrary to expectations in the immediate retention of prose all but two subjects, *A* and *D*, showed a higher efficiency from auditory presentation than from visual.

Table III. gives the results, calculated on a basis of number of words per second retained.

TABLE III

IMMEDIATE MEMORY FOR PROSE (WORDS PER SECOND)

	Vis.	Aud.
A	1.73	1.408
B	1.25	1.44
C	1.5	1.54
D	1.88	1.42
E	1.258	1.44
F	1.26	1.47
G	.965	1.225
H	1.38	1.45
I	.912	1.03
Average	1.35	1.38

Subject *D* is very dominantly visual in type but retains better from auditory presentation.

The same two subjects showed a higher efficiency with auditory presentation of disconnected materials also.

TABLE IV

IMMEDIATE MEMORY FOR DISCONNECTED MATERIALS

	Vis.	Aud.
A	180	182.5
B	178	174
C	166.5	168.5
D	189.5	190
E	167	177.5
F	170.5	160.5
G	171.5	178.5
H	154	168
I	188.5	184
Average	173.9	175.8

2. While there is a positive and fairly high correlation between the memory-span and ability to learn, there are three very marked cases, subjects *C*, *H*, and *I*; *C* ranks 7 in memory-span for disconnected materials and 1 in learning and immediate retention of prose; *H* ranks 9 in memory-span and 2½ in the tests with prose, while *I* ranks 2 in memory-span and 8½ with prose. It is these three cases that produce the very low correlation, −.30 between memory-span and the learning and retention of prose. The differences in ranks

with the other observers are slight. In other words but three of the observers out of nine show the lack of correlation which Meumann regards as the rule.

3. A striking difference between the observers was found in the feeling of certainty that the materials had been learned and the relation between this judgment and its accuracy. This became a source of great difficulty in determining the ranks in the learning of connected discourse. In no case could an errorless reproduction be given but the variability in number of errors was very great ranging from 1 by subject *D* in visual exposition, to 46 by subject *H* in auditory narration.

4. As to the relation between the accuracy of learning prose and the time required to learn, we cannot say that the slow learners are the most accurate or vice versa. *F* who gave the most accurate reproductions in the pooled results was comparatively slow; *D*, the most accurate in reproducing from visual presentation, learned rapidly; *G* learned slowly and made numerous errors in reproducing; *C*, a rapid learner, made fewer errors than some of the slower learners. These differences would indicate that there is no necessary relation of time and accuracy in mediate learning of prose.

Conclusions

1. Nouns presented visually for mediate learning show the highest of the correlations with the other memory functions tested and seem, therefore, to be the best index of general memorial efficiency; mediate syllables presented visually afford the second highest correlation.

2. There is a high correlation between mediate and immediate retention if a sufficient number and variety of measurements for each type of memory are taken, and the results amalgamated to determine ranks.

3. There is a high correlation between the memory span, or immediate retention for disconnected materials, and the ability to learn the same.

4. Six of the nine observers showed a high correlation between the memory-span and the pooled mediate and im-

mediate learning of prose; three subjects, two of whom are sisters, occupied great enough differences in ranks to make the average correlation inverse. The conclusion would seem to be that for the majority of individuals there is a high correlation between the memory-span and the ability to master prose.

5. Mediate retention shows higher intercorrelations than immediate retention.

6. Disconnected material for mediate retention shows higher correlations than does connected material.

7. The correlations between the mediate learning of connected materials, and the immediate learning of the same are higher than the corresponding correlations for disconnected materials.

8. The auditory presentation showed slightly better results for the immediate retention of both disconnected materials and prose.

9. The immediate retention of nonsense syllables was better with auditory presentation than with visual.

10. The mediate learning of both connected and disconnected materials was slightly better with visual presentation.

11. The correlations between the visual and auditory presentations within the same type of material are very high.

12. There seems to be no relation between the speed of learners and the accuracy of their performances.

THE EFFECT ON FOVEAL VISION OF BRIGHT SURROUNDINGS—III

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In previous reports of work on this subject under similar title (1, 2) it was intimated that the results there given relating to the brightness-difference threshold lacked in definiteness, the reason for this being in part at least due to the form of apparatus used to furnish the test-stimuli. The purpose of the present paper is to describe a modification of the apparatus used, by which those defects were overcome. The results obtained by the latter are to be the subject of a subsequent communication.

Reference to the first of the series will show that the results so far reported were obtained by the use of a differential method. The two halves of the test-field to be judged by the observer were parts of two illuminated milk-glasses thrown into visual juxtaposition by means of a double prism. The difference in brightness of these two was brought about by shifting one of them so as to alter its distance from the actual or virtual source of light, the other remaining stationary. The areas of the glasses used were rather large and there is always difficulty in securing uniformity of such transmitting media. Further, there is difficulty in securing uniform illumination over such areas. There was therefore no positive assurance that the brightness-difference across the line of division between the halves of the field was equal at all parts of that line, or that the separate halves of the field were each of uniform brightness. And finally, there was no positive way of knowing exactly when that difference was zero. There are still other ways in which the method has been improved which will be mentioned later.

The essential differences may be described at the outset. One is that the new method is additive, not differential.

That is, the difference between the two halves of the field is obtained by superposing an additional brightness upon one half of a constantly visible field of uniform brightness. This is brought about by partial reflection from a plate glass surface, and readily permits of independent measurement of the field brightness and of that of the added difference; which is a distinct advance over the photometric procedure of measuring the difference as such.

The second change consists in the projection of the test-field by a lens. This makes it possible to locate the image (which is the thing actually observed) at any distance from the observer. As a matter of fact it is at present placed in the plane of those parts of the surroundings which lie adjacent to it, making it easy and natural for him to accommodate accurately for it.

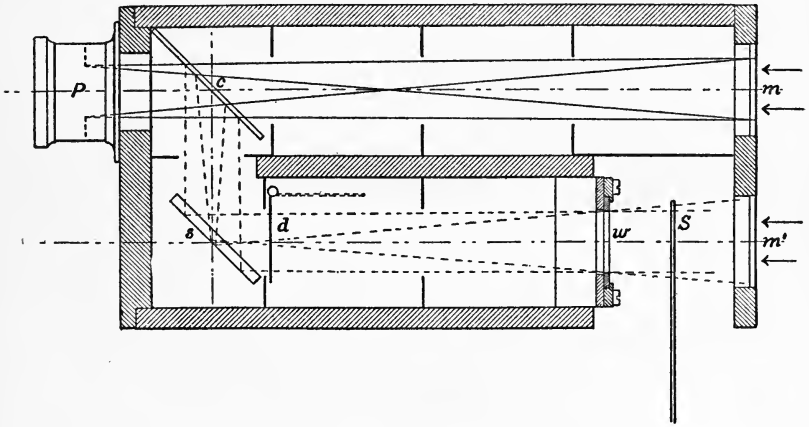

Fig. 1. Device for projecting test field and secondary image adding brightness to one half.

Reference to the figures will make clear how this is done. Fig. 1 represents the part embodying the principal change in the apparatus. It is simply a camera with its lens at *P*, by which an image of the milk-glass *m* is projected at some point on the other side of *P*. A second milk-glass *m'*, illuminated by the same source as *m* is so placed as to fill a small window *w* as seen from any part of the lens-aperture at *P* by its light reflected once from the silvered mirror *s* and again from the clear plate glass *c*. The result is that at some point beyond *P* there are two images; one of *m*, the light from it having suffered a minor loss in passing through the clear glass *c*, the other resulting from reflection at *s* and *c* and being an image of the window *w*. The two paths *mP* and *wscP* being made equal and the two mirrors being properly adjusted the images of *m* and *w* will coincide. It is to be noted that the latter of these two paths is possible owing to partial reflection at *c*, the reflection at *s* being not far from complete; while in the case of the former the path is direct with a small loss by transmission through *c*. The result is that the two images are of unequal brightness. With equal illumination on *m* and *m'* the brightness of the image of the window *w* is about 8 per cent. of that of the image of *m*.

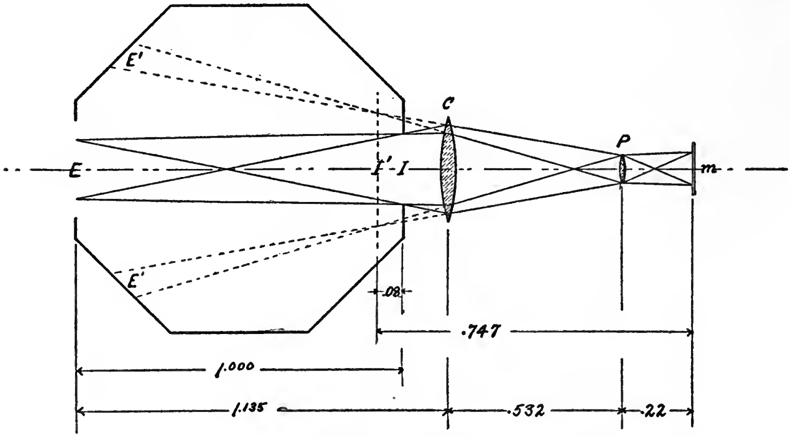

FIG. 2. The optical system and its principal dimensions (meters).

The window is provided with a shutter which may be slid sidewise to the extent of half its (horizontal) length, thereby cutting off half of its image, the other half being unobstructed is superposed on the image of *m* and brings about a measurable difference of brightness in the two halves of the field, which by sliding the shutter one way or the other may be made an increment to one half or the other. The exposure is made by a door situated at *d*, in the path of the light from *w*, which when closed cuts off the addition, leaving the field of uniform brightness.

The brightness of the surroundings is controlled as in the previous work (1) as indicated in Fig. 3. The principle of the optical system is shown diagrammatically in Fig. 2, where the transverse dimensions are exaggerated five-fold for the sake of clearness. The image from the camera would without further provision fall at *I′* and viewed directly by an eye placed at *E* much of the light would be lost since, to illustrate, the light from the extremes of the image would fall at *E′E′* and not enter the eye at all. The result would be that the image would be seen limited by the aperture of *P*. This is obviated by the use of a condensing lens at *C* by which the image of the aperture *P* is brought to a focus upon the eye, all the light from the image *I* falling within the circle thus formed. It will be seen from the figure to be a consequence of this that light from any part of *I* reaches the eye, diverging as it does so, exactly as though it came from a solid diffusing surface, provided that the eye be placed within

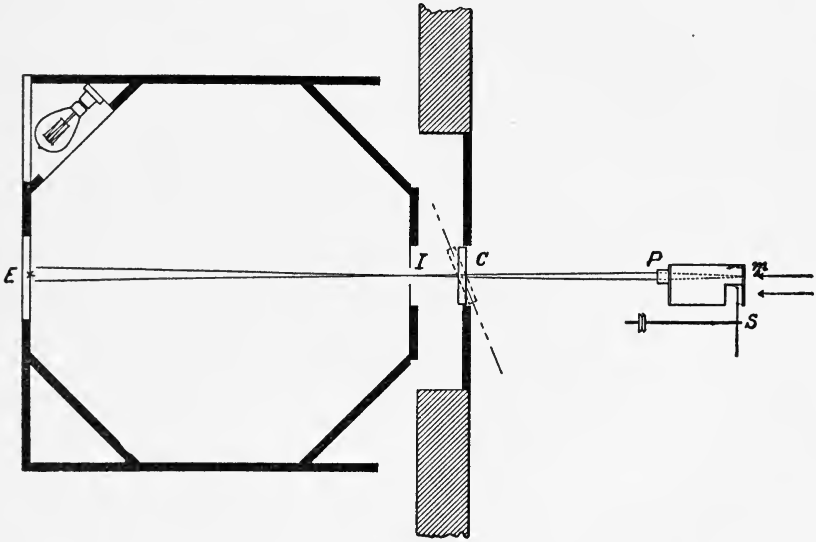

Fig. 3. General arrangement of apparatus. The test field is seen at *I* by the eye at *E*, surrounded by the illuminated inner surface of the polyhedral chamber *E–I*.

the image *E*; and that no light whatever from *I* falls anywhere else. This accomplishes the control of the field-brightness independently of that of the surroundings.

It is to be noted that the limiting edge of the field is the edge of the opening *I* in the surface forming the surroundings. As a matter of fact it is only necessary that the image of the intercepting edge of the window at *w* be in focus in this place provided the milk-glasses *m* and *m′* be large enough to intercept every possible optical path through the opening *I* and the aperture *P*. The latter (*m′*) is some 5 cm. from *w* and is large enough to fulfil this condition, and *m* being of the same size and nearly in focus is necessarily adequate.

Both mirrors, *c* and *s*, Fig. 1, had to be selected so that the double reflection in each case gave apparent coincidence of the vertical lines in the two images. Commercial plate glass was used, of which the sides are rarely quite parallel, but by careful examination the proper axis is readily found and the piece for use cut accordingly. Double reflection also results in slightly different lengths of path *wscP*, Fig. 1, but when the mirrors are selected as just said the several images of the edge intercepting all intersect the visual axis but at slightly different distances from the observer's eye. Monocular vision is used and under these circumstances this does not occasion any disturbance whatever.

P is a photographic lens of a fair grade having a focal length of 16.7 cm. and used with an aperture of 17 mm. *C* is a reading glass of the usual type 15 cm. in diameter and having a focal length of 37 cm. It is inclined as shown in Fig. 3 to avoid the reflection of light emerging through the opening *I* back into the eye of the observer at *E*. This does not occasion distortion of the image of the vertical intercepting edge of *w* since the latter in the position used during observation intersects the axis of the system.

The opening at *I* is 3.4 by 4.6 cm. Its projection at *m* and at *w* is 1.6 by 2.2 cm. The window *w* is somewhat larger than this, especially in its vertical dimension to allow for the overlapping of the double images. The milk-glasses *m* and *m′* are 3 by 4 cm. Other dimensions are given in Fig. 2.

The brightness of the addition to one half of the field is cut down by a variable sector disc mounted on a rotator of the Lummer-Brodhun design, originally intended for the rotation of paper color-discs. In place of the latter two sectors of black cardboard are used, each with two openings of 90°. This gives a maximum opening of 180° which may be reduced to 0 or set at any intermediate point while the sector is in rotation. The rotator is placed as shown in Fig. 3 with the edge of the sector at *S*, Fig. 1. The maximum addition possible is therefore about 4 per cent. with this arrangement, that is, half of that found with the sector removed. When under certain conditions this is insufficient a smoke glass is placed in the path *mP* and the illumination increased to compensate. The addition is then greater for the same brightness of field.

The illumination of the field as indicated by the arrows in the figures is from a source to the right of *m* and *m'*, which lights them both at once. A 500-watt tungsten filament vacuum lamp rated at 115 volts is enclosed in a cubical tin box painted white inside with a milk glass window 20 by 20 cm. in one side. The lamp is run at 87 volts since at that voltage a color match between the field and its surroundings is obtained. The latter are as previously described (1), lighted with lamps of the same type run at normal voltage, the necessity for the difference in running voltage arising chiefly from differences in selective reflection and transmission of the media which play parts in the two cases.

The photometric measurements are made with a Beckstein portable photometer calibrated against a surface of known brightness, by forming an image of the field in the plane of the photometer and so measuring the brightness directly. The field-brightness and the addition are measured separately by stopping one or the other path in the camera, the latter with the light unobstructed by the sector. This gives a ratio from which the ratio with any given sector-opening may easily be computed.

The various brightnesses of the surroundings are first measured relatively to one another in an exactly similar way.

Owing to the fact that their larger extent caused a larger amount of light to be scattered within the photometer their absolute brightness is computed from that of one of the series; which is obtained by first equating surroundings and field by means of the photometer, then darkening the surroundings and measuring the unchanged field as before.

The lamps are all on one circuit supplied by a storage battery and the voltage is accurately checked in the course of observation. The exposure is controlled electrically by a pendulum which automatically limits it to a predetermined interval. It is initiated by the observer through a key, and results from the opening of a small door *d*, Fig. 1, as indicated. The observer's eye is thus exposed to the uniformly bright field up to the instant the door admits the additional light to one half and the experiment begins.

The advantages of this arrangement over the one previously described are in brief:

1. The small extent of diffusing medium necessary to form the image decreases the magnitude of both the inequalities in its properties and those of its illumination. With the addition cut off it is not possible to detect difference in different parts of the field even with the photometer.

2. The point of equality of the two halves of the field (or at least a fixed reference-point) is therefore established in the physical sense when no addition is permitted to either half of the field.

3. The method used permits the measurement of the difference separately and directly, which is a decided gain in accuracy over its measurement *as* a difference.

REFERENCES

1. COBB AND GEISSLER. *Psychological Review*, XX., pp. 425–447, 1913.
2. COBB. *Ibid.*, XXI., pp. 23–32, 1914.

ON COLOR INDUCTION WITH REFERENCE TO COLOR RECOGNITION

BY MARY ALMACK[1] AND G. F. ARPS

Ohio State University

I

The Problem.—The precise nature of after-images of all sorts and their exact rôle in the determination of sensational and representational states of consciousness subsequent to the appearance of these after-images, remain, in spite of the enormous literature, clouded in doubt and surrounded by controversy. The interminable nature of the primary stimulus, the leakage of light through the sclerotic and iris coats of the eye, the varying sensitivity of the retina bring about the greatest diversity in the visual after-image. These relatively uncontrollable influences combine to make the after-effects of the primary stimulations very uncertain even for a particular observation or series of observations made in a given time, so that the features of after-images are, perhaps, as well characterized by their differences as their likenesses.

From the above it follows that the after-effects of a primary stimulation will affect a secondary stimulation recognizably different at different times. The lack of constancy here, not only in the primary stimulus but in the secondary as well, becomes an important source of error, as will be seen below.

The rôle of the after-image in the general economy of human behavior is wrapped in quite as much uncertainty as the character of the image itself. James[2] expressed the opinion that "we shall probably never know just what part retinal after-images play in determining the train of our thoughts. Judging by my own experiences I should suspect it of being not insignificant." From the large number of cases cited in this reference, it is obvious that the function,

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[1] Graduate student in psychology.

[2] 'Principles,' Vol. II, p. 84.

as well as the nature of the after-effects of stimulation is enveloped in considerable obscurity.

The nature of the after-effects of primary stimulation has long been an object of investigation but relatively little intensive work has been done on the influence of such effects on subsequent stimulation. A quantitative determination of the effects of negative after-images upon the threshold values for secondary color stimulations seems not to have been systematically undertaken. The purpose of the present study[1] is (1) to determine the limens of sensitivity for the commonly denominated primary colors when mixed with black and (2) to determine the limens of sensitivity for each of the same colors after adaptation to (*a*) blue, (*b*) yellow, (*c*) green and (*d*) red. With reference to the influence of adaptation, our problem is, by *how much* does the color and brightness of one stimulus affect the color and brightness of a second stimulus seen directly after it.

With reference to color recognition, Purkinje[2] observed that the primary colors were seen in their true tone as daylight advanced in the following order, blue, green, yellow and red. No limens for sensitivity were determined. Aubert,[3] by varying the daylight illumination on colors 10 mm. square, found the following order of color sensitivity, orange, red, yellow, blue, green. In a later experiment he employed a Masson disc and increased the proportion of color mixed with gray. The order of recognition now changed to orange, yellow, green, red and blue. He found that a little less than two or three degrees of color were needed to determine the recognitive limens of the colors when mixed with black. These experiments do not agree with our results as to the order of sensitivity for the colors red, yellow, blue and green when mixed with black and without the influence of primary stimulation. Our results are confirmed by Raehlemann[4]

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[1] An investigation of the recognitive limens for the four primary colors when (1) mixed with gray and (2) mixed with white will follow the present study.

[2] Purkinje, J., *Beiträge zur Kenntniss des Sehens*, 1825, 11, p. 109.

[3] Aubert, H., 'Untersuchungen über die Sinnesthätigkeit der Netzhaut,' *Pogg. Annal.*, 1862, CXV., pp. 87–116. Aubert, H., 'Physiologie der Netzhaut,' pp. 138–150.

[4] Raehlmann, E., 'Über Schwellenwerte d. verschiedenen Spectralfarben an versch. Stellen d. Netzhaut. A.F.O.,' 1874, XX., pp. 232–254.

who, on increasing the intensity of light necessary to give a spectrum, found the recognitive order of sensitivity to be, green, yellow, blue, violet and red. These results are for the limens without the effects of primary stimulation. Geissler[1] determined the limenal saturation values for various colors for each eye separately and for both eyes. The limens for saturation do not follow the order as given by Aubert, Raehlmann and Bentz. For the most part these studies bear upon the recognitive limens for colors independent of the after-effects of primary stimulation.

The influence of after-effects of previous stimulation upon color-matches was studied by Buhler,[2] v. Kries,[3] Hering,[4] Tschermak,[5] Watson[6] and others. There seems to be general agreement that the values of color-matches remain undisturbed by after-effects of previous stimulation, especially (v. Kries) so far as these matches apply to foveal vision.

After-effects of various kinds of primary stimuli have received the attention of a large number of investigators. As early as 1743 Buffon[7] reported that the after-image of a color fused with the background of another color (secondary stimulation) in such a way as to form a third color. It is interesting that the determination of the recognitive limens of colorless and chromatic secondary stimuli as affected by after-images of primary stimuli received little attention. Investigators, for the most part, observed qualitative changes in sensations of secondary stimulation due to previous stimulation.[8]

II

The apparatus employed in this study consisted of a four-spindle color-mixer, an induction chamber, a modified Kühlmann time apparatus and a seconds pendulum.

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[1] Geissler, L. R., 'Exp. on Color Saturation,' *Am. Jour. Psych.*, Vol. 24, pp. 171–179.

[2] Buhler, Dissert., University Freiburg, 1913.

[3] v. Kries, *Arch. f. Anat.*, 1878.

[4] Hering, *Arch. f. d. ges. Physiol.*, LIV., 1893.

[5] Tschermak, *Arch. f. d. ges. Physiol.*, 1898.

[6] Watson, *Proc. Roy. Soc. London*, 1913.

[7] Buffon, 'Dissert. sur les Couleurs Accidentelles,' Memoires de l'Acad. de Sciences de Paris, 1743, p. 213.

[8] Parsons, 'An Introduction to the Study of Color Vision,' pp. 105–112.

Four-Spindle Color-Mixer.[1]—The color-mixer, shown in Plates I., II., III., was equipped with an adjustable shutter *A*, with a micrometer screw attachment eight threads to the inch read on scale *B* in eighths of inches, and a hand drive *C* scaled to one hundred divisions for a full turn on *D*. Clutch *F* when set (*F′* in the phantom picture, Plate II.) secured the contact of wheel *E* (*E′* in Plate II.) with both wheel *G* attached to the motor *K*, and wheel *H* on the spider *O*. Wheel *M* on the same axis as wheel *H* was thus revolved when the clutch was set and the motor started. Special red, yellow, green, and blue color discs were held securely on the four wheels *M* of the spider by the thumb nuts *N*. (A full description of these discs is given below.) The photograph of the back of the apparatus (Plate III.) shows the method by which the revolving spider was made secure in each of the four positions by pin *P* extending through groove *R*. This enabled each of the discs to be brought into position for observation. A small shield *S* fitted over the adjustable shutter *A* in such a manner that on closure of the shutter the circular opening, as shown in the phantom picture, coincided with the circular opening in the shield. A large neutral gray shield *T*, which in the photograph appears below the apparatus, covered the entire front of the mixer, the small shield *S* being behind opening *V*.

Induction Chamber.—The dull black induction chamber was provided with interior lights. Two of these (16 c.p. each) indirectly illuminated sheets of paper eighteen inches square of the Milton Bradley standard red, blue, yellow and green. The end of the hood to the induction chamber was so shaped as to fit the face, thus excluding all other light stimuli.

Modified Kühlmann Time Apparatus.—Reference to the time apparatus, Plate IV., will make clear its mode of operation. As soon as the electromagnets became active, they attracted the armature 6, attached to the clutch 1. This

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[1] This piece of apparatus was designed by Dr. A. P. Weiss, who kindly supervised the making of Plates I., II. and III. and who rendered other valuable technical assistance. A more complete account of this piece of apparatus will be published in the near future.

(Almack and Arps.) PLATE I.

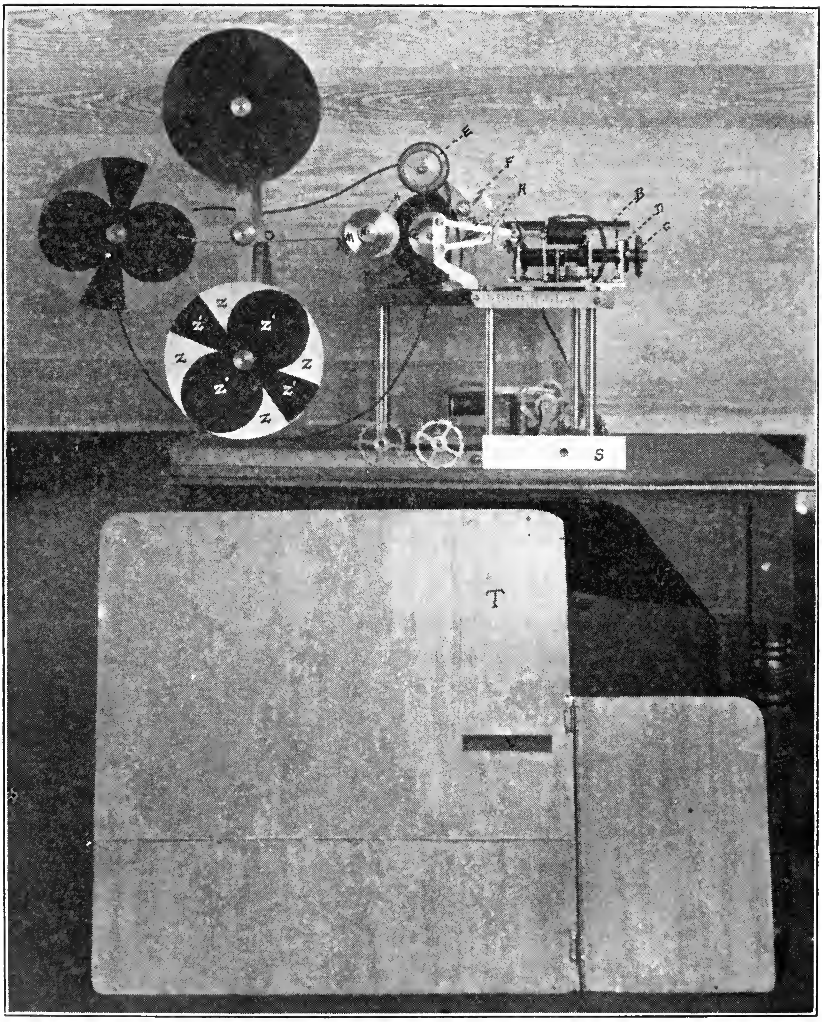

Plate I. Front View of Complex Color-mixer.

(ALMACK AND ARPS.) PLATE 2.

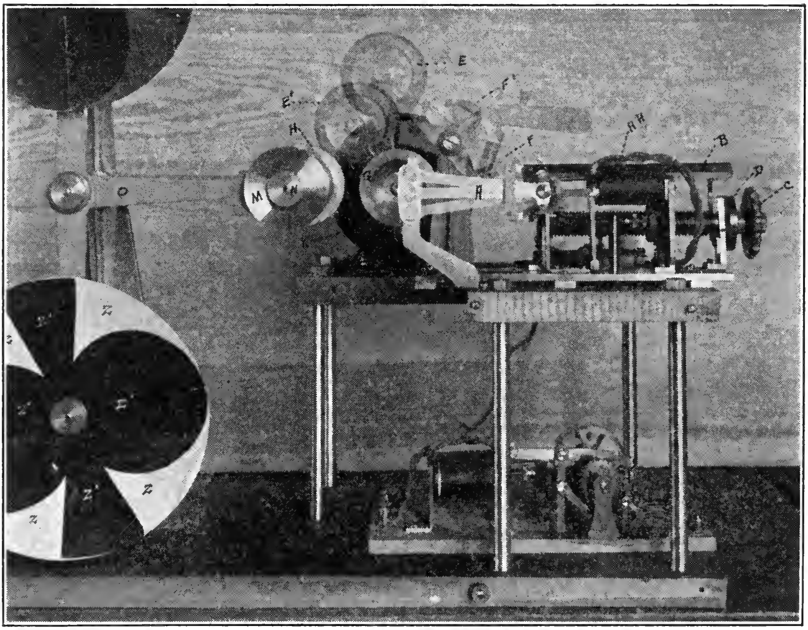

PLATE II. Front View of Complex Color-mixer. Phantom Picture.

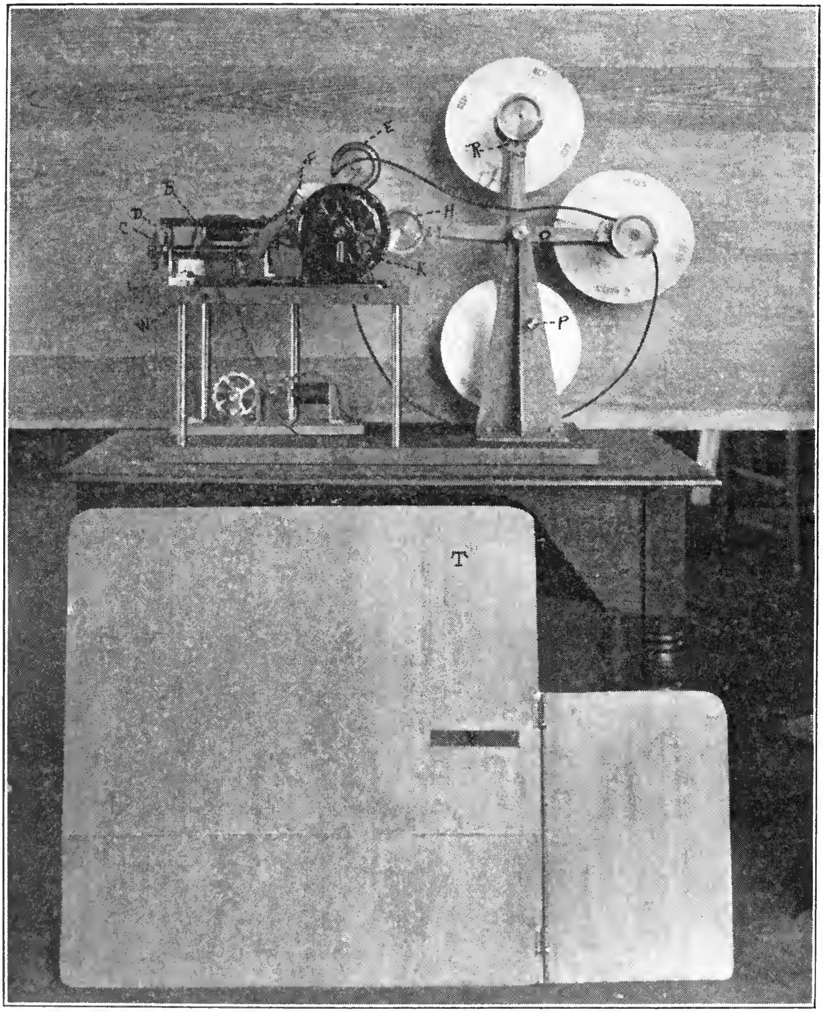

Plate III. Back View of Complex Color-mixer.

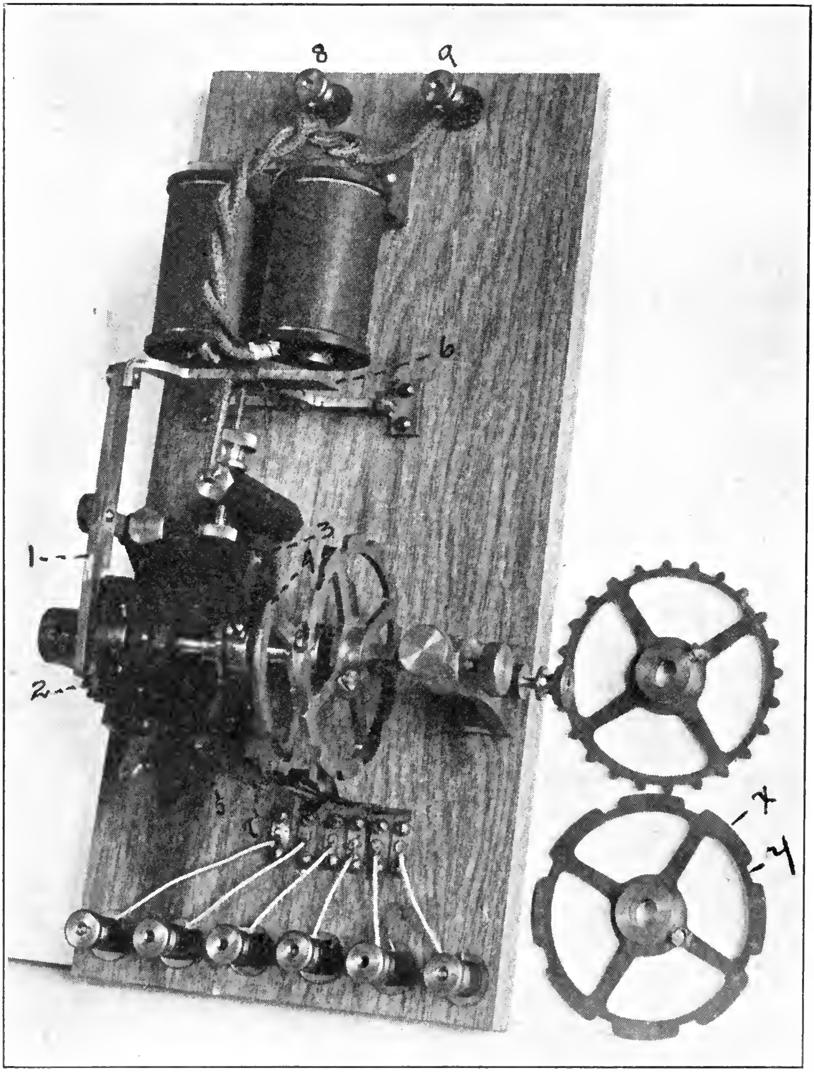

PLATE IV. Modified Kühlmann Time Apparatus.

clutch fitted into the toothed wheel 2 and each attraction of the magnet pulled the wheel one notch forward. The two wheels, 3 and 4, on the same axis with wheel 2, were so cut that at certain intervals they formed contacts with connections 5 and 7 respectively, while at other intervals, there was no contact. *X* and *Y*, referring to the separate wheels at the right of the apparatus, designate contact and no contact respectively.

Seconds Pendulum.—The seconds pendulum is a modified form of similar pieces of apparatus common to psychological laboratories. In this particular pendulum the contact was made by the point of the pendulum swinging through a mercury cup.

Color Discs.—Special color discs were made by pasting overlapping black paper discs five and one eighth inches in diameter on Milton Bradley color discs ten inches in diameter of the standard colors blue, yellow, green and red. Two strips of the same black paper, of ten degrees each, were added to the color portion. The character of the discs is shown in the photographs (Plates I. and II.). The color portions are designated by *Z*, the added black strips by *Z′*.

Light Restriction Hood.—A specially devised hood of gray pasteboard and about eight inches in length was so shaped as to fit the face. This hood was used in observing the secondary stimuli, limiting the light that might affect the eyes to that coming from the shutter opening and adjacent area on the gray shield.

III

The manner of articulating the various pieces of apparatus is set forth in Plate V. When the circuit was closed through key, *K*, each contact of the pendulum *S* with the mercury cup 1, completed the circuit through posts 8 and 9 of the time apparatus *T*, thus causing the wheels 3 and 4 (Plate IV.), to revolve one notch at a time. Contact between wheel 3, and connection 5 completed the circuit through the color-mixer. The shutter *A* was then opened by actuation of the armature, to which it was attached, by the electro-magnets (*AA*, Plate II.). The circuit was made through the induc-

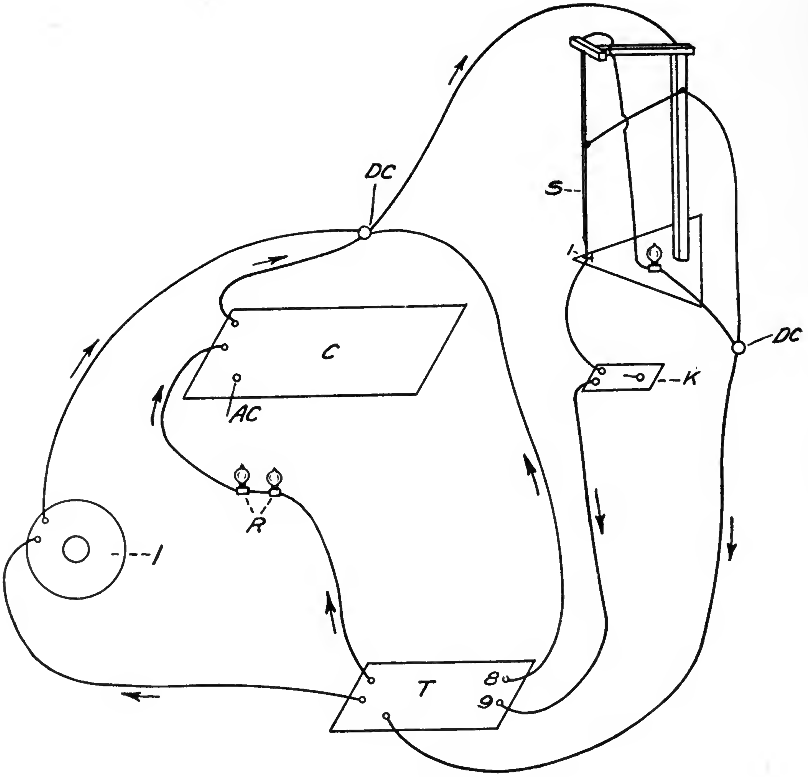

PLATE V. Diagrammatic Arrangement of Apparatus. *I*, Induction Chamber; *C*, Complex Color-mixer; *S*, Seconds-pendulum; *T*, Kühlmann Time Apparatus; *K*, Key; *DC*, Direct Current; *AC*, Alternating Current; *R*, Resistance.

tion chamber when wheel 4 came in contact with connection 7. This wheel (4) controlled the duration of the illumination of the inducing colors (primary stimuli). The motor was attached to an alternating current and controlled by a switch (*W*, Plate IV.).

IV

Observers.—Thirty-five observers functioned in the experiment, all of whom were familiar with the important facts of color vision. Twenty-seven were members of elementary classes in psychology, five members of a class in experimental psychology and three were instructors in the department. Discrepancies in color judgment were avoided, so far as possible, by testing each observer's color vision by means of the Holmgren wools. Observers who could not pass the test satisfactorily were rejected.

Procedure.—The observer was seated facing the mixer directly in front and about one and a half feet from the shutter opening. The induction chamber, at the side of the observer, was so arranged that the position for induction could be attained by merely turning the head to the right. The distance from the shutter opening was kept constant throughout.

Before the experiment was started, instructions were given as to the duration of the primary stimulus (induction), the interval between induction and the opening of the shutter and the manner of reporting the color.

Method Without Induction.—The first judgment was made with the micrometer screw set at five eighths, the second at six eighths and so on, the registering point of the screw being moved one eighth of an inch after each judgment by means of the hand drive *C.* The portion of the revolving disc exposed at five eighths approximated black, but as the micrometer screw on which the small shield and shutter were attached was moved to the succeeding eighths, the degree of color increased with each exposure. The shutter exposure moved from a point near the center of the disc, where there was minimal saturation, toward greater saturation at the outer edge of the disc. Exposures of the same disc, with increasing degrees of saturation, were continued until the color was recognized and confirmed by two immediately following judgments. If the judgment blue, for example, were made the two following judgments must be given as blue or the color was not considered as finally recognized. The limen of sensitivity without induction was first determined.

Method With Induction.—The instructions were repeated with reference to the duration of induction (primary stimulation), the interval between induction and the shutter opening, and the interval between closure of the shutter and the reappearance of the primary stimulus. The position for receiving the primary stimulus was assumed by the observer and the entire apparatus started as in previous experiments. The observer's attention was directed for a period of five seconds to the inducing color in the center of which was placed a small cross, as a fixation point. Two seconds intervened between the cessation of the primary stimulus and the appearance of the secondary stimulus through the shutter opening. This afforded ample opportunity for the observer to fixate the opening through the improvised light restriction hood. The revolving disc was exposed through the shutter opening for a period of one second. An interval of four seconds followed in which the observer reported the color as cognized and prepared for the second induction. The influence of one inducing color was determined in successive experiments for each of the four colors before employing a different inducing color. Experiments were stopped when evidence of fatigue appeared.

The order of presentation of the discs was changed to avoid as far as possible the factor of anticipation. Moreover, none of the observers were aware of the number of colors employed, for the reason that the substitution of the discs and the regulation of the degree of color exposed, were controlled from behind the shield (*T*, Plate I.).

V

Data.—Table I. gives one observer's judgments from the initial to the final micrometer readings for all the secondary

TABLE I

JUDGMENTS OF OBSERVER I

Inducing Colors (Primary Stimuli)	Color Disks (Secondary Stimuli)	Micrometer Readings in Eighths												
		5	6	7	8	9	10	11	12	13	14	15	16	17
None...	Blue....	Bk	Bk	Bk	Bl									
	Yellow..	Bk	Bk	Bk	Bk	Bk	Br	Br	DkG	DkOl	Ol	LtOl	LtOl	YG
	Green...	Bk	Bk	Bk	Bk	DkG	G							
	Red....	Bk	Bk	Bk	Bk	Bk	P	Br	Br	DkR	R			
Blue....	Blue....	Bk	Bk	Bk	Bk	Bl								
	Yellow..	Bk	Bk	Bk	OlG	OlG	OlG	OlG	OlG	LtOl	LtOl	YG	Y	
	Green...	Bk	Bk	Bk	Bk	DkG	G							
	Red....	Bk	Bk	Bk	Bk	Br	P	Br	Br	Br	DkR	R		
Yellow..	Blue....	Bk	Bk	Bk	Bl									
	Yellow..	Bk	Bk	DOl	DOl	Ol	Ol	Ol	Ol	Ol	YOl			
	Green...	Bk	DkG	G										
	Red....	Bk	Bk	Bk	Bk	Br	Br	Br	DkR	R				
Green...	Blue....	Bk	Bk	Bl										
	Yellow	Bk	Bk	Br	Bk	Bk	DkG	OlG	OlG	Ol	LtOl	YOl	Y	
	Green...	Bk	DkG	G										
	Red....	Bk	Bk	Bk	Br	P	RP	RBr	RBr	R				
Red....	Blue....	Bk	Bk	Bl										
	Yellow..	Bk	Bk	Bk	OlG	OlG	OlG	OlG	OlG	LtOl	LtOl	YG	Y	
	Green...	Bk	DkG	G										
	Red....	Bk	Bk	Bk	Bk	Br	Br	Br	Br	Br	Br	Br	Br	BrR

LtOl = light olive. The equivalents of the micrometer readings in degrees of color are found under Table XXIII.

stimuli with and without the influence of primary stimulation. A similar table was made for each of the remaining thirty-four observers but not incorporated separately in this study. The data in these thirty-five tables were rearranged as shown in Table II., except that Table II. contains only the judgments made on 'blue.' Tables III. to VII. inclusive summarize the data under Table II. The judgments on red, green and yellow are given in condensed form under Tables VIII. to XXII. inclusive.

The initial micrometer readings are the same for all colors except green which is three eighths. The final readings are seventeen, twenty, nineteen and eleven eighths for blue, yellow, red and green respectively. The degrees of color equivalent to these micrometer readings are found under Table XXIII.

In the condensed data of nearly all the tables there are

TABLE II

SUMMARY OF JUDGMENTS OF ALL OBSERVERS FOR THE BLUE DISK

Obs.		Micrometer Readings 5/8, Primary Stimuli					Micrometer Readings 6/8, Primary Stimuli					Micrometer Readings 7/8, Primary Stimuli				
		Bl	Y	Gr	R	N	Bl	Y	Gr	R	N	Bl	Y	Gr	R	N
1	Judgments	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bl	Bl	Bk
2	“	DkBl	Bk	DkBl	Bk	Bk	Bk	DkBl	Bl	DkBl	Bk	Bl	Bl	Bl	Bl	Bk
3	“	Bk	Bk	Bk	Bk	Br	PBl	DkBr	P	DkBr	P	DkP	P	P	Bl	Bl
4	“	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bl	Bk	Bk	Bk	DkV	Bl	Bk
5	“	Bk	Bk	Bk	Bk	Bk	BlBk	BlBk	Bk	Bk	Bk	BrBk	Bl	Bk	Bk	Bk
6	“	Bk	DkGr	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	DkBl	Bk	Bk	Bk
7	“	Bk	Gray	P	Bk	P	Bk	Gray	P	P	Bl	PBk	Bl	PBl	P	BlP
8	“	Bk	BrBk	BlBk	Bk	Bk	Gr	BlBk	BlBk	Bk	Bk	Bl	BlBk	BlBk	R	Bk
9	“	Bl	Bl	Bl	Bk	Bk	Bl	Bl	Bl	DkBl	Bk	Bl	Bl	Bl	Bl	BlBk
10	“	Bk	Bk	PBk	Bk	Bk	Bk	BlBk	BlBk	Bk	Bk	BlBk	BlBk	DkBl	PBk	Bk
11	“	P	Bk	DkP	Bk	Bk	P	DkP	DkP	Bk	Bk	Bl	P	DkP	DkP	Bk
12	“	BlBk	Bk	BkBl	Bk	Bk	BlBk	BlBk	DkBl	Bk	Bk	BlBk	Bl	Bl	BlBk	Bk
13	“	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk
14	“	Bk	Bk	Bk	BlBk	Bk	DkBl	DkBl	BlBk	Bl	Bk	Bl	Bl	Bl	Bl	R
15	“	Bk	PBk	Bk	Bk	Bk	BlBk	Bk	Bk	Bk	Bk	Bl	BlBk	BlBk	BlBk	Bl
16	“	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bl	Bl	Bk
17	“	DkGr	GrBk	Bk	Bk	DkGr	Bk	GrBk	BlBk	BrBl	DkGr	Bk	Bl	Bl	Bl	DkGr
18	“	Bk	DkBl	Bk	Bk	Bk	Bk	Bl	DkBl	DkGr	Bk	Bk	Bl	Bl	DkBl	Bk
19	“	Bk	PBr	RBr	Bk	Bk	Bk	PBr	RBr	Bk	Bk	RBk	PBr	RBr	PBk	Bk
20	“	BlBk	Bk	Bk	Bk	Bk	Bl	Bk	BlBk	Bl	Bk	Bl	BlBk	Bl	Bl	Bk
21	“	BlBk	Bk	Bl	PBk	Bk	Bl	Bk	Bl	BlBk	BlBk	Bl	Bk	Bl	Bl	Bl
22	“	BlBk	PBk	Bl	BlBk	Bk	Bl	BlBk	Bl	Bl	BlBk	Bl	Bl	Bl	Bl	Bl
23	“	Bk	BlBk	BlBk	Bk	R	Bk	Bl	Bl	Gr	R	BlBk	Bl	Bl	GrBk	Bk
24	“	DkBl	Bl	Bl	Bl	Bk	DkBl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	DkBl
25	“	DkBr	Bk	DkBr	DkR	Bk	Bk	DkP	DkP	DkBl	Br	DkP	BlP	DkBl	Bl	PBr
26	“	Bk	Bk	BlBk	BlBk	Bk	BlBk	Bk	P	Bk	Bk	Bl	Bk	Bl	Bl	Bk
27	“	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Gr	Bk
28	“	DkBl	BlBk	DkP	Bk	DkP	Bl	Bl	DkP	LBk	DkP	Bl	Bl	PR	BlBk	LtP
29	“	Bk	Bk	Bk	Bk	Bk	Bk	Bk	BlBk	BlBk	Bk	Bk	Bk	DkBl	DkBl	Bk
30	“	Bk	Bk	Bk	Bk	BlBk	Bk	Bk	Bk	Bk	BlBk	BlBk	Bk	Bk	BkBl	BkBk
31	“	PBk	Bk	Bk	Bk	GrBk	DkBl	BrBk	Bk	GrBk	GrBk	Bl	Bk	Bk	Bk	GrBk
32	“	BlBk	BlBk	Bk	Bk	Bk	BlBk	BlBk	Bk	BlBk	Bk	BlBk	BlBk	BlBk	BlBk	PBk
33	“	GrBk	PBk	Bk	Bk	BlBk	PBk	PBk	Bk	P	BlBk	GrBk	Bl	RBk	Bk	BlBk
34	“	Bk	Bk	Bk	BlBk	Bk	BlBk	Bl	DkBl	DkBl	DkBl	DkBl	Bl	Bl	Bl	Bl
35	“	Bl	Bk	V	Bl	Bk	Bl	Bk	V	Bl	Bk	Bl	Bl	V	Bl	BlBk

Abbreviations.—DkBl = Dark Blue; RBr = Red Brown; V = Violet; P = Purple.

Summary of Judgments of All Observers for the Blue Disk

Obs.		Micrometer Readings 8/8, Primary Stimuli					Micrometer Readings 9/8, Primary Stimuli					Micrometer Readings 10/8, Primary Stimuli				
		Bl	Y	Gr	R	N	Bl	Y	Gr	R	N	Bl	Y	Gr	R	N
1	Judgments	Bk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
2	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	DkBl
3	“	DkP	PBr	DkP	Bl	Bl	PBl	P	PBl	Bl	Bl	RP	DkBl	BlP	Bl	Bl
4	“	Bk	BlBk	V	Bl	Bk	Bk	PBk	DkBl	Bl	Bk	Bk	Bl	Bl	Bl	DkBl
5	“	BkBl	P	Bk	Bk	BlBk	BlBk	Bl	Bk	Bl	Bl	Bl	Bl	Bk	Bl	Bl
6	“	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
7	“	PBk	Bl	BlP	P	BlP	P	Bl	BlP	Bl	BlP	P	Bl	Bl	Bl	BlP
8	“	Br	BlBk	Bl	DkBr	Bl	Bk	BlBk	Bl	Bl	Bl	DkBl	Bl	Bl	Bl	Bl
9	“	Bl	Bl	Bl	Bk	BlBk	Bl	Bl	Bl	Bl	BlBk	Bl	Bl	Bl	Bl	Bl
10	“	BlBk	DkBl	Bl	BlBk	Bk	BlBk	DkBl	Bl	BlBk	BlBk	DkBl	Bl	Bl	DkBl	BlBk
11	“	Bl	LtP	Bl	P	P	Bl	Bl	Bl	Bl	PBl	Bl	Bl	Bl	Bl	Bl
12	“	Bl	Bl	Bl	BlBk	BlBk	Bl	Bl	Bl	Bl	BlBk	Bl	Bl	Bl	Bl	Bl
13	“	Bk	Bk	Bk	Bk	Bk	Bl	Bk	Bl	Bk	Bk	Bl	Bk	Bl	Bl	Bl
14	“	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
15	“	Bl	PBk	Bl	Bl	Bl	Bl	BlBk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
16	“	Bk	Bl	Bk	Bl	Bk	BlBk	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bl
17	“	Bk	Bl	Bl	Bl	DkGr	Bl	Bl	Bl	Bl	DkGr	Bl	Bl	Bl	Bl	Bl
18	“	Bk	Bl	Bl	Bl	BlBk	Bl	Bl	Bl	Bl	DkBl	Bl	Bl	Bl	Bl	Bl
19	“	RBr	PBl	P	Bl	P	Bl	PBr	P	Bl	P	Bl	Bl	Bl	Bl	BlP
20	“	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
21	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
22	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
23	“	Bl	Bl	Bl	GrBk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
24	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
25	“	BlP	DkBl	Bl	Bl	P	DkBl	Bl	Bl	Bl	P	Bl	Bl	Bl	Bl	Bl
26	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
27	“	Bk	Bk	Bk	Bk	Bk	Bk	Bk	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bk
28	“	Bl	Bl	BlBk	DkBl	BlP	Bl	Bl	DkBl	Bl	BlP	Bl	Bl	Bl	Bl	P
29	“	BlBk	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	BlBk	Bl	Bl	Bl	Bl	DkBl
30	“	BlBk	Bk	BlBk	BlBk	BlBk	DkBl	BlBk	BlBk	BlBk	BlBk	Bl	BlBk	DkBl	Bl	BlBk
31	“	Bl	PBr	GrBk	Bk	GrBk	Bl	DkBl	Bk	Bl	Bl	Bl	PBl	BlBk	Bk	Bk
32	“	Bl	BlBk	BlBk	nBl	BlBk	Bl	PBl	BlBk	Bk	Gr	Bl	Bl	Bl	Bl	Bl
33	“	Bl	Bl	PBk	DkP	Bl	Bl	Bl	P	DkP	Bl	Bl	Bl	Bl	PBl	Bl
34	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
35	“	Bl	Bl	Bl	Bl	DkBl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl

Abbreviations.—DkBl = Dark Blue; RBr = Red Brown; V = Violet; P = Purple.

SUMMARY OF JUDGMENTS OF ALL OBSERVERS FOR THE BLUE DISK

Obs.		Micrometer Readings 11/8, Primary Stimuli					Micrometer Readings 12/8, Primary Stimuli					Micrometer Readings 13/8, Primary Stimuli				
		Bl	Y	Gr	R	N	Bl	Y	Gr	R	N	Bl	Y	Gr	R	N
1	Judgments	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
2	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
3	"	DkBl	Bl	DkP	Bl	Bl	BlP	Bl	DkBl	Bl	Bl	Bl	Bl	PBl	Bl	Bl
4	"	DkV	DkBl	Bl	Bl	Bl	DkBl	VBk	Bl	Bl	Bl	Bl	DkBl	Bl	Bl	Bl
5	"	Bl	Bl	BlBk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
6	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
7	"	BlP	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
8	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
9	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
10	"	Bl	Bl	Bl	Bl	DkBl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
11	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
12	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl
13	"	Bl	Bk	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
14	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
15	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
16	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
17	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
18	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
19	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
20	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
21	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
22	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
23	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
24	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
25	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
26	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
27	"	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bk
28	"	Bl	Bl	Bl	Bl	DkBl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
29	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
30	"	Bl	BlBk	P	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
31	"	Bl	PBl	DkBl	BlBk	Bk	Bl	Bl	Bl	BlBk	BlBk	Bl	Bl	Bl	Bl	BlBk
32	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
33	"	Bl	Bl	Bl	PBl	Bl	Bl	Bl	Bl	PBl	Bl	Bl	Bl	Bl	PBl	Bl
34	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
35	"	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl

Abbreviations.—DkBl = Dark Blue; RBr = Red Brown; V = Violet; P = Purple.

Summary of Judgments of All Observers for the Blue Disk

Obs.		Micrometer Readings 14/8, Primary Stimuli					Micrometer Readings 15/8, Primary Stimuli					Micrometer Readings 16/8, Primary Stimuli				
		Bl	Y	Gr	R	N	Bl	Y	Gr	R	N	Bl	Y	Gr	R	N
1	Judgments	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
2	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
3	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
4	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
5	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
6	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
7	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
8	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
9	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
10	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
11	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
12	“	Bl	Bk	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
13	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
14	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
15	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
16	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
17	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
18	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
19	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
20	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
21	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
22	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
23	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
24	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
25	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
26	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
27	“	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bk	Bl	Bl	Bl	Bl	Bl
28	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
29	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
30	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
31	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
32	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
33	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
34	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl
35	“	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl	Bl

Abbreviations.—DkBl = Dark Blue; RBr = Red Brown; V = Violet; P = Purple.

certain judgments which involve two colors, or a color and a brightness, as the judgments YGr (yellow green), BlGr (blue green), DkBl (dark blue) and GrBk (greenish black). The variety of judgments was condensed according to the following directions:

1. When brightness was coupled to a color in a judgment the designated brightness was omitted from the record. Dark blue, for example, was recorded as blue.

2. When a judgment involved two colors of equal prominence that color was recorded which corresponded to the color on the disc. Purple blue was recorded as blue.

3. When a judgment involved two colors neither the one nor the other of which was the color on the disc that color was recorded which nearest resembled the disc. The judgment purple brown was recorded purple when the color on the disc was blue.

4. When a single color judgment, or a color judgment coupled with a brightness failed to correspond to the color on the disc the color judgment alone was recorded. The judgment dark brown on the green color disc was recorded as brown.

The 'Aggregation of Re-classified Judgments,' found in Tables III. to XXII. inclusive, constitutes a condensed grouping of all the data. Tables IV. to XXII. inclusive are abridgments of data analogous to that contained in the upper half of Table III.

Table XXIII. contains the degrees of color which were mixed with black for each micrometer reading. For green and red the amount of color was slightly less than for the blue and yellow for the 4/8 and 5/8 readings. In a comparison of the recognitive thresholds this gives a slight advantage or disadvantage to the former colors depending upon the nature of the primary stimulation.

The Curves.—Separate curves (ascending) for each color of the four discs under the five conditions of influence together with the curves (descending) for the accompanying 'black' judgments were plotted from the 'Aggregate Judgments' given in the tables. Ten curves were, therefore, drawn for each color. To prevent crowding the curves two figures are devoted to each color, as Fig. 1 and Fig. 2 for the blue disk. The numerals along the ordinate axes indicate the distribution of the observers with reference to judgments of color for the different micrometer readings which are found along the abscissæ. Beneath these readings are the corresponding degrees of color.

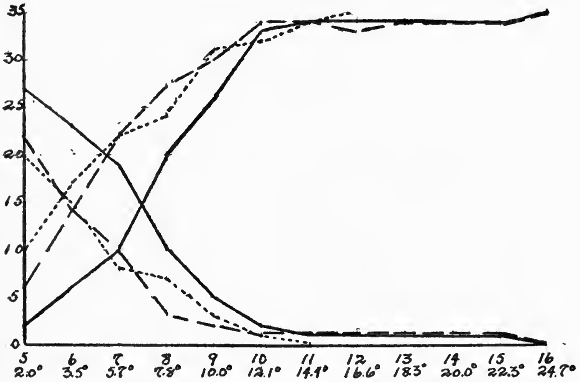

Fig. 1. Recognitive Limens for the Blue Disk.

The intersections of the 'color' curves with the 'black'

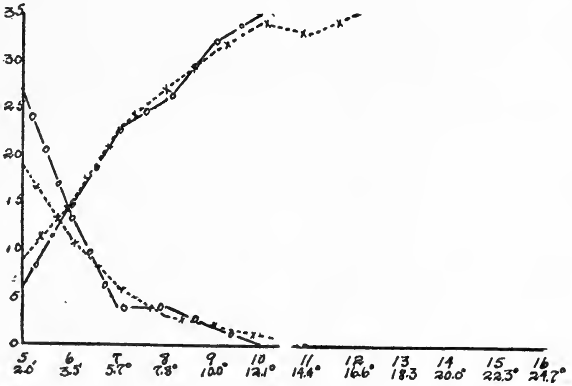

Fig. 2. Recognitive Limens for the Blue Disk. Points of intersection of curves = recognitive limens. Ascending curves = color judgments; descending curves = black judgments. ——— = no influence; . . . . . . = blue influence; ——— ——— = yellow influence; . . .*. . . = green influence; ——o—— = red influence.

FIG. 3. Recognitive Limens for the Yellow Disk.

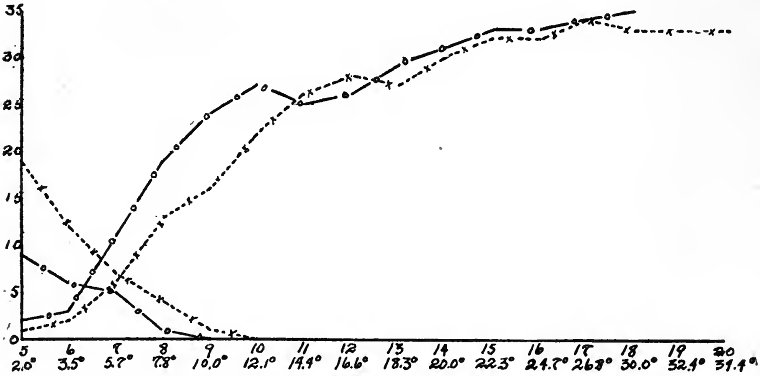

FIG. 4. Recognitive Limens for the Yellow Disk.

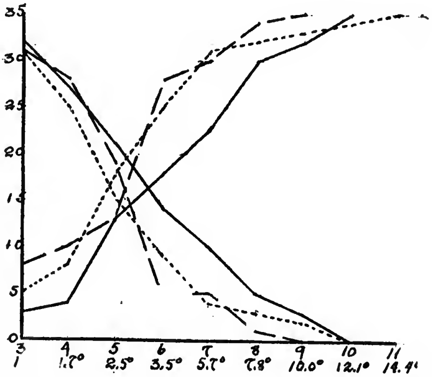

FIG. 5. Recognitive Limens for the Green Disk.

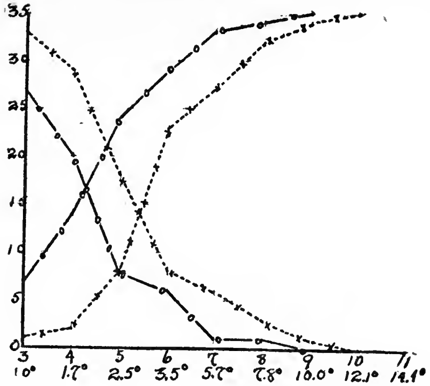

FIG. 6. Recognitive Limens for the Green Disk.

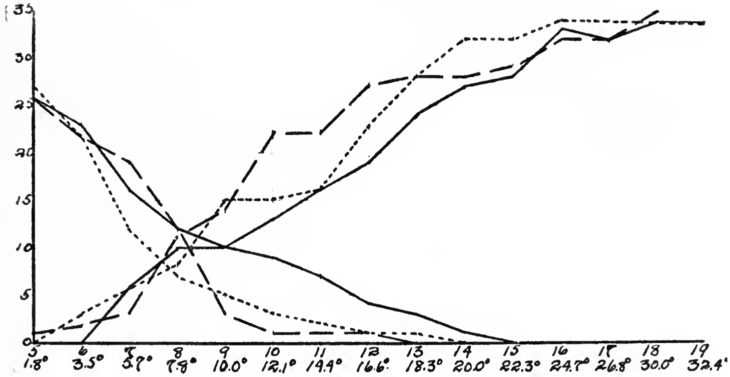

FIG. 7. Recognitive Limens for the Red Disk.

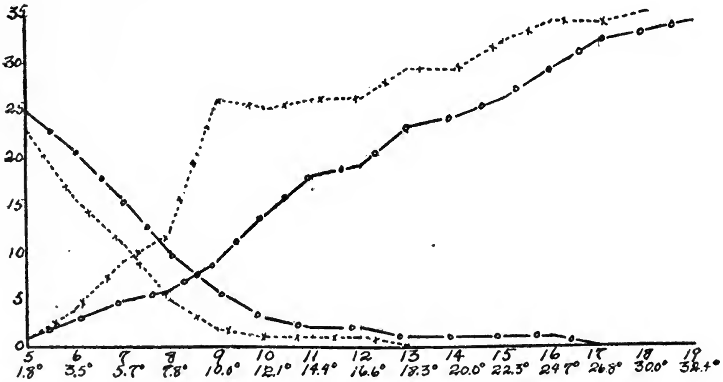

FIG. 8. Recognitive Limens for the Red Disk.

curves (Figs. 1 to 8), mark the recognitive limens for the various colors with and without the effects of primary stimulation. Since at the point of intersection the number of color judgments and black judgments is equal, this was termed the recognitive limen. The limens for the four colors of the color discs as influenced by the four inducing colors are arranged in an ascending series in Fig. 9. This sets forth in a diagrammatic way not only the amounts of color necessary for the recognition of a given color with different influences but also the relation of all the limens of one color with the limens of the remaining three colors.

TABLE III

COMBINED RESULTS OF ALL OBSERVERS FOR BLUE WITH NO INFLUENCE

Judgments	Micrometer Readings in Eighths											
	5	6	7	8	9	10	11	12	13	14	15	16
Bk	27	23	19	10	5	2	1	1	1	1	1	
DkBl		1	1	3	1	3	2					
BlBk	2	4	4	7	5	2						
Bl			4	8	17	26	31	34	34	34	34	35
P	1		1	3								
DkP	1	1										
GrBk	1	1	1	1								
DkGr	1	1	1	1	3							
Br	1	2										
BlP		1	1	2	3	2	1					
R	1	1	1									
PBr			1									
PBk			1									
Gr					1							

AGGREGATION OF RE-CLASSIFIED JUDGMENTS

Bk	27	23	19	10	5	2	1	1	1	1	1	
Bl	2	6	10	20	26	33	34	34	34	34	34	35
P	2	1	3	3								
G	2	2	2	2	4							
R	1	1	1									
Br	1	2										

Numerals under the micrometer readings signify the number of observers making a given judgment. 'With No Influence' means the absence of primary stimulation. The term 'Blue,' preceded by 'with No Influence,' refers to the color of the disk (secondary stimulus), on which judgments are made. Similar explanations apply to Tables IV. to XXII. inclusive. All judgments other than Bk and Bl, except as indicated above, are random judgments; they represent hazards and are not incorporated in the curves. Similar judgments are found in Tables IV. to XXII. inclusive.

TABLE IV

RESULTS OF ALL OBSERVERS FOR BLUE WITH BLUE INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths								
	5	6	7	8	9	10	11	12	13
Bk.	20	15	8	7	3	1			
Bl.	10	17	22	24	31	32	34	35	

TABLE V

RESULTS OF ALL OBSERVERS FOR BLUE WITH YELLOW INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths											
	5	6	7	8	9	10	11	12	13	14	15	16
Bk.	22	14	10	3	2	1	1	1	1	1	1	
Bl.	6	14	22	27	30	34	34	33	34	34	34	35

TABLE VI

RESULTS OF ALL OBSERVERS FOR BLUE WITH GREEN INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths									
	5	6	7	8	9	10	11	12	13	14
Bk.	19	11	6	3	2	1				
Bl.	9	15	23	27	31	34	33	35	35	35

TABLE VII

RESULTS OF ALL OBSERVERS FOR BLUE WITH RED INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths									
	5	6	7	8	9	10	11	12	13	14
Bk.	27	14	4	4	2					
Bl.	6	15	23	26	32	35	35	35	35	35

TABLE VIII

RESULTS OF ALL OBSERVERS FOR YELLOW WITH NO INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths															
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Bk.	15	16	9	5	2	1	1									
Y.	1	2	9	16	21	24	26	28	30	31	32	32	33	33	34	34

Table IX

Results of All Observers for Yellow with Blue Influence

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths															
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Bk	17	12	1													
Y	2	5	10	19	23	27	29	32	33	34	35					

Table X

Results of All Observers for Yellow with Yellow Influence

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths															
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Bk	22	15	4	1	1											
Y	0	2	7	15	20	27	28	30	32	31	33	34	35			

Table XI

Results of All Observers for Yellow with Green Influence

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths															
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Bk	19	12	7	4	1											
Y	1	2	6	13	16	22	26	28	27	30	32	32	34	33	33	33

Table XII

Results of All Observers for Yellow with Red Influence

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths															
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Bk	9	6	5	1												
Y	2	3	11	19	24	27	25	26	29	31	33	33	34	35		

Table XIII

Results of All Observers for Green with No Influence

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths							
	3	4	5	6	7	8	9	10
Bk	32	27	21	14	10	5	3	
Gr	3	4	13	18	23	30	32	35

TABLE XIV

RESULTS OF ALL OBSERVERS FOR GREEN WITH BLUE INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths								
	3	4	5	6	7	8	9	10	11
Bk.	29	25	15	9	4	3	2	1	
Gr.	5	8	18	25	31	32	33	34	35

TABLE XV

RESULTS OF ALL OBSERVERS FOR GREEN WITH YELLOW INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths							
	3	4	5	6	7	8	9	10
Bk.	31	28	19	5	5	1		
Gr	3	5	13	28	30	34	35	

TABLE XVI

RESULTS OF ALL OBSERVERS FOR GREEN WITH GREEN INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths								
	3	4	5	6	7	8	9	10	11
Bk.	33	29	18	8	6	3	1		
Gr.	1	1	8	23	27	32	34	35	

TABLE XVII

RESULTS OF ALL OBSERVERS FOR GREEN WITH RED INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths							
	3	4	5	6	7	8	9	10
Bk	27	20	8	6	1	1		
Gr	7	14	24	29	33	34	35	

TABLE XVIII

RESULTS OF ALL OBSERVERS FOR RED WITH NO INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths														
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Bk.	26	23	16	12	10	9	7	4	3	1					
R.	0	0	6	10	10	13	16	19	24	27	28	33	32	34	34

TABLE XIX

RESULTS OF ALL OBSERVERS FOR RED WITH BLUE INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths														
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Bk.....	27	22	12	7	5	3	2	1	1						
R......	0	3	6	8	15	15	16	23	28	32	32	34	34	34	34

TABLE XX

RESULTS OF ALL OBSERVERS FOR RED WITH YELLOW INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths														
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Bk.....	26	22	19	12	3	1	1	1							
R......	1	2	3	11	14	22	22	27	28	28	29	32	32	35	

TABLE XXI

RESULTS OF ALL OBSERVERS FOR RED WITH GREEN INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths														
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Bk.....	23	16	11	5	2	1	1	1							
R......	1	4	9	12	26	25	26	26	29	29	32	34	34	35	

TABLE XXII

RESULTS OF ALL OBSERVERS FOR RED WITH RED INFLUENCE

Aggregation of Re-classified Judgments

Judgments	Micrometer Readings in Eighths														
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Bk.....	25	21	16	10	6	3	2	2	1	1	1	1			
R......	1	3	5	6	9	14	18	19	23	24	26	29	32	33	34

VI

Results.—Figs. 10 and 11 show the recognitive limens in condensed form for the four primary colors with and without the influence of primary stimulation; they also throw into clear relief the quantitative relations of the various limens. As already indicated these limens are the intersecting points of the curves grouped in Figs. 1 to 8 inclusive.

TABLE XXIII

DEGREES OF COLOR COMMENSURATE WITH MICROMETER READINGS

Eighths	Blue	Color Disks Yellow	Green	Red
4	1.2	1.2	1.7	0.7
5	2.0	2.0	2.5	1.8
6	3.5	3.5	3.5	3.5
7	5.7	5.7	5.7	5.7
8	7.8	7.8	7.8	7.8
9	10.0	10.0	10.0	10.0
10	12.1	12.1	12.1	12.1
11	14.4	14.4	14.4	14.4
12	16.6	16.6	16.6	16.6
13	18.3	18.3	18.3	18.3
14	20.0	20.0	20.0	20.0
15	22.3	22.3	22.3	22.3
16	24.7	24.7	24.7	24.7
17	26.8	26.8	26.8	26.8
18	30.0	30.0	30.0	30.0
19	32.4	32.4	32.4	32.4
20	34.4	34.4	34.4	34.4

Reference to Figs. 10 and 11 indicates the following results:

1. The recognitive limens for blue (7.5–6.7°),[1] red (9–10°) and green (5.4–2.9°) 'without influence' are higher than any other limens for these colors when 'influenced by the after-effects of primary stimuli.'

2. The five limens for blue, red and green form identical series, as to sequence, of decreasing magnitudes. These series conform to the following order of primary stimulations: red, yellow, blue and green. While the relative values of the limens in these three series remain the same, their absolute values vary. For example, the limen for blue, determined after stimulation for red, is 6.1–3.7° while for red under the same conditions the limen is 8.6–9.10° and for green 4.3–1.9°. The maximum and minimum limens for green, blue and red under identical conditions with respect to previous stimulation are 5.7–3.3°; 4.3–1.9° (green), 7.5–6.7°; 5.7–3° (blue) and 9–10°; 7.2–6.1° (red). The differences of these maxima and minima in the direction of red are 3.3–6.7°; 2.9–4.1° and 1.5–3.3°; 1.5–3.1°.

3. The sequence of limens for yellow varies somewhat from those for blue, green and red. In the case of yellow two of

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[1] 7.5–6.7° = micrometer readings (7.5 eighths) and degrees of color (6.7°).

the limens fall together, *i. e.*, the limen for yellow without the after-effects of primary stimulation and the limen with the after-effects of previous stimulation with green is 7.1–5.9°. The sequence of limens for yellow following the order of

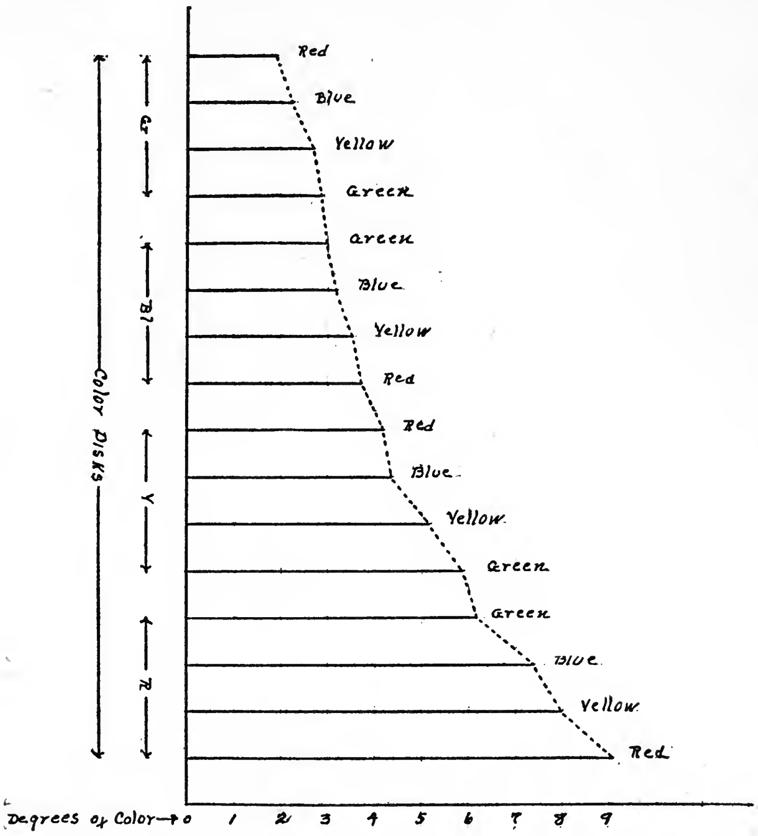

FIG. 9. Quantitative relationship of the various limens for the secondary stimuli (color disks). The terminal readings of the various abscissæ indicate the limens of color sensitivity for the four secondary stimuli as influenced by the different inducing colors. The names of the colors at the ends of the lines indicate primary stimuli (inducing colors).

primary stimulations as shown in the graph (Fig. 11) is: Green (none), yellow, blue and red.

4. The limens for red corresponding to the limens for the three other primary colors are throughout the highest; those

for green are throughout the lowest; while the corresponding limens for blue and yellow fall between the two extremes of the limens for red and green. Fig. 9 represents the relative positions graphically. It will be noted, and this is interesting, that the antagonistic colors red and green have throughout

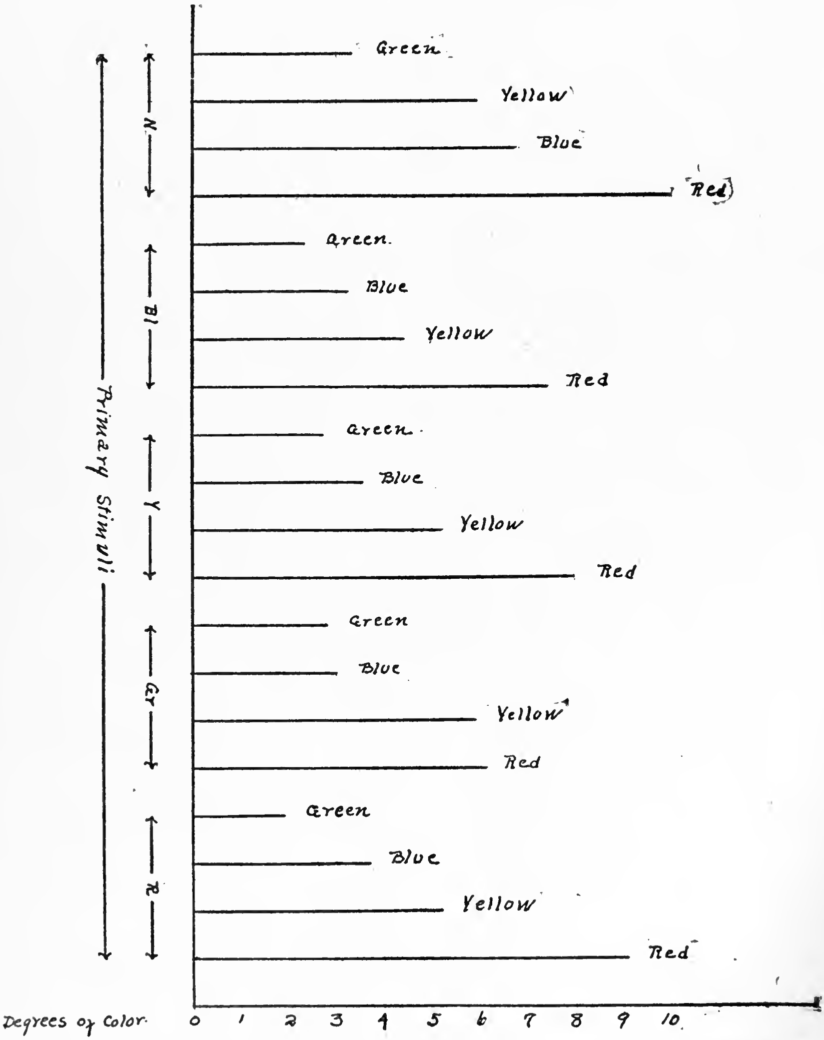

FIG. 10. Limens of sensitivity with and without induction. Color names at the ends of lines indicate the secondary stimuli (color disks). The terminal readings of the various abscissæ indicate the limens of color sensitivity for the four primary colors as influenced by the different primary stimuli.

the highest and the lowest limens respectively while the limens for the complementary colors blue and yellow fall between these two extremes.

Conclusions.—The duration and character of the after-effects of stimulation are so varied and elusive, the exact composition of color papers so difficult of precise determination, the condition of the retina and the composition of incandescent lights so variable and the influence of eye-movements, practice and fatigue so important that deductions from the data should be made with the greatest care.

1. When the after-effects of a primary stimulus is similar to the color of the disc the limen is the lowest of the five limens for this color. This is true for the green, yellow and red color discs employed in this experiment. In these cases the limens are 4.3–1.9°; 6.4–4.4° and 7.2–6.1° respectively. An exception to this conclusion is found in the case of the blue disc in which case the lowest limen is connected with

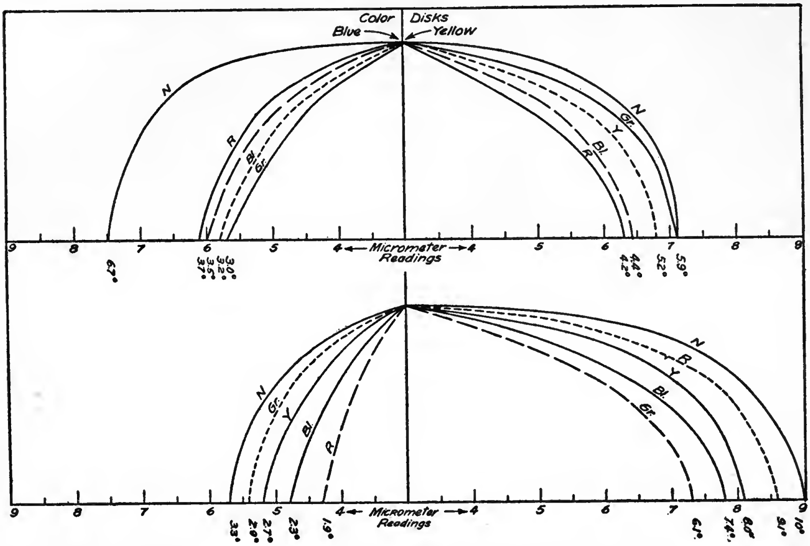

FIG. 11. Recognitive limens as given in micrometer readings and degrees of color. Color abbreviations on curved lines indicate the colors of the primary stimuli. *N* = primary stimulus lacking. ...., indicate primary stimuli followed by after-images complementary to colors on vertical line. —— ——, ——, indicate primary stimuli complementary to colors on vertical line. The points on the vertical line were arbitrarily chosen.

green rather than a primary stimulation of yellow as we might expect. The explanation is probably found in the difference of brightness values of the after-effects of blue and green stimulations, the effects of the latter in this respect being greater. The difference between these two limens is, however, very slight, being .3/8 micrometer reading or .2° of color. In this connection it is interesting to note that Parsons[1] observed: "If the secondary stimulation is the complementary color of the primary, the resulting sensation is that of an extremely saturated complementary color." The lowest limens for the green, yellow and red colors are cases in which the primary stimuli are complementary colors, *i. e.*, red for the bluish green, blue for the yellow and bluish green for the red. The after-images for these primary stimuli would, therefore, be same-colored as the colors on the discs, and tend to raise the saturation of these colors. It is altogether likely that the saturation of the after-image exceeds that of the color on the discs so that the total effect is a compromise between the degree of color with reference to its saturation and the after-image.

With reference to the saturation of a spectral color Parsons[1] observes: "It may be far more saturated than any spectral color so that it is impossible to obtain a comparison light which will match it." The investigations of v. Kries[2] and his students on the effect of the after-images on color matches are instructive in this connection. He found that a yellow paled or became more saturated according to whether the after-image was blue or yellow. It appears that we may probably conclude that the lowest limens for green, yellow and red are traceable to an increase in saturation due to the after-effects of the primary stimuli complementary to these colors.

2. When the primary stimulations are not complementary to the secondary stimulations the influence of the effects of primary stimuli upon the recognitive limens of the secondary stimuli is more difficult to trace. From Fig. 10 it is seen that

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[1] Parsons, *op. cit.*, p. 109.

[2] Citation from Parsons' 'Introduction to Color Vision,' p. 105.

the limens for the colors here investigated are, in eleven cases out of twelve, lower when the primary stimuli are not the complementary of the secondary stimuli. The exception is the limen for yellow when the primary stimulus is green. This limen falls together with the limen of yellow when uninfluenced by primary stimulation.

Theoretically, and perhaps upon the basis of the work of v. Kries (quoted above), we might expect a higher limen for a given color when the after-image of a primary stimulus is complementary to this color. For example, a blue after-image pales a yellow color. If the degree of saturation is a determinant of color recognition then we should expect higher limens for colors when influenced by after-images which are complementary (antagonistic) than when primary stimulations are lacking. Our results are not, in the main, in accord with this expectation. Moreover, our color limens are (except for yellow, in which case the limen is equal) lower when influenced by after-images which are neither complementary to nor same-colored as the color discs than when primary stimulations are entirely lacking.

APPARATUS AND EXPERIMENTS FOR THE INTRODUCTORY COURSE

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I. A Combined Tilting Board and Rotation Table

Apparatus.—This is a modification of the two pieces of apparatus described in Sanford's 'Course.' The board used in the combination is 6 ft. 4 in. long, 2 ft. wide, and 1½ in. thick. At each end a piece of 3½ × 1½ inch pine is nailed on the upper side to serve as a brace as well as to prevent the subject from slipping during the tilting experiment. Near each end notches are cut in the side to serve as grooves for the straps which hold the subject in position. Beneath the center of the board is fastened the swivel screw from an office chair for the horizontal rotation, and across the middle of the board is a steel axle about which the board rotates vertically in the tilting experiment. This axis projects about 3 inches beyond each edge of the board.

The figure (Fig. 1) shows the apparatus used as a tilting board with a subject strapped on as in an experiment. One support for the board, in shape of an A, is permanently fixed to the wall and is made of yellow pine. The other support, of 4 × 3 in. white pine, is a post which extends from floor to ceiling and is removed when not in use. A hole in the ceiling slightly larger than this upright allows it to just slip through and the fit is then made snug by V-shaped wedges. On the bottom of the upright is a small iron projection which fits into a hole in the floor. The bearings for the axis of vertical rotation are Y-shaped and are made of steel. These bearings are securely fastened to the supports just high enough to allow about six inches clearance from the floor when the board stands vertical. Thus the subject may be rotated through all angular positions from the natural upright position to a position in which his head is straight down. An angular scale with a plumb bob is attached to the board so that all angular positions are read directly.

Procedure.—In the introductory course the tilting experiment is conducted as follows: The subject, with eyes closed, is strapped to the board; one experimenter holds each end of the board, and usually a third reads and records the judgments. (*a*) The experimenters slowly rock the board up and down a few times and the subject then directs the movement until he judges his body parallel to the floor. The scale reading is recorded in a column headed 'Level.' (*b*) The board is then slowly rotated so that the subject's head rises; the subject directs the movement until he judges himself to be 'half-way up' and this is recorded under + 45°. (*c*) The rotation is continued until the subject judges himself perpendicular to the floor. Recorded under + 90°. (*d*, *e*) The direction of rotation is now reversed and the subject again indicates the 'half-way' and 'level.' (*f*, *g*, *h*, *i*) Continuing the rotation, the subject's head is moved down and he indicates the − 45° and − 90° points. Reverse the

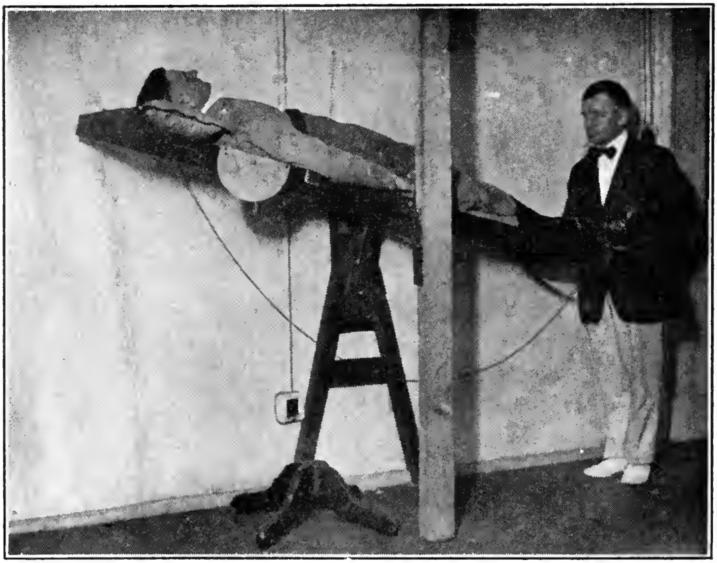

FIG. 1.

movement and determine – 45° and 'Level.' (These last four readings should be taken as rapidly as possible.)

The subject works out the deviation for each position, and in this way learns to what extent one is ordinarily dependent on vision in determining the position of the body.

To use the board as a rotation table for horizontal rotation it is easily lifted from the supports and screwed into the base of an office chair by means of the swivel screw. It is then free to rotate horizontally and is operated by a stout cord fastened to each end of the board.

In the introductory course the rotation table is used as a class demonstration. The subject, eyes closed, lies on the table with his head on a partly inflated air pillow. When the subject detects motion he indicates the direction by raising the hand on the side toward which his head is moving and keeps his hand raised as long as the motion is felt. (*a*) The experimenter at first rotates the table very slowly and the remainder of the class observe that no motion is detected by the subject. (*b*) The rate of motion is increased by the experimenter and then rather suddenly the table is stopped. The subject lowers one hand and raises the other, indicating an apparent rotation in the opposite direction.

(*c*) The experimenter again rotates the table at a uniform rate for several complete revolutions and watches the subject's hands. Then slowly the rate of motion is decreased. Fatigue causes an apparent cessation of movement as shown by the subject dropping his hand, and often a reversal will be indicated on decreasing the rate of motion.

(*d*) Next the subject turns his head so as to rest on one ear and the experimenter rotates the board at a medium velocity. As soon as the subject indicates that the motion has apparently stopped he is directed to turn his head quickly to the other side and note the effect. The direction of stimulation in the fatigued organs is reversed and the sense of motion is taken up again in the same objective direction.

Each student takes notes on his observation of other subjects, and records his own sensations when acting as subject.

II. A SIZE-WEIGHT ILLUSION EXPERIMENT

Numerous size-weight experiments have been offered with varying degrees of success, but they often involve special apparatus. The apparatus used in this experiment (Fig. 2) can be duplicated in any laboratory without extra expense. If any object here mentioned is not available it can easily be replaced by some similar article.

Seventeen articles are included in the experiment; this number may be increased or diminished, as the crucial point is the various classes of objects used rather than their number. The articles may be classed as follows: I. Compact solid, II. flat, III. long slender, IV. cylindrical, V. hollow closed, and VI. hollow open.

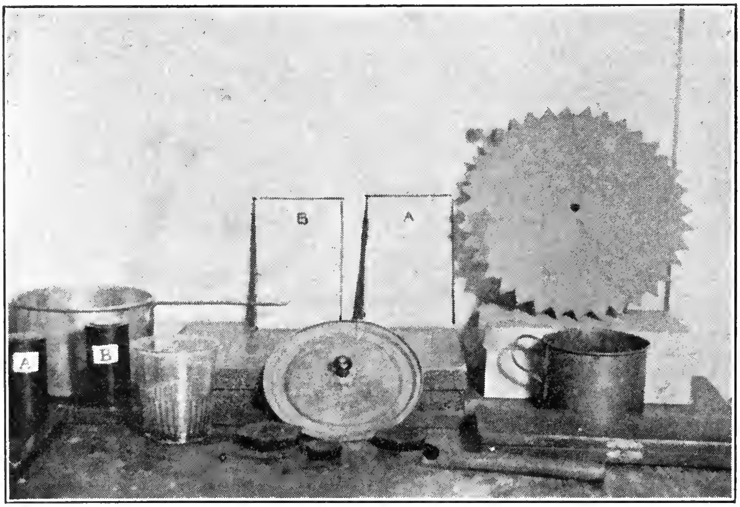

FIG. 2.

Articles Used	Weight, Grams	Class
1. Ordinary sauce pan with handle	381	VI
2. Jastrow cylinder *A*, weighted with shot and cotton	350	IV
3. Milton Bradley pseudoptic box empty	255	V
4. Esthesiometer (Verdin) box	250	V
5. Cold chisel	241	I
6. 200-gram weight	200	I
7. Glass tumbler	200	VI
8. Ruler, anthropometric	140	III

9. Buzz-saw disk of heavy cardboard	140	II
10. Tin cup	101	VI
11. 100-gram weight	100	I
12. Jastrow cylinder *B*	100	IV
13. Sanford envelope *A*	100	II
14. Pasteboard box, Library Bureau	88	V
15. Saucepan lid	85	II
16. Sanford envelope *B*	82	II
17. 50-gram weight	50	I

Procedure.—The articles are distributed at random on a table and the subject is told to arrange them in order of weight by lifting them in pairs. The two objects to be compared are lifted successively in the same hand, with the eyes open. The experimenter may start the subject by placing in proper order the three scale weights (the only objects marked) as an illustration of the problem. These weights will then serve as starting points for further comparisons. After the subject has placed all the articles in order he is allowed to go over the entire set and revise his judgment. When the final order is reached the experimenter gives the subject the correct weight of each article. The subject is instructed to compare his arrangement of the articles with the correct order and to note the relative displacement of each class of articles to right or left, as well as the displacement of the individual articles. No subject acts as experimenter or sees the work of another subject until after he himself has served as subject.

The experiment has been given in the Princeton Laboratory for a number of years and the records of 114 subjects have been kept. In no case has the illusion been absent, while in practically all cases it has been quite pronounced, even though the subject knew the nature of the experiment beforehand. 39 out of the 45 subjects using the articles as here described placed cylinder *A* at the heavy end, while the saucepan was displaced from one to eight places toward the light end and was correctly placed by only three subjects. The pasteboard box, No. 14 in weight order, was placed at the light end by 37 out of 45 subjects and the 50-gram weight was correctly placed by only two subjects. A typical arrangement of the articles is: 2, 5, 6, 1, 11, 9, 13, 4, 17, 6, 16, 10, 15, 8, 12, 3, 14. The individual result in which the illusion was most markedly absent is: 1, 2, 4, 3, 5, 8, 9, 7, 6, 10, 15, 11, 13, 14, 12, 16, 17.

At the same laboratory period the subjects are also given Whipple's Suggestion Blocks.[1]

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[1] Whipple's 'Manual of Mental and Physical Tests,' Test 40. The reference also contains a bibliography of references on size-weight illusions.

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REFLEX SECRETION OF THE HUMAN PAROTID GLAND

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Discovery of the importance of internal secretions for physiological psychology is leading to a widespread interest in the conditions governing secretion. The difficulties of studying the activity of the ductless glands in man are at present almost insurmountable and their relation to the nervous mechanism and to the changing habit-systems of the individual must be judged largely from the conditions in other animals or from analogy with such glands as can be studied directly in man. In an earlier paper I have summarized the work which has been done upon the reflex secretion of the human salivary glands and outlined the problems of psychology which a study of human salivary reactions may help to solve. The chief interest of these glandular reactions for psychology lies in the complex nature of some stimuli to secretion and in the formation of conditioned reflexes (habits of secretion). Before a study of these complex activities can be undertaken it is necessary that we have a fairly complete knowledge of the simpler stimuli influencing secretion. The existing studies of unconditioned secretion in man are too incomplete to give this and are, hence, inadequate as a foundation for work upon conditioned reflexes. The experiments reported in the following pages were undertaken in preparation for a study of the relation of secretion to the more complex activities of the individual and have been directed, primarily, toward the discovery of the stimuli which may influence the secretion of the parotid gland directly or indirectly. While the data obtained is largely physiological, some of the reactions observed seem to rival the complexities of the language mechanism and place the topic of reflex secretion very near the border-line between the sciences of physiology and psychology.

For suggestions concerning the general plan of the work I am indebted to Professor J. B. Watson. In many of the tests, also, in which more than one observer was required and in those in which I have acted as subject he has given generously of his time in conducting the experiments.

TECHNIQUE

The only method of obtaining the secretion of the parotid gland in normal human subjects that has been used by physiologists is that of draining off the saliva from a small canula inserted into the mouth of Stenson's duct. As Ordenstein ('60) states, this method is not very suitable for quantitative work for the reason that one never can be sure that some of the secretion is not leaking past the canula. The expanding walls of the duct also frequently let the canula slip out. A few tests with small canulas quickly convinced me that the method is not practicable for long-continued work.

Fig. 1. Drainage tube used for collecting the secretion from Stenson's duct.

Finally the instrument shown in Fig. 1 was devised and has proved quite satisfactory for the study of the secretion of the parotid gland. It consists of a metal disc 18 mm. in diameter in which two concentric chambers, *a* and *b*, are cut. The inner of these is 10 mm. in diameter and 3 mm. deep; the outer, in the form of a circular groove, is 2 mm. wide and 3 mm. in depth. The two chambers open through the back of the disc into two separate tubes, *c* and *d*, of about 2 mm. bore and 15 cm. length. The tubes are of silver, soft-drawn for greater flexibility, and the disc is heavily silver-plated. In use, the instrument is placed against the inner surface of the cheek so that the central chamber covers the mouth of Stenson's duct and the air is exhausted from the outer chamber by a suction pump, when the disc clings tightly to the cheek. The saliva is free to flow into the central chamber and thence through the tube, *c*, to a suitable measuring device, without constriction of the duct or unusual resistance. When the mouth is closed the tubes lie between the cheek and the upper molars and pass out through the corner of the mouth. The instrument interferes little with talking or eating and may be worn for several hours without discomfort.

For the submaxillary glands a slight modification of the instrument has proved fairly useful. In this the tubes are directed at right angles to the surface of the disc and are bent sharply to pass over the lower incisors. The close proximity of the mouths of Wharton's ducts to the mandible makes it necessary to have the central chamber eccentric so that the suction chamber is reduced to a width of 0.5 mm. on the side next to the mandible. The ducts of the two submaxillary glands open so close together that both must be covered by the instrument and their secretions can not be obtained separately. The great sensitivity of the mucosa of this region and the necessity for depressing the tongue in swallowing make this instrument less satisfactory than that for the parotid.

Various methods of recording the quantity of secretion have been tried, but for the present work the mere counting of the drops as they fall from the drainage tube has seemed sufficiently accurate. The drops vary in size with changes in the rate of secretion and the viscosity of the saliva. With the drainage tube used in the greater part of the work the size of the drops varies from a minimum of 0.06 c.c. to a maximum of 0.07 c.c. with extreme changes in the viscosity of the secretion. For more detailed study of the quantitative relations of the secretion than has been undertaken here measurements of both the quantity and chemical composition of the saliva will be necessary.

THE NORMAL RATE OF SECRETION OF THE PAROTID

Before the rate of secretion of the parotid in the absence of extero-stimulation can be determined the stimulating effect of the method of collecting the saliva must be tested. Where a fistula exists, such stimulation is improbable but where a canula or suction cup is placed in the mouth some excitatory effect is to be expected. The effect of mechanical stimulation from the drainage tube can be tested only indirectly; and excitation from the suction cup described earlier seems ruled out by the following facts. (1) The presence of a large glass rod or other tasteless object in the cheek with the drainage tube does not increase the rate of secretion. (2) Mechanical stimulation of the cheek and gums, where not covered by the drainage tube, does not call out reflex secretion until it becomes strong enough to excite reflex avoiding movements. (3) Manipulation of the drainage tube after adjustment, while it may force out a few drops of secretion from the receiving chamber, does not increase the rate of secretion as measured in successive minutes. From this evidence it seems safe to conclude that the secretion collected from the drainage tube represents fairly the normal secretion of the gland.

To determine the variation in secretion under conditions of as little stimulation as possible I have tested a number of subjects in a quiet state, computing the rate of secretion from a half hour's observation during which the flow remained fairly constant. Measurements were made at different hours in order to control, to some extent, the enteric stimuli. In each case the subject remained quiet, with mouth closed, but without inhibition of the swallowing reflex or of movements adjusting the position of the body. The results of the tests are summarized in Table I. The children tested give a secretion considerably less than the average for adults. The great amount of individual variation makes it impossible to be certain of the sex differences noted by Tuczek ('76).

The data shown in the table can not be considered as showing the rate of secretion in the absence of stimulation. Marked extra-organic stimulation is lacking but the gland is

TABLE I

THE RATE OF SECRETION OF THE LEFT PAROTID GLAND WITHOUT EXTERO-STIMULATION

Subject		Secretion, C.c. per Hour	Conditions of Test
Age	Sex		
26	♂	3.81	11.30 A.M. No food for 12 hours.
		7.74	1.00 P.M. After lunch.
35	♂	1.47	12.00 A.M. Before lunch.
		1.98	1.00 P.M. After lunch.
38	♂	3.47	1.30 P.M. Under alcohol.
		3.24	11.30 A.M. Before lunch.
		6.98	2.00 P.M. After lunch.
24	♂	1.90	9.00 A.M. No breakfast.
		1.30	5.00 P.M. Before dinner.
		3.00	7.00 P.M. After dinner.
25	♀	0.39	11.30 A.M. Before lunch.
		0.65	1.30 P.M. After lunch.
		1.19	10.00 A.M. Two weeks later.
27	♀	4.64	12.00 N. Before lunch.
		6.60	1.00 P.M. After lunch.
8	♂	0.60	3.30 P.M.
12	♀	0.43	4.00 P.M.

subject to almost constant stimulation from the receptors of the mouth and throat. Table II. shows the variations in the rate of secretion in a subject remaining quiet for 20 minutes and partially inhibiting the swallowing reflex. Swallowing is seen to result in an accelerated flow, persisting

TABLE II

VARIATIONS IN THE RATE OF SECRETION DUE TO SWALLOWING

The figures represent the time in seconds intervening between the formation of successive drops. The rapid rate at first is the result of an acid stimulus given to make sure that the instrument was properly adjusted.

Drop	Time	Drop	Time	Drop	Time	Drop	Time
1	—	13	60	25	80	35	55
2	4	14	73	26	120	36	145
3	4	15	152	27	185	37	155
4	9	16	135	28	215	38	155
5	13	17	173	29	209	39	110
6	10	18	204	Shifted position		40	67
7	10	19	125			Swallowed	
8	15	Swallowed		30	88	41	15
Sucked cheeks		20	7	Swallowed		42	15
9	10	21	4	31	19	43	56
10	20	22	16	32	14	44	67
11	31	23	30	33	30	45	83
12	39	24	38	34	20	46	172

for some minutes, and movements of the body also seem to have a slight stimulating effect.[1]

In some subjects, when particularly quiet, the activity of the parotid may cease completely for five or more minutes. Whether this is due to the absence of stimulation or to some inhibitory process is difficult to determine. The data upon paralytic secretion show that after destruction of its nerve supply the submaxillary gland continues to secrete for some months until it finally degenerates. Bradford ('88) failed to obtain this paralytic secretion from the parotid in dogs whence it seems that in the absence of stimulation the parotid does not secrete. No observations on man after section of the nerve supply of these glands are available.

Table III

A Comparison of the Activity of the Right and Left Parotid Glands

The ratio of size of the drops from the right and left sides is 49 to 50.

Stimulus	Total Secretion in Drops		Time Required for the Secretion Recorded (Minutes)
	Right	Left	
No stimulation	29	29	15
Chewing tasteless cube			
Between incisors	13	16	2
Between right molars	15	6	2
Between left molars	4	19	2
Between right and left molars	14	17	2
Sodium chloride, 10% solution			
Uniformly distributed	18	20	2
In right cheek	6	6	1
In left cheek	8	8	1
Hydrochloric acid, 2.50%			
On right side of tongue	19	12	1
On left side of tongue	12	30	1
In right cheek	15	7	1
In left cheek	4	12	1
Uniformly distributed	19	21	1
Tongue scraped with pointed rod			
Right side	6	3	1
Left side	6	10	1

The Relative Activity of the Two Parotid Glands.—Colin ('54) states that in ruminants the parotid glands function alternately, not only when food is being chewed on one side

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[1] From the tests reported on page 484 it seems probable that the apparent excitatory effect of shifting the position really acts by removing the inhibitory effect of muscular tension.

of the mouth but even in the absence of food. In man, for the most part, the glands secrete equally and simultaneously. Drainage tubes were placed over both of Stenson's ducts and the subject was allowed to remain without extero-stimulation for fifteen minutes. The average number of drops per minute secreted by the two glands was: right parotid 1.92; left parotid 1.92 drops. The quantity of secretion from the two glands was thus almost exactly the same (the ratio in the size of the drops from the two drainage tubes being as 49 to 50) and the variations in rate were the same for both. Table III. shows the relative reactions of the two glands to various stimuli. The reactions of the left seem to be somewhat more vigorous than those of the right, which accords with the fact that the subject habitually chews on the left. Bilateral stimulation affects both glands equally. Stimulation of one side of the mouth stimulates the gland of that side to a greater extent than the other. Each gland seems to be most intimately associated with the receptors of its own side.

Reflex Secretion to Stimulation of the Mouth

The direct reflex of the parotid seems to be excited only by stimulation of the receptors within the digestive tract, chiefly those within the mouth. A number of diverse stimuli applied here will excite the reaction and the chief problems are concerned with the distribution of sensitive areas and the quantitative relations of the secretion.

Thermal Sensitivity.—Wulfson,[1] Popielski ('09), and Brunacci ('10) have raised the question of the excitability of the parotid by thermal stimuli. The work of the latter shows excitability only by temperatures above 55 and below 15 degrees Centigrade. These results are confirmed by the following results of my own.

The subject, *L*, whose normal rate of secretion from the left parotid during experiments is about 1 drop per minute, was required to take three sips of water (about 10 c.c.) in quick succession at each of the following temperatures. The number of drops secreted during the three minutes following stimulation was recorded as follows:

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[1] Cited from Babkin ('14).

Temperature of Water	Drops of Saliva
66 degrees	13
61 "	10
55 "	6
52 "	7
41 "	5
36.5 "	7
25 "	7
18 "	7
13 "	10
10 "	8
0 "	10

Increased secretion above that resulting from mechanical stimulation of the mucosa appears only with temperatures above 55 and below 18 degrees. These are well beyond the limits determined by Head and Sherren ('05) for protopathic sensation and suggest the absence of any direct reflex connection between the thermal receptors of the mouth and the parotid glands.

Mechanical Sensitivity.—Mechanical stimulation of the oral mucosa is considered by most investigators to be an efficient excitant of parotid secretion. A careful test of this without interfering stimuli and with the possibility of conditioned reflexes reduced has failed to confirm this view. Table IV. shows the reflex secretion from the left parotid gland obtained from two subjects after mechanical stimulation of the mouth. Stimulation of restricted areas of the mucosa is seen to have little effect except at the back of the tongue and soft palate, where nausea is produced. Some summation of stimuli is apparent in stimulation of the tip of the tongue with a stiff brush, the individual bristles of which gave less violent stimulation than the sharp points of the dividers. In general it seems that very little reflex secretion is obtained from the parotid in response to mechanical stimuli.

The Effects of Chewing.—The experimental literature shows little agreement as to the excitatory effects of chewing movements upon the parotid and no one has attempted an analysis of the relative influence of the different end-organs stimulated when some solid object is chewed. A few tests of the

TABLE IV

REACTIONS TO MECHANICAL STIMULATION OF THE ORAL MUCOSA

The effect of the stimulus is shown by the increase in secretion between the minute before and the minute during stimulation.

Subject	Stimulus	Secretion	
		Before Stimulation	During Stimulation
H....	Blunt glass rod rubbed against		
	lower lip	0	0
	tip of tongue	0	0
	side and back of tongue	1	1
	ditto with tongue-movements	0	1
	soft palate	1	2
L.....	(Stimulus applied for 30 seconds)		
	Blunt ivory rod rubbed against		
	lips	3	2
	tip of tongue	2	5
	middle of tongue	2	5
	back of tongue	2	12
	gums at right	2	5
	hard palate	2	8
	soft palate (nausea)	2	13
	Stimulation with points of dividers		
	lips	2	2
	tip of tongue	2	3
	middle of tongue	2	5
	hard palate	2	5
	Stimulation with stiff brush		
	tip of tongue	3	7
	middle of tongue	2	9
	Stimulation with tasteless cube		
	lower surface of tongue	2	3
	rubbed violently between tongue and left molars	2	5
	between tongue and hard palate	2	4
	Stimulation with 1 c.c. of fine sand		
	rubbed between tongue and hard palate	3	8

effects of chewing convinced me that the adequate stimulus is more complex than any of the previous workers have suggested. A series of experiments was made to distinguish the elements in the constellation of stimuli involved in chewing that are effective in exciting secretion. The tests are summarized in Table V. From the data in this and the preceding table the following conclusions seem justified. (1) Contractions of the muscles of the mandible or changes in the tension of its ligaments do not excite secretion. (2) Pressure on the teeth is, alone, not an effective stimulus. (3) No amount of mechanical stimulation of the mucosa excites as much secretion as light contraction of the teeth upon a foreign object. Chewing does not excite secretion solely by stimulation of the mucosa. (4) Combined mechanical stimulation of the tongue and pressure on the teeth are not so effective as less violent stimulation of the same sort when some object is held between the teeth. (5) Active secretion is excited by the presence of a foreign object held between the teeth.

TABLE V

EXPERIMENTS ON THE STIMULUS FROM CHEWING

The stimulus was continued in each case for one minute. The total number of drops secreted by the left parotid during and after stimulation until the rate returned to normal was found: for comparison the number of drops secreted during an equal time before stimulation is given. The tongue was depressed and unstimulated except where noted.

Stimulus	Secretion			
	Before Stimulation, Minutes		During Stimulation, Minutes	
	1	2	1	2
Chewing movements with contact on normal facets	3	3	3	3
Chewing without contact between teeth	4	5	4	5
Rhythmic contraction of masseters, mouth closed	4	3	3	3
Chewing movements with contact on unused facets	3	3	3	3
Tongue chewed violently between left molars	3	2	10	2
Tongue chewed violently between right molars	3	2	5	2
Foreign object in the mouth				
(For stimulation of mucosa see table IV.)				
Hard rubber cube held lightly between second molars on left	1	1	5	1
Same, gripped tightly	2	2	10	2
Same, chewed lightly	4	1	13	1
Same, chewed violently	2	2	38	2

The reaction seems to be to the total situation rather than to any simple stimulus. The following observation is especially difficult to explain as a reaction to a simple stimulus. With lips drawn back and tongue depressed the subject apposed the first pair of incisors on the left and continued strong rhythmic contractions of the masseters for two minutes, exerting considerable pressure on the teeth and breathing the while through the nose. In the two minutes preceding these movements 3 drops of saliva were secreted; in the two minutes during chewing 4 drops. A disc of hard rubber 2 mm. in thickness was then placed between the same two incisors without contact with the lips or tongue and the chewing rhythm was renewed with the same strength as before. In the two minutes preceding chewing 4 drops of saliva were secreted; in the two minutes during chewing 13 drops. The stimuli added to the local situation by the hard rubber disc were first, tactile, from the particular type of mechanical stimulation transmitted through the teeth, and second, kinæsthetic, from the slightly greater separation of the jaws by the disc. The last is ruled out by further tests with discs of different thickness, to all of which the gland secreted equally. It is not possible to distinguish between the elements of the tactile stimulation at present. Irregular and varying pressure on the teeth is not alone an effective stimulus. The stimulus to secretion seems to be no less complex than that by which knowledge of a foreign object between the teeth is gained.

Colin ('54) and Zebrowski ('05) have found that chewing on one side of the mouth excites chiefly the gland of that side. My observations bring out the additional fact that when chewing occurs on both sides at the same time, less secretion is produced than when one side only is stimulated.

Two equal cubes of hard rubber were used with the drainage tube on the left Stenson's duct. One cube was placed between the left second molars, chewed for two minutes, and then removed. When the rate of secretion returned to normal both cubes were placed between the second molars on the right and left respectively and chewed for two minutes. This was repeated three times with the following results.

Stimulus	Drops of Secretion per Minute
Cube on left	12
Cubes on left and right	5
Cube on left	7
Cubes on left and right	3
Cube on left	8
Cubes on left and right	2
Average, cube on left	9.0
Average, cubes on left and right	3.3

The reduction of secretion with bilateral stimulation must be due either to an inhibitory effect of stimulation of the molars of one side upon the gland of the opposite side, or to some peculiarly effective stimulus arising from asymmetrical movements. Unequal tension on the ligaments of the jaw seemed to fulfill the latter alternative but both passive and active twisting of the jaw are alone without effect so that alterations in the tension of the ligaments seem excluded.

The stimulus offered by chewing has been dwelt upon at some length because the reaction to it illustrates, perhaps better than any other, the complex integration involved in the salivary reflex. Chewing upon a tasteless object excites profuse secretion yet no single element in the complex of stimuli is capable of calling out any marked reaction. The reflex secretion to the stimulus of chewing can not be considered the sum of lesser reactions to the individual components of the stimulus; it is rather a reaction to a particular pattern of stimuli, occurring not at all when any element of the pattern is omitted. This pattern seems no less complex than that by which we recognize the presence of an object between the teeth. The reaction is, however, dissociated from the language mechanism in that it is automatically adjusted to the position of the foreign object in the mouth and is not conditioned by the ordinary inhibitory processes which affect the reactions of striped-muscle.

There is some evidence for an equal complexity in other unconditioned salivary reflexes, particularly in the relations of the stimuli to reflex swallowing and to secretion but the analysis of these is as yet incomplete.

Reactions to Gustatory Stimuli.—A number of extensive studies of reflex secretion to gustatory stimuli have been made. Those of Popielski ('09) and Zebrowski ('05) are the most complete available. Sensitivity to acids, salts, alkalis, fats, and to a large number of food substances has been demonstrated. Popielski has shown that the quantity of secretion elicited by an acid is rather accurately proportional to the degree of ionization of the acid but failed to obtain any uniformity in the reactions to other substances. From tests with sand and water he concluded that the amount of secretion is directly proportional to the quantity of the stimulus-substance used. Zebrowski, from experiments in which his subjects chewed and swallowed measured quantities of bread, concluded that the amount of secretion is proportional to the square-root of the intensity of the stimulus. Brunacci ('10) points out the complexity of the stimulus involved here and thinks that the ratio applies only to the particular case.

The intensity of the reaction to gustatory stimuli is subject to modification by the number and relative excitability of the end-organs stimulated, by the quantity of saliva present in the mouth to dilute the stimulating substance, by the amount of mucus which may be precipitated over the taste buds, and by the length of time during which the stimulus persists, as well as by the quantity and quality of the taste substance placed in the mouth. None of these factors was adequately controlled in Zebrowski's experiments so the significance of his results is questionable. It is particularly difficult to control the distribution of the taste substances in the mouth and the length of time during which the stimulus remains active. In the following tests some attempt was made to do this but with no great success, and the results are at best only approximately correct.

The subject, with the drainage tube on the left Stenson's duct, was warned ten seconds before stimulation and cleared his mouth of saliva as well as possible by swallowing. A measured quantity of solution was then placed in his mouth from a pipette. The subject distributed this over the surface of his tongue by quick movements and held it in his mouth for one minute, then swallowed it, rinsing his mouth immediately with 5 c.c. of water. The volumes of secretion obtained with different quantities of solution are given in Table VI. With only one exception a lesser increase in the secretion follows an increase in the quantity of solution than is to be expected from Zebrowski's formula. The irregularity of the results obtained with chemical stimuli may be due to the fact that with increase in the quantity of solution more taste buds are stimulated. The larger quantities stimulate the base of the tongue and the soft palate which are more

TABLE VI

THE RELATION BETWEEN THE QUANTITY OF SECRETION AND THE QUANTITY OF THE STIMULATING SOLUTION

The second column gives the quantity of secretion during one minute of stimulation and two minutes after removal of the stimulus. The third column gives the quantity expected from Zebrowski's formula.

Stimulus	Secretion in Following Three Minutes (Drops)	Expected Secretion
NaCl 1.00%		
1 c.c.	10	—
2 c.c.	11	14
3 c.c.	12	20
HCl 1.00%		
1 c.c.	17	—
2 c.c.	20	24
3 c.c.	28	29
5 c.c.	28	38
10 c.c.	44	54
HCl 0.25%		
1 c.c.	12.8	—
2 c.c.	16	18
HCl 0.50%		
1 c.c.	17	—
2 c.c.	20.5	24
HCl 2.50%		
1 c.c.	28.7	—
2 c.c.	42.0	40
Cane sugar 10.00%		
1 c.c.	4	—
2 c.c.	4	5.7
3 c.c.	6	6.9
4 c.c.	6	8.0
5 c.c.	4	8.9
10 c.c.	6	12.5
15 c.c.	8	15.5
25 c.c.	7	20.0

sensitive to some than to other chemicals (Table VIII.). Thus the reaction to large quantities of acid is relatively greater than to equal quantities of sugar. The relative excitatory effects of different quantities of solution is doubtless due in large measure to this stimulation of a greater or lesser number of taste buds but other factors also seem to be of importance. The duration of maximal stimulation varies with the quantity of solution since the lesser quantities are diluted more rapidly by saliva than the greater. There is, further, a difference in the mechanical stimulation which, perhaps conditioned by gustatory stimuli, may be effective in exciting secretion by some such complicated mechanism as that involved in the reaction to chewing.

The Relation of the Quantity of Secretion to the Intensity of the Stimulus.—The difficulties in the determination of the excitatory effects of different concentrations of chemical solutions are practically the same as those met with in the study of the effects of different quantities. The experiments on man have shown no more than that strong solutions are more effective salivating agents than weak ones. Accurate results have been made almost impossible by the difficulty in removing the stimulating agent from the tongue at a specified time, the presence of a constant secretion independent of oral stimulation, the persistence of secretion after strong stimulation of the gland, and the possibility of paralysis of the receptors by high concentrations of the solutions.

The last of these is illustrated by the following experiment. Small discs of cotton, 1·cm. in diameter, were saturated with acid solutions and placed, one after the other, over the same group of papillæ on one side of the tongue. The cotton was allowed to remain in place for one minute and the record of drops was taken for this minute only. The same solutions were applied with a brush to a small group of fungiform papillæ. The following quantities of secretion were obtained.

Stimulus	On Side of Tip	On Fungiform Papillæ
HCl 0.25%	4 drops	2 drops
" 0.50%	4 "	3 "
" 1.00%	6 "	2 "
" 2.50%	8 "	3 "
" 5.00%	11 "	6 "
" 7.50%	8 "	5 "
" 10.00%	6 "	4 "

The decrease in the quantities of secretion with solutions of more than 5.0 per cent. is the result of injury to the taste buds. Desquamation of the stimulated areas followed the experiment.

A series of tests was carried out with solutions of hydrochloric acid and of sodium chloride. Other taste substances, quinine sulphate in particular, could not be used because of the difficulty of removing them from the surface of the tongue without prolonged washing. In the tests with salt and acid the following technique was adopted. Thirty seconds before each test 5 c.c. of water were placed in the subject's mouth to dilute the mucus present and the mouth was cleared of saliva as well as possible by swallowing. The remaining procedure was the same as that described on page 473. Each test was delayed until the secretion returned to normal (about 1 drop per minute). Table VII. shows the results of these tests. In no single case were the reactions proportional to the square root of the stimulus intensity. In the majority of cases the range of variation of secretion for different intensities is less than would be expected from Zebrowski's formula. Weber's law of constant increment seems to express the results more exactly, but the control of stimuli is not accurate enough for the computation of any

TABLE VII

THE RELATION OF THE QUANTITY OF SECRETION TO THE INTENSITY OF THE STIMULUS

Drainage tube on the left duct. Stimulus applied for one minute and the total secretion during this and the following two minutes recorded.

Subject	Stimulus	Drops of Secretion in 3 Minutes	Average of — Trials
L.......	HCl 0.25%, 1 c.c.	12.8	5
	" 0.50%, 1 c.c.	17.0	3
	" 1.00%, 1 c.c.	20.0	3
	" 2.50%, 1 c.c.	28.7	4
	" 5.00%, 1 c.c.	40.5	2
	" 0.25%, 2 c.c.	16.0	2
	" 0.50%, 2 c.c.	20.0	2
	" 2.50%, 2 c.c.	42.0	2
	NaCl 0.20%, 1 c.c.	8.0	1
	" 1.00%, 1 c.c.	10.0	1
	" 5.00%, 1 c.c.	14.0	1
	" 25.00%, 1 c.c.	17.0	1
	" 0.20%, 3 c.c.	11.0	1
	" 1.00%, 3 c.c.	14.0	1
	" 5.00%, 3 c.c.	17.0	1
	" 25.00%, 3 c.c.	24.0	1
B.......	HCl 0.50%, 1 c.c.	2.5	2
	" 1.00%, 1 c.c.	7.0	4
	" 2.50%, 1 c.c.	13.0	2
	" 5.00%, 1 c.c.	17.0	2

definite ratio. The technique employed is subject to almost all the objections outlined above and the experiments are reported in their present form only because they suggest that with an adequate technique a very accurate correlation of the intensity of stimulus and reaction may be demonstrated.

Stimulation of Restricted Areas in the Mouth.—The intensity of reaction to stimulation of restricted areas of the mouth has been investigated only by Heyman,[1] working with the dog. His results show a differential sensitivity of different areas of the mucosa to mechanical and chemical stimuli, with considerable variation between different dogs; a variation which corresponds to the different types of distribution of taste buds in man. His original publication is not available but it seems that only gross areas were stimulated.

In man a rather close restriction of stimulation to individual papillæ is possible so that a comparative study of the effect of different chemicals upon the end organs of the same region may be undertaken. Some preliminary tests to this end have been completed, the following technique being used.

The surface of the tongue was mapped out roughly and a regular series of movements of the tongue was adopted in order to control the non-gustatory stimuli. Before each stimulation the mouth was cleared of saliva, the tongue protruded as far as possible without strain, and the surface of the tongue was wiped from tip to base with a soft cotton cloth to erect the papillæ and remove adherent saliva. The taste substance was then applied to individual papillæ in the usual manner with a camel's hair brush. The tongue was next retracted and held depressed so that the taste substance was not spread by contact with the palate. After one minute the mouth was rinsed with 3 c.c. of water and the next test was delayed until the rate of secretion returned to normal.

The most extensive tests were carried out with three points on the dorsal surface of the tongue. These were:

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[1] Cited from Babkin ('14).

(1) a group of three filiform papillæ on the right margin of the tip; (2) two fungiform papillæ just to the right of the mid-dorsal line, 5 cm. from the tip; (3) one circumvallate papilla on the left at the base of the tongue. The quantities of secretion obtained after stimulation of these areas are given in Table VIII. In the tests the rate of secretion before stimulation varies from 1.3 to 2.3 drops per minute. Where the stimulus has little effect the initial rate affects the results considerably and for this reason I have used the increase in rate rather than the absolute secretion in comparing the excitability of different areas. The data are not extensive enough for certain conclusions but suggest that the tip of

TABLE VIII

THE QUANTITY OF SECRETION OBTAINED FROM THE LEFT PAROTID GLAND AFTER STIMULATION OF THE AREAS NUMBERED IN THE TEXT

The average numbers of drops obtained in the two minutes preceding and the two minutes following the application of the stimulus are given. The last column shows the relative increase in secretion resulting from the stimulus in terms of the increase produced by the same stimulus applied to papilla 1. Figures are based on the averages of five trials.*

Stimulus	Papilla	Drops Before Stimulation	Drops After Stimulation	Relative Increase
Water	1	3.7	4.0	1.0
"	2	4.6	5.6	2.6
"	3	3.7	4.7	3.4
HCl 10.00%	1	3.3	8.6	1.0
" "	2	3.2	12.7	1.8
" "	3	3.8	15.8	2.0
Cane sugar 20.00%	1	3.9	4.4	1.0
" " "	2	3.6	5.1	3.2
" " "	3	3.6	6.9	7.4
NaCl 10.00%	1	3.7	6.7	1.0
" "	2	3.2	8.7	2.5
" "	3	4.2	9.4	1.5
Quinine 0.2%	1	3.3	3.6	1.0
" "	2	2.3	3.3	4.8
" "	3	2.6	7.3	20.0
Faradic stimulation	1	1.5	8.5	1.0
" "	2	1.0	4.0	0.47
" "	3	1.5	4.5	0.53

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* In computing these ratios the percentage increase over the initial rate of secretion has been taken as a basis. It is not certain that this method is justified, since there is some evidence that the amount of secretion given in response to a stimulus is independent of the rate of secretion of the gland before the stimulus is applied. The absolute amount of secretion after stimulation may be a more accurate measure of the effectiveness of the stimulus.

the tongue is relatively more sensitive to water, acid, and salt than to other gustatory stimuli; that the base of the tongue is relatively most sensitive to sugar and quinine; that the fungiform papillæ are most readily stimulated by salt. In the case of all but protopathic stimulation, however, the proximal regions of the tongue excite a greater secretion than the distal, irrespective of the quality or intensity of the stimulus. The effect of acid upon other parts of the mouth was tested by the same method. Stimulation of the hard palate, cheeks, lips, and uvula with acid excites no stronger secretion than is obtained with water. The soft palate is considerably more excitable to acid than to water.

THE RATE OF RECOVERY OF THE GLAND FROM EXCITATION

The time required for recovery from stimulation varies with the intensity of stimulation. Table IX. shows the number of drops of saliva from the left parotid obtained in

TABLE IX

THE DURATION OF EXCITATION OF THE PAROTID

The amount of secretion, in drops, obtained during successive minutes after different intensities of stimulation is given. The stimulus was applied for one minute (1 in table) in each case, then rinsed from the tongue with water.

Stimulus	Successive Minutes									Total Secretion after Cessation of Stimulus
	1	2	3	4	5	6	7	8	9	
(Subject B)										
HCl 0.50%	3	0	0	0	1	0	0	—	—	1
" 1.00%	4	0	0	1	0	0	—	—	—	1
" 1.00%	5	2	0	1	0	0	—	—	—	3
" 1.00%	7	2	0	0	1	0	0	—	—	3
" 2.50%	8	4	2	1	1	1	0	—	—	9
(Subject L)										
HCl 0.25%	6	5	1	1	1	1	—	—	—	9
" 0.50%	14	4	2	1	1	2	—	—	—	10
" 2.50%	18	8	3	3	2	1	—	—	—	17
" 5.00%	31	7	3	3	2	3	—	—	—	18
" 0.50% 1 c.c.	12	1	1	3	1	1	—	—	—	7
" 0.50% 2 c.c.	14	5	3	2	2	1	—	—	—	13
" 0.50% 3 c.c.	17	4	2	2	2	1	—	—	—	11
NaCl 0.20%	5	1	2	2	1	0	—	—	—	6
" 1.00%	6	3	1	1	1	1	—	—	—	7
" 5.00%	8	4	2	1	2	1	—	—	—	10
" 25.00%	10	5	2	2	1	1	—	—	—	11
After 1 % HCl continuously for ten minutes	—	9	9	2	2	6	2	2	3	—

successive minutes during and after stimulation with salt and acid. In each test the stimulus solution was held in the mouth for one minute, then the mouth was rinsed with a pipette of water. The time required for the rate of secretion to fall to 1 or fewer drops per minute after the removal of the stimulus was roughly proportional to the intensity of stimulation. As a corollary of this the quantity of secretion after cessation of the stimulus is proportional to the intensity of stimulation (last column, Table IX.). After intense stimulation the heightened secretion may persist for ten or more minutes.

Fatigue of the Unconditioned Reflex

The possibility that fatigue of the direct reflex might interfere seriously with quantitative studies of the secretion led to a series of tests with continued stimulation. These were carried out as follows:

1. Two glass tubes were placed in the mouth in such a way that a constant stream of dilute hydrochloric acid could be directed against the soft palate and allowed to drain off from the tip of the tongue. The stimulating substance could thus be constantly renewed and the question of its dilution eliminated. With 0.25 per cent. acid continuous stimulation was given for ten minutes; with 1.0 per cent. acid for five minutes. The weaker solution excited in ten successive minutes a secretion of 18, 24, 18, 16, 15, 12, 11, 13, 12, 12 drops. The stronger solution gave in five successive minutes 28, 27, 30, 32, 30 drops. In this short time there is little evidence of fatigue. The series with the weaker solution is complicated by the fact that just before the end of the first minute the subject choked and swallowed some acid. The decrease in the rate of secretion following probably represents the recovery from this stimulation of the throat, rather than a fatigue of the reflex.

2. One cubic centimeter of 1.0 per cent. hydrochloric acid was placed in the subject's mouth at the beginning of each minute for 20 successive minutes. The quantities of secretion obtained during this time are shown in the following table.

Minute	Drops of Secretion	Minute	Drops of Secretion
1	10	11	8
2	9	12	8
3	8	13	10
4	9	14	9
5	10	15	9
6	7	16	10
7	8	17	8
8	10	18	10
9	9	19	9
10	9	20	9
Total	89	Total	90

The secretion obtained during the second ten minutes is practically the same as that during the first ten. There is no evidence of fatigue.

3. A 5.0 per cent. solution of sodium chloride was applied in the same way for ten minutes. The quantities of secretion in successive minutes were 6, 7, 6, 5, 5, 6, 5, 5, 5, 6 drops. There is here a reduction of only 7.0 per cent. from the average secretion of the first to that of the second five minutes.

Under the conditions of these tests the direct salivary reflex fatigues very slowly if at all.

Excitation of the Glands by Stimuli from Other Parts of the Digestive Tract

Jänicke ('78) and earlier workers believed that stimulation of the mucosa of the stomach excited salivary secretion. Buff ('88) failed to obtain any increase in secretion by stimulating the walls of the dog's stomach with acid introduced through a fistula and held that reflex excitation of the glands from the stomach does not occur. Ordenstein's data ('60) upon secretion measured for 27 consecutive hours shows some increase in glandular activity after the subject had eaten but the author does not discuss this point.

In the tests reported above upon the rate of secretion without extero-stimulation five subjects were examined immediately before and at intervals ranging from ten to sixty minutes after eating (Table I.). In every case the rate of secretion was greater after the meal than before. Stimulation from particles of food remaining in the mouth was excluded, leaving the alternatives of stimulation from the stomach and intestine or persistent reaction to the long-continued oral stimulation in eating as possible explanations of the heightened rate of secretion. The latter seems to be excluded by the following observation.

The rate of secretion of the first subject recorded in Table I. was tested without extero-stimulation for fifteen minutes and found to be constant at 3.60 c.c. per hour. The subject then chewed 'Zwieback' for fifteen minutes without swallowing any of it, then cleared the mouth of adherent particles and returned to the apparatus. His rate of secretion during the following twenty minutes was found to be 3.60 c.c. per hour, exactly the same as before the oral stimulation. From this it seems that the increase in secretion following a meal is not the result of oral stimulation but is a reflex from some other part of the digestive tract.

The Effect of Olfactory Stimulation

The fact that the odor of food is sufficient to produce salivation has been commented upon frequently (Babkin, '14, p. 24) but I have been unable to find record of any experiments upon the direct reflex to odors in man. Zeliony[1] after ablation of the cerebral lobes of the dog was unable to obtain any glandular reaction to odors; a result which favors the view that the secretion observed by other investigators was a conditioned reflex.

The reactions of the human parotid to odors have been tested with several subjects. The results of tests with three of these are given in Table X. The subjects, with eyes closed, were stimulated for thirty seconds with each of the different odorous substances listed in the table and asked to identify the odors. The stimuli were given at intervals of six or more minutes, and the numbers of drops of saliva obtained during the three minutes preceding and the three minutes following stimulation are compared in the table.

In no case did any marked increase in secretion follow the

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[1] Cited from Babkin ('14).

TABLE X

THE EFFECT OF OLFACTORY STIMULATION UPON THE SECRETION OF THE PAROTID GLAND

The total numbers of drops secreted during the three minutes preceding and the three minutes following stimulation are recorded. The stimuli were applied for 30 seconds.

Odorous Substance	Subject H		Subject B		Subject L	
	Before	After	Before	After	Before	After
Oil of peppermint	2	1	1	2	4	6
“ of bergamot	3	2	......	......	2	3
“ of cloves	......	......	0	0	2	3
“ of wintergreen	......	......	......	......	2	2
Ether	1	2	......	......	4	3
Chloroform	......	......	1	1	......	......
Gum camphor	2	1	1	1	3	2
Amyl alcohol	......	......	2	1	2	6
Ethyl acetate	......	......	1	1	1	2
Ethyl alcohol	......	......	......	......	3	3
Asafetida	1	1	......	......	3	3
Butyric acid	1	2	1	1	2	2
Iodine	......	......	......	......	2	2
Cider vinegar	2	1	2	0	......	......
Fresh toast	2	1	3	1	2	2

stimulation. Amyl alcohol and oil of peppermint seemed to accelerate secretion but both irritate the mucosa of the pharynx, the former producing coughing, and the reaction is probably to this rather than to the odor. The range of odors is fairly broad and the experiments seem to prove that there is no unconditioned reflex secretion to olfactory stimulation.

INHIBITION AND REINFORCEMENT OF SECRETION

The data of Aschenbrandt ('81) upon salivary secretion after irritation of the conjunctival sac and Pawlow's observations ('78) upon inhibition of secretion by injury to the viscera in the dog have been disputed by Buff ('88). He himself lays great stress upon movements of the body as excitants of salivary secretion but gives no experimental evidence in support of his view. The statements of these men and the experiments of Brunacci and DeSanctis ('14), which will be considered later, constitute the only data available upon the indirect modification of the unconditioned reflex.

The series of tests summarized in Table XI. were carried out to test the effect of muscular activity upon the secretion of the parotid. Three types of activity were used, violent effort, rapid movement, and prolonged strain. In each case there was an inhibition of secretion during activity. During activity of this sort movements of the tongue and throat are largely inhibited, but such movements have normally very little effect and it seems improbable that the suspension of secretion is due wholly to the change in the stimuli coming from the mouth. There seems to be an active inhibition of secretion during violent muscular effort.

Table XI

The Influence of Muscular Activity upon the Secretion of the Parotid

Subject L

Stimulus	Average Drops of Secretion per Minute Left parotid	
Subject lifted and held 50 pound weight above his head.		
Before exertion, 4 minutes	0.75	
During “ 2 “	0.00	
After “ 3 “	1.25	
“Setting up exercises”		
Before exertion, 3 minutes	1.25	
During “ 5 “	0.40	
After “ 3 “	1.00	
Arms held extended	Right parotid	Left parotid
Before exertion, 3 minutes	Right 2.0	2.3
During “ 5 “	0.6	0.6
After “ 3 “	1.6	1.6

A further series of tests was carried out with violent protopathic stimulation. The subject's finger-tips were placed against a knife electrode and a faradic current, strong enough to induce profuse perspiration, was directed through the fingers. The rate of secretion with and without faradic stimulation was determined. Brunacci's technique was then adopted, the subject being given 1 c.c. of 0.20 per cent. hydrochloric acid in the first second of each minute during the experiment. The reaction to the acid with and without faradic stimulation was determined. In one instance tickling of the subject's nose and lips was substituted for the faradic stimulation. The results of these tests are shown in Table XII. The protopathic stimulation in every case led to a reduction in the rate of secretion. In test II. (Table XII.) complete recovery from the depression did not take place during the five minutes following the faradization, although the rate of secretion increased slightly with the cessation of stimulation. This is not surprising, however, since the electric stimulation was severe, leaving the subject badly shaken. A ten minute rest, test III., resulted in complete recovery of the normal glandular activity.

TABLE XII

THE EFFECT OF PROTOPATHIC STIMULATION UPON THE RATE OF SECRETION

The rate was determined before, during, and after intense protopathic stimulation continued for the time intervals indicated. In tests II, III, and IV one cubic centimeter of 0.20% HCl was given at the beginning of every minute.

Stimulus	Average Drops per Minute	Duration of Test, Minutes
Test I.		
Normal rate without stimulation	1.00	10
With faradic stimulation of fingers	0.75	10
Test II.		
(One c.c. of HCl at the beginning of each minute)		
Before faradization	12.30	4
During "	8.60	5
After "	9.20	5
Test III.		
Before faradization	11.80	11
During "	9.00	10
Rest for ten minutes		
After faradization	12.10	7
Test IV.		
Lip and nose tickled with a feather		
Before stimulation	12.10	7
During "	7.60	5

Where the acid was given continuously the possibility of fatigue of the direct reflex must be considered. The data given on pages 480–481 show that within the time-limits of this experiment the direct reflex does not fatigue. Moreover, the change in the rate of secretion at the beginning of faradic stimulation was not gradual, as would be expected in fatigue, but abrupt. This is shown by the following figures from tests III. and IV.

	Drops Secreted in Last Three Minutes before Stimulation			Drops Secreted in First Three Minutes during Protopathic Stimulation		
Minutes	1	2	3	1	2	3
Test III	15	12	13	8	9	9
" IV	9	13	13	3	8	8

A serious defect in these experiments arises from the fact that the protopathic stimulus modifies the movements of the mouth and may prevent the usual distribution of the gustatory stimulus. The subject was warned to keep the distribution of the acid on the tongue uniform, but there is no certainty that this was done. Where rather large quantities of concentrated solution are used this source of error is certainly less important than where threshold intensities are used as in the experiments of Brunacci and DeSanctis reviewed below. The results of the tests with protopathic stimulation are consistent and it seems probable that there is an actual inhibition of secretion with very intense stimulation.

THE INFLUENCE OF MENTAL WORK AND EMOTION UPON THE RATE OF SECRETION

Brunacci and DeSanctis ('14) have studied the effect of mental work upon the secretion of the parotid, finding a smaller quantity of secretion during activity than during repose. I have been unable to verify this result and believe that the apparent inhibition resulted from the technique used by these investigators. They stimulated their subjects once per minute with a very dilute solution of acetic acid and recorded the resultant secretion when the subjects were at rest and when they were translating from a foreign language or computing. A diminished secretion during mental activity was interpreted as evidence for inhibition. The importance of the distribution of the stimulus solution in the subject's mouth has been emphasized already. Any distraction may alter this distribution, which depends largely upon the movements of the tongue. The solutions used by Brunacci and DeSanctis were near the threshold intensity and there is no evidence from their experiments that the reduction of secretion was not due to a limited distribution of the stimulus solution when the subjects were occupied.

The following experiment shows an excitatory rather than an inhibitory effect of mental work upon the normal secretion of the gland. The subject was required to square three-place numbers without visual assistance for 20 minutes, to remain quiet for 20 minutes, and then to resume computation for 20 minutes more. The intensity of concentration is assured by the fact that the subject, who had had no experience with this type of computation, returned correct answers to three of twelve problems and made errors of only one figure in five other products. The average rates of secretion obtained were:

During first period of multiplication	1.80	drops	per	minute
During period of rest	1.23	"	"	"
During second period of multiplication	1.88	"	"	"

The rate of secretion during each of the periods was almost constant. An unmistakable increase in secretion resulted from the intense mental application. Similar, but less pronounced results have been obtained with other subjects having a lesser rate of secretion without extero-stimulation. In one case reduced secretion appeared during computation. This subject obviously used the fingers largely in computation while the others seemed to rely wholly upon the speech mechanism. This suggests a possible correlation between the implicit system employed and the effect of mental work upon secretion.

The inhibition of salivary secretion during fear has been noted frequently but there is no definite evidence as to the effects of other types of emotion. Colin ('45) states that salivary secretion is increased during sexual excitement. Von Bechterew ('08, p. 452) says: "Every pleasant sexual excitation is undoubtedly accompanied by salivary secretion; it is questionable only whether this is a reflex from the genitals or (as seems to me more probable) a psychic effect." Since a certain grade of erotic emotion is easily aroused in the laboratory it seemed worth while to test the extent to which the statements of these authors, who give no experimental evidence, can be applied to the parotid gland. For the stimulus to sexual excitement a collection of pornographic literature and pictures was used. The rate of secretion of the subjects while quiet or reading indifferent material was determined. They were then given the erethitic material and the rate of secretion while this was examined was determined. Finally a second determination of the normal rate was made. Table XIII. shows the effect of the erotic emotion. In every instance it resulted in a reduction of the rate of secretion. The exciting effect of the material used has been tested in other experiments and there is no doubt of its erethitic nature. The tests seem extensive enough to establish the inhibitory effect of erotic emotion.

TABLE XIII

THE INHIBITORY EFFECT OF EROTIC EMOTION

The average secretion in drops per minute is recorded. The subjects read indifferent material for 20 minutes or more (first column), then examined the erethitic material for ten minutes (second column).

Subject	Secretion with Indifferent Material. Drops per Min.	Secretion with Erethitic Material. Drops per Min.
B	0.36	0.11
B	0.77	0.00
T	0.80	0.36
M	0.20	0.10

A second effect of this emotion seems to be an almost complete inhibition of the swallowing reflex. The secretion accumulates in the mouth and subjects have been seen to drool saliva while reading the pornographic literature although the secretion of the parotid glands was partially suppressed. This probably accounts for the statements of Colin and von Bechterew, whose observations do not seem to have been made upon isolated glands.

It has not been possible to obtain strong emotion of other types in the laboratory. In one instance, when a long series of tests was made with a disagreeable mixture of hydrochloric acid and sugar, the subject became rebellious and, when the experimenter insisted on continuing the work, angry. The reactions of the parotid became less intense and quite irregular under these conditions.

Conditioned Reflexes Appearing without Training

None of the students of human parotid secretion has been able to demonstrate conditioned reflexes. The reason for this is, perhaps, that they have made no attempt to reproduce in the laboratory the environment in which the subjects were accustomed to obtain food. Early in my own experiments it became evident that no increase in secretion followed the mention of food. As it seemed possible that lack of real hunger was responsible for the absence of secretion the first test for the conditioned reflex was made under rather severe conditions. The subject abstained from food for three days, the drainage tube was then placed over the left Stenson's duct and a list of fifty-three words, fifteen of which were names of foods, the others indifferent, was read to him. The words were given at two-minute intervals and the secretion recorded in drops per minute. The average secretion following the names of foods was 2.00 drops per minute; following the indifferent words it was 2.10 drops per minute. There is absolutely no evidence of a conditioned reflex.

With the sight of food as a stimulus quite different results were obtained. In this test the subject abstained from food for twenty-four hours and was then shown a variety of foods. No increase in secretion appeared with the sight of food. The subject was then allowed to eat a single raw oyster, rather heavily seasoned with paprika. An immediate change in the salivary reaction to the sight of food followed. This may be illustrated in detail by a test with a bar of almond chocolate, which shows the extreme sensitivity of the reflex. In the following table the number of drops of saliva secreted during successive minutes is given on the right, the conditions of stimulation are given on the left.

No extero-stimulation	1
	0
Chocolate placed in subject's hand	4
	3
	4
Subject smelled chocolate	5
Brought chocolate to lips but kept mouth closed	9

Held chocolate at arm's length	4
	3
	3
Told to eat but stopped as chocolate reached lips	7
	6
	2
Chocolate snatched away	0
	0
	3
	2
	2
	1
Chocolate given back and held at arm's length	4
	3
	4
	3
Chocolate snatched away	1
	1
Chocolate given back	4
	2
	4
Chocolate eaten	32
	13

Like results were obtained in other tests but varied with the particular food used. The effects of handling different food-stuffs are shown in Table XIV. Where no secretion was excited by the sight of food, raising the food toward the mouth was sufficient to excite secretion, except in the case of the peppermint which the subject dislikes.

TABLE XIV

EXCITATION OF THE PAROTID BY VISUAL AND OLFACTORY STIMULATION FROM FOOD

The subject was forbidden to eat. Secretion without stimulation was recorded for two minutes, food was then given to the subject and the secretion recorded for four minutes more. The quantities are given in drops.

Stimulus	Successive Minutes					
	Before Stimulation		During Stimulation			
	1	2	3	4	5	6
Peeling orange	2	1	2	1	1	1
Holding plate of oysters	1	1	1	1	1	1
Holding peppermint candy	1	1	0	0		
Peeling banana	1	1	2	3	3	1
Holding chocolate	1	0	4	3	4	4
Holding glass of beer	1	1	3	2	2	1

Tests involving the same movements of hands and mouth as those used with the food but in which inedible substances were held resulted in no increase in secretion, so that it seems quite certain that the results obtained with food substances are due to conditioned reflexes to the visual and olfactory characters of the food.

During the experiments reported on pages 473–477 the formation of conditioned reflexes to the sight of the bottles of acid and other objects used in giving gustatory stimuli was noted but discussion of these must be postponed to a later paper.

Summary

The experiments reported form a preliminary survey of the conditions affecting the secretion of the parotid gland. Only the direct reflex has been considered extensively but the data obtained upon this phase of secretory activity furnishes a basis for an investigation of some of the acquired reflexes of the human autonomic nervous system. While all classes of stimuli which may affect secretion have not been tested it is probable that those employed embrace the ones which are most influential and which might enter as conflicting elements into studies of acquired reflexes. Chemical analysis of the saliva has not been possible but the first tests of conditioned secretion may be restricted advantageously to quantitative experiments which will not be affected seriously by this lack.

The experiments thus far reported seem to justify the following conclusions:

1. Direct reflexes of the parotid gland are excited by mechanical, chemical, and protopathic stimulation of the oral mucosa. Lack of an adequate method of limiting the distribution of chemical stimuli has prevented accurate quantitative experiments.

2. There is probably no direct reflex to thermal stimuli unless they are of protopathic intensity.

3. The secretion produced when a foreign object is chewed involves a specific reaction to a complex group of stimuli.

4. The reaction of the parotid is most intense when the homolateral mucosa is stimulated.

5. The quantity of secretion varies with different chemical stimuli applied to different parts of the mucosa but is usually greatest when the proximal region of the tongue is stimulated.

6. The presence of food in the stomach excites secretion.

7. There is no direct reflex to olfactory or to ordinary visual, auditory, or tactile stimulation.

8. Parotid secretion is partially inhibited by violent muscular activity. Mental work (involving movements of the tongue and throat?) increases the secretion.

9. Erotic emotion reduces the quantity of secretion.

10. Reflex secretion is excited by the sight and odor of food but is conditioned by hunger, by the previous experience of getting food under the conditions of the experiment, and by complex emotional factors which have not been analyzed.

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POSITIVE ACCELERATION IN IMPROVEMENT IN A COMPLEX FUNCTION

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Concerning the general characteristics of the practice curve in most mental and motor functions which lend themselves to psychological investigation, there can be little doubt. Measurements of gain in efficiency have been made in tests which vary from those of distinct simplicity, such as rate of tapping, tossing balls, marking letters, to those which present more complex mental and motor adjustments of which illustrations could be found in typewriting, telegraphic sending and receiving, and mental multiplication.

In studies of such functions, made at various places, in some cases upon numerous individuals, certain peculiarities of the curve are revealed; namely, a rapid initial rise, under the usual conditions of practice, following this a gradual decline in the rate of improvement, until a point is reached when no amount of further practice under normal circumstances will result in any increase in ability. In other words, there is a continuous decrease in the rate of improvement indicating the entrance of the law of diminishing returns, a decrease which is more rapid at the beginning of the work than in its later stages. Some curves obtained in peculiar functions such as tossing balls, which do not appear to follow this law, can be brought under the general case when the basis of scoring is altered, so as to make a gain in score at the end of the practice period more adequately represent the corresponding gain in efficiency. In fact it may be said that to a certain extent one of the best tests of a fair system of scoring in the general run of functions, is to inquire whether the curve obtained shows the general characteristics of a rapid initial .rise followed by negative acceleration in improvement which eventually brings the curve parallel to the time axis.

In this paper attention is directed to the first part of the practice curve which, in the simple and even in the more complex functions that have thus far been investigated, has shown a decreasing rate of improvement from almost the commencement of the work. That is, if we take a simple test, such as tapping, it is possible that while the subject is adapting himself to the work he may for a very short time show a positive acceleration in improvement, but if work is continued this soon gives way to a pronounced negative acceleration which brings the efficiency to its physiological limit. In a simple function this period of adaptation is too brief to admit of measurement, in the case of an adult, but there can be little doubt that it is present and would reveal itself if the gain in efficiency could be measured over short enough periods of time. On account of this rapid adaptation of previous powers to the task in hand, it has been assumed that negative acceleration in improvement is a universal law for all practice curves.

In the case of improvement when very young children are examined, the same type of curve would not necessarily be expected; it might be supposed that for a longer period the more practice the child had, the greater would be the rate of improvement, for each habit acquired in the process would make it easier for the next habit to be gained; in this way a curve indicating a law of increasing returns would be obtained, the curve being convex to the time axis. As practice is continued, the positive acceleration would give way to negative acceleration, and eventually the physiological limit would be reached. That is, the curve would consist of four parts: (1) the first part where there is a positive acceleration in improvement; (2) the second part, a straight line, where the acceleration is zero; (3) the third part, characteristic of the adult curve, where a negative acceleration in improvement is found; (4) the straight part of the curve parallel to the time axis where there is no further improvement.

In the case of adults, if measurements of improvement in suitable functions were made, negative acceleration might prove to be far from a general rule in learning. In the content subjects such as history or geography it is possible that each fact acquired makes it easier to acquire other facts and to fit them into a system, so that day by day, on any reasonable basis of scoring the law of increasing returns would make itself evident. Practice in the acquirement of a foreign language or in acquiring skill in shorthand would probably show similar results. In so far as experiments have been made upon improvement in these complex functions they fail, perhaps owing to the lack of sufficient subjects, to give any indication of positive acceleration, but they reveal a much greater tendency towards zero acceleration than the simpler functions which have been so closely examined in the psychological laboratory.

It would appear then if evidence of the law of increasing returns is to be obtained investigation should be confined to those functions in which improvement extends over very long periods of time, and which initially are very complex. Such a test as Arai[1] underwent, in which mental multiplication of four place numbers by four place numbers was necessary might be repeated using more subjects; for in a test of this type, the period of adaptation to the work must be more prolonged than in a simple test. In fact Arai's scores at the commencement of the work do show a minimum of negative acceleration, to such an extent that if the results are pressed to their limit they might be used as evidence in the case of a single subject of the entrance of very slight positive acceleration in improvement. Such an experiment would be difficult to repeat on account of the time involved, the closeness of the work, and the uselessness of the exercise and the product. It is therefore essential to seek other functions, which may be equally or more complex, but which admit of measurement without the necessity of producing elaborate and artificial conditions.

In the ordinary course of instruction in telegraphic operating a study of the initial rate of improvement would undoubtedly yield very interesting results, but here it is difficult to obtain the necessary facilities. The one study that has been performed upon this subject by Bryan and Harter[2]

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[1] Arai, T., 'Mental Fatigue,' Teachers College, Columbia College Contributions to Education, No. 54.

[2] Bryan, W. L., and Harter, N., 'Studies in the Physiology and Psychology of the Telegraphic Language,' *Psychological Review*, Vol. 4, pp. 27–53, and Vol. 6, pp. 345–375.

does not indicate that with the two individuals involved there was anything more than a prolonged period of steady improvement, that is, the acceleration was zero; certainly it gives no evidence of positive acceleration as a common method of learning. Typewriting is another example of a function which requires a long period of practice before the limit of improvement is reached. Both methods of learning, the sight, and the touch method, have been investigated by Book,[1] Rejall, Hill, and Thorndike.[2] A short description of Book's experiments will suffice to summarize the general conclusions. Using the sight method with two subjects, Book found the usual type of improvement curve, a rapid initial rise, throughout which may be traced the negative acceleration, as is shown by the curves for the two individuals being concave towards the time axis. Using the touch method, which in its initial stages is more difficult, a different type of curve was obtained, in which the rise is less rapid; negative acceleration does not appear pronouncedly, the general rate of improvement being steady for the first ninety hours in the case of one of the subjects. With the other subject, although negative acceleration is less pronounced than when the sight method was used, the curve is distinctly concave towards the time axis. Rejall, Hill and Thorndike obtained the same results. In all these studies the small number of subjects makes general conclusions impossible, for individual differences in improvement are just as great as in static efficiencies.[3] If general laws are to be deduced, or if anything approaching evidence of the methods by which we learn is to result, these experiments must be performed upon numerous subjects under the same conditions, for there is no guarantee that the few subjects already investigated are representative.

Fortunately under the ordinary conditions of school work such a study is possible; typewriting as taught in the various

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[1] Book, W. F., 'The Psychology of Skill with Special Reference to its Acquisition in Typewriting,' University of Montana Publications in Psychology: Bulletin No. 53, Psychological Series No. 1.

[2] Rejall, A. E., and Hill, L. B., and Thorndike, E. L., 'Practice in the Case of Typewriting,' *Pedagogical Seminary*, Vol. XX, No. 4.

[3] Chapman, J. C., 'Individual Differences in Ability and Improvement,' Columbia University Contributions to Education, No. 63.

business schools and high schools of commerce furnishes conditions, which while in no way equal to the exactness of laboratory experiments are yet sufficiently under control to be serviceable. Moreover, the subjects who may be as many as desired, are working under normal conditions, which from the standpoint of the psychology of normal learning is more interesting than the work done under the trying conditions of the psychological laboratory. The nearer an approach is made to measuring improvement under the natural conditions of school, the more likely are the results to give a useful idea of the general nature of changes in the rate of improvement.

The present research was undertaken with a double object: (1) to obtain evidence with a view to a systematic study of the psychology of skill from the objective standpoint. (2) To investigate the rate of improvement of the subjects, in this particular school, using this particular method of instruction, so as to establish norms by which the efficiency of other methods could be judged. The latter investigation has interest from the administrative standpoint; in the commercial schools of the country, various methods of instruction are employed; it is a matter of some importance to see which method yields the most satisfactory results. No attempt will be made in this paper to establish norms, except in so far as the particular curves selected at random indicate a general rate of improvement. The study is not yet complete for the practice of the subjects extends over two school years, and up to the present the writers only have the material of one year's work for the same subjects, the subjects who started typewriting exercise in September, 1914. Only one half of the improvement curve is as yet complete, but the results during the first eighty hours' practice are of sufficient interest to warrant special attention, as they illustrate very clearly important points in the learning process, which have not been emphasized and have even been overlooked in previous studies. In particular we shall consider the over-emphasis which has been laid on negative acceleration in improvement as an almost invariable accompaniment of extended practice.

The general method of the research was to test, each week in the school year, the speed of a mixed class, ages 16–18, in typewriting. The material used consisted of extracts from Addison's 'Essays,' which were selected so as to be of approximately equal difficulty. This material was printed and on a certain day of the week administered to the whole school; the object being to copy as much of the material as possible in five minutes. Care was taken that the various sections who took the test early in the day did not communicate with the later sections. The copy each week was new, though being taken from the same author a similarity in style persisted. The following report from each individual taking part in the test was obtained, in addition to the extract that was typed in the five-minute period: (1) Hours practised since last test, (*a*) in school, (*b*) out of school; (2) gross number of words typed in five minutes; (3) number of errors in (*a*) words, (*b*) punctuation, (*c*) spacing; (4) net score on the basis of one point for each word, less one point for each error; (5) net score on the basis of one point for each word less five points for each error. Before the test started very careful inquiries were made to find whether the subjects had had previous practice in typewriting, if so, for how long, and what method of learning had been used. The results as recorded in this paper are worked out from scores on the basis of report (4), that is upon the total number of words attempted less one word for each error. The experimental errors and individual fluctuations are too great to justify the reduction of the scores to so fine a measure as number of strokes; the time that would be involved in such reduction would yield no adequate return.

When the research has extended over the two years devoted to type-writing there will result improvement curves for about fifty individuals, over a period extending from 20 to about 250 hours of practice. During this period except for rare absences each individual will have been tested at approximately three-hour intervals. This preliminary paper, however, will deal with only two sections, (1) those who, starting with 20 hours' practice, have now had about 80 additional hours; (2) another section, while starting with 75 hours of practice, have now had a further period of 90 hours.

Composite curves, unless the subjects have nearly the same initial ability and rate of improvement, are apt to lead to misunderstanding and to mask important effects which occur in the individual practice curves; for this reason in presenting the data of this study, individual cases will be cited, which may be regarded as typical of the general case.

Group Commencing Work with 20 Hours' Practice

Dealing with the section that had just commenced typewriting, it was not feasible to measure improvement from the very early hours, for in the touch method the attempt is first made to familiarize the learner with particular portions of the keyboard; so that the power of typing a continuous piece would be no test of improvement; in fact, even after ten or fifteen hours there would be a decided tendency if the basis of scoring was one point off for each word in which there was an error, for the score to be negative, that is, in the touch method, unless a certain proficiency is reached, work of any reasonable kind is out of the question. This would not apply to learning by the sight method, where accuracy and a steady improvement is the general rule. Whereas in the touch system certain elementary habit groups have to be formed before the individual can attain a positive score at all; to this extent the scale is inadequate, for a zero score need not represent the zero of typewriting ability. However, any other method of scoring unless we reduce to strokes correct, would suffer from greater disadvantages than this present method. It is perhaps legitimate to assume that an individual who on the average makes a mistake in each word has, from the practical standpoint, something which approaches zero ability in the trait. The scoring on the present basis has the great advantage of being straightforward, and the data are given in such form that the reader can work out the scores penalizing errors in any way that may seem more reasonable; no method of computation will effect the main thesis of the paper.

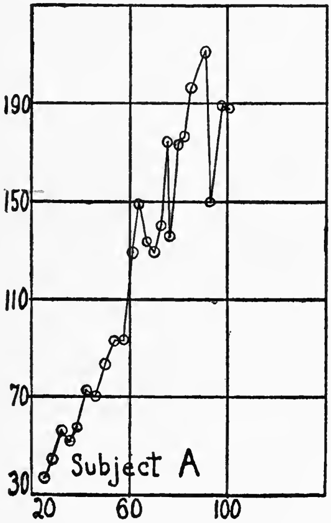
For convenience of presentation we have selected eight individuals at random from the group that has been measured while practicing over a period extending from 20 to 100 hours.

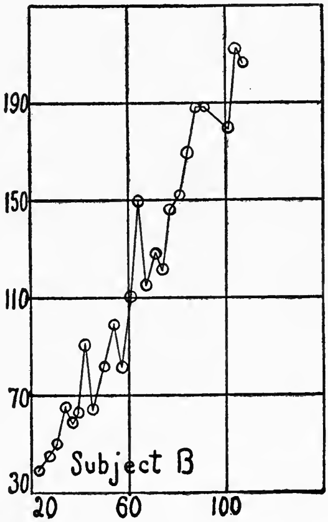
TABLE I

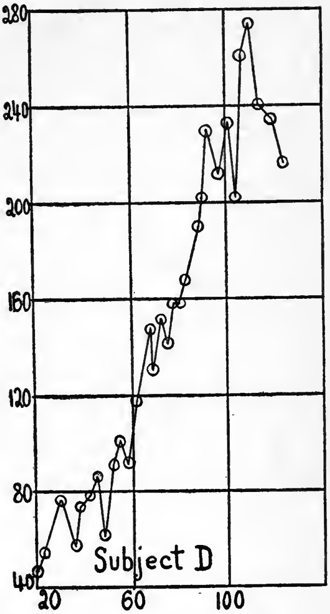
Subject *A*				Subject *B*				Subject *C*				Subject *D*				Subject *E*				Subject *F*				Subject *G*				Subject *H*			
Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score
25	38	1	37	23	43	4	39	20	31	0	31	20	48	2	46	20	20	0	20	20	41	0	41	20	50	6	44	22	43	1	42
28	44	0	44	27	46	1	45	23	35	0	35	23	56	1	55	23	23	0	23	23	53	0	53	24	59	2	57	26	40	0	40
32	56	0	56	30	52	2	50	26	47	1	46	30	79	3	76	29	38	1	37	27	74	3	71	27	71	1	70	29	62	1	61
35	56	4	52	34	66	1	65	29	40	1	39	36	60	2	58	34	32	3	29	30	67	0	67	30	68	3	65	33	85	1	84
38	62	5	57	37	60	1	59	31	56	1	55	38	74	0	74	38	54	3	51	32	82	3	79	32	77	1	76	36	80	2	78
42	75	2	73	39	67	4	63	34	52	1	51	42	79	1	78	42	59	0	59	34	84	2	82	35	89	2	87	38	97	5	92
46	76	6	70	42	93	2	91	36	61	0	61	45	87	1	86	48	68	2	66	36	97	0	97	37	122	3	119	41	112	6	106
50	86	3	83	45	67	3	64	39	55	0	55	48	63	1	62	52	53	2	51	39	82	0	82	40	86	7	79	44	91	3	88
54	99	6	93	50	83	1	82	44	60	0	60	52	92	1	91	58	89	4	85	43	104	0	104	45	91	5	86	49	96	2	94
57	93	0	93	54	102	3	99	47	65	0	65	55	102	1	101	63	119	7	112	47	132	2	130	47	107	3	104	52	124	6	118
61	132	3	129	57	83	1	82	50	65	0	65	58	93	1	92	68	106	4	102	51	128	1	127	50	108	3	105	55	99	4	95
64	152	3	149	61	116	5	111	54	84	0	84	62	119	1	118	73	106	2	104	55	161	3	158	54	130	4	126	59	125	14	111
67	145	12	133	64	155	5	150	57	85	1	84	66	149	1	148	76	133	2	131	58	159	2	157	57	132	4	128	62	134	12	122
70	130	3	127	67	125	10	115	60	90	3	87	69	132	2	130	80	148	2	146	61	149	2	147	59	138	4	134	65	139	8	131
73	142	2	140	71	135	7	128	63	98	0	98	72	152	0	152	84	141	5	136	64	168	1	167	62	139	2	137	68	138	11	127
75	176	2	174	74	131	10	121	66	95	1	94	75	144	2	142	88	123	0	123	68	178	3	175	65	124	2	122	71	137	9	128
76	139	4	135	77	156	10	146	68	98	0	98	78	162	4	158	93	156	3	153	71	185	0	185	68	138	2	136	75	140	7	133
79	175	2	173	81	161	9	152	71	123	0	123	80	162	4	158	97	217	5	212	74	225	3	222	71	145	5	140	78	162	8	154
82	180	4	176	84	177	8	169	74	113	2	111	83	171	3	168	100	186	6	180	77	201	2	199	74	142	2	140	81	152	9	143
85	198	2	196	88	196	8	188	77	118	3	115	86	199	9	190	106	210	1	209	80	231	3	228	77	151	1	150	84	161	6	155
91	216	5	211	91	200	12	188	79	120	2	118	89	204	2	202	112	216	4	212	83	216	1	215	84	151	3	148	87	164	13	151
93	152	2	150	96	250	6	244	82	120	0	120	92	234	4	230	118	188	1	187	86	233	1	232	87	146	4	142	90	170	8	162
98	197	8	189	101	187	8	179	88	186	3	183	97	213	1	212	134	209	2	207	91	211	4	207	92	154	2	152	95	202	7	195
100	195	7	188	104	223	11	212	92	148	2	146	101	236	3	233	...	...	...	...	95	225	4	221	95	171	2	169	98	168	9	159
...	...	...	...	107	215	9	206	95	162	2	160	104	204	4	200	...	...	...	...	99	214	4	210	...	...	...	...	101	177	7	170
...	...	...	...	...	...	...	...	97	161	1	160	107	264	3	261	...	...	...	...	102	229	2	227	...	...	...	...	102	173	8	165
...	...	...	...	...	...	...	...	...	...	...	...	110	279	5	274	...	...	...	...	105	209	5	204	...	...	...	...	105	175	10	165
...	...	...	...	...	...	...	...	...	...	...	...	114	240	0	240	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
...	...	...	...	...	...	...	...	...	...	...	...	119	236	1	235	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
...	...	...	...	...	...	...	...	...	...	...	...	124	218	2	216	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...

In Table I the scores of these individuals are recorded on the basis described previously. The periods of time practiced are reduced to the nearest hour, as any finer scale would not be consistent with the observational error.

The data of these tables are presented in graphical form in the diagrams that follow; where the abscissæ represent the times of practice and the ordinates are the net scores.



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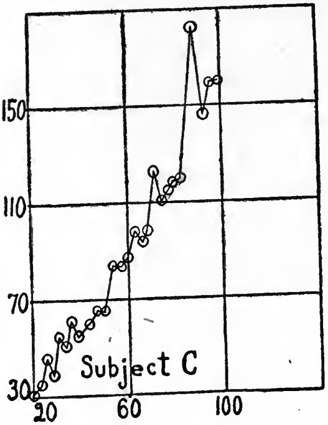
Subject D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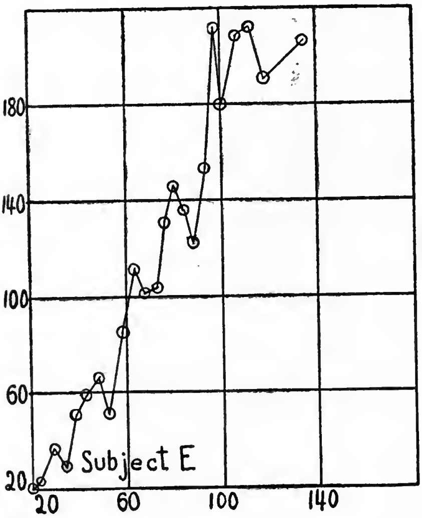
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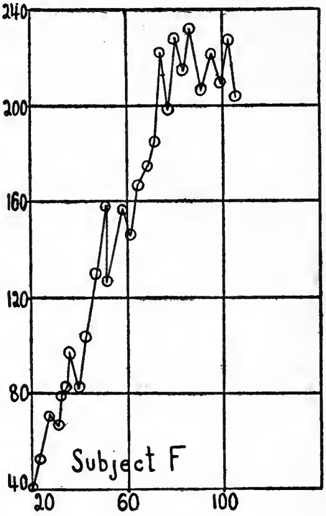
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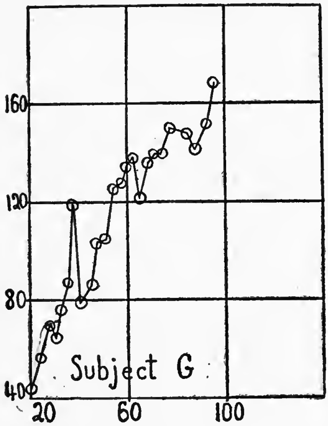
Subject F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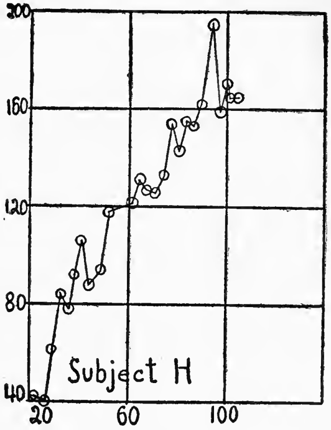
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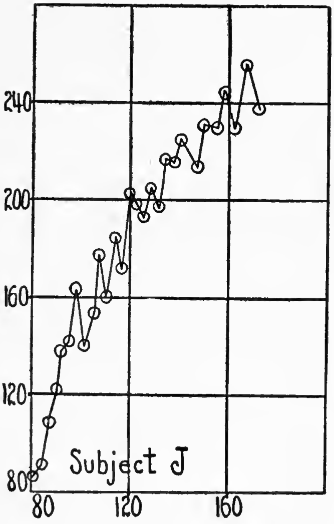
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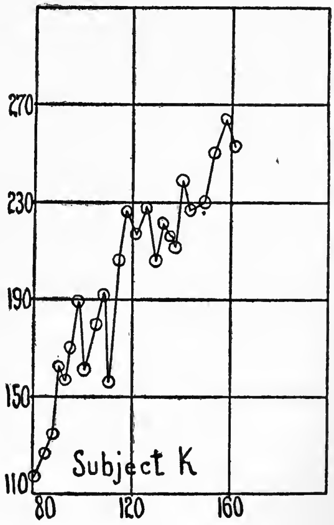
Subject K
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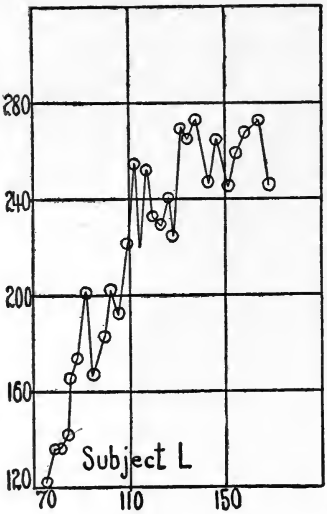

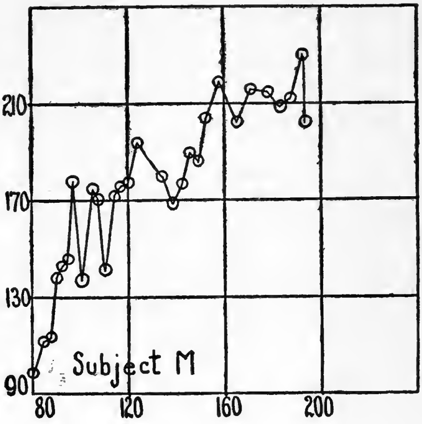

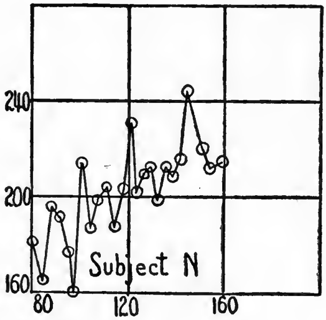

The curves, for individuals measured over the 20 to 100 hours of practice show in the majority of cases striking evidence of the entrance of a positive acceleration in improvement in a motor function trained over a considerable period of time. The records were selected at random and only in the two cases shown (subjects *G* and *H*) was there any evidence of negative acceleration; in one case (subject *F*), a straight line has resulted, showing that the rate of improvement was steady over the period measured. In all the other cases (subjects *A* to *E*) positive acceleration appears. There is no need to enter into the reasons for this acceleration, they have been discussed shortly in the earlier parts of the paper. A priori consideration would indicate that positive acceleration must be obtained in functions of great complexity, and this research reveals the fact that a common method of teaching touch typewriting, the Curry Method, has resulted in a type of improvement which theory would suggest.

Group Commencing Work with 75 Hours' Practice

It is interesting to compare the rate of learning of the earlier section with that of the later section. In the latter case the individuals had had approximately 75 hours of practice; as a consequence we might expect that the part of the curve which corresponds to the period of positive acceleration would have been passed. The curves obtained would therefore be concave to the time axis, as opposed to the earlier curves which are convex in that direction.

In the following table the results of five individuals (subjects *J* to *N*) selected at random are recorded.

The diagrams present the above tables in graphical form.

In these cases it will be seen that the typical adult improvement curves are obtained as the result of the entrance of negative acceleration with extended practice. The shape of these curves is in marked contrast to those of the earlier group where positive acceleration is present.

Summary

In this experiment it has been shown that over a period of practice of 60 hours, it is quite common for positive acceleration in improvement to take place when typewriting by the touch method is studied under school conditions. Measurements were made at weekly intervals of 100 individuals in the typewriting class of a school of commerce. Results selected at random from the group practicing for 20–100 hours show initial positive acceleration whereas for the group practicing 75–165 hours, the ordinary type of practice curve, indicating negative acceleration in improvement, is the general rule. While no claim is made that positive acceleration in improvement, over short periods of time, is universal in adult learning, yet in complex traits it is probably always present to a certain extent and must be considered in framing a psychology of skill.

TABLE II

Subject *J*				Subject *K*				Subject *L*				Subject *M*				Subject *N*			
Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score	Hours Practiced	Gross Score	Errors	Net Score
80	89	3	86	80	119	2	117	75	125	3	122	80	98	0	98	80	188	7	181
84	94	3	91	84	130	3	127	78	141	5	136	84	113	1	112	84	178	10	168
87	110	1	109	87	139	4	135	81	141	4	137	87	115	2	113	88	204	8	196
90	124	2	122	90	164	1	163	84	151	9	142	90	141	3	138	91	202	10	192
92	140	2	138	92	159	2	157	85	167	2	165	92	145	2	143	95	185	8	177
95	144	2	142	94	175	5	170	88	175	1	174	94	152	7	145	97	167	7	160
98	166	3	163	97	190	1	189	91	202	1	201	97	183	5	178	100	220	6	214
101	144	4	140	100	165	4	161	94	170	3	167	100	143	6	137	104	195	8	187
105	155	1	154	105	185	5	180	99	190	7	183	105	178	2	176	107	201	2	199
107	180	2	178	107	197	5	192	102	205	3	202	107	178	7	171	111	209	5	204
110	161	1	160	110	160	4	156	105	195	3	192	110	147	6	141	114	194	7	187
114	188	3	185	114	211	5	206	109	230	8	222	114	175	3	172	118	209	5	204
116	174	2	172	117	230	4	226	112	260	5	255	117	180	4	176	121	235	4	231
119	208	5	203	121	221	4	217	114	225	5	220	120	185	8	177	123	209	7	202
122	201	3	198	125	229	2	227	117	259	7	252	124	195	1	194	126	214	4	210
125	198	5	193	129	210	4	206	120	242	9	233	134	191	11	180	129	220	7	213
128	208	3	205	132	223	2	221	123	236	7	229	138	179	10	169	132	204	5	199
131	200	3	197	135	218	2	216	126	248	7	241	142	182	5	177	135	220	7	213
134	220	3	217	137	215	3	212	128	233	8	225	145	195	5	190	138	215	6	209
137	220	4	216	140	242	3	239	131	275	6	269	149	193	7	186	141	221	5	216
140	228	3	225	143	228	1	227	134	275	10	265	152	212	8	204	144	250	5	245
147	217	3	214	149	233	3	230	137	281	8	273	158	222	3	219	151	220	7	213
150	235	4	231	153	251	1	250	143	252	5	247	165	209	7	202	154	219	6	213
155	234	4	230	158	268	4	264	146	274	9	265	171	225	9	216	159	220	5	215
158	248	3	245	161	256	3	253	151	250	5	245	178	222	7	215	...	...	...	...
162	234	4	230	....	....	...	...	154	264	5	259	183	219	10	209	...	...	...	...
167	257	1	256	....	....	...	...	158	272	4	268	187	216	4	212	...	...	...	...
172	241	3	238	....	....	...	...	163	278	5	273	192	237	7	230	...	...	...	...
....	....	....	....	....	....	...	...	168	250	4	246	197	202	0	202	...	...	...	...

It is with pleasure that we record our obligations to the authorities of the West High School of Commerce of Cleveland for their courtesy in affording facilities for this research, and especially to Mr. James S. Curry for his valuable assistance in the administration of the tests.

THE INFLUENCE OF MENTAL AND PHYSICAL WORK ON THE FORMATION OF JUDGMENTS IN LIFTED WEIGHT EXPERIMENTS

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1. INTRODUCTION.

The controversy concerning the effect of mental and physical work upon cutaneous sensitivity is so well known that we may pass over it without going into detail. Griesbach (14, 15)[1] was the first to claim that the two-point threshold was increased by mental work and hence suggested this as a measure of mental fatigue. This method found many adherents, among whom we may mention Eulenberg (7), Heller (18), Joteyko (19), and Vannod (34). The method was revived by Binet (3) and Schuyten (30, 31, 32), and among the later experimenters who have obtained confirmatory results we may mention some further experiments by Griesbach (16) and investigations by Abelson (1), Bonoff (5), Chabot (6), Sakaki (28) and Schlesinger (29).

The method, however, has been subjected to a great deal of criticism from both theoretical and experimental points of view. Kraepelin (20, 21) was the first critic and he was followed in rapid succession by Leuba (22), Germann (10), Gineff (11) and Bolton (4). Some of the other investigators who discredit the findings of Griesbach and his followers are Meumann (25), Ritter (27) and recently Martyn (24). These

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[1] The numbers in parentheses refer to the references at the end of this paper.

investigators not only failed to confirm the applicability of the method but they presented certain theoretical criticisms of a telling nature. Chief among these, perhaps, is the difficulty of æsthesiometric determinations and the difficulty of exact and constant stimulation even with the newer forms of instruments now in use. Bolton in particular shows the complexity of the problem and the large number of factors involved. The small number of determinations on each individual, made by the earlier investigators, has come in for criticism. Both the matters of the technique and the number of determinations were rectified, to a certain extent, by some of the later investigators who obtained confirmatory results.

Martyn, the most recent investigator, who had employed the method, criticizes it and her own results by asserting that the spatial threshold is not constant enough for use for this purpose. One reason is that the subjects experienced difficulty in maintaining the same judgment with regard to the threshold of 'twoness.' This would seem very probable because there can be no doubt that the old categories of one-point, two-point and doubtful by no means exhaust the psychological possibilities in investigations of this sort. Recently Gates (9) in studying the sensitivity of this modality, had to admit nine categories of judgments, namely: point, point-circle, circle, circle-line, line, line-dumbbell, dumbbell, dumbbell-two-point and two-point.

We cannot decide upon the validity or invalidity of the Griesbach method for measurements of this sort. It might be of interest to note, however, that those experiments in which confirmatory results were obtained were largely of the class experiment type, and that when the investigations were carried into the laboratory, under much more rigidly controlled conditions, the confirmation was largely lacking.

It is interesting to note that the tactual sensitivity is practically the only modality upon which investigators have studied the effect of physical and mental work. Gineff (11) and Meumann (25) both suggested a number of years ago that if work had this effect upon tactual sensitivity, a similar effect should also be apparent for the other modalities.

Some few observations have been made for some of the other modalities, but one searches almost in vain throughout the literature for such studies. Vannod (34) and Binet (3) note a decrease in pain sensitivity as a result of mental work but they do not present their findings with any great degree of conviction. Baur (2) notes a decrease in auditory sensitivity on the basis of a most unsystematic investigation. Graziani (12, 13) finds that mental work has an effect upon both visual and auditory sensitivity. Recently several experiments have been performed with the aid of the Martin method of the measurement of induction shocks. Gruber (17) finds a decrease in the irritability of the dissected muscle as a result of muscular work, which may raise the threshold for contraction as much as 600 *per cent.* Finally Martin, Withington and Putnam (23) find a decrease in the sensitivity to faradic stimulation in human subjects from day to day throughout the week, with a return to the original high point after Sunday's interruption of the weekly routine. This they ascribe to fatigue.

In summary, then, we note that all of the measures of sensitivity which have been employed, were obtained for the stimulus threshold, that intensity of stimulus which could be just perceived. No investigations have been made on the effect of physical and mental work upon the difference thresholds, although these determinations can be made with much greater ease and precision than those for the stimulus thresholds. We also note that most of the investigations have been in the field of cutaneous sensitivity and here the results have been of an exceedingly divergent character. Very few experiments have been performed to ascertain the effect of mental or physical work upon sensory modalities other than the tactual.

2. Methods and Procedure

It, therefore, seemed of value to determine the effects of physical and mental work upon the difference thresholds for some sense modality other than the cutaneous. Experiments in lifted weights at once suggested themselves, inasmuch as with them there has been developed a technique which is extremely well understood and which leaves but little to be desired.

Our materials consisted of five pairs of small brass cylinders which were employed as stimuli. Each pair consisted of a standard stimulus of 100 grams and a comparison stimulus of 88, 92, 96, 100 or 104 grams. These stimuli were placed about the circumference of a table with a revolving top so that the stimuli could be brought successively directly under the hand of the subject. Thus the space errors were eliminated. The time errors were present in the first order, *i. e.*, the standard stimulus was always lifted first. The rate of lifting was regulated by the beats of a metronome so that the time error remained constant. Immediately after the lifting of each comparison weight a judgment was given verbally in terms of the second weight, the subject employing the three categories of 'lighter,' 'equal' and 'heavier,' which were defined in the usual manner. The observed relative frequencies of the different categories upon each of our comparison pairs thus obtained were treated in accordance with the methods of calculation developed by Urban.[1]

We employed five subjects throughout the investigation.[2] All of the subjects were advanced students in experimental psychology at Clark University. Several of these subjects had participated before in lifted weight experiments and were familiar with the particular technique which we employed. All of the subjects were given a preliminary series of 100 liftings of each of our comparison pairs before the beginning of the experiment. This was done so as to eliminate the early stages of progressive practice which Urban (33) and Fernberger (8) have shown to be of considerable effect. After this preliminary series our experiments were divided into two groups.

We first investigated the effect of mental work upon the

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[1] For a more detailed description of the stimuli, the method of lifting and the treatment of results, *cf.* S. W. Fernberger, 'On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli,' *Psychol. Monog.* XIV (Whole No. 61), 1913, 6–11; S. W. Fernberger, 'On the Elimination of the Two Extreme Intensities of the Comparison Stimuli in the Method of Constant Stimuli,' *Psychol. Rev.*, XXI., 1914, 337–340; F. M. Urban, 'Hilfstabellen für die Konstanzmethode,' *Arch. f. d. ges. Psychol.*, XXIV., 1912, 236–243.

[2] My thanks are due to the following, who kindly consented to act as subjects in this investigation: Miss E. Bowman and Messrs. H. R. Crosland, B. Hori, F. J. O'Brien and R. B. Teachout.

judgments to lifted weights. In this series, each daily session consisted of a test period before the mental work was performed, then a period of mental work which was followed by another test period. In the test period before the mental work we obtained, at each session, twenty judgments upon each of our five comparison pairs. These were broken up into two periods of ten judgments upon each pair with a rest period between these groups. The passing of 50 judgments without a rest seemed to be well within the fatigue and attention span of our subjects. Then came a period of one half hour's duration devoted to mental work. This mental work consisted in the reading of a passage of difficult German. The subjects were informed that they would have to report upon the section read and thus we had a check upon the fact that real, conscientious mental work was performed. After this period of one half hour's mental work, we had another test period of similar length to the one before the mental work was performed. Again we obtained 20 judgments upon each of our five comparison pairs with a rest period after the first group of ten judgments. During this rest period, the subjects were asked to report upon the material which they had read. Hence we believe that we obtained rest from the physical work of the actual lifting without any recovery from the effects of the mental work. We always employed a warming-up series before the judgments of each group of liftings were recorded. In this manner we obtained 300 judgments upon each of our five comparison pairs and from each of our five subjects for both of the test periods before and after the period of mental work. Our discussion of the effect of mental work is, therefore, based on a consideration of 15,000 individual judgments. The judgments for each subject were calculated in groups of 50 on each comparison pair, and hence each group of 50 contains the judgments from two and a half days' experimentation.

The criticism may be raised that one half hour's mental work is not a sufficiently extended period to show its effects upon the nervous system, which seems to be exceedingly resistant to effects of this sort. Unfortunately one is bound down, in such an investigation, to the single hour, as one cannot obtain trained subjects for a longer period due to their other academic duties. And it may well be argued that the considerably longer periods employed by other investigators, particularly in the class experiments, are open to grave criticism. We cannot help but believe that class-room work from the early forenoon to the middle of the afternoon will involve a considerable amount of physical as well as mental work. We believe that if one is to test the effects of mental work and have that as pure as possible and with a minimum admixture of physical work, that one must employ relatively short periods with exceedingly intensive mental work. All of our subjects frequently volunteered the information that the period and the materials which we employed gave rise to fatigue sensations.

In our second group of experiments on the effects of physical work upon judgments for lifted weights, it was possible to control the work problem with much greater precision. The liftings were all made with the right hand and hence we believed that if the work would have any effect, it would be better to fatigue the particular group of muscles involved in the lifting. An adaptation of the Mosso ergograph proved to be exceptionally useful for our purpose, as with this we were able to fatigue the muscles of the right hand and forearm—those muscles particularly involved in the lifting. The physical work was carried on in every case to an extreme amount, indeed up to the point of painful fatigue. In the case of one subject, *B*, who had some trouble with the tendon of the second finger of the right hand, we had to abandon the ergograph as the means of obtaining the physical work and we substituted a hand dynamometer and found that continued contractions with this instrument suited our purpose very well.

In this portion of our experiment we had first, a test period before the physical work was performed and then a series of work and test periods following. In the test period before the physical work we obtained, at each session, 25 judgments on each comparison pair. These were divided into five groups of 5 judgments on each comparison pair with a rest period between. Thus by using exceedingly short test periods with very frequent rests we were able to eliminate the factor of physical fatigue entirely in this part of the experiment. After the completion of this first test period, the subject was placed in the ergograph and did physical work until pain entered into the contractions and then he immediately started lifting in the second test series until we obtained five judgments upon each of our comparison pairs. The subject then performed physical work again and another short series of liftings was obtained. Five such alternating work and test periods completed the sitting. In every case, in both the test period before and after the physical work, the subjects were allowed a number of liftings for purposes of warming-up before the judgments were recorded. In this manner we obtained 300 judgments on each of our five comparison pairs and from every subject, for both the test series before and after the physical work. Our discussion of physical work is, therefore, based upon 15,000 individual judgments. For purposes of calculation, these judgments were divided into groups of 50 on each pair of weights, and hence each group of 50 contains the results of two days' experimentation. It may be of interest to note that all of our experiments, with the exception of those for subject *C*, were performed in the forenoon.

3. Results

(*a*) *The Effects of Mental Work*

We shall, of course, treat our results of the series of mental and physical work separately. Let us first consider the results of the effect of mental work on the judgments to lifted weights. The results of our calculations of the relative frequencies of the different categories of judgments, obtained and treated in accordance with the procedure of the method of constant stimuli, will be found in Tables I.–X. All of the tables show a similar arrangement. The first column contains the numbers of the series, *i. e.*, the groups of 50 judgments upon each comparison stimulus in the order in which they were taken. The second and third columns contain the values of h_1 and h_2, the coefficients of precision of the curves of the psychometric functions of the lighter and heavier judgments

Table I

Subject *B*. Before Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.09055	0.10886	94.26	96.68	2.42	95.47
2	0.11971	0.10184	95.89	98.45	2.56	97.17
3	0.08502	0.10363	93.94	98.36	4.42	96.15
4	0.12301	0.13520	94.05	95.92	1.87	94.98
5	0.11711	0.13464	95.86	96.99	1.13	96.42
6	0.12980	0.12406	96.45	98.03	1.58	97.24
Average...	0.11087	0.11804	95.08	97.40	2.33	96.24

Table II

Subject *B*. After Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.10424	0.10436	94.09	97.78	3.69	95.94
2	0.12446	0.12515	95.22	97.81	2.59	96.52
3	0.08028	0.08179	92.00	96.35	4.35	94.18
4	0.10542	0.10311	94.54	95.68	1.14	95.11
5	0.13895	0.14736	94.73	96.42	1.69	95.58
6	0.12116	0.13849	96.19	97.44	1.25	96.82
Average...	0.11242	0.11671	94.46	96.91	2.45	95.69

Table III

Subject *C*. Before Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.10299	0.09393	91.49	94.18	2.69	92.84
2	0.11878	0.10108	90.76	93.38	2.62	92.07
3	0.10476	0.07940	89.36	91.26	1.90	90.31
4	0.13669	0.11818	91.17	93.20	2.03	92.18
5	0.16710	0.13594	92.44	94.08	1.64	93.26
6	0.12146	0.11100	91.52	93.38	1.86	92.45
Average...	0.12530	0.10659	91.12	93.25	2.12	92.18

Table IV

Subject *C*. After Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.09954	0.08442	91.30	93.82	2.52	92.56
2	0.09921	0.11657	90.42	93.46	3.04	91.94
3	0.13530	0.11903	92.22	93.84	1.62	93.03
4	0.13208	0.12068	91.63	93.40	1.77	92.52
5	0.13379	0.11945	92.76	94.56	1.80	93.66
6	0.15220	0.13871	91.45	92.99	1.54	92.22
Average...	0.12535	0.11648	91.63	93.68	2.05	92.66

Table V

Subject *H*. Before Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.14350	0.13550	92.50	99.88	7.38	96.19
2	0.11102	0.09812	90.42	99.50	9.08	94.96
3	0.12468	0.09262	91.58	101.19	9.61	96.38
4	0.13390	0.11796	92.42	101.40	8.98	96.91
5	0.13708	0.11232	92.66	100.90	8.24	96.78
6	0.15555	0.12410	91.10	100.64	9.54	95.87
Average...	0.13429	0.11344	91.78	100.58	8.80	96.18

Table VI

Subject *H*. After Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.13170	0.12328	91.63	99.55	7.92	95.59
2	0.09925	0.11106	89.32	99.29	9.97	94.30
3	0.11804	0.08389	91.99	101.70	9.71	96.84
4	0.13244	0.12711	92.65	101.03	8.38	96.84
5	0.11716	0.11327	90.83	100.81	9.98	95.82
6	0.15146	0.13188	91.20	101.03	9.83	96.12
Average...	0.12501	0.11508	91.27	100.57	9.30	95.92

Table VII

Subject *O*. Before Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.11417	0.13127	96.44	98.52	2.08	97.48
2	0.15103	0.14150	96.03	98.26	2.23	97.14
3	0.11691	0.14226	95.55	99.18	3.63	97.36
4	0.12280	0.12378	94.96	98.39	3.43	96.68
5	0.11253	0.09650	94.83	98.93	4.10	96.88
6	0.15199	0.11070	93.90	96.42	2.52	95.16
Average...	0.12824	0.12434	95.28	98.28	3.00	96.78

Table VIII

Subject *O*. After Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.12811	0.11191	96.29	98.29	2.00	97.29
2	0.11300	0.12860	97.26	99.24	1.98	98.25
3	0.13500	0.11968	96.55	99.74	3.19	98.14
4	0.10271	0.11240	94.74	98.38	3.64	96.56
5	0.13033	0.12967	94.10	96.93	2.83	95.52
6	0.11534	0.12262	94.41	96.22	1.81	95.32
Average...	0.12075	0.12081	95.56	98.13	2.58	96.85

Table IX

Subject *T*. Before Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.12385	0.09169	92.80	99.13	6.33	95.96
2	0.11902	0.09120	93.88	101.78	7.90	97.83
3	0.11656	0.12708	93.77	99.59	5.82	96.68
4	0.12498	0.16431	93.57	97.78	4.21	95.68
5	0.12784	0.14392	92.87	98.25	5.38	95.56
6	0.13974	0.12991	92.26	97.65	5.39	94.96
Average...	0.12533	0.12468	93.19	99.03	5.84	96.11

Table X

Subject *T*. After Mental Work

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.09755	0.10914	92.31	99.14	6.83	95.72
2	0.12610	0.15518	93.46	100.21	6.75	96.84
3	0.13511	0.13000	93.87	98.54	4.67	96.20
4	0.12684	0.14451	93.41	98.29	4.88	95.85
5	0.13512	0.15347	93.10	98.68	5.58	95.89
6	0.14904	0.13406	92.07	98.46	6.39	95.26
Average...	0.12829	0.13773	93.04	98.89	5.85	95.96

respectively. The next two columns, labeled *D* and *I*, give the values of the lower and upper thresholds respectively. The next columns give the values of the interval of uncertainty, *I–D*. It will be remembered that one half of this interval is the Threshold of Volkmann, which is recognized as the measure of the sensitivity of the subject. Finally, the last columns contain the values of the point of subjective equality. In each row will be found the values obtained from a treatment of the relative frequencies of the series indicated in the first column. The bottom row gives the average of the values for all of the series.

Two tables are given to the results of each subject. The tables with odd numbers (I., III., etc.) contain the constants of the psychometric functions and the values which follow from them for the series before mental work. The tables with the even numbers (II., IV., etc.) contain the similar values for the series after mental work.

The results for Subject *B* (Tables I. and II.) show that, on the average for all of the results, there is exceedingly little change in the size of the values of the coefficients of precision for either the lighter or heavier judgments (h_1 and h_2). The coefficient of precision is, on the whole, slightly greater for the less judgments after mental work, but this holds true in only three of the six individual groups. On the other hand, the coefficient of precision of the heavier judgments is slightly smaller after mental work, but this again is true in only three cases of the six groups. Both thresholds (*D* and *I*) for this subject assume slightly lower values as a result of the mental work. For both the thresholds in the direction of decrease and increase this is true in five out of the six cases. But the two thresholds decrease in size as a result of the mental work to approximately equal amounts and hence the interval of uncertainty is practically the same for the average of all of the results. For the individual groups, the interval of uncertainty is larger in two cases, smaller in two cases and in two cases it remains practically the same. The point of subjective equality is, on the average, over one half a gram lower after mental work, but for the individual groups, this value is lower in only four cases and higher in two.

The results for Subject *C* (Tables III. and IV.) show tendencies as a result of mental work that are somewhat comparable to those of Subject *B*, just considered. The average of the coefficients of precision for the lighter judgments is almost identical in the results before and after mental work. In the individual groups, this value is higher three times and lower three times after the mental work, as compared with the corresponding values before the work was performed. On the average, the coefficient of precision for the heavier judgments is greater after the mental work but this is true for only four of the six individual groups. Both of the thresholds in the directions of decrease and increase are higher after mental work than before the work was performed. But this relation is by no means constant. For the threshold in the direction of decrease, the values for the individual groups are larger in three cases and smaller in three after the mental work; while for the threshold in the direction of increase, the values after the mental work are higher in four groups out of the six. The values of the interval of uncertainty before and after the work for this subject, are almost identical, the value after the work being, on the average, only 0.07 gram less than the corresponding value before, the values for the individual groups being smaller four times and larger twice after the mental work. The point of subjective equality is nearly half a gram larger after mental work than before, on the average, but three of the individual groups show values that are smaller than the corresponding values before mental work.

Subject *H* (Tables V. and VI.) shows values, as a result of the mental work, which are very different from those of the two subjects which we have discussed just above. For this subject, the coefficient of precision of the lighter judgments is less on the average after the mental work, and, indeed, the values are less in every one of the six individual groups. The average coefficient of precision for the heavier judgments is slightly greater after the mental work, but is greater in only three of the six individual groups. The threshold in the direction of decrease is lower, on the average, after the mental work by a little over half a gram, but again is less in only three of the six individual groups. The average values of the threshold in the direction of increase are almost identical, being less in three cases, larger in two and almost equal in one of the individual groups. Inasmuch as the average upper threshold remains constant while the lower threshold assumes a lower value, the size of the interval of uncertainty after the mental work is greater than before the work was performed, by exactly half a gram. The individual values of the interval of uncertainty are greater after mental work in five out of six cases. The average point of subjective equality is slightly lower after mental work, being lower in four of the six groups.

The results of Subject *O* (Tables VII. and VIII.) are again different from those of any of the three subjects already discussed. For this subject, the average values of the coefficients of precision for both the lighter and heavier judgments assume lower values after the mental work, considerably lower in the case of the coefficient of precision for the lighter judgments.

However, the values for the individual groups in this case are lower in only three of the six cases, while for the coefficient for the heavier judgments, the values are lower in four of the six cases after mental work. The average value of the lower threshold after mental work is slightly higher than its corresponding value, but is higher in only three of the six individual groups. The upper threshold, on the other hand, after mental work assumes a slightly lower value than its corresponding quantity, but is lower in only three cases, higher in two, with one case almost identical for the individual groups. Hence the average value of the interval of uncertainty for this subject is over four tenths of a gram less after the mental work, and is less in five of the six groups. The average values of the point of subjective equality are almost identical; being 0.06 gram higher after mental work, but being higher in only three of the six groups.

Subject *T* (Tables IX. and X.) shows values which again are different from the others. For this subject, the average values of the coefficients of precision for both the lighter and heavier judgments are larger after the mental work; very considerably larger in the case for the coefficient for the heavier judgments. For the individual groups, the coefficient of precision for the lighter judgments is larger, after mental work, in five cases out of six; while that for the heavier judgments is larger, after mental work, in five cases out of six. Both thresholds assume, on the average, slightly lower values after the mental work; that in the direction of decrease being lower in four cases out of six; that in the direction of increase being lower in only two cases out of six. The average values of the interval of uncertainty before and after mental work are almost identical; the size of the interval after the work being only 0.01 gram larger, but being larger in four of the six individual groups. The average value of the point of subjective equality after the mental work is slightly lower than the corresponding value before the work was performed, but it is lower in only three of the individual groups.

One is impressed, we believe, by the extreme variability of the apparent effect of mental work as shown by the comparison of the values of the different groups of 50 judgments on each comparison pair for the same subjects. In the case of very few values are any general tendencies of increase or decrease apparent for any subject. In only one case (Subject *H*, h_1) are all of the values after mental work either greater or less than those before the work was performed in all of the six individual groups. In only five cases are there tendencies strong enough to show a regular increase or decrease in five of the six groups. These are: Subject *B*, the two thresholds decrease after mental work; Subject *H*, the interval of uncertainty is greater, and in Subject *O*, it is less after mental work; and in the case of Subject *T*, the coefficient of precision for the lighter judgments is less. In all of the other averages for the values for each subject, the quantities contain at least two individual group readings which are in opposition to the average.

We find not only this great variability of the apparent effect of mental work upon the values for each individual subject, but also when we compare the average values for all of the subjects. These average values are given in Tables XI. and XII. These tables show the same arrangements as the others,

TABLE XI

AVERAGE OF ALL SUBJECTS. BEFORE MENTAL WORK

Subjects	h_1	h_2	*D*	*I*	Interval of Uncertainty	Point of Subjective Equality
B	0.11087	0.11804	95.08	97.40	2.33	96.24
C	0.12530	0.10659	91.12	93.25	2.12	92.18
H	0.13429	0.11344	91.78	100.58	8.80	96.18
O	0.12824	0.12434	95.28	98.28	3.00	96.78
T	0.12533	0.12468	93.19	99.03	5.84	96.11
Average...	0.12481	0.11742	93.29	97.71	4.42	95.50

TABLE XII

AVERAGE OF ALL SUBJECTS. AFTER MENTAL WORK

Subjects	h_1	h_2	*D*	*I*	Interval of Uncertainty	Point of Subjective Equality
B	0.11242	0.11671	94.46	96.91	2.45	95.69
C	0.12535	0.11648	91.63	93.68	2.05	92.66
H	0.12501	0.11508	91.27	100.57	9.30	95.92
O	0.12075	0.12081	95.56	98.13	2.58	96.85
T	0.12829	0.13773	93.04	98.89	5.85	95.96
Average...	0.12236	0.12136	93.19	97.64	4.45	95.42

except that the values in each row are the average values for each subject, whose initial is found in the first column. The bottom row contains the averages of these, or the average of all of the judgments obtained from this part of the experiment. Table XI. contains the values obtained before and Table XII. those obtained after the mental work.

An examination of these tables reveals the fact that the coefficient of precision for the lighter judgments assumes a larger value, after the mental work, for two subjects, smaller for two and in one case the values are almost identical. The average for all of the subjects gives values which are very similar, but which are slightly smaller after the mental work. In the case of the coefficient of precision for the heavier judgments, the values after mental work are larger in the case of three subjects and smaller in the case of the other two; on the average this value is somewhat larger after the work for the entire series of experiments. The threshold in the direction of decrease is slightly smaller after the mental work but only to the extent of 0.10 gram. Three subjects give results which conform to this average while two do not. The threshold in the direction of increase is again slightly smaller, on the average, after mental work; the difference being only 0.07 gram. One subject gives results which do not conform, three show such little variation that the values may be considered identical while the results of the fifth subject conform to the average. The average values for all of the subjects for the interval of uncertainty before and after mental work are almost astoundingly similar; the interval after mental work being only 0.03 gram larger than the corresponding value. Two subjects conform to this average, two subjects do not and one gives practically identical values. The total average of the point of subjective equality after mental work is slightly lower than the corresponding value before work, the difference being only 0.08 gram. Three subjects conform to this average and two do not.

We have pointed out the extreme variability in the results for each individual in the apparent effects of mental work upon the judgments to lifted weights, and we have also pointed out a similar extreme variability in the comparison of the results of the different subjects. We also wish to emphasize the extremely great similarity of the average results for all of the subjects. This coincidence of the total average results for the values before and after mental work is particularly marked when we consider both the upper and lower thesholds, the interval of uncertainty and the point of subjective equality. Great variability has always been found in lifted weight experiments and we believe that the divergence of the results for each subject may be explained in terms of chance variations. When we consider the final averages, we must conclude that intensive mental work, for a short period, but work which was intensive and prolonged enough to produce fatigue sensations, has no apparent effect upon the judgments in lifted weight experiments.

(*b*) *The Effects of Physical Work*

Let us now turn to a discussion of our results on the effects of physical work on the judgments to lifted weights. These

TABLE XIII

SUBJECT *B*. BEFORE PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.13636	0.14526	97.27	97.94	0.67	97.60
2	0.13418	0.14172	96.62	98.29	1.67	97.46
3	0.11666	0.14800	96.30	99.23	2.93	97.76
4	0.13227	0.12778	97.37	98.85	1.48	98.11
5	0.10690	0.11453	96.83	97.89	1.06	97.36
6	0.12142	0.11756	98.24	98.71	0.47	98.48
Average...	0.12463	0.13248	97.10	98.48	1.38	97.80

TABLE XIV

SUBJECT *B*. AFTER PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.12054	0.12131	94.09	95.39	1.30	94.74
2	0.13296	0.12288	93.16	95.78	2.62	94.47
3	0.09924	0.11286	90.94	95.77	4.83	93.36
4	0.11694	0.12350	92.93	95.31	2.38	94.12
5	0.13052	0.12771	92.81	94.64	1.83	93.72
6	0.11168	0.11550	94.11	95.30	1.19	94.70
Average...	0.11865	0.12063	93.01	95.36	2.36	94.18

TABLE XV

SUBJECT *C*. BEFORE PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.12348	0.11851	91.85	93.58	1.73	92.72
2	0.11800	0.12964	91.17	92.90	1.73	92.04
3	0.13254	0.11846	92.22	93.99	1.77	93.10
4	0.15145	0.14368	92.27	95.27	3.00	93.77
5	0.14538	0.14870	94.51	96.89	2.38	95.70
6	0.14349	0.13071	94.70	96.90	2.20	95.80
Average...	0.13573	0.13162	92.79	94.92	2.14	93.86

TABLE XVI

SUBJECT *C*. AFTER PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.12705	0.09996	89.57	91.18	1.61	90.38
2	0.13432	0.10337	88.52	90.49	1.97	89.50
3	0.12487	0.10422	88.75	91.65	2.90	90.20
4	0.13480	0.11038	89.55	93.39	3.84	91.47
5	0.12012	0.12333	90.65	93.86	3.21	92.26
6	0.12802	0.10395	90.72	93.82	3.10	92.27
Average...	0.12820	0.10754	89.63	92.40	2.77	91.01

TABLE XVII

SUBJECT *H*. BEFORE PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.12380	0.14242	92.84	99.80	6.96	96.32
2	0.09811	0.11658	91.45	99.90	8.45	95.68
3	0.09594	0.12205	92.97	100.93	7.96	96.95
4	0.11776	0.13025	94.13	101.73	7.60	97.93
5	0.13010	0.14963	94.26	100.30	6.04	97.28
6	0.13092	0.13979	94.57	100.48	5.91	97.52
Average...	0.11610	0.13345	93.37	100.52	7.15	96.95

TABLE XVIII

SUBJECT *H*. AFTER PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.15748	0.13562	90.04	98.21	8.17	94.12
2	0.13603	0.12253	89.41	98.58	9.17	94.00
3	0.12114	0.09264	89.82	98.88	9.06	94.35
4	0.11706	0.10188	90.36	99.52	9.16	94.94
5	0.13048	0.11251	91.34	99.04	7.70	95.19
6	0.14048	0.11513	90.74	98.42	7.68	94.58
Average...	0.13378	0.11338	90.28	98.78	8.49	94.53

TABLE XIX

SUBJECT *O*. BEFORE PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.11384	0.13896	94.91	96.83	1.92	95.87
2	0.12345	0.14300	95.69	98.39	2.70	97.04
3	0.14334	0.14892	97.36	99.06	1.70	98.21
4	0.10154	0.12504	98.10	100.29	2.19	99.20
5	0.12512	0.12482	95.75	98.07	2.32	96.91
6	0.13998	0.14207	96.93	98.71	1.78	97.82
Average...	0.12454	0.13714	96.46	98.56	2.10	97.51

TABLE XX

SUBJECT *O*. AFTER PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.12629	0.12700	94.32	95.51	1.19	94.92
2	0.11889	0.14745	93.96	97.22	3.26	95.59
3	0.14872	0.15246	95.34	97.22	1.88	96.28
4	0.14464	0.12841	96.08	98.62	2.54	97.35
5	0.12183	0.12834	93.36	96.31	2.95	94.84
6	0.12630	0.13734	93.98	96.44	2.46	95.21
Average...	0.13111	0.13683	94.51	96.89	2.38	95.70

TABLE XXI

SUBJECT *T*. BEFORE PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.14418	0.13678	94.92	98.52	3.60	96.72
2	0.13720	0.15238	95.97	98.89	2.92	97.43
3	0.11700	0.14766	95.60	98.98	3.38	97.29
4	0.11454	0.12721	95.11	98.96	3.85	97.04
5	0.11680	0.13795	95.89	98.89	3.00	97.39
6	0.11808	0.12788	94.98	99.14	4.16	97.06
Average...	0.12463	0.13831	95.41	98.90	3.48	97.16

TABLE XXII

SUBJECT *T*. AFTER PHYSICAL WORK

Series	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
1	0.12796	0.13186	91.64	97.02	5.38	94.33
2	0.15804	0.13962	92.00	96.71	4.71	94.36
3	0.13068	0.13192	91.55	96.38	4.83	93.96
4	0.12498	0.12212	91.96	97.03	5.07	94.50
5	0.11493	0.12847	91.79	96.97	5.18	94.38
6	0.10940	0.11967	90.89	96.57	5.68	93.73
Average...	0.12766	0.12894	91.64	96.78	5.14	94.21

results are given in Tables XIII.–XXII. These tables have a similar arrangement to Tables I.–X. Two tables are again given to a presentation of the results of each subject, tables with the odd numbers (XIII., XV., etc.) containing the results for the series before physical work, and the tables with the even numbers (XIV., XVI., etc.) containing the results for the series after physical work.

An examination of these tables shows that physical work has the same general effect upon the values of the thresholds, the interval of uncertainty and the point of subjective equality for all of our subjects. The tendency in all of these cases is always in the same direction,—the variations being only in degree. Hence we will treat these values later as a group.

Let us first turn our attention to a consideration of the coefficients of precision of the lighter judgments. For Subject *B* the average value is less after the physical work than before it; this is true in five cases out of the six individual groups. The same tendency is true for Subject *C* with four of the six groups confirming the average. The other three subjects, however, give opposite results, as the coefficient of precision of the lighter judgments is larger on the average after physical work than before it. In the case of Subject *H* five of the six groups confirm the average; for Subjects *O* and *T* three of the six groups.

For all of our subjects the value of the coefficient of precision of the heavier judgments is smaller after the physical work on the average. For Subject *B*, this average is considerably smaller, five of the six groups confirming the average. In the case of Subject *C*, the average coefficient is very much smaller after the physical work and all of the individual groups show the same tendency. Five of the six groups of Subject *H* agree with the average which is considerably smaller after physical work. For Subject *O*, the coefficient of precision of the heavier judgments after physical work is slightly smaller, but there is only an inconsiderable difference, only two groups agreeing with the average. The values after physical work for Subject *T* are somewhat lower with all six groups showing a tendency in the same direction as the average.

The effects of physical work become very apparent upon the values of the thresholds in the direction of decrease. In the case of every individual group and for every subject, the lower threshold after physical work assumes a lower value than the corresponding quantity before the work was performed. The amount of decrease is by no means constant for the different groups and varies to a certain extent for the different subjects. The average differences in the different cases are: for Subject *B*—4.09 grams; for Subject *C*—3.16 grams; for Subject *H*—3.09 grams; for Subject *O*—1.95 grams and for Subject *T*—3.77 grams.

The effects of physical work upon the upper thresholds are in the same direction and equally as apparent. The thresholds in the direction of increase are smaller after physical work than before it and this relation holds true for every individual series for every subject. The degree of this effect upon the threshold varies not only for the same subject from series to series, but also for the different subjects. The average differences in these cases are: for Subject *B*—3.12 grams; for Subject *C*—2.52 grams; for Subject *H*—1.74 grams; for Subject *O*—1.67 grams and for Subject *T*—2.12 grams.

Inasmuch as the differences between the sizes of the lower threshold before and after physical work is greater than those of the upper threshold, there is a tendency for the two thresholds to draw further apart and hence to increase the size of the interval of uncertainty. This interval is greater on the average, after physical work for every subject and, indeed, for every individual group with the exception of two (Subject *C*, Series 1 and Subject *O*, Series 1). The amount of difference again varies for different groups for the same subjects and for the different subjects. The average amount of increase of the size of the interval of uncertainty for the different subjects is: for Subject *B*—0.98 gram; for Subject *C*—0.63 gram; for Subject *H*—1.34 grams; for Subject *O*—0.28 gram and for Subject *T*—1.66 grams.

Inasmuch as both the thresholds in the directions of decrease and increase assume lower values after physical work, there is a marked lowering of the point of subjective equality. This decrease in size of the point of subjective equality, as a result of physical work, is true invariably for every group of every subject. Again there is considerable variation as to the extent of this effect of physical work both for the same subject at different times and when we compare the averages for the different subjects. The average decrease of the size of the point of subjective equality for the different subjects is: for Subject *B*—3.62 grams; for Subject *C*—2.85 grams; for Subject *H*—2.42 grams; for Subject *O*—1.81 grams and for Subject *T*—2.95 grams.

For convenience we have brought together the averages of all of our five subjects into Tables XXIII. and XXIV. The first of these tables contains the values for the lifted weight series before physical work, and the latter table the values for these after the work was completed. These tables are similarly constructed to the others in this paper. Each row contains the values for the subject whose initial appears in the first column. The bottom row contains the averages for all of our results in this portion of our experiment.

TABLE XXIII

AVERAGE OF ALL SUBJECTS. BEFORE PHYSICAL WORK

Subjects	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
B	0.12463	0.13248	97.10	98.48	1.38	97.80
C	0.13573	0.13162	92.79	94.92	2.14	93.86
H	0.11610	0.13345	93.37	100.52	7.15	96.95
O	0.12454	0.13714	96.46	98.56	2.10	97.51
T	0.12463	0.13831	95.41	98.90	3.48	97.16
Average...	0.12513	0.13460	95.03	98.28	3.25	96.66

TABLE XXIV

AVERAGE OF ALL SUBJECTS. AFTER PHYSICAL WORK

Subjects	h_1	h_2	D	I	Interval of Uncertainty	Point of Subjective Equality
B	0.11865	0.12063	93.01	95.36	2.36	94.18
C	0.12820	0.10754	89.63	92.40	2.77	91.01
H	0.13378	0.11338	90.28	98.78	8.49	94.53
O	0.13111	0.13683	94.51	96.89	2.38	95.70
T	0.12766	0.12894	91.64	96.78	5.14	94.21
Average...	0.12788	0.12146	91.81	96.04	4.23	93.93

The average for all of the subjects shows that the value of the coefficient of precision for the lighter judgments after physical work is slightly larger than the corresponding value for the series before the work was performed. The averages of three subjects agree with this general average, while two subjects fail to confirm it. The total average of the coefficient of precision of the heavier judgments is considerably less after the physical work and the averages of all of the subjects are in agreement with this. In the case of all of our subjects, the thresholds in both the directions of decrease and increase are considerably smaller after the physical work. The total average difference is, in the case of the lower threshold, 3.22 grams; and, in the case of the upper threshold, 2.24 grams. The interval of uncertainty is considerably larger after physical work and this is found to be true for all of our subjects. This interval is 0.98 gram larger after physical work for the total average. All of the subjects also show a marked decrease in the size of the magnitude of the point of subjective equality after physical work; the difference in the total average for all of our subjects being 2.73 grams.

The extremely great increase in the size of the interval of uncertainty (0.98 gram for the total average) is of exceedingly great importance. It is one half of this interval which is the recognized measure of sensitivity and hence we must conclude that, for our experiments, the average sensitivity of all of our subjects was lowered over 30 *per cent.* as a result of the physical work. This is an exceedingly large increase and would have a great influence in any experiments in lifted weights in which the fatigue element should enter. We wish to note, however, that our physical fatigue was extreme, inasmuch as we worked the particular set of muscles involved in the lifting up to the point where pain entered into the work. It is of interest to note that physical work impairs the real efficiency of the sensitivity directly and not as a distraction. Mitchell (26) found that distractions in lifted weight experiments decreased the size of the interval of uncertainty, while we obtained the exactly opposite result, inasmuch as we found that physical work increased the size of this interval, which of course represents a lowered sensitivity.

The amount of decrease in the size of the point of subjective equality is another very evident effect of physical work in lifted weight experiments. This fact means that, after the physical work, the time error was more effective and hence led to a greater overestimation of the second weight than had been the case before the physical work had been performed. This was, perhaps, due to the fact that the physical work seemed to have considerable effect upon the absolute impressions of the weights themselves. Our subjects frequently volunteered the information that, after the work periods, the weights seemed absolutely very light.

4. Summary and Conclusions

The above results and discussion may be summarized as follows:

1. Intensive mental work, for the period of a half hour, does not seem to have any influence on the formation of judgments in lifted weight experiments, although this work was extensive and intensive enough to arouse fatigue sensations. On the average of a large number of liftings from each of five subjects, the values of the interval of uncertainty (twice the measure of sensitivity) and of the point of subjective equality are found to be almost identical for the groups of liftings before and after the mental work was performed. Certain tendencies of increase and decrease in these values, in the magnitudes of the thresholds and in the coefficients of precision of the lighter and heavier judgments are to be found in the averages of some of our subjects. These tendencies, however, are so variable, when we compare the results of the different subjects, and indeed, in most cases, when we compare the different groups of reactions of the same subject, that we must conclude that they are due to chance influences and not to the mental work.

2. Very intensive physical work of the muscles utilized in the liftings, on the other hand, has a marked and exceedingly great influence on the formation of judgments in lifted weight experiments. Physical work has little or, at least, a variable effect on the coefficient of precision for the lighter judgments. Physical work has a tendency, strongly marked and invariably

present, to decrease the size of the coefficient of precision of the heavier judgments. Physical work tends to decrease, strongly and invariably, the size of both the lower and upper thresholds, the decrease being much more marked on the lower than the upper threshold. Also the size of the interval of uncertainty is markedly increased by physical work, by nearly a gram for the total average of all of our experiments. This is a very marked decrease in the sensitivity of over 30 *per cent.*, which must be considered in lifted weight experiments. Physical work has also a marked and invariable effect of decreasing the size of the point of subjective equality or, in other words, there is a greater tendency to overestimate the second weight of each comparison pair after physical work.

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THE TRABUE COMPLETION TEST AS APPLIED TO DELINQUENT GIRLS[1]

BY ALIDA C. BOWLER

The purpose of this study was two-fold:

1. To determine whether an individual's graded completion test achievement might be considered indicative, to some extent, of his general mental ability.

2. To obtain a practical, graded series short enough to be completed in fifteen or twenty minutes.

The material used consisted of the original 56 mutilated sentences presented by Prof. M. R. Trabue at the meeting of this section December 31, 1914. These were printed on one sheet of paper, folded to form four pages. Sentences 1–20 were on the first page, 21–36 on the second, 37–47 on the third, and 48–56 on the fourth.

The subjects were called in to an ordinary school room in small groups of from 3–5. Although on each page was a printed line of instruction which read "On each line of dots write the word which makes the best meaning," they were always given a simple oral explanation before they received the sheets. They were told something like this: "There is a sheet on which you will find some sentences. Something has been left out of each sentence. I want you to read each one, think about it, and then write in the word which seems to you to make the best sense. Wherever there is a blank you must write a word. Just one word in each blank." They were then given the sheets with page one folded out, and having completed that the sheet was turned and they were told to do the same with the next page, and so on. In almost every case they were allowed to work as long as they wished. Occasionally a word of encouragement was spoken.

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[1] Contributed from the Bureau of Juvenile Research, Columbus, Ohio. Read before Section L of the American Association for the Advancement of Science at Columbus, Dec. 29, 1915.

In some cases of low mentality they were not urged to keep at work when they seemed to have reached their limit and wished to "give up." Thus a certain amount of error may have crept in. That is, a girl of that type might possibly have filled a single blank correctly here and there and thus raised her total score slightly.

The test was given through the kind coöperation of Mrs. Margaret McNamara, chief matron, to some 256 delinquent girls at the Ohio State Girls' Industrial School. They range in actual chronological age from 13 to 18 years. Each had previously been given a rating by the Yerkes-Bridges Point Scale. These ratings range from 40–97 points.

In scoring results each blank correctly filled was given one point credit. Since it was our desire to test the ability to complete an idea logically, rather than the ability to use language correctly, it soon became evident that a system of half-credits was desirable. For, with this type of girl, inelegant diction and grammatical errors are more the rule than the exception. It was quite common to find "The kind lady *give* the poor man a dollar," "The *girls* plays with her doll all day," etc. Blanks calling for prepositions were very likely to be filled awkwardly. They made little discrimination between adjective and adverb, frequently writing, "The stars *are* brightly at night," and "The poor baby cries as if it were *bad* sick." Each blank which was so filled as to develop the idea logically, even though awkwardly, was given half-credit. Each girl was then given: (1) a score on her performance by sentence groups or pages, and (2) a score which represented the total number of blanks correctly filled.

For purposes of convenience in studying results, the subjects were divided by means of their point-scale ratings into six groups. Group *A* consists of 12 girls scoring 90–97 inclusive, group *B*, 62 girls scoring 80–89, *C*, 64 girls scoring 70–79, *D*, 70 girls scoring 60–69, *E*, 37 girls scoring 50–59, and *F*, 11 girls scoring 40–49. The curves in Fig. 1 show the average percentage achievements of each group for the four pages of the test, (*a*) being the curve for page 1, sentences

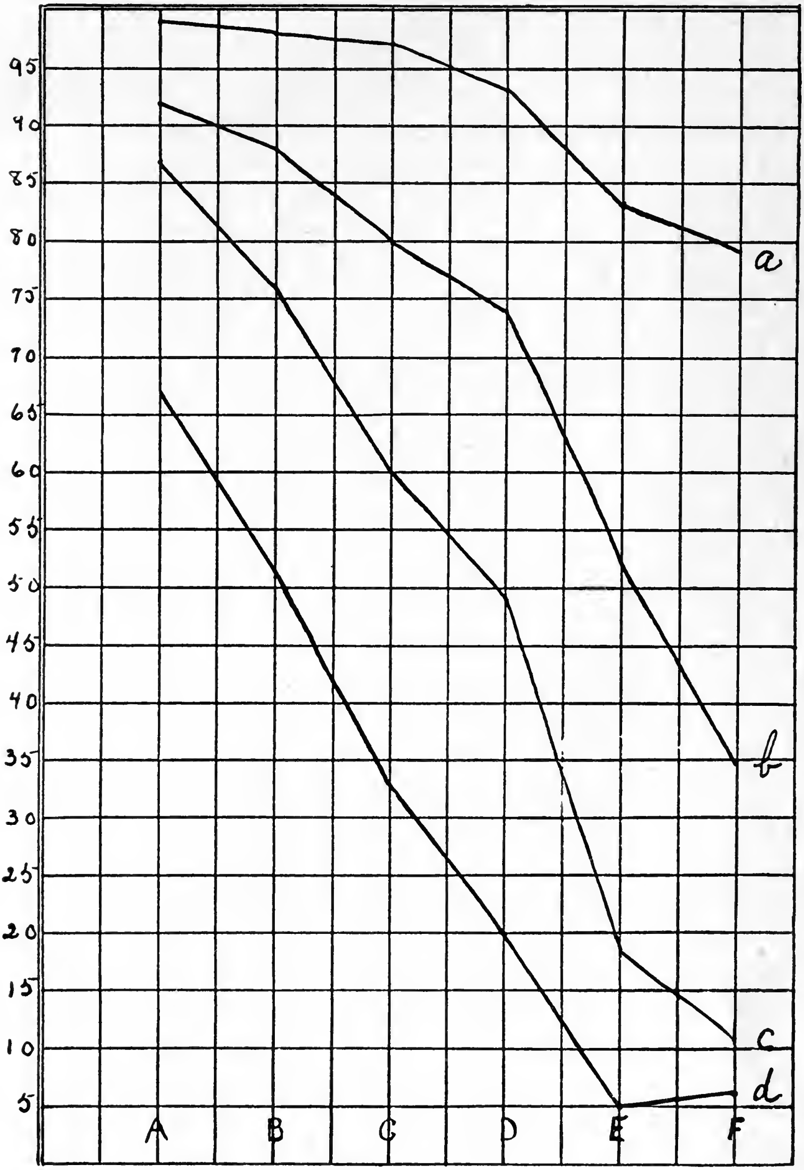

FIG. 1. *a* = aver. per cent. scores on sentences 1–20; *b* = aver. per cent. scores on sentences 21–36; *c* = aver. per cent. scores on sentences 37–47; *d* = aver. per cent. scores on sentences 48–56.

1–20, (*b*) for page 2, sentences 21–36, (*c*) page 3, sentences 37–47, and (*d*) page 4, sentences 48–56. These curves show that there is a decrease in ability from group to group in each case, except 1 *d*, groups *E* to *F*, and it should be observed that *F* is a very small group. It is evident that sentences 1–20 are too easy to detect any great differences for girls who can make sixty or more points by the Yerkes-Bridges Scale. The same may be said for sentences 21–36. From this point on, however, there is a decided drop from group to group. Fig. 1 also shows that the test is essentially a graded one. Within each group there is a decrease in achievement for each succeeding page of the test. Nowhere do the curves cross.

From this glimpse of the results in fragments it is easy to pass to Fig. 2, in which curve (*a*) represents the average percentage achievement for the six groups for the entire 56 sentences. The decrease in ability is quite regular and corresponds rather strikingly to the average point scale scores for the six groups as represented by curve (*c*). This close correspondence suggested the working out of the correlation between the two. By the Pearson "product-moments" method this coefficient of correlation, between the Yerkes-Bridges Point Scale scores and percentage achievements of individuals in the Completion Tests, proved to be .79, with a P.E. of .0157. It therefore seems reasonable to conclude that an individual's grade completion test achievement is, to some extent, indicative of his general mental ability.

The time spent upon the test varied from twenty minutes to one hour. In order to cut down the series to one which could be completed in fifteen or twenty minutes, a careful study of the value of the sentences was made. The percentage of the entire 256 correctly completing each sentence was determined. An arrangement in order of difficulty based upon this computation proved somewhat different from that derived by Professor Trabue from New York school children. Sentence 4, correctly completed by 98 per cent., became first while 54, correctly completed by but 4 per cent., became last. A shorter series was then made up, eliminating, so far as possible, those sentences in which half-errors were constantly made. The choice fell upon sentences 4, 19, 3, 6, 5, 14, 22, 26, 29, 18, 30, 28, 35, 39, 36, 45, 47, 50, 32, 52, arranged in that order by reason of the

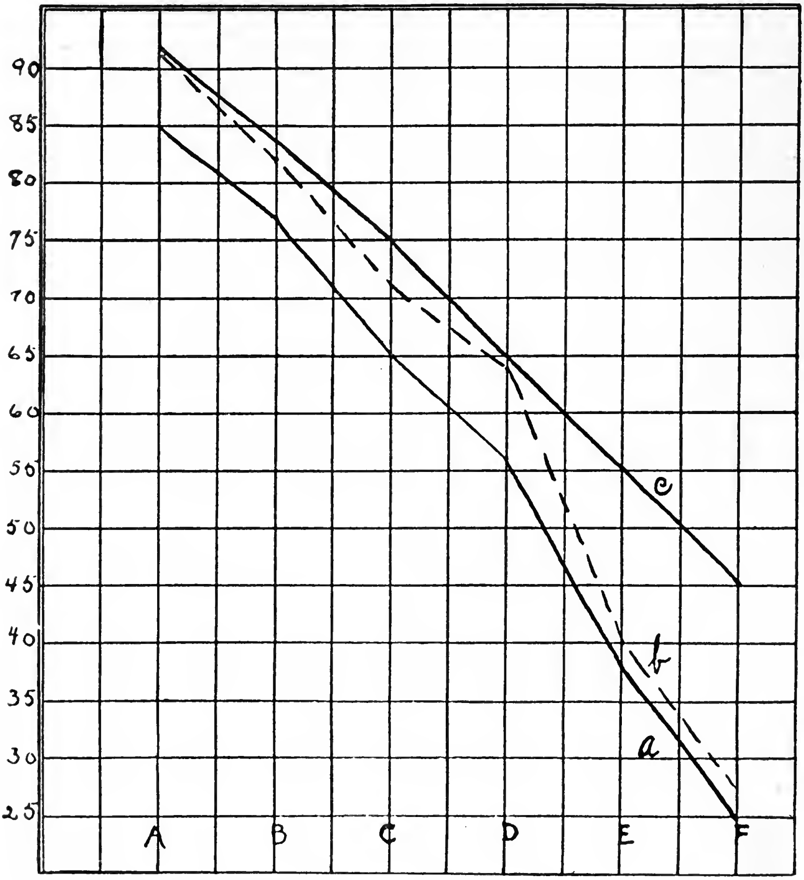

FIG. 2. *a* = aver. per cent. scores on entire test; *b* = aver. per cent. scores on 20 suggested sentences; *c* = aver. Yerkes-Bridges Point Scale Scores.

percentage of our subjects correctly completing each. These make up a suggested series of 20 sentences, with a total of 50 blanks, increasing gradually in difficulty from sentence 1, correctly completed by 98 per cent., to sentence 20 correctly completed by but 9 per cent. Curve (*b*) in Fig. 11 represents the average achievement of each group of subjects on these 20 sentences. It so nearly parallels curve (*a*), which represents the entire series of 56, as to seem to be quite as effective a test. Moreover it seems doubly practical, since it is graded and is short enough to be completed in fifteen or twenty minutes. It is also easily scored, on a percentage basis, if each of its fifty blanks be given 2 points credit for correct filling, and 1 point for logical, but awkward, completion.

SHORT COMPLETION TEST OF TWENTY SENTENCES, AND FIFTY BLANKS

256 Scores Summarized in Curve (*b*) *of Fig. 2*

1. We are going........school.
2. The girl fell and........her head.
3. I see........man and the boy.
4. The........is barking at the car.
5. Men........older than boys.
6. When the........grows older he........be a man.
7. Time........often more valuable........money.
8. Boys who play....... ....mud get their hands ....
9. Boys and........soon become........and women.
10. The best........to sleep is at night.
11. The boy who........hard........do well.
12. The poor little........has........nothing to......; he is hungry.
13. Brothers and sisters........always........to help ....other and should........quarrel.
14. If a person injures one by........, without having intended any........, one should...... ....insulted.
15. Worry........never improved a situation but has ....made conditions.........
16. To........many things........ever finishing any of them........a........habit.
17. Children should........that after all nobody is ....to care much more........their success than...... ....parents.

18. When two persons.......about.......which neither understands they........almost........to disagree.

19. One's........do........always express his thoughts.

20. When one feels drowsy and......., it.......happens that he is........to fix his attention very successfully....... anything.

THE EFFECT ON FOVEAL VISION OF BRIGHT SURROUNDINGS—IV

BY PERCY W. COBB

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The problem dealt with in the present paper is essentially the same as one previously treated under similar title. (1). The present work is however limited to the investigation of difference-threshold under various conditions as to brightness of field and surroundings. The modification of the apparatus from the form in which it was formerly used has been made the subject of a separate communication. (2).

The observations were taken under fourteen different sets of conditions as indicated in the tables of results. These sets naturally divide themselves into two groups, the set designated *o* being common to the two. In this case the field and its surroundings are of equal brightness, and the two groups are completed, the one (*a* to *g*) by keeping the surroundings at the same brightness as in the case of *o* and using various brightnesses of field, and the other (*u* to *z*) similarly by keeping the field constant and varying the brightness of the surroundings.

PROCEDURE

The observations were taken in groups of 100, which consisted of 25 different stimuli each presented 4 times, the entire group of 100 being given in shuffled order with a short rest after each 25 exposures. Such a group of experiments occupied about one half hour. Five such groups constituted the work for each observer under each set of conditions; in all 500 experiments, 20 on each stimulus.

The 25 different stimuli consisted of 1 in which no difference existed between the two halves of the field, and 12 each with the difference on the right and left halves, these differences increasing by uniform steps from 1 to 12 units. As it developed in the course of the work that as a rule a certain practice effect was taking place these units had to be changed from time to time.

The observer, after three seconds' observation of the test-stimulus, rendered one of three judgments: either that the right or the left half of the field appeared the brighter, or that neither of these two judgments could be rendered. The judgment was therefore as to the direction of the difference.

The 5 successive experimental sessions under each of the 14 sets of conditions were so distributed over the entire time of the whole 70 that each was represented as fairly as possible at all parts of the period.

METHOD OF COMPUTATION

It was at first intended to compute the results by the method of constant stimuli but, as stated, the stimuli had to be changed during the course of the work. Even where this was not the case the number of experiments on each stimulus, to have been sufficient to obliterate all inversions of the first order, would have been prohibitively large considering the large number of sets of experimental conditions to be investigated. It was therefore decided to stop with the number planned and a method of computation was worked out to suit the case.

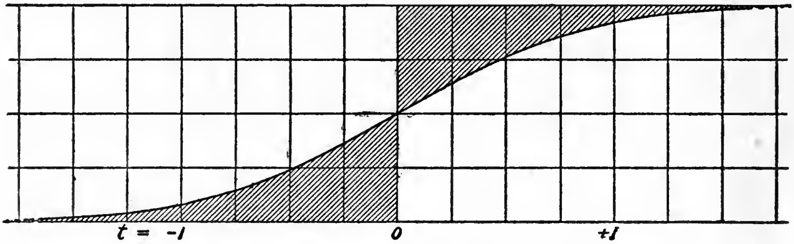

FIG. 1. The theoretical frequency curve: $y = \int_{-\infty}^{t} e^{-t^2} dt$.

This is based on the fundamental table of Fechner, in turn founded on the Gauss law of error, and consists in finding the point on the stimulus-scale such that the sums of the 'wrong' judgments on either side are equal. Fig. 1 is a plot of the values of the Fechner table, the values of t as abscissæ and the frequencies, f, as ordinates, and it is to be noted that the ordinate at 0, the threshold, cuts the curve in such a way that the two shaded areas in the figure are equal. Further it may be shown by summating the frequency-values obtained from the table that these areas are each equal to 0.2821. For the theoretical frequency-curve then

$$\int_{-\infty}^{0} f dt = \int_{0}^{\infty} (1 - f) dt = 0.2821. \tag{1}$$

If x represent any stimulus-value, the relation between t and x is such that $h(x - x_0) = t$, and therefore $hdx = dt$. The above then becomes

$$h \int_{-\infty}^{x_0} f dx = h \int_{x_0}^{\infty} (1 - f) dx = 0.2821. \tag{2}$$

Knowing the values of $\int_{-\infty}^{x} f dx$ and $\int_{x}^{\infty} (1 - f) dx$ for all values of x would enable us from the above double equation (2) to find the value of x_0 for which these two are equal, and at the same time to find the value of h from their common value at the point x_0.

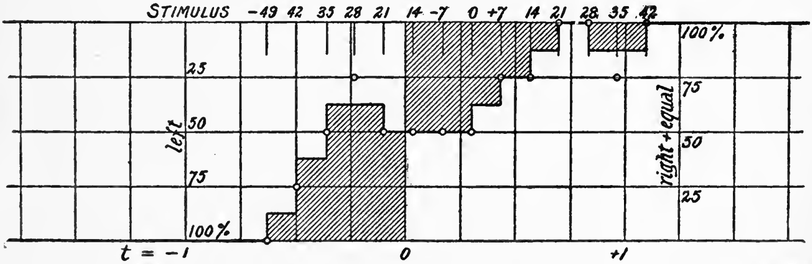

FIG. 2. The experimental frequencies. The threshold and the value of h were first computed, then the value of t corresponding to each experimental stimulus. Against these are plotted the frequencies (small circles) to illustrate the method of computation. Compare with Table I.

How these values may be worked out from a group of observations of limited number at each stimulus-point can be made clear from Fig. 2 and Table I.

TABLE I

METHOD OF COMPUTATION OF THRESHOLD AND DIFFUSION

Compare with text and Fig. 2.

Stimulus = x	−56	−49	−42	−35	−28	−21	−14	−7	0	7	14	21	28	35	42	49
$w = r + e$	0	0	1	2	3	2	2	2	2	3	3	4	4	3	4	4
l	4	4	3	2	1	2	2	2	2	1	1	0	0	1	0	0

For the intervals Δx																
	f	0	.125	.375	.625	.625	.500	.500	.500	.625	.750	.875	1.000	.875	.875	1.000
	$1-f$	1.000	.875	.625	.375	.375	.500	.500	.500	.375	.250	.125	.000	.125	.125	0

$W = \Sigma_{-\infty}^{x_1} f\Delta x$	0	0.125	0.500	1.125	1.750	2.250	2.750	3.250	3.875	4.625	5.500	6.500	7.375	8.250
$L = \Sigma_{x}^{\infty}(1-f)\Delta x$	4.750	3.875	3.250	2.875	2.500	2.000	1.500	1.000	0.625	0.375	0.250	0.250	0.125	0

In the present special case Δx is constant throughout and equal to 7 points on the scale. For simplicity it is taken as unity in computing the last two lines of figures, and the factor 7 is introduced later in computing the value of h. See equation (3), and footnote.

In Fig. 2 the experimental frequencies obtained at the various stimulus-points in a specimen group of experiments are indicated by the small circles. The mean frequency, f, taken as applying over any interval Δx, extending from x' to x'', is the mean of the two frequencies at x' and x''[1] and a value $f\Delta x$ is so obtained which approximates the area of the experimental frequency-curve included between the ordinates x' and x''. By progressively adding these products $f\Delta x$ from the point where $f = 0$, and similarly treating the products $(1 - f)\Delta x$, the values of W and L[2] of the table are respectively obtained. Inspection of these values shows that they are equal somewhere between $x = -21$ and $x = -14$. Linear interpolation locates the point at -15.75, which is therefore the value x_0 sought, for at this point

$$W = L = 2.125 \times 7,^2 \qquad (3)$$

---

[1] If the numbers of experiments at x' and x'' are unequal, say n' and n'' respectively, and of these w' and w'' respectively are the numbers of judgments of a certain class, the weighted mean frequency for the interval x' to x'' will be:

$$\frac{\frac{w'}{n'}n' + \frac{w''}{n''}n''}{n' + n''} = \frac{w' + w''}{n' + n''}.$$

[2] If the steps of the stimulus-scale (Δx) are unequal each f and each $(1 - f)$ should be multiplied by the corresponding Δx before this addition is performed. In the present case these are all equal (7 points on the scale) and are not incorporated in the values of W and L given in the table. Under the circumstances it is only necessary to introduce this factor when h is to be computed, which is done here.

and since W and L are respectively approximations to the two integrals in (2) we have further

$$hW = hL = 0.2821, \tag{4}$$

from which

$$h = \frac{0.2821}{2.125 \times 7} = -.0190$$

and the probable error $\left(= \frac{0.477}{h} \right)$ is 25.2.

This method of computation depends on two assumptions:

(1) That the deviations of the threshold from its mean position follow the Gauss law of error. This assumption is also necessary in threshold determination by the conventional method of constant stimuli.

(2) That it is approximately true that for the theoretical frequency-curve

$$\frac{f' + f''}{2} \Delta x = \int_{x''}^{x'} f dx.$$

Consequently the stimulus-intervals must be sufficiently short so that the error introduced will be small enough to be negligible in proportion to the other possible errors inherent in the experimental conditions and in the method of computation. Further, the stimuli must be so chosen as to be representative of the entire uncertain region.

It is to be added that by this method no observation is left out of account. This can not be said of the method of limits, which completely ignores all those judgments which lie between the two critical judgments of the series. The method is even applicable to a series of one judgment on each stimulus, giving in such a case an approximate threshold value and measure of diffusion.

The fundamental assumptions and possibly also the procedure are perhaps not free from all objections. The method is an approximation and is to be judged as such. It furnishes a value for the threshold and an index of its fluctuations in the form of a probable error, with (in the writer's opinion) as great accuracy as is warranted by the data from which these results are derived.

RESULTS

Each group of 100 judgments furnished, by the method of computation just discussed, two points in the stimulus scale: one corresponding to the appearance of greater brightness on the left half of the field, and one on the right. For each of these it was possible to work out a measure of diffusion in the form of probable error as described in the foregoing. This latter is in what follows simply designated 'diffusion' and is always expressed in the same units as the threshold discussed in connection with it. For such a group of 100 experiments the mean threshold is one half the stimulus-interval between these two points on the scale. Similarly the diffusion is the mean of the two diffusion values applying to the right and left threshold points respectively.

TABLE II

OBSERVER *C*

	Brightness		Absolute		Fractional	
	Field	Surroundings	Threshold	Diffusion	Threshold	Diffusion
a	436.	17.3	1.37	2.50	0.32%	0.57%
b	128.	17.3	0.354	0.623	0.28	0.49
c	35.3	17.3	0.0969	0.211	0.27	0.60
o	17.6	17.3	0.0439	0.0859	0.25	0.49
d	8.92	17.3	0.0378	0.0643	0.42	0.72
e	2.11	17.3	0.0159	0.0299	0.75	1.42
f	0.538	17.3	0.0160	0.0259	2.97	4.81
g	0.0	17.3	0.0126	0.0212	——	——
u	17.5	121.	0.145	0.245	0.83	1.41
v	17.6	46.0	0.0695	0.130	0.40	0.74
(*o*)	(17.6)	(17.3)	(0.0439)	(0.0859)	(0.25)	(0.49)
w	17.6	8.64	0.0504	0.0957	0.29	0.54
x	17.6	2.15	0.0595	0.108	0.34	0.62
y	17.6	0.547	0.0638	0.136	0.36	0.78
z	17.6	0.0	0.0721	0.106	0.41%	0.60%

Each result given in Tables II., III. and IV. is the mean of the five obtained from the five groups of experiments with one observer under that set of experimental conditions. The results are stated in the tables as absolute brightness-difference, and in the last two columns under 'Fractional' as difference per cent. brightness of the less bright half of the field. Table V. gives the mean results for the three observers. Except the last two columns all values in these tables are expressed in candles per square meter.

TABLE III

OBSERVER *J*

	Brightness		Absolute		Fractional	
	Field	Surroundings	Threshold	Diffusion	Threshold	Diffusion
a	436.	17.3	1.68	2.73	0.38%	0.63%
b	128.	17.3	0.351	0.659	0.27	0.52
c	35.3	17.3	0.0941	0.158	0.27	0.45
o	17.6	17.3	0.0378	0.0741	0.22	0.42
d	8.92	17.3	0.0274	0.0437	0.31	0.49
e	2.11	17.3	0.0133	0.0231	0.63	1.10
f	0.545	17.3	0.0116	0.0141	2.13	2.59
g	0.0	17.3	0.00806	0.0130	——	——
u	17.5	121.	0.104	0.140	0.59	0.80
v	17.6	46.0	0.0500	0.0870	0.28	0.50
(*o*)	(17.6)	(17.3)	(0.0378)	(0.0741)	(0.22)	(0.42)
w	17.6	8.64	0.0399	0.0702	0.23	0.40
x	17.6	2.15	0.0425	0.0985	0.24	0.56
y	17.6	0.547	0.0398	0.101	0.23	0.57
z	17.6	0.0	0.0490	0.128	0.28%	0.73%

Since the results of Tables II. to IV. are the means of five, which are each in turn the mean of two, a right and a left, there remain to be considered the variations of these among themselves. They are given in Table VI., where all

TABLE IV

OBSERVER *M*

	Brightness		Absolute		Fractional	
	Field	Surroundings	Threshold	Diffusion	Threshold	Diffusion
a	436.	17.3	2.72	2.66	0.62%	0.61%
b	128.	17.3	0.690	0.806	0.54	0.63
c	35.3	17.3	0.164	0.206	0.47	0.59
o	17.6	17.3	0.0672	0.101	0.38	0.57
d	8.92	17.3	0.0614	0.0763	0.69	0.86
e	2.11	17.3	0.0180	0.0328	0.85	1.56
f	0.538	17.3	0.0170	0.0197	3.17	3.66
g	0.0	17.3	0.0102	0.0238	——	——
u	17.5	121.	0.137	0.217	0.78	1.24
v	17.6	46.0	0.0874	0.144	0.50	0.82
(*o*)	(17.6)	(17.3)	(0.0672)	(0.101)	(0.38)	(0.57)
w	17.6	8.64	0.0994	0.131	0.57	0.75
x	17.6	2.15	0.130	0.118	0.74	0.67
y	17.6	0.547	0.124	0.143	0.70	0.82
z	17.6	0.0	0.146	0.177	0.83%	1.01%

TABLE V

THREE OBSERVERS, GEOMETRIC MEAN VALUES

	Brightness		Absolute		Fractional	
	Field	Surroundings	Threshold	Diffusion	Threshold	Diffusion
a	436.	17.3	1.84	2.63	0.42%	0.60%
b	128.	17.3	0.441	0.692	0.35	0.54
c	35.3	17.3	0.114	0.190	0.32	0.54
o	17.6	17.3	0.0482	0.0863	0.27	0.49
d	8.92	17.3	0.0399	0.0599	0.45	0.67
e	2.11	17.3	0.0156	0.0283	0.74	1.34
f	0.540	17.3	0.0144	0.0190	2.71	3.57
g	0.0	17.3	0.0101	0.0183	——	——
u	17.5	121.	0.127	0.196	0.73	1.12
v	17.6	46.0	0.0672	0.117	0.38	0.67
(*o*)	(17.6)	(17.3)	(0.0482)	(0.0863)	(0.27)	(0.49)
w	17.6	8.64	0.0585	0.0958	0.33	0.55
x	17.6	2.15	0.0691	0.108	0.39	0.61
y	17.6	0.547	0.0680	0.125	0.39	0.71
z	17.6	0.0	0.0802	0.134	0.46%	0.76%

TABLE VI

VARIATIONS IN THE RESULTS OF SEPARATE SERIES PER CENT. MEAN VALUE

For the Various Sets of Conditions

	Threshold				Diffusion			
	Left	M.V.	Prac.	m.v.	Left	M.V.	Prac.	m.v.
a	150	27	15	15	97	13	5.8	13
b	138	28	9	20	96	13	0.3	13
c	135	31	11	27	94	20	9.3	15
o	104	25	6	26	100	13	3.2	13
d	118	26	13	19	98	23	1.7	19
e	149	28	18	22	94	9	1.5	5
f	121	34	20	17	95	19	7.0	13
g	142	47	25	34	96	22	7.3	21
u	147	36	26	18	92	15	3.3	8
v	125	28	10	26	103	15	5.0	12
(*o*)	(104)	(25)	(6)	(26)	(100)	(13)	(3.2)	(13)
w	112	31	17	23	101	17	2.2	16
x	145	24	11	16	94	17	5.0	15
y	147	23	7	17	94	18	6.0	17
z	140	22	3	17	99	22	7.3	21

For the Different Observers

C	141	26	12	20	97	17	4.6	14
J	141	27	10	20	97	18	9.5	13
M	118	34	19	24	96	17	−0.1	16

Final Averages

	134	29	14	21	97	17	4.6	14

the values are expressed, for purposes of comparison, per cent. of the corresponding mean values.

It is to be noted that the left threshold was almost invariably larger than the right. A larger difference had to be added to the left half of the field than to the right in order to be at the point of perceptibility. This difference is expressed in the first column of figures in Table VI. under 'Left.' The value given is the left threshold per cent. of mean right and left. The final average of these is 134, meaning that on the whole the left threshold is about twice (134 : 66) as large as the right.

The next column 'M.V.' gives the mean variation of the five threshold values. The third column 'Prac.' gives the practice effect as it applies to the threshold, that is, the average decrease (per cent. mean) found to have taken place between the succeeding groups of 100 experiments under like conditions. It was computed by the method of least squares as the value of m in the equation $y = -mx + c$, where y is the threshold for the group and x its ordinal numeral in the course of five. This equation was selected as a first approximation to the unknown practice function.

Obviously a certain fraction of the mean variation as given in the second column may be supposed to be due to the effect of practice in reducing the value of the threshold. If the thresholds conformed accurately to y in the formula $y = -mx + c$ there would be, in a group of five, a mean variation due to this fact alone equal to $1.2m$, consequently the mean deviation from the value of y could not be expected, on the whole, to be as great as the mean deviation from the flat mean. It was thought too that the mean variation from y would furnish a better index of the effect of purely accidental factors on the uniformity of the results. This was therefore computed and its values appear in the next column (m.v.) of the table. It is to be noted that its value is by no means the difference between the gross M.V. and that ($1.2m$) ascribed to the effect of practice.

In the last four columns of the table the diffusion-values have been treated in a manner exactly parallel to the treatment of the thresholds just described.

Discussion

Deviations from the Mean.—As to the variations just described, a glance at the last line of Table VI. will show that the threshold is much more variable on the whole than the diffusion. The gross mean variation of the latter is some 40 per cent. less than that of the former and the practice effect 67 per cent. less. As to the preponderance of the threshold-value for a difference added to the left half of the field, we have seen that the left threshold is about twice as great as the right. For the diffusion the difference is all but negligible, the ratio left to right being 97 : 103 and is in the contrary direction to the similar difference in the case of the threshold.

The preponderance of the left threshold is sufficiently marked to be worth some discussion in detail. It might be explained in several ways and the considerations in which the present work is capable of throwing light on the point will be taken up at once.

First, as to whether the use of one eye or the other may throw the point of no apparent difference to one side or the other. Two of the observers (*C* and *J*) used the right eye exclusively, the other one (*M*) the left. It is to be noted that in all three cases the left threshold is the greater, but that this difference per cent. in the case of *M* is less than the mean difference as 18 : 34. Since the threshold in the case of *M* is (mean of 14 cases) 1.45 times the mean threshold for the three, the absolute difference in this particular is as 26 : 34. That is, neither absolutely nor relatively is the difference between the left and the right threshold as great for *M* as it is on the average. Nevertheless the use of the left eye did not reverse the disparity as it occurred for the right eye, either in the case of the threshold or its diffusion, and for that reason the use of one eye can hardly be held to account for the one-sidedness of the judgments.

Another possibility is that the field had an initial difference or non-uniformity of brightness in the direction of higher brightness on the right half. Such a difference if of threshold magnitude might easily escape detection with the photometer and would probably tend to the apparent diminution of the right threshold. If, however, this were the case the initial difference would be a constant fraction of the field-brightness and the difference between the right and left thresholds would tend to disappear as the threshold became larger and larger relatively to the field-brightness, as at *f* and *g*. More especially would this be so in the case of *g*, since here the initial brightness of the field is so small that it was not possible to see the image of the field projected in the field of the photometer. Inspection of the table shows that at *g* the preponderance of the left threshold is greater than the mean preponderance and stands sixth in order in the series of fourteen cases. And here an initial difference between the two halves of the field is out of the question.

The observer announced his judgment by depressing a telegraphic key, once for 'left side brighter' and twice for 'right.' It is supposable that where his sense-impression was more or less indeterminate he might be influenced by some sort of preference in his choice of the two reactions. If the difference between the left and right thresholds is to be explained by this it would seem to follow that this difference would go hand in hand with the indeterminateness of the sense-impression. The latter would be expressed by the relative magnitude of the mean variation. Against this possible explanation is the fact seen from the table, that at *o* where the field and its surroundings are of equal brightness the right and left thresholds are nearly equal, the inequality increasing in all cases with inequality of brightness between the field and its surroundings. That is, at *o* there is a minimum preponderance of the left threshold in the two series *a* to *g* and *u* to *z*. The mean variation shows no such minimum, but rather a progressive increase from *a* to *g* and from *z* to *u*.

There seems then to be nothing left but to state the fact that the left threshold is larger than the right. This "space error" is larger the greater the difference in brightness between the field and its surroundings for either direction of that difference.

The mean variations (M.V.) show a very definite trend, namely to be higher at those points where the surroundings are brighter than the test-field (at *u* and *g*) and *vice versa* (lower at *a* and *z*). This would seem to be due to a similar trend in the practice effect which also shows a tendency to decrease from *u* to *z* and from *g* to *a*. This decrease is not so regular as in the case of the mean variation in that there seems to be a minimum in its course at *o*, the point of equality of field and surroundings, but since the M.V.'s show no definite trend of this sort we may conclude that the practice effect is the cause of it. The effect of practice, then, is on the whole greater with a contrast-darkened field.

Similarly considered, the diffusion-values show, in a less definite way, certain consistent variations. As was stated, the inequality of these values for the right and left sides of the test-field was in the opposite direction from the corresponding difference in the case of the threshold, and was relatively much smaller in amount. That is, while the threshold on the left side is in general larger there is at the same time a greater consistency of judgment. And the inequality seems to increase in the direction of less diffusion on the left side, progressively with the increase of brightness-inequality in either direction between the test-field and its surroundings.

The mean variations distribute themselves differently from those of the threshold, and it is only in a slight degree that they show any consistent departure from uniformity. Further it is difficult to think of the significance of such departures in terms of concrete facts. It does not therefore seem profitable to regard them—and the same is to be said of all of the last three columns of Table VI., including the practice effect—as more than an index of the consistency of the diffusion-values of the preceding tables. Viewed as such they indicate a slightly greater consistency of the diffusion-values at *a* and *u* than at *g* and *z*, that is the diffusion-values seem to be more consistent on the whole where the brightness-conditions of the visual field are at an absolutely higher level, irrespective of the relative brightness of the test-field and its surroundings.

Individual Differences.—In general, the thresholds of the two observers *M* and *J* might be said to vary in a parallel way with changes in conditions, although this is not strictly true.

The threshold of *M* is on the whole large, that of *J* small; the wide difference probably being due to the lack of practice on *M*'s part. This is indicated by the fact that his practice effect is larger than the mean practice effect as 19 : 14 relatively to the respective means, or, since his threshold is 1.45 of the mean his practice effect is to the mean as 27 : 14 in absolute amount.

Observer *C* stands, with respect to the general magnitude of his threshold, between the two others. At the point of equality of brightness between test-field and surroundings, *o*, his threshold is not much in excess of that of *J*; but it shows a tendency to change with changes in conditions in a way that is far from parallel with the changes in the other two. These deviations are indicated in the following:

Ratio of Brightness, Test-Field / Surroundings	Due to	
	Increase	Decrease
	of Absolute Brightness	
High	Increase of Test-Field *o* to *a* Parallel increase of thresholds *M* and *J*. Less in the case of *C*, so that at *a* his is below that of *J*.	Decrease of Surroundings *o* to *z* All increase; *M* greatly, *C* moderately, *J* scarcely at all so that here *C* becomes progressively *worse* than *J*.
Low	Increase of Surroundings *o* to *u*	Decrease of Test-field *o* to *g*
	Uniformly a parallel increase in the thresholds of *M* and *J*, with that of *C* diverging from that of *J* and coming, at the extreme conditions (*u* and *g*), to coincide with that of *M*.	

The above scheme is only to be taken as showing the relatively different tendencies of the three observers as their results fluctuate with changes in the conditions. What may be drawn from it in the way of conclusion is that as the test-field becomes darker than its surroundings *C*'s threshold increases relatively more than that of the others; while in the opposite case the result depends upon the absolute brightness of the conditions—with the test-field brighter than the surroundings his threshold becomes relatively larger than that of the others at low absolute brightness, and smaller at high absolute brightness.

It seems then that the vision of this observer is on the whole less adaptable to low brightness-conditions, more especially where the condition is one of an object of brightness which is low relatively to its surroundings.

The Effect of Different Brightness-Conditions.—The final results stated in Table V. are graphically shown in Figs. 3 to 5 inclusive.

The difference threshold may be viewed in two ways: first as an absolute quantity, a certain difference in brightness; or second, as a fraction of the absolute brightness of the field with respect to which it is the least perceptible difference. The results have here been treated in both ways.

In Fig. 3 are plotted as ordinates the absolute differences in candles per square meter. The abscissæ are according to the case: in the curves *ag*, *a′g′*, the brightness of the test-field, the surroundings being kept constant; and in the curves *uz*, *u′z′*, the brightness of the surroundings, the test-field being kept constant.

It is to be noted that when either of these factors is raised above the point of equality the threshold increases, and does so almost as a linear function of the variable brightness-condition, as would be expected in the case of change in the field-brightness, from Weber's law. However, the surroundings are far less effective in bringing about a threshold increase than is the brightness added to the test-field itself. Computing from the results at *o*, *u* and *b*, Table V., we find that for 1 unit of brightness added to the field the threshold increases on the average 0.0035 unit; while for a similar increase in brightness of the surroundings the threshold increases only 0.00085 or about 1/4 as much. Similarly, with respect to the diffusion-value, the surroundings are about 1/5 as effective as the field itself. For the conditions of the experiment, especially as to size of test-field, the relative 'extinction' values are then about as 1 to 4 for the surroundings and the field respectively.

The general conception of such a thing as 'extinction' value for any part of the visual field is however misleading and therefore objectionable. It implies that the threshold at a given point on the retina is invariably raised by increased incidence of light upon that or any other part of the retina. Further inspection of the curve *uz* (see also Fig. 4) will show that this is not the case, for although the threshold declines

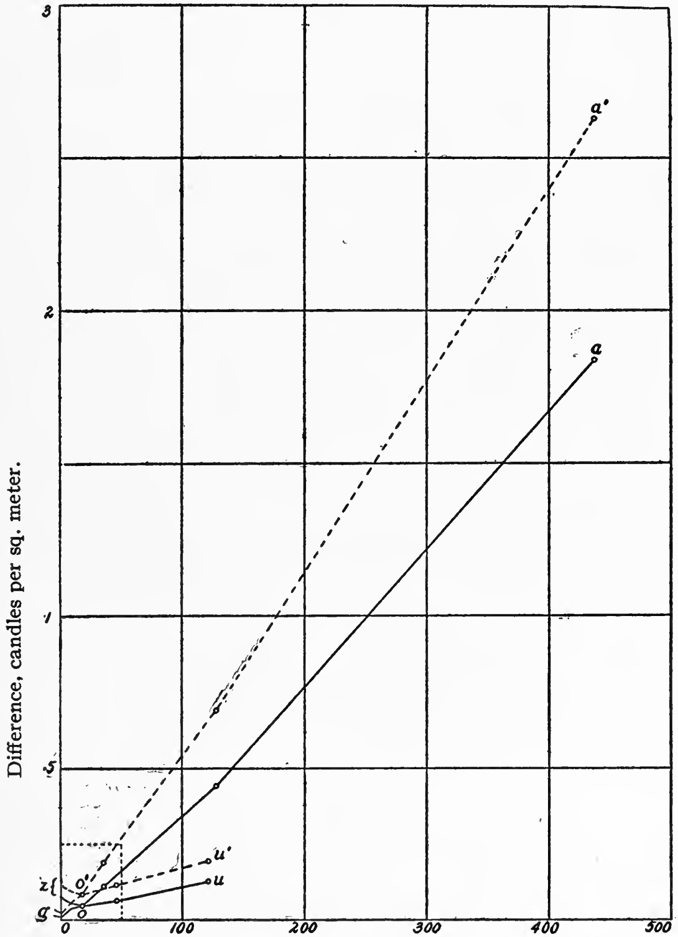

Fig. 3. Absolute values of the threshold (solid lines) and diffusion (broken lines) plotted against: *a-o-g*, absolute brightness of the test field and *u-o-z*, of the surroundings. In the former case the surroundings, and in the latter the test field were kept constant at a brightness of 17+ candles per square meter.

in value as the surroundings are brought down to the point of equality in brightness with the field, further reduction in the brightness of the surroundings has the opposite effect of increasing the threshold (*o* to *z*) to a final moderate value when the surroundings are made completely dark.

The other curve, *ag*, Fig. 3, shows that with the surroundings constant and the field of variable brightness a change in the threshold occurs uniformly the same in direction as that in the field-brightness. But if the field alone be considered and the surroundings left out of consideration the threshold demanded by a strict interpretation of Weber's law would be represented by a straight line from the origin of coordinates. Such a line passing through the threshold at *o* would fall below the curve *ag* at all other points. Simi-

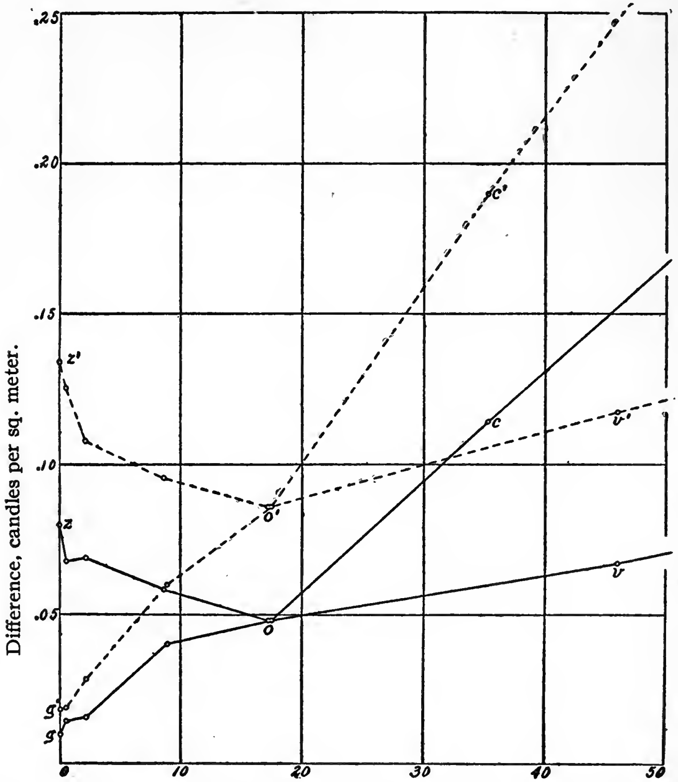

Fig. 4. Repetition of the portion of Fig. 3 enclosed in dotted lines, to a larger scale. Test field variable in curves *c-o-g*, surroundings in *v-o-z*.

larly, the threshold according to Weber's law for a field of constant brightness, corresponding to the curve *uz* would be represented by a horizontal line, which would necessarily assume a similar relation to *uz*. The tendency is, then, for the threshold to assume a minimal value at or near the point of parity of field and surroundings.

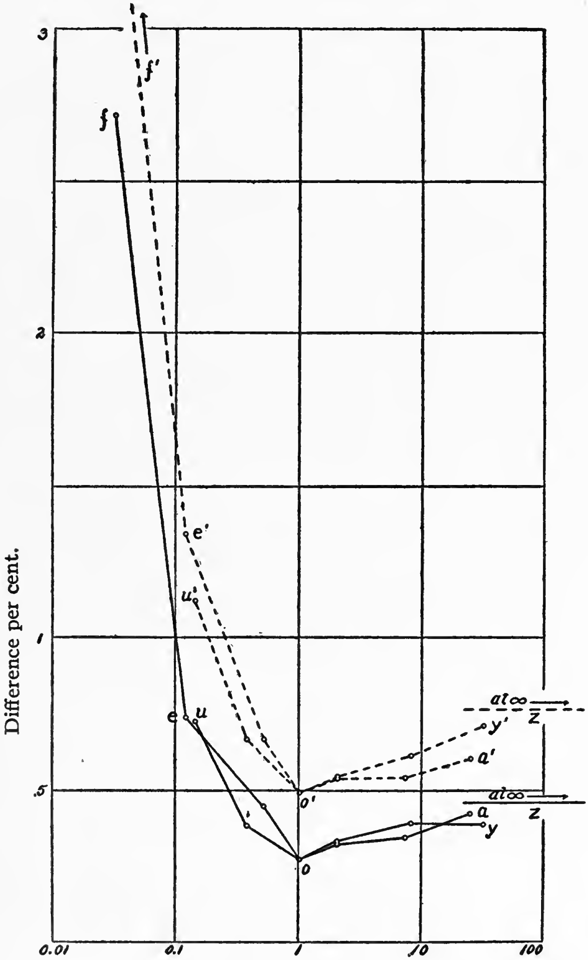

Fig. 5. Fractional threshold (solid lines) and diffusion values (broken lines) plotted against ratio of brightness of test field to that of surroundings. This last is plotted logarithmically while the numbers appearing are the actual values of the ratio.

This fact appears more clearly in Fig. 5. Here the ordinates are not plotted as brightness but as difference per cent. brightness of test-field; and the abscissæ are not brightness units but ratios, brightness of test-field to that of surroundings. For convenience the latter ratios are plotted logarithmically.

What appears at once is (and what is said here applies to the diffusion-values as well, a' to f' and u' to y') that the values from a to f, and from u to y so plotted, give quite similar curves, and the threshold as a fraction is a fairly definite function of the ratio that obtains between the test-field brightness on the one hand and the brightness of the surroundings on the other. When this latter ratio is near unity the threshold fraction is minimal. As the surroundings come to be in excess of the test-field in brightness, whether from their own increase or from diminution of the brightness of the field, the threshold fraction increases promptly and ultimately to an enormous extent. In the opposite case, when the field is relatively brighter than the surroundings, there is also an increase in the threshold fraction, but it is relatively slow and limited in amount.

In view of the relative smallness of the experimental differences mentioned in the last sentence it is perhaps well, at this point, to examine the results and determine with what degree of probability they indicate differences in the direction stated.

The mean magnitudes of the experimental differences may be obtained from Table V. (Fractional Threshold), or more precisely from Table VII., where the mean results are stated to one more decimal place under the column head T_m. The probable error of the difference is to be obtained from the probable errors of the two quantities concerned, which are, in turn, to be obtained from the mean variations of the individual quantities going to make the mean.

In Table VII., under the heading referring to each observer the first column gives the individual's threshold as plotted in Fig. 5, per cent. field brightness. The second column gives the mean variation and the third the probable error of the individual's threshold.

If T represents the individual's threshold and R the sum of the differences between it and the N separate measurements of which it is the mean, taken without regard to sign; then M.V. as here stated is expressed by the formula: M.V. = R/TN, that is, the mean variation is expressed as a fraction of T, the quantity to which it applies.

The probable error is found by the formula:

$$E = 0.8453 \frac{R}{N\sqrt{N-1}}$$

and the fractional probable error given in the next column of the table is:

$$\frac{E}{T} = 0.8453 \frac{\text{M.V.}}{\sqrt{N-1}},$$

which in this case, since $N = 5$, is equal to 0.423 M.V.

T_m is the geometrical mean of the three T's from which it is derived. That is, $T_m = \sqrt[3]{T_1 \cdot T_2 \cdot T_3}$, and the formula for the probable error of such a function in terms of the three T's and their corresponding probable errors is:

$$E_m = \frac{T_m}{3}\sqrt{\left(\frac{E_1}{T_1}\right)^2 + \left(\frac{E_2}{T_2}\right)^2 + \left(\frac{E_3}{T_3}\right)^2},$$

which is not the fractional error but the probable error expressed in the same units as T_m.

The last section of the table gives the successive differences (D) between the final mean thresholds, and the probable errors of these differences computed by the formula:

$$E^D = \sqrt{E_m'^2 + E_m''^2}.$$

P and $p (= 1 - P)$ are simply the two segments of the area under the probability curves in the respective cases which are separated by the ordinate $D = 0$. The form of the probability curve is determined by the value of E_d, and the values of P and p may be obtained by the help of one of a number of tables in common use, among which is Fechner's fundamental table, and p is one-half the probabilty of an accidental difference equal to or greater than the experimental difference.

TABLE VII

ESTIMATE OF PROBABILITY AS TO EXPERIMENTAL DIFFERENCES

	Observer *C*			Observer *J*			Observer *M*			Mean		Differences			
	T	$\frac{\text{M.V.}}{T}$	$\frac{E}{T}$	T	$\frac{\text{M.V.}}{T}$	$\frac{E}{T}$	T	$\frac{\text{M.V.}}{T}$	$\frac{E}{T}$	T_m	E_m	D	E_d	P	p
o	.25	.20	.085	.22	.20	.085	.38	.36	.152	.274	.017				
												.050	.031	.86	.14
c	.27	.25	.106	.27	.19	.080	.47	.49	.208	.324	.026				
												.022	.036	.66	.34
b	.28	.24	.102	.27	.19	.080	.54	.40	.169	.346	.025				
												.067	.037	.89	.11
a	.32	.28	.119	.38	.27	.114	.62	.25	.106	.423	.027				
o	.25	.20	.085	.22	.20	.085	.38	.36	.152	.274	.017				
												.059	.032	.89	.11
w	.29	.42	.178	.23	.37	.157	.57	.15	.064	.333	.027				
												.060	.035	.88	.12
x	.34	.20	.085	.24	.31	.131	.74	.20	.085	.393	.023				
												.006	.033	*.55	.45
y	.36	.24	.102	.23	.31	.131	.70	.13	.055	.387	.023				
												.070	.035	.91	.09
z	.41	.09	.037	.28	.32	.136	.83	.26	.110	.457	.027				

It will be noted that the probabilities of these successive differences are in favor of the experimental differences, with the exception of the differences *c* to *b* and *x* to *y*. Questioning in a similar way the extreme difference between *o* and *a* we have $D = 0.149 \pm 0.032$ from which $P = 0.999$ and $p = 0.001$; and for *o* and *z*, $D = 0.183, \pm 0.032$, $P = 0.99994$ and $p = 0.00006$.

Since the differences in the other direction from *o* are all of as great or greater magnitude than the ones just discussed it is scarcely necessary to question them so closely.

Attention is to be drawn to the fact that in the foregoing computation the gross values of the mean variations have been used (M.V., Table VI.), and that no allowance has been made for the practice effect. As this latter is in all cases in the same direction such allowance might legitimately be made, since in so far as it may be demonstrably present it can not be held to throw uncertainty into the experimental differences obtained.

Such a deduction would seem, on the whole, to reduce the probable errors in the ratio 29 : 21 (M.V. and m.v., last line, Table VI.) and would, if introduced, have the effect of materially augmenting the value of P (Table VII.) and diminishing p. That is, the differences are more, rather than less, significant than the last table would indicate.

The results then could be most consistently stated, perhaps, by saying that proportionality is the factor in the brightness of the visual field that predominantly determines the sensitivity of brightness discrimination. Predominantly; because an exception is to be noted. As will be seen from Fig. 5 the portions *oa* and *ou* of the curves fall on the whole below the portions *oy* and *oe* respectively, indicating smaller threshold fractions, while at the same time they represent similar conditions as to proportionality but absolutely higher brightness. What is thus stated for the threshold applies even more consistently to the diffusion-values, as will be seen by comparing $o'a'$ and $o'u'$ with $o'y'$ and $o'e'$.

We may then conclude that if the experiments had been conducted under conditions of several-fold higher brightness the results would have been somewhat modified on the whole in the direction of higher sensitivity.

Theoretical Considerations.—The simplest explanation—or analogy perhaps—for the phenomenon of contrast that has come to the writer's attention is a physico-chemical one.[1] If we omit for the sake of simplicity the phenomena of color, and confine ourselves to the consideration of brightness simply—or, in the terminology of Hering to the consideration of the black-white scale of sensation—we may assume in the retinal elements a light-sensitive substance S, which under the action of light is dissociated into two substances W and B, which always tend to return by reassociation into the form

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[1] The inspiration to the discussion which follows was furnished by an article by L. T. Troland (3). I wish here to acknowledge the same and excuse myself in advance if I trespass upon or misrepresent him in any way.

of the substance S. The resulting reversible reaction will be represented by the formula:

$$.S \rightleftarrows W + B.$$

Obviously, increasing the amount of W or B will tend to reverse the reaction. We will assume that B is freely diffusible from one element of the retina to another while W is not. A strong reaction in one part of the retina will then increase the amount of B in another part and tend to reverse the reaction there, reducing the amount of W.

Quantitatively, the available amount of S will be a remainder, its original amount whose concentration we will say is constant, S, having been reduced by an amount equal to the concentration of W at any instant. This remainder $S - W$ is acted upon by the light-flux, whose activity will be represented by L, proportional to, or a linear function of, the flux-density. The rate at which the reaction proceeds will then be $L(S - W)$ and the rate of the reversal will be as WB. The constant necessary here we may suppose to be implied in L and we have then for the condition of equilibrium the equation:

$$L_1(S - W_1) = W_1B, \tag{5a}$$

which we will suppose to represent the condition in that portion of the retina receiving the image of the test-field, while an exactly similar equation holds for the surrounding retina:

$$L_2(S - W_2) = W_2B. \tag{5b}$$

Since we have assumed that B is perfectly diffusible throughout the retina its concentration at equilibrium condition will be the same at all parts as is indicated in the above. Further, if B does not diffuse out of the retina, and k is the fraction of the latter occupied by the image of the test-field we will have:

$$B = kW_1 + (1 - k)W_2. \tag{6}$$

It will be seen from this that with very low brightness of surroundings L_2 being small, W_2 will also be small and (5*a*) comes to approach the form:

$$L_1(S - W_1) = kW_1^2, \tag{7}$$

whereas when the surroundings are very bright, L_2 and W_2 being very large, (5*a*) tends to take the form:

$$L_1(S - W_1) = W_1C, \tag{8}$$

in which C is constant with reference to W_1, and is the value of B from (6) when W_2 is so large that W_1 may be neglected in comparison with it.

In discussing (5*a*) it will be convenient to drop the subscripts and we then have:

$$L = \frac{WB}{S - W}, \tag{9}$$

and assuming that a constant increment in W is the condition of a threshold difference in sensation, the condition of Weber's law is expressed by: $(\Delta L/L)\cdot(1/\Delta W)$ = a constant, and $(dL/L)\cdot(1/dW)$ may be taken as a mathematical index of the value of the fractional threshold.

1. By performing the necessary operations we find that in the scheme the fractional threshold is a minimum when $W = \frac{1}{2}S$ and $L = B$.

2. Plotting the values of W, from (9), against the values of log L as abscissæ we find that there is a certain region of stimulation in this schema within which a very close approximation to Weber's law holds. That is, the curve so obtained falls, for a portion of its course, very close to a straight line.

3. If we remember the conditions for the minimum threshold given under (1) we see that the stimulus value corresponding to it may only be increased by increasing B, which again, since $W = \frac{1}{2}S$, may, from (6), only be increased by increasing W_2, in its turn dependent on an increase in L_2 as is evident from inspection of (5*b*). That is: in the schema the increase in the stimulation corresponding to the minimum fractional threshold must go hand in hand with an increase in the brightness of the surroundings and *vice versa*.

This brings us to consideration of certain serious defects in the assumptions made and the physico-chemical picture built upon them. In the first place the value of the stimulus,

L, corresponding to a minimal threshold is strictly limited, since for minimal threshold $L = B$ and B may never be in excess of S which we have assumed to be a constant. However, we have assumed L to be a function of the actual intensity and not that intensity itself. This escape involves a further assumption that the constants of this function are variable with the general level of stimulation of the retina, say with the state of adaptation, and introduces a complication that makes it appear that the present line of discussion has been carried as far as is warranted. Still, the consequence of these assumptions, that a point of intensity could be reached at which no minimal threshold is possible, is not of itself fatal. For the reason that the result was the same for the three observers who otherwise showed very striking differences it is hardly to be concluded that the minimal threshold at the point of equality of field and surroundings was due to the unintentional selection of a critical brightness for these conditions. But there is no reason to think that this state of things should necessarily hold equally well for extremely high or extremely low general brightnesses. The writer does not know of any attempt to investigate the question under these conditions. The work of König and Brodhun (4) has shown that Weber's law breaks down under these extreme conditions, and the present work would seem to indicate that the general conclusion, as to proportionality of stimuli for like results, is subject to modification in view of the absolute intensity of the general stimulation. There is, in short, no reason to think that any conclusion drawn from experimental data obtained under mean conditions is valid for the extremes.

There are other shortcomings to be mentioned as applying to the schema just outlined. The assumption has been made that the chemical action of light upon the retina brings about a temporarily enhanced decomposition or dissociation of a sensitive substance; that the products of this action are not wasted but are wholly conserved (which is biologically improbable), and that the reformation of the sensitive substance takes place wholly by reassociation and not by regeneration. This too is out of harmony with such physiological knowledge as we possess, for it is reasonably certain that visual purple is reformed in both ways, by the recombination of its decomposition products and by regeneration from the blood-stream.

It is perhaps clear now that this physico-chemical hypothesis has not been brought in here because the writer believes it adequate as a physiological theory of vision. The aim had rather been to give an intimation of the possibilities inherent in the principles of physical chemistry as an aid to the construction of such a theory.

It is after all doubtful if the foregoing speculations are adequate to the explanation of a certain point shown in the present results. On referring to Fig. 5 it will be seen that the depression in the curves corresponding to the minimal threshold at the point of equality of field and surroundings (at *o*) is somewhat sharper than the other parts of the curves seem to warrant. This is not sufficiently pronounced to be beyond doubt, yet it is strongly suggestive of some influence at work that makes this point a critical one in a way that is not at all to be deduced from equation (9).

It would seem rather that some other form of disturbance enters here, not at all implied in the equation, but dependent on the simple fact of the presence of contrast rather than being related to it in any quantitative way; and the possibility that the presence of contrast in any considerable degree sets up disturbances in the eye, perhaps originating by reason of a 'storm' in the nervous connections of the sensory end-organs and involving reflexly the muscles through their connections.

That disturbances of the latter might readily have the effect of increasing the threshold seems quite possible in view of the work of Holt (5), who has shown that during voluntary movements of the eye a certain refractory period exists, within which retinal impressions fail to obtain conscious recognition. He further gives it as his opinion (*loc. cit.*, p. 42) that during certain parts of the reflex movements of the eyes these periods may also be present, but not demonstrable on account of their brevity. Assuming that contrasts in the visual field set up a disturbance in the neural connections of the extrinsic muscles it is not far to a very plausible explanation of a critical minimal threshold at the point where contrast is absent.

It does not seem possible that accommodative disturbance could be of significant effect in the present case where the retinal areas concerned are so large, unless indeed it were of such magnitude as to be easily detectable at once by the observer; and needless to say, no evidence of such disturbance was apparent.

On the other hand, the pupillary unrest, possibly consequent upon the presence of contrasting brightnesses in the visual field, would seem, if present, to have important consequences. Hering has pointed out that the dimming of a stimulus gives rise to a vivid negative after-image, and a sudden pupillary contraction has the effect of dimming the actual stimulus which is the retinal image. The negative image so brought to pass would no doubt have the effect of diminishing the effectiveness of the stimulus and so increase the size of the difference threshold. It is not so clear at once that the positive image due to a sudden dilatation of the pupil will tend to obscure a brightness difference. However the work of Dittler and Orbeli (6) makes it appear that the sensitiveness of the eye in this respect is maximum when the eye is best adapted to the brightness of the stimulus. If this is generally true *any* change in the size of the pupil would tend temporarily to increase the threshold.

As to any differences in the average pupillary diameter for different experimental conditions, it is clear that these would have only the effect of changing the general intensity of the retinal image without altering the brightness ratios, and in work recently reported by Nutting (7) it was made clear that only a small fraction of the brightness difference in any two cases is compensated by change in the pupil. This would mean that the increase of brightness of the visual field in the same ratio all around would result in a similar increase in the illumination of the retinal image throughout, but in a somewhat smaller ratio. This fact must be taken into consideration in comparing the individuals of either pair of the curves in Fig. 5 having common ordinate at *o* and *o′*. As we recede from *o* or *o′* the ordinates of the individuals of the pair represent the results at different absolute brightness, the difference being greater the more remote from *o*. The concomitant difference in the pupillary size works only an insignificant compensation as to flux density at the retina; and since in the observation of gross areas such as the halves of the present test-field minute changes in the dioptric precision of the eye are insignificant, the difference between the two members of either of these pairs of curves must be attributed to the differences in absolute brightness in the experimental conditions and not to any secondary effect.

SUMMARY

1. A method of computation is used which is based on the same assumptions as the classical method of constant stimuli. Like the latter it yields a value for the threshold and one for the diffusion of the results, but it is applicable to a set of very few experiments on each stimulus.

2. An almost constant tendency was observed to render judgments of greater brightness on the right side, to the effect that the left threshold appears on the average twice the magnitude of the right. Examination of the conditions fails to give satisfactory explanation of this except as a 'space error.'

3. The mean variation of the threshold results as between individual series is 29 per cent. of the threshold. A fairly constant tendency for the threshold to decrease appeared as the experiments progressed, and a due allowance for this fact makes 21 per cent. seem to be a better index of accidental variations.

4. The diffusion of results shows much greater consistency than the threshold when similarly examined. The space error is negligible, the practice effect about one third that of the threshold, and the mean variation less, being 17 and 14

per cent. as compared with 29 and 21 for the threshold. Since the magnitude of the range of diffusion changes hand in hand with the threshold when the conditions change, it would seem that the diffusion of the results is a better criterion of the sensitivity of the eye than the threshold as used in the present work.

5. The chief individual difference in the observers is the relatively small adaptability of one of these to a test field of brightness low relatively to its surroundings.

6. The changes in the sensitivity of the eye due to various brightness conditions, when estimated by the value of the fractional threshold, are seen to be predominantly dependent on the ratio that exists between the field brightness and that of the surroundings. As this ratio drops below unity the threshold increases rapidly to a large extent. As the ratio rises above unity the threshold rises, but slowly and to a small extent. Improvement with practice is more pronounced in the former case.

7. A minimum in the threshold exists at or near the point of equality of field and surroundings. This is too decided to be quantitatively due to the intensity of contrast and seems rather to be referable to the mere presence of contrast and a consequent disturbance of fixation. It is most marked in the results of the least practiced observer, *M*.

The observers, according to the initials used to designate them, are Dr. H. M. Johnson, Mr. Erich Martienssen and the writer; who wishes, in conclusion, to express his thorough appreciation of their co-operation, especially that of Mr. Martienssen, who acted as technical assistant as well.

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DISCUSSION

In the article entitled 'The Müller-Lyer Illusion with Children and Adults' in the June number of this JOURNAL there is a slight discrepancy in Tables I. and II. in column four showing the averages of the three judgments, to which Professor W. V. Bingham was good enough to call my attention. The average of the three judgments in column four is not exactly the same as the average of the three individual judgments. This, of course, would have been the case if the same number of judgments had entered into the three individual judgments. Unfortunately we omitted mention of the fact that there were a few cases at many different ages which were only given one or two judgments. We were at first concerned with arriving at norms for the three individual judgments separately, and only later were the averages for all those who had given three judgments computed. The discrepancy in the tables is slight and in no way influences the conclusions drawn from the data. I have to thank Professor Bingham for calling my attention to this oversight.

RUDOLF PINTNER

A CORRECTION

In our article entitled "A New Method of Heterochromatic Photometry," Journal of Experimental Psychology, No. 1, an error occurs. On p. 4, line 21, 3 cm. should read .3 cm.

C. E. FERREE
GERTRUDE RAND