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CONTENTS OF VOLUME III

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CONDITIONED EMOTIONAL REACTIONS

BY JOHN B. WATSON AND ROSALIE RAYNER

In recent literature various speculations have been entered into concerning the possibility of conditioning various types of emotional response, but direct experimental evidence in support of such a view has been lacking. If the theory advanced by Watson and Morgan[1] to the effect that in infancy the original emotional reaction patterns are few, consisting so far as observed of fear, rage and love, then there must be some simple method by means of which the range of stimuli which can call out these emotions and their compounds is greatly increased. Otherwise, complexity in adult response could not be accounted for. These authors without adequate experimental evidence advanced the view that this range was increased by means of conditioned reflex factors. It was suggested there that the early home life of the child furnishes a laboratory situation for establishing conditioned emotional responses. The present authors have recently put the whole matter to an experimental test.

Experimental work has been done so far on only one child, Albert B. This infant was reared almost from birth in a hospital environment; his mother was a wet nurse in the Harriet Lane Home for Invalid Children. Albert's life was normal: he was healthy from birth and one of the best developed youngsters ever brought to the hospital, weighing twenty-one pounds at nine months of age. He was on the whole stolid and unemotional. His stability was one of the principal reasons for using him as a subject in this test. We

[1] 'Emotional Reactions and Psychological Experimentation,' *American Journal of Psychology*, April, 1917, Vol. 28, pp. 163–174.

felt that we could do him relatively little harm by carrying out such experiments as those outlined below.

At approximately nine months of age we ran him through the emotional tests that have become a part of our regular routine in determining whether fear reactions can be called out by other stimuli than sharp noises and the sudden removal of support. Tests of this type have been described by the senior author in another place.[1] In brief, the infant was confronted suddenly and for the first time successively with a white rat, a rabbit, a dog, a monkey, with masks with and without hair, cotton wool, burning newspapers, etc. A permanent record of Albert's reactions to these objects and situations has been preserved in a motion picture study. Manipulation was the most usual reaction called out. *At no time did this infant ever show fear in any situation.* These experimental records were confirmed by the casual observations of the mother and hospital attendants. No one had ever seen him in a state of fear and rage. The infant practically never cried.

Up to approximately nine months of age we had not tested him with loud sounds. The test to determine whether a fear reaction could be called out by a loud sound was made when he was eight months, twenty-six days of age. The sound was that made by striking a hammer upon a suspended steel bar four feet in length and three-fourths of an inch in diameter. The laboratory notes are as follows:

> One of the two experimenters caused the child to turn its head and fixate her moving hand; the other, stationed back of the child, struck the steel bar a sharp blow. The child started violently, his breathing was checked and the arms were raised in a characteristic manner. On the second stimulation the same thing occurred, and in addition the lips began to pucker and tremble. On the third stimulation the child broke into a sudden crying fit. This is the first time an emotional situation in the laboratory has produced any fear or even crying in Albert.

[1] 'Psychology from the Standpoint of a Behaviorist,' p. 202.

We had expected just these results on account of our work with other infants brought up under similar conditions. It is worth while to call attention to the fact that removal of support (dropping and jerking the blanket upon which the infant was lying) was tried exhaustively upon this infant on the same occasion. It was not effective in producing the fear response. This stimulus is effective in younger children. At what age such stimuli lose their potency in producing fear is not known. Nor is it known whether less placid children ever lose their fear of them. This probably depends upon the training the child gets. It is well known that children eagerly run to be tossed into the air and caught. On the other hand it is equally well known that in the adult fear responses are called out quite clearly by the sudden removal of support, if the individual is walking across a bridge, walking out upon a beam, etc. There is a wide field of study here which is aside from our present point.

The sound stimulus, thus, at nine months of age, gives us the means of testing several important factors. I. Can we condition fear of an animal, *e.g.*, a white rat, by visually presenting it and simultaneously striking a steel bar? II. If such a conditioned emotional response can be established, will there be a transfer to other animals or other objects? III. What is the effect of time upon such conditioned emotional responses? IV. If after a reasonable period such emotional responses have not died out, what laboratory methods can be devised for their removal?

I. The establishment of conditioned emotional responses. At first there was considerable hesitation upon our part in making the attempt to set up fear reactions experimentally. A certain responsibility attaches to such a procedure. We decided finally to make the attempt, comforting ourselves by the reflection that such attachments would arise anyway as soon as the child left the sheltered environment of the nursery for the rough and tumble of the home. We did not begin this work until Albert was eleven months, three days of age. Before attempting to set up a conditioned response we, as before, put him through all of the regular emotional

tests. *Not the slightest sign of a fear response was obtained in any situation.*

The steps taken to condition emotional responses are shown in our laboratory notes.

11 Months 3 Days

1. White rat suddenly taken from the basket and presented to Albert. He began to reach for rat with left hand. Just as his hand touched the animal the bar was struck immediately behind his head. The infant jumped violently and fell forward, burying his face in the mattress. He did not cry, however.

2. Just as the right hand touched the rat the bar was again struck. Again the infant jumped violently, fell forward and began to whimper.

In order not to disturb the child too seriously no further tests were given for one week.

11 Months 10 Days

1. Rat presented suddenly without sound. There was steady fixation but no tendency at first to reach for it. The rat was then placed nearer, whereupon tentative reaching movements began with the right hand. When the rat nosed the infant's left hand, the hand was immediately withdrawn. He started to reach for the head of the animal with the forefinger of the left hand, but withdrew it suddenly before contact. It is thus seen that the two joint stimulations given the previous week were not without effect. He was tested with his blocks immediately afterwards to see if they shared in the process of conditioning. He began immediately to pick them up, dropping them, pounding them, etc. In the remainder of the tests the blocks were given frequently to quiet him and to test his general emotional state. They were always removed from sight when the process of conditioning was under way.

2. Joint stimulation with rat and sound. Started, then fell over immediately to right side. No crying.

3. Joint stimulation. Fell to right side and rested upon hands, with head turned away from rat. No crying.

4. Joint stimulation. Same reaction.

5. Rat suddenly presented alone. Puckered face, whimpered and withdrew body sharply to the left.

6. Joint stimulation. Fell over immediately to right side and began to whimper.

7. Joint stimulation. Started violently and cried, but did not fall over.

8. Rat alone. *The instant the rat was shown the baby began to cry. Almost instantly he turned sharply to the left, fell over on left side, raised himself on all fours and began to crawl away so rapidly that he was caught with difficulty before reaching the edge of the table.*

This was as convincing a case of a completely conditioned fear response as could have been theoretically pictured. In all seven joint stimulations were given to bring about the complete reaction. It is not unlikely had the sound been of greater intensity or of a more complex clang character that the number of joint stimulations might have been materially reduced. Experiments designed to define the nature of the sounds that will serve best as emotional stimuli are under way.

II. When a conditioned emotional response has been established for one object, is there a transfer? Five days later Albert was again brought back into the laboratory and tested as follows:

11 Months 15 Days

1. Tested first with blocks. He reached readily for them, playing with them as usual. This shows that there has been no general transfer to the room, table, blocks, etc.

2. Rat alone. Whimpered immediately, withdrew right hand and turned head and trunk away.

3. Blocks again offered. Played readily with them, smiling and gurgling.

4. Rat alone. Leaned over to the left side as far away from the rat as possible, then fell over, getting up on all fours and scurrying away as rapidly as possible.

5. Blocks again offered. Reached immediately for them, smiling and laughing as before.

The above preliminary test shows that the conditioned response to the rat had carried over completely for the five days in which no tests were given. The question as to whether or not there is a transfer was next taken up.

6. Rabbit alone. The rabbit was suddenly placed on the mattress in front of him. The reaction was pronounced. Negative responses began at once. He leaned as far away from the animal as possible, whimpered, then burst into tears. When the rabbit was placed in contact with him he buried his face in the mattress, then got up on all fours and crawled away, crying as he went. This was a most convincing test.

7. The blocks were next given him, after an interval. He played with them as before. It was observed by four people that he played far more energetically with them than ever before. The blocks were raised high over his head and slammed down with a great deal of force.

8. Dog alone. The dog did not produce as violent a reaction as the rabbit. The moment fixation occurred the child shrank back and as the animal came nearer he attempted to get on all fours but did not cry at first. As soon as the dog passed out of his range of vision he became quiet. The dog was then made to approach the infant's head (he was lying down at the moment). Albert straightened up immediately, fell over to the opposite side and turned his head away. He then began to cry.

9. The blocks were again presented. He began immediately to play with them.

10. Fur coat (seal). Withdrew immediately to the left side and began to fret. Coat put close to him on the

left side, he turned immediately, began to cry and tried to crawl away on all fours.

11. Cotton wool. The wool was presented in a paper package. At the end the cotton was not covered by the paper. It was placed first on his feet. He kicked it away but did not touch it with his hands. When his hand was laid on the wool he immediately withdrew it but did not show the shock that the animals or fur coat produced in him. He then began to play with the paper, avoiding contact with the wool itself. He finally, under the impulse of the manipulative instinct, lost some of his negativism to the wool.

12. Just in play W. put his head down to see if Albert would play with his hair. Albert was completely negative. Two other observers did the same thing. He began immediately to play with their hair. W. then brought the Santa Claus mask and presented it to Albert. He was again pronouncedly negative.

11 Months 20 Days

1. Blocks alone. Played with them as usual.

2. Rat alone. Withdrawal of the whole body, bending over to left side, no crying. Fixation and following with eyes. The response was much less marked than on first presentation the previous week. It was thought best to freshen up the reaction by another joint stimulation.

3. Just as the rat was placed on his hand the rod was struck. Reaction violent

4. Rat alone. Fell over at once to left side. Reaction practically as strong as on former occasion but no crying.

5. Rat alone. Fell over to left side, got up on all fours and started to crawl away. On this occasion there was no crying, but strange to say, as he started away he began to gurgle and coo, even while leaning far over to the left side to avoid the rat.

6. Rabbit alone. Leaned over to left side as far as possible. Did not fall over. Began to whimper but reaction not so violent as on former occasions.

7. Blocks again offered. He reached for them immediately and began to play.

All of the tests so far discussed were carried out upon a table supplied with a mattress, located in a small, well-lighted dark-room. We wished to test next whether conditioned fear responses so set up would appear if the situation were markedly altered. We thought it best before making this test to freshen the reaction both to the rabbit and to the dog by showing them at the moment the steel bar was struck. It will be recalled that this was the first time any effort had been made to directly condition response to the dog and rabbit. The experimental notes are as follows:

8. The rabbit at first was given alone. The reaction was exactly as given in test (6) above. When the rabbit was left on Albert's knees for a long time he began tentatively to reach out and manipulate its fur with forefingers. While doing this the steel rod was struck. A violent fear reaction resulted.

9. Rabbit alone. Reaction wholly similar to that on trial (6) above.

10. Rabbit alone. Started immediately to whimper, holding hands far up, but did not cry. Conflicting tendency to manipulate very evident.

11. Dog alone. Began to whimper, shaking head from side to side, holding hands as far away from the animal as possible.

12. Dog and sound. The rod was struck just as the animal touched him. A violent negative reaction appeared. He began to whimper, turned to one side, fell over and started to get up on all fours.

13. Blocks. Played with them immediately and readily.

On this same day and immediately after the above experiment Albert was taken into the large well-lighted lecture room belonging to the laboratory. He was placed on a table in the center of the room immediately under the skylight. Four people were present. The situation was thus very different from that which obtained in the small dark room.

1. Rat alone. No sudden fear reaction appeared at first. The hands, however, were held up and away from the animal. No positive manipulatory reactions appeared.

2. Rabbit alone. Fear reaction slight. Turned to left and kept face away from the animal but the reaction was never pronounced.

3. Dog alone. Turned away but did not fall over. Cried. Hands moved as far away from the animal as possible. Whimpered as long as the dog was present.

4. Rat alone. Slight negative reaction.

5. Rat and sound. It was thought best to freshen the reaction to the rat. The sound was given just as the rat was presented. Albert jumped violently but did not cry.

6. Rat alone. At first he did not show any negative reaction. When rat was placed nearer he began to show negative reaction by drawing back his body, raising his hands, whimpering, etc.

7. Blocks. Played with them immediately.

8. Rat alone. Pronounced withdrawal of body and whimpering.

9. Blocks. Played with them as before.

10. Rabbit alone. Pronounced reaction. Whimpered with arms held high, fell over backward and had to be caught.

11. Dog alone. At first the dog did not produce the pronounced reaction. The hands were held high over the head, breathing was checked, but there was no crying. Just at this moment the dog, which had not barked before, barked three times loudly when only about six inches from the baby's face. Albert immediately fell over and broke into a wail that continued until the dog was removed. The sudden barking of the hitherto quiet dog produced a marked fear response in the adult observers!

From the above results it would seem that emotional transfers do take place. Furthermore it would seem that the number of transfers resulting from an experimentally produced conditioned emotional reaction may be very large. In our observations we had no means of testing the complete number of transfers which may have resulted.

III. The effect of time upon conditioned emotional responses. We have already shown that the conditioned emotional response will continue for a period of one week. It was desired to make the time test longer. In view of the imminence of Albert's departure from the hospital we could not make the interval longer than one month. Accordingly no further emotional experimentation was entered into for thirty-one days after the above test. During the month, however, Albert was brought weekly to the laboratory for tests upon right and left-handedness, imitation, general development, etc. No emotional tests whatever were given and during the whole month his regular nursery routine was maintained in the Harriet Lane Home. The notes on the test given at the end of this period are as follows:

1 Year 21 Days

1. Santa Claus mask. Withdrawal, gurgling, then slapped at it without touching. When his hand was forced to touch it, he whimpered and cried. His hand was forced to touch it two more times. He whimpered and cried on both tests. He finally cried at the mere visual stimulus of the mask.

2. Fur coat. Wrinkled his nose and withdrew both hands, drew back his whole body and began to whimper as the coat was put nearer. Again there was the strife between withdrawal and the tendency to manipulate. Reached tentatively with left hand but drew back before contact had been made. In moving his body to one side his hand accidentally touched the coat. He began to cry at once, nodding his head in a very peculiar manner (this reaction was an entirely new one). Both hands were withdrawn as far as possible from the coat. The coat

was then laid on his lap and he continued nodding his head and whimpering, withdrawing his body as far as possible, pushing the while at the coat with his feet but never touching it with his hands.

3. Fur coat. The coat was taken out of his sight and presented again at the end of a minute. He began immediately to fret, withdrawing his body and nodding his head as before.

4. Blocks. He began to play with them as usual.

5. The rat. He allowed the rat to crawl towards him without withdrawing. He sat very still and fixated it intently. Rat then touched his hand. Albert withdrew it immediately, then leaned back as far as possible but did not cry. When the rat was placed on his arm he withdrew his body and began to fret, nodding his head. The rat was then allowed to crawl against his chest. He first began to fret and then covered his eyes with both hands.

6. Blocks. Reaction normal.

7. The rabbit. The animal was placed directly in front of him. It was very quiet. Albert showed no avoiding reactions at first. After a few seconds he puckered up his face, began to nod his head and to look intently at the experimenter. He next began to push the rabbit away with his feet, withdrawing his body at the same time. Then as the rabbit came nearer he began pulling his feet away, nodding his head, and wailing "da da." After about a minute he reached out tentatively and slowly and touched the rabbit's ear with his right hand, finally manipulating it. The rabbit was again placed in his lap. Again he began to fret and withdrew his hands. He reached out tentatively with his left hand and touched the animal, shuddered and withdrew the whole body. The experimenter then took hold of his left hand and laid it on the rabbit's back. Albert immediately withdrew his hand and began to suck his thumb. Again the rabbit was laid in his lap. He began to cry, covering his face with both hands.

8. Dog. The dog was very active. Albert fixated it intensely for a few seconds, sitting very still. He began to cry but did not fall over backwards as on his last contact with the dog. When the dog was pushed closer to him he at first sat motionless, then began to cry, putting both hands over his face.

These experiments would seem to show conclusively that directly conditioned emotional responses as well as those conditioned by transfer persist, although with a certain loss in the intensity of the reaction, for a longer period than one month. Our view is that they persist and modify personality throughout life. It should be recalled again that Albert was of an extremely phlegmatic type. Had he been emotionally unstable probably both the directly conditioned response and those transferred would have persisted throughout the month unchanged in form.

IV. "Detachment" or removal of conditioned emotional responses. Unfortunately Albert was taken from the hospital the day the above tests were made. Hence the opportunity of building up an experimental technique by means of which we could remove the conditioned emotional responses was denied us. Our own view, expressed above, which is possibly not very well grounded, is that these responses in the home environment are likely to persist indefinitely, unless an accidental method for removing them is hit upon. The importance of establishing some method must be apparent to all. Had the opportunity been at hand we should have tried out several methods, some of which we may mention. (1) Constantly confronting the child with those stimuli which called out the responses in the hopes that habituation would come in corresponding to "fatigue" of reflex when differential reactions are to be set up. (2) By trying to "recondition" by showing objects calling out fear responses (vsual) and simultaneously stimulating the erogenous zones (tactual). We should try first the lips, then the nipples and as a final resort the sex organs. (3) By trying to "recondition" by feeding the subject candy or other food just as the animal is shown. This method calls for the food control of the subject. (4) By building up "constructive" activities around the object by imitation and by putting the hand through the motions of manipulation. At this age imitation of overt motor activity is strong, as our present but unpublished experimentation has shown.

Incidental Observations

(a) Thumb sucking as a compensatory device for blocking fear and noxious stimuli. During the course of these experiments, especially in the final test, it was noticed that whenever Albert was on the verge of tears or emotionally upset generally he would continually thrust his thumb into his mouth. The moment the hand reached the mouth he became impervious to the stimuli producing fear. Again and again while the motion pictures were being made at the end of the thirty-day rest period, we had to remove the thumb from his mouth before the conditioned response could be obtained. This method of blocking noxious and emotional stimuli (fear and rage) through erogenous stimulation seems to persist from birth onward. Very often in our experiments upon the work adders with infants under ten days of age the same reaction appeared. When at work upon the adders both of the infants arms are under slight restraint. Often rage appears. They begin to cry, thrashing their arms and legs about. If the finger gets into the mouth crying ceases at once. The organism thus apparently from birth, when under the influence of love stimuli is blocked to all others.[1] This resort to sex stimulation when under the influence of noxious and emotional situations, or when the individual is restless and idle, persists throughout adolescent and adult life. Albert, at any rate, did not resort to thumb sucking except in the presence of such stimuli. Thumb sucking could immediately be checked by offering him his blocks. These invariably called out active manipulation instincts. It is worth while here to call attention to the fact that Freud's conception of the stimulation of erogenous zones as being the expression of an original "pleasure" seeking principle may be turned about

[1] The stimulus to love in infants according to our view is stroking of the skin, lips, nipples and sex organs, patting and rocking, picking up, etc. Patting and rocking (when not conditioned) are probably equivalent to actual stimulation of the sex organs. In adults of course, as every lover knows, vision, audition and olfaction soon become conditioned by joint stimulation with contact and kinæsthetic stimuli.

and possibly better described as a compensatory (and often conditioned) device for the blockage of noxious and fear and rage producing stimuli.

(b) Equal primacy of fear, love and possibly rage. While in general the results of our experiment offer no particular points of conflict with Freudian concepts, one fact out of harmony with them should be emphasized. According to proper Freudians sex (or in our terminology, love) is the principal emotion in which conditioned responses arise which later limit and distort personality. We wish to take sharp issue with this view on the basis of the experimental evidence we have gathered. Fear is as primal a factor as love in influencing personality. Fear does not gather its potency in any derived manner from love. It belongs to the original and inherited nature of man. Probably the same may be true of rage although at present we are not so sure of this.

The Freudians twenty years from now, unless their hypotheses change, when they come to analyze Albert's fear of a seal skin coat—assuming that he comes to analysis at that age—will probably tease from him the recital of a dream which upon their analysis will show that Albert at three years of age attempted to play with the pubic hair of the mother and was scolded violently for it. (We are by no means denying that this might in some other case condition it). If the analyst has sufficiently prepared Albert to accept such a dream when found as an explanation of his avoiding tendencies, and if the analyst has the authority and personality to put it over, Albert may be fully convinced that the dream was a true revealer of the factors which brought about the fear.

It is probable that many of the phobias in psychopathology are true conditioned emotional reactions either of the direct or the transferred type. One may possibly have to believe that such persistence of early conditioned responses will be found only in persons who are constitutionally inferior. Our argument is meant to be constructive. Emotional disturbances in adults cannot be traced back to sex alone. They must be retraced along at least three collateral lines—to conditioned and transferred responses set up in infancy and early youth in all three of the fundamental human emotions.

THE ORGANIC EFFECTS OF REPEATED BODILY ROTATION[1]

BY COLEMAN R. GRIFFITH

Table of Contents

I. Introduction

It has been frequently observed that a rapid spin upon the heel or a few turns on a merry-go-round may lead to a confused and uncomfortable state of mind and to an uncertain control of the body. So sensitive is the organism to disturbances of this kind that a quick jerk of the head to the right and to the left is often sufficient to induce a momentary giddiness; while under more violent conditions, the effects may be severe and widespread.

[1] From the Psychological Laboratory of the University of Illinois.

A. Historical Setting.—Scientific interest in the nature and the causes of dizziness and of the concomitant loss of bodily control goes back almost a hundred years.[1]

About that time the discovery was made by Flourens[2] that this kind of disturbance could be induced by exciting certain portions of the ear or by surgical insults of one kind and another to the cerebellum and to other central structures. The facts of dizziness had already been admirably described by Darwin[3] and Purkinje[4]; and the relation of the ear to these facts raised a good deal of discussion, for it had been commonly supposed that the ear was concerned with hearing alone. But experimental evidence soon made it clear that a part of the internal ear was not in any way connected with the mediation of auditory qualities, and with this demonstration a new group of problems was proposed to experimental physiology and psychology. It was first necessary clearly to establish the precise relation obtaining between the excitation of the internal ear and the effects incident to a disturbed balance. As a result of the painstaking work of a large number of investigators this relation was established in such a manner as to bring about the discovery of a new end-organ in the semicircular canals, *viz.*, the hair cells in the ampullæ. With this discovery, investigators went on to describe the mode of excitation of the new end-organ, to delineate its central connections, and to analyse the effects produced by its excitation. Notable progress was made in spite of theorists who from time to time insisted that the canals were the end-organs for the appreciation of time and space, or for noise, or for apprehending the direction of sound. By the year 1900, three hundred experimental investigations

[1] A detailed survey of scientific literature on the semicircular canals based on a review of some seven hundred references, together with a bibliography of over a thousand titles, will presently be published.

[2] Flourens, P., 'Recherches expérimentales sur les propriétés et les fonctions du système nerveux.' Bailliere, Paris, 1842, pp. 438–501.

[3] Darwin, E., 'Zoönomia, the Laws of Organic Life,' 1795. 3d ed., 1801, pp. 327–356.

[4] Purkinje, J., 'Mitteilungen über Scheinbewegungen und über den Schwindel,' *Bull. d. naturw. Sektion d. Schles. Gesellsch. f. vaterland. Kultur*, 1825–26, 10. Reprinted in Aubert's 'Studien über die Orientierung,' Tübingen, 1888.

and more had accumulated a considerable body of doctrine about the canals and their functions. For example, it was maintained that the inertia of the liquid within the membranous labyrinth or even the inertia of the whole membranous structures provided the adequate stimulus for the vestibular end-organs. The excitation initiated was supposed to set up, through the central connections of the vestibular nerve, *i.e.*, the vestibular nucleus and the cerebellum, all kinds of equilibratory adjustments. Some of these were, according to Ewald, no more than variations in tonic effect, while many other investigators insisted that the adjustments were more positive, taking the form of movements compensatory to bodily displacement. In short, a great amount of circumstantial evidence had been accumulated going to show that the structures in the semicircular canals were *end-organs* concerned chiefly in the maintenance of equilibrium. Furthermore, it had come to be commonly accepted that balance or the maintenance of position was reflexly carried out by excitations from these end-organs.

Now chief,—because regular in appearance and easy of control,—among the movements excited by the stimulation of the semicircular canals, are certain ocular movements known as 'nystagmus.' In fact, these ocular movements are so constant in their appearance as to have won for themselves the designation of 'simple reflexes.' It is upon these movements that most of the recent work has been done, the shift in point of interest occurring in the following manner. About 1906 Robert Bárány of Vienna recognized the clinical significance of such a close relation between the ear and the eyes and he immediately began, therefore, to make clinical use of the great amount of work that had been done on the canals.[1] And thus, as is common in the history of the sciences, attention shifted from the question as purely academic to the practical application of the facts already at hand. It was Bárány's hope to use the ocular effects of vestibular stimulation as an index to the functional integrity of the neural

[1] Bárány, R., 'Untersuchungen über den vom Vestibularapparate des Ohres reflektorisch ausgelösten rhythmischen Nystagmus und seine Begleiterscheinungen,' *Monatschr. f. Ohrenhk.*, 1906, 40, 193–297.

paths involved, especially of the paths lying within the cerebellum itself. One of the contemporary scientific problems is, therefore, directed toward the nature of these ocular effects of stimulation of the canals. In this connection, it is significant to note that almost, if not all, of the evidence for the function of the canals rests upon unrepeated excitations of various parts of the mechanisms concerned. That is to say, the stimulation of the end-organs in the semicircular canals results immediately in a complex group of organic and mental effects. Certain of these effects, and especially the ocular movements, regarded as reflex, have been put to important use in the clinical laboratory. No consistent account has been taken of the effect of continued excitation, either upon the ocular movements or upon the rest of the body.

B. Statement of Problem.—The problem of this investigation, therefore, is to describe the organic effects of repeated bodily rotation. It was observed by many of the earlier investigators that, when the vestibular branch of the eighth nerve was cut, or one or more of the canals injured, the compensatory movements either of the eyes or of the head and the changes in posture were at first intense but gradually disappeared within a few days. It is strange that this fact did not suggest that continued or repeated excitation of the end-organs in the canals would lead to a profound modification in the effects usually produced. As a matter of fact, however, we have failed to find an instance where a consistent effort has been made to repeat ampullar stimulation in the same organism a great number of times. This failure is partially due, no doubt, to the fact that almost all of the earlier work on the subject involved surgical means of excitation, a method which naturally excludes extensive repetition. Furthermore, the immediate effects are so complicated and then appear in such different forms in different organisms that clear thinking and a concensus of opinion have been almost impossible. We shall, therefore, undertake to excite the vestibular end-organs in the human subject a large number of times in succession and then seek to describe the organic effects issuing from this repeated stimulation.

C. Discussion of Methods.—Various methods are used in the clinical laboratory for the arousal of vestibular activity. The method of rotation was first used by Purkinje, and almost every investigator since his time has used it in one form or another. Bárány adapted it, as well as other methods, to the clinical laboratory. The method of rotation commends itself as preferable to other methods by the ease with which it can be controlled and also by reason of its adaptability.

The galvanic method has likewise had a long history. Purkinje,[1] Remak,[2] Brenner,[3] Hitzig,[4] Kny,[5] Breuer,[6] Jensen,[7] and many others used it for the excitation of 'vestibular sensations.' Of these investigators, Hitzig was the first to give a clear and accurate account of 'galvanic vertigo.' (op. cit., p. 716ff.) Jones[8] describes the recent use of the galvanic method; but the method has not been found so practicable as other methods. The thermal or caloric method has had a similar history. Goltz[9] said in 1870 that the effects of the use of hot and cold water had become common knowledge by that time. Hitzig,[10] Breuer,[11] and Bornhardt[12] have used the caloric method extensively. In case the tympanic membrane is broken, it is possible to excite the canals by pressure.

[1] Purkinje, J., 'Ueber die physiologische Bedeutung des Schwindels und die Beziehung derselben zu den neuesten Versuchen über die Hirnfunction,' *Rust's Mag. f. d. ges. Heilk.*, 1827, 23, 284–310.

[2] Remak, R., 'Galvanotherapie der Nerven und Muskelkrankheiten,' Berlin, 1858.

[3] Brenner, R., 'Untersuchungen und Beobachtungen auf dem Gebiete des Elektrotherapie,' Leipzig, 1868, I, 75ff; II, 30ff.

[4] Hitzig, E., 'Ueber die beim Galvanisieren des Kopfes enstehenden Störungen der Muskelinnervation und der Vorstellungen vom Verhalten im Raume,' *Arch. f. Anat. u. Physiol.*, 1871, 716–772.

[5] Kny, E., 'Untersuchungen über den galvanischen Schwindel,' *Arch. f. Psychiat.*, 1887, 18, 637–658.

[6] Breuer, J., 'Neue Versuche an den Ohrbogengängen,' *Pflüger's Arch. f. d. ges. Physiol.*, 1888, 44, 87–156.

[7] Jensen, P., 'Ueber den galvanischen Schwindel,' *Pflüger's Arch. f. d. ges. Physiol.*, 1896, 64, 182–222.

[8] Jones, I. H., 'Equilibrium and Vertigo,' 1918, pp. 247ff.

[9] Goltz, F., 'Ueber die physiologische Bedeutung der Bogengänge des Ohrlabyrinths,' *Pflüger's Arch. f. d. ges. Physiol.*, 1870, 3, 172–192.

[10] Hitzig, E., *op. cit.*

[11] Breuer, J., *op. cit.*

[12] Bornhardt, A., 'Experimentelle Beiträge zur Physiologie der Bogengänge des Ohrlabyrinths,' *Pflüger's Arch. f. d. ges. Physiol.*, 1876, 12, 471–521.

Alexander,[1] after describing the methods we are discussing, laid claim to the first use of the method of pressure; but Bárány[2] says that several investigators had observed the effects of pressure before Alexander. Mechanical stimulation with needles and by cutting as well as chemical stimulation have frequently been resorted to. Spamer[3] used all of these methods with keen critical ability. From this account, it is apparent, therefore, that Bárány's sole contribution to method has been the adaptation of these historical methods to the clinical laboratory.

II. The Experiments

A. The Method of Periodic Rotation.—Of the various means of exciting the canals which we have just discussed, we shall be concerned alone with the method of rotation. We shall, moreover, insist that the periodic use of this method is opposed in spirit and in practice to the method of single excitations and to observations of the immediate effects of such excitations. We shall find that the use of the method throws an entirely different light on certain present conceptions of the nature both of the organic and of the mental effects of ampullar stimulation. In our experiments, each subject was given a rotation-series once a day (Sundays and a few unavoidable lapses excepted), a series comprising five trials to the right and five to the left, given alternately (*i. e.*, one trial to the right and then one to the left, and so on) and each trial consisting of ten revolutions. For the most part, one minute's rest was given between trials, with three minutes between every five trials. Three or four subjects were used simultaneously, the series being continued in each case so long as the subject was available.

B. The General Procedure.—For the purposes of our experiments, a modified Bárány rotating chair was used. This chair, which is described by Jones,[4] has suffered certain

[1] Alexander, G., 'Die Funktion des Vestibularapparates,' *Bericht u. d. IV. Kongress f. exper. Psychol.*, 1911, 74–94.

[2] Bárány, R., see Alexander, G., *op. cit.*, p. 91.

[3] Spamer, C., 'Experimenteller und kritischer Beitrag zur Physiologie des halbzirkelformigen Kanale,' *Pflüger's Arch. f. d. ges. Physiol.*, 1880, 21, 479–590.

[4] Jones, I. H., *op. cit.*, pp. 233ff.

American modifications but these are of doubtful value for research. The chair used in our experiments was especially designed for research by Professor Bentley.

In the first place, the chair was so constructed as to enable the operator to stop it gradually by a friction-brake rather than suddenly as in certain clinical models. The reason for and significance of this provision will presently appear. The chair was also insulated and wired in such a manner that the observer could graphically register, by the pressure of a key placed in his hand, either during or after rotation, any time intervals or events which he might from time to time be instructed to note and to record. In addition, contact points were adjusted at intervals of 90 degrees about the base of the chair so that its rate of revolution could be indicated on the smoked drum. By comparing the records from these contact points with the records made from a second's pendulum, it was possible to find that the chair was rotated with an average mean variation of less than one-tenth of a second per revolution.[1] By means of two projections, one in front and one at the back, an almost continuous impulsion was given to the chair. In this way, jarring and irregularities in the rate of rotation were so far reduced that observers repeatedly described the motion as similar to that of a well-balanced object along a smooth and oily surface.[2] A click from the second's pendulum and the visual cue from its swing gave to the experimenter a wholly dependable kinæsthetic and visual rhythm, making it possible to start and to stop the chair and to maintain its speed with a negligible degree of variation. The records of the smoked drum indicated that the chair attained full speed at the end of one second, the mean variation of error being less than one fifth of one second. The regular rate of rotation—one turn in two seconds for ten

[1] Dunlap asserts that the variation in the clinical laboratory has been as great as 13–27 sec. when a 20 sec. period was prescribed. See *J. Amer. Med. Ass.*, 1919, 73, 54.

[2] Absolute control of the rotation is vital if constancy is to be expected. The otologists have quite overlooked this factor, as is betrayed by the unevenness of the rotation and the abrupt stop. Several observers in this investigation who have taken the otological examination for Army Aviation declare that the rotation here given was far better controlled than that given during the official tests.

turns—was then maintained until the stop, which was accomplished evenly and with a mean variation of approximately one ninth of one second of error. A push-button connected with a magnetic signal marker enabled the experimenter to register on the smoked drum the number of eye-movements made after the chair came to rest. In a similar way was registered the time at which all apparent movement in the observer's visual field ceased. Two kinds of apparent movement in the visual field follow rotation. (1) An easily recognized jerky movement of objects, which is a function of the eye-movements described above, is followed (2) by a 'flowing' in the visual field which persists for several seconds. The visual field then clears up, save for whatever after-images may have been induced by the prolonged period of fixation. The disappearance of the jerky movements was alone considered in making our reports. (See Plate I, *a*, *b*.).

As regards the registration of the number of eye-movements, the following procedure was adopted. An Appunn lamella used for practice purposes served also to determine the accuracy of the experimenter in counting the number of eye-movements. Vibration-rates up to seven a second were accurately counted, and by counting every other vibration of the lamella the experimenter was able to record with accuracy rates up to twelve in the second, and considerably beyond with a small calculable error. The eye-movements are easier to record than are the vibrations of the lamella and, moreover, within the group of observers here reported, none showed an original rapidity of movement greater than five in the second. There were thus three values obtained from each subject as the series proceeded; (1) the subject's own record of the cessation of apparent movement of objects, (2) the record of the eye-movements made, and (3) of the duration of the after-nystagmus. Measurements of the amplitude of the movements were made with the aid of a reading microscope.

The method of treating these records can be seen in Table I, which is chosen at random from the original records. The table represents a single day's rotation-period of ten turns, five to the right and five to the left, done alternately. It can be seen that for a decreasing group of measures the m.v.'s are relatively small. The regularity with which the first measures exceed and the last measures fall short of the average indicates throughout the investigation the regularity with which the decrease takes place. That is to say, these mean variations are, for the most part, an index of the regularity of decrease in the measures averaged and are not a true measure—as the m.v. should be—of reliability.

TABLE I

Trial	Rotation to Right						Rotation to Left					
	Time	M.V.	No.	M.V.	App. Mvt.	M.V.	Time	M.V.	No.	M.V.	App. Mvt.	M.V.
1	19.0	2.8	38.0	1.6	19.0	2.6	20.0	3.4	39.0	2.8	22.0	4.8
2	16.0	0.2	39.0	2.6	17.0	0.6	18.0	1.4	39.0	2.8	18.0	0.8
3	16.0	0.2	37.0	0.6	16.0	0.4	16.0	0.6	37.0	0.8	17.0	0.2
4	17.0	0.8	34.0	2.4	16.0	0.4	15.0	1.6	35.0	1.2	15.0	2.2
5	13.0	3.2	34.0	2.4	14.0	2.4	14.0	2.6	31.0	5.2	14.0	3.2
Average	16.2	1.44	36.4	1.92	16.4	1.28	16.6	1.42	36.2	2.56	17.2	2.24
Aver. R. and L.	.....	.....	....	....	.....	....	16.4	1.43	36.3	2.24	16.8	1.76

A fixation point was provided on a large black background upon which were placed vertical white strips 12 mm. wide and 25 mm. apart. The fixation point was a small cross in the center of the field set on one of the strips of white and was 1 m. from the eyes of the subject. These vertical strips enabled the subject to indicate the cessation of apparent movement with much more ease and accuracy than he could have done with the otologist's indefinite fixation outside a window.[1] The position of each observer was held constant by a rod adjusted to the side of the chair in such a way that it stood at an angle of 30 degrees from the vertical. The head of the individual was then placed in such a position—by the use of an improved head-rest—that a line connecting the middle of the shoulder with the middle of the ear was

[1] As a matter of fact, we shall show in a later paper that fixation is an important item in the conditions that govern the appearance of the organic as well as the mental effects of rotation. Fixation at some point out of a window as used by the otologists in the official tests of the Army Air Service, *e.g.*, is extremely inaccurate and undependable.

parallel with the rod. It was thus made certain that the head of the subject was always in such a constant position that the horizontal canals were approximately parallel with the floor and at right angles to the axis of rotation. After the 'ready' signal and at the 'now' signal, the subject was turned ten times in twenty seconds[1] and stopped by means of the brake so that a line on the foot-rest of the chair exactly coincided with the line placed on the floor. After a little practice, the experimenter found that this could be accomplished with an average error of less than 2 cm.

C. The Subjects.—The subjects used during these experiments were chosen at random from the various courses in psychology and were for the most part upper-classmen.[2] Before using them in the experiments, they were given the usual turning tests for 'vestibular normality.' That is to say, the clinical laboratory has been working on the assumption that if the ocular movements following rotation persist for an average time of 25 sec., in so far forth, the subject is 'normal.' Usually, however, further tests are given such as

[1] There is nothing sacred about this rate of rotation, although some of the otologists seem so to regard it. The appearance of the effects of rotation is directly proportional to the rate and the number of turns. That is to say, one revolution at any ordinary speed will not, as a rule, produce nystagmus, but two turns may. The amount of nystagmus then increases as the number of turns increases. The ratio has not been established and neither have the limits, although it is known that the duration of the after-nystagmus increases with a constant rate of rotation (1 rev. in 2 sec.) up to fifteen turns. Bárány found that the after-nystagmus suffers decided change in kind when the number of rotations is made as high as sixty. Ocular and other organic effects generally appear in an unpracticed individual after two complete turns. The speed of ten rotations in 20 sec. was chosen partly because it admitted adequate comparison of the results with those of other investigators and partly because the effects aroused are of sufficient intensity to furnish suitable conditions for observation and description. Any further increase in the intensity of the effects would tend to defeat the purpose of the experiment.

[2] The writer wishes to acknowledge his appreciation of the services of all the subjects who willingly submitted themselves to the trying conditions of the experiment. The names follow: V. B. Adams, H. C. Burleson, J. W. Cannon, L. K. Cecil, S. M. Dietrich, A. R. Elliott, W. H. Griffith, D. T. Harris, H. O. Hope, S. C. Nag, L. R. Raines, W. H. Rayner, J. A. Sanders, A. D. Sinden, H. M. VanDoren, R. W. Wuestermann. H. C. Burleson acted as experimenter while the writer was a subject. The writer wishes also to express his sincere appreciation of the aid and counsel of Professor M. Bentley, under whose direction and criticism this investigation was undertaken and carried through.

the 'past-pointing' test and the 'falling' test. Both of these tests depend upon systemic innervations from the end-organs in the canals. (See Jones, I. H., 'Equilibrium and Vertigo,' 1918, *passim.*) An otologist who had been selected by the Surgeon General's Office to give tests to candidates for the Air Service pronounced all our subjects 'normal' after reviewing our results and giving some of the tests himself. He assured us that our subjects gave an average after-nystagmus of acceptable duration; that they past-pointed to the right and to the left on an average of two or three times; and finally that they fell to the right and to the left in a pronounced manner.

III. The Organic Results of Rotation

Under the experimental conditions just described, there is produced, both during and after rotation, an elaborate array of organic and mental effects. In the naïve subject all of these effects are usually of great severity and complexity. Occasionally they become overwhelming, as in nausea.[1]

A. Local Effects: Nystagmus.—All of the organic effects have commonly been assigned, without much criticism or analysis, to two gross classes. As we have just seen, the unfailing regularity with which the ocular effects appear has earned for them a separate place. These effects, which are sometimes called 'vestibular nystagmus,' are characteristic. As rotation begins, the eye tends to lag behind the movement of the head, and then suddenly to recover its original position, only to lag behind again, and so on. After the cessation of the rotatory movement, the order of the ocular deflections is just reversed. The slow phase, which took place in the direction opposite to the original rotation, now takes place in the direction of the bodily movement. The quick phase, which previously took the direction of rotation, now runs

[1] During the course of our experiments three subjects were obliged to discontinue the series temporarily, on the first day, because of nausea. In one case only, however, did vomiting take place. Almost all the subjects were slightly nauseated on the first day but rarely on the second. As has been frequently noted, all of the organic effects are tremendously increased in intensity if the axis of position is changed at any time during rotation or during the persistence of the after-nystagmus. One subject (*O*) responded to the falling test by repeated clonic contractions of the lower limbs.

in the contrary direction. In case the head is bent forward or to the side, the movement is circular but of the same general kind. We are here concerned only with the horizontal movements aroused by rotation about an axis lying in or near the head. Furthermore, the ocular effects will make up the bulk of our experimental evidence on the nature of the organic effects of rotation and we shall be primarily concerned with the movements occurring after rotation ceases.

B. Systemic Effects: Muscular Innervations: Visceral and Vascular Changes.—The other organic effects which have come to occupy a prominent place are certain innervations of the limbs and of the body as a whole due to a diffuse involvement of the whole skeletal musculature. If the limbs or the head are held free from support, they tend to move in a characteristic manner. The head may turn to one side or to the other, depending upon the direction of bodily rotation, and the limbs on the side of the body toward the rotation tend to move in the same direction. Furthermore, the immanence of nausea, of excessive perspiration, and the 'feel' of internal changes in the viscera, the lungs and the head suggest that the excitation of the canals has a direct influence on many of the internal organs and upon the autonomic system.

Aside from these general organic effects, which we shall consider in a secondary way only, there is a large class of effects that are commonly dismissed as 'mental.' They are usually spoken of as 'vertigo,' 'dizziness' and nausea. As a matter of fact, analysis reveals a host of kinæsthetic sensations localized less clearly about the trunk and the limbs than about the neck, the face, and especially the eyes. The trained observer discovers a background of organic processes which seem finally to become localized about the lower end of the esophagus. Visceral coolness and pressure, clearer than the other processes, are occasionally accompanied by a 'bad taste' in the mouth, an excessive flow of saliva, hints of a disagreeable olfactory quality, and a diffuse 'feel' of peripheral warmth; and the culmination of the whole experience may come in the form of kinæsthetic pulls and tensions throughout the viscera and abdominal walls—the processes characteristic of nausea and reversed peristalsis. In a subsequent investigation we shall undertake in a serious manner to analyze these psychophysical factors and to point out their relation to the whole experience.

IV. Physical and Physiological Factors which Modify the Organic Results of Rotation

We come now to the central part of our experimental enquiry. The whole course of the investigation goes to show that nystagmus is only a small part of the total organic effect of rotation[1] and that all of the effects, both local and systemic, tend to decrease in complexity and in intensity and finally to disappear under the influence of prolonged turning. That is to say, our investigation shows that these effects can be profoundly modified in their appearance by certain physical and physiological factors which we shall immediately describe.

A. Modification by Periodic Repetition.—Of the progressive changes occurring on the organic side, the most outstanding is the decrease from day to day and the decrease within a single diurnal series of the duration of the after-nystagmus, of the number and the amplitude of the ocular movements, and finally, of the time of apparent movement in the visual field, due to periodic repetition. Tables II. and III. give, for each of 16 subjects, the averages for rotation alternately to the right and to the left in the successive daily series. Figure 1 is a typical curve based on the results obtained from subject *K*.

When Tables II., and III., and Fig. 1 are carefully scrutinized the following facts appear:

(*a*) Every observer shows more or less decrease in the duration of nystagmus, in the number of ocular movements,

[1] The otologists have drawn a peculiar and curious distinction between nystagmus and the other effects of rotation, principally vertigo. For example, Jones (*op. cit.*, p. 5) says that "ear-stimulation produces certain definite phenomena—a rhythmic jerking of the eyes known as nystagmus, and a subjective sensation of turning which may be termed a systematized vertigo. The two distinct phenomena then are nystagmus and vertigo." We shall have occasion to discuss this distinction at another time. At present we must insist that the distinction is but a gross logical separation of the effects induced.

TABLE II

AVE. TIME (SECS.) OF AFTER-NYSTAGMUS FROM TEN ROTATIONS (5 TO R. AND 5 TO L.) OF TEN REVOLUTIONS EACH, FOR SIXTEEN SUBJECTS (*A–P*).

Series	Subjects																																	
	A	*B*	*C*	*D*	*D* Con.	*E*	*E* Con.	*F*	*F* Con.	*F* Con.	*F* Con.	*F* Con.	*F* Con.	*G*	*G* Con.	*G* Con.	*G* Con.	*G* Con.	*H*	*H* Con.	*I*	*J*	*K*	*K* Con.	*L*	*L* Con.	*M*	*M* Con.	*N*	*N* Con.	*O*	*P*	*P* Con.	*P* Con.
1	17.9	17.6	22.9	16.4	5.1	17.2	2.9	18.5	10.9	9.1	9.0	6.9	5.3	17.6	10.2	7.3	4.7	1.6	24.5	4.2	14.4	20.0	21.5	7.3	18.2	4.1	21.7	6.9	18.7	8.7	15.9	25.5	8.7	3.7
2	18.4	8.7	22.2	13.5	6.1	14.9	3.4	18.3	9.8	9.6	9.5	6.0	4.8	16.4	10.8	7.7	5.0	1.0	19.6	4.1	15.0	14.5	21.5	5.5	16.5	3.6	18.7	8.9	18.2	6.9	7.3	27.0	7.5	2.5
3	17.2	6.4	21.9	12.3	6.7	14.7	2.6	16.4	10.4	9.9	8.1	6.9	4.1	15.5	10.0	7.7	4.3	1.0	16.6	3.7	13.8	17.7	22.4	5.6	12.0	2.9	16.4	5.8	16.7		6.0	25.2	5.5	1.0
4	14.6	6.4	23.7	14.7	6.0	12.0	2.2	13.5	9.3	9.2	8.3	6.3	4.5	14.0	9.8	8.0	5.0	0.7	13.5	3.5	12.1	12.1	17.1	5.3	15.1	3.1	15.3	6.4	15.6		3.3	22.3	6.7	0.0
5	13.0	4.8	21.9	15.5	5.6	10.5	0.7	14.8	9.4	9.4	7.6	6.4	4.0	13.5	8.9	8.8	4.0	0.2	11.8	2.6	14.0	11.8	16.2	3.6	12.5	2.8	15.5	6.3	14.8		1.9	18.3	6.5	0.0
Av.	15.8	8.8	22.5	14.7	5.9	13.9	2.3	16.3	10.0	9.4	8.5	6.5	4.5	15.4	9.9	7.9	4.6	0.9	17.2	3.6	13.9	15.2	19.7	5.4	14.8	3.3	17.5	6.8	16.8	7.8	6.9	23.7	7.0	1.4
6	9.6	4.6	19.4	13.1	6.6	8.5	0.0	13.5	10.4	8.8	8.4	5.6		12.6	9.3	7.1	3.6	0.0	10.4	1.3	12.1	10.4	15.3	3.0	11.3	2.0	14.3	6.8	13.9		1.5	16.8	6.3	
7	8.0	2.5	18.5	12.9	5.3	8.7	0.0	12.4	10.6	9.0	7.4	6.3		10.8	8.4	7.7	2.8	0.0	8.6	0.0	12.6	11.1	14.2	3.8	11.5	3.0	18.1	9.6	13.5		1.2	16.9	5.1	
8	7.3	3.1	18.6	12.0	6.8	7.2		11.4	10.4	8.9	8.3	5.4		10.0	8.5	5.8	3.8		9.1	0.0	11.1	11.4	12.4	2.9	10.5	1.6	12.3	10.1	13.3		1.5	14.2	5.4	
9	8.8	2.3	20.4	9.4	6.3	7.6		11.3	10.0	9.7	7.3	6.1		10.7	8.5	5.7	2.8		7.5	0.0	10.7	11.8	11.3	2.6	9.2	2.1	11.3	6.1	10.3		0.1	13.2	5.3	
10	8.5		17.2	8.2	7.4	6.4		13.6	11.1	8.4	7.8	5.0		11.4	8.9	5.5	3.3		8.3		10.0	11.4	10.2	2.1	9.0	0.6	10.1	4.9	11.9		0.0	11.9	5.7	
Av.	8.4	3.1	18.8	11.1	5.3	7.7		12.4	10.5	9.0	7.8	5.7		11.1	8.7	6.4	3.2		8.8		11.3	11.2	12.7	2.9	10.3	1.8	13.2	7.5	12.6		0.8	14.6	5.6	
11	7.3		18.2	8.4	7.5	6.5		11.5	9.5	9.1	6.5	6.1		11.1	9.1	6.4	2.8		7.1		12.5	11.0	11.1	2.4	7.9	0.0	11.1		9.4			10.5	4.7	
12	6.7		18.9	8.8	4.6	5.7		11.4	10.4	8.2	7.0	5.4		10.3	8.5	5.4	2.3		7.6		10.6	10.9	9.5	1.8	7.2	0.0	8.5		9.5			11.8	4.1	
13	6.2		17.4	9.1	5.8	6.9		10.7	10.1	10.3	6.8	5.5		11.4	8.9	5.5	2.2		6.7		9.7		9.0	1.2	6.4		11.6		9.6			11.2	4.2	
14	4.8		15.9	7.8	3.8	5.3		11.5	10.7	7.9	7.2	4.8		10.4	8.4	5.4	2.3		5.6		9.9		7.2	0.0	5.2		7.3		8.9			7.3	3.6	
15	3.8		14.6	7.0	3.0	5.3		9.8	9.7	9.8	6.2	5.1		10.1	7.9	5.4	2.0		4.7		10.2		7.3	0.0	5.3		9.1		7.8			7.3	3.5	
Av.	5.5		17.0	8.2	4.9	5.9		11.0	10.1	9.1	6.7	5.3		10.6	8.4	5.6	2.3		6.3		10.6	10.9	8.8	1.1	6.4		9.5		9.0			9.6	4.0	
Decrease (sec.)	14.1	15.3	8.3		13.4		17.2						14.5					17.6		24.5	4.2	9.1		21.5		18.2		16.8		11.8	15.9			25.5
Decrease (%)	80	87	36		81		100						84					100		100	29	45		100		100		77		63	100			100
Rate of decrease per trial	.94	1.70	.55		.45		.82						.18					.27		1.11	.28	.75		.77		.73		.67		.70	1.59			.78

Table III

Ave. No. of Ocular Movements from Ten Rotations (5 to R. and 5 to L.) of Ten Revolutions Each, for 16 Subjects (*A–P*).

Series	Subjects: *A*	*B*	*C*	*D*	*D* Con.	*E*	*E* Con.	*F*	*F* Con.	*F* Con.	*F* Con.	*F* Con.	*F* Con.	*G*	*G* Con.	*G* Con.	*G* Con.	*G* Con.	*H*	*H* Con.	*I*	*J*	*K*	*K* Con.	*L*	*L* Con.	*M*	*M* Con.	*N*	*N* Con.	*O*	*P*	*P* Con.	*P* Con.
1	15.5	10.9	36.5	8.2	3.8	20.2	2.9	39.0	25.0	13.6	11.2	9.5	6.9	34.5	17.7	10.9	5.2	1.8	56.1	5.3	20.8	47.9	32.5	10.6	26.5	5.5	35.2	9.7	35.0	13.6	10.8	47.5	7.0	2.6
2	20.3	4.3	40.5	5.5	5.0	22.6	3.1	39.5	20.3	12.6	13.6	6.7	5.2	31.2	18.6	10.9	5.2	1.2	51.3	4.3	25.4	26.8	30.4	7.2	28.0	5.2	30.7	11.0	30.2	9.9	8.1	37.0	5.4	2.0
3	18.0	2.3	38.8	5.1	5.5	16.3	2.7	36.3	22.8	16.6	10.5	9.1	4.8	31.0	17.3	11.0	4.3	1.2	38.7	4.1	20.6	39.9	40.4	6.2	18.6	4.8	24.9	7.2	35.2		5.8	40.2	4.6	0.5
4	14.1	2.6	39.9	5.9	4.7	13.0	1.9	30.7	15.7	14.2	12.3	7.6	6.0	26.6	18.3	11.7	5.2	0.7	31.2	3.7	24.6	25.3	30.5	8.1	28.4	4.7	25.6	8.0	27.5		4.0	35.8	5.7	0.0
5	11.9	4.0	40.8	7.2	4.7	11.0	0.7	33.1	17.8	16.4	10.7	8.2	4.0	26.9	14.5	11.7	3.8	0.2	27.2	2.8	25.6	29.1	29.5	4.4	20.6	3.7	25.9	7.4	25.3		2.0	27.0	6.2	0.0
Av.	15.9	4.8	39.3	6.4	4.7	16.7	2.2	35.7	20.3	14.7	11.7	8.2	5.4	30.0	17.3	11.2	4.7	1.0	40.9	4.0	23.4	33.8	32.6	7.3	24.4	4.8	28.4	8.6	30.6		6.1	37.5	5.8	1.0
6	7.9	2.6	34.9	5.2	5.8	9.5	0.0	30.6	18.7	13.3	13.8	6.6		25.0	15.2	11.1	3.7	0.0	22.9	1.5	26.6	24.6	24.0	3.3	21.0	2.9	23.7	5.3	24.8		1.7	25.2	5.2	
7	5.0	2.1	34.7	6.3	4.6	9.9	0.0	27.5	15.4	14.8	9.8	8.0		19.9	12.9	10.2	2.8	0.0	18.5	0.0	25.2	22.3	20.0	5.1	19.1	3.6	28.1	11.0	20.8		1.6	20.5	4.5	
8	4.5	3.3	37.4	6.1	5.9	7.9		24.5	19.3	10.6	13.4	7.3		17.4	13.1	7.5	4.0		16.6	0.0	21.8	26.1	20.9	3.3	19.0	1.9	17.3	11.2	20.7		1.4	19.1	4.7	
9	9.9	2.8	37.4	4.5	4.6	7.7		27.2	15.0	14.3	9.3	8.2		18.1	11.9	7.2	3.0		11.4	0.0	22.0	30.7	19.1	2.9	16.9	2.5	14.5	6.9	17.7		0.1	18.2	4.7	
10	8.4		30.5	3.8	5.9	5.2		32.1	18.9	11.1	12.8	7.2		20.0	13.4	7.8	3.3		12.1		18.4	27.4	17.2	2.0	15.4	0.5	13.7	5.2	16.1		0.0	16.3	4.4	
Av.	7.1	2.7	35.0	5.2	5.7	8.0		28.4	17.5	12.8	11.8	7.4		20.1	13.3	8.7	3.4		16.3		22.8	26.0	20.2	3.3	18.3	2.3	19.4	7.9	20.0		1.0	19.8	4.7	
11	8.2		33.3	3.5	6.9	6.5		26.7	15.2	18.3	8.3	7.9		18.1	13.3	7.4	2.9		12.0		25.0	26.2	15.5	2.4	12.3	0.0	15.7		14.7		0.0	12.6	4.0	
12	7.9		33.4	3.7	3.1	5.1		28.6	16.8	12.9	11.3	6.4		17.5	11.4	6.3	2.5		10.6		18.4	22.3	14.0	1.8	11.5	0.0	11.6		13.7			15.5	3.6	
13	5.1		32.4	3.6	5.1	6.1		24.9	17.2	18.0	8.9	6.2		19.2	12.8	6.5	2.3		9.6		16.0		13.1	1.3	10.6		18.0		13.4			10.9	3.2	
14	4.9		30.4	6.1	2.6	4.2		29.8	17.9	11.0	11.1	6.2		17.9	12.0	5.4	2.2		7.0		18.0		9.1	0.0	7.7		11.4		12.2			5.2	3.3	
15	3.6		29.5	3.5	1.9	4.8		20.4	14.2	15.1	7.9	6.8		17.1	11.6	6.2	1.9		6.2		15.2		9.2	0.0	7.8		11.6		11.4			5.5	2.6	
Av.	5.9		31.8	4.1	3.9	5.4		26.1	16.3	15.1	9.5	6.7		18.0	12.2	6.4	2.4		9.0		18.5		12.2	1.1	10.0		13.6		13.1			9.9	3.3	
Decrease (No.)	11.9	8.1	7.0		6.3		20.2						35.0					34.5		56.1	5.6	25.6		32.5		26.5		30.0		25.1	10.8			47.5
Decrease (%)	76	74	19		77		100						90					100		100	27	53		100		100		85		72	100			100
Rate of decrease per trial	.79	.90	.47		.21		.92						.37					.51		2.55	.37	2.13		1.17		1.06		1.20		1.48	1.20			1.40

and in the time of apparent movement in the visual field.[1] It will be observed that the three representative values decrease together. The decrease in duration of nystagmus varies from 29 per cent. in the case of subject *I*, to 100 per cent. for subjects *E*, *G*, *H*, *K*, *L*, *O* and *P*. The average amount of decrease for all subjects in these tables turns out to be 79 per cent. or more than three fourths of the initial time. Furthermore, there is every reason to believe that had the several series been continued sufficiently, all subjects would have decreased 100 per cent. The decrease in the number of movements varies from 19 per cent. for subject *C* to 100 per cent. for subjects *E*, *G*, *H*, *K*, *L*, *O* and *P*; again, an average of 79 per cent. The same ratio holds true also for the duration of the apparent movements in the visual field.

Fig. 1.

(*b*) There is a wide individual difference in the initial values. Subject *I* gave an average nystagmus time for the first ten trials of 14.4 sec. while subject *P* gave 25.5 sec. It will be observed that most of these initial values are below those given by the otologists as constituting 'normality.' It will be recalled that this 'normality' value was about 25 sec. As a matter of fact, the average of all our subjects for the first ten trials is but 18.1 sec. This one comparison is an outstanding example of the sensitivity of the ocular responses to repetition. The low average found in our case is due to the fact that our initial values are an average of the first *ten* rotation periods of ten revolutions each. Within these ten

[1] As can be seen from Fig. 1, the line showing the time of apparent movements almost coincides with the line showing the time of nystagmus. That is to say, a table similar to Tables II. and III., but giving these times of apparent movement, would almost duplicate Table II., and we have not, therefore, included such a table.

TABLE IV

SHOWING, FOR EACH OF 16 SUBJS., TIME OF AFTER-NYSTAGMUS, NO. OF OCULAR MOVEMENTS, AND TIME OF APPARENT MOVEMENT IN THE VISUAL FIELD FOR (*a*) THE FIRST TRIAL TO THE RIGHT AND TO THE LEFT, (*b*) THE AVERAGE OF THE FIRST TEN TRIALS (5 R. AND 5 L.), AND (*c*) THE LAST TRIAL TO THE RIGHT AND TO THE LEFT. A TRIAL CONSISTS OF TEN REVOLUTIONS.

		Subjects																
		A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*	*M*	*N*	*O*	*P*	Ave.
Time of After-nystagmus (secs.)	First Trial R.	17.0	25.0	31.0	18.0	16.0	18.0	19.0	31.0	22.0	23.0	20.0	22.0	21.0	19.0	26.0	32.0	22.5
	First Trial L.	22.0	20.0	23.0	24.0	20.0	27.0	17.0	25.0	23.0	29.0	24.0	20.0	26.0	22.0	22.0	30.0	23.4
	Av. 1st 10 Trials	17.9	17.6	22.9	16.4	17.2	18.5	17.6	24.5	15.4	20.0	21.5	18.2	21.7	18.7	15.9	25.5	18.1
	Tenth Trial R.	15.0	14.0	22.0	11.0	12.0	16.0	18.0	24.0	15.0	17.0	20.0	13.0	18.0	16.0	11.0	21.0	16.4
	Tenth Trial L.	17.0	14.0	19.0	24.0	16.0	16.0	16.0	20.0	13.0	18.0	20.0	18.0	14.0	17.0	8.0	21.0	16.8
No. of Ocular Movements	First Trial R.	13.0	15.0	55.0	10.0	20.0	38.0	39.0	58.0	25.0	53.0	44.0	37.0	36.0	38.0	20.0	50.0	34.4
	First Trial L.	24.0	12.0	42.0	12.0	21.0	46.0	35.0	50.0	23.0	71.0	41.0	25.0	44.0	31.0	18.0	52.0	34.2
	Av. 1st 10 Trials	15.5	10.9	36.5	8.2	20.2	39.0	34.5	56.1	20.8	47.9	37.5	26.5	35.2	35.0	10.8	47.5	30.1
	Tenth Trial R.	11.0	9.0	32.0	5.0	17.0	38.0	33.0	61.0	21.0	37.0	33.0	21.0	28.0	29.0	7.0	50.0	27.0
	Tenth Trial L.	15.0	8.0	30.0	10.0	18.0	35.0	25.0	51.0	18.0	45.0	31.0	27.0	24.0	29.0	7.0	38.0	25.6
Time of Apparent Movement (secs.)	First Trial R.	17.0	20.0		17.0	16.0	20.0	19.0	30.0	22.0	25.0	20.0	23.0	20.0	19.0	26.0	32.0	21.7
	First Trial L.	20.0	18.0		24.0	20.0	20.0	18.0	25.0	21.0	30.0	24.0	20.0	22.0	18.0	22.0	30.0	22.1
	Av. 1st 10 Trials	16.7	15.9		17.1	18.0	18.6	17.1	24.1	11.7	19.6	21.7	16.7	19.7	17.7	15.7	26.5	18.4
	Tenth Trial R.	15.0	14.0		12.0	14.0	16.0	18.0	24.0	13.0	16.0	20.0	9.0	17.0	16.0	11.0	24.0	16.7
	Tenth Trial L.	15.0	11.0		22.0	19.0	16.0	13.0	20.0	9.0	16.0	20.0	10.0	13.0	17.0	8.0	21.0	16.0

rotation periods the decrease is already so great as appreciably to lower the average time of nystagmus.

By referring to Table IV. we get a more detailed analysis of the first ten trials for our sixteen subjects. It will be seen from this table that the average initial nystagmus times for these subjects were R. 22.5 sec., and L. 23.4 sec., values which fall close to the median assumed by otologists to be "normal."[1] The striking fact, however, is the decrease within the first few minutes of rotation, a decrease which is apparently due to the cumulative effect of repetition.

(*c*) The decrease in all the values is of a characteristic kind. We have just pointed out the marked decrease within the first ten trials. Reference to any of the preceding tables or to the figure will show that a large part of the decrease takes place in the first few days. That is to say, there is a sudden drop in the curves which represent the temporal course of the nystagmus from day to day. We have found that all the subjects showed an average decrease of 79 per cent. in the time of nystagmus. But an average decrease of 57 per cent. occurs during the first half of the series, leaving the remaining 22 per cent. to be distributed over the last half of a series. Table V. shows, in percentages, this decrease

TABLE V

Subjects	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*	*M*	*N*	*O*	*P*	Av.
% decrease first half.	55	72	19	54	63	50	56	71	35	50	66	60	63	41	90	70	57

[1] In view of the otological practice, these standard or 'normal' times should not be taken too seriously. In one list of about 1800 'official' examinations the duration of the after-nystagmus actually observed (omitting the extreme times) ran as follows (after one turn of 10 revolutions to the right):

Duration.	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31 sec.
No. examined . .	32	70	41	137	24	*239*	49	137	102	137	146	158	54	66	27	98	10

The greatest number of cases for 'normal' men seems, then, to fall, not at 25 sec. or 26 sec., as we should expect, but at 20 sec. The table shows a striking distribution about 20 sec. and 30 sec. Possibly the otologist's watch has a preference for round numbers. If it has, it yields in a fair and openminded way at 26, the exact mean in the 'allowed' normal range of 16—36, and also at 16, the lowest permissible 'normal' for aviators. The sudden drop on either side of 16 sec. would seem to emphasize the high clinical importance of that number; while the small number of 15-sec. cases would seem to suggest that Nature does her best, in a great human emergency, to follow the prescriptions and to exemplify the discoveries of her learned children.

during the first half of all the series of the sixteen subjects. These figures are more significant than at first appears. Only a few of the series were carried to completion, *i.e.*, to zero. On this account, the distribution of the amount of decrease cannot be fully represented. In those cases where the series did decline to zero the average decrease for the first half of the trials was 70 per cent.

Another indication of the difference in the rate at which decrease occurs early and late in a series is found in the size of the corresponding mean variations. Whereas the m.v. usually runs 1.5–2.0 early in a series, it hardly ever rises above 0.5 late in a series. We have already pointed out that the m.v. depends primarily upon a constant decrease in the measures averaged and not upon irregularities due to the method of observation.

(*d*) The rate of decrease is, again, a matter of individual differences. Subj. *B* decreased at the rate of 1.7 sec. per trial, while Subj. *F* decreased at the much slower rate of .2 sec. per trial. The same relative values hold also for the number of movements and for the duration of the apparent movement. Here again, the rate of decrease is greater during early trials than later in the series. In the cases just mentioned, the rate of decrease for the first ten series in Subj. *B* was 2.5 sec. per trial and in Subj. *F* it was .7 sec. per trial. For the last ten series, Subj. *B* showed a rate of .5 sec. and Subj. *F* but .01 sec. per trial.

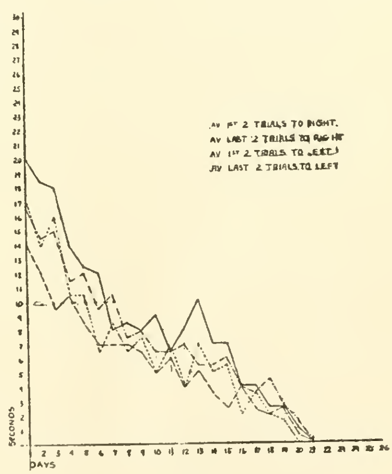


Fig. 2

Other averages testify to the decrease of all values from day to day. (1) If the first trials of each day are thrown together, it will be found that the first trial of any single day is not so large as the first trial of the day preceding, although it may be a little larger than the *last* trial of the preceding day. That is, there seems to be a slight tendency to revert to the original values because of the lapse of the 24 hours. This factor will be studied in detail below. The decrease from day to day as shown by all first and last turns of rotation to the right and the same for rotation to the left is presented in Fig. 2 (Subj. *E*). This again is a typical curve. It exemplifies the various facts and regularities of decrease from day to day.

(2) There is, however, a still more striking statement of the sensitiveness of nystagmus to practice than we have yet given. So far we have mainly considered decreases from day to day. Table VI. shows decreases that also take place within a single day. Here again the decrease is of the same kind as that already described. It will be noted that the total decrease during the first five days is notably greater than during the last five days.

Table VI

Subject *E*

Amount of decrease in time of nystagmus within single rotation-periods of ten trials each. Averages of the first two trials to the right and to the left on the first five days are compared with the averages of the last two trials to the right and to the left during the same five days and so on for succeeding five-day periods.

		Right			Left		
		T.	No.	A.M.	T.	No.	A.M.
1st 5 days...	Av. 1st 2 trials..........	16.6	21.6	14.8	13.9	16.3	14.5
	Av. last 2 trials.........	13.6	16.5	13.2	10.9	12.8	10.1
	Decrease...............	3.0	5.1	1.6	3.0	3.5	4.4
2d 5 days....	Av. 1st 2 trials..........	9.8	11.5	8.7	9.5	9.5	10.2
	Av. last 2 trials.........	7.9	8.9	7.4	7.2	7.3	7.7
	Decrease...............	1.9	2.6	1.3	2.3	2.2	2.5
3d 5 days ...	Av. 1st 2 trials..........	8.1	7.6	8.7	6.1	5.4	6.8
	Av. last 2 trials.........	5.6	4.9	5.7	4.9	3.6	5.0
	Decrease...............	2.5	2.7	3.0	1.2	1.8	1.8
4th 5 days...	Av. 1st 2 trials..........	4.0	4.0	4.7	4.1	3.7	4.7
	Av. last 2 trials.........	2.9	2.6	3.4	2.3	2.2	2.9
	Decrease...............	1.1	1.4	1.3	1.8	1.5	1.8

(3) Again, the decrease of the ocular effects from day to day is demonstrated as follows: The *number* of movements *per second* tends, in most subjects, to decrease along with the more apparent decrease of time-values. This fact is a measure of the decrease in the intensity of the ocular effects, while the preceding facts have been concerned only with the *duration* of the movements and with their *total number*. For example, Subj. *E* decreased from an average of 1.18 movements per second in the first five periods to 0.96 in the last five periods. Subj. *F* decreased from 2.20 to 1.35; Subj. *G* from 1.95 to 1.10; Subj. *J* from 2.76 to 2.27; Subj. *I* from 1.49 to 1.33; Subj. *H* from 2.35 to 1.17. Two subjects showed a slight apparent increase. It was found, however, that these two subjects, who had very wide initial movements, fell into the habit of gazing fixedly in the field of vision, a procedure which decreases the *amplitude* of the eye movements but increases the *speed* and probably the *time* of nystagmus.

(4) And, finally, we discover that the *amplitude* of the eye-movements becomes less and less as a series proceeds. Readings of the after-nystagmus taken through a reading microscope show that the eye movements in some observers decrease from an amplitude of 8 mm. to an amplitude of less than 0.5 mm.[1] For example, in the case of Subj. *O* the amplitude of the movements took the following course:

TABLE VII

Days	Ampl.	Days	Ampl.
1	6.0 mm.	6	1.6 mm.
2	4.0 "	7	1.2 "
3	3.5 "	8	0.8 "
4	2.4 "	9	0.5 "
5	1.9 "	10	0.0 "

So far we have been concerned wholly with the ocular effects of rotation. It has been pointed out, however, that these effects are but a portion of the whole organic group. Now all of the other effects follow the same course as the

[1] See Bárány, R., 'Apparat zur Messung der Rollbewegungen des Auges,' *Zeitschr. f. Sinnesphysiol.*, 1911, 45, 59–62, for a description of an apparatus made and used by him for measuring certain eye-movements.

ocular resultants. Most of the subjects were more or less nauseated during the first trials. Nevertheless, within a day or two, all nausea disappeared for most of the subjects and within three or four days no trace of it was left. The same is true of the tendency to excessive perspiration and of the tendency to hold the breath during rotation.

We have already observed that one of the principal tests for vestibular 'normality' given by the otologists is the test called 'past-pointing.' It was found in the case of naïve observers that they tended after rotation to point past,—either to the right or to the left of,—any object which had been previously designated. We have found, however, that, after practice, subjects no longer tend to 'past-point' in this way. That is to say, it is possible, by measuring the amount of past-pointing, to demonstrate that, as practice continues, the angle of deflection of the hand and arm becomes constantly lessened; so that, finally, there is no deviation at all. A quantitative experiment was performed in the following manner. The subject was instructed to hold his arm loosely in front, a pencil held lightly in his fingers, resting upon a large sheet of paper. He was further instructed to let the arm move as it would and if it should at any time move off the paper, to return as nearly as possible to the starting place and to let the arm again move involuntarily. Under these conditions, it was found that, during early rotational periods, either arm tended to move out and away from the body, no matter what the direction of rotation. During the stopping period, the arm moved in and toward the body for a moment and then resumed its original direction with rapidly decreasing deflection. Finally it was found that these movements tended to drop out. Fig. 3, made up from the actual records, will schematize the facts concerning these movements.

It will be seen from Fig. 3 that the movement of the right arm to the right during rotation to the right must be caused by a definite innervation of the muscles of that arm. On the other hand, the movement of the right arm to the right during rotation to the left suggests that the inertia of the arm is the principal factor. The same is true of the left arm.

The movement of the left arm to the left during rotation to the left seems to be the result of definite innervation, whereas the movement of the left arm to the left during rotation to the right suggests mere inertia. Introspective reports of the difference in the clearness and intensity of the muscular

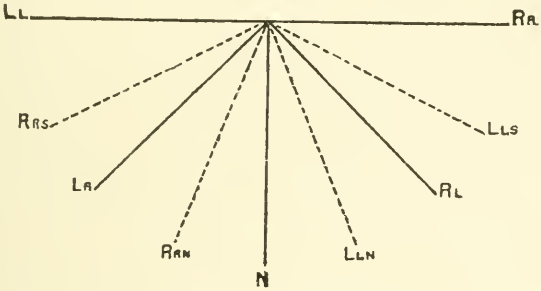


FIG. 3. *N*, the direction of movements made with either the right or the left hand while the body is at rest. *Rr*, direction of movements made by the right hand during rotation to the right. *Ll*, direction of movements made by the left hand during rotation to the left. *Rl*, direction of movements made by the right hand during rotation to the left. *Lr*, direction of movements made by the left hand during rotation to the right. *Lls*, direction of movements made by the left hand just as rotation to the left is ceasing. The direction then changes to *Lln* and finally to *N*. *Rrs*, the direction of movements made by the right hand just as rotation to the right is ceasing. The movements then take the direction of *Rrn* and finally of *N*. As practice continues, *Ll* and *Rr* drop down to *Lr* and *Rl*, then to *Rrn* and *Lln* and finally take the direction of *N* as repetitions increase.

sensations set up during this time bear out the fact that the one is the result of definite innervation while the other is expressive of inertia. The lower limbs tend to behave in this same general manner, *i.e.*, the right leg during rotation in either direction tends to swing to the right while the left leg during rotation in either direction tends to swing to the left.

B. Physical Conditions.—Thus far we have found the appearance of nystagmus to be inversely proportional to the number of turnings. That is to say, the time of nystagmus, the time of apparent movement of objects, and the number of eye-movements decrease very materially from day to day as rotation is continued. So sensitive is nystagmus to practice of this kind that an appreciable decrease in the above values occurs within a single period of ten trials. We have also pointed out certain peculiarities of this decrease. Furthermore, we have shown that the ocular movements decrease in number and amplitude, a fact which is paralleled by the decrease and final disappearance of the other organic effects, such as past-pointing. In other words, nystagmus is not a fixed response to rotation: it is clearly affected by repetition. We now proceed to still further evidences of the modifiability of the ocular responses.

1. *Speed of Turning.*—It has already been suggested that the time of nystagmus is directly proportional to the number and to the speed of turnings. We have found that a single rotation will rarely, if ever, result in any observable effects. After two revolutions, there is usually a brief flicker of the eyes. From this number up to the full number ten, the effects increase rapidly in duration and in intensity. Even after much practice any change in the rate of rotation or in the number of revolutions will change the appearance of the nystagmus. For example, near the end of their respective series, subjects *E* and *H* were rotated twice as fast as in the regular series, *i.e.*, ten times in ten seconds. The values from the regular series at this time for Subj. *E* were 3.4, 3.1, 4.4 and for Subj. *H* 4.2, 5.3, 4.4. After the trials at increased speed the corresponding values for *E* were 14.5, 17.5, 14.0 and for *H* 17.5, 35.0, 15.5. The very large increase under the new conditions is evidence enough that the speed of rotation seriously affects the appearance of the nystagmus even though much practice has already taken place. It is significant, however, that the larger values are but a fraction of the values obtained at a similar speed before any practice.

2. *The Mode of Stopping.*—Subj. *C* was stopped slowly and abruptly on alternate turnings, care being taken that no advantage fell in either direction. The average nystagmus for all slow stoppings was 16.9 sec. and the number of movements was 30.9. For abrupt stoppings the averages were 21.7 and 40.6. That is to say, nystagmus was increased both in duration and in intensity when a subject was abruptly stopped.

3. *The Effect of a Brief Reversal of the Direction of Rotation.*—Returned aviators reported that frequently during the official test given by the otologists the rotating chair had

been allowed to swing past the correct stopping place and had then been brought back to position before readings were taken. Table VIII. shows the results obtained from Subj. *N* by allowing him in alternate trials to move one quarter turn too far in the direction of rotation and then to bring him back to the usual stopping place. The additional time required was one second. It is clear that such a brief return movement serves considerably to decrease the time of nystagmus and the number of eye-movements.

TABLE VIII

Trials	Normal Stop			Excess and Return		
	T.	No.	A.M.	T.	No.	A.M.
First 50	17.5	33.5	17.4	15.7	26.8	15.4
Second 50	14.2	25.7	14.2	10.9	14.4	10.9
Third 50	10.5	16.7	10.7	7.4	9.5	7.5
Final 20	9.3	14.9	9.2	6.3	8.6	6.2

C. Temporal Conditions.—The organic effects of rotation are also modified by temporal conditions. These are of two kinds, *viz.*, the time of day at which rotation takes place, and the interval that elapses between successive rotation periods.

TABLE IX

Five-day Periods	Time	No. of Mvts.	Time of App. Mvt.
1	1.1	2.2	1.1
2	1.3	6.9	1.3
3	0.5	3.5	0.4
Total	2.9	12.6	2.8
4	0.9	3.7	0.9
5	1.7	4.5	1.3
6	1.1	3.0	1.2
Total	3.7	11.2	3.4

1. *The Time of Day.*—Subj. *F* was rotated regularly every morning at 7:30 and every evening at 6:30. Both periods came just after meals. Efforts were made to keep other factors constant. The averages for the morning turnings (32 in all) were 10.4, 19.6, 10.8 and those for the evening

turnings (31 in all) were 9.5, 16.1, 9.9. That is, the values for the evening trials were a little smaller than for the morning trials. That this difference is more significant than at first appears, may be seen by referring to Table IX. The figures in this table represent the average amount by which the evening turnings were less than morning turnings in terms of five-day periods.

2. *The Effect of Intervals.*—(*a*) The effect of short rest intervals between trials. Subjects *E* and *H* were alternately rotated with no rest between successive turnings and with rest (2 min. between R. and L. and three min. before another pair to R. and L.) between successive turnings. Subj. *E* shows that the decrease is faster (measured here by smaller averages) when no rest is allowed between turnings (Table X.). Subj. *H* yields no positive evidence for the effect of

TABLE X

	E			*H*		
	T.	No.	A.M.	T.	No.	A.M.
No Rest						
Av. first 50 trials	9.8	11.6	9.2	13.8	29.4	11.6
Av. second 50 trials	3.5	3.2	3.8	4.6	6.2	4.5
Rest						
Av. first 50 trials	11.7	13.1	11.6	13.1	27.7	12.0
Av. second 50 trials	4.9	4.5	5.4	5.6	5.9	4.7

intervals between trials. In other words, we have a further suggestion that the reduction in the time of nystagmus is dependent not upon fatigue but upon some factors at present unknown.

(*b*) The effect of long rest intervals between series. It was impossible to avoid missing a day or two now and then as the various series went on. We have taken advantage of this fact in Table XI. by counting the number of decreases and increases in the values after no interval and comparing these sums with the decreases and increases falling after a short interval in the series of one or two days. By inspecting the Table (XI.) it will be clear that an interval occurring in a series does not greatly alter the course of the nystagmus. Although there are relatively more increases than there are decreases after an interval than is the case where no interval is involved, yet the difference is not significant. All that can possibly be demonstrated is that the slight diurnal loss of improvement mentioned on p. 34 is a little more evident. On the other hand, we can legitimately cite these facts as further evidence that fatigue does not play a part in the decrease of nystagmus during practice.

Table XI

Effect of a Small Interval of No-practice on the Course of the Nystagmus. The Relative Number of Decreases and Increases in the Time of Nystagmus where no Interval Has Occurred is to be Compared with Similar Values where an Interval Has Occurred

Subject	No Interval						Interval					
	Decreases			Increases			Decreases			Increases		
	T.	No.	A.M.	T.	No.	A.M.	T.	No.	A.M.	T.	No.	A.M.
A........	9	9	8	1	1	2	3	3	3	1	1	1
B........	5	4	4	1	3	3	1	1	0	0	0	1
D........	15	13	15	10	11	9	2	2	2	2	2	2
E........	10	10	11	4	5	3	4	4	4	1	1	2
F........	33	30	28	22	25	27	0	0	0	5	5	5
G........	32	32	33	19	18	18	7	8	7	3	3	4
H........	13	15	15	3	1	1	5	5	4	0	0	1
N........	10	11	10	3	2	3	2	3	3	1	0	0
P........	15	17	15	8	5	8	5	4	5	3	3	3
Q........	14	12	—	6	9	—	3	3	—	5	5	—
R........	9	9	—	9	10	—	4	1	—	1	4	—
S........	11	10	—	3	4	—	4	3	—	1	2	—
T........	9	9	—	6	7	—	1	3	—	2	0	—
W........	26	28	27	16	16	15	4	6	6	6	6	6
D........	22	25	20	8	6	10	2	2	2	5	5	5
Total....	233	234	186*	119	123	99*	47	48	36*	36	37	30*

* These values are small because subjects *Q*, *R*, *S*, and *T*, were rotated with lenses and it was, accordingly, impossible to observe the apparent movement of the visual field. Subjects *C*, *I*, *J*, *K*, *L* and *M* were rotated every other day or every third day and are not, therefore, included in this table.

Still more striking is the persistence of the effects of practice over long intervals of time. We have shown elsewhere[1] that after from four to eight weeks, the effects of practice are still in evidence. That is to say, a subject whose nystagmus time has decreased to 5–6 secs., may some weeks later give, at most, only 8–10 secs. of ocular movement.

[1] Paper read before the Illinois State Academy of Sciences, Feburary, 1920. To be published in the *Proceedings* of the Academy.

For example, by referring to Tables II., and III. it will be seen that the last values for Subj. *F* in the regular series were 4.0, 4.0, 4.3. Two months later the averages were 7.5, 9.7, 8.2, and within ten trials (5 to the L. and 5 to the R.) had decreased to 5.5, 7.5, 5.5. In a similar way, Subj. *P*, who had lost all ocular movement with practice, gave after five months but three seconds of after-nystagmus. Further evidence was gained from five men of long experience in the aviation service. It was found in such cases that, although the first turnings might yield high numbers,[1] but a little practice was required to reduce all the values. For example, Subj. *AA*, who had been flying for eighteen months, was tested seven months after his discharge. Table XII. shows the results.

Table XII

Series	Right			Left		
	Time	No.	App. Mvt.	Time	No.	App. Mvt.
1	21.0	40.0	21.0	17.0	25.0	17.0
2	18.0	29.0	18.0	15.0	22.0	15.0
3	14.0	18.0	14.0	11.0	12.0	11.0
4	11.0	12.0	11.0	8.0	10.0	8.0

It is clear, therefore, that although the initial nystagmus times and other temporal values may approximate results obtained before practice in flying, the influence of practice is rapidly re-established. Furthermore, none of these subjects past-pointed or gave other report of organic disturbance.

D. The Effect of the General Organic State.—Some twenty times during the series of investigations subjects came to the laboratory suffering from organic disturbances in no way connected with the experiments themselves. Occasionally a subject came in from violent exercise or from exertion on excessively warm days. In all these cases, the time of nystagmus, the number of eye-movements, and the time of the apparent movement of the visual field were appreciably

[1] The high initial values of men coming from long practice in flying is not contradictory to our results. They, in common with whirling dancers, show a smaller nystagmus time only when turned under the conditions that have obtained during practice. That is to say, the effect of repetition is immediately apparent under one set of conditions only. Nevertheless, as we shall presently see, there is a rapid "transfer effect."

increased. For example, the averages of all the trials for these subjects just the day before the organic disturbances were 17.4, 28.7, 16.6. The averages of the trials taken while the subjects were thus organically disturbed were 20.1, 30.5, 19.1. The average of the first trials after recovery (recovery being assumed from the subject's own report) were 16.4, 27.2, 15.7. From these figures, the effect of the organic disturbance is apparent. Furthermore, the figures show that the effect is only temporary and that, in fact, it conceals a real decrease. In almost every case the results obtained after recovery were found to be about the same as the temporal course of the nystagmus at that time would indicate should have been the case. In addition to these facts, certain individuals reported on coming to the laboratory that they were feeling unusually fit. In these cases, the average of the trials on the preceding day were 18.7, 42.0, 18.5, while the averages on the day in question were 12.8, 28.4, 11.6, and the averages on the following day were 15.4, 31.3, 15.1.

For example, on July 7, 1919, Subj. *M* arose very early, worked hard all morning and was rotated just after returning from a rapid walk on a hot day. The preceding day his average values were 14.3, 23.7, and 12.5. On July 7, the values rose to 18.1, 28.2, and 18.2, while on July 8, the values dropped to 12.3, 17.3, and 12.3. This typical case shows that the increase was not a permanent matter, but that it merely covered a real decrease. On August 1 the average values for this Subj. (*M*) were 6.8, 5.3, and 6.7. On August 2 and 3 when *M* was suffering from a digestive disturbance the average values rose to 9.8, 11.1, and 10.0. But on the next period, the numbers declined again to 6.1, 6.9, and 6.5. We have already pointed out that, in the case of white rats, organic disturbances may have a pronounced effect upon the time of the nystagmus and upon the rate at which decrease takes place.[1] The diurnal differences noted just above may rest on conditions of the same order. All of these facts suggest the wide organic implications of nystagmus and of the condi-

[1] Griffith, C. R., 'The Decrease of After-nystagmus During Repeated Rotation,' *The Laryngoscope*, 1920, 30, 135.

tions of its decrease. These implications must be considered in any explanation of nystagmus and of its susceptibility to practice.

TABLE XIII

DECREASE WITH LENSES IS PARALLELED BY DECREASE WITHOUT LENSES: DECREASE WITHOUT LENSES IS PARALLELED BY DECREASE WITH LENSES

Subject	Date	Lenses		No Lenses	
		Time	No.	Time	No.
Q	May 14	45.9	53.6	29.0	57.0
	July 1	32.6	35.9	14.5	18.5
	July 8	19.9	24.7	9.5	13.0
	July 15	21.3	27.3	7.0	6.5
	July 16	20.8	26.4	7.5	8.0
	July 31	16.1	22.5	5.5	5.0
R	May 14	35.9	38.6	25.5	25.0
	May 27	26.6	17.1	18.0	12.5
	June 2	25.5	25.5	14.0	13.0
	June 5	16.2	10.7	7.5	7.0
	June 10	20.2	16.8	8.0	5.5
	June 14	12.0	7.2	6.0	4.5
S	May 13	33.0	53.7	25.0	40.0
	May 27	26.2	43.8	18.0	33.5
	June 3	20.7	32.9	13.0	22.5
	June 11	17.4	27.8	10.0	17.0
	June 13	15.3	22.7	7.0	13.0
T	May 14	50.0	88.5	22.5	62.5
	May 16	39.1	73.7	20.5	52.5
	May 21	30.1	60.9	15.0	34.0
	May 24	25.5	52.0	12.5	24.5
	May 26	18.7	38.1	9.5	20.0
P	May 12	44.5	29.5	25.5	47.5
	July 10	16.0	6.0	11.8	15.5
	July 14	11.0	20.5	7.3	5.5
	July 30	15.2	9.0	4.7	4.0
	Aug. 1	11.0	10.0	4.2	3.2
O	July 12	37.0	29.0	15.9	10.8
	July 19	15.0	9.5	1.9	2.0
	July 28	9.0	5.0	0.1	0.1
N	June 30	32.0	49.0	18.7	35.0
	July 10	27.5	39.5	10.3	22.8
	July 17	15.5	27.5	7.8	11.4
	July 19	13.5	24.0	6.9	9.9
H	May 16	31.5	67.5	24.5	56.1
	May 30	21.0	44.0	7.1	12.0
	June 12	11.0	5.5	1.3	1.5
	June 14	8.5	5.0	0.0	0.0

E. Modification by 'Transfer.'—We saw above (p. 42) that aviators of long flying experience may have large initial values but that the effects of practice are rapidly restored. We then suggested that the high initial values were due to a change of the conditions. By inspecting Table XIII. it becomes clear that if sample trials making use of one set of conditions, *e.g.*, rotation without the use of lenses before the eyes, are taken during a series making use of another set, *e.g.*, rotation with lenses before the eyes, there actually is a 'transfer effect' from the one set of conditions to the other. That is to say, the decrease in the time of nystagmus without the use of lenses is paralleled by a decrease in the time if lenses are used at any part of the series. It has been contended by the otologists, for example, that any 'apparent' decrease in the duration of nystagmus may be offset by the use of lenses. Our results show that this cannot be the case inasmuch as a corresponding decrease has occurred when lenses are used temporarily, a modification which is equivalent to the decrease that would have occurred during a constant use of lenses. This modification by 'transfer' may be seen to advantage in the following facts. Subj. *P*, whose initial values were 25.5, 47.5, and 26.5, gave, during rotation with the head inclined forward through 160 degrees, a circular nystagmus with the following values: 24.7, 35.6, 25.0. When the head was placed sidewise the results were 26.9, 51.7, 28.1. At the end of the practice series under 'normal' conditions, trials were again taken with the head bent forward and to the side with the following results:

TABLE XIV

	Head Forward				Head to Right Side		
	Time	No.	App. Mvt.		Time	No.	App. Mvt.
Aug. 8	12.2	14.4	14.0	Aug. 12	11.5	27.7	13.2
Aug. 9	12.2	12.5	14.0	Aug. 13	11.2	23.5	12.5
Aug. 10	8.1	10.7	9.7	Aug. 14	9.2	18.0	9.7
Aug. 11	7.4	11.2	8.2				

At the time of the initial trials, the nausea and dizziness were severe, but at the end of the 'normal' series, the subject experienced no uncomfortable effects of the unusual position, showing that the effect of practice was more deep-seated than the ocular effects alone. Moreover, this subject and also other subjects have enjoyed an increasing ability to change the position of the head both during and after rotation without experiencing the tremendous increase in the intensity of the organic effects which usually accompany such a change in position.

V. Conclusions

We have discovered, then, the following facts about the organic effects of rotation, especially about the local effects called nystagmus. We have found that, as turning is repeated from day to day, the duration of the after-nystagmus, the number of ocular movements made, and the duration of the apparent movement rapidly decrease. The major part of this decrease occurs within the first few days. The decrease takes place not only from day to day but also within a period of ten trials on any single day. The amplitude of the ocular movements and the number of movements made per second also decrease as repetitions increase. Furthermore, certain other organic effects, especially those known as past-pointing, decrease in the same manner. We have also found that the time of nystagmus changes with the speed of rotation and with the number of revolutions and that it is increased when the chair is abruptly halted. As to other conditions under which nystagmus varies in degree and amount, we have found that we must consider: (*a*) the time of day during which rotation is carried on; (*b*) the amount and number of rest-intervals between turnings and between series; and (*c*) the general organic state of the subject. Finally, nystagmus may be modified indirectly by 'transfer.' In general, we have found the organic effects of rotation to be highly variable in their appearance and, moreover, so amenable to practice that they may entirely disappear within a relatively short time, provided rotation is repeated from day to day.

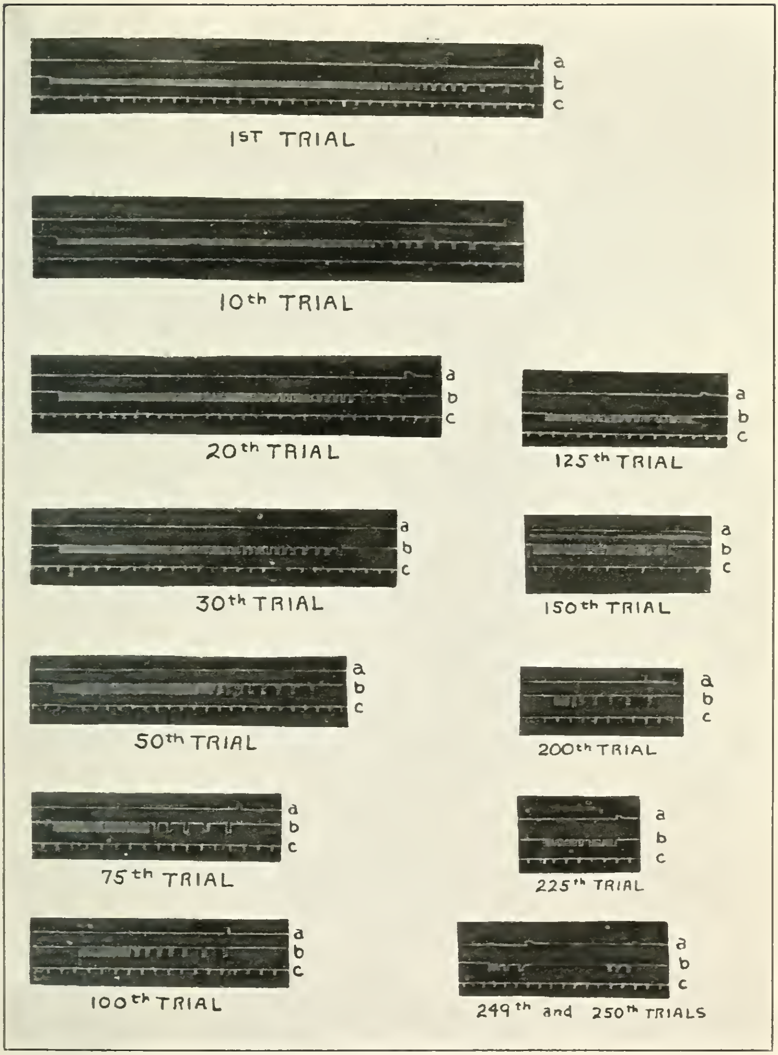


PLATE I.

a. Time of apparent movement.
b. Number of movements.
c. Time in seconds.

A SERIES OF FORM BOARDS

BY GEORGE OSCAR FERGUSON, JR.[1]

University of Virginia

I. *The Boards.*—In an attempt to devise a series of form boards of graduated difficulty of performance, eight boards of uniform size and of six holes each were designed. The boards were thirteen inches long, ten inches wide and one inch thick, and were stained light brown. The blocks were five eighths of an inch thick and were stained dark brown on both sides and on the edges. There were no pegs or handles on the blocks. The holes were one half inch deep, so that the blocks, when in position, projected one eighth of an inch above the surface of the boards and were easily removable from the holes.

All of the holes were of geometrical shapes and no two were alike. The principles employed to vary the difficulty of the boards were as follows:

1. Differences in the simplicity or complexity of the outline of the holes.

2. Some of the boards had only one block for each hole while others had two blocks for each hole.

3. In some of the boards the blocks could be fitted into the holes with either side uppermost; in other boards only one side of the blocks could be uppermost.

4. A given edge of a block could be either toward or away from the subject or to his right or left, when fitted into some of the boards; in other boards a given edge of a block had to face a definite direction with regard to the subject.

[1] The writer is indebted to Professor R. S. Woodworth for suggesting that a series of form boards be made and for access to the Columbia psychological workshop in making them; to Mr. A. S. Otis for constructive suggestions as to the scoring of the boards; to Messrs. O. R. Johnson, L. E. Becraft and W. B. Gibson for testing many of the subjects from whom results were obtained; and to his wife for assistance in making the computations.

5. Where there were two blocks for a hole, in some boards their positions were interchangeable within the hole while in others they were not.

6. The adjacent edges of two blocks in a hole were beveled in some boards while in others they were plain. The beveling was done by cutting, at an angle of 45 degrees to its surface, the original single block which fitted a hole, thus making two blocks with complementary beveled edges.

7. In some boards the two blocks in a hole were fitted to each other by means of a tongue and groove. The tongues and grooves were made by cutting, equally from each side, at an angle of 45 degrees, the original single block which fitted a hole. The two blocks resulting from the cutting thus had a tongue five sixteenths of an inch long fitting into a complementary groove.

8. In some of the boards where there were two blocks to a hole, certain of the blocks in different holes had the same surface shape, but differed in that one was tongued while the other was grooved or in that their edges were beveled in opposite directions.

These principles of varying the difficulty of the boards were combined in various ways in designing the series. Preliminary trials with a number of subjects showed that while all of the boards differed in ease of performance, four of them were comparatively easy while four were comparatively hard, the gap between the two sets of four being as great as the variation within either set. Four additional boards were then constructed, based upon the principles used in making the original eight, and it was found that these four boards were of such difficulty as to fill the gap existing between the two halves of the original set.

From the series of twelve boards six were finally selected to constitute the series described herein. These six were selected because of the regularity of their gradation from easy to difficult and because of their reliability as indicated by the distribution of their results. Fig. 1 exhibits their design.

II. *Procedure.*—All twelve boards were given to 170 subjects in the elementary grades and in college. The six

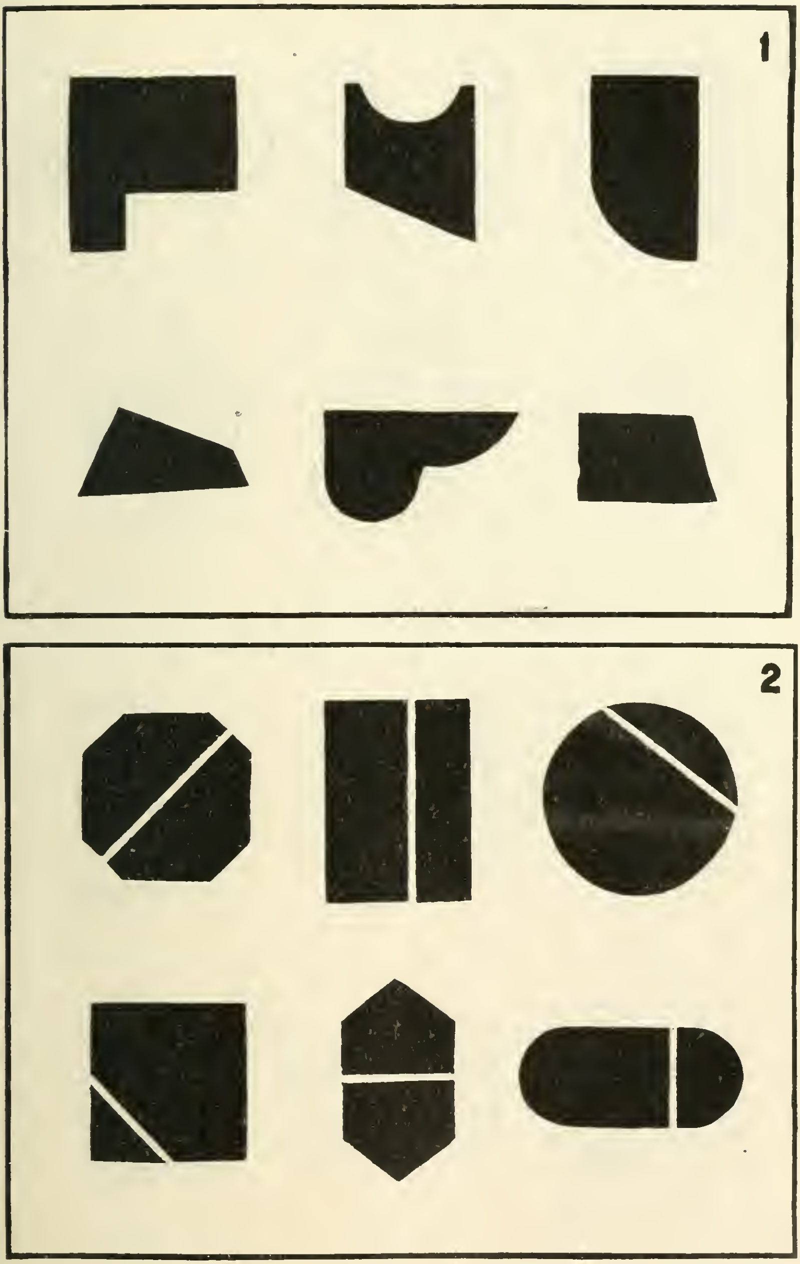


FIG. 1. Diagram of the Six Boards. *A* = block with overhanging beveled edge; *B* = block with beveled edge fitting under the overhang; *C* = block with projecting tongue; *D* = block with groove to fit tongue.

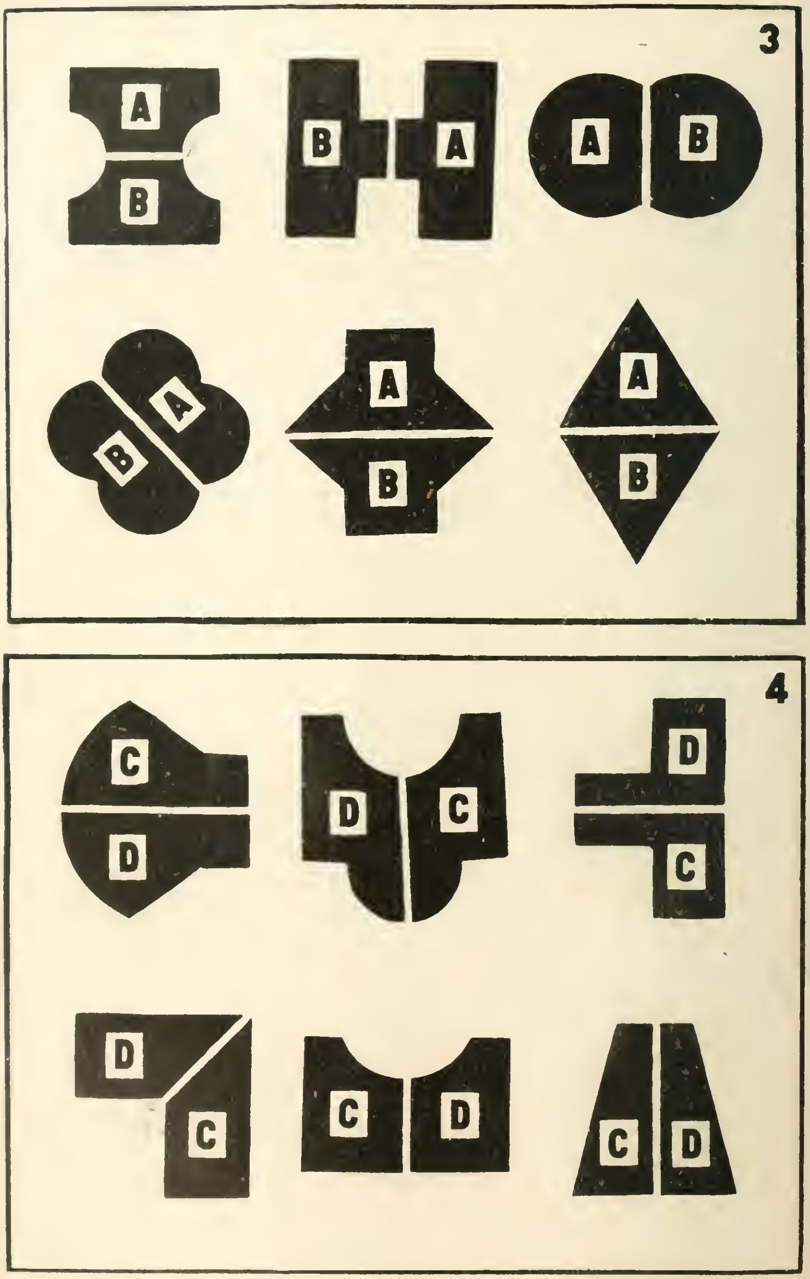
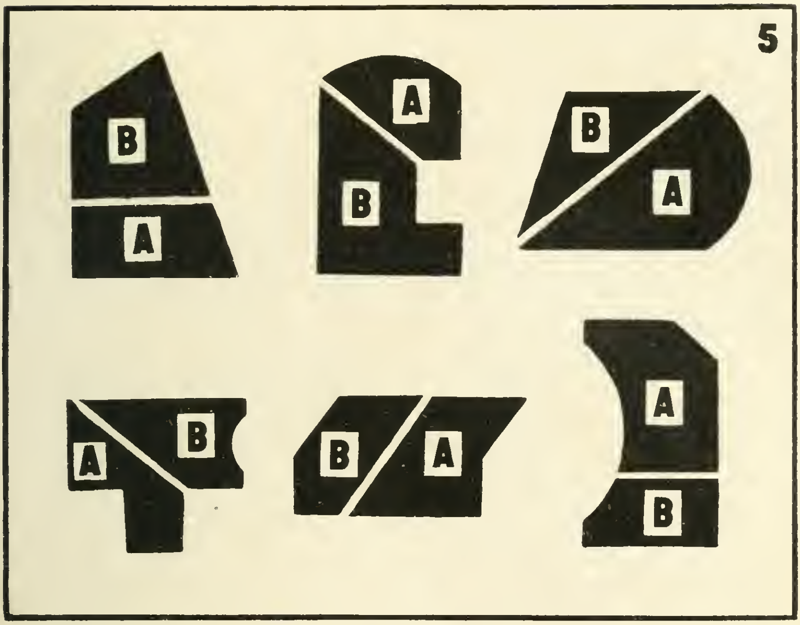


Fig. 1.—Continued.



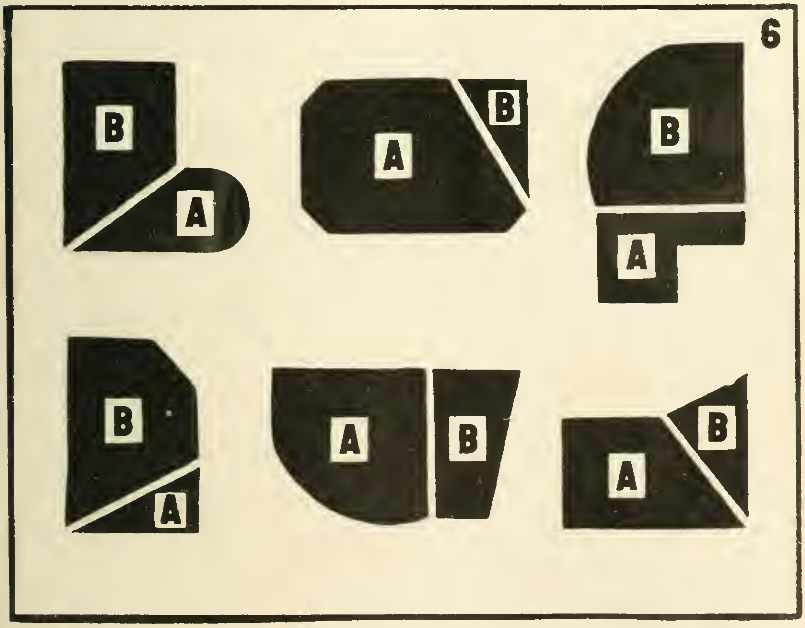


FIG. 1.—Continued.

boards finally selected were then given to 194 additional subjects in the elementary grades, high school and college. In all, 364 individuals were tested with the six selected boards. The number in each grade appears in Table II. Some of the elementary school children tested were in the public school of Williamsburg, Va.; others, with the high school pupils, were in the public school of Hamilton, N. Y.; the college students were in Colgate University. There was no discernible difference between the records of the Williamsburg and the Hamilton children.

The boards were always presented in a definite order, the easier first and the harder last. But it was found that the results for the six selected boards were the same when they were given in the series of twelve as when they were given as a series of six. Each board was presented separately, with the subject facing the lower side as the boards appear in Fig. 1. The blocks were arranged in chance order at the side of the board opposite the subject, who was allowed to choose his own position and to use one or both hands. In giving verbal directions the aim was to make sure that each child understood perfectly what was to be done, rather than to give stereotyped instructions. The directions were approximately as follows:

"I have here a board which has six holes cut in it. There is a wooden block (or there are two wooden blocks) in each hole. The blocks fit the holes exactly. I am going to empty all of the blocks out of the holes and place the board in front of you. When I lay the board down I want you to put all of the blocks in their proper places as quickly as you can. Do you understand? (Repeat directions.) Then begin."

III. *Scoring.*—The scoring was based entirely on the time required to place all of the blocks in the holes, no account being taken of errors or of partial completions. When the last block was properly placed the time was recorded. A maximum period of five minutes was allowed for each board. When five minutes elapsed without success, the correct solution was shown by the experimenter and another board was presented.

Table I

Distribution of the Scores for Each Board in Each School Grade
Actual figures reduced to percentages.

Score	Grade															
	1	2	3	4	5	6	7	8	I	II	III	IV	F	S	J	S
	Board 1															
5	9	18	25	33	38	74	76	80	95	87	94	93	90	100	100	100
4	41	36	50	50	47	23	24	20	5	13	6	7	10	—	—	—
3	22	25	17	11	9	3	—	—	—	—	—	—	—	—	—	—
2	12	11	4	6	3	—	—	—	—	—	—	—	—	—	—	—
1	12	7	4	—	3	—	—	—	—	—	—	—	—	—	—	—
0	3	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	Board 2															
5	—	—	4	6	3	3	14	20	5	20	12	20	40	33	37	32
4	22	25	29	67	59	67	57	60	74	60	81	67	50	58	56	60
3	25	28	25	11	19	23	29	20	21	20	6	13	10	8	6	4
2	22	21	29	11	12	8	—	—	—	—	—	—	—	—	—	4
1	19	14	12	11	6	—	—	—	—	—	—	—	—	—	—	—
0	12	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	Board 3															
5	—	—	—	—	—	—	—	—	—	—	—	—	—	8	6	4
4	—	3	4	11	9	18	29	40	37	47	37	47	60	83	81	64
3	6	7	29	28	28	46	38	30	47	40	31	27	30	8	12	28
2	9	18	8	17	28	23	24	10	—	7	25	20	10	—	—	4
1	22	25	38	28	25	13	—	10	16	7	6	7	—	—	—	—
0	62	46	21	17	9	—	10	10	—	—	—	—	—	—	—	—
	Board 4															
5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
4	—	—	—	6	—	—	14	5	5	13	12	13	30	33	44	32
3	—	7	—	11	6	23	24	40	32	47	50	47	50	42	37	36
2	13	7	8	11	16	21	29	25	32	13	25	27	10	8	6	20
1	37	29	58	45	50	51	24	20	26	13	—	7	—	17	12	12
0	50	57	33	28	28	5	10	10	5	13	12	7	10	—	—	—
	Board 5															
5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
4	—	—	—	—	—	—	—	—	—	—	—	—	10	—	12	12
3	—	—	—	—	—	5	10	30	16	7	31	13	30	67	50	40
2	—	—	4	6	6	13	24	15	26	20	37	47	30	25	25	24
1	16	11	8	33	31	51	38	40	32	60	12	27	20	8	12	24
0	84	89	88	61	62	31	29	15	26	13	19	13	10	—	—	—
	Board 6															
5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
3	—	—	—	—	—	3	—	—	—	7	—	—	10	17	12	12
2	—	—	—	6	6	8	10	10	11	7	6	20	10	25	50	40
1	—	—	4	11	12	26	33	40	21	33	37	40	50	33	31	16
0	100	100	96	84	81	66	57	50	68	53	56	40	30	25	6	28

A number of plans of scoring were tried. Attempts to express scores in terms of the actual time required to solve boards were unsatisfactory because of the practical necessity of setting a time limit. A record of the number of boards which were not solved within the time limit was unsatisfactory

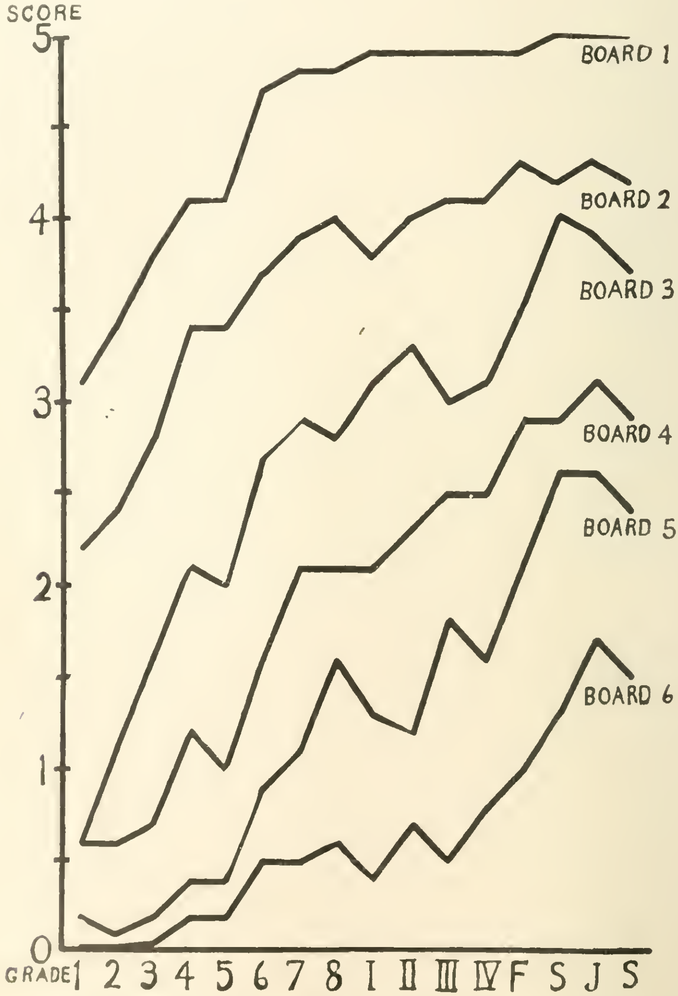


Fig. 2. Average Score for Each Board in Each School Grade.

as a score in that it did not distinguish between any except the most pronounced differences of ability. The plan finally adopted was chosen because it gave the most nearly normal distribution of results and because of its simplicity. Each board was scored in the same way and the sum of the scores on the six boards was taken as the final score. A correct solution of a board in less than 30 seconds was scored 5;

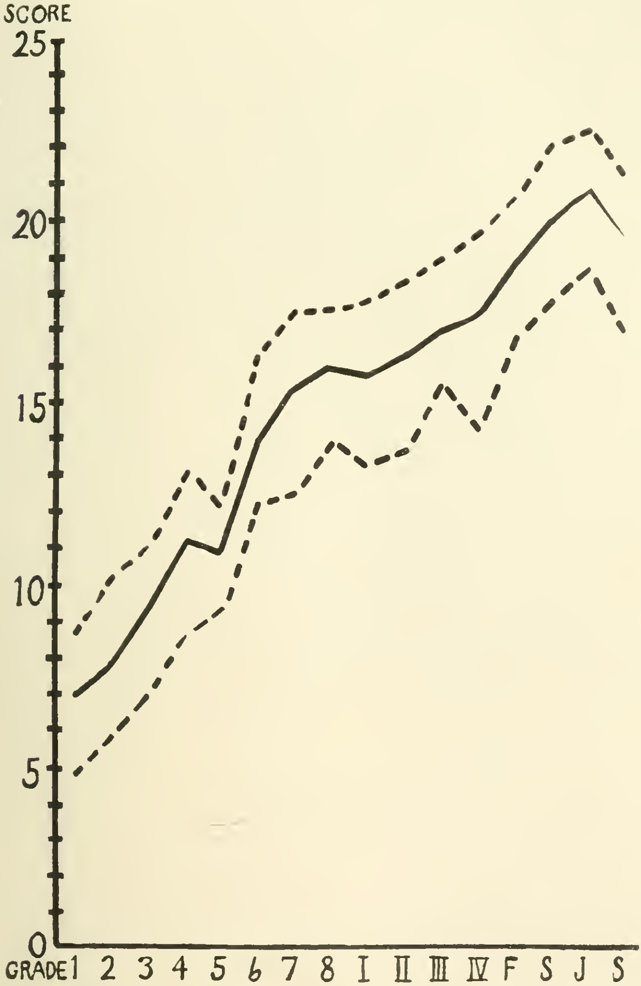


Fig. 3. Solid line = median score in each school grade for the series of six boards. Broken lines = quartiles.

a solution in less than 60 but more than 29 seconds was scored 4; a solution in less than 100 but more than 59 seconds was scored 3; a solution in less than 150 but more than 99 seconds was scored 2; a solution in less than 301 but more than 149 seconds was scored 1; if a solution was not obtained in 300 seconds the score was 0. These values are shown in the following table:

Time	0	30	60	100	150	301
Score	5	4	3	2	1	0

The records obtained in the elementary grades, in addition to being scored by using the time intervals indicated, were also scored by using other intervals for the sake of comparison. In the fifth and sixth grades, containing 71 pupils, use of the intervals 0, 15, 30, 60, 120, 301 gave a coefficient of correlation of .98 with results from the intervals adopted. In these grades, intervals of 0, 60, 120, 180, 240, 301 gave scores which correlated .95 with scores obtained when the adopted intervals were employed.

IV. *Results.*—Fig. 2 exhibits the score obtained on each board by each grade. Table I. shows the distribution of the scores of each grade on each board. It is evident that the boards increase in difficulty from No. 1 to No. 6 and that the higher scores on each board are obtained by the higher grades.

Fig. 3 exhibits the median score and the upper and the lower quartiles for each grade on the series of six boards. The average distance of the quartiles of each grade from the grade median is 2.1 points of the scale; a range of 4.2 points includes 50 per cent. of the individuals in a grade. Table II. shows the detailed distribution of the scores. The relationship between score and grade expressed in Table II. gives a coefficient of correlation of .81. This is as high as the coefficient obtained between Alpha, Beta or Stanford-Binet scores and reported school grade when these tests were given to unselected drafted men at Camp Lee. It may be noted that the median form board score for each school grade is roughly the same as the median age of the grade.

Thirty-six of the pupils of the sixth grade in the Hamilton school, who had been tested with the form board series, were given the army Alpha test and were rated by their teacher, who had taught them for six months, for intelligence and for class standing. The rating was in percentages, since that was the method in regular use in the grade. Coefficients of correlation of .51, .50 and .56 were obtained between the ratings in Alpha, estimated intelligence and class standing, respectively, and score in the form board series. Alpha gave coefficients of correlation of .73 and .68 with estimated intelligence and class standing, respectively, and estimated intelligence and class standing correlated .90 with each other.

TABLE II

DISTRIBUTION OF THE SCORES IN EACH SCHOOL GRADE FOR THE SERIES OF SIX BOARDS

Score	Grade															
	1	2	3	4	5	6	7	8	I	II	III	IV	F	S	J	S
25																1
24														1	2	
23													1	3	1	2
22									1			1	2	2	2	1
21							1			2	1	1	1	3	2	4
20								1	1		2	1	3	3	3	2
19						1	1	1	1		1	2	2	4	1	5
18						2	1	2	1	2	3	2	3	1	3	2
17					1	2	4	3	4	1	1	1	2	4		2
16				1		5	2	3	1	3	3		3	2	1	4
15				1	1	6	2	3	3	1	2	2	1	1	1	1
14			1	1	2	3	1	1	1	2	1	2	1			
13		2		1	1	6	3	3	2	3	1	1	1			
12	1	2	2	2	4	5	2	1	2							1
11	1	2	3	4	7	5	1		1	1	1	1				
10	2	1	2	2	6	2	1		1			1				
9	3	4	5	2	3	1	1	1								
8	5	2	4		3	1		1								
7	4	3	1	1	1		1									
6	3	4	3	2	1											
5	4	3	1	1	1											
4	3	1	1		1											
3	2	1	1													
2	3	2														
1	1	1														
0																
No.	32	28	24	18	32	39	21	20	19	15	16	15	20	24	16	25

Sixty negro children, fifteen selected by chance from each of the first four grades of the Williamsburg colored school, were tested with the form boards with a view to comparing their scores with those of sixty white children similarly selected from the first four grades of the white school of the same city. The comparison showed that 33 per cent. of the negro children equaled or exceeded the median score of the white. In such tests as the ordinary completion, analogies or other tests of controlled association involving verbal symbols, only about 20 per cent. of negro elementary school children equal or exceed the median score of whites in the same grades.

When the performance of the girls in the form board series was compared with that of the boys, grade by grade throughout the elementary school, it was found that 48 per cent. of the girls equaled or exceeded the median of the boys.

V. *Summary.*—The six boards of the series increase in difficulty from No. 1 to No. 6 by fairly constant increments. There is also a fairly constant rate of increase in the scores obtained by successive school grades on each of the boards. The series as a whole appears to be adapted to the testing of individuals throughout the school system, from the primary grades to college. The median score obtained by a given grade is roughly the same as the median age of the grade; within a given grade the median deviation is about two points of the scale.

The basis of scoring is the time required to fit the blocks into the boards. The boards are presented one at a time, beginning with the easiest, and the subjects are instructed to fit the blocks into the holes as quickly as they can. Solutions of a board within time limits of 30, 60, 100, 150 or 300 seconds give scores of 5, 4, 3, 2 or 1, respectively. If a board is not solved within five minutes the score is zero. The sum of the scores on the separate boards constitutes the total score.

The results obtained indicate that boys do as well as girls on the boards, or perhaps slightly better; that about 33 per cent. of negro children score as high as the median of white children; that within a given school grade performance on the boards correlates approximately .50 with class standing, estimated intelligence or army Alpha score; that performance on the boards correlates about .80 with school grade.

AN APPARATUS FOR DETERMINING ACUITY AT LOW ILLUMINATIONS, FOR TESTING THE LIGHT AND COLOR SENSE AND FOR DETECTING SMALL ERRORS IN REFRACTION AND IN THEIR CORRECTION

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This apparatus was devised in response to a request by the Eye Division of the U. S. Naval Hospital for a means of making a quick and accurate test of acuity at low illumination. Experience has shown, roughly speaking, that only 25–30 per cent. of the men on the battleships have a sufficiently keen acuity at low illuminations to qualify for all branches of the lookout and signal service work. The apparatus provides for a wide range of illumination in just noticeably different steps (beginning at 0.07 meter-candle or lower), with no change in the color value of the light and with a specification at each step of the intensity of light falling on the test-object.

Among the requirements for an apparatus for determining acuity at low illuminations or the effect of change of illumination, the following points may be mentioned. (1) A means of changing the illumination by small amounts over a wide range, beginning at or below the threshold for the test-object employed, without changing the color value of the light. If in making this change the color value of the light is altered it is obvious that another factor affecting the results is introduced. (2) A means of keeping constant for an indefinite length of time any desired intensity of illumination and of reproducing this intensity at will. Also the test-object must be uniformly illuminated. (3) A means of specifying accurately at any point in the scale the intensity of light falling on the test-object. And (4) it is desirable that the apparatus employed for controlling the illumination can be used with the test-objects already accepted in clinic practice.

The most difficult problem one has to face in constructing an apparatus for determining the minimum amount of light that permits of the discrimination of a given test-object, more particularly if that object consists of a line of test letters, is to secure a uniform illumination of the line. This problem is relatively unimportant at the illuminations ordinarily used in acuity testing, because at these illuminations acuity changes so slowly with chan e of intensity of light that the differences which may occur throughout the line of test letters do not ordinarily produce a detectable effect on the results of the test. However, if no more care is exercised at the threshold to secure uniformity of illumination than is ordinarily used at the higher illuminations, no single intensity at the source will serve for the discrimination of all of the letters of the line. We were able satisfactorily to meet this difficulty in only one way, namely, by selecting an aperture sufficiently small to permit of its uniform illumination and projecting a magnified image of this aperture on the test card. That is, an aperture was selected of such a size and shape that when magnified fivefold it gave a band of light which just blocked off one line of the test letters. It is obvious that this aperture could be made of different sizes and shapes, depending upon what is wanted in the projected image. For example, two or three lines of test letters could be blocked off if desired, or the whole card or any part of it could be illuminated, etc. There is no reason, moreover, why the aperture could not be made adjustable in size to suit the needs and preferences of the individual operator. In one model of the apparatus these apertures were cut in a series of slides which could be inserted in the projection tube just outside the lamp house in grooves in a light-tight boxing. A convenient means was thus provided for changing the aperture, if desired, during a series of tests without having to open the lamp house. The source of light is a well-seasoned Mazda C lamp of the round bulb or stereopticon type of 100, 250 or 500 watts, depending upon the range of illumination that is desired. This lamp is installed vertically in the roof of the lamp house at such a height that its filament is well above the aperture which is to be illuminated. In order to secure a uniform and diffuse illumination of the aperture the lamp house is lined with opal glass ground on one side. The aperture, 6 x 1 cm., is cut at the center of the cap covering the inner end of the projection tube. Further to aid in the even illumination of the aperture, it is covered with a slide of ground glass. To prevent the overheating of the lamp house, a rather elaborate ventilating system is provided consisting of a light-tight ventilating hood at the top and a series of holes on two sides at the bottom of the housing, furnished with light-tight shields. The changes in the intensity of light are produced by means of an iris diaphragm. When such a diaphragm is placed either at the front or back surface of the focussing lens, changes in the flux of light can be produced without any alteration in the size or the shape of the image produced by the lens, just as happens, for example, in the action of the iris of the eye. At a suitable point in the circumference of the diaphragm is fastened a pointer which, as the diaphragm is opened and closed, moves over a translucent millimeter scale. This scale is mounted over a slot in the projection tube and receives its illumination from the light inside of the tube. The inside of the tube is painted a mat black. At the further end of the projection tube, 18.1 cm. from the illuminated aperture, in a brass ring and collar is mounted the focussing lens. This lens is 7.5 cm. in diameter and has a focal length of 14.8 cm. A different strength of lens could have been used and different relative distances of aperture and test card from the lens. With appropriate variations in these factors the distance of the lantern from the test card and the amount of magnification of the projected image may be varied. Any increase of magnification results of course in a decrease in the brightness of the image, hence an increase in the scale of brightness of image with no change in its size could have been obtained by increasing the size of the aperture and decreasing correspondingly the amount of magnification. In the construction of the present apparatus a 14.8 cm. focal length lens was used because it could be obtained the most readily on the market of the diameter needed. On the platform supporting the lamp house are mounted a small Weston ammeter and a small rheostat to guard against fluctuations in the current and consequent fluctuations in light intensities. In order that any line of the chart may be illuminated at will, the lamp house is mounted on the end of a rod which is raised and lowered by means of a rack and pinion. The test card is mounted at a distance of 81 cm. from the focussing lens. A photograph of the apparatus is shown in Fig. 1.

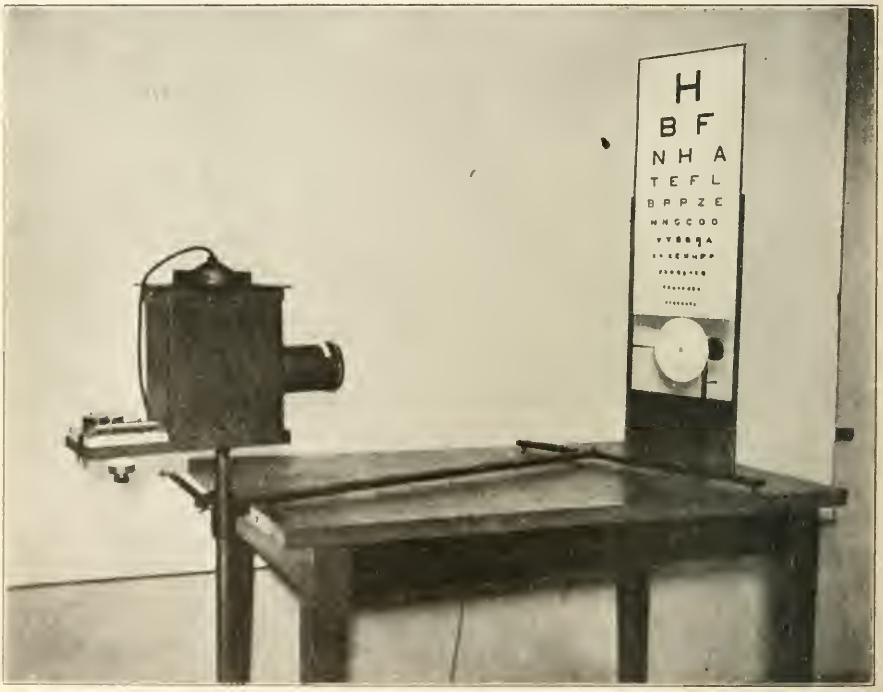

FIG. 1.

In order that the intensity of light used at any time may be known, a calibration chart is provided in which are given the readings on the millimeter scale and the equivalent meter-candle values at the test card. This calibration was accomplished as follows: The lamp house was removed and mounted on a photometer bar at a distance from the photometer head equal to its original distance from the test card. The scale was then gone over point by point and the meter-candle value of the light at the photometer head was measured. The calibration chart is shown in Fig. 2 *a*. In Fig. 2 *b* is shown the calibration curve in which the divisions of the millimeter scale are plotted against meter-candles at the test card. These values are corrected to conform at the center of the card to the cosine law.

Diaphragm Setting	Meter-candles	Diaphragm Setting	Meter-candles
82.5	9.19	46.0	2.46
82.0	9.04	44.0	2.23
80.0	8.54	42.0	2.02
78.0	8.06	40.0	1.81
76.0	7.58	38.0	1.61
74.0	7.12	36.0	1.43
72.0	6.67	34.0	1.27
70.0	6.23	32.0	1.12
68.0	5.81	30.0	0.97
66.0	5.43	28.0	0.82
64.0	5.06	26.0	0.69
62.0	4.71	24.0	0.58
60.0	4.36	22.0	0.48
58.0	4.02	20.0	0.38
56.0	3.72	18.0	0.28
54.0	3.45	16.0	0.21
52.0	3.18	14.0	0.16
50.0	2.93	12.0	0.11
48.0	2.69	10.0	0.07

FIG. 2*a*. Calibration Table.

For our own use in the laboratory we have preferred to substitute for the Snellen chart a single test character, the broken circle (the international test object), which can be turned in different directions and the judgment of its direction rather than the recognition of the character be required of the observer as a test of discrimination. Our reasons for this preference are as follows: (1) A test letter may be recognized when it is not seen at all clearly. Recognition is too dependent on extraocular functions to be used with precision as a measure of ocular capacity. (2) The different letters of the Snellen chart set an unequal task for the resolving power of the eye. (3) An objective check is had on the judgment. This is especially helpful in case of children, and the unintelligent, untrained and subjective type of adult. (4) By the use of the same test character, turned in different directions at will, all possibility of learning the test series is eliminated. Also the test-object becomes much more valuable for the detection of astigmatisms. And (5) at low

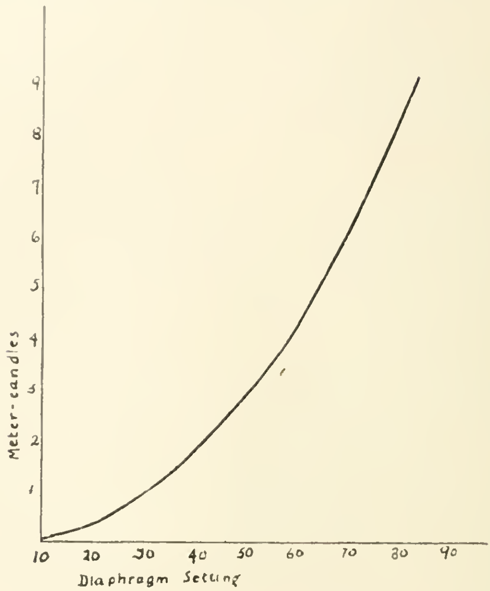

FIG. 2*b*. Calibration Chart.

illuminations the eye fatigues very rapidly. Thus if the task is the reading of the whole line of letters the results obtained measure not only acuity, but the power to sustain acuity which may or may not be compatible with the purpose of the test.

As stated in the introduction, the apparatus was designed to meet a specific testing need of the Navy. However, it has in addition the following laboratory and clinic uses. (1) Photopic acuity may be tested under the conditions of a constant and uniform illumination of known intensity. In case the test-object is a line or chart of letters, provision is made that each letter receives, within sensible limits, equal amounts of light. (2) Scotopic or twilight vision may be tested—also the amount and rate of scotopic adaptation. A precise and feasible means is thus afforded for testing the light sense insofar as it affects the power to see clearly. (3)

If the image of the aperture is projected on a blank white surface of good reflecting power, more particularly if its size and shape are changed to that of a square or circle of dimensions favorable for making a sensitive judgment, the apparatus can be used with an equal degree of precision and convenience for testing the light sense directly in terms of the amount of light required to arouse just noticeable sensation. That is, the threshold of sensation can be determined in terms of meter-candles of light falling on the test surface; or, knowing the coefficient of reflection of this surface and the breadth of pupil, in terms of the amount of light entering the eye. The testing of the light sense will probably always remain foundation work in the clinic routine. The testing of scotopic acuity, for example, is not sufficiently differential in all cases between refraction defects and the hemeralopias and other retinal deficiencies to serve as a satisfactory substitute. There is, at present, we are told on very good authority, no satisfactory instrument for testing the light sense available to the ophthalmologist. And (4) by making it possible to determine with great exactness the minimum of illumination at which the test-object can just be discriminated, the apparatus provides a very sensitive means for detecting small errors in refraction and in their correction, as will be demonstrated in a later paper.

Sensitivity for detecting small errors in refraction and in their correction could also be added to the acuity test by using a test-object, small changes in the size of which could be made. However, no means has as yet been provided for making small changes in the size of any acuity object sufficiently complicated in form to test simultaneously the resolving power of the eye in any great number of meridians which is, we believe, a very important feature in determining the exact location of an astigmatism or the exact amount and placement of its correction, more particularly when a cycloplegic is not used.

We consider this an important feature in testing for an astigmatism because of our belief that the astigmatic eye in the attempt to compensate for its defect has in many cases at least acquired unusual powers and habits of accommodation. Our belief in this is based on three sets of observations. (1) In the use of the astigmatic charts without a cycloplegic in cases of low astigmatism, one is frequently annoyed by the astigmatic indication shifting from one meridian to another. (2) We have found observers who could voluntarily, in some cases requiring considerable practice, shift the meridian showing the astigmatic indication. And (3) in our test of astigmatism based on the relative speed of discrimination in the different meridians, *American Journal of Ophthalmology*, 1918, I., pp. 3–16, the speed of discrimination for the least favorable meridian can be increased by practice almost to equal that in the most favorable meridian with an equal amount of practice. This result is so noticeable that in order to make the test sensitive we were compelled to eliminate as much as possible the opportunity for the practice effect. This was done in two ways. (1) The series was always begun below rather than above the minimum time of exposure required just to detect the direction in which the test-object was turned. And (2) meridians were inserted in the series clearly outside of the region of maximum astigmatic effect in order to break up any progressive tendency to accommodate especially for the meridian showing the poorer resolving power. The fact that the eye can with long exposures discriminate a fineness of detail in its unfavorable meridian which it is utterly unable to master with short exposures and that this excess lag can be overcome in a considerable measure by practice seems to indicate that it has the power through its active accommodation to overcome in part the effect of meridianal inequalities in resolving power, at least when a test-object taxing the resolving power in only one meridian is turned successively into different meridians. In any event it seems only the part of sound procedure in testing without a cycloplegic to guard against the possibility of selective meridianal accommodation by the use of a test-object which taxes the resolving power of the eye in as many meridians as possible.

But even if a test-object complicated in form and minutely adjustable in size were available, a device for determining the minimum illumination at which the test-object subtending the standard visual angle can just be discriminated would afford a still more sensitive means for the detection of low astigmatisms and small errors in the amount and placement of their correction. This follows rather obviously from the fact that for all but very low intensities acuity changes slowly with change of illumination. That is, for all but very low intensities small differences in acuity correspond, comparatively speaking, to large changes in illumination. Used as we have recommended, the illumination scale becomes in effect an amplified indicating scale by means of which the relatively slight differences in acuity, represented by the proper correction of an error in refraction and small deviations therefrom, may be detected with comparative ease and certainty. It is not infrequent perhaps to find that in cases of low astigmatism, with the full illumination of a test-object presenting no smaller gradations in visual angle than are found in the Snellen chart, the observer is able to detect no difference in the ease or clearness of discrimination of the test character through a range of from 20–40 degrees in the placement of the correction. This difficulty is especially annoying in the case of children and the unintelligent, untrained and subjective type of adult. In such cases the apparatus shown here is especially helpful. With it a minimum is left to the comparative and observational powers of the subject. All that he is required to do is to indicate the direction in which the test-object points, the most favorable amount and placement of the correction being determined by the minimum amount of illumination at which he is able correctly to give this indication. The apparatus possesses ample sensitivity, as our results will show, for the detection of an error of 5 degrees and less in the placement of the correction of a low astigmatism or of 0.12 diopter and less in the amount of the correction.

In a table to be given in a later paper it will be shown, for example, that an error of 5 degrees in the placement of the correction of an astigmatism produced by a 0.25 diopter cylinder required as an average for five eyes 66.5 per cent. more light for the discrimination of the test-object than the correct placement; in case of an astigmatism produced by a 0.75 diopter cylinder it required 107.2 per cent. more light than the correct placement. The large number of scale divisions between the settings of the light control for the correct and incorrect placements of the cylinder will be shown also in these results. In case of the 5-degree displacement of the correction of 0.25 diopter astigmatism this difference averaged 9.6 for the five eyes. Since the apparatus can readily be set to half divisions, 19 settings of the light control could have been made with precision between the values needed for the true correction and the 5-degree displacement. This shows that the sensitivity of the apparatus far exceeds the present possibilities of the precise manipulation of the correcting cylinders.

In case of an error of 0.12 diopter in the amount of the correction, 54.6 per cent. more light was required for the least favorable meridian; and in case of an error of 0.25 diopter, 108.9 per cent.

The relation of the illumination scale to the detection of small errors in refraction and in their correction may be stated briefly as follows: Insofar as the test-object is concerned, clearness of seeing depends upon the value of the visual angle subtended and the intensity of the illumination. It follows from this that either the illumination scale or the visual angle scale may be used for the detection of errors in refraction, *i.e.*, in the diagnostic procedure either the illumination may be held constant and the visual angle varied, or the converse. Since the visual angle scale sustains by convention a 1 : 1 relation to acuity while acuity changes slowly with change of illumination for all but very low intensities, the illumination scale possesses the greater sensitivity for the detection of small errors in refraction—also the greater ease and feasibility of contrivance and manipulation. Used in this way the illumination scale becomes in effect an amplifying scale—somewhat analogous to the use of the tangent scale in detecting small deflections in the magnet system of a galvanometer—and has an advantage in sensitivity in proportion to the amplification. In clinic practice it has been shown to be of particular value in determining the exact amount and placement of the correction of astigmatisms. That is, if the eye has equal resolving power in all meridians, the amount of light required just to discriminate the test-object in all meridians will be the same; if the resolving power is not equal, the amount of light required will be different in the different meridians and different by an amount proportional to the amplification represented by the illumination scale. A more detailed discussion of a feasible and convenient means of using the illumination scale in office and clinic practice will be given later.

Attachment for Testing the Light and Color Sense

A consideration of the foundation principles of the acuity apparatus reveals at a glance that they lend themselves readily to light and color sense testing for clinic purposes. In order to convert the apparatus in the form described in this paper into a light sense tester three features are needed: (*a*) the choice of an aperture such that when magnified five-fold a stimulus is obtained of a size and shape suitable for a sensitive judgment of the threshold of sensation; (*b*) the provision of a suitable surface on which to project the magnified image of the aperture; and (*c*) a means of reducing the intensity of light from the acuity threshold to the light sense threshold, *i.e.*, from the amount needed just to discriminate the standard acuity object to the amount needed just to arouse the light sensation. The iris diaphragm used in the present form of apparatus, range of pupil 5–65 mm., does not provide for this range of intensity without changing the source of light. It is obvious that an attachment for the further reduction of the light which does not interfere in any way with the use of the apparatus for the acuity work, would afford a more convenient means of securing the lower intensities than the changing of the source of light. Provision has been made for this in two ways: (*a*) by neutral absorption screens or filters; and (*b*) by a Nicols prism (polarizer and analyzer). The attachment is made so that it will hold either of these reducing agencies, leaving the operator an option as to which shall be used. The filter holder is made from three grooved metal strips, 8 cm. long and of appropriate width and thickness, built into a three sided rectangular figure open at the top. It is fastened to a narrow collar which slips over the end of the projection tube of the acuity apparatus and is held in place by a set screw. The holder is provided with three grooves into which one, two or three filters 8 x 8 cm. may be inserted as desired, or the metal plate which holds the Nicols prism. The Nicols prism is mounted in telescoping tubes in the customary manner for reducing light intensities, one tube containing the polarizer and the other the analyzer. At the end of the tube containing the analyzer is a large, milled head by means of which very small angles of rotation may be made. The angle of rotation is read by means of a graduated dial, 6 inches in diameter, and an indicator with a Vernier scale, attached respectively to the tubes containing the polarizer and the analyzer on either side of their junction. The tube containing the polarizer is firmly mounted in a brass plate, 8 x 9 cm., with its axis coincident with the normal to the plate at its center. When the Nicols is to be used instead of the filters, this plate is inserted in one of the grooves of the filter holder. So inserted its axis is in the principal axis of the projection lens of the acuity lantern and the inner end of the polarizer is in contact with the outer surface of the projection lens. When the filters are employed to reduce the light they are inserted in the holder to give the large initial cut down and the further graded reduction is made by the iris diaphragm of the acuity lantern. When the Nicols is employed, the iris diaphragm is set at its minimum aperture, 5 mm., and the further reduction is made by the Nicols and read from its scale.

The testing of the color sense is provided for by inserting color filters in the beam of light. These filters may be inserted at the illuminated aperture; in the filter holder in front of the iris and lens; or, with a slightly different construction of projection tube, back of the lens as near to the iris as possible. The simplest of these possibilities, from the standpoint of the construction and operation of the apparatus, is to insert the filter in the holder immediately in front of the lens and cut down the light intensity by means of the iris diaphragm. If it should be desired or considered technically more correct, however, to produce the changes in intensity after the light has been passed through the filter, this result can be accomplished either by inserting the filter at the illuminated aperture or anywhere in the projection tube back of the iris, or by placing it in the holder in front of the lens, with the iris held constant, and changing the intensity by means of the Nicols prism.

Color sense apparatus for clinic purposes seems at present, so far as the central field is concerned, to be limited to the testing of such gross deficiencies as are classed as color blindness. They are of little use for detecting the smaller changes which mark the advance and recession of many pathological conditions. The present apparatus is designed for detecting and measuring the degree of deficiency in terms of the amount of light of a given range of wave-lengths which is required just to arouse the color sensation.

REACTION-TIME SYMPTOMS OF DECEPTION

BY

WILLIAM M. MARSTON

I. Problem

The experiment hereinafter reported was performed in the Harvard Psychological Laboratory during the Academic Year 1913–1914. At that time the writer of the present article, at the suggestion and under the direction of Professor Hugo Münsterberg, began experiment upon what was then planned to be a series of psycho-physiological problems in the field of legal testimony. The first psycho-legal problem to be approached was the investigation of the psycho-physiological symptoms of the deceptive consciousness. The systolic blood pressure symptoms of lying were reported upon by the present writer in the Journal of Experimental Psychology for April, 1917. Since that time considerable work has been done by the present writer and others upon deception tests in connection with the various psychological tasks undertaken for the Federal Government during the recent war. The present writer, in returning to the further laboratory investigation of the deceptive consciousness, is led to believe in the light of above-mentioned Army deception test experiments, that the practical value of psychological studies in this field lies almost wholly in a complete and comprehensive scientific discovery and analysis of all the psychological symptoms of deception rather than in attempted use of one isolated set of these symptoms for detection of deception on the part of witnesses or criminals.

A glance at the legal situation becomes necessary in pointing the practical problem. Mr. Bielaski, active head of the Bureau of Criminal Investigation for the Department of Justice during the war, expressed the opinion that the future of all deception tests lies in the possibility of their introduction as a basis for expert testimony, just as the various neuropsychiatric tests are now used as the basis for testimony by alienists in cases of alleged insanity. In order to qualify a psychologist as an expert upon deception a psychological "patent medicine" is not sufficient. The court must be convinced that a sufficiently sound and fundamentally scientific knowledge of all the psychological symptoms of deception are available to enable the alleged expert, not only to give his opinion to the jury, but also to answer hypothetical questions put to him under cross examination, showing upon what state of psycho-physiological fact said opinion has been based. A "patent medicine" is a secret formula known only to the owner of the copyright and to the United States Bureau of Patents. The maximum value of many such proprietary remedies lies in the suggestion exercised upon the mind of the patient by the mystery of the contents, with the resulting apparently miraculous effect of the remedy upon the disease. It is to avoid exactly this suggestion of the miraculous or mysterious upon the minds of juries that our present trial systems demand, not only expert testimony, but the qualification of the expert by proof of the existence of a commonly known and recognized body of scientific fact upon which the expert bases his opinion. Thus, it is extremely doubtful whether a single deception test, such as the association reaction time test, the Benussi breathing test, or the systolic blood pressure test, would be admitted as a basis for expert testimony in court without a broader foundation of psychological fact than is at present available. In short, the court will not be satisfied to be assured by psychological authorities that this or that deception test does detect deception; many medical authorities might similarly testify as to certain patent medicines. The courts, through long experience in the analysis of testimony, are not looking for patent medicines; they want a free and common knowledge of all the ingredients.

We must, therefore, seek objective, quantitative measurements of the psycho-physiological symptoms of the deceptive consciousness. If a psychologist has to go on the stand and testify that, in his opinion, the alibi of a defendant indicted for first degree murder is a lie, it seems obvious that no qualitative analysis of the symptoms during testimony will be sufficient. The quantitative analysis may indeed show only relative difference or significance but, nevertheless, quantitative measurements are in themselves definite and susceptible of common knowledge. The immediate problem of the experiment here reported was to study the reaction times of the subject during deception. Analysis of reaction time association experiments and tests reported in the literature shows that judgments have been based jointly upon the qualitative and quantitative aspects of the results. The qualitative aspect of these tests lies in the association reaction words, and the quantitative in the reaction times recorded. Our problem in the present experiment was to eliminate the words and isolate, if possible, the quantitative measurements. In all previously reported tests no unanimity of qualitative results has been reported but, on the other hand, so great a unanimity of quantitative results has generally been found that psychologists seem to have accepted as established the thesis that deceptive emotions tend to increase both association reaction times and mean variations. To test this thesis is the particular problem of the present experiment.

II. Method

Ten different subjects were used in the principal experiment; six subjects were graduate students of psychology who might properly be designated as trained subjects, and four were undergraduates of competent general psychological training but with no specialized laboratory experience. The method adopted purposed the exclusion of all mental elements extraneous to the interaction of deceptive consciousness and reaction times.

Series A.—Pasteboard index cards were prepared, carrying upon one side two columns of words, ten words to the column, printed in parallel columns on one side of the card. The cards were placed blank side up before the subject who was instructed at a given signal to turn over the card and give an association word with each word in one of the lists on the card as fast as he could. The two columns of words on a card were called L. and R. (left and right) and the two opposite directions in reading each list were called U. and D. (up and down). The cards were presented to the subject in groups of eight. Before giving the signal for the subject to turn over each card, the experimenter instructed the subject to choose either the right or the left column on that card and to proceed through that column either up or down. In any four cards out of each group of eight the subject was to obey the experimenter's instructions and in the other four out of that group the subject was to reverse instructions by taking the opposite list and proceeding in the opposite direction. The subject was to deceive the experimenter as to the identity of the cards where instructions were reversed by proceeding to make his associations with the list on these cards as rapidly as on the cards where he obeyed instructions. The experimenter, of course, did not know which of the lists were done truthfully according to direction, and which were done deceitfully against direction. The truthful cards were called C. (correct) and the deceitful cards O. (opposite). At the end of each group of eight cards the experimenter attempted to judge by means of the reaction times alone which four lists the subject had performed C. and which four O. The reaction times were taken with a stop watch operated by a magnet; the circuit being closed first by a Dunlap voice key, but later, when this proved unsatisfactory, by a tapping signal of the subject.

Series B.—After preliminary series *A* had been run off, the quantitative aspect of deceptive reaction times was still further isolated by substituting lists of two digit numbers for the words upon the cards presented to the subject. In addition to the directions of L. or R. and U. or D. which the subject might obey or reverse the experimenter now instructed the subject to add or subtract from each number on the list two, three, four or five. If the subject were determined to deceive upon any card he not only chose the column and direction opposite to the experimenter's instructions, but also added when instructed to subtract and vice versa. By this method it was intended to eliminate all meaning from the symbols dealt with in the subject's mental process, and to isolate as far as possible, the factor of deceiving the experimenter by uniformity in the reactions required of the subject. Time was allowed between the instruction and the starting signal for the subject to perform the central process of reversing the instructions, and the watch was started simultaneously with pronouncement by the subject of his first result; so that reaction times recorded actually included the time used in nine additions or subtractions. The interest of every subject but one was intrigued to a high degree (as reported in introspection and as evidenced by behavior during the experiment). This condition was established largely by the aggressiveness of the experimenter and the consequent keen desire of the subjects to "fool" or beat him at the game of deception.

A total of eleven series of tests were recorded, one of the ten subjects taking part in two series. Each series of tests exclusive of preliminary series *A* included 40 sets, eight lists to the set, as above described, four sets being run through in about an hour. Every subject reported positively that he attained the desired attitude of deception and each number list was given four times, each time with a different digit to subtract or add, in order to exclude as far as possible any chance influence of combinations of figures peculiarly difficult to any individual subject. In the principal experiment total averages were made separately for all O. reaction times and for all C. reaction times of each subject. Comparative studies and further analyses of these averages were made, as will appear in detail hereafter.

III. Results

The results of this experiment disclose two distinct types of reaction time behavior during deception. First, the positive type was clearly evident in four out of ten subjects. This is the type, hitherto believed to be practically universal, which is characterized by increased length of reaction times during deception, with accompanying increase of mean variations. Secondly, a negative type manifested itself with equal distinctness in three out of ten subjects. In the cases of these three subjects reaction times during deception appeared to be consistently shortened with, however, no consistent accompanying effect upon mean variations. Thirdly, a mixed group appeared in the cases of four subjects who completely shifted from positive to negative types on different days, behaving consistently within the positive or negative types on each single day's work.

A. Positive Type

No detailed study of results to prove the existence of this type will be necessary in the present experiment, since the positive deceptive type is probably most usual, and has been sufficiently established by all previous experiments reported within the reaction-time field. Table I. gives a summary and analysis of the quantitative reaction time results obtained for the four positive type subjects in this experiment.

Table I

Subject	Total Average "C" Reaction Times	Total Average "O" Reaction Times	Positive Difference Between "C" and "O" Averages	M. V. "C" Reaction Times	M. V. "O" Reaction Times	Positive Difference Between "C" and "O" M. V.	Probable Error	Average Percentage Correct Judgments
A.....	10.8344	14.546	3.7116	1.572	1.8331	1.8331	.1906	81.89
D.....	4.6592	5.5605	.9013	.5192	.5882	.069	.0568	81.57
E.....	7.3848	8.5181	1.1333	.7727	.8118	.0391	.09072	67.42
S.....	10.48	12.2771	1.797	1.598	2.402	.804	.2163	75.00

Hitherto it has generally been assumed in the diagnostic experiments that the m.v. for the critical or deceptive words will be greater than that for the non-critical. In the positive type the results of this experiment substantiate those of the diagnostic investigators. In the case of all four of the subjects of positive type the average m.v. for the deceitful reaction times exceeds the average m.v. for the truthful measurements by a margin that is probably significant. This state of fact also has been well established in the literature.

The introspections of positive type subjects, when analysed to the lowest common denominator, show *fear* appearing in many guises, and this report of fear at crucial points does not appear in the introspection of the negative type subjects. One subject is "afraid that he will obey the instructions of the experimenter rather than his own opposite setting." Another subject feels "withdrawal from the experimenter while attempting to deceive," "feeling of nausea," "shrinking feeling." Both seem palpable expressions of a real fear content. "Tension" is also introspected. This "tension" does not seem to be that indefinable attribute of feeling tone postulated by Wundt, but consists of a very concrete and often localized muscular tension, sometimes in the feet, in clenching of the hands, in throat muscles, breathing muscles, muscles of the mouth, and especially at the diaphragm. One subject described this tension as "a restraint across the ribs, like indigestion." "Excitement" is a second introspective element reported solely by positive type subjects. It is variously described as "nervousness," "exhaustive excitement," and "general emotionality." I shall defer any discussion of the exact nature of this element to the consideration in a later report of blood pressure results obtained in war work which seem to have significant bearing upon the much disputed tridimentional theory of Wundt. Let me anticipate, however, by stating that I take this excitement to be an awareness of the visceral expression of any of the three major emotions expressed in the sympathetic division of the autonomic nervous system or of any other emotion which has flowed over into that system. The confusion of motor impulses which is very evident in the behavior of positive type subjects, is described introspectively as "conflict of more processes during the deceptive lists." Auditory, kinæsthetic, and visual imagery of the experimenter's instructions recur in consciousness and the mind is reported to "become blank." Subjects introspect that, in the truthful reactions, the experimenter's directions reinforce the motor setting while, in the deceptive reactions, above mentioned, imagery tends to break it up. They "forget whether they are adding or subtracting."

The behavior of the positive type subjects, which was carefully noted upon each record by the experimenter, tends to bear out the introspection of the positive type as reported above. The flush, traditionally indicative of guilt, is almost always present with the positive type. Frequent obvious confusions and embarrassments are noted together with evidences in facial expression of great effort, which effort, nevertheless, proves unsuccessful in precluding numerous clearly manifested inhibitions and interferences of motor impulses.

What, then, is the mechanism by which positive type reaction times are delayed? Jung and his school unanimously ascribed the increase of reaction times to emotion, and, from the above reported results, emotion of one sort or another would certainly seem to be the prime cause. We may go a step further, I believe, with the data in hand, and postulate *fear* variously expressed and always more or less Freudianly concealed, even in the introspection, as the prime cause underlying delayed reaction times in the positive type subjects. The witness, unable to concentrate because of the inevitable physiological expressions of his fear, becomes more and more introspectively aware of the fear content itself, and proceeds to exert great effort to suppress this fear, yet, since the only method of successfully suppressing emotion is to eliminate this emotion from the focus of consciousness by concentration, such increased effort only tends to bring intellectual and motor processes into consciousness in addition to the fear content already present. At this step, introspective awareness of the then conscious difficulties of performing the task in hand act as a new and adequate stimulus to a new set of fear reactions (which, of course, constantly enhance themselves through the adrenal autonomic mechanism). The final consequence, which is of course inevitable in any intellectual task performed under distraction, is a conflict and confusion of motor impulses directed to the work in hand, with prolonged inhibitions as the final step. In short, the positive type subject, engaged in the game of deceit, makes a tremendous effort to win, but, like the beginner at golf or tennis, he is unable to concentrate his attention upon the end to be attained, with the result that consciousness of muscular movements and the general modus operandi precludes success and he misses the stroke. The positive type subject is the unsuccessful liar in the detection of whom trial lawyers and jurists have delighted through the centuries.[1]

B. Negative Type

Table II. shows that, taking the analysis of averages just as it stands, the negative type is precisely as clean cut and well-marked as the positive.

Table II

Subject	Total Average "C" Reaction Times	Total Average "O" Reaction Times	Negative Difference Between "C" and "O" Averages	M. V. "C" Reaction Times	M. V. "O" Reaction Times	Negative Difference Between "C" and "O" M. V.	Probable Error	Average Percentage Correct Judgments
C......	9.85147	9.48308	.36839	1.6294	1.5693	.0600	.1831	78.67
G......	11.4955	10.7975	.6980	1.1425	1.1764	.0339	.1837	75.00
K......	8.4701	7.9361	.5341	.8794	.5716	.3078	.1017	75.00

A careful study of the individual data, together with a comparison of the averages for different days, shows, however, that the balance is much more unsettled. The negative attitude seems much harder to maintain. These conclusions are formed principally from the greater relative number of reversals of reaction time behavior; although unlike the reaction times of the mixed group no two of these reversals ever occur together, and in no instance does a reversal of type persist during an entire day's work. Transferring our attention, for the moment, from laboratory experimentation to ordinary everyday experience, I believe it will be commonly recognized that there are individuals among our acquaintance who can lie faster and more fluently than they can tell the truth. This type of liar never has been discovered in association reaction time experiments as reported in the literature, probably for the very simple reason that such individuals were wiped out in the general averages and left unaccounted

[1] It is of interest to note that the positive type give the most extreme and erratic blood pressure changes; but exhibit the least marked breathing changes, according to Benussi.

for when experimenters reported as their final net result a certain percentage of cases successfully detected by association reaction time tests. Indeed, in both the experiments of Washburn and Leach[1] and Yerkes and Berry[2] close inspection of the quantitative results will reveal instances of this unrecognized negative type. Under the conditions of the present experiment it seems clear that, the confusing and complicating factors of the equation having been eliminated, and the bare quantitative reaction time behavior alone having been retained, the negative type appears with unmistakable definiteness. Indeed, during the latter part of the experimentation, after this type had begun to manifest itself, the experimenter was able to bring his percentage of correct judgments from 32 per cent. to 75 per cent. in the cases of subjects *G* and *H*, merely by picking the faster reaction time lists as deceptive.

In two of the three subjects of negative type it will be noted from Table II. that negative m.v. differences of .06 and .3078 appear. Subject *G*, on the other hand, shows a positive m.v. difference of .0339. Just what value is to be placed upon these somewhat equivocal m.v. differences is rather problematical. In general, it would seem to show that while the positive type generally tends to show positive m.v. differences, the negative type is not so definitely correlated with any particular m.v. behavior.

Subjects of the negative type give very little introspective data indicative of fear. Occasionally the subject reported "worry" or some similar emotional state of which fear is probably an ingredient, but these elements were always reported in the fringe of consciousness only. Anger, on the other hand, was frequently introspected and seemed to be aroused in negative type subjects by the least difficulty in arithmetical processes during the deceptive lists. Negative type subjects reported that they felt a certain "tension" coming on (especially in the breathing muscles which would seem to bear on Benussi's results); but that they felt that

[1] *American Journal of Psychology*, 1910, pages 63–67.

[2] *American Journal of Psychology*, 1909, pages 22–37.

this tension would interfere with the speed of their reaction times and they, in consequence, were able voluntarily to relax. It is this relaxation which the positive type of subject never reports himself able to accomplish. No negative type subject reported "excitement." All, however, reported a definite "plan." This element is reported as persisting in the consciousness of negative type subjects both as a motor organization and as an intellectually retained concept and is an element entirely lacking in the deceptive consciousness of the positive type as reported. Both motor setting and concept are relegated to the fringe of consciousness when actual arithmetical processes commence, but certainly both seem to co-exist with the other elements during the entire deceptive consciousness. The plan seems to include: (*a*) plan to go slower on the truthful reactions and to welcome any difficulties which will lengthen these reaction times; (*b*) plan to relax muscular tension whenever it reaches consciousness; (*c*) plan to keep just within the maximum speed during deceptive reactions but not to try to exceed this rate lest subject should "break into inhibitions." In this connection it is interesting to note that both positive and negative types believed that deceptive reactions were much slower than their truthful ones. The negative type subjects reported a "tremendous feeling of effort with occasional necessity for voluntarily inhibiting persistent imagery of one sort or another which may disturb attention." This voluntary inhibition seems to be successful with the negative type as evidenced in the quantitative results.

The behavior of the negative type subjects, as noted by the experimenter, was almost wholly calm, confident, and showed a high degree of intellectual concentration. The guilty flush was almost altogether absent and the manner of the subject was usually more convincing upon deceptive lists than upon truthful ones. Occasionally inhibitions were noted but 95 per cent. of all such inhibitions occurred during the truthful lists, and as reported by the subjects' introspection seemed to be welcomed by the witness and even voluntarily prolonged. The experimenter frequently observed, however, the tendency in negative type subjects to avoid the experimenter's eyes; and this symptom suggests that fear is present with subjects of the negative type also, although such subjects are successful in preventing the emotion from emerging into the focus of attention.

In the negative type we find the successful liar. The psycho-physiological mechanism by which reaction times are delayed with subjects of the positive type is voluntarily controlled and stopped at its inception by negative type witnesses, with the result that we have the intellectual task of sustaining deception at the very center of attention. The mind then being concentrated upon the end to be gained is able to use all its ingenuity and skill in deceiving the experimenter, and, since no mind is capable of exact judgments of time over a long series of reactions, the only flaw in the negative type's efforts at deception lies in said efforts being *too successful.* This, however, is a reaction time difference of much more unstable and nicely balanced character than is the positive type difference, since any temporary discomfiture of the negative type during deception or any chance impairment of general mental fitness will probably delay the deceptive reactions just enough to make them practically indistinguishable from truthful measurements. The negative type subject then is the one who, through the centuries, has had the joke on lawyers and juries.[1]

Table III. shows at a glance why further analysis of two apparently positive type subjects and two as obviously negative type subjects is deemed necessary. Each row of figures on Table III. represents one day's work and it will be seen that the characteristic which differentiates these four subjects from the general positive and negative types is the complete reversal of reaction-time behavior from one type to the other during an entire day's work. It is, of course, to be expected that occasional reversals of reaction-time behavior should occur and where such reversals occur normally scat-

[1] The blood pressure symptoms of the negative type show a more even curve but with just as great or even greater net rise in b.p. than is found in the positive type. According to Benussi the breathing of the successful liar shows greater modification than that of the unsuccessful.

tered through the general tests no significance could be attached thereto; yet, when the subject so completely shifts from one type to the other, that, after the first series of tests on the reversal days, the experimenter was able to attain a percentage of over 90 in his correct judgments of deception by postulating a shift in type, it seems valuable to further subdivide positive and negative type individuals who are subject to these clear-cut reversals. For the sake of convenience in reporting this further analysis the writer has called

C. Mixed Group

Table III

Mixed Group											
Positive						Negative					
Subject B			Subject F			Subject C			Subject I		
Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative
2	2	0	3	1	2	2	0	2	*2	2	0
2	2	0	4	4	0	4	0	4	*4	4	0
2	2	0	4	3	1	4	2	2	*6	6	0
*2	0	2	4	3	1	*3	2	1	*3	3	0
2	2	0	*4	1	3	4	1	3	4	1	3
*2	0	2	4	4	0	4	1	3	4	0	4
4	3	1	4	4	0	*4	3	1	4	1	3
*6	0	6	3	3	0	4	0	4	4	0	4
4	4	0	3	3	0	*3	2	1	4	0	4
4	3	1	*4	0	4	*4	4	0	4	1	3
8	8	0	*6	0	6	4	0	4	4	0	4
6	6	0	4	4	0	3	0	3	4	0	4

these four subjects a mixed group, although it should be understood that the name is not intended in itself to carry any significance. When the above-mentioned reversals are checked up with the behavior of the subject on reversal days, we find that on negative reversal days of positive subjects the physical and mental condition of the subject seems to have been exceptionally good, with frequent evidences of the subject's being in good spirits, or particularly happy over some occurrence in his personal affairs. Also, behavior on these reversal days seems correspondingly to shift to negative type behavior, carrying with it a change in the general tone of the introspection. In short, it appears that when these positive type subjects were in an exceptionally favorable physical and mental condition they were able to attain the successful poise of the negative type in deception. Again, it will be noted in Table III. that subject *I* shows a marked positive reaction time type for the first four days of experimentation, and that thereafter he attained the negative speed of reaction and held it to the finish. This seems clearly to suggest preliminary practice with resultant skill in the game of deception. It is also to be noted that subject *C* reported himself "fatigued or physically tired" on every day when his reaction times lapsed into the positive type.

TABLE IV

Mixed Group					
Type		Positive		Negative	
Subject		B	F	C	I
Totals of reversal days	Average "C." lists	9.0781	13.893	8.175	6.658
	Average "O." lists	7.8844	12.1135	8.76875	8.7709
	Difference	−1.1938	−1.7807	+ .5938	+2.1129
	M. V. "C." lists	1.1889	1.95	.972	.996
	M. V. "O." lists	.9718	1.1188	2.322	2.123
	Difference	− .2171	− .8312	+1.350	+1.127
Totals without reversal days	Average "C." lists	7.308	11.848	9.5416	6.7125
	Average "O." lists	8.492	13.307	8.3887	5.4125
	Difference	+1.1839	+1.459	−1.1529	−1.3
	M. V. "C." lists	.8733	1.502	1.2016	1.0764
	M. V. "O." lists	1.2539	2.427	1.1693	1.0292
	Difference	+ .3806	+ .925	− .0322	− .0472
Total of all days	Average "C." lists	7.7014	12.096	9.38319	6.903
	Average "O." lists	8.3569	13.162	8.43219	6.234
	Difference	+ .6556	+1.066	− .951	− .669
	M. V. "C." lists	.9434	1.556	1.1754	1.0764
	M. V. "O." lists	1.1912	2.269	1.3011	1.0292
	Difference	+ .2912	+ .713	+ .1257	− .0472
	Average per cent. correct judgments	81.61	78.03	80	78.83
	Probable error	.1061	.1828	.1340	.1326

Table IV. shows evidence, in further analysis of reversal day averages as compared with total type averages for above four subjects, tending to indicate sharp contrasts in quantitative measurement between reversal day and general type reactions. Of course these reversal averages could be left buried in the general quantitative average, but above further analysis shows the sharp distinctness of the averages of the unified groups when same are separated. In general the study of Table IV. is intended to contrast the two markedly opposite statistical tendencies of the reaction times of mixed group subjects. It may be noted that both positively inclined subjects in the mixed group show positive m.v. differences for their total average m.v., while negative m.v. differences appear for the negative reversal groups. The two negatively inclined subjects of the mixed group, moreover, show negative total m.v. differences, with positive m.v. differences for positive reversal groups. The writer does not attach any great significance to this m.v. as a criterion, since the actual differences were very small and would be very hard to follow up in any practical test. It is of interest, however, to note that, other factors being equal, the m.v. differences seem to follow the general type of the subject.

It appears to the writer that the mixed group probably takes in the whole middle range of liars. Only very good liars succeed in keeping every single day's work up to negative type deception speed; and most negative type liars probably have their "off days" when they are unable to command their attention and mind to the high degree necessary to shorten the reaction times during deception. On the other hand it is a very poor liar who is never able to attain sufficient concentration to throw himself for a day into the class of successful deceivers. It would seem to the writer that, after practice in deception was eliminated, the predominant type of any subject could easily be detected by a sufficiently long series of measurements; and it may be said that the experimenter in the present experiment, after once determining the general type of a subject, found reversal days exceptionally productive of accurate judgments because, during such reversals, the unnatural tendency of the subject seemed to be exaggerated and very consistent.

Psycho-legal Conclusions

It seems to the writer that this experiment has added one stone to the foundation necessary to qualify the psychological expert on deception before the court. The district attorney points to the flushed face and obvious confusion of a witness for the defense; but our psychologist is put on the stand to testify that he has proved the defendant to be of negative deception type. Attorney for the defense then can place before the jury the psychological fact that, were a negative type witness lying, no such flushed face and obvious confusion would appear, for the negative type witness is the gifted liar who would be expected to exhibit much less confusion were he lying than if he were telling the truth. Or our expert may succeed in discrediting the star witness for the state solely because said witness, being a subject of negative reaction time type, was oversuccessful in going through a terrific cross examination with unbroken rapidity and consistency. The stress and high tension of the layman's mildest experience in court, either as witness or as a criminal defendant, should, I believe, tend to keep the individual well within his predominant type; yet our knowedge that there exists a mixed group subject to sharp reversals of reaction time behavior, should lead the boldest psychological expert to check up his reaction time results with other test data before venturing an opinion in court. In any event, if psychological authority can succeed generally in disabusing the minds of the average jury of the conviction that the slightest hesitation, delay, or confusion in the testimony of any witness is indicative of deception, or that rapid and clever answers in cross-examination are indubitable earmarks of a white conscience, a constructive development in the weighing of evidence should be effected in an innumerable number of trial cases.

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AN EXPERIMENTAL STUDY OF DIZZINESS[1]

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Table of Contents

I. Introduction

A. Historical Setting.[2] Scientific interest in dizziness and vertigo began as long ago as 1795, when Erasmus Darwin[3] first summarized the facts as then known. Purkinje, however, seems to have been the first writer to put the matter to a test in the laboratory.[4] His experiments, which have

[1] From the Psychological Laboratory of the University of Illinois.

[2] A survey of the principal work already done on the structure and functions of the semicircular canals, together with a more detailed account of the history of the psychological problem, is soon to appear.

[3] Darwin, E., 'Zoönomia, the Laws of Organic Life,' 1795 (3d ed., 1801), pp. 327–356.

[4] Purkinje, J., 'Beiträge zur näheren Kenntnis des Schwindels nach autognostischen Daten,' *Med. Jahrb. d. Osterr. Staates*, 1820, 6, 79–125.

since become popularized in the parlor game of whirling with inclined head about a stick, led to a remarkably complete account of the apparent movement of objects in the visual field after rotation. He noted, for example, that the curious movement of such objects always took place in a plane perpendicular to the axis of rotation, no matter how the head was subsequently moved. By way of explanation, Purkinje related all of his observed facts to inertia of the soft parts of the body, particularly the brain. So impressive, it seems, was his demonstration that, for a number of years, no further descriptions were given. In fact, it was not until 1874 that anything further was added to the psychological description of dizziness. In that year, however, Crum Brown[1] and Mach[2] independently attacked the problem in such a way as to show just where the future task was to lie. Crum Brown was the first to use the phrase 'the sensation of rotation,' and since his time frequent references have been made to an 'equilibratory sensation,' to a 'sensation of motion,' and to 'static sensations.' The tendency towards this usage was undoubtedly strengthened by an accumulating body of evidence that a new end-organ discovered, in the years just preceding, in the semicircular canals was closely related to bodily movement and to the maintenance of equilibrium. Crum Brown concluded his investigations in the following manner: "As far as I am aware, the sense of rotation has not hitherto been recognized either by physiologists or by psychologists as a distinct sense; but a little consideration and a few experiments seem to me to be enough to show that it really is so. By means of this sense we are able to determine,—*a*, the axis about which rotation takes place; *b*, the direction of the rotation; and *c*, its rate. . . . In ordinary circumstances we

[1] Crum Brown, A., 'A Preliminary Note on the Sense of Rotation and the Functions of the Semicircular Canals of the Internal Ear,' *Proc. Roy. Soc. Edin.*, 1874, 8, 255–257; 'On the Semicircular Canals of the Internal Ear,' *Proc. Roy. Soc. Edin.*, 1874, 8, 370–371; 'On the Sense of Rotation and the Anatomy and Physiology of the Semicircular Canals of the Internal Ear,' *J. of Anat. and Physiol.*, 1874, 8, 322–331.

[2] Mach, E., 'Physikalische Versuche über den Gleichgewichtssinn des Menschen,' *Wiener Sitz. d. Kais. Akad. d. wiss.*, 1873, 68, 124–140; 'Grundlinien der Lehre von den Bewegungsempfindung,' Leipzig, 1875.

do not wholly depend upon this sense for such information. Sight, hearing, touch, and the muscular sense assist us in determining the direction and amount of our motions of rotation, as well as of those of translation; but if we purposely deprive ourselves of such aids we find that we can still determine with considerable accuracy, the axis, the direction, and the rate of rotation."[1]

Crum Brown's reference to the part played by other sense-departments applies also to another mode of attacking the question, *viz.*, that adopted by Mach. We have already seen that Purkinje sought to explain the facts of rotation by appealing to the inertia of the soft parts of the body and to the sensations resulting therefrom. Early in the history of the semicircular canals the close relation between the canals and the musculature of the body had been noted. Furthermore, a good deal of attention was given to Sir Charles Bell's alleged 'muscular sense.' It is not strange, therefore, that the perception of position and of movements came to be described in terms of sensations from the skin, the joints, the muscles, and the eyes. In 1875 Mach came to the conclusion that although "die Bewegungsempfindungen lassen sich nicht erklären durch die Wirkung der sensiblen Elemente der Knochen und des Bindegewebes, nicht durch die Wirkung der Haut, der Muskel, des Blutes oder des Hirns," nevertheless "ein Mitwerken dieser Factoren bei Erkenntniss der Locomotion nicht vollständig ausgeschlossen werden kann."[2] Later he concluded that of the mental processes involved in the perception the most important is that of vision which, combined with 'skin-sensation' and 'with changing innervations' yields 'a conception of our body as in motion.' Mach doubted that 'special motor sensations exist which proceed from this apparatus (*i.e.*, the canals) as from a sense-organ'; but he thought that the apparatus simply 'disengages innervations after the manner of reflexes.' He regarded as untenable, then, the view that knowledge of equilibrium and of movement is directly mediated by means of the semi-

[1] Crum Brown, A., *Proc. Roy. Soc. Edin.*, 1874, 8, 255.

[2] Mach, E., 'Grundlinien der Lehre von den Bewegungsempfindungen,' Leipzig, 1875, p. 125.

circular canals.[1] In a similar way, Wundt has questioned the existence of special sensory qualities from the canals, since the deaf and dumb acquire "mit Hilfe anderer Sinne, namentlich des Tast- und Gesichtssinnes, eine so vollständige Orientierungsfähigkeit im Raume, dass er, so schwer er den Mangel des Gehörsempfinden mag, den dieses Orientierungsorgans kaum zu bemerken scheint. . . . Eine Organ, das in solcher Weise durch andere vertreten wird, kann aber keine spezifische, nur ihm eigentümliche Funktion haben."[2] Wundt finds, however, that the 'tonische Organ' is functionally related to the other senses. These facts lead him to conclude that "der tonische Sinn kein für sich isolierbares Vorstellungsgebiet umfasst, da er niemals für sich allein Wahrnehmungen des Gleichgewichts und der Bewegungen des Körpers oder gar des Raumes im allgemeinen vollziehen kann, ähnlich wie ja selbst der Sehende niemals durch den blossen Tastsinn Wahrnehmungen äusserer bestateter Objekte gewinnt, bei denen nicht, auch ohne dass er es will, Assoziationen mit den Vorstellungen des Gesichtssinnes mitwirken." The semicircular canals are for him, 'a kind of inner organ of touch.'[3]

Holt has made the first really serious attempt to analyze the experience of rotation. He has found, by experiment, that rotation yields three groups of processes. The first group is made up of 'sensations which proceed from extra-peripheral stimuli,' such as currents of air, light and sound. The second group 'are the sensations from proprio-ceptive organs in joints, muscles and other tissues, which are stimulated by the inertia of the trunk, limbs, internal organs, and even perhaps of the blood, and by their centrifugal moment.' Finally, in the third group are 'the true sensations of motion'; and of the three groups these 'true sensations of motion' are the only ones essential. On examination, however, these sensations prove to be merely the feeling that either the body or objects about the body are in rotation or else they are more or less

[1] Same, 'Analysis of the Sensations' (tr. Williams), 1897, pp. 65-66; 73-77.
[2] Wundt, W., 'Grundzüge der physiologische Psychologie,' (6th ed.), 1910, 2, 507.
[3] Wundt, W., *op. cit.*, p. 508.

parallel to 'innervations of one kind and another.'[1] Recent writers of text-books have met the problem in the traditional way. The usual descriptions of the ear and of the flow of the liquid and of the reflex nature of the responses, together with a description of the field of vision during dizziness, make up the chapters on this 'sense-department.' In its simplest form the psychological problem has been stated thus: Does the excitation of the canals result in a new sensory quality or does the experience gain its character because of the kind and number of other sensory components? Pillsbury states the question in this form and answers it provisionally only. He "speaks for the view that the sensation of giddiness is not a true sensation of the vestibular nerve, but rather a sensation of the alimentary canal, due to reflexes excited by the organs of equilibration" and that "visual sensations, kinæsthetic sensations, and sensations due to displacement of the large visceral organs also aid in keeping the balance and in appreciating the movements of the body."[2] Titchener is persuaded that the canals "act reflexly, without furnishing sensations, or at any rate furnish sensations of little strength, and of a pressure-like kind that blends indistinguishably with the kinæsthetic sensations from the tissues beneath the skin."[3] But he also finds, under other conditions, sensations appearing as a compression or lightness in the head, and a distinct feeling of squeeze in the region of the ears.[4] Warren is still more positive of vestibular sensations of a 'particular quality.' The "sensations of position and sensations of motion" are not only different in quality from one another but "the sensations from the three semicircular canals may also differ in quality" just as local signs are found in the sense of touch.[5] On the other hand, Külpe is quite doubtful of a new sensory quality, although he does admit that 'giddiness' may be taken as

[1] Holt, E. B., 'On Ocular Nystagmus and the Localization of Sensory Data during Dizziness,' *Psychol. Rev.*, 1909, 16, 377–397.

[2] Pillsbury, W. B., 'Fundamentals of Psychology,' 1916, p. 203.

[3] Titchener, E. B., 'Text-book of Psychology,' 1910, 173–182.

[4] *Op. cit.*, pp. 179–180. In his more recent book (A beginner's psychology, 1915, p. 56) Titchener speaks more positively of "the sensation of 'swimming' when the head is sharply jerked, and the sensation of dizziness when we twirl on our heels."

[5] Warren, H. C., 'Human Psychology,' 1919, p. 213.

representing the activity of the canals in consciousness; but "it is difficult to say what the common element in sensations of giddiness is when we have abstracted from the objective disturbance of the coördination of movements and its various concomitant phenomena."[1]

B. The Problems.—It is clear, therefore, that no common opinion is to be found among the writers we have mentioned in this brief historical survey. In fact, we can point out at once two phases of a problem upon which further evidence is needed. In the first place, there is an urgent demand for a more detailed and adequate description of all the processes that arise when a subject is rotated. Furthermore, it will be necessary to classify these processes as regards their importance in the experience of dizziness. That is to say, there are undoubtedly some processes more or less essential to the appreciation of rotation and the selection of these processes must be based upon experimental evidence. This task leads directly to the second phase of the problem. Is there a process or group of processes which stand related to the end-organs in the semicircular canals as the visual processes stand related to the rods and cones in the retina?

Another problem concerns us in this investigation. It springs from a recent clinical use of the term 'vertigo.' We refer to a current distinction drawn by the otologists between vertigo and an ocular effect of rotation commonly known as 'nystagmus.'[2] Now nystagmus is—as they maintain—a simple reflex, and it is not, therefore, subject to change under practice. In a recent article, we have shown that this contention is without foundation.[3] We have shown, on the contrary, that the ocular effects, as well as all the other organic effects of rotation, tend to decrease and finally to disappear under repetition. Our parallel problem here, then, will be to determine the probable modification of the mental results of rotation. In brief, this is our present

[1] Külpe, O., Outlines of Psychology, (Tr. Titchener), 1895, pp. 150 ff, p. 378.

[2] See, for example, Jones, I. H., 'Equilibrium and Vertigo,' 1918, p. 5.

[3] Griffith, C. R., 'The Organic Effects of Repeated Bodily Rotation,' This Journal, 1920, 3, p. 46. See also Griffith, C. R., 'The Decrease of After-Nystagmus during Repeated Rotation,' *The Laryngosope*, 1920, 30, 129-137.

problem, (*a*) What are the mental processes aroused during stimulation of the end-organs of the semicircular canals and what is their relative significance to the experience of rotation as a whole? And (*b*) what effect will continued repetition and other factors have upon these mental processes?

II. The Mental Effects of Rotation

Our method of experimentation has already been described in a previous article, which was devoted to the bodily consequences of rotation.[1] The method was the method of periodic repetition. In each trial our subjects were revolved ten times.[2] There were ten successive trials taken each day for a considerable number of days. Introspective reports were called for after each trial. The instructions were varied from time to time, as we shall presently designate. We varied our conditions one after another, and we submitted the whole experience to an elaborate fractionation in time. In congruence with previous investigators, we have found that continued rotation results in an experience which is highly complex. We now proceed to an analytic description of the experience on its mental side, first by way of gross description and afterward with detailed analyses.

A. Principal and Accessory Processes.—Of the principal processes that are aroused by rotation we may mention clear kinæsthesis from the eyes, from the head and neck, and from the limbs. Also, there are visceral pressures, coolnesses, and surging or 'welling' qualities. Pressure and even pain may be localized deep within the head and high up in the nasal passages, while all kinds of pulling strains are reported from the region of the diaphragm. Occasionally some of these processes become very clear and reach a climax in the experience of nausea. The processes of this group appear to be aroused by some other cause than the inertia of the body or

[1] Griffith, C. R., this Journal, 1920, 3, p. 20.

[2] The writer wishes to express his appreciation of the services of the subjects. They were V. B. Adams, M. A. Beard, A. R. D'Angelo, W. A. Diesel, M. E. Broom, L. K. Cecil, A. R. Elliott, L. R. Raines, R. W. Wuestermann. H. C. Burleson noted the results from the writer. The writer wishes also to acknowledge his indebtedenss to Professor Madison Bentley for his constructive criticism and supervision throughout the investigation.

the fact that the body is rotated in a resistant medium. On the other hand, and in accord with Holt,[1] we have put into the group of accessory processes such events as are obviously aroused by the inertia of the body, *e.g.*, changing pressure on the buttocks and at the back of the head. In addition, a slightly cool pressure can be localized on the side of the face toward rotation and there is a continuous displacement of the direction of sounds and of lights and shadows.

III. The Factors which Modify the Mental Results of Rotation

All that we have said elsewhere of the organic conditions and results of rotation holds true, as well, for the mental concomitants and effects. Not only is the complexity of mental processes markedly reduced under periodic repetition, but the numerous effects of rotation are just as sensitive to variation in their physical and mental setting as we have found the organic effects to be to variations in the physical and organic conditions.

A. Physical and Physiological Factors.—We are here obliged to insist again that the organic and mental results of rotation are not two different groups but that they constitute a single experience. The organic group provides the conditions for the modifications of mind. We may expect, therefore, that any change in their appearance, as a result of changes in the physical conditions of arousal, will be paralleled by corresponding mental changes. We have found, for example, that variations in the rate and number of turnings, the mode of stopping, the time of day, the length of an interval between turnings, and the general organic set are conditions under which the organic results are modified. The mental effects are likewise modified under the same conditions. For example, the intensity of apparent movement in the visual field and the feeling of 'dizziness' are greatly increased by a sudden stop or by a reversal in the direction of actual bodily movement. These effects are likewise of greater intensity

[1] Holt, E. B., *Psychol. Rev.*, 1919, 16, 377.

when the organic state is abnormal, or when no rest intervals are given, or when the rate of rotation is increased, and so on.

B. Periodic Repetition.—A refined analysis of the mental effects of rotation is made possible only by the proper fractionation of the whole course of events. As we have seen, under rotation mind is exceedingly complex. The mere fact that such gross designations as 'vertigo' and 'dizziness' have delayed further analysis for decades bears testimony to the unusual character of the mental effects produced. We have already pointed out that most of the complexes involved are kinæsthetic and organic, and every introspective psychologist knows how tenaciously such complexes resist final analysis. The very discovery, however, that this complexity does decrease under repetition is an advantage; for the gradual disappearance of one process after another makes possible a fuller description of those that remain. And again, the disappearance of certain processes betrays their presence in a preceding stage. The period of rotation has, for our purposes, been fractionated as follows: (*a*) The period preliminary to any given day's rotation; (*b*) the period beginning with the 'ready' signal and ending with the perception of the first impulse of rotation; (*c*) the period during which rotation is attaining maximal speed, *i.e.*, the first half-turn; (*d*) the period during which the speed of rotation is constant; (*e*) the period from the application of the brake to the inception of after-nystagmus; (*f*) the period from the appearance to the disappearance of the after-nystagmus; (*g*) the period from the end of the after-nystagmus to the next turn; (*h*) the period following the completion of the day's series. We shall now proceed to discuss each of these periods.

(*a*) The period preliminary to the day's rotational series. The description of this period is, of course, quite incidental. There are aroused, however, certain processes which supply a constant factor throughout the experiment and which have, on that account, to be substracted each time from the whole group of processes which appear under rotation. Among them are the tactual processes from contact with the chair and various cutaneous and subcutaneous pressures indicative of the maintenance of posture. The subject is not in the most comfortable position, for the head is bent forward and, in some cases, the knees are slightly cramped. This position gives rise to achy pulls in the back of the neck and the characteristic fatigue-like pressure about the knees from holding one position for any length of time. It has been found, however, that these processes which arise from the sitting position can be readily detected and left out of account when describing the distinctive mental effects of rotation. The five subjects from whom introspective reports were obtained were instructed to take account of these processes before each day's series and to discriminate them as carefully as possible from the processes set up by rotation.

(*b*) The period beginning with the 'ready' signal and ending with the perception of the first impulse of rotation. The second period corresponds to the preparatory period (Vorperiode) of the reaction experiment. Before rotation begins there is a flood of visual imagery and of kinæsthetic processes. This is the only time at which these processes are at all numerous, for the rotation period itself is peculiarly devoid of imagery so far as our subjects reported. The kinæsthetic processes come as characteristic pervasive thrills, tension about the abdomen and about the chest as in holding the breath.

"I felt this time just as if I had plunged into a tank of cold water. There is a peculiar gripping about the chest and a thrilly feeling deep inside." (P.)

For some subjects these processes mean a fairly complete reinstatement of past experiences in swings, on merry-go-rounds, and especially in high places. All of these processes form a background to the 'feel' of expectation which is carried in part by the verbal kinæsthesis meaning 'what is it going to be like?" and in part by a pressure quality from the throat, diffuse kinæsthetic pressures and pulls from the whole body, meaning a kind of setting or preparation against something about to happen, and a very clear thrilly tingling about the 'pit of the stomach' which radiates in all directions and which is more or less pleasantly toned. The kinæsthetic processes are localized in the lower limbs, the arms and the buttocks and in the facial muscles, especially about the mouth. After several turnings, the visual and other extraneous imagery almost completely drops out. But the processes constituting 'expectancy' are fairly constant and may reappear at any time. They may even become quite definite in their anticipatory reference.

"Had a very peculiar experience this time. The processes meaning expectation all suggested rotation to the left. Instead I was rotated to the right and there was instituted the same kind of reorganization as is usually experienced only upon stopping." (P.)

Toward the end of the series, however, they become fleeting, more like a suggestion of expectancy, just as the experimenter puts his hand to the chair to begin rotation. As would be expected, all of the processes of 'expectancy' blend into the groups of similar processes aroused just after rotation begins. In case any subject has been previously nauseated or has been sea-sick, this second period is characterized also by clear olfactory imagery. The writer has been surprised at the readiness with which this imagery is aroused as a part of the 'feel' of expectancy.[1] They carry the meaning of disgust and repugnance and are, in some cases, the signal for the arousal of a kind of psychophysical attitude expressed in the exclamation: "I am sure to be nauseated!" In case a subject has become nauseated by the turning, (care was continually taken to prevent it when possible) the olfactory processes form a fairly constant part of the preliminary period through a large part of the series, even though all hint of nausea has long since dropped out. In such cases, it augments the intensity of the 'feeling of expectation,' especially the organic components, and leads to a kind of permanent psychophysical disposition just the opposite to that induced in one of the later periods.

(*c*) The period during which the rotation is attaining maximal speed. Extraneous processes which are similar to

[1] It may be that psychologists would not have failed, as they usually have, to discover olfactory images had they regarded such appropriate occasions as the present.

Holt's first group[1] can be briefly described. It is difficult to estimate just what part they play in producing the apprehension of rotation during the first turns of a series. That it may be a large part is suggested by the fact that, as the series proceeds and other processes drop out, they come to carry almost the whole meaning of rotation. Among the processes found in our experiment are (1) a changed distribution and intensity of pressure from the occiput (head-rest), (2) kinæsthetic sensations from the arms and the lower limbs aroused by resisting the inertia of the body and (3), most prominent of all, a rather clear and intense change in the pressures about the buttocks, probably due to a slight shift in the center of gravity of the body. There is also a light cool pressure on the side of the face in the direction of rotation, lights and shadows begin to move across the field of vision, and there is an apparent change in the direction of extraneous sounds.

Other processes are, however, of more immediate interest. The clearest of these is (4) an intense kinæsthetic play about the eyes. Distinct changes in the quale of these processes can be detected. There is a kind of pressure about the eyeballs which increases in intensity and in distribution and then suddenly gives way, only to return again, and so on in rhythmic fashion. Our observers found it impossible to localize this kinæsthesis on one side of the eyeball rather than on the other. The whole eyeball seemed affected. As the series proceeds, the intensity of this first kinæsthetic flutter about the eyes seems to decrease until, at the end, it has almost, if not quite, disappeared.

"Just as rotation begins there is a decided play of tension or pulling about the eyes." (P.)

"As rotation starts there is a kind of kinæsthetic setting about the eyes." (K.)

"Just as rotation started I felt the usual kinæsthetic flutter about the eyes." (L.)

"The usual rhythmic processes about the eyes hardly ever occur any more. The rotation starts with no more concern than as if I shook my head." (P.)

[1] Holt, E. B., *Psychol. Rev.*, 1909, **16**, 377.

Just before the speed of rotation reaches its maximum, there is aroused in unpractised subjects a tremendously complex mass of kinæsthetic and organic processes from almost the entire body. Constituents can be localized more clearly in the lower limbs, the side of the body toward the direction of rotation, in the arms and the shoulders, all about the chest, in the back and, most clearly of all, in the neck and about the face. During the first periods of rotation these processes arise immediately after rotation begins and reach a high degree of complexity by the time the rotation has come to its maximal speed. The organic or visceral processes then continue in an intensified form from the processes making up the 'feel' of expectancy. This is particularly true where those processes have carried an incipient nauseation. After rotation has been continued for some time these processes do not arise until the end of the second or third turn. All that is left in this period after long practice is the fleeting kinæsthetic flutter about the eyes just before rotation comes to full speed. The swimming 'feel' does not enter into this preliminary period.

"The ocular kinæsthesis at starting has now become but a faint quiver. It disappears during rotation." (K.)

(*d*) The period during which the speed of rotation is constant. The complexity of this period of rotation is at first so great as to make an adequate description almost impossible. It is not strange that the whole group of events making up the period have been dismissed with such words as 'vertigo' or 'a swimming sensation.' As before, the extraneous processes may be profitably eliminated. The pressure about the back of the head arising from the head-rest becomes in this period a clue to the proper position of the head for the head tends to swing in the direction of rotation. The most prominent of the other processes are the succession of lights and shadows and of sounds that flit by. The cool pressure which begins with the rotation, continues throughout this period, but it is of small significance during first turnings. So also with the pressure from the arms and the buttocks. One group of extraneous processes, however, does play a special part in the experience. For one observer, visual and tactual images of a long, smooth, oily, steel rail, like the smoothness of a piston rod in an engine, carry very largely the meaning of motion.

"There was present this time a kind of tactual imagery of smoothness and oiliness which took up the whole of consciousness. The effects of rotation were not nearly so marked." (P.) Sometimes the kinæsthetic and organic qualities so completely disappear that the subject regards himself as standing still and the visual shadows and the auditory qualities as floating past him. On these occasions, the after-effects are greatly reduced—especially the organic complexes. On the other hand, there is a type of visual imagery which carries the meaning of a dizzy height around which the subject is being whirled and from which he is looking down into a dismal abyss. Such imagery was usually accompanied by a marked increase in all the muscular and visceral effects of rotation; the after-nystagmus was longer and the whole experience much troubled. The appearance of these or of any other imaginal materials of a similar kind was, however, rare.

"Frequently today had visual imagery of various moving objects and some verbal kinæsthesis in counting the number of rotations, but such processes are generally rare." (P.)

As the daily sessions continue, all the effects become greatly reduced in clearness, duration, intensity and qualitative characteristics. As this reduction takes place, there is a corresponding increase in the part that the extraneous processes play. Finally, the whole meaning of rotation is carried by them, although the kinæsthesis in the neck is still clear. This latter gives the meaning of some kind of movement and it might be found that, with the other processes eliminated, it is sufficient to carry the meaning of rotation. It still retains its characteristic distribution on the side of the body toward rotation, as do also the vague remnants of the processes that were formerly so clearly localized about the arms and legs. It can safely be said, however, that as rotation continues, the perception of it comes to be carried more and more in terms of the extraneous processes rather than in terms of the remnants of kinæsthesis. The extraneous processes are almost the only ones which maintain their intensity unchanged whatever the amount of practice. As the other processes begin to drop out, these processes come to play a larger and still larger part until at last they give almost the entire perception of the direction of rotation.

"Rotation seems to be carried almost entirely in terms of the extraneous processes today." (K.)

"The perception of movement was carried this time very largely in terms of tactual, thermal and auditory processes." (K.)

"Was quite sure this time that, in addition to the usual kinæsthesis about the neck and face, the only processes which carried the meaning of the direction of rotation were the tactual processes—coolness (of air), and auditory qualities." (P.)

"The perception of rotation carried very largely in terms of coolness, etc., today. I believe that if they were not present, I should not be able to distinguish between rotation and linear movement. The neck and the face kinæsthesis might carry the meaning; but I doubt it." (M.)

All of the organic and kinæsthetic processes which were aroused during the starting period run their course with a good deal of intensity as rotation continues. The rhythmic kinæsthesis about the eyes becomes almost painful. By the time the first turn is completed the whole body is involved. The visceral qualities may become violent. The somewhat diffuse thrilly qualities of 'expectancy' suddenly become localized into a kind of drawing pressure about the 'pit of the stomach' and gradually rise to a point just below the diaphragm where part of the intensity is lost in favor of a pulling pressure throughout the diaphragm and in the abdominal walls and a part in favor of tension about the colon and the anus. From the diaphragm there ascends into the œsophagus, a heavy pressure which causes a sort of gulping. (P.) Just as the pressure in the diaphragm comes to full intensity, a very diffuse warmth spreads over the whole body, perspiration starts, a bad taste appears in the mouth, there is an excessive flow of saliva, an achy spasmodic pressure about the stomach and unless rotation is stopped, a 'nauseated feeling' spreads over the whole body and vomiting may take place. (P, N, L, K.) In the meantime, a lumpy pressure or swelling has arisen deep within the chest and has ascended to the head, where it may spread out as diffuse achy pressure, or it may become localized about the side of the head toward rotation or over the eyes. (P, K.) There then appears a kind of swimming within the head and with this feeling the individuality of all the other effects is lost. Even the very clear tensions and pulls in the arms, neck, back, legs, face, and especially the eyes, seem to lose their specific character and become fused into the 'swirling sensation.' To quote from the records:

"There was a kind of stuffy ache in the stomach and in the higher viscera, and a kind of tenderness and pressure at the anus." (P.)

"There was an unpleasant kind of pressure low in the trunk and a periodic pressure and pulling about the anus." (N.)

"Found this time a kind of tension about the anus and similar in quality to that about the chest." (P.)

"Felt decided tension in the diaphragm, the abdominal wall and up through the chest, becoming less clear about the throat but very clear at the base of the brain and in the head. There was a distinct 'swelling' pressure in the head." (K.)

"Felt this time a kind of spasmodic pressure or pulling at the lower end of the œsophagus and in the stomach region. Also distinct pressure about the anus." (P.)

"There was a general organic thrill of expectancy just before rotation began. This became definite but of same quality; after rotation started was localized high in the viscera. After two or three revolutions it became fused with other processes from the legs, arms, eyes, etc., so that it was hard to isolate them any longer. Just then a swimming quality developed. Later a distinct pressure bobbed up within the lungs near the heart and there was a slight 'choking' complex." (K.)

"Vertigo seems to be entirely a matter of a peculiar fullness in the head and a pressure localized almost painfully about the nose and in the eyes." (P.)

"Very peculiar change this time, Previously it has been hard to single out the items because they seemed so confused and the peculiar whirling sensation in the head which seemed to depend upon an odd kind of pressure and lightness confused me. This time, however, the whole experience seemed broken up. There were many specific items to be found but the swimming sensation was no longer present." (M.)

"This time the whole experience was again broken up save just at the last when I seemed to lose control of myself. Confusion set in and there was immediately a fleeting return of that whirling sensation in the head." (P.)

"This time, just as the confusion and whirling sensation began, I set myself as definitely as I could against it. The setting was carried mainly in terms of pressure in the feet and buttocks as if I were trying to hold back an automobile. With this setting the confusion disappeared, as did also the swimming, and I was then able to describe the several single items." (N.)

"Seems as if there was a large fluid mass in the head which was whirling around very rapidly and which feels very much as a flowing stream of water looks. This became very confusing until I set myself against it. It then seemed to break up into pressure in the head and also a lightness on one side, and also pressures and pulls all about the face and neck and especially in the eyes." (P.)

It is clear that the swimming sensation is not a permanent part of the experience of rotation. Within a few turns it has entirely disappeared and does not return again. In fact, as rotation continues from day to day, all of these events decrease in intensity and duration, and in the end almost all of them disappear.

"I found the usual processes present this time but again all of them were barely noticeable. There was slight kinæsthesis about the eyes as rotation started, and then in the neck and arms and a little about the chest (slight tendency to hold breath yet). There were only very meager pressure qualities from the viscera and in the head." (P.) "Tension about the rectum, abdominal wall, some internal pressure and chest kinæsthesis all of low intensity are the items in a kind of setting or attitude of tension which is present during each period of rotation now. This is coupled with the neck-and-arm and face tension. Both of these are different from those qualities which seem like an internal rising coolness which becomes pressure in the neck and in the head and nose." (K.) "No particular processes today. Everything has become but a vague hint of what it used to be." (M, N.)

The visceral qualities change a little as practice continues. It is as if they were exposed to the air and a light cool breeze were blowing upon them, giving a very curious but delightfully pleasant thrill. This thrilly quality lasts but a turn or two and then rises and changes into a weak and diffuse pressure about the diaphragm. Finally even this thrilly quality drops out and all that is left is a dim pressure about the diaphragm.

"A very diffuse head and stomach pressure as from the presence of a slightly distended balloon seems to be about all that is left of the former organic complexity." (P.)

"The eye kinæsthesis has become reduced to vague hints.' (K.)

"Dim organic processes pretty well diffused through the diaphragm and lower œsophagus were about all this time." (K.)

"Some kinæsthesis about the chest, as in holding the breath, very slight organic pressures and pullings in the visceral organs, and some pressure which arose and spread out through the diaphragm and about the lungs." (M.)

With the disappearance of some of these more prominent items, it is possible to attend to a new group.

"Behind the group of clear and distinct processes which formerly seemed to make up the core of vertigo, is a dim, diffuse, undifferentiated mass of processes no one of which seems to rise above another, but on various occasions, such as stopping, the mass seems to shift in color or size or position, or something of that kind. This shift is very subtle, like the changing curtains of an aurora borealis. When one watches an aurora it seems to change; but what is changed or at which place cannot be described. You know it has changed. So here, these background processes change as rotation starts and then, as full speed is attained, there is a settling back or retreating until the stopping period. Occasionally definite processes shoot our like a streamer from the aurora. These processes are always pulls or tensions or pressures from the trunk near the diaphragm and often there is a feeling of warmth or of coolness from the visceral region. The warmth is localized higher up and more peripheral than the cold. The affective quality is variable." (P.)

Just what neural conditions are responsible for this background of undifferentiated processes is hard to say although the work of Camis[1] and others suggests that the stimulation of the canals may have a more definite effect upon vascular and motor conditions than has as yet been determined. Historically, of course, the notion that there are movements of the viscera and of other soft parts of the body which account for the perception of movement has been a common one. Four of the subjects reported this organic shift just after the major complexity of vertigo was gone. It seems to have dropped out during the later trials.

A third group of organic processes consists of a very dull kind of pressure or 'swelling' which seems to arise out of the trunk and ascend into the head like the raising of the level of water in a tank. These are the processes which come most directly from the disappearance of vertigo. As rotation continues, this pressure becomes more intense and clear, being localized sometimes about the ears, sometimes over the eyes, but generally deep within the head. Along with this pressure there is also a decided pressure from the upper part of the nose, and the internal part of the nose feels raw—just as if one were about to suffer a recurrent nose-bleed.

"Found this time that the pressure about the upper part

[1] Camis, M., 'Contributions à la physiologie du labyrinthe; Observations ulterieures sur les phenomènes vaso-moteurs,' *Arch. ital. de biol.*, 1911, 56, 277–288; "La glycosurie consecutive à la destruction des canaux demi-circulaires chez les chiens,' *ibid.*, 289–300.

of the nose, as in nose-bleed, was more clear and distinct. The nose became tender and raw." (P.)

"The pressure rising at the back of the head became heavy and achy this time as rotation continued." (K.)

"The headache I had at the beginning of the turning was changed into a distinct pressure quality high up in the nose." (L.)

"Pressure in nose this time and also a kind of fresh feeling of tenderness as if one had just douched the nasal passages with salt water." (P.)

Here again, the effect of practice is to decrease in intensity and duration all of the processes concerned. Other things being equal, the decrease in the intensity of the processes aroused by rotation proceeds without interruption until all that is left during the actual rotation-period is the rather diffuse kinæsthetic quality from the face and neck.

"All qualities are becoming less and less intense and shorter, save those from the neck and about the face." (N.)

These processes, which were not at all clear, gave promise of disappearing altogether. As practice continued they arose later and later in the turning period until, at the end of the series, they appeared only when the rotation was half over.

"Neck-kinæsthesis and nose-pressure not apparent this time until the rotation was half over. None of these processes ever appear any more until late in the rotation-period." (P.)

"Did not get anything clear until rotation was almost over. Then only the vaguest hints of pressure from the viscera and kinæsthesis from the face." (M.)

There was no change in the quality for rotation to the right or to the left save that the kinæsthesis was always clearer on the side toward rotation, the pressure from the head appearing to change location with the direction of rotation, especially late in the series. Just what kind of a change this was could not be determined. Also all processes seemed to be a little clearer during rotation to the right than to the left, with two subjects.

"All processes seemed to be a little clearer and more intense during rotation to the right." (P.)

"All processes were less clear and less durable this time." (Rotation to the left.) (K.)

(*e*) The period from the application of the brake to the inception of after-nystagmus. The most startling experience resulting from rotation occurs during this period. At first the period is the most upsetting and ordinarily it is responsible for the production of nausea. There is a shift in the pressure at the back of the head, a decided change in the quality of the pressure about the buttocks, and the extraneous visual and tactual processes drop out. A painfully intense kinæsthesis appears localized about the eyes and then suddenly gives way to a series of rapid fluctuations similar in intensity (although more rapid) to those described above.

"The rhythmic appearance of kinæsthesis in the eyes does not appear until stopping has been completed and comes as a climax to that long intense process which starts just as stopping starts and which carries the meaning of the deflection of the eyes in the direction of rotation. The feeling of rotation in the opposite direction and the jumping of the visual field does not begin until this rhythmic kinæsthesis begins." (P.)

"A very distinct change in eye-kinæsthesis was felt as stopping began. The eyes were directed toward the side opposite to rotation. They gradually and involuntarily moved over to the extreme opposite and then began the jerking movements. The shift in the kinæsthetic processes about the face and neck was very clear this time." (K.)

"Just as rotation stopped there was intense kinæsthesis from the eyes, as if they were being pulled in the direction of rotation and, just as stopping was complete, the kinæsthesis reached its maximal clearness and intensity, becoming almost painful, and then (as the after-nystagmus began) it gave way to a rhythmic variation in intensity." (L.)

The intense kinæsthesis carries with it the meaning of movement of the eyes in the direction of the preceding rotation. Even when ocular kinæsthesis has about disappeared, this one process remains. The perception of rotation in the reverse direction never begins until it gives way to the rhythmic nystagmus movements. The most outstanding fact is the total reorganization that occurs in the musculature of the body. There is a sudden shift in these processes, the more intense processes giving way to a feeling of relief or of release and other muscles are localized as the source of intense pressure.

"The stopping period is full of kinæsthetic pulls and tensions and pressures localized mainly in the eyes and in the neck." (P.)

"I caught very clearly this time the shift in the kinæsthetic set during the stopping period. The kinæsthesis during actual rotation was not intense, but was clear, and then, just as rotation stopped, there was a kind of mental pause followed by a shift in the locus of pressure. It seemed to go to the other side of the body, especially in the arms and the shoulders." (K.)

"The change which takes place during stopping is more than a resetting. It is like a kinæsthetic and organic shifting as if some change took place in the position of the organs or substance internal to the skin and muscles. Sometimes this change is clear and sometimes it can be reported only as something different—a kind of organic and kinæsthetic background which seems to change tint-value, as it were. Sometimes 'welling' and pressure are distinguishable; and then again analysis is baffled." (P.)

This shift or change in locus is as temporary as are the other effects of rotation, and soon (under repetition) the kinæsthesis in the eyes is the only clear process left. The pressure in the head is very apt to increase temporarily. The eyes are opened as soon as rotation ceases and the whole effect of rotation counter to the real rotation is carried to the field of vision. The description of the field of vision we have found to make up a large part of previous descriptions. If the eyes remain closed, the kinæsthetic processes about the eyes and the fast disappearing kinæsthetic and organic processes in the body at large carry the entire meaning of rotation. After much practice, the complexity of the stopping period becomes less, but it is the last complex to give way.

(*f*) The period from the appearance to the disappearance of the after-nystagmus. The rhythmic kinæsthesis from the eye, which begins as soon as rotation comes to a full stop, continues in the initial turnings for some 20–25 secs. In fact, it is the only thing that is peculiar to this period. The pulls about the eyes are almost painful at first and absolutely uncontrollable. Even after some practice, when a subject has learned to control himself during actual rotation, the confusion persists during this throbbing movement of the eyes when the individual comes to rest. In some cases it carries, for a time, the perception of rotation in the opposite direction; but ordinarily there is only a fluency or a jumpiness of the visual field in a plane corresponding to the plane of rotation. If the eyes are kept closed, the perception of rotation is always present. The kinæsthesis from the arms and shoulders, which may be very intense at first, persists for a few seconds; but that from the neck and face usually continues on with about the same clearness. There are no organic processes peculiar to this period. Whatever is present is but the persistence of sensations set up during the actual rotation. The kinæsthesis about the eyes grows less and less and it finally disappears. The jumpiness of the visual field disappears at the same time.

"The visual field jumps no longer. It is a little blurry and confused for a moment and then clears up suddenly about the same time as the general release in tension is felt about the face and eyes." (P.)

(*g*) The period from the end of the after-nystagmus to the beginning of the next turn. We have found that the pressure from the head and from the upper part of the nose continues almost unabated during the course of the after-nystagmus. Following the last eye-movement there is a period of a few seconds in which these processes continue and then suddenly there is a complete relaxation of the tenseness about the face, the jaws, the eyes, and in the upper part of the neck. Coincidentally there comes a peculiar and unique change in the feelings about the head. For example:

"I paid particular attention to the after-period this time.

There is a great deal of kinæsthesis—much more than I had supposed—about the face, and the release of the tension was very marked. The pressure in the head which began about the second turn and continued through the nystagmus-period, took a decided change. There was a clear and decided decrease in pressure as though something had given away lower down and the level of pressure had suddenly fallen as the level of water falls when an opening is made at the bottom of a dish. The disappearance of pressure in the nose left the usual 'raw' feeling." (P.)

"The pressure in the head begins about the second rotation. It is quite intense during the stopping period and then, a few seconds after nystagmus, there is a general release of tension all about the face and a lowering of the level of pressure in the nose which leaves a salty 'raw' feeling. The whole experience is of something falling down deep within the brain and neck, which leaves a kind of fresh coolness as opposed to the former stuffiness." (P.)

The change that takes place here, however, is not different from the changes we have found from the first, so far as regards the effect of practice. That is to say, at the end of a series this general lowering of pressure and release of tension drops out. We have even found that the initial pressure itself does not arise.

A similar modification of pressure and tension may be induced at any time, if the position of the head is changed, during the rotation or at any prior time, to exactly the 'release' which we have described. It is also possible to reestablish in this way the 'vertigo' or 'sensation of swimming.'

"As I bent the head forward, there was a distinct change in pressure, the pressure disappearing as well as the kinæsthesis about the face and neck, save in the eyes where there was that characteristic fleeting, hazy, filmy kind of pull in all directions as if the eyes didn't know where to go. This all gave a feeling of light-headedness which is almost like vertigo. As the pressure leaves the head and neck region, the head seems wobbly and 'lightly propped.' It is more of an effort to keep the head in one position than to move it." (P.)

"Felt the same lightness in the head again after changing the position of the head following rotation. The 'swimming' seems to result from an absence of the pressure, which is usual up to this time. There is also an absence of kinæsthesis in the neck which leaves the head with a feeling that it can wobble all over. There is also a peculiar kinæsthesis about the eyes, as if they were going to move in all directions at once. The visual field looks blurry for a moment, like the gravel behind a fast moving train." (P.)

"The feeling of lightness and of dizziness, after rotation is over, is marked mainly by the absence of pressure in the head and nose but with the presence of a strange kind about the eyes, as if the eyes were trying to move in all directions at once and the result is a kind of blankness or grayness of the visual field. The visual field looks as it does during the onset of nausea or after one has suddenly arisen." (K.)

"As I bent head forward this time I felt a kind of creepy coolness arising internally, deep in the chest, moving rapidly through the neck to the brain and then a darkening of the visual field and a sense of confusion and light-headedness." (N.)

All of these things seem to mark the final cessation of vestibular excitation, whatever it may have been. Whenever the "release" described takes place, the subject is ready to change his position or to be rotated again without expecting any of the previous disturbances to carry over. Furthermore, the subject usually takes at this time a deep breath—the first since rotation began.

(*h*) The period following the completion of the day's series. After the first few day's practice, subjects reported that it was several hours before they completely recovered from the effects of rotation. This is natural in the case of those subjects who became nauseated, for such a severe organic disturbance could not but affect the organism for some time. Interest lies, however, in the case of those individuals who were not nauseated and who nevertheless reported that they felt as if they had been engaged in hard manual labor. Some felt as if they had just lifted a heavy weight;

others as if they had run up a long flight of stairs, and still others as if they had recovered from sickness and were still weak. The reference was made by almost all subjects that the feeling was one of fatigue in the whole body and especially in the legs. Occasionally there was a diffuse light-headedness. This effect tended, along with the others, to become less prominent as the series proceeded.

"The final processes today were about as usual. There is always a kind of draggy pull at the base of the brain which seems gradually to accumulate during the series. There is a tingling of the feet and elsewhere, as if the body had been for some time under tension and strain. These processes generally last about a half hour. The head seems heavy, as if ready to fall in any direction at any time and there is a tired feeling in the neck. These processes are not nearly so intense or clear, however, as they were early in the series." (P.)

The foregoing recital confirms our statement that the experience of rotation is exceedingly complex. Evidence has been adduced to show that there is not a new quality which can be called the 'sensation' of rotation. It is proper only to speak of the perception of rotation, the perception resting upon a host of kinæsthetic pressures, pulls and tensions from all over the body and of pressures and pulls from the viscera, pressures and lack of pressure from the head, and so on. When all of these events reach a certain degree of complexity they seem to fuse together into what has been called 'vertigo' or 'dizziness.' Our analysis goes to show that neither of these terms refers to a single item; but rather to the state of mind regarded as a whole. As one becomes adjusted to rotation, this total state disappears and the whole course of events suffers disintegration. It is then possible to reinstate the 'swimming' components and so to identify the individual items in the whole group. Finally, we have found that the mental results of rotation behave in the same manner under repetition as do the organic results. That is to say, the processes which we have designated as principal processes tend to decrease in complexity and in intensity and clearness

Table I

Distribution in Groups of 50 Rotation-periods of the Various Mental Processes Aroused by Rotation. Subject *P*.

Mental Qualities		Tendinous Strain						Muscular Pressure						Fatigue					
Groups of 50 Rotation Periods		1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th
Kinæsthesis referred to	Eyes	45	41	23	15	14	8	47	40	32	16	11	5	11	7	3	0	0	0
	Face	40	31	18	12	7	7	49	31	23	17	8	2						
	Neck	46	45	21	18	14	11	45	36	27	13	7	1	35	16	11	10	1	1
	Arms	47	31	19	18	12	3	49	30	22	17	6	4						
	Back	32	24	14	7	4	1	41	33	19	4	0	0						
	Legs	30	19	12	6	5	3	27	16	4	0	0	0	13	6	3	0	0	0
	Shoulders	27	23	7	2	1	0	20	17	6	1	0	0						
	Chest	31	15	3	0	0	0	45	32	19	11	6	6						

Groups of 50 Rotation Periods		1st	2d	3d	4th	5th	6th
Imaginal processes referred to	Vision	5	1	0	1	0	0
	Touch	3	8	3	0	0	0
	Kinæsthesis	5	3	1	0	0	0
	Smell	10	4	0	0	0	0
Extraneous processes referred to	Taste	6	2	0	0	0	0
	Hearing	2	3	8	10	17	23
	Vision	13	10	11	15	14	16
	Touch	8	10	17	23	27	33

Mental Qualities		Cold and Warmth						Pressure						Pain + Pressure						Welling Pressure					
Groups of 50 Rotation Periods		1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th
Organic and Vascular Processes Referred to				(Cold)																					
	Anus								8	2	0	0	0												
	Bowels	16	3	0	0	0	0	41	16	3	0	0	0												
				(Warmth)																					
	Stomach	18	2	0	0	0	0	47	12	0	0	0	0												
				(Warmth)																					
	Diaphragm	6	2	0	0	0	0																		
	Chest							27	18	14	7	6	0							25	17	11	10	4	0
	Esophagus																			31	16	5	3	3	1
	Neck																			42	12	14	11	3	2
				(Cold)																					
	Head	4	2	0	2	2	1							19	14	11	7	6	6	49	37	31	23	13	10
	Nose													16	12	7	6	6	4	45	29	21	18	16	12
	Skin	42	22	10	0	0	0																		

and finally to disappear. The accessory processes, on the other hand, tend to increase in clearness and so to carry the perception of rotatory movement.

That there is a decided decrease in the principal effects may be further deduced from Table I. It will be seen that, whereas the processes directly dependent upon rotation decrease in number as a series progresses, the extraneous processes actually increase in number, a result in accordance with the introspective reports that the perception of rotation comes after long practice in turning to be carried almost entirely by these processes. Furthermore, upon examining Fig. 1,

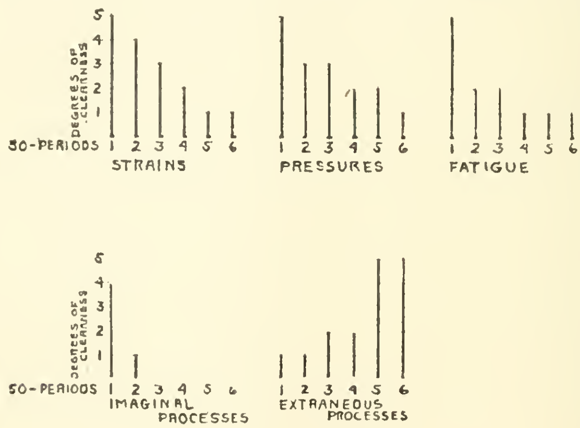

Fig. 1. Subject *P*

it will be observed that the clearness of the essential processes likewise decreases whereas the clearness of the extraneous processes increases. In a similar schematic way, it could be demonstrated from introspective reports that the intensity and duration of both of these groups decrease and increase respectively. We have already referred to the fact that, toward the end of the series, the kinæsthetic processes appeared only after the fourth and fifth rotations and we have also at hand ample evidence of the reduction in duration.

C. The Direction of Attention.—The effect of changing the direction of attention was demonstrated in the following manner. Two subjects, *I* and *J*, were instructed to regard as attentively as possible a definite fixation point on alternate trials. During the remaining trials they were given a problem in 'mental' multiplication to solve, while looking passively, only, at the point of fixation.[1] In these experiments it was thought that this alternation of the conditions might confirm or condemn the otologist's assertions that whatever decrease takes place is the result of "voluntary gaze-fixing."[2] It can be seen from Table II. that the effect of holding the fixation mark tends to leave at a higher level the duration of the nystagmus, the number of movements, and the time of apparent movement. As regards the *amount* of decrease, it appears that, for subject *I*, the decrease during the series (compare first and last values) is greater in the problem series than in the steady fixation series. The problem series gives less decrease, however, in *J*'s case for the distraction of the problem.

Table II

Fixation	*I*			*J*		
	T	No.	A. M.	*T*	No.	A. M.
Av. first 50 trials	14.8	23.4	11.1	15.9	35.4	10.9
Av. last 50 trials	12.2	18.5	10.7	12.7	31.2	8.5
Decrease	2.6	4.9	0.4	3.2	4.2	2.4
Problem	*T*	No.	A. M.	*T*	No.	A. M.
Av. first 50 trials	13.2	18.3	10.3	12.3	26.0	11.2
Av. last 50 trials	9.0	10.4	8.9	9.9	20.5	8.8
Decrease	4.2	7.9	1.4	2.4	5.5	2.4

It is not clear, however, just what is involved in attending to a fixation point. From introspection it was discovered that the kind of attention present in this case was accompanied by a great deal of kinæsthesis from the face, especially from the forehead, neck and eyes. The subjects 'stared'

[1] The problems were chosen experimentally and were designed to keep the subject busy as long as the after-nystagmus continued. Care was taken, however, not to make the problems so difficult as to confuse the subject and thus to defeat the purpose of the trial.

[2] See Manual Medical Research Laboratory, Wash., D. C., 1918. Also Fisher and Babcock, *J. Amer. Med. Ass.*, 1919, 72, 780; Griffith, C. R., 'Concerning the Effect upon Nystagmus of Repeated Rotation,' *The Laryngoscope*, 1920, 30, 22–25.

with all their might in an effort to overcome the confusion resulting from the nystagmus. It is not possible, therefore, to attribute the decrease in the intensity of the ocular effects of rotation during practice to the art of 'gaze-fixing.'

D. The effect of the psychophysical determination. The facts we have just presented are further illuminated by the results issuing from a changed mental determination. The introspective records have already suggested the influence of this mental pre-condition.[1] We have seen that the determination induced by certain imaginal processes profoundly influenced the course of the subsequent processes. We have also seen that, under the influence of the instruction given above, the effects were increased in intensity. Further instructions were now given to the effect that the subject should on some occasions induce the 'staring' attitude and on other occasions the resisting attitude of the whole body. In this way it was discovered that the setting of the whole organism against the confusion of rotation served to check the confusion and to shorten the duration of the processes, a result quite contrary to that obtained by staring at a fixation mark. For example:

"I tried to inhibit nystagmus this time. Attitude of resistance was carried in terms of tension about the legs, buttocks, arms, and back. The whole body was set in resistance. Found that I was quite successful,—success being carried in terms of greatly reduced confusion." (P.)

During a group of thirty trials Subj. *P*'s averages for trials involving no setting were 20.1, 8.5, 20.6, and for trials involving the resistance of the whole body 10.4, 5.4, 10.4. It seems, then, that such a psychophysical preadjustment, while not accomplishing much early in a series, may later reduce very materially the time of nystagmus, the number of movements, and the time of the apparent movements.[2]

E. Visual Factors.—Attention has frequently been called to the effect of changes in visual factors on the appearance of

[1] See p. 99.

[2] This sort of decrease is not to be confused, however, with the decrease that issues directly from practice. A subject may reduce the time of nystagmus considerably by setting himself against it, but the decreases last only as long as this attitude is maintained. For example, such artificial decreases do not reappear the next day.

nystagmus. For instance, Bárány says that nystagmus may be completely inhibited by looking fixedly in the direction of the slow component of the movement. We are here interested, however, not in the matter of voluntary inhibition, but in the ways in which the appearance of the mental effects of rotation may be modified by changes in the visual conditions.

1. Character of the visual field during rotation. We shall first discuss the effect of the character of the visual field during rotation. Subj. *M* was rotated in the regular manner, but on alternate trials he was blindfolded during the period of rotation while during the remainder of the trials his eyes were left open. It was found (see Table III.) that the nystagmus was greatly decreased in time when the eyes were kept open during rotation and that the amplitude of the movements was smaller. Subj. *M* also reported a greatly decreased amount of dizziness when the eyes were open and during the early part of the series the nausea and other organic disturbances were less intense.

Table III

Trials	Closed			Open		
	T.	No.	A. M.	T.	No.	A. M.
First 50	17.6	31.0	15.9	14.5	21.3	13.2
Second 50	14.2	23.0	13.9	11.4	12.6	10.6
Third 50	10.8	17.4	10.8	6.7	7.5	6.9
Fourth 50	9.1	12.3	9.2	4.6	4.4	4.6

2. In the second place, we shall discuss the effect of the character of the visual field after rotation, or, in other words, the effect of the mode of fixation. Three variations in the mode of fixation were required of our subjects. These were (*a*) the effect of near-fixation versus far-fixation, (*b*) the effect of no-fixation, and (*c*) the effect of lateral fixation. In the first case, the subject was allowed in part of the trials to fixate a point about one hundred feet distant, *i.e.*, well beyond the proximal point for parallel axes, and in the other part of the trials the subject fixated a point six inches distant. Under these conditions, it was found that the time of nystagmus, the number of eye-movements, and the time of apparent

movement were approximately twice as great during far-fixation than during near-fixation. See Table IV. This difference appears not only in the groups of five-day periods but also for the average of the whole series. The most pronounced difference is found in the number of the eye-movements.

Table IV

Trials	Far Fixation			Near Fixation		
	Time	No. of Mvt.	App. Mvt.	Time	No. of Mvt.	App. Mvt.
Av. first 50........	15.8	30.5	13.8	7.9	12.1	7.5
Av. second 50......	10.1	18.9	9.7	5.7	7.7	5.5
Av. third 50.......	5.6	9.0	5.6	2.9	3.5	2.9
Av. fourth 50......	3.2	4.7	3.2	1.2	1.2	1.2

b. As regards non-fixation, three variations in method were used. In the first case, Subjs. *R* and *S* were rotated as usual, but their after-nystagmus was observed while they were wearing lenses of 20 diopters. In the second group, Subjs. *Q* and *T* used lenses, but they were also stopped facing a homogeneous surface of grey paper. In the third case, Subjs. *D* and *G* were rotated as usual, but the nystagmus time was obtained by observing from the side as a piece of cardboard 3 x 6 in. was held before the eyes of the subject. In all these cases, the effort was made to eliminate, so far as possible, all fixation. The results are so nearly alike that they will be treated together in Table V.

An examination of the table shows that the initial values are much larger than in the case of 'normal' vision. In fact, the average initial time of nystagmus (not including Subjs. *G* and *V*, who took this series after a long series under other conditions) for these subjects is 41.0 sec. which is over twice as large as the initial values for usual fixation. In other words, this value is as much larger than the value obtained under normal conditions as the value obtained under normal conditions is larger than that obtained under near-fixation (at 6 in.). The initial values obtained from Subjs. *G* and *V* are pertinent. Both of these subjects had had long previous practice. Subj. *G*, *e.g.*, had lost all ocular movement under

Table V

Decrease in Time (Sec.) of Nystagmus and No. of Ocular Movements for Subjects *D*, *Q*, *R*, *S* and *T* while Wearing + 20 Diop. Lenses

Trial	Time of Nystagmus													No. of Movements												
	Subjects													Subjects												
	D	*D* Con.	*D* Con.	*Q*	*Q* Con.	*Q* Con.	*R*	*R* Con.	*R* Con.	*S*	*S* Con.	*T*	*T* Con.	*D*	*D* Con.	*D* Con.	*Q*	*Q* Con.	*Q* Con.	*R*	*R* Con.	*R* Con.	*S*	*S* Con.	*T*	*T* Con.
1	18.0	10.8	1.9	45.9	20.9	18.2	35.0	32.3	22.2	33.0	26.2	50.0	30.1	13.5	7.6	1.9	45.4	27.4	24.5	37.2	28.6	17.0	53.7	43.8	88.5	60.9
2	17.8	10.9	1.2	44.3	21.7	20.1	35.9	26.6	17.4	32.0	25.4	41.1	27.6	16.9	10.6	1.2	47.8	32.0	26.5	38.6	17.1	12.7	52.1	39.3	89.4	57.5
3	17.3	8.1	0.4	44.6	19.9	18.2	33.0	25.4	16.1	31.0	25.2	39.0	25.8	14.7	7.5	0.4	46.7	24.7	25.3	29.1	22.0	12.8	50.2	37.9	73.2	45.0
4	14.0	6.2		43.4	20.8	19.0	39.6	29.1	12.0	36.2	20.7	39.1	21.8	9.7	4.8		49.6	29.3	26.5	32.6	24.1	7.2	58.3	32.9	73.7	51.2
5	14.5	7.5		35.1	19.9	18.2	39.2	27.3	6.0	29.8	17.8	40.5	24.8	12.2	7.2		43.1	26.8	24.8	34.5	24.7	4.5	48.9	25.7	76.5	55.8
Av.	16.3	8.7		42.6	20.6	18.8	35.6	28.1	14.7	32.4	23.0	41.9	26.0	13.4	7.5		46.5	28.0	25.5	34.4	23.3	10.8	52.6	35.9	80.3	54.5
6	9.3	6.1		29.5	25.6	18.9	35.0	25.5		25.4	17.7	31.4	24.8	7.8	5.3		37.2	29.4	26.3	29.9	25.5		45.0	29.7	64.1	52.8
7	11.3	4.3		31.3	19.6	19.7	36.5	24.3		28.1	22.1	31.8	22.9	8.5	3.6		34.7	24.0	29.6	16.0	16.9		50.7	34.1	63.9	47.8
8	8.6	4.1		26.2	21.3	19.0	29.5	16.2		31.3	17.4	35.0	25.5	8.1	4.7		27.5	28.8	24.9	23.4	10.7		48.0	27.8	65.5	52.0
9	10.0	2.6		32.6	21.3	18.3	31.2	17.5		33.2	16.2	38.7	20.4	8.8	2.7		35.9	27.3	25.1	27.8	11.9		49.8	24.1	69.4	43.5
10	9.5	3.9		27.7	20.8	16.1	31.6	20.2		27.2	15.3	27.8	18.7	6.7	3.1		39.5	26.4	22.5	19.4	16.8		43.1	22.7	62.9	38.1
Av.	9.7	4.2		29.4	21.7	18.4	33.0	20.7		29.0	17.7	33.0	22.5	7.9	3.9		35.0	27.2	24.7	23.3	16.3		47.3	27.7	65.2	46.8
Amount of decrease			17.6			29.8			29.0		17.7		31.3			13.1			22.9			32.7		31.0		50.4
% of decrease			98			65			83		53		62			97			50			88		58		57

the conditions then obtaining.[1] Under the new conditions, where fixation is eliminated, his nystagmus time was 18.8 sec. The same is true of Subj. *D*. These facts suggest that essentially different conditions obtain during the wearing of lenses, and also that the effect of lenses is to increase very markedly the time of nystagmus and the number of movements. It is not at all improbable that 'normal' nystagmus movements are gotten only when fixation is eliminated; or, in other words, any kind of fixation is possible only when the ocular muscles are being innervated from other sources than the vestibular tracts. Vestibular excitations are superimposed, therefore, upon excitations already taking place when a subject is fixating any point whatsoever.

Further examination of Table V. makes it evident that the factors which we found to be operative in the decrease under usual conditions also appear here; *viz.*, a large part of the decrease takes place at first, and there are wide individual differences in the rate of decrease and in initial values.[2] These facts, especially the observation that decrease occurs with practice, even though fixation is eliminated, are again of significance in view of the contention of certain otologists that the small decrease they found was due to 'gaze-fixing' and not to practice.[3]

c. The effect of lateral fixation. Attention has frequently been called to the fact that if, during rotation, one looks laterally in the direction of rotation the time of nystagmus is greatly increased while to look in the direction opposite to rotation causes a decrease in the time of nystagmus.[4] That is to say, the time of nystagmus is increased if the subject looks in the direction of the quick component of the ocular movements. This is also true during the after-nystagmus. For example, Subjs. *J*, *W* and *D* were rotated and so stopped

[1] See Griffith, C. R., this Journal, p. 25, Table I.

[2] Griffith, C. R., this Journal, pp. 27.

[3] See Fisher and Babcock, *J. Amer. Med. Ass.*, 1918, 72, 780.

[4] Fridenberg, P., 'The Non-acoustic Functions of the Labyrinth,' *Trans. Amer. Otol. Soc.*, 1908, 11, 181–182; Bárány, R., 'Untersuchungen über den vom Vestibularapparates des Ohres reflektorisch ausgelösten rhythmischen Nystagmus und seine Begleiterscheinungen,' *Monatschr. f. Ohrenhk.*, 1906, 40, 193–297.

Table VI

Decrease in Time of Nystagmus and No. of Movements for Subjects *W* and *D* under Lateral Fixation. Averages of 10 Trials (5*R* and 5*L*) of 10 Turns Each.

Trials	Time of Nystagmus										No. of Movements									
	Subjects										Subjects									
	W	*W* Con.	*W* Con.	*W* Con.	*W* Con.	*W* Con.	*D*	*D* Con.	*D* Con.	*D* Con.	*W*	*W* Con.	*W* Con.	*W* Con.	*W* Con.	*W* Con.	*D*	*D* Con.	*D* Con.	*D* Con.
1	28.0	13.2	9.7	11.3	8.2	5.7	25.9	17.4	11.6	7.7	71.1	29.4	21.0	27.0	16.6	9.3	46.5	33.8	19.6	7.6
2	28.7	14.5	12.4	11.2	7.0	5.9	24.7	13.6	12.0	8.8	74.6	33.7	27.4	23.4	14.6	10.8	44.8	38.5	20.0	9.5
3	27.0	15.2	13.2	9.7	6.7	4.9	24.3	16.9	14.1	6.1	72.0	32.1	26.4	24.9	13.8	10.1	43.8	35.3	23.2	6.8
4	26.8	15.6	12.4	10.7	5.9	4.8	23.8	17.3	12.5	7.1	52.1	33.4	23.7	25.2	11.7	9.1	44.0	39.7	25.5	7.7
5	24.0	13.7	11.4	11.3	5.9	4.1	20.1	12.0	11.7	6.2	50.7	28.9	23.6	26.6	13.3	7.6	41.2	23.2	19.3	6.1
Av.	26.4	14.4	11.8	10.8	6.7	5.0	23.7	15.4	12.4	7.2	64.1	31.5	24.3	27.5	14.0	9.4	44.0	34.1	21.5	7.5
6	21.0	15.8	11.2	10.8	6.3		22.4	13.4	10.3	5.8	43.1	31.8	21.7	20.9	14.7		37.9	26.7	16.3	5.6
7	17.8	15.6	12.3	8.5	6.1		23.7	13.7	9.2	2.9	40.1	33.6	24.2	18.5	12.2		51.2	23.9	12.3	2.3
8	16.1	11.7	12.4	9.2	5.1		20.2	13.0	10.1	1.0	40.6	24.8	27.1	21.5	9.6		44.5	22.8	12.2	0.9
9	15.7	13.5	13.2	9.6	6.3		13.7	12.2	7.7	0.0	37.1	28.6	23.0	21.4	12.3		32.9	21.6	10.2	0.0
10	12.4	13.5	10.8	8.2	5.2		17.3	11.6	8.6	0.0	27.3	29.9	25.8	17.5	11.1		35.0	20.1	10.0	0.0
Av.	16.7	14.0	11.9	9.3	5.8		19.4	12.8	9.2	1.9	37.6	29.7	25.4	19.9	12.0		40.3	23.0	12.2	1.8
Amount of decrease						23.9				25.9						63.5				46.5
% of decrease						85				100						89				100

that it would be necessary for them to fixate the prescribed mark out of the corner of the eye. After being rotated to their left they were instructed to look laterally, during the period of the after-nystagmus, to the right and when rotated to the right they looked, during the after-nystagmus, to the left. In other words, the gaze was directed with the quick component of the ocular movements. Under these conditions, it was found that the time of nystagmus was increased and that the number and speed of movements were enormously increased although the amplitude of the movements was smaller than during direct fixation. Table VI. shows the results from these three subjects. The average initial time is 26.7—a value 8.7 sec. larger than the corresponding value for normal conditions. Still more important is the fact that decrease in all values takes place as a result of practice in the same general manner as was found to be the case above. Bárány has declared that unless the nystagmus is intense it may be entirely concealed by looking straight ahead.[2] That the decrease in nystagmus found above is not, therefore, a result of a temporary concealment due to looking directly ahead but that it is a genuine decrease which takes place even though the conditions of fixation are such as to bring about almost a maximal effect (we have already pointed out that a maximal effect is produced when fixation is entirely eliminated) is indicated by the decrease found during lateral fixation. It was true in these cases that after-nystagmus could be almost if not quite eliminated by looking laterally in the opposite direction; but each of our subjects reported that, at first, the eye seemed to be under strain; later in the series when the intensity of nystagmus had decreased, the strain entirely disappeared.

IV. Conclusion

We come now to a concluding statement of the facts issuing from our experiments and from our introspective analyses. We have found the experience of dizziness or vertigo to be made up of a large number of processes the most

[2] Bárány, R., *op. cit.*, pp. 211, 217, 225.

prominent of which are (1) kinæsthesis from the eyes and neck and in the arms, (2) pressure from the region of the abdominal viscera, the chest and head, and (3) certain vascular processes which supply an obscure background and which give to the whole experience a characteristic shading. We have found, moreover, that the whole experience of dizziness becomes less complex and less intensive under periodic repetition. Nowhere have we been able to discover a process which could be called a 'sensation of rotation' or a 'sensation of movement.' As a result of our analysis, we are warranted in speaking only of the *perception* of movement. Furthermore, we are justified not only in saying that all of the mental processes resulting from rotation may be modified by a variety of physical and physiological factors, but also that certain mental factors modify all of the results of rotation, both organic and mental. That is to say, the organic and mental effects of rotation are an integrated group of events highly sensitive in their appearance and duration to the direction of attention, psychophysical determination, the character of the visual field, and the mode of ocular fixation.

INTERDEPENDENCE OF JUDGMENTS WITHIN THE SERIES FOR THE METHOD OF CONSTANT STIMULI

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The present experiment was devised to ascertain whether there was operative, for the method of constant stimuli, any effect of one judgment upon another within the series; that is to say, whether the passing of a judgment of a certain category has any influence upon the succeeding judgment. In other words, we wish to ascertain if the order of the presentation of the series of stimuli has any effect upon the relative frequencies of the categories of judgment.

In order to determine this matter experimentally, lifted weights were used as stimuli. The stimuli were small brass cylinders—1 inch in height and $2\frac{1}{2}$ inches in diameter—loaded to the proper weight with solder. No stimulus varied as much as 10 milligrams during the experimentation. These weights 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 observer. 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 errors remained constant throughout the experiment. Immediately after the lifting of each comparison weight, a judgment was given verbally in terms of the second weight. The subject employed the three categories of 'lighter,' 'equal' and 'heavier,' which were defined in the usual manner. The observed relative frequencies of the different categories of judgment upon each of the comparison pairs thus obtained were treated in accordance with the methods of calculation developed by Urban.[1]

[1] For a more detailed description of the stimuli, the method of lifting and the treatment of the results, *cf.* S. W. Fernberger, 'On the Relation of the Methods of

The series of stimuli consisted of five pairs of weights. The standard stimuli were always 100 grams and the comparison stimuli 88, 92, 96, 100 and 104 grams. Two such series were mixed together; thus the results of these series are directly comparable inasmuch as they were taken simultaneously. In the first series (known hereinafter as Series A), the 96 and 100 gram comparison pairs followed the 104 gram comparison pairs. In the second series (known hereinafter as Series B), the 96 and 100 gram comparison pairs

TABLE I

ORDER OF THE SERIES OF STIMULI

Table Numbers	I	II	III
1	104*A*	104*A*	88*A*
2	**96*A***	**96*A***	**100*B***
3	92*A*	92*A*	104*A*
4	104*B*	88*A*	**96*A***
5	**100*A***	**96*B***	92*B*
6	88*A*	92*B*	88*B*
7	**96*B***	104*B*	**96*B***
8	92*B*	**100*A***	92*A*
9	88*B*	88*B*	104*B*
10	**100*B***	**100*B***	**100*A***

immediately followed the 88 gram stimuli. Hence there were, in the one series, two stimuli of critical interest following the lightest pairs, and, in the other series, the same two stimuli following the heaviest pairs. The 96 and 100 gram comparison stimuli were chosen as the critical weights because they were most likely to fall within the interval of uncertainty and, also, because they are two of the three stimuli of the series which are likely to be most heavily weighted mathematically.[1]

The actual orders of the presentation of the stimuli employed are given in Table I. The numbers in the first column indicate the successive positions of the pairs of stimuli as

Just Perceptible Differences and Constant Stimuli,' *Psychol. Mono.*, 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.

[1] *Cf.* F. M. Urban, 'Die Praxis der Konstanzmethode.' Leipzig, 1912.

arranged about the turning top table. In the other three columns are the corresponding weights of the comparison stimuli. The three columns contain the different arrangements which were given to the subjects at different times during the experimentation. The arrangements of the stimuli were changed in order to prevent the observers from learning the order of the weights. In Table I., for the sake of clearness, we have printed the numbers of the critical stimuli in heavy-faced type. The letters *A* and *B*, which follow each stimulus number, indicate the Series *A* or *B* within which the stimulus was included. It will be noted that the 92 gram weights invariably follow the 96 gram stimuli in all of the three arrangements. The heaviest and lightest pairs (88 and 104 grams) follow different stimuli in the same and in the different arrangements. These stimuli, for the most part, are judged so frequently and so easily 'lighter' and 'heavier' that it did not seem worth while to have them follow pairs of the same intensity. Such an arrangement would have necessitated the introduction of four more pairs of stimuli into the series; the data from which could not have been used. It will be remembered that each of these comparison stimuli was compared with a standard stimulus of 100 grams.

Four observers took part in this investigation. They were Lucy Day Boring, Ph.D., Edwin G. Boring, Ph.D., Carroll C. Pratt, A.M., and Matsusaburo Yokoyama, A.M.[1] These observers were untrained in the particular technique of lifting which was employed, although all of them have had considerable experience in psychological experimentation. Each subject was practiced for several hours in the manner of lifting until his hand movements became relatively automatic. After that period of practice, all of his judgments were recorded.

The instructions given the observers emphasize the fact that each comparison stimulus is to be judged solely in terms of *its own* standard stimulus which has just preceded it. The actual instructions regarding this point were as follows: "The stimuli will be presented to you in an unbroken series.

[1] This experiment was performed in the Laboratory of Experimental Psychology, Clark University, during the autumn of 1919.

You will have to divide them subjectively into pairs and then judge each second weight in terms of the first weight which has just preceded it." Naturally the repeated performance of the observers during the entire experiment assured the separation of the series of stimuli into pairs in the sense of this initial instruction. With this experimental arrangement, 500 comparison judgments were obtained on each of the ten pairs of stimuli from each of the four observers. At the end of this group of experiments, 50 more judgments, with knowledge of results, were obtained on each of the ten pairs from each of the four observers. Hence this paper has an empirical basis of 22,000 individual comparison judgments.

Table II. *A*

Subject I., Series *A*

Series A	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.......	0.84	0.14	0.02	0.68	0.26	0.06	0.36	0.40	0.24	0.32	0.30	0.38	0.00	0.08	0.92
II......	0.78	0.18	0.04	0.62	0.32	0.06	0.44	0.26	0.30	0.30	0.28	0.42	0.08	0.22	0.70
III.....	0.88	0.12	0.00	0.56	0.30	0.14	0.54	0.24	0.22	0.20	0.38	0.42	0.06	0.24	0.70
IV.....	0.80	0.20	0.00	0.64	0.26	0.10	0.58	0.24	0.18	0.22	0.36	0.42	0.08	0.14	0.78
V......	0.94	0.06	0.00	0.70	0.12	0.18	0.44	0.26	0.30	0.22	0.28	0.50	0.06	0.22	0.72
VI.....	0.92	0.06	0.02	0.58	0.32	0.10	0.48	0.26	0.26	0.22	0.28	0.50	0.06	0.14	0.80
VII....	0.96	0.04	0.00	0.72	0.18	0.10	0.52	0.28	0.20	0.24	0.34	0.42	0.08	0.14	0.78
VIII ...	0.86	0.10	0.04	0.68	0.20	0.12	0.48	0.34	0.18	0.30	0.16	0.54	0.10	0.14	0.76
IX.....	0.96	0.04	0.00	0.72	0.20	0.08	0.44	0.32	0.24	0.20	0.32	0.48	0.04	0.14	0.82
X......	0.82	0.18	0.00	0.60	0.30	0.10	0.46	0.32	0.22	0.26	0.22	0.42	0.04	0.14	0.82
W. K...	0.86	0.14	0.00	0.78	0.14	0.08	0.48	0.38	0.14	0.30	0.22	0.48	0.04	0.16	0.80

Table II. *B*

Subject I., Series *B*

Series B	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.......	0.68	0.26	0.06	0.58	0.26	0.16	0.22	0.34	0.44	0.10	0.20	0.70	0.06	0.06	0.88
II......	0.74	0.22	0.04	0.62	0.30	0.08	0.36	0.26	0.38	0.08	0.28	0.64	0.06	0.22	0.72
III.....	0.84	0.12	0.04	0.58	0.32	0.10	0.36	0.30	0.34	0.06	0.26	0.68	0.06	0.20	0.74
IV.....	0.80	0.14	0.06	0.58	0.28	0.14	0.34	0.32	0.34	0.06	0.30	0.64	0.04	0.14	0.82
V......	0.84	0.12	0.04	0.68	0.16	0.16	0.30	0.30	0.40	0.06	0.20	0.74	0.06	0.12	0.82
VI.....	0.80	0.10	0.10	0.70	0.18	0.12	0.22	0.44	0.34	0.10	0.32	0.58	0.02	0.20	0.78
VII....	0.88	0.10	0.02	0.58	0.28	0.14	0.12	0.36	0.52	0.08	0.22	0.70	0.04	0.12	0.84
VIII ...	0.80	0.18	0.02	0.56	0.34	0.10	0.22	0.28	0.50	0.04	0.28	0.68	0.04	0.16	0.80
IX.....	0.86	0.10	0.04	0.56	0.34	0.10	0.16	0.34	0.50	0.08	0.30	0.62	0.02	0.04	0.94
X......	0.78	0.20	0.02	0.64	0.26	0.10	0.18	0.20	0.62	0.12	0.24	0.64	0.04	0.08	0.88
W. K...	0.80	0.16	0.04	0.52	0.30	0.18	0.42	0.24	0.34	0.08	0.34	0.58	0.08	0.08	0.84

Table III. *A*

Subject II., Series *A*

Series A	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.......	0.78	0.18	0.04	0.52	0.24	0.24	0.32	0.36	0.32	0.26	0.30	0.44	0.04	0.10	0.86
II......	0.76	0.16	0.08	0.50	0.24	0.26	0.36	0.30	0.34	0.18	0.24	0.58	0.04	0.06	0.90
III.....	0.76	0.16	0.08	0.50	0.30	0.20	0.24	0.38	0.38	0.16	0.20	0.64	0.04	0.06	0.90
IV.....	0.72	0.24	0.04	0.32	0.46	0.22	0.18	0.50	0.32	0.06	0.44	0.50	0.04	0.16	0.80
V......	0.80	0.18	0.02	0.48	0.36	0.16	0.24	0.44	0.32	0.18	0.30	0.52	0.02	0.02	0.96
VI.....	0.82	0.14	0.04	0.46	0.32	0.22	0.30	0.40	0.30	0.14	0.34	0.52	0.06	0.04	0.90
VII....	0.72	0.22	0.06	0.44	0.30	0.26	0.30	0.32	0.38	0.22	0.22	0.56	0.00	0.10	0.90
VIII ...	0.70	0.26	0.04	0.42	0.40	0.18	0.22	0.46	0.32	0.12	0.34	0.54	0.04	0.06	0.90
IX.....	0.70	0.28	0.02	0.36	0.40	0.24	0.42	0.30	0.28	0.16	0.38	0.46	0.02	0.12	0.86
X......	0.66	0.28	0.06	0.36	0.40	0.24	0.34	0.36	0.30	0.14	0.36	0.50	0.00	0.08	0.92
W. K...	0.72	0.20	0.08	0.42	0.34	0.24	0.48	0.36	0.16	0.14	0.40	0.46	0.04	0.10	0.86

Table III. *B*

Subject II., Series *B*

Series B	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.......	0.76	0.22	0.02	0.46	0.38	0.16	0.22	0.30	0.48	0.06	0.14	0.80	0.00	0.16	0.84
II......	0.76	0.12	0.12	0.54	0.26	0.20	0.16	0.26	0.58	0.12	0.08	0.80	0.04	0.06	0.90
III.....	0.64	0.28	0.08	0.44	0.38	0.18	0.16	0.26	0.58	0.14	0.18	0.68	0.04	0.10	0.86
IV.....	0.60	0.36	0.04	0.24	0.54	0.22	0.10	0.38	0.52	0.04	0.24	0.72	0.00	0.18	0.82
V......	0.58	0.36	0.06	0.36	0.44	0.20	0.14	0.32	0.54	0.10	0.14	0.76	0.00	0.06	0.94
VI.....	0.62	0.28	0.10	0.26	0.44	0.30	0.08	0.32	0.60	0.04	0.18	0.78	0.00	0.06	0.94
VII....	0.58	0.28	0.14	0.36	0.34	0.30	0.12	0.20	0.68	0.08	0.08	0.84	0.00	0.08	0.92
VIII ...	0.62	0.32	0.06	0.34	0.48	0.18	0.10	0.22	0.68	0.04	0.18	0.78	0.00	0.06	0.94
IX.....	0.60	0.30	0.10	0.28	0.52	0.20	0.08	0.26	0.66	0.04	0.22	0.74	0.00	0.08	0.92
X......	0.62	0.28	0.10	0.38	0.32	0.30	0.10	0.24	0.66	0.02	0.24	0.74	0.00	0.08	0.92
W. K...	0.70	0.22	0.08	0.40	0.42	0.18	0.14	0.34	0.52	0.06	0.24	0.70	0.00	0.10	0.90

Table IV. *A*

Subject III., Series *A*

Series	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.......	0.80	0.12	0.08	0.60	0.18	0.22	0.36	0.26	0.38	0.32	0.26	0.42	0.06	0.16	0.78
II......	0.84	0.08	0.08	0.66	0.08	0.26	0.40	0.22	0.38	0.38	0.20	0.42	0.06	0.14	0.80
III.....	0.88	0.06	0.06	0.68	0.14	0.18	0.50	0.24	0.26	0.30	0.26	0.44	0.08	0.14	0.78
IV.....	0.76	0.12	0.12	0.56	0.26	0.18	0.50	0.22	0.28	0.38	0.22	0.40	0.12	0.12	0.76
V......	0.80	0.12	0.08	0.62	0.20	0.18	0.52	0.18	0.30	0.38	0.24	0.38	0.08	0.12	0.80
VI.....	0.78	0.16	0.06	0.56	0.22	0.22	0.54	0.30	0.16	0.32	0.26	0.42	0.10	0.20	0.70
VII....	0.84	0.10	0.06	0.66	0.18	0.16	0.62	0.22	0.16	0.48	0.24	0.28	0.04	0.18	0.78
VIII ...	0.74	0.10	0.16	0.66	0.14	0.20	0.54	0.16	0.30	0.48	0.08	0.44	0.04	0.10	0.86
IX.....	0.86	0.08	0.06	0.54	0.22	0.24	0.54	0.18	0.28	0.38	0.22	0.40	0.02	0.10	0.88
X......	0.78	0.14	0.08	0.70	0.20	0.10	0.58	0.26	0.16	0.32	0.26	0.42	0.04	0.06	0.90
W. K...	0.94	0.02	0.04	0.72	0.12	0.16	0.54	0.06	0.40	0.36	0.14	0.50	0.00	0.10	0.90

Table IV. *B*

Subject III., Series *B*

Series	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.......	0.66	0.14	0.20	0.56	0.08	0.36	0.30	0.14	0.56	0.10	0.24	0.66	0.10	0.08	0.82
II......	0.72	0.18	0.10	0.44	0.28	0.28	0.26	0.18	0.56	0.08	0.18	0.74	0.04	0.16	0.80
III.....	0.84	0.08	0.08	0.66	0.12	0.22	0.32	0.12	0.56	0.12	0.12	0.76	0.08	0.12	0.80
IV.....	0.70	0.18	0.12	0.54	0.20	0.26	0.22	0.16	0.62	0.08	0.14	0.78	0.10	0.16	0.74
V......	0.78	0.16	0.06	0.62	0.14	0.24	0.24	0.08	0.68	0.14	0.14	0.72	0.08	0.12	0.80
VI.....	0.70	0.18	0.12	0.46	0.34	0.20	0.22	0.14	0.64	0.12	0.16	0.72	0.04	0.26	0.70
VII....	0.80	0.14	0.06	0.68	0.16	0.16	0.26	0.22	0.52	0.16	0.14	0.70	0.08	0.14	0.78
VIII ...	0.84	0.04	0.12	0.74	0.12	0.14	0.20	0.16	0.64	0.06	0.12	0.82	0.06	0.14	0.80
IX.....	0.78	0.16	0.06	0.58	0.24	0.18	0.24	0.22	0.54	0.08	0.18	0.74	0.02	0.06	0.92
X......	0.78	0.18	0.04	0.72	0.20	0.08	0.22	0.16	0.62	0.08	0.18	0.74	0.06	0.04	0.90
W. K...	0.94	0.04	0.02	0.72	0.12	0.16	0.20	0.14	0.66	0.10	0.10	0.80	0.00	0.10	0.90

Table V. *A*

Subject IV, Series *A*

Series	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.......	0.80	0.18	0.02	0.46	0.34	0.20	0.36	0.26	0.38	0.22	0.26	0.52	0.08	0.12	0.80
II......	0.80	0.14	0.06	0.54	0.22	0.24	0.38	0.24	0.38	0.24	0.26	0.50	0.04	0.18	0.78
III.....	0.76	0.14	0.10	0.60	0.18	0.22	0.44	0.30	0.26	0.24	0.28	0.48	0.00	0.18	0.82
IV.....	0.64	0.26	0.10	0.42	0.34	0.24	0.34	0.40	0.26	0.24	0.28	0.48	0.00	0.18	0.82
V......	0.70	0.20	0.10	0.54	0.34	0.12	0.40	0.36	0.24	0.30	0.34	0.36	0.00	0.08	0.92
VI.....	0.66	0.22	0.12	0.54	0.20	0.16	0.46	0.28	0.28	0.32	0.28	0.40	0.00	0.08	0.92
VII....	0.76	0.16	0.08	0.56	0.24	0.20	0.48	0.18	0.34	0.28	0.30	0.42	0.00	0.12	0.88
VIII ...	0.68	0.22	0.10	0.58	0.18	0.24	0.44	0.20	0.36	0.34	0.14	0.52	0.02	0.14	0.84
IX.....	0.80	0.14	0.06	0.54	0.32	0.14	0.46	0.28	0.26	0.30	0.20	0.50	0.00	0.10	0.90
X......	0.86	0.10	0.04	0.60	0.26	0.14	0.50	0.18	0.32	0.28	0.24	0.48	0.00	0.10	0.90
W. K...	0.70	0.24	0.06	0.60	0.30	0.10	0.48	0.16	0.36	0.28	0.22	0.50	0.00	0.10	0.90

Table V. *B*

Subject IV., Series *B*

Series	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.......	0.62	0.26	0.12	0.36	0.36	0.28	0.16	0.24	0.60	0.06	0.26	0.68	0.00	0.14	0.86
II......	0.64	0.26	0.10	0.40	0.32	0.28	0.16	0.18	0.66	0.04	0.22	0.74	0.02	0.08	0.90
III.....	0.68	0.16	0.16	0.38	0.30	0.32	0.08	0.28	0.64	0.06	0.20	0.74	0.02	0.08	0.90
IV.....	0.66	0.26	0.08	0.38	0.36	0.26	0.06	0.32	0.62	0.06	0.16	0.78	0.00	0.14	0.86
V......	0.74	0.22	0.04	0.40	0.44	0.16	0.10	0.24	0.66	0.06	0.20	0.74	0.00	0.10	0.90
VI.....	0.74	0.18	0.08	0.52	0.30	0.18	0.10	0.28	0.62	0.06	0.18	0.76	0.00	0.08	0.92
VII....	0.76	0.10	0.14	0.56	0.34	0.10	0.12	0.24	0.64	0.08	0.18	0.74	0.00	0.12	0.88
VIII ...	0.76	0.16	0.08	0.48	0.30	0.22	0.14	0.20	0.66	0.06	0.12	0.82	0.02	0.12	0.86
IX.....	0.78	0.16	0.06	0.52	0.34	0.14	0.10	0.24	0.66	0.04	0.14	0.82	0.00	0.16	0.84
X......	0.80	0.20	0.00	0.56	0.34	0.10	0.12	0.30	0.58	0.02	0.24	0.74	0.00	0.14	0.86
W. K...	0.66	0.26	0.08	0.62	0.34	0.04	0.14	0.28	0.58	0.04	0.18	0.78	0.00	0.10	0.90

The observed relative frequencies of the different categories of judgment are to be found in Tables II. to V. inclusive. Two tables are given to the results of each subject,—one table for Series *A* and the other for Series *B*. Hence Tables II. *A* and II. *B* contain the results of Series *A* and Series *B* respectively for Subject I.; Tables III. *A* and III. *B* for Subject II., and so forth. These tables are all similar in form. In the first column are numbers designating the fractions of 50 judgments on each comparison pair in the order in which they were taken. These are the fractions into which we divided the results for purposes of calculation. In the next three columns are the observed relative frequencies of judgment obtained on the 88 gram stimuli. In the next three columns are the observed relative frequencies of judgment on the 92 gram stimuli, and so on. In the bottom row of each table, marked W. K., are the observed relative frequencies of judgment of the series with knowledge taken from each observer at the end of the experiments.

The observed relative frequencies in each column, although not identical, are still very similar. When one compares the similar columns for the two series for the same subject, however, certain regular discrepancies are apparent. It will be found that the values for the three categories of judgment for the 88, 92 and 104 gram stimuli show a very close approximation when one compares the values in the two tables for the same subject. Likewise, the observed relative frequencies for the equality judgments of the 96 and 100 gram weights are similar. It will, however, be noted, in every case, that the values of the observed relative frequencies of the 'lighter' judgments for the 96 and 100 gram stimuli are larger in Series *A* than in Series *B*. Similarly, in every case, the values of the observed relative frequencies of the 'heavier' judgments for the 96 and 100 gram stimuli are smaller in Series *A* than in Series *B*. It will be remembered that the 96 and 100 gram stimuli were the critical weights.

From this it is evident that the order of the presentation of the series of stimuli has a marked effect upon the relative frequencies of the categories of judgment. When pairs which contain the 96 and 100 gram weights followed those which contained the 104 gram stimuli (Series *A*) there was a strong tendency for them to be judged 'lighter.' When they followed the 88 gram stimuli (Series *B*), there was an equally strong tendency for them to be judged 'heavier.'

These relations are more obviously apparent when we throw the observed relative frequencies into the form of the curves of the psychometric functions. These curves are represented in Figures 1–4. Each figure represents the results

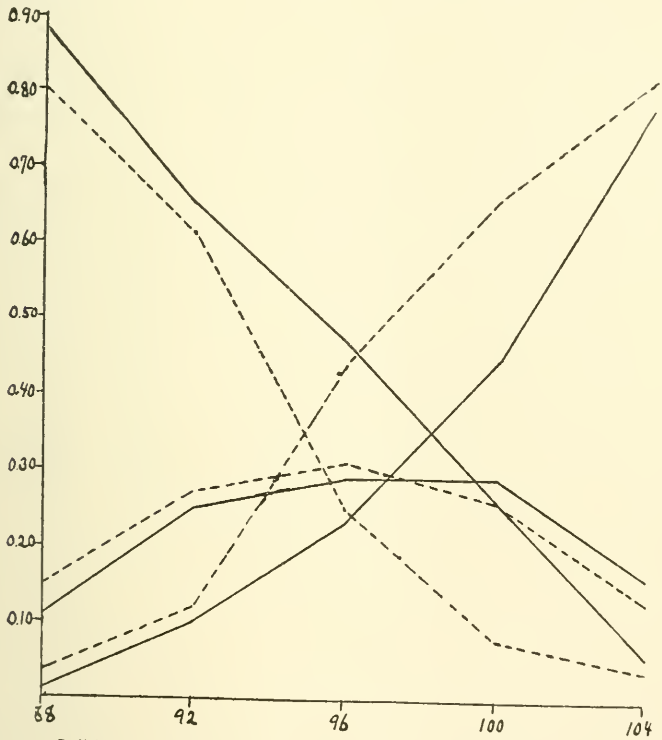

FIG. 1. Solid line represents Series *A*. (96 and 100 gram stimuli following 104.) Broken line represents Series *B*. (96 and 100 gram stimuli following 88.)

of one observer. The curves for Series *A* and Series *B* are superimposed upon one another. The charts are constructed in similar fashion. The intensities of the comparison stimuli are laid off along the abscissa, while the observed relative frequencies are erected as ordinates. These curves of the psychometric functions represent the averages of the values found in the first ten rows of Tables II.–V.

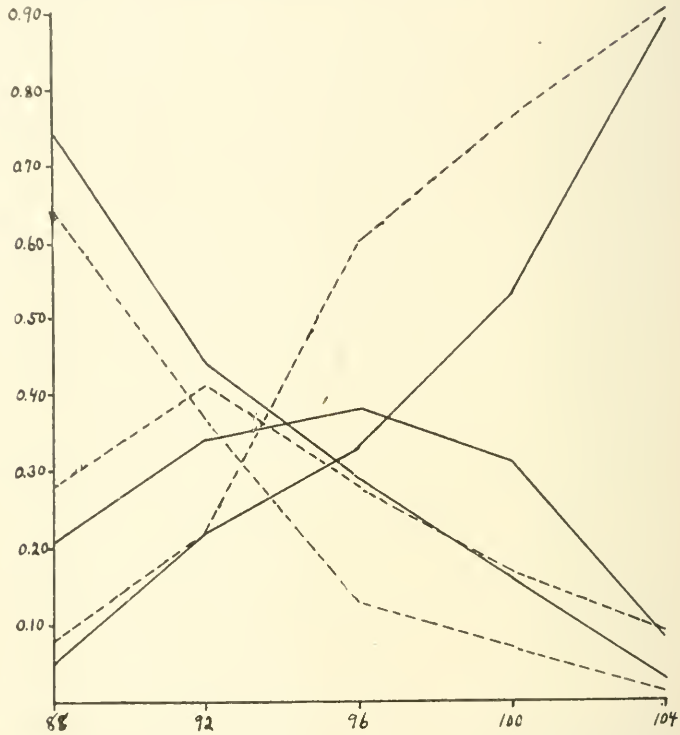

Fig. 2. Solid line represents Series *A*. (96 and 100 gram stimuli following 104.) Broken line represents Series *B*. (96 and 100 gram stimuli following 88.)

In a consideration of these curves several relations become apparent. It will be observed that the curves of the psychometric functions of the equality judgments are relatively similar in form for the two series. This relation is particularly true for Subjects III. and IV. (Figs. 3 and 4) and especially true for Subject I. (Fig. 1). In considering the curves of the psychometric functions of the 'lighter' judgments, it will be noted, for all of the subjects, that the curves of the two series are relatively coincident for the lesser intensities of stimulus. Then the curves draw apart for the values of 96 and 100 grams—the critical stimuli—and finally the curves assume very similar values for the 104 gram weights. The curves of the psychometric functions of the 'heavier' judgments have similar form relations to those for the 'lighter' judgments, except that the spatial relations are reversed. Hence, at those points where the two curves separate for the two critical stimuli, in the case of the 'lighter' judgments the curve for Series *A* (96 and 100 following 104) has values greater than those for Series *B* (96 and 100 following 88); whereas in the case of the 'heavier' judgments, the values for Series *A* are less than those for Series *B*.

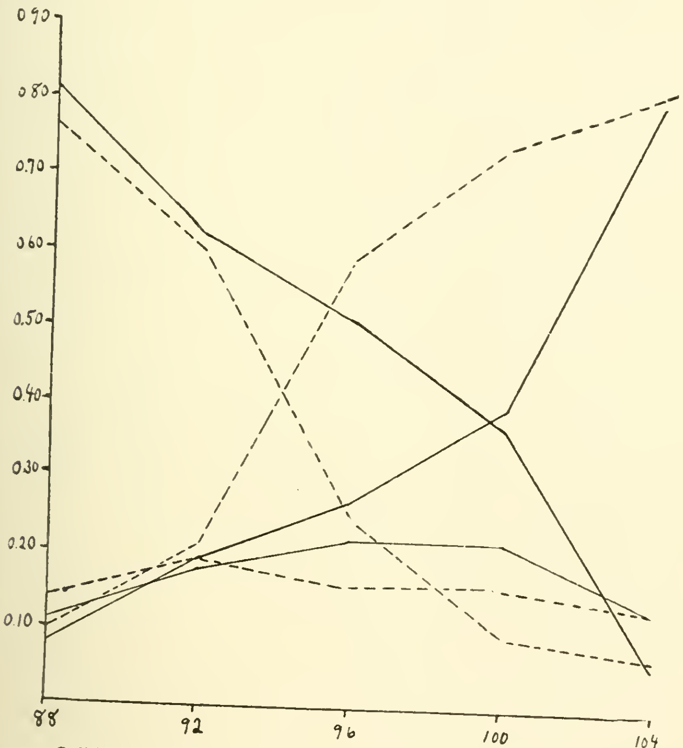

FIG. 3. Solid line represents Series *A*. (96 and 100 gram stimuli following 104.) Broken line represents Series *B*. (96 and 100 gram stimuli following 88.)

It is of interest to study the effects of these differences of the observed relative frequencies for the two series upon the constants of the psychometric functions, upon the values of the thresholds, and upon the values of the points of subjective equality and of the intervals of uncertainty. These values are given in Tables VI.–IX. Two tables are devoted to the values of each subject. The values for Series *A* of Subject I. are to be found in Table VI. *A* and the values for Series *B* for the same subject are to be found in Table VI. *B*. The tables are similar in form. In the first columns are recorded

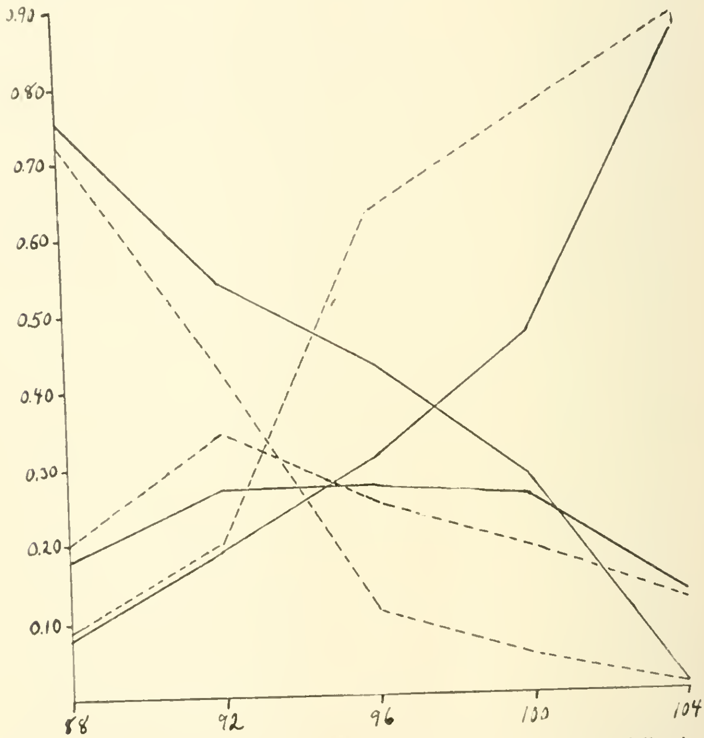

FIG. 4. Solid line represents Series *A*. (96 and 100 gram stimuli following 104.) Broken line represents Series *B*. (96 and 100 gram stimuli following 88.)

the numbers of the fractions of 50 judgments on each comparison pair into which the complete data were divided. In the next two columns are the values of the coefficients of precision of the curves of the psychometric functions of the 'lighter' and 'heavier' judgments respectively (h_1 and h_2). In the next two columns are the values of the lower and upper thresholds respectively (S_1 and S_2); and in the next column the values of the interval of uncertainty ($S_2 - S_1$). In the last columns are the values of the point of subjective equality

Table VI. *A*

Subject I., Series *A*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.	0.0914	0.1450	95.16	99.70	4.54	97.94
II.	0.0876	0.1041	94.58	100.67	6.09	97.88
III.	0.1092	0.0970	94.80	100.86	6.06	97.65
IV.	0.0967	0.1215	95.07	100.47	5.40	98.08
V.	0.1292	0.0891	95.42	99.73	4.31	97.18
VI.	0.1154	0.1247	94.92	99.59	4.67	97.35
VII.	0.1283	0.1223	96.12	100.36	4.24	98.19
VIII.	0.0994	0.1138	95.61	99.87	4.26	97.89
IX.	0.1562	0.1351	95.06	99.71	4.65	97.21
X.	0.0905	0.1277	95.00	100.01	5.01	97.93
Ave.			95.17	100.10	4.92	97.73
P. E.			0.094	0.096	0.147	0.078
W. K.	0.1145	0.1413	95.80	100.19	4.39	98.23

Table VI. *B*

Subject I, Series *B*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.	0.1005	0.1244	91.86	97.13	5.27	94.77
II.	0.1077	0.1105	93.05	98.97	5.92	96.05
III.	0.1215	0.1145	93.45	98.76	5.31	96.03
IV.	0.1231	0.1147	93.05	98.22	5.17	95.54
V.	0.1285	0.1217	93.68	97.54	3.86	95.56
VI.	0.1365	0.0985	93.16	98.73	5.57	95.50
VII.	0.1124	0.1258	95.36	97.27	1.91	96.37
VIII.	0.1297	0.1228	92.43	97.80	5.37	95.04
IX.	0.1467	0.1410	92.64	97.22	4.58	94.90
X.	0.1211	0.1319	92.78	97.09	4.31	95.02
Ave.			93.15	97.87	4.73	95.48
P. E.			0.199	0.161	0.547	0.118
W. K.	0.1040	0.1116	93.27	98.44	5.17	95.94

$\left(\frac{c_1 + c_2}{h_1 + h_2}\right)$. At the bottom of the columns for the thresholds, the intervals of uncertainty and the points of subjective equality are the averages for these values within the column, with the probable errors of these averages. Finally in the laws rows—marked W. K.,—are given the values for the series with knowledge obtained after the other data had been collected.

When one compares the values for the coefficients of precision of the psychometric functions for the 'lighter' judgments (h_1), one finds that the values for Series *A* are almost invariably smaller than the corresponding values for Series *B*. This relationship holds for almost every group with every subject. It means that the curves of the 'lighter' judgments descend more steeply for the heavily weighted

Table VII. *A*

Subject II., Series *A*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I	0.0906	0.1018	93.22	98.79	5.57	96.17
II	0.0985	0.1057	92.82	97.54	4.72	95.26
III	0.1044	0.1244	92.26	97.64	5.38	95.19
IV	0.1083	0.0999	90.43	98.93	8.50	94.51
V	0.1105	0.1398	92.47	98.31	5.84	95.73
VI	0.1056	0.1149	92.67	98.28	5.61	95.59
VII	0.0787	0.1080	91.99	97.54	5.55	95.20
VIII	0.0994	0.1212	91.11	98.25	7.14	95.03
IX	0.0871	0.1016	91.56	99.14	7.58	95.65
X	0.0793	0.1083	90.81	98.19	7.38	95.07
Ave.			91.93	98.26	6.33	95.34
P. E.			0.198	0.122	0.293	0.098
W. K.	0.0905	0.0965	92.46	99.27	6.81	95.97

Table VII. *B*

Subject II., Series *B*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I	0.1315	0.1305	91.68	96.96	5.28	94.31
II	0.1143	0.1189	92.07	95.57	3.50	93.85
III	0.0923	0.1117	90.48	96.58	6.10	93.82
IV	0.1234	0.1094	88.88	96.92	8.04	92.66
V	0.0938	0.1369	89.25	96.01	6.76	93.26
VI	0.1308	0.1224	89.15	95.06	5.91	92.00
VII	0.1017	0.1175	89.28	94.40	5.12	92.03
VIII	0.1282	0.1407	89.63	95.50	5.87	92.70
IX	0.1273	0.1217	89.07	96.39	7.32	92.65
X	0.1388	0.1146	89.87	95.08	5.21	92.22
Ave.			89.94	95.85	5.91	92.95
P. E.			0.240	0.183	0.597	0.384
W. K.	0.1286	0.1207	90.67	96.53	5.86	93.51

TABLE VIII. *A*

SUBJECT III., SERIES *A*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I	0.0912	0.0850	94.25	99.23	4.98	96.65
II	0.0945	0.0837	95.26	98.92	3.66	96.99
III	0.1057	0.0962	95.72	99.80	4.08	97.66
IV	0.0708	0.0794	94.96	100.13	5.17	97.70
V	0.0831	0.0903	95.50	99.83	4.33	97.75
VI	0.0777	0.0836	96.02	100.96	4.94	98.58
VII	0.0840	0.0929	96.97	101.30	4.33	99.25
VIII	0.0695	0.0822	96.11	98.83	2.72	97.59
IX	0.0914	0.0981	94.56	99.04	4.51	96.90
X	0.0921	0.1126	95.64	99.74	4.10	97.89
Ave.			95.50	99.78	4.28	97.70
P. E.			0.169	0.179	0.154	0.166
W. K.	0.1024	0.1195	97.01	98.20	1.19	97.65

TABLE VIII. *B*

SUBJECT III., SERIES *B*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I	0.0858	0.0754	91.94	94.72	2.78	93.24
II	0.1068	0.0951	91.47	96.17	4.70	93.68
III	0.1159	0.1038	93.92	96.47	2.55	95.13
IV	0.0926	0.0870	91.76	96.04	4.28	93.83
V	0.0910	0.1010	92.79	96.12	3.33	94.54
VI	0.1011	0.0825	91.35	96.74	5.39	93.77
VII	0.1085	0.1063	93.66	97.36	3.70	95.49
VIII	0.1346	0.1053	93.56	95.97	2.41	94.62
IX	0.1300	0.1324	92.52	96.28	3.76	94.42
X	0.1212	0.1409	93.17	96.50	3.33	94.96
Ave.			92.61	96.24	3.62	94.37
P. E.			0.203	0.143	0.204	0.154
W K	0.1783	0.1409	93.92	95.95	2.03	94.82

values for Series *B*. Similar relations hold for the coefficients of precision of the psychometric functions of the 'heavier' judgments (h_2). Here the coefficients of precision for Series *A* are usually smaller than the corresponding values for Series *B*. An inspection of the curves of the psychometric functions of the two series (Figs. I–IV.) shows the relation. It will be remembered that the different observed relative frequencies of judgment are differently weighted mathematically; therefore the values of h_1 and h_2 indicate the steepness of the curves of the psychometric functions of the 'lighter' and 'heavier' judgments predominately for the central values.

Table IX. *A*

Subject IV, Series *A*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.	0.0897	0.1046	93.08	98.66	5.58	96.09
II.	0.0984	0.0865	93.63	97.57	3.94	95.50
III.	0.0817	0.0904	94.24	99.07	4.83	96.81
IV.	0.0606	0.0887	91.15	99.00	7.85	95.82
V.	0.0618	0.1006	93.54	99.70	6.16	97.36
VI.	0.0500	0.0948	93.82	99.10	5.28	97.28
VII.	0.0713	0.0984	94.54	98.78	4.24	97.00
VIII.	0.0710	0.0924	93.77	98.16	4.39	96.25
IX.	0.0744	0.1185	94.66	98.78	4.12	97.19
X.	0.0902	0.1225	95.38	98.69	3.31	97.29
Ave.			93.78	98.75	4.97	96.66
P. E.			0.242	0.123	0.2982	0.1459
W. K.	0.0638	0.1214	94.44	98.52	4.08	97.11

Table IX. *B*

Subject IV., Series *B*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.	0.1114	0.0976	89.95	95.84	5.89	92.70
II.	0.1158	0.1120	90.20	95.26	5.06	92.69
III.	0.1210	0.1003	90.20	94.80	4.60	92.28
IV.	0.1298	0.1118	90.08	95.64	5.56	92.65
V.	0.1402	0.1269	90.86	96.08	5.22	93.34
VI.	0.1432	0.1293	91.43	95.78	4.35	93.50
VII.	0.1385	0.1121	91.88	95.88	4.00	93.67
VIII.	0.1325	0.1175	91.51	95.52	4.01	93.39
IX.	0.1622	0.1228	91.61	96.06	4.45	93.53
X.	0.1763	0.1264	91.86	96.86	4.86	93.89
Ave.			90.96	95.77	4.80	93.16
P. E.			0.168	0.116	0.1357	0.1146
W. K.	0.1330	0.1339	91.42	96.30	4.88	93.86

When one compares the values of both the lower and upper thresholds (S_1 and S_2) for Series *A* and *B*, a very striking difference is apparent. In both cases the values for Series *B* are less than those for Series *A*. This relation is true for every fraction and for every subject (*cf.* Tables VI.–IX.). In the case of the lower threshold, the average difference for all four observers is 2.43 grams. The averages for each individual observer are: for Subject I., 2.02 grams; Subject II., 1.99 grams; Subject III., 2.89 grams and for Subject IV., 2.82 grams. The total average difference for all four observers, in the case of the upper threshold, is 2.79 grams. The averages for each individual observer are: for Subject I., 2.23 grams; Subject II., 2.41 grams; Subject III., 3.54 grams and for Subject IV., 2.98 grams. It will be noted that the magnitudes of the differences in the case of the upper and lower thresholds are similar but by no means identical. In the case of every subject, the amount of shift of the upper threshold along the abscissa is greater than that for the lower threshold. Hence the effect of the changed order of the series seems to exert a slightly greater influence upon the upper limen. Absolutely, these differences in the position of the upper and lower threshold values on the abscissa are very large.

The average values of the intervals of uncertainty and of the points of subjective equality for all four subjects are brought together in Table X. In the second and third

TABLE X

Subjects	Average Interval of Uncertainty		Average Points of Subjective Equality		Differences Average Intervals of Uncertainty	Index of Significance of Average Intervals of Uncertainty	Differences Average Points of Subjective Equality	Index of Significance of Average Points of Subjective Equality
	Series *A*	Series *B*	Series *A*	Series *B*				
I......	4.92	4.73	97.73	95.48	0.19	0.1233	2.25	0.9999
II......	6.33	5.91	95.34	92.95	0.42	0.2335	2.39	0.9798
III....	4.28	3.62	97.70	94.37	0.66	0.5419	3.33	0.9999
IV.....	4.97	4.80	96.66	93.16	0.17	0.2736	3.50	1.0000
Ave....					0.36	0.2931	2.87	0.9949

columns are the average values of the intervals of uncertainty for Series *A* and *B* respectively; in the next two columns, the average values of the points of subjective equality for Series *A* and *B* respectively, and, in the next column, the average differences between the two intervals of uncertainty found in the second and third columns of the same table. In the next column are the values of the probability that the difference will not vary by an amount greater than itself, which is an index of the significance of the difference.[1] In the last two columns are found the differences in the average points of subjective equality, and the indices of significance of these differences. In the bottom line are the average differences between the intervals of uncertainty and between the points of subjective equality, together with the average values of the indices of significance in each case.

The averages of the size of the interval of uncertainty are fairly similar. The general average for all four subjects gives a value of 0.36 grams with an average index of significance of only 0.2931. The largest difference for any observer is that for Subject III., with a value of 0.66 grams and an index of significance of 0.5419. These differences are relatively small and the indices of significance are relatively low. One is tempted to remark that they are due to chance errors and may, therefore, be disregarded. A closer examination of the facts, however, leads to a different conclusion.

It will be noted that the average differences between the intervals of uncertainty for Series *A* and *B* are in the same direction and bear the same sign for all four subjects. In every case the interval of uncertainty for Series *B* is smaller than the corresponding value for Series *A*. It is improbable that this uniformity is merely a chance result, since it holds in 70 *per cent.* of the fractions into which the complete data were divided. For Subject I., the results of 7 fractions agree with the sign of the general average, with 3 fractions opposed. The figures for the other observers are: Subject II., 7 fractions agree with 3 opposed; Subject III., 8 fractions agree with 2 opposed; Subject IV., 6 fractions agree with 4 opposed.

[1] These values are calculated in the following manner. The probable error of the difference is first obtained by the formula $PE_D = \sqrt{PE_A^2 + PE_B^2}$. One then determines a value (z) which is the ratio between the difference and the probable error of the difference and which is defined by the formula $z = D/PE_D$. One then determines the value of P_z or the probability that the difference will not vary by an amount greater than itself in a special table of the probability integral. William W. Johnson, 'The Theory of Errors and Method of Least Squares,' New York, 1892, Table II., p. 154.

An explanation of this state of affairs is to be found in a further consideration of the curves of the psychometric functions. It will be remembered that the values of the coefficients of precision of the curves of the psychometric functions of both the 'lighter' and 'heavier' judgments are larger for Series *B* than for Series *A*. Hence these curves for Series *B* would be steeper than those for Series *A*, since the larger the value of the coefficient of precision, the steeper the curve. As a result of this increase in the steepness of the curves of the psychometric functions for the 'lighter' and 'heavier' judgments, one might expect the slight narrowing of the interval of uncertainty in Series *B* which actually does occur. This difference in the size of the intervals of uncertainty, however, although it does not seem to be due to chance results, is relatively slight and perhaps may properly be disregarded from a practical point of view.

When we consider the points of subjective equality for our two series we find an extremely significant difference in their values. In every fraction for every subject the point of subjective equality for Series *B* (96 and 100 grams after 88 grams) assumes a lower value than the corresponding one for Series *A* (96 and 100 after 104 grams). The averages of these differences of the points of subjective equality are given, for the four subjects, in next to the last column of Table X. In the last column of the same table are the indices of significance of this difference. The average difference for all four subjects is 2.87 grams, with an index of significance of 0.9949. The smallest difference for any observer (Subject I.) is 2.25 grams, but it has an index of significance of 0.9999 or mathematical certainty. The lowest index of significance is 0.9798 (Subject II.) which is, nevertheless, absolutely very high. Moreover the differences between the points of subjective equality for every fraction and for every observer have the same sign.

An inspection of Figs. 1–4 emphasizes and explains this difference. It will be remembered that the point of subjective equality (defined by the formula $\left(\frac{h_1 + h_2}{c_1 + c_2}\right)$ is the point of intersection of the curves of the psychometric functions for the 'lighter' and 'heavier' judgments. Due to the distortion of the parts of the curves for the values of the 96 and 100 gram comparison stimuli in the two series, these curves cross at different places with regard to the abscissa. Hence it will be observed that the curves for Series *B* always cross at lower values on the abscissa than those for Series *A*.

These relations are by far the most significant that come out of the present investigation. A shift in the point of subjective equality of more than $2\frac{1}{2}$ grams on the average is of exceedingly great moment. This is especially important if the investigator believes that the point of subjective equality may give a determination of the sensitivity of the subject as well as the interval of uncertainty.[1]

One is able to make a further analysis of the relation of these differences in the points of subjective equality from the present data. In the first place, the subjects were unpracticed in the particular technique of the lifting in the beginning of the experiment, but they reached a high stage of practice during the actual experimentation. It has been shown[2] that the passing of 5,000 judgments in this sort of experiment makes the subject highly practiced. Inasmuch as we have divided the entire data for each subject into 10 fractions of 50 judgments on each comparison pair, we are able to study the effect of progressive practice on data of this sort. The values for the conclusions on this point are to be found in the next to the last columns of Tables VI.–IX. It is found that for every subject there is a tendency for the size of the differences between the points of subjective equality to increase with practice. These relations become obvious when the differences for any subject are thrown into the form of a curve. The increase of the size of the difference, however, is far from regular and is not very marked.

[1] *Cf.* F. M. Urban, 'The Method of Constant Stimuli and Its Generalizations,' *Psychol. Rev.*, XVII., 1910, 237ff.

[2] F. M. Urban, 'Der Einfluss der Uebung bei Gewichtsversuchen,' *Arch. f. d. ges Psychol.*, XXXIX., 1913, 271–311.

S. W. Fernberger, 'On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli,' *Psychol. Mono.*, XIV. (Whole No. 61), 1913, 19–41.

At the end of the experiment each subject was given a series of 50 judgments, with knowledge, on each comparison pair, in an effort to determine whether the effects of this factor could be eliminated by simple instructions. The observer was informed of the divergence between his judgments in Series *A* and Series *B* and was again instructed to judge the pairs independently. In spite of this repetition of the instructions, none of the subjects shows appreciable change in his judgments. This fact becomes obvious when one examines the observed relative frequencies of the different categories of judgment—found in the last rows of Tables II.–V.—or when one examines the values for the coefficients of precision, the thresholds, the intervals of uncertainty or the points of subjective equality—found in the bottom rows of Tables VI.–IX. Hence it may be concluded that this additional knowledge has no influence upon the results. It seems that one cannot expect to eliminate this influence by simple instruction to the subject—at least to the habituated observer. The present results do not show whether proper instructions at the beginning of the experimentation, when the subject is entirely naïve to the technique, might or might not eliminate the effects of this factor.

Just what may be the nature of the processes underlying these results is not brought to light by the present experiment. All that one can say, at present, is that there is some sort of a contrast effect existing between the pairs of stimuli. It is very tempting to say that we have here a type of expectation which tends toward a reversal of judgment from one pair to the succeeding. In other words, when the subject gives a 'lighter' judgment, he is, by so doing, somehow or other 'set' toward a tendency to give a judgment of 'heavier' on the succeeding pair. And when a subject gives a 'heavier' judgment, he is, by so doing, somehow or other 'set' toward a tendency to give a judgment of 'lighter' on the succeeding pair. If this be the state of affairs, this 'set 'is not a clearly conscious process. All of the subjects were surprised when their statistical results were explained to them, and all insisted that such a form of expectation was not consciously present. Thus the best that we can say, with our present knowledge, is that some sort of a contrast effect exists between the pairs of stimuli in the usual arrangement for the method of constant stimuli.

This contrast effect is not to be confounded with the errors of expectation present in the method of just percepitble differences.[1] In the method of just perceptible differences the factor of expectation enters just once in each series and its presence results either in the subject's reversing his judgment too soon or in his failing to reverse his judgment at the proper time. This state of affairs is due to the fact that the subject realizes that he is being presented with either an ascending or a descending series of stimuli. In the present case, on the other hand, the factor of contrast is always in the same direction and may occur as frequently as there are successive pairs of stimuli to be judged.

It seems incredible that this factor of contrast must have been present in all of the experimental work which has been done by the method of constant stimuli without its presence having been noted. None of the published curves of the psychometric functions shows forms that anyway approximate the curves shown in Figs. 1–4 of this paper.[2] The principal reason for this failure to note the factor of contrast seems to be that the curves of the psychometric functions which have actually been constructed have been formed by using the complete data for any given subject. Inasmuch as the order

[1] *Cf.* E. B. Titchener, 'Experimental Psychology,' II., Part II., 1905, 133ff.

[2] *Cf.* F. M. Urban, 'The Application of Statistical Methods to the Problems of Psychophysics.' Philadelphia, 1908.

F. M. Urban, 'Die psychophysischen Massmethoden als Grundlagen empirische Messungen,' *Arch. f. d. ges. Psychol.*, XV., 1909, 261–355; XVI., 1910, 168–227.

F. M. Urban, 'The Method of Constant Stimuli and Its Generalizations,' *Psychol. Rev.*, XVII., 1910, 229–259.

F. M. Urban, 'Ueber die Methode der mehrfachen Fälle,' *Arch. f. d. ges. Psychol.*, XVII., 1910, 367–411.

F. N. Maxfield, 'An Experiment in Linear Space Perception,' *Psychol. Mono.*, XV. (Whole No. 64), 1913.

D. Mitchell, 'The Influence of Distractions on the Formation of Judgments in Lifted Weight Experiments,' *Psychol. Mono.*, XVII. (Whole No. 74), 1914.

A. L. Ide, 'The Influence of Temperature on the Formation of Judgments in Lifted Weight Experiments' (University of Pennsylvania Thesis), Philadelphia, 1919.

of the series of stimuli have been frequently changed, so as to eliminate the possibility of a subject learning any particular order, the relations between the particular stimuli have been frequently reversed and, hence, have tended to cancel one

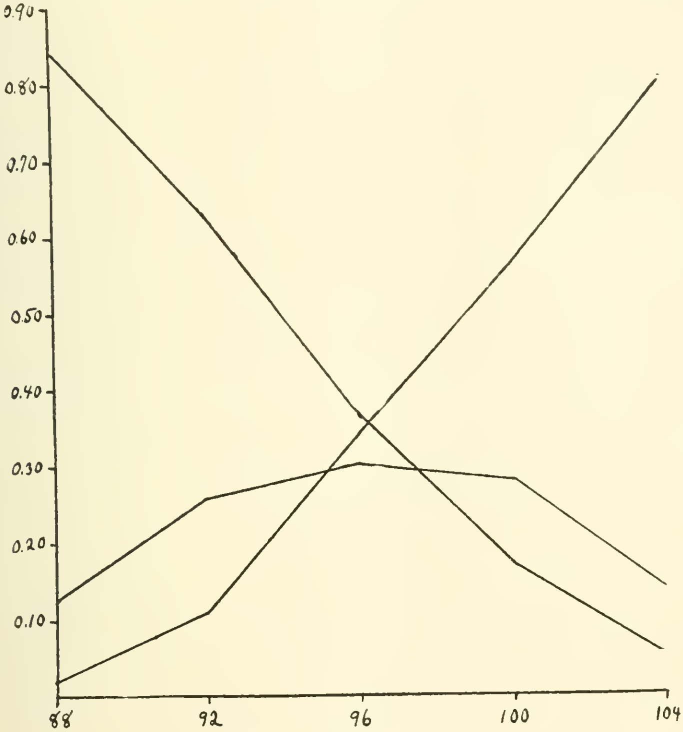

FIG. 5.

another. Ide,[1] in his recent experment, went so far as to change the order of the series after every 25 revolutions of the table. The particular order used was determined by chance. Hence, on the one hand, the grouping of a number of such orders would tend to cancel out the effect of this factor and would give a resultant curve for the complete data which closely resembles the theoretical form of the $\Phi(\gamma)$ hypothesis.

Indeed, combining the data of Series *A* and *B* for any one subject gives a resultant combined series of curves which are close—by inspection at least—to the theoretical form of the

[1] Ide, *op. cit.*, p. 8.

$\Phi(\gamma)$ hypothesis. Such a combined series of curves for Subject I. are represented in Fig. 5. These combined curves appear close to the $\Phi(\gamma)$ form for all three categories of judgment. It will also be noticed that the abscissa value for the maximum frequency of the equality judgments closely approximates the abscissa value for the intersection of the curves for the 'lighter' and 'heavier' judgments—a fact that indicates the symmetry of the functions. If these curves in Fig. 5 are compared with those in the papers quoted above, it should be remembered that we employed only five pairs of stimuli while the other investigators employed seven pairs. We have eliminated the 'tails' of the curves since they approach closely to the values of zero and unity.

It would seem evident that certain irregularities and inversions found in the published curves[1] may be more adequately explained on the basis of the presence of this factor of contrast than by explaining the facts in terms of chance variations in the results. In these cases, we believe that the different arrangements employed, although they tended toward a cancellation of the influence of contrast for some intensities of the comparison stimulus, were not so arranged as to give perfect cancellation for all of the intensities. The ideal method would be to have every pair follow every other pair an equal number of times. Assuming five pairs of weights, this condition would require 120 differently arranged series, since the permutations of 5 things taken 5 at a time is 120. But if the series were not separated, but run successively one after the other, as in the present experimental arrangement, then the relationship carries over from series to series, and the end term of the one series affects the initial pair of the next. Under these conditions, 24 series would give every paired order equally often.

A practical method for the control of this factor may be that which was unconsciously employed by former investigators: *viz*, (1) by the use of great care in the arrangement of the order of the stimuli; (2) by frequent change in the order of the stimuli so that there is a tendency for each

[1] *Cf.* especially Maxfield, *op. cit.*

weight to follow every other weight an equal number of times during the experiment. In this way the contrast factor will be in opposite directions and thus there will be a tendency for its effects to cancel.

A more systematic way of handling the influence of this factor of contrast between the pairs of weights in the method of constant stimuli, would be to present two series of stimuli to the observer, similar but with opposite objective relations and mixed one with the other—such a double series as was employed in the present investigation—and then to 'pool' the results of both series in the calculations. In this case, since the influences are in opposite directions, they tend to cancel each other. Indeed, it is found in the present results, that, if the data of the two series are combined, a single set of curves of the psychometric functions are obtained which seem to be very close to the theoretical form of the $\Phi(\gamma)$ hypothesis. (See Fig. 5.) Such a procedure would not increase either the number of results necessary for the determination of a threshold value, or the time and labor necessary for the accumulation of the data or its subsequent calculation.

Conclusions

1. There is present, in the method of constant stimuli as applied to lifted weights, a contrast effect between the pairs of stimuli. If a comparison stimulus, which is likely to fall within the interval of uncertainty, immediately follows the lightest pair, for which, of course, the judgment is usually 'lighter,' there is a strong tendency that it will be judged 'heavier.' If it follows the heaviest pair—which usually gives the judgment 'heavier'—there is an equally strong tendency that it will be judged 'lighter."

2. The presence of this factor of contrast causes a distortion of the curves for the psychometric functions for the 'lighter' and 'heavier' judgments. It has a slight effect upon the size of the interval of uncertainty and a very marked effect upon the position of the thresholds and of the point of subjective equality.

3. There seems to be a slight tendency for the effects of this factor of contrast to increase with practice.

4. It seems doubtful if simple instructions given the observer will eliminate the effects of this factor of contrast—for the habituated subject at least.

5. A method of eliminating the effects of the factor of contrast by handling the objective relations of the stimuli themselves is suggested.

NOTE ON MR. G. J. RICH'S 'STUDY OF TONAL ATTRIBUTES'

In his 'Study of Tonal Attributes' (*Amer. Journal of Psychology*, 1919, *30*, 121ff.) Mr. Rich tries to plant the dagger of fact in the bosom of my theory of sound. For one who is comfortably ensconced in the shelter of a special experimental task, it is fine sport to try a shot at the birds of theory that cross the open. But it is the bird that makes the sport. And I think it well to show that Mr. Rich has missed his mark.

Mr. Rich first dispatches me in a single brief sentence: "The idea that the volume of a tone decreases by halves as the tone becomes an octave higher implies, mathematically, that volume cannot follow Weber's law: a direct contradiction of the results obtained by Rich" (p. 127).

But both the implication and the contradiction are illusory.

Weber's law says that 'in comparing objects and observing the distinction between them, we perceive, not the difference between the objects, but the ratio of this difference to the magnitude of the objects compared.' In Mr. Rich's experiments, accordingly, the objects are the different rates of vibration that we call tones and the ratio is e.g. the differences 5, 13, 24 to the standards 275, 550, 1150 vbs. per sec. (p. 150). So, Weber might have said, in comparing octaves we note the ratio 2x to x vbs. In fact he said: "In music we apprehend the relations of tones without knowing their vibration rates . . . and in the same way we apprehend the magnitudes of sensation or of force in the comparison of weights" (v. Titchener, 'Exper. Ps. Quant.' Pt. II., xiv, xvi). Weber's theory makes no assumption of any kind about the real nature of the sensations by which the comparisons are made.

Feeling, no doubt, that his first stroke might not be fatal, Mr. Rich strikes a second time with greater force. "In particular the concept of musical interval as being the result of volume-ratio is mathematically inaccurate. If the differential limen for volume is proportional to the stimulus and if, further, the Weber-Fechner Law is accepted, then it must follow that the volume-*difference*, not the volume-ratio, is constant for the same interval at any point in the scale. This implies, of course, that the volumes of tones in octaves cannot be halved in size as one proceeds upwards in the scale" (p. 163).

A theory must agree with the facts, surely. But if it *is* a theory, it will not agree with *any other theory*. So if Mr. Rich accepts Fechner's theory of Weber's law, and if my theory of the octave is not to be classified under that theory, then he will consider my theory wrong (perhaps without really considering it more closely).

But I do not myself think that any convincing arguments have hitherto been brought to the support of Fechner's theory. In order to show this briefly let me distinguish between functional, phenomenal, and real equality of differences between sensations.

Merely functional equality may be exemplified by some of Mr. Rich's experiments (pp. 150f.), for his observers "found considerable difficulty in describing the basis of volume-judgments." In fact they often "did not really know what a difference in volume is." And yet they gave these statistical results that "tend to follow Weber's law." We must not forget that an observer cannot really assert the presence of a just noticeable difference, but merely whether the presented sensations are different or not. It is surely obvious nowadays that a limen can only by a considerable licence be called a *fact*. *A fortiori* the question of the 'equality' of merely functional limens is still more problematical.

Functional and phenomenal equality is somewhat better exemplified in the case of easily noticeable differences, with respect to which an observer not only yields statistically constant results, but is also often conscious and sure of the presence of equality. A musical interval repeated higher or lower in the scale may still legitimately be held to illustrate this procedure (although, *e.g.*, Stumpf considers that the equalities of distance-judgments in tones do not coincide with repeated intervals. But his view is a very tentative and questionable one).

Now it is a familiar fact that the attempt has been made to gauge the real relation between functional limens by reference to the numbers of them that fill out larger differences that are phenomenally equal. The results have not spoken decisively in favor of the real equality of limens. It is difficult to see how they could be expected to do so. For the inference has no real basis at all. It contains no term involving real equality as a standard of reference. We can assume, of course, that phenomenal equality means real equality. But then the whole argument will have only the force of this assumption. And its force seems to me to be in itself nil.

This is reinforced by the consideration that the phenomenal equality in question is an equality of difference, not an equality of substance. It is therefore a functional equality again, not of behavior, of course, or of statistical judgments, but of single judgments: *i.e.*, it is a psychical function. Hence we still have the problem of the nature of the real basis of this function. There are many possibilities, real equality being only one of these. Real proportions of all kinds are also possible.

So, if the liminal and the supraliminal series disagree, some differences (*i.e.*, one or the other series) must be unequal. If they agree, they prove nothing except this consistency.

It may be recalled that Weber demonstrated his law for short lengths of line. Mr. Rich knows that I look upon tonal volumes as 'longitudinally' variable extents. Is Weber's law then mathematically inconsistent with Weber's findings for short lines?

But is there no simpler method of solving this problem of real equality? Common sense would doubtless assume a relation of identity between a line-sensation and the thing measured as its stimulus. And there is a close connection between them. But the very experiments of Weber on these lines compel us to make an important distinction between the sensation-line and the physical line. For the experiments do not succeed unless the sensation-line is carefully maintained as a unity and is not broken up into parts by juxtaposition or overlaying. So while common sense would grant that limen differences are evidently not equal but proportional, Mr. Rich, who rejects this view, would necessarily be left again unable to say whether they are equal or different, the probability being that they are unequal.

I pointed out in my book on Sound (p. 66f.) that my inference regarding the octave might well be held to be a *real* measurement of tonal volumes. I am not sure whether the inference is perfectly convincing, but it seems to be somewhat surer than it is in what I have referred to as the common-sense view of the short line measurement. For these are not estimated relatively to one another by any psychological inference, but by physical measurement, whereas my assessment of the relations of volume between a tone and its octave is based upon purely psychological inferences regarding the facts of volumic change, pitch change, grades of fusion, etc. If the truth of the common-sense view is granted, then limens in tonal volumes are not equal differences any more than they are for common sense in short lines. They are proportional differences.

From my theory I was prepared to meet such experimental results as Mr. Rich's; and his preliminary paper seemed to me to be confirmation of my theory. I was surprised to find that he thought his results contradicted my theory.

There is another point to urge. Even if we were to grant that all equal (and limen) differences (functional and phenomenal) are also really equal, there is nevertheless such a thing as constancy of proportion among sensory forms—phenomenal constancy. Any visual letter or pattern demonstrates this. Why should this constancy of proportion not appear in sounds also? And why not in intervals in particular? What right has Mr. Rich to assert that 'in particular, the concept of musical interval as being the result of volume-ratios is mathematically' or otherwise at all 'inaccurate'? The experiments of his that I know have no bearing on the question.

Incidentally Mr. Rich has misunderstood (p. 125f.) my paper of 1914. I never accepted Révész's Qualität as my meaning of pitch. Pitch was for me then the ordinal attribute I hold it to be. Nor is "the whole of [my] treatment of the attributes in the interest of their explanatory value for fusion" and "not in the interest of tonal analysis" (p. 127). On the contrary my most important notion of the ordinal nature of pitch was and is established largely in independence of the facts and problems of fusion. And however it may be with the real independence and the prospects of attempts to analyse tones with a perfectly detached mind I should feel sorry for any statistical treatment of the results that gets into conflict with the interests of tonal fusion. However much of fact they might contain, they would have to seek a new environment (or is it a new "attitude"?) and be born again.

HENRY J. WATT

REPLY TO DR. WATT'S "NOTE"

The first difference between Dr. Watt and myself obviously lies in the acceptance or non-acceptance of the Weber-Fechner Law. I have shown in two separate studies that the differential limen for volume is proportional to the stimulus. If, for example, the relative difference-limens for Observer B in the earlier study (JOURNAL OF EXPER. PSYCHOL., I, 1914, 17) are averaged, they will be found to be .03605; hence every time that the frequency of a tone is increased by this fraction, the tone will be just noticeably smaller. There will, then, be for this observer approximately 19 just noticeable differences in every octave.[1] These just noticeable differences are to be taken, under the Weber-Fechner hypothesis, as *equal* decrements of sensation. Let us designate them by the letter a; and let us further call the volume of some tone (say, a tone of 275 vs.), m. Then the volume of a tone one octave higher (550 vs.) will be $m - 19a$; and that of a tone two octaves higher will be $m - 38a$. Thus the volumes of three tones standing in successive octaves will be: m; $m - 19a$; $m - 38a$. But Dr. Watt assures us that volume is halved for every octave that we go up the scale ('The Psychology of Sound,' 82); that is, that:

$$m = 2(m - 19a),$$

and

$$m - 19a = 2(m - 38a).$$

These two equations, however, are inconsistent with each other. There is no value of a, other than zero, for which they will both hold. If, then, my results are sound (and only further experimental work can displace them), and *if* the usual interpretation of Weber's Law is accepted, the volume of a tone is not halved as one proceeds up the scale by octaves. Any theory that so contends leads to a mathematical contradiction.

Dr. Watt has, of course, the right to prefer any law of his own to the Weber-Fechner Law. My criticism of his theory was always contingent upon the acceptance of that law, for I was aware that Dr. Watt held that the "assumption ('that the difference indicated by the words 'just perceptible difference' is . . . always equal

[1] Since $(1.03605)^{19.6} = 2$.

. . .') is unjustifiable" ('Psychology,' 23). But I was not aware that he held that limens are proportional differences. The 'Psychology of Sound' gives no hint of such a view, without which there is no justification for saying that my results were 'expected.' It is surely not enough for the theorist to say *ex post facto* that he expected from his theory a given experimental result; it is obligatory upon him to show a passage in his previous statements that, in some sort and in some measure, predicts the result. Dr. Watt has failed to adduce any evidence that he "expected" the differential limens for volume to be proportional to the stimulus. Indeed, it seems strange that, writing in January, 1917, he neglected to fortify his theory by an experimental 'confirmation' published eleven months earlier.

In comparing the limens for volume with those for lengths of line, Dr. Watt has apparently fallen into the stimulus-error. Weber's experiments showed the proportionality of the just noticeable difference to the *physical* line, not to the sensory line. In the same way I have shown that the just noticeable difference for volume is proportional to the physical sound-wave, not to the 'felt' volume. If Dr. Watt regards the attribute of volume as a characteristic of the neuro-physical process of hearing ('The Psychology of Sound,' 96f.), rather than as a characteristic of the sensory experience, I can have no quarrel with him nor he with me; for we are not talking about the same thing. If, on the other hand, Dr. Watt fails to distinguish between volume as a characteristic of stimulus and volume as a characteristic of sensation, he falls so positively into the stimulus-error that psychophysical discussion is useless. I have been and am now speaking only about 'felt' volume, about volume as an attribute of sensory experience, and not about any volumic feature of the neuro-physical process of hearing.

Dr. Watt further asserts 'that a limen can only by a considerable license be called a fact,' because it cannot be directly observed, but is always the result of statistical treatment of observations of mere equality or of difference. A limen is, of course, not a fact of direct observation; it is a value which sums up and represents a large number of facts. Scientific activity cannot limit itself to the primary facts of phenomenological observation. If it did, science would still be in the dark ages, and experiment would be impossible. All the principles of electricity would be non-existent, because no one has yet directly observed the electric current. The various laws of memory could not be admitted to our scientific thinking, because no one has as yet directly observed the strengthening of an associative tendency with repetition or its decay with time. If Fechner may in any sense be said to have raised psychology to the rank of an exact science, it is because he introduced experimental methods of indirect observation and statistical treatment, and thereby gave to psychology the essential and manageable equivalents of whole groups of primary facts. Does Dr. Watt wish us to cast aside these experimental methods, and to return psychology to its former place as a speculative discipline?

In his last paragraph Dr. Watt accuses me of misinterpreting his 1914 paper. On page 13 of that paper he says: "Révész calls the difference of pitch within the octave 'quality' and an accompanying distinguishable difference he calls 'height.' *I accept the distinction intended* without the least hesitation, but it seems to me more correct to class the differences as differences of 'pitch' and of 'volume.' *Pitch, as I mean it, is Révész's quality*" (italics mine). How can Dr. Watt now declare that he 'never accepted Révész's "Qualität" as [his] meaning of pitch?'

Dr. Watt may, no doubt, take some interest in tonal analysis. But it is difficult to believe that that is his primary interest when we are told that: "Obviously the decision regarding these things [auditory sensations and their attributes] cannot be made on the basis of the facts themselves, but only on the basis of the solution which may independently be offered for the musical relationships of the octave" ('The Psychology of Sound,' 51), and that he 'should be sorry for any statistical treatment of the results that gets into conflict with the interests of tonal fusion. However much of fact they might contain, they would have to seek a new environment . . . and be born again.' After making these statements, upon what ground can Dr. Watt seriously deny that his treatment of the tonal attributes is primarily in the interest of fusion, and that the facts of tonal analysis are only of secondary importance in his theory?

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THE INFLUENCE OF THE GROUP UPON ASSOCIATION AND THOUGHT

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INTRODUCTION

If social psychology is to achieve the title of an independent science, it is high time that its many speculative theories and crude generalizations be subjected to experimental methods. The data of this science, it appears to the writer, may be for convenience subsumed under two heads, viz.: (1) the behavior of an individual in direct response to social stimulus, that is in response to some form of behavior in others, and, (2) behavior which is the response to a non-social stimulus, *e.g.*, a column of figures to be added, or a meal to be eaten, when such response is modified by the presence and actions of other persons. Responses to direct and incidental social stimuli are, in brief, the two classes of data for social psychology.

The following experiments bear upon certain problems of the second class of data mentioned. The method employed was to compare the mental processes (in this case association and thought) of the individual when alone with his reactions to similar and equivalent stimuli when a member of a "co-working or co-feeling" group. In this manner the part played by incidental or contributory social stimulation was determined.[1]

[1] A brief historical account of the study of the influence of the group upon the individual may be found in an article by W. H. Burnham: 'The Group as a Stimulus to Mental Activity,' *Science*, N.S., 1910, Vol. 31, 761–767.

General Method.—It was considered advisable to eliminate all incentives to rivalry which were not inherent in the very nature of the situation (*i.e.*, individuals working on similar tasks in one another's presence). The subjects were instructed not to regard their work as competitive; overt comparisons between individuals were also prohibited. The time given for the tests was constant, hence no one subject finished before the others. In this way rivalry, which is a distinct social problem and which should be studied separately, was reduced to its natural minimum. Each subject, however, was instructed to acquire the attitude of doing his best in both the group and the solitary work.

The subjects were arranged in groups, containing from 3 to 5 subjects each. The groups had no changes of personnel during a whole experiment. The subjects were upper classmen and graduate students in psychology at Harvard and Radcliffe Colleges. They were 26 in number, though not more than 15 were used in any single experiment. There were 24 men and 2 women. In age they ranged from 20 to 40 years, 26 being the average age.

In the group work the subjects were seated one on each side of a table 3 feet by 5 feet in dimensions. In groups of 5 two subjects sat at one of the longer sides. The same seats were retained by subjects throughout the course of an experiment. Care was taken to secure conditions, such as type of table, light, air, seating of the subjects, etc., in the rooms used for solitary work comparable to those conditions in the room where the subjects worked as a group.

The free chain associations which were to be written were started by a stimulus word, for example 'building' or 'laboratory,' written at the top of a sheet of paper given to each subject. The same stimulus words were employed in the two conditions, *T* and *A*.[1] It was also emphasized in group work that the same stimulus word was given to all. It is not believed that the presence of the experimenter in the group work materially affected the results of the social influence.

In all experiments except the first constant intervals of

[1] *I.e.*, 'Together' and 'Alone.'

time were given, in the group by spoken signal, and alone by buzzers placed in each room and tuned down to inobtrusive intensity. Control tests were given in the group, using the buzzer for signals in order to determine whether the buzzer itself played a part in the results. No difference was found in the average, between group tests given by the buzzer and those given by verbal signal. The writing materials (pen, pencil, etc.) used by each subject were kept as constant as possible throughout the experiment.

Experiment I

Free Chain Association

1. *Procedure.*—The first experiment, introductory in character, was the only one in which the amount of work was constant, and the time required to finish was taken as the objective result for each subject. Sheets of paper were ruled for writing 100 words. Three tests were given within the hour. Only one group, consisting of but 3 subjects, was used for the experiment. The experimental hour came once per week, and the experiment lasted about 12 weeks. There were weekly alternations of the social conditions, together (T) and alone (A).

The papers were placed before the subjects face down. At the signal they were reversed and the subjects glanced at the word given at the top and proceeded to write their free associations one below the other. The writing of the successive words of sentences or phrases was prohibited as was also the serial association of numbers. While working together the time for each subject was taken by the experimenter; when working alone each subject timed himself with a watch or stopwatch. A rest of three minutes was given between test sheets. Immediately after the completion of each test, both together and alone, the subject was required to mark with distinguishing symbols certain kinds of ideas, as follows: (1) *Personal associations* (ego-centric), that is, words related directly and intimately with the subject's own past as experience not likely to play the same part in another's association trends; and (2) words which were written without discoverable connection with any of the preceding words, that is, 'free-rising ideas.'

Treatment of Data.—In the following pages the term 'social increment' is used to indicate a gain in the average quantity of work done in the group over the average done alone. 'Social decrement' indicates a loss in quantity in the group performance. Corresponding gains and losses in *quality* of the work in the group are termed 'social supervaluents' and 'social subvaluents' respectively. The social increments, decrements, super- and subvaluents, are always expressed as a percentage of the average quantity or quality of the work done alone.) In this experiment the individual social increments or decrements are given on the right in Table I.

The results expressing the social influence upon the kind of associations (personal, objective, etc.) were too meager to admit conclusions. They are therefore omitted for this first experiment.

TABLE I

AVERAGE TIME SCORE FOR 100 WORDS

(No. of trials for each individual: Alone 9, Together 12)

Subject	Alone	Together		
Bar.	4.3	3.9	Gain together.	9.3%
Stu.	5.8	5.0	Gain together.	13.8%
Lan.	3.3	3.4	Gain alone.	3.0%
Average.	4.4	4.1		

3. *Discussion of Results.*—Two out of three subjects have a social increment. Both of these increments were much greater than the social decrement of the other subject. For the group there was an average gain in time over solitary work of three tenths of a minute. The first rough indications therefore point toward an increased number of free associations produced in the group.

EXPERIMENT II

Free Chain Association

1. *Procedure.*—The study of free associations under the social influence was now continued using a larger number of subjects and tests. The groups used all came twice within a

week, the experiment lasting three weeks. There were 15 subjects arranged in 3 groups of 5 subjects each. It was decided to use fairly frequent alternations of the conditions *T* and *A*, thus equalizing effects of practice. One group (*C*) underwent alternations of "together" and "alone" *on the same day:* sometimes two and sometimes four alternations. A combination was used of the sequences *T A T A* (or *A T A T*) and *A A T T*. If the tests of a certain day began with *T* and ended with *A*, those of the following day would begin with *A* and end with *T*. The entire series moreover began with *T* and ended with *A*. Hence the initial lowering due to newness of the task and the final possible increase due to practice would favorably affect only the score for work done *alone*. The other two groups (*A* and *B*) changed their social condition on successive days. Group *A* began with a day in *T* and ended the series with *A*; group *B*, as a check, began the series with *A* and ended with *T*.

The routine of this experiment had one important difference from that of the preceding. The *time* was now the constant factor, and the *number of words written* was the measure of the association process. The interval of each test was three minutes, given with a suitable preparatory interval, by voice in the groups, and by buzzer when the subjects worked alone in separate rooms. A short rest period was allowed between tests.

Another important variation was the division of the work of each test into three *periods* of *one minute each*. After one minute of the time had elapsed the experimenter (in the groups) directed "draw line," whereupon each subject quickly made a line under the word he was then writing or had just written, and then continued with his work. This was repeated at the expiration of the second minute. When the subjects worked alone these signals for the divisions were given by short strokes of the buzzer. The rooms of the subjects when working alone were interchanged from day to day in order to obviate the effect of the peculiarities of any one room upon the work of a subject.

To the two types of associations required to be marked in experiment I. were added two more, viz: (1) words, other than the first, suggested mainly by the stimulus word; and (2) words suggested by the immediate surroundings. Introspection was required immediately after each test together with a rough estimation of the degree of the rivalry consciousness expressed on a scale of 0 to IV. In all other respects not mentioned the procedure was the same as that in experiment I.

2. *Treatment of Data.*—The individual tables are omitted because of lack of space. Table II. presents the individual averages. Practice effects at the beginning which appeared in the individual records have been eliminated from the averages by the following rule. In the first day's work (group *A* and *B*) all tests which are lower than every single score made in tests on later days are ruled out.

TABLE II

INDIVIDUAL AVERAGES OF ASSOCIATIONS

Subject	No. Trials[1]		Alone				Together			
			No. Asso. per Min.			Total No. Assoc.	No. Assoc. per Min.			Total No. Assoc.
	A	*T*	1st. Min.	2d. Min.	3d. Min.		1st. Min.	2d. Min.	3d. Min.	
And.	13	15	18.3	18.3	19.3	56.	20.5	18.7	19.	58.3
App.	11	10	21.5	20.8	20.3	62.7	23.6	20.2	19.6	63.4
Cut.	5	6	23.8	22.4	21.6	67.8	28.1	25.8	24.6	78.7
Hor.	11	10	19.3	18.2	16.6	54.3	20.3	16.9	17.2	54.4
Hos.	6	6	15.1	14.1	13.5	44.2	16.5	15.6	16.1	48.3
Hun.	13	16	18.7	18.9	19.	56.8	19.8	16.8	18.4	55.1
Kno.	14	15	18.7	18.5	20.	57.3	20.	20.4	17.6	58.2
Lan.	13	15	19.3	18.8	18.9	56.8	20.2	20.2	20.7	61.2
Pep.	13	12	24.2	21.9	22.2	68.4	24.2	23.5	23.5	71.2
Pre.	14	16	24.9	22.2	22.7	69.9	26.4	23.7	22.2	72.4
Rob.	12	13	22.	21.6	21.3	65.	24.1	24.3	24.6	73.
Spe.	14	15	22.3	19.2	25.7	67.4	22.2	23.	22.2	67.5
Sto.	14	16	15.2	12.5	12.3	40.1	16.5	15.	15.1	46.5
Tul.	13	12	27.5	25.	24.3	76.9	28.2	26.8	25.7	80.9
Woo.	5	10	21.8	20.6	19.	61.4	22.8	21.4	21.5	65.7
Average	11.4	13.5	20.8	19.5	19.8	60.3	22.2	20.8	20.5	63.6

3. *Discussion of Results.* (*a*) *Quantity of Associations.*—Table III. presents the average number of associations, together and alone, for each subject, together with the per cent. of gain under the social condition to which it belongs. We find our first experiment amply verified. 93 per cent.

[1] Exclusive of the trials eliminated owing to effect of practice.

of the subjects (14 out of 15) produce more associations in the group than they produce alone. The social increments are not large, but their preponderance is conclusive. Cut, Rob, and Sto have considerable social increments (from 12 per cent. to 16 per cent.). The average social increment also of the 14 is twice as great as the social decrement in the case of the one exception to the favorable group influence, Hun. The number of associations produced by all subjects together is also slightly greater than their average alone (63.6 to 60.3).

TABLE III

PERCENTILE GAINS IN AVERAGE NUMBER OF ASSOCIATIONS

Subject	Ave. No. Associations		Per Cent. of Gain	
	Alone	Together	Alone	Together
And	56.	58.3		4.1
App	62.7	63.4		1.1
Cut	67.8	78.7		16.
Hor	54.3	54.4		.2
Hos	44.8	48.3		7.8
Hun	56.8	55.1	3.	
Kno	57.3	58.2		1.5
Lan	56.8	61.2		7.8
Pep	68.4	71.2		4.
Pre	69.9	72.4		3.7
Rob	65.	73.		12.3
Spe	67.4	67.5		.1
Sto	40.1	46.7		16.4
Tul	76.9	80.9		5.2
Woo	61.4	65.7		7.
Average	60.3	63.6	3.	6.2
Mean variation	7.6	8.5	—	4.1

Number of subjects having higher average number of associations together....... 14
Number of subjects having higher average number of associations alone......... 1
Number of subjects having equal average number of associations together and alone 0

The mean variation among the subjects is higher, relatively to its mean, in group work than in solitary (8.5 compared with 7.6). Hence we find increased variability accompanying a social influence toward increased mental activity.

Let us now consider the *distribution of the social increment*. The question proposed is whether the increase due to the presence of the group was equally distributed, or whether it occurred chiefly at the beginning, the middle, or the end of the three-minute period. The record of each subject in the three minutes is shown in Table II. We may compare the results of the three-minute periods both in number of subjects who have social increments in those minutes respectively, and also in the percentile value of the gain in group work shown in the average of all subjects in the three minutes respectively. This latter comparison is taken from the averages at the foot of Table II. We may speak of this gain as the 'group social increment.' Table IV. presents the above relations.

TABLE IV

	1st. Min.	2d. Min.	3d. Min.
No. of subjects having greater number of associations *together*..	13	12	9
No. of subjects having greater number of associations *alone* ...	1	3	6
No. of subjects having equal number of associations *T* and *A* .	1	0	0
Group Social Increment { Amount of excess of Together over Alone....	1.4	1.3	.7
Group Social Increment { Per cent. of excess of Together over Alone....	6.7	6.6	3.5

In both proportion of subjects having a social increment and in the amount of the increment itself, we thus find that the superiority of the group condition in speed of associations exists throughout the test, but is greatest in the first minute and least in the third minute. The second minute is not far below the level of the first, the drop in increase due to the social influence coming well toward the end of the task.

If we compare the three one-minute periods with each other in the two social conditions separately (Table II.) we find the following. The averages of all subjects alone (20.8, 19.5, and 19.8) indicate a drop in the second minute, followed by a slight rise (probably an end spurt) in the last minute. The averages together, on the other hand, (22.2, 20.8, and 20.5) form a steady decrease to the end. This result suggests that the effect of the group is at first a stimulating and later a steadying one.

Another possible interpretation seems to be that during the first minute when the associations come with great facility, the social influence counts for a relatively greater addition of speed than toward the end of the test when, through fatigue and comparative exhaustion of complexes and vocabulary, the facility of writing associations has decreased. Under difficult conditions therefore being alone tends to favor concentration. Group work on the other hand contributes no such benefit to the final and more difficult stages of the task.

(*b*) *Quality of Associations.*—The four general types of association, mentioned under procedure, were counted in each individual test. The average number of each type per test for each subject, in group and solitary work separately, is given in Table V.

TABLE V

AVERAGE NUMBER ASSOCIATIONS OF VARIOUS TYPES

Subject	Alone				Together			
	Personal	Free Rising	Sugg. by Stim. Word	Sugg. by Surroundings	Personal	Free Rising	Sugg. by Stim. Word	Sugg by Surroundings
And.....	8.9	.3	1.9	3.1	2.8	.4	.2	2
App.....	17.8	.4	.1	5.9	24	.4	.4	8.7
Cut.....	20.2	.8	0	0	17.1	2.6	0	0
Hor.....	48.3	.7	.3	1.2	47.7	1.3	1.4	6.8
Hos.....	19.5	.8	1.3	0	13	.6	.5	0
Hun.....	11	1.2	2.7	.3	11.5	1.6	1.5	3.4
Kno.....	1.9	3.4	0	2.4	1.3	2	0	1.7
Lan.....	16.2	.8	.8	2.2	12.8	.5	.7	1.5
Pep.....	19.5	4.5	.4	4.7	15.2	2.8	.3	12.5
Pre.....	7.2	0	1.6	0	7	.1	.9	1.7
Rob.....	45.5	.1	.2	1.5	43.9	.7	.3	4
Spe.....	2.1	0	0	0	2	.1	0	.3
Sto......	18.5	.1	.1	0	27.3	0	.1	0
Tul.....	15.6	1.1	.2	1.6	12.4	1	0	1.2
Woo. ...	11.4	1	.3	1.1	10.1	1.2	1.5	0
Average.	11.4	1.1	.6	1.6	16.5	1.2	.5	2.9
M. V....	8.1	.8	.6	1.3	10.3	.9	.4	2.7

	No. Subjects Having Greater No. Alone	No. Subjects Having Greater No. Together	No. Having Equal No. Alone and Together
Personal Assoc..................	12	3	0
Free rising assoc..............	5	9	1
Assoc. Sugg. by Stim. Word....	7	4	4
Assoc. Sugg. by Surroundings ..	5	7	3

The first type, and the one yielding the clearest result, is that of *personal* associations. 80 per cent. (12 out of 15) of all the subjects wrote down more personal associations *alone* than together. There is evidently some sort of attitude assumed by the individual in the group which takes him 'out of himself' and directs his ideas toward outside objects, and as we shall show later, to the actual presence of the others. In the group we are inclined to expand in our thought; we become objective rather than egocentric, present rather than retrospective.

Secondly, *words suggested by the immediate surroundings* appear to be more numerous in the group than in the solitary condition. If we consider the average of all subjects, the tendency is marked, for the average together is almost twice as great as that alone (2.9 to 1.6). Individuals considered, we find that 3 show no tendency either way while 7 produce more words relating to the surroundings in the group, and 5 produce more alone. Hence we find that an environment of active persons is more likely to intrude upon one's trend of thought than an environment of mere space and furniture such as in the solitary condition.

The third type of associations, the 'free-rising' ideas, also occur more frequently in the group. The averages give only a slight increase for the group work; but the individual records show that 64 per cent. (9 out of 14) of persons affected either way produce more of these spontaneous ideas in the group than they do alone. It is possible that 'free-rising' words result here from that greatly facilitated flow of associations characteristic of work in the group.

Finally, we may note that *words*, other than the first, *suggested mainly by the stimulus word* are more numerous in the solitary than in the group condition, as shown by the average of the individuals and especially by the number of individual cases (ratio of 7 to 4). The explanation, though obscure, may lie in the longer persistence of the original trend of thought in the solitary than in the social setting.

(*c*) *Correlations.*—There are, finally, several correlations to be described. The first is that between the rank of individuals in speed of association and their rank in regard to the favorableness of the group influence on their work. We find, in harmony with the results of other investigators, and with the writer's own study of attention and mental work, that there is an inverse correlation—though here it is a very small one (− .12).[1] Important exceptions act to reduce the index of correlation: for example, Cut is high in both respects; Hor is low.

A further correlation was developed to indicate to what extent the social increments were due to that irreducible minimum of conscious rivalry characteristic of all co-working. It was found that only 3 subjects experienced no rivalry at all. The remainder, 13, all recorded on the average of all tests more rivalry in the group than alone. The ratio of this increase was 1.9 to 1. The correlation between this excess of rivalry consciousness 'together" and the size of the social increment was found to be so slight as to be negligible. It was .23. There was also no correlation (.02) between the subject's report of mere vividness of consciousness of the group (or its absence) and the amount of the stimulation to speed afforded by the group.

(*d*) *Introspection.*—There was substantial evidence from the introspection of awareness of being "*drawn out*" by the presence of the group, so as to produce associations of a more objective type, as previously shown in the results. As to the group influence on speed, two clear cut factors appear in some cases in the same report. The first is an *impeding* influence owing to sensory distraction, emotional factors such as over-stimulation in rivalry, self-prejudicial comparisons with others, and the like. The second and stronger factor is *facilitation.* Numerous stimuli indicative of the rapid work of one's neighbors serve as a drive to greater effort. The principle here involved is probably that commonly known as "suggestion" or "imitation" in superficial accounts of group and crowd phenomena.

Experiment III

Free Chain Association

1. *Procedure.*—The method used in Experiment II. was considered imperfect on the following ground. Associations

[1] All correlations in this paper were obtained by the use of the rank method, using the formula:

$$r = \frac{6\Sigma(d^2)}{n(n^2-1)}.$$

are as a rule produced more rapidly than they can be written, and therefore the writing down of each one in turn does not allow the most rapid play of which the process is capable. The results may measure, not the associational ability itself, but simply the speed of writing. A short experiment was accordingly prepared in which the subject was to write down, not *every* word, but every *fourth* word which occurred to him. With a little practice the subjects rendered automatic the rhythm of writing every *fourth* word only. The division of the three minute period into 3 parts was not made in this experiment. The subjects and grouping were the same as for experiment II. The tests were presented on two days, within the period of a week. The sequences of alternation were as follows, the horizontal line dividing the work of the two days:

Groups *A* and *C*		Group *B*	
T			*A*
T			*A*
	A	*T*	
	A	*T*	
T			*A*
	A	*T*	
	A	*T*	
	A	*T*	
T			*A*
T			*A*
	A	*T*	
T			*A*

2. *Treatment of Data.*—The number of associations written in each test was multiplied by 4 and tabulated. The record of Tul is omitted owing to insufficiency of data. The result of the first test was eliminated whenever it was found to be lower than the score of any succeeding test under the same social condition. Table VI. presents the usual summary of individual records.

3. *Discussion of Results.*—The results shown in Table VI. indicate again a distinct though less pronounced advantage for work done in the group. Two subjects show a social equivalence. Of the rest 66 per cent. produce more associations in the group. The group average for the associations together is greater than the average alone, and the average of

the social *increments* is very much greater than the average of the social decrements.

TABLE VI

COMPARISON OF AVERAGE NUMBER OF ASSOCIATIONS. (ALSO PERCENTILE GAINS)

Subject	No. of Trials		Av. No. Associations		Per Cent. of Gain	
	A.	*T*	Alone	Together	Alone	Together
And	5	6	130	130	—	—
App	6	5	150	158	—	5.3
Cut	6	5	166	163	1.8	—
Hor	6	6	113	117	—	3.5
Hos	6	6	80	85	—	6.2
Hun	5	6	82	82	—	—
Kno	6	6	89	101	—	13.4
Lan	6	6	117	125	—	6.8
Pep	6	6	113	111	1.7	—
Pre	6	6	119	126	—	5.9
Rob	4	6	121	118	2.4	—
Spe	5	6	126	129	—	2.4
Sto	6	6	54	60	—	11.1
Woo	4	5	120	119	.9	—
Average	5.5	5.7	112.8	116	1.7	6.8
M. V.			21	20.1	.4	2.7

Number of subjects having greater number of associations *Together* 8
Number of subjects having greater number of associations *Alone* 4
Number of subjects having equal number *Together* and *Alone* 2

The number of trials was too small to allow the mean variation to carry much significance. Comparing however the mean variation of the social increments with that of the decrements, we find distinctly more variability in the former. In other words the social influence, when it affects workers favorably, affects them also in very varying degrees. Considering, not increments, but actual number of associations written together and alone, the variability both relatively and absolutely is greater alone.

The correlation between the individual's ability to associate rapidly and the size of his social increment is still inverse, and somewhat greater than usual (− .53).

The consciousness of rivalry was in most cases slight in amount, and occurred in slightly over half the individuals. The effect on the work however was perhaps noticeable. Between excess of rivalry together and the size of the social increment there was the small correlation of .41.

Experiment IV

Free Chain Association

1. *Procedure.*—Since the conclusions of experiment III. were based on rather few results the experiment was repeated using fewer subjects but about twice as many tests. This time no stimulus word was given: the subject thought of his own initial word. Another difference was that every *third* word, instead of every *fourth*, was written. The subjects numbered eight and were divided into two groups of 4 each. The experiment covered about five weeks, each group being tested once per week. There was an average of four tests in each hour that the groups were tested. The sequence of social conditions employed a combination of alternations in successive days and alternations within the same day. Group *A* began with *T*, and group *B* with *A*. (In tabulating the number of words set down was the actual number of associations *written*. The individual summaries are presented in Table VII.)

Table VII

Comparison of Average Number of Associations. (Also Percentile Gains)

Subject	No. Trials		Average No. Associations		Percent of Gain	
	A	*T*	Alone	Together	Alone	Together
Aza	9	9	25.5	27.5		7.8
Cur	8	10	18.6	19.6		5.3
Han	5	4	35	37		5.7
Ric	9	15	24.9	22.8	8.4	
Rot	8	16	16.9	17.3		2.3
Sli	9	10	19	20		5
Tan	8	16	29	29.6		2
Tay	7	9	47.1	42.8	9.1	
Average	8	11	27	27	8.7	4.7
M. V.			8.2	7.1	.35	1.7

Number of subjects having greater number of associations *Together* 6
Number of subjects having greater number of associations *Alone* 2
Number of subjects having equal number of associations *Together* and *Alone* 0

3. *Discussion of Results.*—The general results of this experiment verify those of the one preceding. A somewhat greater proportion of subjects (75 per cent.) have a social increment. This proportion is still, however, less than the

93 per cent. who attained social increments in the mechanical task of writing down *every* associated word.

The average increment is less than the average decrement owing to the unusually large decrements of the two subjects who did better alone. This fact probably justifies the conclusion of individual differences, the two subjects, Ric and Tay, requiring the solitary condition for their best efforts. Work upon reasoning (see Exp. VI.) corroborates this tendency in the case of Ric.

The mean variation agrees with that of experiment III. in showing a greater variability in work done alone. (The mean variation of increments and decrements is here without value owing to the small number of cases.) We should take into account here the part played by rivalry in producing the social increment of the results. The correlation in this experiment is very high (vide infra). Where rivalry is effective (*i.e.*, not merely present) in group work there is a tendency toward uniformity among the members.

The correlation between the subject's associative ability and the degree to which his work is increased by the group is still inverse, though small (− .19). Here also we find toward the extremes conspicuous exceptions to the inverse correlation.

We noted in our last experiment a slight tendency for conscious rivalry to become a cause, or at least an accompaniment, of the group stimulus. We now find that tendency verified. There was found a positive correlation of .89 between the excess of consciousness of rivalry together and the size of the social increment. Rivalry therefore plays a greater part in the speed of associations for the most part merely *thought* than it does in the case of associations whose flow is limited to the speed of their *writing*. This result is doubtless due in part to the readier improvability of the mental association speed than of the speed of writing.

The significance of inhibitions in producing the solitary decrement is suggested by a positive correlation between an excess of inhibitions alone and the tendency to improve in the group. The correlation found (.76) indicates that the low level of the subject's work alone is connected with his inhibitions—these inhibitions occurring for him more numerously in the solitary than in the group condition. Alone there is evidently a blocking, traceable perhaps to a lapse of attention or effort, or to forgetting "where one is." Under the stimulus of the group the response receives greater facilitation.

Experiment V

Controlled Association—Contrasted Occupations

As a minor digression from the usual setting of the experiments, an attempt was made to compare not the work of the subject alone with his work in the group, but to compare his accomplishment, always in the group, in tasks *similar* with that in tasks *opposite* to the occupations of his fellows. Half of the group were seated at one side of the table and instructed to write words all bearing upon one specified topic (*e.g.*, winter), while the other half, seated opposite, wrote words all bearing on the opposite theme (summer). The solitary condition was not used in this experiment.

A test pair consisted of two performances upon a given theme, one the result of working in the manner described above, the other produced at another time when all members of the group wrote upon the *same* topic. Only about three such test pairs were obtained from each subject. Their total for the experiment was 35. Of these pairs 19 showed an excess of associations written in the common occupation over those written in contrasted work. In only 16 pairs was the advantage with the contrasted occupation. The average excess also in the common work was greater (7) than the average excess where it occurred in the contrasted setting (5).

So far as these results go there seems to be an advantage in tasks in which all are working in agreement over work done while pursuing diverse trends of thought. The tests given are however too meager for certainty; they indicate merely an interesting possibility for further investigation.

Experiment VI

Thought Process

1. *Procedure.*—An experiment was finally performed which extended the study of the social influence to the more intellectual functions involved in reasoning. Statements have been made by various writers that this "higher" quality of process is better performed in solitude. Tests of critical and original thinking were therefore devised in the nature of discursive reasoning. Short passages were selected from the works of Epictetus and Marcus Aurelius which admitted of considerable argument, for and against. The task of the subjects was to write down all the arguments, as many and as strong as possible, which they could think of to *disprove* the point made in the passage given. The epigrams both together and alone were presented in legible handwritten form, one copy to each subject. At the beginning of the group tests it was emphasized that they were all writing on the same statement. The time allowed for writing the ideas in a single test was 5 minutes. A separate passage was used for each test.

Nine subjects were used, arranged in two groups, *A* and *B*. Approximately 20 tests were given alone and 20 in the group. The total period covered by the experiment was 2 months. The social condition was changed (from *A* to *T*, or from *T* to *A*) on successive days. Group *A* began with *T*, group *B* with *A*. The passages selected naturally varied somewhat in suggestiveness; but it is believed that in a series of 20 a fair uniformity was obtained for the two social conditions. This is still more likely since only two authors were used, and those two are singularly constant in the tenor of their utterances.

2. *Treatment of Data.*—Each test written was graded as to quality of the arguments proving the negative of the statement. For this purpose the following scale was used. A distinct, clear, and (for the subject) forceful idea going directly to the question received a score of 3. Developments, extensive illustrations of the point made, giving pertinent

opinion of an authority, refining or re-stating the question to remove its logical objections, suggesting an alternative proposition, and the like, each counted 2. Emphatic statements or interjections of rejection, quotations merely stating the opposite, personal aphorisms, repetition of an argument already given, qualification or withdrawal or a former argument, rather irrelevant arguments, and so on, were each scored 1. For each subject each type of idea or argument thus contributed its proper score to each test; and by averaging the sums of these scores for the various tests there were found the average individual scores for ideas. These results are presented in Table VIII.

TABLE VIII

REASONING

Average Scores for Ideas

Subject	No. Trials		Score of Ideas		Per Cent. of Gain	
	A.	*T.*	Alone	Together	Alone	Together
Aza	19	22	5.6	6.6		18
Cur	19	16	7.5	7.6		1.3
Han	15	24	12.6	14		11
Ric	23	19	9.7	9.7		
Rot	17	20	4.5	4.8		6.6
Sli	19	20	9	10.2		13.3
Tan	23	19	7.7	8.2		6.5
Tay	2	5	9	10.2		13.3
Wil	12	15	7	8.4		20
Average	16.6	17.7	8	8.8		11.2
M. V.			1.7	1.9		4.9

Number of subjects having higher idea score *together* 8
Number of subjects having higher idea score *alone* 0
Number of subjects having equal idea scores *together* and *alone* 1

In addition, the total number of ideas of the three types, together and alone, found in the work of each subject, were averaged separately. Table IX. presents this comparison of averages of ideas types. Taking the number of each type as a per cent. of the total number of ideas in the given social condition (*i.e.*, together or alone), one may compare the relative contributions, together and alone, of the several types to the total idea score for the subject. This comparison is shown in Table X. Finally the words written in each test were counted, and the average number of words per test for the different individuals, together and alone, was computed. Table XI. contains these averages together with the percentile gains under the proper social condition.

Table IX

Average Numbers of Idea Types

Subject	Average Numbers per Test of the Various Types; 3's, 2's and 1's[1]					
	Alone			Together		
	3's	2's	1's	3's	2's	1's
Aza	1.3	.6	.3	1.5	.9	.2
Cur	1.5	1.3	.3	1.6	1	.6
Han	3.2	1.2	.3	3.5	1.4	.6
Ric	2.3	1.1	.3	2.2	1.3	.4
Rot	1	.5	.6	1.2	.4	.4
Sli	1.9	1.5	.1	2.5	1.2	.3
Tan	2.4	.1	.0	2.6	.1	.05
Tay	2.5	.5	.5	2	2	.2
Wil	1.7	.7	.4	1.8	1.2	.6
Average	1.97	.8	.31	2.1	1.0	.36

Table X

Percentile Comparison of Idea Types

Subject	Per Cent. of Average Total Ideas Comprised by Ideas of the Various Types—3's, 2's, 1's					
	Alone			Together		
	3's	2's	1's	3's	2's	1's
Aza	59	27	14	58	34	8
Cur	48	42	10	50	31	19
Han	68	26	6	64	25	11
Ric	62	30	8	57	33	10
Rot	48	24	28	60	20	20
Sli	54	43	3	63	30	7
Tan	96	4	0	94	4	2
Tay	72	14	14	48	48	4
Wil	61	25	14	50	33	17
Average	63	26	11	60	29	11

[1] These figures are *numbers* of cases only. The idea scores, as given in Table VIII., were obtained by multiplying the number of 3's by 3, the 2's by 2, and the 1's by 1. and then adding these products. This was done in the separate tests however, since the averages given in Table IX. contain small inaccuracies due to decimals.

COMPARISON OF PERCENTAGES

Type 3	Number of subjects having higher percentage for type 3 together	3
	Number of subjects having higher percentage for type 3 alone	6
Type 2	Number of subjects having higher percentage for type 2 together	4
	Number of subjects having higher percentage for type 2 alone. (1 subj. equal)	4
Type 1	Number of subjects having higher percentage for type 1 together	6
	Number of subjects having higher percentage for type 1 alone	3

TABLE XI

AVERAGE SCORES FOR WORDS WRITTEN

Subject	Score of Words		Per Cent. of Gain	
	Alone	Together	Alone	Together
Aza	69	90.4		31
Cur	81.4	80.9	.6	
Han	148	144.1	2.6	
Ric	98.4	108		9.7
Rot	29.8	39.6		33
Sli	119.7	125.6		4.9
Tan	58.6	67.5		15.1
Tay	158	167.8		6.2
Wil	100.4	96.2	4.1	
Average	96	102	2.6	16.6
M. V.	32.1	30.3	1.1	10.2

Number of subjects having higher word scores together........................ 6
Number of subjects having higher word scores alone........................ 3
Number of subjects having equal word scores together and alone........................ 0

3. *Discussion of Results.*—From Table VIII. it will be seen that of the 9 subjects used, 1 had equal average idea scores together and alone. The remaining 8 *all* had higher idea scores when working in the group. The average idea score together for all subjects showed also a social increment: it was 8.8, while the average alone was 8. The individual social increment, generally rather large, ranged from 1.3 per cent. to 20 per cent., 5 out of 9 being above 10 per cent.

Turning to the percentile importance of each idea type alone as compared with its importance together, as shown in Table X., we find that 6 out of 9 subjects had a higher percentage of superior ideas (counting 3) while working alone. The number of the subjects, together and alone, having the higher percentage of "2" ideas was *equal.* There were (reciprocally to the "3" class) just 6 out of 9 subjects who had a higher percentage of the lowest type of ideas (counting 1) while working in the group. The averages of all subjects indicate also a higher performance, relatively, so far at least as the proportion of superior ideas is concerned, while working alone. There is thus demonstrated a social subvaluent for argumentative or discursive reasoning. This finding is no doubt in accord with commonly observed facts of life. Who has not been aware, upon retrospection, of the low order of logical value in many arguments given under such a strong social influence as that of political meetings and oral debates? There seems to be a *spreading out* of our throught rather than a strong output of separate original ideas of logical worth. Group thought is *extensive*; individual thought is, to some extent, *intensive*.

May not this "extension" in group thought be also characterized as "wordiness"? It seems quite logical to call it this, for Table XI. shows that 6 out of 9 subjects wrote more words in the group than they did alone. The averages of the individuals' scores also show the group gain in number of words written (102 to 96). A third evidence lies in the excess in the average of the social increments (16.6) over the decrements (2.6). This disclosure is consistent with the results of all previous experimentation on the social influence. There has been throughout a clear increase of the *quantitative* aspect of mental processes and mental work in the group condition. Our association experiment of writing every word in free thought may be compared with the writing of every word in controlled thought in the present experiment. In both cases every subject but one showed, in the quantity of words written, a distinct social increment.

4. *Introspection.*—There is some evidence in the reports of the awareness of the social subvaluent in the thought process.

SUMMARY OF CONCLUSIONS

A. The Influence of the Group upon Association

I. *Quantitative Aspects*

1. The main result of the preceding experiments on association is the conclusion that the *presence of a co-working*

group is distinctly favorable to the speed of the process of free association. In various tests from 66 per cent. to 93 per cent. of the subjects show this beneficial influence of the group.

2. The beneficial group influence is *subject to variation according to the nature of the task.* In the more mechanical and motor requirements, such as writing *each word* associated, the group stimulus is more effective than in the more highly mental or more purely associational tasks such as writing only every *third* or *fourth* word.

3. There are *individual differences* in susceptibility to the influence of the group upon association. One type, who are nervous and excitable, may succumb to the distracting elements of the group activity and may show either no effect, or else a social decrement.

4. *In its temporal distribution* the beneficial effect of the group is greatest in the first part of the task and least toward the end of the task.

5. There is a tendency for the *slow individuals to be more favorably affected* in speed by the group co-activity than the more rapid workers. There are, however, certain striking exceptions.

6. *The variability in output* among the individuals varies generally with the social influence. Hence it is usually greatest in the group work. A striking exception to this occurs in the tests where rivalry is correlated with the social increment, and where only every third or fourth word is written. Here the variability is greatest in the solitary work. This result is in agreement with that of earlier investigators working on different processes.

7. There is suggestive but *not conclusive* evidence that the output of associations in a group where all the members are forming associations in the same category is greater than that in groups in which the members are divided in the trend of their associations between opposite or contrasted categories.

II. *Qualitative Aspects*

8. A greater number of *personal associations* are *produced alone* than in the group.

9. In harmony with this fact is the tendency for subjects to produce *ideas suggested by their immediate surroundings* with *greater frequency in the group* than alone.

10. Less clear cut, but very probable, are the tendencies to produce a *greater number of "free rising" ideas in the group*, and to produce a greater number of words *suggested mainly by the initial stimulus word when working alone.*

III. *Factors in the Social Influence*

11. There are two opposing groups of factors in the influence of the social condition upon the association process. They are:

(1) Facilitating Factors:

(*a*) *Facilitation of movement* by perceptions or ideas of movements in others near us.

(*b*) *Rivalry* intrinsic in the bare social setting of a group working together. Rivalry is well correlated with the beneficial influence of the group in tests of a more mental sort (and less mechanical) such as writing every *fourth* word only. It is not so correlated when each word is written.

The beneficial effects of the group in experiments where the rivalry consciousness is closely correlated with this influence is less than in experiments where it is not so correlated, but where other factors—for example, motor facilitation—serve as the stimulus of the group.

(2) Impeding Factors: distraction, over-rivalry, emotions. Of the two groups, the facilitating is by far the more important in the total effect upon the work.

12. Beside the comparisons already indicated, we may note the general agreement of our work with that of earlier students in the *speed* improvement of mental operations, as shown by the quantity of the product, under conditions of working with others.

B. The Influence of the Group upon the Thought Process

13. In the highly controlled association of the thought process, as typified in written argument, more ideas are pro-

duced in the group than when working alone. Again we find an increased flow of thought owing to the social stimulus.

14. Among the ideas so produced, those of superior quality, however, are of relatively greater frequency in the solitary than in the group work. Ideas of a lower logical value are relatively more numerous in the group work.

15. More words are used in the arguments produced in the group than in those produced in solitude.

16. From the above facts, and also from the introspection of the subjects, we may conclude that the presence of the group influences the reasoner toward a more conversational and expansive form of expression. The more intense logical thinking of solitude gives way in the group to extensity of treatment.

17. These results appear to be related to the common observation that work requiring imagination or more concentrated and original thought is best performed in seclusion. There is also a connection suggested with the writer's experiments upon the social influence in attention and mental work.[1] In that investigation, as well as in the present, the social influence was found to improve the quantity but not the quality of the mental performance.

[1] To be published in the near future.

ATTRIBUTIVE *VS.* COGNITIVE CLEARNESS

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TABLE OF CONTENTS

I. Introduction

In Professor Titchener's article on the 'Psychological Concept of Clearness,[1] in which he meets the polemical part of Britz's dissertation entitled, 'Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit,'[2] he calls attention at the end of the article to the need of repeating Britz's experiments, for as he says, 'experiment is best met by experiment.'[3]

The present paper reports the results of a series of experiments carried out so far as possible under the conditions laid down by Britz.

II. Apparatus and Method of Procedure

We found it very difficult to reconstruct the experimental procedure from Britz's description. His account is so meager, and he makes so many omissions in his exposition, that we were forced to provide many of the methodical details ourselves. Consequently, instead of referring to Britz's work for the method, we give a complete description of it here.

Apparatus.—Since we did not possess a Schumann tachistoscope, we substituted for it a Wundt gravity tachistoscope. This was rendered almost noiseless by an arrangement of felt pads; indeed the slight thud from the falling shutter very soon became habitual, so that none of the conditions of the experiment was changed by this substitution.

Upon the white exposure cards, 7 x 10 cm. in area, were glued a series of small rectangles cut from colored papers. They were 7 x 12 mm. in area, and were arranged with the longitudinal axis vertical, at a separation of 5 mm., in a horizontal row in the center of the cards. Ten colors were used: two shades of red, an orange, a yellow, a green, a blue, a violet, a purple, a gray, and a black.[4]

[1] *Op. cit.*, *Psych. Review*, XXIV., 1917, 43–61.

[2] Britz, C. A., *op. cit.*, Saarlouis, 1913.

[3] *Op. cit.*, 60.

[4] These are the colors that Britz used (*op. cit.*, 55); but he did not state specifically what qualities he selected. We used the following: R 1, the 'Farbenkreis Rot,' *i.e.*, the 'Hering Red,' Stoelting color No. 1; R 2, the Milton-Bradley 'Red, tint No. 2'; O, the Milton-Bradley 'Orange'; Y, the 'Farbenkreis Gelb,' *i.e.*, the 'Hering Yellow,'

Forty exposure cards were prepared. On half, Series I., there were 5 rectangles; and on half, Series II., there were 6. The sequence of the colors was different on every card, but it was so arranged that every color appeared as often as every other, and equally often in the first, second, third, fourth, fifth, and sixth (for cards of Series II.) places. A particular color never appeared more than once upon the same card. The specific arrangement of the various colors upon the exposure cards appears below in Table I:

TABLE I

THE SPECIFIC ARRANGEMENT OF THE COLORS ON THE EXPOSURE CARDS

Series I						Series II						
Card Number	Order[1]					Card Number	Order[1]					
	1	2	3	4	5		1	2	3	4	5	6
1	5	10	4	2	7	1	1	4	9	3	2	7
2	4	1	5	8	3	2	10	7	5	2	8	6
3	7	9	6	10	2	3	3	8	1	6	10	4
4	8	4	5	3	1	4	5	9	2	7	3	10
5	10	7	2	9	6	5	9	5	4	1	6	8
6	1	2	7	5	3	6	4	1	7	10	5	2
7	2	6	7	9	5	7	6	10	8	9	1	3
8	1	8	3	4	10	8	2	6	10	5	7	1
9	8	10	9	6	4	9	7	2	3	8	4	9
10	2	9	4	7	5	10	8	3	6	4	9	5
11	3	8	10	1	6	11	1	2	5	10	7	8
12	3	5	10	1	8	12	3	9	4	6	1	10
13	4	2	8	5	9	13	8	3	2	1	5	6
14	10	3	2	6	7	14	4	7	9	2	6	3
15	6	1	9	3	4	15	5	4	7	8	10	9
16	9	3	6	4	1	16	9	5	6	3	2	7
17	5	4	8	2	10	17	10	6	8	4	3	1
18	7	6	3	8	2	18	2	10	1	9	8	4
19	9	5	1	7	8	19	7	1	3	5	9	2
20	6	7	1	10	9	20	6	8	10	7	4	5

Stoelting color No. 5; G, the Milton-Bradley 'Green'; B, the 'Farbenkreis Blau,' *i.e.*, the 'Hering Blue,' Stoelting No. 13; V, the Milton-Bradley 'Blue-Violet'; P, the 'Farbenkreis Blau-rot (Purpur),' *i.e.*, the 'Hering Purple,' Stoelting No. 15; Gy, No. 12 in the Hering Gray series; and Bk, Stoelting No. 18, not of the Hering Gray series.

The 'Farbenkreise' which we have in the Cornell Laboratory came to us from the manufacturer with an inversion of two qualities, the Bluish Green and the Greenish Blue samples were interchanged. We mention this fact, since similar errors may have been made in other 'Kreise.'

[1] From left to right.

1, red 1; 2, red 2; 3, orange; 4, yellow; 5, green; 6, blue; 7, violet; 8, purple; 9, gray; 10, black. For the tint and chroma see footnote above.

Six exposure times were used which varied, by intervals of 40 σ, from 40 σ, the shortest exposure, to 240 σ, the longest exposure. The exact times as measured by the Hipp chronoscope and its control apparatus were:[1]

a	*b*	*c*	*d*	*e*	*f*
40.4 ±.9	80.1 ±1.1	119.5 ±2.7	160.6 ±2.6	199.5 ±3.7	239.8 ±5.2

Every card was exposed once at every exposure time. The order was determined haphazardly.

The apparatus was set up in a dark room, and the exposures were made in artificial daylight illumination.[2] The source of light was two 100-watt nitrogen-filled Mazda lamps. These with the 8-inch roundels of 'Daylite glass' were placed in front, to right and left of the tachistoscope, so that no shadows fell upon the exposure cards.

The observations were made through a telescope of 4 diameters magnification. The telescope projected through a large black screen which separated the observers from the apparatus. The eye-piece was 1.50 m. from the exposure card.

Observers.—The observers were Professor H. P. Weld (W); Professor W. S. Foster (F); and the author (D).[3] They were all highly practiced in the introspection of attributive clearness. W and F worked without specific knowledge of the problem other than that given in the instructions. D did not look over the data of W and F until after the completion of the experiment. This had the advantage that his own results were not influenced by the work of the other

[1] Britz does not state precisely what exposure times he used. He merely says that the longest was around 250 σ, and the shortest about 40 σ. He does not say how many steps there were between these extremes, nor whether he employed them all an equal number of times.

[2] Britz made his exposures by daylight, and says that "the greatest care was taken to insure constant illumination" (*op. cit.*, 55). We have found that daylight varies so greatly from hour to hour and from day to day that where constancy is desired artificial light must be employed. For the effect of artificial daylight see A. J. Brown, *Am. J. of Psychol.*, XXVII., 1916, 427–429.

[3] Our thanks are due to Dr. G. J. Rich and to Dr. L. B. Hoisington for acting as experimenter while D observed.

observers; but also the disadvantage that the development of the experiment could not be followed.

The observers served three times a week. The experiment began in February, 1917, and continued until June. By this date all the observers had completed 240 experiments, that is, every stimulus card had been exposed once at every exposure time.

Instructions.—The observers were given the following instructions, which they were required to read at the beginning of every experimental hour:

"At the signal 'ready' place your better eye to the telescope and turn to the fixation point. At the exposure, which will follow the signal 'now,' give your attention to the entire card.

"Immediately after the exposure you are:

(*a*) to name and describe what you have seen; and

(*b*) to give as exhaustive a description as possible of the processes involved during the course of the experiment, particular emphasis being placed upon the report of attributive clearness." [1]

The observer wrote his report while E arranged the apparatus for the next exposure. The observer's desk was illuminated by a third 'daylite' lamp, so that the observations, as in Britz's experiment, were performed in daylight adaptation.

III. Results

The data readily lend themselves to quantitative treatment;[2] and therefore in what follows the quantitative and qualitative aspects shall be carried side by side. Every observer, as we have noted above, was given 240 experiments; in half there were five colored rectangles on the exposure cards, and in half there were six. Altogether, then, 1320 colored rectangles were exposed, every one of which should

[1] We desired to reproduce Britz's instructions, but he does not quote them; consequently we cannot be certain that we have done so. We have, however, incorporated in our instructions all the positive statements that he gives us (*op. cit.*, 55, 62).

[2] Britz did not treat his data quantitatively. He says (*op. cit.*, 55): 'Die Art unserer Problemstellung brachte es mit sich, dass wir auf quantitative Resultate verzichten konnten.'

be accounted for in the observer's reports, either as omitted or as described.

A. Classification of Cognitive Phases

Our first task, following Britz, was to classify the observers' reports upon the basis of the descriptions. This classification, together with the number of cases occurring at every step, appears for the three observers in the 8th, 13th and 18th columns of Table II. These data corroborate Britz's findings. We have been able to identify and to verify the ten phases that he reports. We have further been able to extend the classification to 17 steps.

Below the first step we place the omissions and the subjective reports. The omissions, as Britz observed, were rare.[1] That they were rare is shown by the fact that W omitted only 1.9 per cent.; F, only 0.3 per cent.; and D, 1.2 per cent. The omissions were of two kinds: first of areas that were not noticed, as when, for example, a six-figure card is cognized as five; These cases are shown in the table opposite 'Omitted without knowledge'; and secondly, of areas that were noticed, as when, for example, an interval or an unfilled space was reported at the point that the omitted figure objectively occupied. In illustration we quote from a report of observer D: "there is a space between the two blues, but I did not perceive anything in it, merely cognized the interval" (99). These cases are shown opposite 'Omitted with knowledge.' Britz did not note omissions of this second kind.

We found that the subjective reports, *i.e.*, reports in which more colors are described than objectively appear on the cards, were likewise of two kinds: first, those in which the form of the impression was more or less obscure, being either not mentioned at all, or else reported merely as a spot, flash, speck, blur, etc.; and secondly, those in which the form was definitely cognized as being rectangular. As an example of the first we quote from a report of W on a card from Series I. (a five-figure card) in which, referring to a sixth quality, he says: "I also saw a color, which I called Pink, to the right of

[1] *Op. cit.*, 56: 'Diese Fälle sind zwar selten, kommen aber immerhin vor.'

Green; this pink is a blur of color but is of very good chroma" (26). As an example of the second kind we quote a report of F, following an experiment with a card from Series II. (a six-figure card): "There were definitely seven rectangles" (27).

Though Britz observed that subjective reports were made, he did not distinguish these two kinds.[1] He noted, however, that they occurred more frequently than the omissions. Our results confirm this, and show that W gave 7.8 per cent. subjective reports; F, 6.7 per cent.; and D, 2.0 per cent. At times, all of the observers reported 6 colors when only 5 were exposed; and 7, 8, and even 9 colors when only 6 were exposed.

Britz quite properly excludes such cases,—subjective reports and omissions—from his classification.[2] They were not part of the observer's perception, and therefore do not properly belong to a perceptual classification.

Britz regards as his lowest step the cases in which the observers describe their impressions as spots without being able to say anything definitely about them.[3] We were able to verify this step, as the following reports reveal: "Last two areas to the right vague, certain of them, but they are without color or contour, are not sharp or distinct from the background" (D 48); "Experienced two other fields, but can not name them. They had no distinct form" (W 46). Though these cases do not contain any knowledge of quality or of form, they do of number; "two" in each case were experienced. One of our observers, W, gave cases in which not even this bit of positive knowledge was vouchsafed, and therefore marks a step that is lower in the scale than the lowest reported by Britz. As examples of this step we give the following: "Others experienced, came as flashes, that is all I know about them" (48); "Others, cannot describe them however" (52). It will be observed that neither F nor D reported experiences of this kind; that F did not,

[1] *Op. cit.*, 56.

[2] *Op. cit.*, 56: "Unser besonderes Interesse setzt da ein, wo zum wenigsten irgend etwas, wenn es auch noch so geringfügig zu sein scheint, an dem Farbfelde beobachtet wird."

[3] *Ibid.*, 56.

may be due to the few cases which he reports with indefinite form (see Tables II and III); that D did not, may be due to the fact that he was familiar with the exposure cards.

Our next step, the third, accords with the second step of Britz, in that a tint, *i.e.*, lightness or darkness, is ascribed to the impressions. Examples of this are: F (175) "At the end of the series two dark impressions appeared"; D (20) "Have only a general impression of the three other areas. One on the left dark." The conclusion of this report illustrates our next, the fourth step, for D continues; "other two were colored, but I have no knowledge of the quality." This corresponds with Britz's third step, and may further be illustrated by a report from W: "The blur next the blue was extended, but without distinct outline,—it was perceived as colored" (43). Again; "there were two other colors that almost got 'noted,' but the exposure time was too short" (38).

The next step, comparable with Britz's fourth, includes all the cases in which the general quality of the impressions is reported. "The impressions from the extreme left are reddish, that is all that I can say about them" (F, 144). "I feel sure that there was an orange or some hue much like it in the field. I wish I could describe the 'feel' I had as the shutter fell, the 'feel', 'knowledge' that I saw something clearly, but could not tell just what it was,—that if I had had another instant of time, the color would have been named; but I can say no more about it" (W, 38).

Our next step, the 6th, corresponds with Britz's sixth. We classified at this level those reports in which a specific color is ascribed to an indefinite form. For example: "The first color on left was yellow, the outlines were indistinct—my impression now is that I 'marked' or 'noticed' the yellow as the shutter closed" (W, 8). "The last two to the right came to me merely as color, as yellow and purple" (F, 223).

We obtained a number of cases analogous to those grouped in Britz's fifth step, in which the specific quality reported is only one of the components of the stimulus, as the following examples taken from Britz and from our reports show: "Erkannt wurde eine Rotnüance. Im Nachbild aber ein reines Orange, daher wird ein Orange angenommen. Jedoch nur in Rotnüance sicher, nicht in seiner Gelbnüance" (p. 58); (W, 58) "Violet was first noted as blue, it wasn't until after the shutter fell that judgment was changed to violet. This has occurred before. As a rule, O, P, and V, are noted in their specific quality, but there have been times when the first judgment of O and P was R, and of V was B." (W, 96) "First apperceived as red; when it was named just after exposure was called red; but visual image and memory of it was that it was purple. Certain that purple was the color."

Britz includes in this step all those cases in which "bei 'zusammengesetzten' Farben nur die eine Komponente zum Bewusstsein kommt, z.B., beim Orange nur die Rotqualität" (p. 61). We did not construct a separate step for reports of this kind, because we thought that this marks a shift in the criteria of classification. Until now the observers' reports were accepted at their face value, and no effort was made to go behind them to the objective stimulus, or to processes which admittedly occurred after the exposure period. Here, however, this has been done, and reports have been classified upon a new basis. If this 'modus operandi' is admitted here, then the classes already discussed should be further divided accordingly as the reports are correct or incorrect; and if incorrect, accordingly as they are partially or totally wrong. A justifiable procedure in an 'Aussage' problem, but not in the present study, in which we are only concerned with the subjective experiences which have been aroused during the exposure of the color fields!

Our seventh step, the last with indefinite form, corresponds with Britz's eighth. The contour is still indefinite, but the quality is fully described, as the following introspections show: "Green, good saturation, medium tint; doesn't refer to any particular place; extended over the right end" (F 111); "Blue and yellow, blurry, more like flashes of color; yellow very light tint, poor chroma" (W 45); "Perception of colored spots. Predominant tone seemed to be blue. One speck to right was yellow, bright and rich, contrasted with the blue which was dark and poor saturation" (D 99).

We now turn to the reports in which the rectangular form is definitely experienced. As Table I. shows, the majority of the impressions were of this kind. Comparatively few belong to the steps that we have thus far described. This is brought out more clearly in Table III., in which the reports are divided according to the form experienced by the observer. The table shows that W perceived the rectangular form in 86 per cent. of his reports; D in 94 per cent. of his reports; and F perceived it in 98 per cent.

The eighth, ninth, tenth, eleventh, twelfth, and thirteenth steps are, with the exception that the perceptual forms were reported as rectangular, the counterparts of the first six steps described above, as the following examples from the observers' reports will show:

The eighth step: W (31), after describing two colored rectangles says: "There were others, I am sure of that, but I can not describe them." F (38) after describing four colored rectangles, continues: "Can't say whether there were 6, 7, 8, or even 9 in the series, but know that there were others." Observer D, as Table I. shows, did not give reports of this nature; he was always definite as to the number of impressions perceived.

The ninth step: W (20), after giving the description of 4 rectangular areas, says: "There were two others, but I cannot describe them." F (97) "7 rectangles, but the quality of only 4 perceived." D (127), reporting 8 rectangular areas, described the quality of four and then concluded: "Haven't the least idea of the qualities of the other areas."

The tenth step: W (10) "There were two other rectangular dark grays, I cannot say whether they had any color"; and again (227), "There was one area to the left of Green that I almost apprehended. I was left with the feeling that if I had had another instant I could have made it out. It was dark, I think." F (222) "Think that in the rest of the series light and dark alternated. I had a sort of 'checker-board' feeling for them." D (51) "Fifth rectangular area was light, but beyond that I can not say."

The eleventh step: W (164) "The remaining areas were cognized as colored, but I cannot name them." F (125) "Seven colored rectangles; can describe only the first four." D (71) "The first area was colored; perceived that, but couldn't cognize the quality."

The 12th step: W (168) "Bluish, cannot describe it beyond that." F (125) "Last rectangle colored, may have been yellow, a very light gray, nearly white, or some other very pale color." D (157) "Clear perception of five rectangular colored areas. . . . The fourth was cognized as reddish, may have been a red, orange red, or a purple. Only know the general hue."

The thirteenth step: W (215) "Red, cannot be sure of its tint or chroma." F (95) "Purple is the color of the fourth rectangle from the left. Just a name, I can't describe it." D (11) "First rectangle from the left, blue, just hue."

The following reports characterize the 14th step: W (237) "Rectangle, violet, dark, cannot say about chroma"; F (74) "Two blue rectangular figures, they were different, don't know just exactly how, think that one was dark and one light"; F (108) "Two reds, one very dark, other slightly orange-red, and lighter." We may identify this step with Britz's seventh phase.

Our next step, to which the fourteenth is very closely related, is described in the following reports: D (1) "Perception of six colored fields upon white background. The three to the left were definitely bounded and rectangular, while those to the right were patches of color without definite outlines. Of the rectangular areas, the first was colored; the second, was yellow, bright; and the third was blue, well saturated." The fourteenth step includes the reports in which hue and brightness are perceived, the fifteenth those in which hue and chroma are perceived. Other examples of this type are: F (147) "Do not think that blue-green was very well saturated"; F (216) "Purple and orange were not of good saturation"; W (124) "Blue, pretty good chroma, I cannot say as regards tint."

The sixteenth step is comparable with Britz's ninth. The descriptions are complete; form, hue, tint, and chroma are definitely reported, but not the 'paper' character. In illustration the following reports are quoted: W (97) "Last rectangle to the right red, a dark red, good chroma"; F (71) "Perception of 5 colored rectangular areas . . . third blue, medium tint, and rather poor saturation; 4th green, slightly yellowish of fairly light hue, and fair saturation"; D (57) "A number of rectangular areas. . . . The extreme left one was jet black, the extreme right one was a rich, dark, deep blue."

Our last, and the seventeenth step, is illustrated by the following: W (22) "In the black I saw the texture of the paper"; (117) "Black, shiny, enamel-like"; F (79) "Black, was not velvet black, but a sort of dirty, or handled, or rubbed black"; (234) "First rectangle light orange, poor saturation, looked like a cold water paint on a wall, a bit like those in the laboratory"; D (56) "The fourth rectangle gray, medium tint, certain about it, could even see the grain of the paper"; (70) "Rectangle to the extreme right blue, light, but of good chroma, paper quality, hard and shiny"; (87) "The first rectangle was purple, dark, well saturated, but papery, dull, flat"; (108) "The central rectangle was gray, which was quite definite, it was a little darker than middle gray, more clearly cognized than at any other time in the series, even perceived the grain of the paper." This step finds no counterpart in Britz.

Neither can we, for that matter, give a step which is the exact complement of Britz's tenth phase; not because we lack reports of the kind that he quoted,—for our observers did occasionally give similar accounts,—but because such action would mark a shift in our criteria of classification, a point which we have already sufficiently discussed.

A résumé of the various phases described above is offered in Table II., which gives, besides, the number of cases occurring at every step. Table III., which follows it, shows the number of times the colored rectangles were perceived with and without definite form.

Both tables show that the reports may be divided into classes which differ according to the perception of the form.

TABLE II

SHOWING THE VARIOUS COGNITIVE PHASES, THE NUMBER OF CASES OCCURRING AT EVERY PHASE, AND A DIVISION OF THESE CASES ACCORDING TO THE ATTRIBUTIVE CLEARNESS OF THE PERCEPTIONS

Degrees of Cognition	Step	Observer W: Clearness: Not Reported	W: One Level: Upper	W: Two Levels: Upper	W: Two Levels: Lower	W: Three Levels: Upper	W: Three Levels: Middle	W: Three Levels: Lower	W: Total	F: Clearness: Not Reported	F: One Level: Upper	F: Two Levels: Upper	F: Two Levels: Lower	F: Total	D: Clearness: Not Reported	D: One Level: Upper	D: Two Levels: Upper	D: Two Levels: Lower	D: Total
Omitted																			
Without knowledge		3	6	0	6	0	0	1	16	1	0	0	1	2	5	1	0	5	11
With knowledge		4	3	0	5	0	0	0	12	0	1	0	1	2	3	1	0	2	6
Indefinite form																			
Added		1	5	0	9	0	0	1	16	1	0	0	2	3	3	1	0	4	8
Others experienced	1	1	2	0	2	0	0	0	5	0	0	0	0	0	0	0	0	0	0
A definite number experienced	2	18	16	0	35	0	0	6	75	0	2	0	0	2	9	3	0	25	37
Tint	3	1	3	0	6	0	0	0	10	0	4	0	0	4	0	3	0	3	6
Colored	4	6	5	0	11	0	0	4	26	1	2	0	0	3	0	4	0	11	15
General quality	5	4	1	0	6	0	0	0	11	1	2	0	0	3	1	0	0	4	5
Specific quality	6	5	2	2	10	0	0	1	20	0	6	0	0	6	0	0	0	0	0
Hue, tint, chroma	7	11	11	0	14	0	2	0	38	1	2	0	0	3	5	2	0	1	8
Definite form																			
Added		33	23	0	39	0	0	0	95	16	63	0	11	90	3	4	0	13	20
Others experienced	8	7	7	0	8	0	0	0	22	2	4	0	9	15	0	0	0	0	0
A definite number experiences	9	109	60	0	93	0	0	0	262	24	91	0	29	144	19	111	1	71	202
Tint	10	4	2	0	6	0	0	0	12	2	25	0	0	27	13	56	5	29	103
Colored	11	54	20	3	74	0	0	3	154	5	53	2	10	70	7	67	4	28	106
General quality	12	12	13	7	5	0	0	0	37	31	147	15	10	203	13	82	48	12	155
Specific quality	13	27	21	17	5	2	2	0	74	62	245	41	1	349	29	135	69	9	242
Hue and tint	14	16	12	15	3	0	0	0	46	14	56	8	0	78	4	50	29	0	83
Hue and chroma	15	2	1	0	0	0	0	0	3	2	26	0	0	28	1	9	0	0	10
Hue, tint and chroma	16	164	113	179	7	2	3	0	468	27	238	30	0	295	25	180	88	0	293
Paper character	17	0	0	1	0	1	0	0	2	5	49	7	0	61	1	29	8	0	38
Total		482	326	224	344	5	7	16	1,404	195	1,016	103	74	1,388	141	738	252	217	1,345

Table III

Showing the Total Number of Times the Colored Rectangles were Perceived with and without a Definite Form, and a Division according to the Attributive Clearness of the Perceptions

O.	Perception of Form	Attributive Clearness							Total
		No Report	One Level	Two Levels		Three Levels			
				Upper	Lower	Upper	Middle	Lower	
W....	Indefinite	47	45	2	93	0	2	12	201
	Definite	428	272	222	240	5	5	3	1,175
F.....	Indefinite	4	18	0	2	0	0	0	24
	Definite	190	997	103	70	0	0	0	1,360
D....	Indefinite	18	13	0	48	0	0	0	79
	Definite	115	723	252	162	0	0	0	1,252

Table II. shows that within each of these classes the steps are comparable; and that it is only in the very highest qualitative phases that 'form' seems indubitably bound up with the perception.

This classification marks, step by step, the development of the perception from the simple to the complex. It is logically derived from the reports and the descriptions of the observers. But what has this logical classification to do with attributive clearness? We turn for an answer to the introspections, to that second part of the instructions which requires the observers to 'give as exhaustive a description as possible of the processes involved during the course of the experiment, particular emphasis being placed upon the report of attributive clearness.'

B. Complexity of the Problem

Before turning to this evidence it may be well to mention, as a sort of qualifying introduction, that neither the method nor the instruction was conducive to good psychological description. The observers were required to note meaning and process, and to give a full report of both. An extremely difficult instruction! for the observers tended either to concentrate upon the fulfillment of the first part, in which case the description of process was vague and uncertain; or to concentrate on the introspection of process, in which case the statements of meaning (in the last analysis that is what naming and describing the colored rectangles implies) were neglected and incomplete. We need not be surprised, therefore, that under these unfavorable conditions cognitive clearness should have been confused with (and reported for) attributive clearness.[1] The first part of the instructions 'set' the observers for meaning; it is only natural that this attitude should have been carried over to the second part, and the meaningful kind of clearness, the cognitive, have been mistakenly reported for the attributive. That this is the natural and, indeed, unavoidable consequence of the instructions is shown by the fact that all of Britz's observers, according to his own showing, reported solely the cognitive type of clearness; and further by the fact that our own observers, who had had extensive practice in the introspection of attributive clearness, and who had never experienced any difficulty in reporting it in other experiments in which clearness-reports were called for, found their task very arduous under the conditions of this experiment.

That Britz's observers were reporting cognitive clearness only is shown by the sample introspections that he gives (p. 60); and also by the fact that the expression 'clear' was used only in the higher perceptual phases. Britz himself points out: "Ueber den 'Klarheitsgrad,' der hier anzunehmen wäre, wurde meistens nichts geäussert. Auf den höheren Stufen aber, zu denen wir nun gelangt sind, ist die Aussage insofern modifiziert, als die Beobachter selbst zu Protokoll geben, das eine Farbe klar oder gar sehr klar war" (p. 59). Additional evidence is afforded by the following: "Sie ordnen für den jeweiligen Versuch sehr oft aus eigener Initiative die Farben nach ihrer Klarheit" (p. 59); and "Wir sehen, dass die Vpp. sich zwar sehr oft damit begnügen, die eine Farbe als klarer einer anderen gegenüberzustellen, aber andererseits doch auch manchmal anzugeben wissen, warum sie der betreffenden Farbe grössere Klarheit zuschreiben" (p. 60). That the observers could arrange the colors in order of clearness, and could explain why one color was clearer than

[1] For a differentiation of these two kinds of clearness see E. B. Titchener, 'Feeling and Attention,' 1908, 238 f; and S. S. Colvin, 'The Learning Process,' 1911, 255 f.

another, is proof positive that Britz was dealing with the cognitive type.

That our observers found it difficult to report both meaning and process is best shown in their own words. W says in one of the early reports: "The report is so complicated that it seems foolish to talk about attributive clearness"; and again, "Under the conditions of this experiment I have no criteria for distinguishing attributive clearness from cognitive." W was also throughout the experiment more or less disturbed by after-images. "The colors persisted in the after-period, and at present I have the notion that the relative clearness in the after-period influences my report of the processes during the time of the exposure. That is to say, it is very difficult to draw a line, with everything else that I have to do, between these two periods." Again, in a later experiment, "I am 'set' this morning for the problem of clearness during the exposure-period, and I find it difficult to remember the colors distinctly enough to describe them." F is even more explicit. He says, in one of the early experiments, "The instructions ask too much for an adequate report, and any side of the report (naming, describing, number, order, form, etc.) could, it seems to me, be 'set for' without making the least difference in the processes which go on in the actual exposure period"; and again, in a later experiment, "You know a lot about the colors after the exposure is over, but I am perfectly sure you lose a lot; as you start writing other aspects become non-reportable. It isn't so much the way you set yourself before the exposure that determines the report, but how you go to work after the exposure. I feel pretty sure that if E should call out after the exposure 'Report on chroma' I could report all chromas adequately, whereas usually by naming colors, mentioning clearness, brightness, etc., I lose certainty about chroma." D expressed a similar idea: "What I report doesn't depend so much upon what I experience as upon where I start the report. When I concentrate upon the naming and description of the colors, my report on process is uncertain, and *vice versa*."

C. Classification of Attributive Clearness

The reports were not only classified upon the perceptual basis given above, but they were also classified upon the basis of sensory clearness. In some of the experiments, because of the inherent difficulties mentioned above, the observers were not always willing or able to record clearness; these cases appear in the tables under the caption "Clearness not reported." In other experiments, the processes aroused by the stimulus were all experienced at one (the upper) level; in still others they were divided into two levels, the clear and the obscure; and in others again the processes were reported as having been experienced at three levels of clearness. These cases appear in the Tables under the respective captions "Clearness, One Level," "Clearness, Two Levels," and "Clearness, Three Levels."

1. *Clearness Not Reported.*—In rather a large proportion of the experiments—large, considering that the observers were especially directed to report upon clearness—clearness was not recorded. This was true in over 1/3 of the experiments for W, in nearly 1/8 for F, and in almost 1/10 for D. These occasions for F were evenly distributed between the two series of exposure cards, Series I. and Series II.; so that for him reportability of clearness was not so much a matter of simplicity of material as it was a matter of direction of effort: *e.g.*, (48) "I still am not always able to report upon clearness. The fact that I have to name, and that naming is not an absolute immediate process, but one which interferes with the memory after-image, prevents me from making a good report on the processes of the exposure-period itself." While this is also true of W and D, they are influenced to a greater extent by the complexity of material; for both show a greater number of failures in Series II.

All of the perceptual steps are represented in these reports; but relatively fewer low steps, relatively fewer perceptions of indefinite form (as is shown by Table III.), are reported under these conditions than under the conditions in which clearness is reported. Thus do the results bear out the observers' introspections given above: when full effort is given to recognizing and naming the colors the task is more adequately performed than when attention is divided between this and a description of process.

Table IV

Showing the Number of Experiments in which Attributive Clearness Was not Reported; and the Number in which One, Two and Three Levels Were Reported

Observers	Series	Attributive Clearness			
		Not Reported	One Level	Two Levels	Three Levels
W..........	I...........	33	35	49	3
	II..........	48	21	49	2
	Total......	81	56	98	5
F..........	I...........	17	92	11	0
	II..........	16	86	18	0
	Total......	33	178	29	0
D..........	I........ ..	3	90	27	0
	II..........	20	48	52	0
	Total......	23	138	79	0

2. *Clearness One Level.*—In the reports of all the observers every one of the 17 perceptual steps is represented when the processes aroused by the stimulus-card are judged to be of equal clearness, as Table II. and the following introspections show: W (32) "Five areas. Naming from right to left they were black rectangle, good deep black, a rich black; next a blue rectangle, good chroma, medium tint; next, yellow rectangle, I cannot describe it; then there were two others. The field as a whole was clear." Here we have one level of clearness, and at least three levels of cognition. Again from W, "Gray, a little darker than middle gray; pink, might have been a purple of light tint; red, cannot describe further; blue, good chroma, dark tint; three other colored areas. Duration of gray and pink rectangles was longer than that of other areas. Red and blue were momentary flashes. If I have not said it before I now wish to say for all time that the colors are undoubtedly noted, cognized, apprehended, as

objects, as 'Reds' and 'Blues,' etc. The name of the hue is the name of the object. There is no awareness of attributes during exposure; of that I am now sure. After exposure I can hold the 'object' in imagery or 'memory,' and by taking successively the hue, tint and chroma attitudes I can describe it. My reports of clearness are also post-exposure descriptions. The experience is recalled as vivid or obscure. I avoid inferring that, because the object has several degrees of cognitive clearness, it must therefore have several degrees of attributive clearness. Sometimes this seems to be the case, but many times it has seemed to me that a color cognized at low degree was for a brief duration as clear attributively as if cognized at a high degree. This is true in the present instance; all the impressions at the time of the exposure seemed equally clear" (150). Here we have one level of attributive clearness, and beside one addition at least five levels of cognition. F (60): "5 colored rectangles, purple, black, gray, blue, yellow; all on upper level, all equally clear during exposure. Grey the only one particularized, it was a medium light grey, seemed to me to have a slight lustre. If I had not tried to get all the colors by name I think I could have particularized the others." Here we have one level of clearness, and certainly two degrees of cognition. Again (71), "Gray, green, red, 'something,' blue. I named as fast as I could but could only name four. Green and blue came up easiest and were the ones particularized. Green rectangular, slightly yellow rather than blue, of fairly light hue and fair saturation. Blue, rectangular, was not dark, rich blue, but a medium tint and rather poor saturation. Grey and red rectangular, grey light, red just a name. 'Something' was a color, that is all that I can say. All processes during time of exposure at upper level." The processes were at one level of clearness, and yet the colored rectangles were cognized at four. D (8) "5 rectangles, knew the qualities of all immediately after the exposure, but before I had completed the naming—I started from the left—I lost my knowledge of what I had perceived. First was yellow, light tint, good chroma; second, purple, rich deep color;

memory after-image had disappeared before I had named the third, have a feeling that it was blue or bluish, but am certain that it was dark. Can say very little about the last two, except that the last one was the darker of the two. At the time of the exposure all the processes were equally clear, and attention was evenly distributed over the entire field." One level of clearness, and several degrees of cognition!

3. *Clearness Two Levels.*—When two levels of clearness were observed, the lower perceptual steps were never reported from the upper level; and conversely the highest perceptual steps were never reported from the lower level. Indeed, in the upper clearness levels the form was, with two exceptions for W, always perceived; whereas the perceptions with indefinite form were reported proportionally, and for W and D actually, more frequently from the lower level than from any other basis of report.

This result seemingly indicates, at least as far as form is concerned, that attributive clearness is one of the conditioning factors in the perception. We shall return later to a discussion of this point; but we should observe here that the condition, if such it be, is not absolute; for the perceptual steps, reported from the two levels, overlap. Processes of very different degrees of clearness may be similarly cognized. All the observers give instances of this overlapping. For illustration, W at the 11th perceptual step cognized 3 areas from the upper level of clearness and 74 areas from the lower level; at the twelfth step, 7 from the upper and 5 from the lower; at the thirteenth step, 17 from the upper and 5 from the lower; at the fourteenth, 15 from the upper and 3 from the lower; and at the sixteenth step, 179 areas from the upper level of clearness and 7 from the lower. The results of the other observers are corroborative. The following introspections are selected by way of illustration: W (104) "Orange, good chroma, medium to light tint, form definite, to left of center; blue good chroma, light, form definite, next to orange, nearer center; purple, fair chroma, medium tint, form definite, last to right; about three other colors. The three named seemed to stand out from the remaining colors as attributively more clear. I again think that there were two levels, and that the lower did not change during exposure and neither did the upper. The colors reported were apprehended but not named during exposure; they were described from memory after-image." F (7) "Purple at center; orange next to purple to left; yellow next to purple to right; other colors present. I could match the purple with a series of purples; it impressed itself upon me as a particular purple quality (hue, tint and chroma combined). I do not feel at all able to match the orange or the yellow; they seemed ordinary colors to me. The three colors reported seem to stand at a high level of clearness. Other colors and background all at lower level." D (24) "The three left-hand areas were very clear, the areas to the right were unclear. The qualities of the left-hand areas were red, light-blue, and black. The red was a rich red of medium tint; could pick it out from a series it was so definite. The light blue was also definite, light, but of good chroma. The black was jet. All three of these areas were rectangular and sharply outlined and distinct from the background. Of the two areas on the right I can report that the quality of the outside color was dark. Can report absolutely nothing about the fourth area. Neither of these two areas was distinctly defined from the back-ground."

3. *Clearness Three Levels.*—Besides the two types of clearness already mentioned, W reported a three-level type, *i.e.*, the processes during the exposure were at three levels of clearness: some were maximally clear, some were of medium clearness, and some were obscure. The upper and middle levels were cognized at approximately the same perceptual steps, that is, at the higher; whereas the processes of the third clearness level were cognized at the lower perceptual steps. We shall return later to a discussion of the multi-level type; here we note that W reported this type only 5 times, and those in the early part of the experiment.

Thus in sensory clearness differentiated from cognitive.[1]

[1] Though we prefer the name 'clearness,' we think on the whole for the sake of clarity and unambiguity that Titchener's recent substitution 'vividness' is preferable. The term 'clearness' may then be given over to cognition. *Cf.* E. B. Titchener, 'A Beginner's Psychology,' 1915, 66; *Psych. Review.*, XXIV., 1917, 55.

A comparison of the perceptual classification with the attributive classification (Table II.) reveals four alternatives: an impression may be attributively clear and cognitively clear; attributively clear and cognitively unclear; attributively unclear and cognitively clear; or attributively unclear and cognitively unclear.

So far nothing new has come from our study. We have, besides substantiating Britz's perceptual classification, merely verified what has long been known about the distribution of consciousness during a tachistoscopic exposure.[1] Yet our work shows that, even under the unfavorable conditions of Britz's experiment, attributive clearness can be realized. Indeed, if in the light of our results we return to Britz, we can find instances of attributive clearness; not to be sure in what his observers report as 'clearness,' for they used the term in another sense as the following plainly shows, "Bei unseren Versuchen wird der Ausdruck Klarheit lediglich in dem Sinne gebraucht, dass nach der Ansicht der Versuchperson eine bestimmte Farbennüance in ihrem charakteristischen Tone, ohne noch irgend einen Zweifel zuzulassen, erlebt wurde;"[2] nor in the sense in which Britz himself applied the term; but incidentally, in phenomena reported and neglected. For example, "Wir haben z.B. gesehen, dass eine Farbe, das Blau, nur als Qualität usw. erlebt wurde,—nach unserer Skala also in einem ziemlich niederem Stadium,—und trotzdem sehr hervorstechen kann."[3] This account with its process 'standing forth' is very suggestive of what we mean by sensory clearness, as also frequently is the observer's employment of 'Eindringlichkeit.' But Britz was

[1] E. B. Titchener, 'Feeling and Attention,' 1918, 238. F. Schumann, *Bericht des I. Kong. für exp. Psych.*, 1907, 170.

[2] *Op. cit.*, 69. Britz continues: "Nach meiner Ansicht is dies eine Bezeichnung, die man speziell nur dem spezifisch 'Farbigen' gegenüber zu benutzen pflegt, bei Grau and Schwarz wird sie wohl kaum gebraucht werden." This is in direct contradiction to the evidence on p. 60, where an illustration of his higher phases Britz quotes from 14 introspections. Clearness is mentioned in six: It is used twice in connection with green, and once in connection with red, yellow, gray and black. If these introspections are at all representative, clearness is used with black and gray as with the colors.

[3] *Op. cit.*, 61.

in search of a name instead of a phenomenon, and he consequently overlooked these leads which, had he followed them, might have brought fruitful results.

The aspect of clearness which our observers reported satisfies the requirements of an attribute: it is inseparable and independently variable. That these conditions are fulfilled is shown in the preceding tables. Whenever the experiment permitted of an attributive description our observers reported clearness. The Tables show a number of cases in which the observers failed to report clearness; but their failure does not mean that the criterion of inseparability has been violated; it signifies merely that, under the conditions of the experiment, an attributive description of the processes was occasionally not possible. We see from Table II. that processes of very different qualities have the same clearness; and contrariwise that processes of the same quality have very different clearnesses; and thus is the criterion of independent variability satisfied.

D. Conditions of Attributive Clearness

The dual-level reports, in which the clear and the obscure processes existed side by side in the same consciousness, offer an excellent opportunity for studying the conditions of clearness effective in this experiment.

1. *Position.*—The frequency in which the various parts of the exposure-card appeared at the upper level of clearness is different for all three of the observers. As is shown by Table V., in the greatest per cent. of W's dual-level reports (44 per cent.) the upper level of clearness is at the right end of the exposure card; whereas in 86 per cent. of F's reports it is at the center of the card; and in 55 per cent. of D's reports the upper level is at the left end of the card. W reports almost no cases (4 per cent.) in which the upper level is at the left end; F very few in which it is other than at the center; and D reports only 6 per cent. in which the upper level is at the right end. These data show that certain areas are favorable for clearness. There are, however, such great individual differences between the three observers—they

Table V

Showing the Number of Times, in Percentages, that the Upper Level, of the Dual-level Reports, Appeared at the Various Positions on the Exposure Card

O.	Position of Upper Clearness Level				Total Reports
	Left	Center	Right	Ends	
W....	4	38	44	14	98
F.....	7	86	7	0	29
D....	55	31	6	8	79

could hardly have been greater—that if we accept position as a determinant of clearness, as has recently been suggested by Bollman[1] we must regard it as a condition peculiar to every observer.

2. *Fixation.*—Two observers, F and D, reported fixation in a great proportion of the experiments. D reported it casually, while F used it as a means of reference, and therefore reported it in a greater per cent. of the cases. The number of times that the fixation was reported and the per cent. of times that it was reported at the left, at the center, and at the right, appears for the three kinds of reports—those in which clearness was not given, those in which it was given at one level, and those in which it was given at two levels—in Table VI. The differences in the fixation in the three kinds of report is too small to be of significance.

Table VI

Showing the Position of the Fixation Point in the Experiments in which No Attributive Clearness Was Reported, in Those in which the Attributive Clearness of the Procesess Aroused by the Stimulus Card Was at One Level, and in Those in which it Was at Two Levels

O.	Fixation	Attributive Clearness					
		Not Reported	Number Cases	One Level	Number Cases	Dual Level	Number Cases
F.......	Left.....			1.5%		4%	
	Center ..	100%	19	95.5%	134	92%	25
	Right...			3.0%		4%	
D.......	Left.....	11.2%		1.2%		2%	
	Center ..	88.8%	18	98.8%	83	96%	42
	Right...					2%	

"Number Cases" shows the number of cases in which the fixation was reported.

[1] Alma Bollman, *Am. J. of Psych.*, XXXI., 1920, 87–90.

The position of the fixation in the dual-level reports, however, appears to be significant. For, as is shown by Table VII., where a correlation is drawn between the fixation and the position of the upper level of clearness, fixation at the right or at the left insures for both observers, that that area will be clearly experienced. When the fixation is at the center, the correlation is not so great, though it is not much less for F. Still, F, shows exceptions which are more or less the rule with D.[1] In nearly half of the experiments in which the fixation is at the center, the upper level of clearness for D is at the left; in not quite a third of them the position of the upper clearness level corresponds with the point of fixation.

These facts indicate that fixation is one of the conditions

TABLE VII

CORRELATION OF THE POINT OF FIXATION WITH THE POSITION OF THE UPPER CLEARNESS LEVEL

O.	Fixation	Position of Upper Clearness Level				Number Cases
		Left	Center	Right	Ends	
F.....	Left.......	4%				25
	Center	4%	84%	4%		
	Right.....			4%		
D....	Left.......	2%				42
	Center....	41%	31%	12%	12%	
	Right.....			2%		

"Number of Cases" shows the number of dual-level reports in which the fixation was reported.

of clearness in this experiment. It is cut across in D's case by some other factor or factors, which almost totally obscure it. In F's case, on the contrary, the correlation is so high that fixation appears for him to be the sole condition necessary for clearness. Since D's data show the effect and presence of other conditions, it may also be that there are other conditions operative in F's case, and that their presence is concealed in his general results,—their action being in the same direction as the point of fixation. This, indeed, as we shall presently see, turns out to have been the case.

[1] These cases and the 'One Level' reports show that our observers were following the directions to spread the attention over the entire card and not to concentrate at the point of fixation.

3. *Order of Report.*—All three observers reported that they were consciously disposed toward a certain definite order of naming the colors. W was set to report the colors from right to left: (6) "I set myself to give equal attention to the field and to name the colors from right to left." D in a large per cent. of the cases, following the order in reading, began his reports from the left. The method, however, was the result of conscious adaptation: "Resolved, in order to get more of the colors, to name them in order from left to right." F, in the early experiments, began his reports as D by reproducing (2) "from left to right as in reading," but he very soon adopted what he called the natural method; "I find that there is a natural order of naming the colors; I don't at all name from left to right, but from the middle in both directions, usually first to the left and then to the right."

TABLE VIII

SHOWING THE ORDER OF NAMING THE COLORS IN THE EXPERIMENTS IN WHICH NO ATTRIBUTIVE CLEARNESS WAS REPORTED, IN THOSE EXPERIMENTS IN WHICH THE ATTRIBUTIVE CLEARNESS OF THE PROCESSES AROUSED BY THE STIMULUS WAS AT ONE LEVEL, AND IN THOSE IN WHICH IT WAS AT TWO LEVELS

O	Report from	Attribntive Clearness					
		Not Reported	Number Cases	One Level	Number Cases	Dual Level	Number Cases
W.......	Left.....	7.5%	80	5.8%	52	13.6%	81
	Center ..	27.5%		38.4%		50.6%	
	Right...	60.0%		44.2%		30.8%	
	Ends....	5.0%		11.6%		4.9%	
F.......	Left.....	30.4%	23	8.6%	150	17.7%	28
	Center ..	69.5%		89.4%		82.3%	
	Right...			2.0%			
	Ends....						
D.......	Left.....	63.6%	22	81.0%	132	54.5%	68
	Center ..	36.3%		13.7%		32.3%	
	Right...			0.8%		6.0%	
	Ends....			4.5%		7.3%	

"Number Cases" shows the number of cases in which the method of reporting was designated.

As is shown in Table VIII. the observers followed the method toward which they were 'set' pretty consistently throughout the experiment. F shows the greatest consistency, W the least. Indeed, W in more than 50 per cent. of the cases of the dual-level reports names the colors first that are at the center.

Table IX. shows the correlation between the method of naming the colors and the position of the upper level of clearness of the dual-level reports. The correlation is very high for all the observers. For example: when W named the colors first at the center, the upper level of clearness in the greatest per cent. of the cases was at the center; when he named first at the left, the upper level of clearness was in the greatest number of cases at the left; when he named the ends first, the ends in most cases were clear and the center obscure. So also for the other two observers.

TABLE IX

CORRELATION BETWEEN METHOD OF REPORTING AND POSITION OF UPPER LEVEL OF CLEARNESS OF THE DUAL-LEVEL REPORTS

O.	Report from	Position of Upper Clearness Level				Number Cases
		Left	Center	Right	Ends	
W....	Left.......	1.2%	2.4%	5.0%	5.0%	81
	Center....	1.2%	34.6%	14.8%		
	Right.....	2.4%	1.2%	21.0%	6.2%	
	Ends......			1.2%	3.7%	
F.....	Left.......	7.1%	7.1%	3.5%		28
	Center....		78.8%	3.5%		
	Right.....					
	Ends......					
D....	Left.......	50.0%	1.5%	1.5%	1.5%	68
	Center....	4.4%	26.4%	1.5%		
	Right.....		1.5%	3.0%	1.5%	
	Ends......			1.5%	5.8%	

"Number Cases" shows the number of cases of the Dual-Lever reports in which the Method of Reporting was designated.

These tables corroborate the observers' introspections and show, we believe, that the tendency or 'set' to name in a particular spatial order is a determinant of clearness. It might be argued that the order of reporting, instead of being a condition of clearness, is itself conditioned by clearness. That may be true under certain conditions. Here, however, we must regard the general tendency to name from certain fixed points, a tendency that our observers reported in their introspections and that was clearly shown in their results, as a determinant of clearness.

This determinant explains the results of the correlation of fixation and the position of the upper clearness-level. It accounts for the apparent discrepancy of D's and the uniformity of F's results. D, as we have found, was 'set' toward the left; his fixation was at the center. The one condition pulled one way, the other condition another way. Under such a conflict of determinants we should expect to find that now the one and now the other was effective; and unless there were other determinants which cut across these two, that the upper level of clearness would be at the one or the other of these two areas, and that it would most frequently be at the area of the stronger determinant. That represents the facts in D's case: the upper level of clearness is at the center and left in 5/6 of the cases; it is at the left more frequently than at the center; so that, if our logic is correct, the predetermination, the 'set' to name from the left, is for D in this experiment a stronger determinant of clearness than fixation. For F, the two conditions are effective in the same direction; they facilitate each other. F's fixation was at the center, and he was set to name first at the center. His results are consistent throughout; the clearest area in 86 per cent. of the cases was at the center. W unfortunately did not report fixation; his results, however, show the same discrepancies as D's, and are, we think, to be explained in the same manner.

4. *Quality.*—The qualities of the stimuli, irrespective of the quality reported by *O*, were noted and recorded according as they appeared in the upper and the lower levels of clearness. The number of cases and the per cent. for each level of clearness are given in Table X. for every quality.

This table shows that some of the qualities are more frequently experienced in the upper clearness-level than others, *i.e.*, some are clearer in their own right. Therefore, since every color appeared equally often, and an equal number of times at the various positions on the card, we conclude that quality is a determinant of clearness.

Table X

Reports of the Dual-Level Formation, showing the Number of Times and the Per Cent. that the Different Colors Fell in the Upper and the Lower Levels

O.	Level of Clearness	Quality									
		R_1	R_2	O	Y	G	B	V	P	Gy	Bk
W....	Upper......	18 36%	14 24%	26 49%	31 57%	27 50%	40 77%	20 33%	17 36%	17 30%	24 52%
	Lower......	32 64%	44 76%	27 51%	23 43%	27 50%	12 23%	41 67%	30 64%	40 70%	22 48%
F.....	Upper......	10 63%	5 36%	13 76%	14 82%	10 63%	12 80%	16 76%	11 79%	4 24%	9 50%
	Lower......	6 37%	9 64%	4 24%	3 18%	6 37%	3 20%	5 24%	3 21%	13 76%	9 50%
D....	Upper......	24 61%	13 27%	20 57%	33 80%	13 30%	42 87.5%	31 70%	22 63%	17 35%	33 72%
	Lower......	15 39%	34 73%	15 43%	8 20%	30 70%	6 12.5%	14 30%	13 37%	31 65%	13 28%

The rank-order of the colors according to the frequency in which they appeared in the upper level of clearness is given in Table XI. There is a fairly close agreement among

Table XI

Rank-Order of the Colors according to the Frequency in which they Appeared in the Upper Level of Clearness

O.	Rank Order									
	1	2	3	4	5	6	7	8	9	10
W........	*B*	*Y*	*Bk*	*G*	*O*	R_1	*P*	*V*	*Gy*	R_2
F........	*Y*	*B*	*P*	*O*	*V*	R_1	*G*	*Bk*	R_2	*Gy*
D........	*B*	*Y*	*Bk*	*V*	*P*	R_1	*O*	*Gy*	*G*	R_2

the observers as regards the colors that make for clearness and those that make of obscurity. *Y*[1] and *B* are most frequently in the upper level, whereas R_2 (the tint) and *Gy* are most frequently in the lower. For the other colors, which occupy intermediate positions, the individual differences are greater; only one is ranked uniformly by all the observers, the saturated red, R_1, which is in sixth place.[2]

[1] This corroborates Titchener's introspection (*cf.*, *Textbook of Psychology*, 269).

[2] Against the general belief that red is a quality that is inherently clear, both the saturated color and the tint were uniformly ranked low by our observers. The principle upon which this quality is used in danger signals must be of another origin.

This condition explains some of the irregularities which we have noted above. It explains, for example, the cases of W and D in which the ends were at the upper levels of clearness and the centers obscure; and also the cases in which F, though doubly conditioned for clearness at the center, reported the clearer area at the right or at the left.

5. *Length of Exposure.*—The dual-level formation is to a slight extent correlated with length of exposure. Evidence is given in Table XII. which shows that the greatest per cent. of the dual-level reports were obtained during the short exposures. This result holds for all three observers. It becomes more apparent when the exposures are arranged in three groups, as rapid, medium, and slow.

TABLE XII

THE PER CENT. OF THE DUAL-LEVEL REPORTS THAT OCCURRED AT THE VARIOUS EXPOSURE TIMES

O.	Length of Exposure					
	a	*b*	*c*	*d*	*e*	*f*
W....	18.4	20.6	17.3	16.3	14.2	13.2
F.....	20.7	24.1	13.7	17.3	13.9	10.3
D....	22.8	17.7	15.2	15.2	15.2	13.9

Though the length of exposure appears to be a condition of the dual-level reports, it is not a condition of clearness in those reports. The results presented in Table XIII. show no correlation between the position of the upper level of clearness and the exposure-time for W and F. D, however, gives a slight correlation; the center was more frequently reported clear during the short exposures, and the left more frequently during the medium and long exposures. Nevertheless, we do not regard this result as evidence of a determinant of clearness; rather it shows under what conditions the determinants already mentioned are effective for D. In the short exposures, his fixation is the more effective; in the medium and the long, his predisposition for the left is the stronger.

Table XIII

Correlation of the Position of the Upper Clearness Level of the Dual-Level Reports with the Time of Exposure

O.	Position of Upon Clearness Level	Exposure Time					
		a	*b*	*c*	*d*	*e*	*f*
W....	Left........	1%		1%		2%	
	Center.....	7%	8%	6%	5%	5%	7%
	Right......	7%	8%	8%	8%	7%	6%
	Ends.......	3%	4%	3%	3%	1%	
F....	Left........	3.4%		3.4%			
	Center.....	13.9%	24.1%	10.3%	13.9%	13.9%	10.3%
	Right......	3.4%			3.4%		
	Ends.......						
D....	Left........	8.8%	8.8%	10.1%	10.1%	10.1%	7.6%
	Center.....	10.1%	6.3%	2.5%	5.0%	3.8%	3.8%
	Right......	2.5%	2.5%	1.3%			
	Ends.......	1.3%		2.5%		1.3%	2.5%

6. *Complexity of Material.*—Complexity of material, as well as length of exposure, seems to condition the dual-level reports. As Table IV. above shows, 'one-level' reports are more frequently given with the cards of Series I than with those of Series II. Furthermore, the reports of the dual-level type are more frequently given for the cards of Series II than for those of Series I. Nevertheless, complexity of material does not seem to be a condition of clearness in the dual-level reports; for the positions of the upper clearness levels are very nearly the same for both types of material.

Summary.—From the facts presented it is apparent that the conditions of clearness in this experiment are position fixation, predisposition, and quality. The first one of these, position, can be explained in terms of the other three. Length of exposure and complexity of material are conditions of the dual-level reports, that is, of the range of the upper level, but are not conditions of clearness itself. If the exposure is very rapid, if the material is very complex, some processes will necessarily be obscure, and others, depending upon the conditions named above, will be clear.

E. Conditions of Cognition

In this experiment, beside the image of memory, the principal basis of the descriptions is the memory after-image. Evidence is furnished by the following introspections:

W (10) "Both of the colors persisted in and were described from the memory after-image"; (49) "Cognized after exposure from the memory after-image and the memory image"; (68) "I describe accurately and easily from memory after-image or from image of memory"; (150) "The colors are undoubtedly noted, apperceived, cognized, apprehended as objects, as 'reds,' 'blues,' etc.; there is no awareness of attributes during exposure. After exposure I can hold the 'object' in memory after-image or memory, and by taking successively the hue, tint, and chroma attitudes, I can describe it." (166) "I now feel fairly certain that cognition during exposure is either 'that is familiar' or 'I know what it is.' Both approximate to the direct apprehension which is the ultimate stage of recognition. I have reported many times that colors are never named until after exposure."

F (61) "More and more naming seems to me to be done after exposure and dependent upon the memory after-image to a great extent." (63) "Impressions persist for a brief period after exposure as positive or memory after-image and during that time I name them (aud.-kin. verbal) and write them down. Then I try to characterize or further describe them. During this time some individual colors come up in images of the sort I think Meumann calls 'primary memory.' Here some colors come up full and complete and with a feeling of familiarity and assurance. They look as they do in a true memory after-image. Others refuse to come up in image at all, or come up as 'habitual images' with a feeling of helpfulness or uncertainty"; (241) "Trying to see if I can tell whether the persistence of the impressions after exposure is a positive or a memory after-image, which I haven't been sure about. This time I turned my eyes away (towards the paper as quickly as I could after the exposure and before naming colors). It is hard to do; I wanted to keep looking in the original direction. Even after I looked away the colors seemed localized in the original position. My best judgment therefore is that the fundamental image, with the sensations the chief basis of my naming and describing, is a memory after-image"; (242) "No doubt about it, the colors stayed a long time in the original position when I turned my eyes away; it is a memory after-image."

D (9) "Named from the memory after-image"; (17) "The colors were named from memory after-image"; (19) "Named from memory, lost the memory after-image, have feeling of uncertainty about the color qualities named"; (31) "Never name the colors during the exposure, all that comes after. Immediately after the exposure I name verbally from memory after-image"; (63) "Immediately after exposure name (verbal-kinæsthetically) from memory after-image." (179) "During the exposure I merely receive the impressions; immediately after, I cognize the colors, mainly from memory after-image"; (235) "Whenever the memory after-images disappear I am unable to continue the naming, though I am still able to report from memory that there were a certain definite number of qualities that I didn't name."

Cognition is therefore dependent upon the clearness, strength, quality and duration of the memory images, that is, upon the conditions of impression and retention; and upon practice, direction of effort, order of report, ease and facility with which the verbal associations necessary for report arise; that is, upon the conditions of reproduction.

The conditions of impression which act as determinants of cognition in this experiment are:

1. *Clearness.*—As we have mentioned above (pp. 18 f.) and as is shown in Table II., attributive clearness is one of the conditioning factors in the perception. When the dual-level type of consciousness is reported, the processes at the upper level of clearness are cognized at a high perceptual step; and conversely the processes at the lower level are usually cognized at a low perceptual step. The statement regarding the lower level is qualified by 'usually' because the results show that obscurity is not always paralleled by a low degree of cognition. There are certain conditions, as we shall see, which cut across clearness, and which make for cognition even though the processes have been obscurely experienced.

2. *Position.*—The per cent. of times that hue was cognized at different parts of the exposure card is shown for every one

of the 240 experiments in Table XIV. No position for W has a great superiority; what advantage exists, however,

TABLE XIV

SHOWING THE PER CENT. OF TIMES THAT THE HUE OF THE VARIOUS POSITIONS OF THE EXPOSURE CARD WAS COGNIZED

O.	Position Cognized			
	Left	Center	Right	Ends
W	33%	25%	35%	7%
F	35%	54%	11%	
D	71%	22%	2%	5%

lies in favor of the right, the same area which we found in the dual-level reports to be most frequently in the upper clearness level. The central position for F and the position at the left for D possess as great an advantage for cognition as they proved to possess for clearness.

3. *Fixation.*—In confirmation of Britz[1] we found that fixation is a condition of cognition. F and D reported their fixations in 178 and 143 cases respectively. The number of times that the different areas were cognized with the various fixations is shown in Table XV. The results of F sustain

TABLE XV

CORRELATION OF AREAS COGNIZED WITH THE POINT OF FIXATION

O.	Fixation	Position Cognized				Number Cases
		Left	Center	Right	Ends	
F	Left	1.8%				
	Center	28.1%	58.0%	9.5%		178
	Right	.6%		1.8%		
D	Left	2.8%				
	Center	61.6%	26.5%	2.0%	5.5%	143
	Right	.7%				

"Number Cases" shows the number in which the fixation was reported.

Britz: fixation at the left is paralleled by cognition at the left; at the center, by cognition at the center; and at the, right, by cognition at the right. The results of D, however

[1] *Op. cit.*, 63. We also add that the exposure field lay well within the zone of clearest vision. Physiological factors, therefore, need not here be considered.

Table XVI

Qualities Reported for the Various Stimuli

Observer W.

Qualities Reported	W. Color of Stimulus: R1	R2	O	Y	G	B	V	P	Gy	Bk	Total
Red 1	52	8	17				1	7	2		87
Red 2	1	15									16
Orange	1	6	58	1							66
Yellow		1	3	97	2						103
Yellowish		2									
Tan											
Brown											
Yellow green											
Green	1	1			70			1			73
Blue green											
Blue						92	30	6	4	2	134
Light blue						1	2		1		
Violet			1			5	25				31
Purple	5	12	2	1			1	50		1	72
Gray		1							20		21
Light		1									
Dark	1					1			1	2	
Black	1								11	72	84
No quality reported	70	85	51	33	60	33	73	68	93	55	621
Times correctly cognized	52	15	58	97	70	92	25	50	20	72	551
Times incorrectly cognized	10	32	23	2	2	7	34	14	19	5	148

Observer F.

Qualities Reported	F. Color of Stimulus: R1	R2	O	Y	G	B	V	P	Gy	Bk	Total
Red 1	75	14	18			3	1	10	2		123
Red 2		10									10
Orange	5	11	73		1			3	2		95
Yellow	2	4	7	110	2		2	1		1	129
Yellowish		1									
Tan											
Brown											
Yellow green			1	4	4						
Green	1	2	2		85			2	1		93
Blue green							2	1			
Blue	4	4	5		4	98	80	11	18	6	230
Light blue				1							
Violet	1		2		1		3	7	1	3	18
Purple	9	52	1	1	2	2	1	59	3		130
Gray	3	4	1		1	1	3	1	65	4	83
Light			1	3			1				
Dark	1				1	3	1	3	3	1	
Black	3		1			2		3	3	90	102
No quality reported	28	30	20	13	31	23	36	31	34	27	273
Times correctly cognized	75	10	73	110	85	98	3	59	65	90	668
Times incorrectly cognized	29	92	39	9	16	11	93	42	33	15	379

Observer D.

Qualities Reported	D. Color of Stimulus: R1	R2	O	Y	G	B	V	P	Gy	Bk	Total
Red 1	63	7	13	1	17		3	9	9	2	114
Red 2		25	1		5		2	3			36
Orange	2	7	38	1	17			3	4		72
Yellow			30	112	6						148
Yellowish	1	2	5		2						
Tan	1	3									
Brown	2	2		1				2	1	5	
Yellow green	1		2								
Green	1	4	1		8						14
Blue green											
Blue					1	111	51	16	3	9	191
Light blue						3	36	7	3		
Violet							1				1
Purple	6	2			3	2	2	25	10		50
Gray	2	5			1		2	2	21		33
Light		6	2		6		2	1	5		
Dark	2	2	1			6	3	7	5	4	
Black	2							11		97	110
No quality reported	49	67	39	12	66	10	30	46	70	15	404
Times correctly cognized	63	25	38	112	8	111	1	25	21	97	501
Times incorrectly cognized	20	40	55	8	58	11	101	61	41	20	415

point less obviously to this conclusion. He seems, no matter what the fixation, to be predisposed toward the left. Nevertheless we regard his results as confirmatory. In view of his disposition, a condition which we shall later consider, the only explanation for the large per cent. cognized at the center is fixation.

4. *Quality.*—The qualities reported for the various stimuli are shown in detail for every observer in Table XVI. The stimuli are placed at the head of the table. The figures below, which appear opposite the colors listed at the left under the caption "Quality reported," indicate the number of times the stimuli were cognized as of those qualities. The figures in *italics* show the number of times the stimuli were correctly cognized. Opposite the legend "No quality reported" appears the number of times that the stimuli were not cognized. At the foot of the table a summary is made of

TABLE XVII

RANK-ORDER OF THE COLORS CLASSIFIED ACCORDING TO THE FREQUENCY THAT THEY WERE COGNIZED, THAT THEY WERE CORRECTLY COGNIZED, AND THAT THEY WERE INCORRECTLY COGNIZED

O.	Rank Order									
	1	2	3	4	5	6	7	8	9	10
W........	*Y*	*B*	*Bk*	*G*	R_1	*O*	*Gy*	*P*	R_2	*V*
F........	*Y*	*B*	*Bk*	*G*	*O*	R_1	*P*	*V*	*Gy*	R_2
D........	*Y*	*B*	*Bk*	R_1	*O*	R_2	*P*	*Gy*	*G*	*V*

the number of times that the stimuli were correctly and incorrectly cognized. For example, W cognized the saturated red stimulus, R_1, 52 times as red, once as light red, once as orange, once as green, 5 times as purple, once as dark, and once as black; 70 times he failed to cognize it; 52 times he cognized it correctly; and 10 times he cognized it incorrectly. An examination of this Table reveals that some stimuli were cognized more frequently than others (as measured inversely by the number of times that no quality was reported); that some were cognized correctly more often than others; and that some were cognized incorrectly more often than others.

The rank-order of the colors, figured upon this threefold basis, is given in Table XVII.

This table clearly shows that quality is a condition of cognition. Some qualities, *Y*, *B*, and *Bk*, are uniformly ranked at the head of the list, while others, *V*, *Gy*, and *P*, are as consistently at the lower end.

The conditions that we have thus far considered are, we believe, effective for cognition only in so far as they are effective for clearness. We now return to conditions which are effective for cognition alone.

5. *Length of Exposure.*—As is shown by the data of table XVIII., fewer colors were named after the short exposures than after the exposures of medium length, and fewer were named after the exposures of medium length than after the exposures of the longer durations. The difference is small, but is constant for all the observers. We conclude that cognition is conditioned by, and varies directly as, the length of the exposure.

TABLE XVIII

AVERAGE NUMBER OF 'COLORS' COGNIZED AT EVERY EXPOSURE TIME

O.	Exposure Time						
	a	*b*	*c*	*d*	*e*	*f*	Total
W......	2.40±.54	2.60±.49	2.95±.28	3.00±.45	3.20±.59	3.15±.65	2.9±.52
F......	4.17±.66	4.27±.65	4.45±.68	4.50±.65	4.62±.72	4.52±.70	4.4±.69
D......	3.20±.84	3.45±.87	3.62±.71	3.70±.64	3.80±.76	3.90±.76	3.6±.82

6. *Color Sensitivity.*—Observers W and F have normal color-vision, and their records do not reveal any anomalies of color cognition. The reports of D, who was known beforehand to be partially color-blind, show marked irregularities of this kind. D seems, as shown in Tables XVI and XVII, to be incapable of cognizing green. He fails to perceive a quality, when the green stimulus is exposed, in exactly half of the cases, 66 out of 132; and when the stimulus is perceived, it is cognized as red or orange in nearly two thirds of the cases.[1]

The conditions affecting retention which act as determinants of cognition are:

[1] D cognized the red stimuli correctly more frequently than did either of the other observers, and his errors were relatively not greater.

7. *Complexity of Material.*—The number of times that one, two, three, four, five, and (for cards of Series II) six colors were cognized appears below in Table XIX.

TABLE XIX

NUMBER OF QUALITIES COGNIZED PER EXPOSURE CARD FOR THE TWO SERIES OF EXPOSURE MATERIAL

O.	Series	Number Cognized						Average and M. V.
		1	2	3	4	5	6	
W....	I........	1	31	68	16	4		2.9±.51
	II........	2	31	65	21	1	0	2.9±.53
F.....	I........	0	1	7	25	87		4.6±.48
	II........	0	4	27	52	30	7	4.1±.72
D....	I........	2	7	41	44	26		3.7±.77
	II........	1	16	36	54	13	0	3.5±.76

An inspection of these data reveals that complexity of material had but little if any effect upon W. His range of cognition in both series of exposure cards was about 3 colors, and the distribution of his reports was approximately the same in both series. What little difference exists, however, as is shown by the m.v., lies in favor of the simpler materials. This relation is more clearly shown in the results of F and D. These observers report the higher ranges more frequently and on the average cognize more colors when the cards of Series I. were used. Greater opportunity for cognition is more than outweighed by the disadvantage of greater complexity.

F's introspections bear out his results: (155) "The big outstanding difference between experiments is 5 colors *vs.* more than 5; easy *vs.* hard colors to name"; (64) "When 6 or more colors are exposed there is a 'blank' immediately after exposure. I do not start to name them nearly so quickly as when only 5 are exposed. It is as if I were helpless or had to set myself a new task, whereas in the exposure of 5 colors I go straight ahead naming after exposure. The memory after-image of 5 colors seems to take care of itself (usually all 5 persist), whereas with 6 or more colors only a few appear in memory after-image."

(8) *Duration of the Memory Image.*—The longer the duration of the memory images, the more colors were cognized, and the more complete were the qualitative descriptions. The duration is dependent upon the conditions which we have just considered and also, as was frequently reported by F and D, upon a condition which very much resembles retroactive inhibition. Immediate activity tends to destroy the memory images.

F (48) "Naming is not an absolutely immediate process but one which interferes with the memory images"; (49) "Five colors only, all on upper level; naming them drove the memory after-image out of mind, so I cannot give a description of them"; (76) "Naming drove the other colors out of mind as colors"; (100) "About 7 colors; the trouble was that I couldn't name them fast enough and that naming some drove others out of mind."

D (8) "Knew all the qualities immediately after the exposure. If I could have set them down with one stroke of the pen could, I am quite sure, have reproduced them all. I started to name from the left, and before I had reported the third I had lost memory of the series"; (13) "Turned immediately after the exposure to report. This activity dispersed the memory images"; (113) "Could name all the colors immediately after exposure, they were all familiar, but when I turned to set them down in my report lost my knowledge of them."

The conditions of reproduction which act as determinants of cognition are:

(9) *Practice.*—For the purpose of discovering the effect of practice the experiment was divided into thirds, and the average number of colors named by the observers was calculated for every one of these periods. The results are shown in Table XX. After an initial improvement there is an actual decrease in the average number of colors named. Both the improvement and the decrease, as the experiments upon apprehension have shown, may be explained by habituation to the experimental requirements, by the development of tricks and schemes of grouping and reporting, by ennui and

lack of interest. In so far as these factors are inherent in practice, practice is a condition of cognition.

TABLE XX

AVERAGE NUMBER OF COLORS REPORTED FOR THE THREE PERIODS OF THE EXPERIMENT

O.	Division of the Experiment		
	1	2	3
W	2.80±.60	3.01±.39	2.92±.50
F	4.11±.73	4.72±.68	4.26±.63
D	3.52±.74	4.00±.71	3.91±.75

(10) *Direction of Effort.*—When the observers make an effort to recognize and name the colors the task is more adequately performed, more colors are named, and higher perceptual steps are reported than when he 'sets' himself to the description of process. Following are examples:

F (5) "I was set for color." (All the colors were named, 5.) (33) "Set myself hard to get all the colors I could." (All colors named, 6.) (242) "Set myself to name and describe all the colors I could, trusting to luck for introspection. I can't report adequately on clearness with this set." (All the colors named, 5.) (33) "Didn't set myself for as many as possible this time, having realized that I am not quite obeying directions when I put so much emphasis on naming, so set myself to follow directions." (Four colors named, Series I.) (241) "Gave my full attention to the report of process." (Two colors named, Series II.)

W (54) "I find that I am trying to name more colors." (Named three, Series II.) (146) "I was set to name, and the names were touched off by the exposure." (Named four colors, Series I.) (93) "Reported the clearness and duration of the processes, then turned to name the colors and could name but two." (A card of Series II employed.)

D (63) "Set for hue." (Named five colors, Series I.) (49) "Resolved before the experiment to get all the colors, was determined." (Named them all, Series I.) (30) "Able to name only two qualities." (Began his report with a description of process; a card from Series II. employed.)

(11) *Order of Report.*—As has been mentioned, the effect of fixation was almost obscured in D's case by his disposition to name the colors from left to right as in the manner of reading. This tendency would, of course, place the right end and a part of the middle at a disadvantage. F's disposition to report from the center, first from the center left, and then from the center right, would place the center and the left at a great advantage and impair the cognition of the right end. W was disposed to name from the right to the left, and in most of the experiments he did so; but, as is shown above in Table IX., he began his reports in the greater per cent. of cases from two points: from the right end in which case the right would possess an advantage; and from the center, in which case, since his range of cognition was about three (Table XVIII.), the left would possess an advantage. Since the center in both cases would not suffer the same impairment as the opposite end, we should expect to find the various areas of the exposure card about equally well cognized.

That a disposition toward a definite order of report is a condition of cognition is shown by our results; for the positions cognized (Table XIV.) closely correspond to these predictions. Britz also noticed this condition.[1]

12. *Association.*—Cognition is dependent also upon the freedom and ease with which associations arise. Not only do the objective results give evidence of this effect, but one of the observers also corroborates it in his reports: "Cognition depends very much upon the ease of naming, *i.e.*, the facility of the association of verbal images with the sensed qualities" (D 17). At times the associations facilitate one another and the naming goes easily. Examples are:

W (186) "Easily named, cognized during exposure as familiar."

F (147) "The names came easily."

D (8) "Familiar colors, knew all of them at exposure."

At other times the associations inhibit one another, the observers suffer a kind of mental block, something akin to

[1] *Op. cit.*, 63.

terminal inhibition, and the processes of cognition are interrupted. The reports are affected by this in various ways:

1. Only those qualities are reported that have been cognized up to the moment of 'block'; examples are:

W (68) (Named two colors, was inhibited by the third) "Hesitated at pink, I think the struggle for this name drove the others out of memory."

F (96) "Gray, red, ——, disturbed by trying to name color to left of red, couldn't get name."

D (147) "Named the colors easily until I came to third, which I cognized as white. Considerably disturbed, for I knew there wasn't such a quality in the series; feeling of surprise and bewilderment which persisted and interfered with the cognition of the rest of the series."

2. The habitual order of naming is changed; examples are:

F (144) *O* began his report in his usual manner, naming from center to left, met a 'difficult' color, and then without hesitation proceeded to the cognition of the colors to the right; "Blue at center, some color which I can't name at left, reddish orange to right of center, and yellow to right of it."

D (100) "The qualities at 1 and 2 'difficult,' so named in reverse order from right, for I have learned if I hesitate in the naming I shall name but few."

3. The meaning is carried in other terms and the colors are correctly named in the report; examples are:

W (57) "The violet was noted first and called *B*, renamed in report as violet"; (88) "Both *O* and *P* were first cognized as red, correction made in report. The cognitive confusion is very hard to describe; I cannot say how the meaning of *R* carried the *O* character of the *O* impression; there was a feel of "not red" and this feel attached itself also to the *P*. I think there is also a feel of strangeness about the *P*, as if to say 'You can't be *R*,' then 'purple' came verbally with feeling of relaxation."

D (93) "Second color salmon, I carried it by word 'something'"; (104) "Named 'red' but all the time meant *P*, that is, I gave one name and meant another. The wrong name seemed to carry the meaning as well as the correct, for when I came to report I made correction without hesitation"; (156) "The meaning of O and P was carried in an organic glow"; (170) "Coudn't get name for quality at 1, called it 'blue' and went on. This *plus* a mass of organics gave me O for the report"; (233) (Named all five of the colors.) "Did not hesitate over the ones I coudln't immediately name, called them 'um' and passed on as: 'um-black-um-blue-yellow." (In the report the 'ums' were recorded respectively as *R* and *O*.)

4. The number cognized is curtailed; examples are:

F (84) "All five of the colors on upper level, but names didn't come quickly enough, and besides I hesitated over V (which was second one named), so there was nothing to name when I came to end of series." (Named three colors of card of Series I.) (168) "Delayed too long on naming P to get the rest well"; (174) "Words wouldn't come as I tried to name, and I lost memory of most of the colors before I succeeded in getting names."

5. The quality that causes the inhibition is omitted; examples are:

F (127) "The first color did not name easily so I skipped it." D (9) "Couldn't cognize 2, so skipped it and continued naming"; (77) "Couldn't get a name for 4, so passed on to 5, so as not to lose it by hesitating over 4."

Summary.—The conditions of cognition which obtain in this experiment are clearness, and all the determinants of clearness—position, fixation, quality and order of report; length of exposure; color sensitivity; complexity of the material; the duration of the memory image; practice; direction of effort; and the facility of verbal association. These conditions are effective in the impression, the retention, or the reproduction.

F. Corollaries

1. *Levels of Attention*.—Our first corollary is a confirmation of the dual-level formation of the attentive consciousness. F and D reported the two-level formation, a clear focus and an obscure background, throughout. Their reports merely corroborate the findings in other experiments regarding the

dual-level type.[1] W, however, reported the multilevel formation, and we at first thought that we had in his case confirmatory evidence of the existence of this type. He reported the formation 5 times, 3 times within the first 12 experiments, the fourth time in the 29th, and the last time in the 118th experiment. The first and second times he expresses no uncertainty about the report: (3) "Describing from right to left, green, yellowish, good chroma, medium tint; blue, orange, can't describe them further; others there but I cannot name them. Green was most clear, at upper level; blue and orange next, of medium clearness; others obscure, at lower level." The third time he was uncertain": . . . the others seemed to be at a third level of clearness. I say 'seemed to be' for I am not sure, but think the memory after-image and report cut across the experience." The fourth time, in the 29th experiment, he was even more uncertain: "Whether they are levels in cognition or clearness I cannot say; I have as yet found no criteria for judgment." By the 118th experiment he had gained these criteria, and was able to say that the multilevel formation that he had reported did not mark sensory levels of clearness but degrees of cognition. We quote this report in full: "Green, yellowish, good chroma, medium to light tint. Orange, reddish, good chroma, medium tint. A number of others, perhaps 4, some of them I am sure were colored. Green first caught attention, then the orange. Some of the others were clear, but I do not know what they were. Still others were there and they seemed to be at a still lower level. There thus seemed to be three groups, (1) those I named, (2) those I could almost name, (3) those that were simply there as meaningless impressions. I cannot say even whether they were 'bunte Farben' or greys. My impression is that (1) and (2) were about equal in attributive clearness, on the upper level, and (3) much lower in clearness, on the lower level. This is what I have before reported as the multilevel type." Though similar occasions again occurred in which the cognitive degrees stood thus apart, he

[1] K. M. Dallenbach, *Am. J. of Psych.*, XXIV., 1913, 506–7. E. E. Cassel and K. M. Dallenbach, *ibid.*, XXIX., 1918, 207.

thereafter reported nothing but the dual-level type. If then our study had ended with the 117th experiment, we should have concluded that W belongs to the multilevel type. His subsequent analysis and reduction show that he is, in fact, of the dual-level type, and lead us to raise anew the question whether there are ever more than two degrees of clearness observable in the processes of a given consciousness.[1]

2. *Minor differences of sensory clearness* were very frequently reported by our observers in the upper level, but were not noted in the lower. The following introspections are typical. W (141) "Field as a whole clear. The black and blue stood out momentarily above the others. This is the 'wrinkle' pattern." W (142) "Green caught attention

[1] Three cases of the multilevel type have been reported in the experimental literature. We proceed to examine them. (1) W. Wirth (*Phil. Stud.*, XX., 1902, 493 f.) found under certain conditions that there were more than two levels of clearness in his own consciousness. He maintains that at any moment of attention all possible degrees of apperception may be represented. A reexamination of Wirth's work, in the light of our results, shows unmistakably that he was judging and reporting cognitive clearness. All possible degrees of cognitive clearness, to be sure, may exist in a given consciousness. (2), (3) L. R. Geissler, in his work on the measurement of attention (*Am. J. of Psych.*, XX., 1909, 527 f.), reports two cases of the multilevel type. As he himself recognizes and points out (p. 527), his experiments dealt not with a simultaneous but with a successive consciousness. They lasted, for his observer B, from 15 to 22 seconds (p. 518), and for his observer C, 12 seconds (p. 517). Consequently the reports may have been either temporal accounts or, if viewed as a whole, general averages. The introspections that Geissler offers in confirmation of this type are so abridged that it is difficult to reconstruct the consciousness that lay behind them. It is to be regretted that he does not quote in full the introspective replies to the questionary (p. 519) answered at the end of every experiment. The answer concerning the temporal relations would have an important bearing upon this subject. It is a fair inference from the few passages quoted that observer B was giving a temporal account of the clearness variations; for in none of the examples does B say that all the levels described occurred simultaneously. C obviously reported general averages. Whether the levels that she mentions occurred simultaneously or successively she does not say; but the point in her case is irrelevant; for it is plain from her account of her criteria of clearness that she was judging cognitive clearness throughout. She says that her judgment of clearness "is a matter of clearness in memory; if the circles stand out plainly in memory I put them high in the scale, if not I put them low" (p. 521). Again, B observed in 30 experiments, and C in 130. Geissler does not state how many reports of the multilevel type each observer gave, nor how they were distributed throughout the total number: whether they were more frequent in the early experiments than in the late, were grouped toward the end of the series, or were more or less evenly distributed. These considerations, as our work with W shows, are of great importance. The existence of the type is not proved by Geissler's evidence.

and held it throughout, the orange certainly was as clear, but do not think that the purple and yellow although in the same level were as clear. The others were at a very low level." F (140) "Five colors, . . . all on upper level, but last two not so clear as first three." (9) "Five impressions, three center rectangles with long axis vertical, blue, red, green, . . . cannot name the other two. Impressions of blue were clearest, but all named on upper level; others obscure."

These results corroborate Titchener.[1] He observed the clearness 'wrinkles' in the processes at the upper level, and likewise failed to note them in the lower. There is, of course, no reason *a priori* why there should not be differences of clearness, or rather of obscurity, at the lower level as well as of clearness at the upper levél; the range and number of degrees of cognitive clearness in the lower level (of the dual-level reports, Table II.) far exceed the range and number in the upper level. It may possibly be, as Titchener says, that under very favorable conditions these variations would be observed.[2] Under our conditions, however, they were not found. So we conclude with Titchener, "that—whatever the case at the lower level—there are noticeable differences of clearness in the processes at the upper level of consciousness." [3]

3. *Range of Attention.*—In experiments on the range of attention, range has always been determined by the number of objects—lines, letters, words, numbers, figures etc.—which can be named or reproduced after a momentary exposure; that is, by the number cognized. This procedure is justifiable on the assumption that all the impressions which appear at the upper level of clearness are cognized; and, conversely, that all the impressions cognized appear at the upper level. But our study has shown that neither of these assumptions are correct. All the attributively clear impressions were not cognized (note the number of times the impressions were at one level, Table IV., and the number of times 5 and 6 colors

[1] 'Feeling and Attention,' 1908, 230–242; *Psych. Review*, XXIV., 1917, 57.

[2] 'Feeling and Attention,' 232.

[3] *Op. cit.*, 238.

were cognized, Table XIX.); nor were the impressions which were cognized, since cognition is dependent upon many conditions besides attributive clearness, always in the upper level of clearness. Indeed, as we have shown above (Tables II. and III.), obscure processes were not infrequently cognized.

These experiments upon the range of attention therefore give us merely a range of cognition, and the statements in current textbooks that the 'grasp of visual attention covers from four to six simultaneously presented simple impressions' is a statement concerning visual apprehension, not attention.

IV. Summary of Results

The results of this study may be summarized as follows:

1. Britz's perceptual steps have been verified and his classification has been extended.

2. The complexity of the problem and the inherent difficulties of the method, which required both a report of meaning and an exhaustive description of process, are offered as a possible explanation of Britz's failure to obtain data regarding attributive clearness.

3. Our introspections have been classified upon the basis of sensory clearness. Four classes were obtained: (1) a group in which clearness was not mentioned; it was not under the conditions of the experiment reportable; (2) a group in which one level, the upper was reported; (3) a group in which two levels were reported; and (4) a group in which three levels were reported.

4. On the basis of the observers' reports the distinction between attributive and cognitive clearness has been confirmed: an impression may be attributively clear and cognitvely clear; attributively clear and cognitively unclear; attributively unclear and cognitively clear; attributively unclear and cognitively unclear.

5. The conditions of clearness which obtain in this experiment are: position, fixation, order of report, and quality. Length of exposure and complexity of material affect the range of clearness, but do condition clearness itself.

6. The conditions of cognition are clearness and all the

conditions which make for clearness: position, fixation, order of report, and quality; length of exposure, color sensitivity, complexity of material, duration of memory image, practice, direction of effort, and facility of verbal association.

7. There are never more than two degrees of clearness observable in the processes of a given consciousness.

8. Minor differences of clearness, or 'wrinkles,' exist in the upper clearness-level.

9. The experiments hitherto made upon 'range of attention' are perceptual experiments, in which the end-result is the range, not of attention, but of cognition.

CURVES OF GROWTH OF INTELLIGENCE

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Up to the present time most investigations in the use of mental tests have been directed toward the practical value of such tests from the point of view of prognosticating the future behavior of individuals. Because of the comparative recency of the work in mental testing, there has been little opportunity to make systematic measurements of the intelligence of a number of individuals over a period of years. This type of research will undoubtedly give us the best knowledge of the growth of intelligence. In the meantime, however, we may endeavor to gain some knowledge of the growth curve from an examination of group tests given to a large number of children of various ages.

Part I

The purpose of the results here presented is to suggest a few possibilities of individual prognosis to be expected as soon as group tests can be refined to the point of being as accurate as the individual examinations used in the past. Simplification of methods of administering, scoring and evaluating tests to the point where any school teacher of average intelligence can accurately give, score, and statistically evaluate mental and educational group tests, will be a big advance in the direction of securing the masses of data so necessary for determining group norms.

Our results are based on the median mental ages of 1,987 children, tested on a series of six group tests.[1] Chronological-age-mental-age distributions for all school ages from six to

[1] Pintner, R., 'The Mental Survey,' New York, 1917, 116 pp.

thirteen were obtained. To secure additional accuracy, the mental ages were grouped in classes of one half year mental age. This distribution table is too extensive to print here and is also unnecessary for the discussion. The percentages

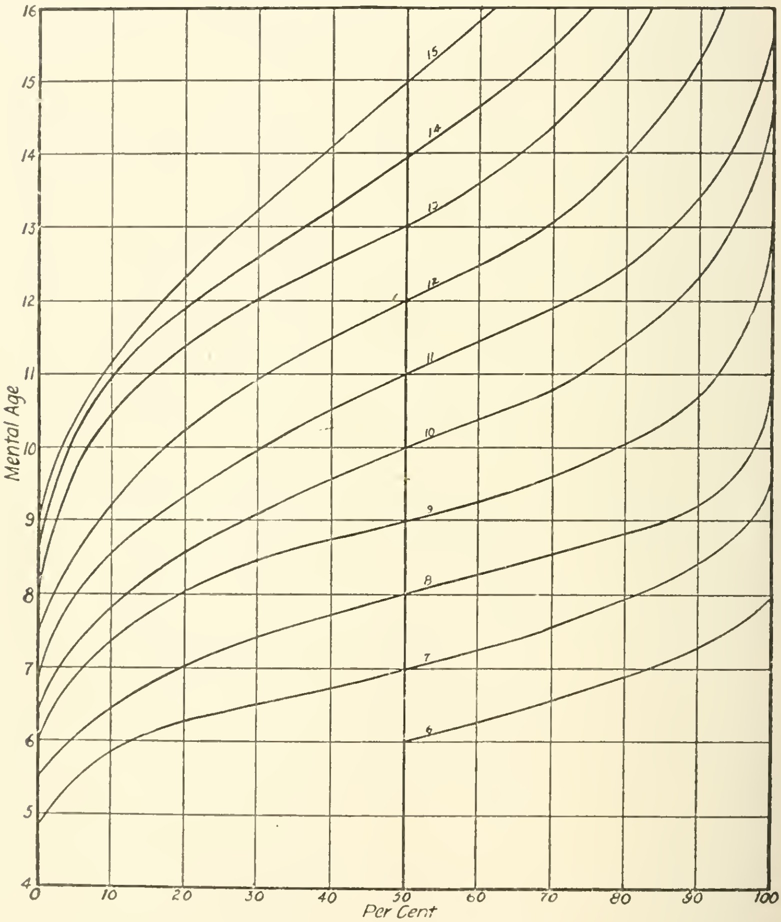

Fig. 1. Distribution of Mental Ages, by Chronological Age and by Per Cent.

derived from the table were turned into a cumulative distribution by chronological age and by mental age with the result as shown in Fig. 1. Very small irregularities have been ironed out of these curves by mechanically smoothing the curves in plotting. Each curve of Fig. 1 represents a

Table I

Chronological Age Distributions of the Mental Age Limit reached by the Designated Percentile Person of each Chronological Age Group

(Disregarding the sampling involved, and assuming constancy of percentile rank of the individual, the figures in the body of the table are ordinates for individual curves of growth of intelligence, as indicated.)

Percentile Person	Mental Age at the Chronological Age:									
	6	7	8	9	10	11	12	13	14	15[1]
Lowest....	—	4.8	5.5	6.0	6.45	6.8	7.5	8.0	8.65	8.95
5......	—	5.45	6.05	6.9	7.3	8.0	8.55	9.70	10.15	10.45
10......	—	5.85	6.45	7.35	7.8	8.55	9.2	10.45	10.95	11.15
15......	—	6.1	6.75	7.75	8.25	8.95	9.8	10.95	11.5	11.8
20......	—	6.25	7.0	8.05	8.55	9.35	10.25	11.35	11.85	12.3
25......	—	6.4	7.25	8.3	8.85	9.7	10.6	11.7	12.25	12.8
30......	—	6.5	7.4	8.45	9.1	10.0	10.9	12.0	12.6	13.25
35......	—	6.6	7.55	8.65	9.3	10.25	11.2	12.3	12.9	13.7
40......	—	6.7	7.7	8.7	9.55	10.5	11.45	12.55	13.25	14.1
45......	—	6.85	7.9	8.85	9.75	10.8	11.75	12.8	13.6	14.55
50......	6.0	7.0	8.0	9.0	10.0	11.0	12.0	13.0	14.0	15.0
55......	6.15	7.1	8.15	9.15	10.2	11.25	12.25	13.3	14.3	15.4
60......	6.25	7.25	8.35	9.3	10.35	11.45	12.5	13.6	14.6	15.8
65......	6.45	7.4	8.4	9.45	10.55	11.7	12.75	14.0	15.05	—
70......	6.55	7.55	8.55	9.6	10.8	11.9	13.05	14.4	15.5	—
75......	6.7	7.75	8.65	9.8	11.1	12.15	13.45	14.9	16.0	—
80......	6.9	7.95	8.80	10.55	11.4	12.45	14.0	15.45	—	—
85......	7.1	8.15	8.95	10.3	11.85	12.9	14.55	—	—	—
90......	7.3	8.4	9.20	10.7	12.35	13.4	15.3	—	—	—
95......	7.6	8.8	9.65	11.4	13.15	14.2	—	—	—	—
Highest...	8.0	9.6	10.9	13.0	14.6	15.7	—	—	—	—

cumulative percentage distribution (ogive) of the mental age of all the cases of the chronological age designated on each curve. This chart was drawn to a very large scale, enabling accurate graphical interpolations to be made. The points of intersection of each chronological age curve and the respective percentage ordinates of Fig. 1 were thus graphically determined. Thus from the figure we can find the mental age equivalent of any percentile of any of the chronological ages. For example, the forty percentile eight year old has a mental age of 7.7, and so forth. The points of intersection, graphically determined, for every fifth percentile of all chronological ages are shown in Table I.

From Table I, we may plot directly percentile curves of growth of intelligence, chronological age being used as abscis-

[1] Age 16 not included. At most percentiles their M.A.'s are not as high as the 15-year-olds, due to mental retardation. They are also one to two years or more retarded in school.

sae, and mental age as ordinates. The result of such plotting is shown in Fig. 2, the circles representing plotting points. Only each ten-percentile curve is here shown. The plotting points are given for only five of the curves to prevent confusion.

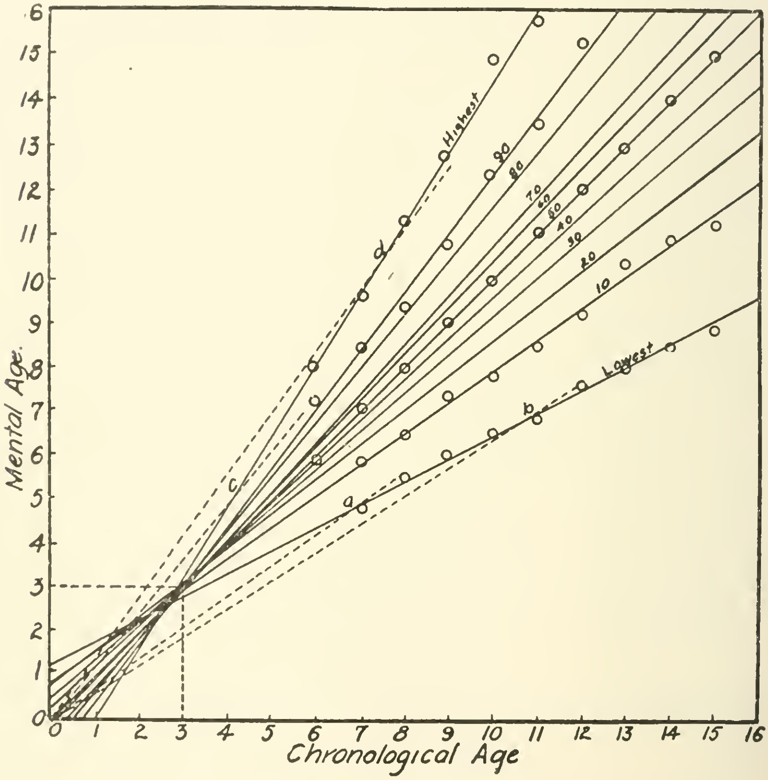

Fig. 2. Plotted Least Square Equations of Percentile Curves of Growth.

$$\text{Now the equation I.Q.} = \frac{\text{Mental Age}}{\text{Chronological Age}} \qquad (1)$$

may be written

$$M = \text{I.C.}, \qquad (2)$$

where M = mental age; I = I.Q.; and C = chronological age. This equation is a straight line. By the method of *least squares* the straight line equations of best fit of the actual percentile distributions have been calculated for each 10-

percentile and have been plotted on Fig. 2. The equations of these lines are shown in Table II. With the tests used, and with the distributions shown in Fig. 2, greater constancy of I.Q. on the whole would result from using the formula,

$$I = \frac{M - 3}{C - 3} \tag{3}$$

since the straight lines are seen to intersect at approximately $C = 3$, $M = 3$.

TABLE II

EQUATIONS OF LEAST SQUARE STRAIGHT LINES OF BEST FIT OF DATA OF TABLE I ($M = I \cdot C + k$)

Percentile Curve	Straight Line Equation
Highest	$M = 1.589C - 1.536$
90	$M = 1.327C - .991$
80	$M = 1.213C - .587$
70	$M = 1.128C - .397$
60	$M = 1.057C - .150$
50	$M = 1.000C + .000$
40	$M = .931C + .261$
30	$M = .858C + .581$
20	$M = .784C + .813$
10	$M = .705C + .884$
Lowest	$M = .518C + 1.260$

Figure 2, although statistically correct, gives an entirely erroneous view of the probable actual differences in intelligence. None of these curves have any decided tendency towards becoming horizontal in the later chronological ages, indicating decrease in the rate of growth, as is known to be the case. We need a diagram, therefore, that will show the gradual decrease in the growth of intelligence with increasing chronological age. To obtain such curves we must obviously calculate the difference in amount of intelligence from one chronological age to the other. These amounts will determine the gradually decreasing distances for our mental age ordinates.

In Table III. are shown the standard deviation intervals between the median mental ages (half-year mental age classes) by chronological age. This table was constructed from the original frequency distribution table of mental ages by chrono-

TABLE III

STANDARD DEVIATION INTERVALS OF THE SEVERAL MENTAL AGES AS MEASURED BY THE SEVERAL CHRONOLOGICAL AGE DISTRIBUTIONS

Chronological Age	Standard Intervals (Sigma)																			
	6.0–6.4	6.5–6.9	7.0–7.4	7.5–7.9	8.0–8.4	8.5–8.9	9.0–9.4	9.5–9.9	10.0–10.4	10.5–10.9	11.0–11.4	11.5–11.9	12.0–12.4	12.5–12.9	13.0–13.4	13.5–13.9	14.0–14.4	14.5–14.9	15.0–15.4	15.5–15.9
6	.54	.44	.59																	
7	.97	.64	.38	.36	.46	.48														
8			.47	.48	.44	.64	.49	.35												
9			.53	.48	.36	.61	.34	.29	.45	.40										
10					.21	.40	.29	.22	.38	.26	.20	.24								
11							.25	.24	.33	.25	.26	.24	.35	.18						
12									.24	.26	.25	.21	.32	.19	.20	.15				
13											.16	.23	.33	.22	.25	.15	.20	.18		
14													.20	.27	.13	.19	.19	.16		
15															.25	.13	.30	.20	.12	.21
16																	.22	.24	.16	.06
Weighted Sigma Interval	.75	.54	.48	.43	.37	.55	.34	.27	.35	.29	.22	.23	.31	.21	.20	.16	.23	.19	.14	.13
Cumulative Sigma Interval	.75	1.29	1.77	2.20	2.57	3.12	3.46	3.73	4.08	4.37	4.59	4.82	5.13	5.34	5.54	5.70	5.93	6.12	6.26	6.39

logical ages. For each chronological age cumulative percentage frequencies for each mental age step in the table were calculated. The points represented by these cumulative percentages were then turned into sigma values by means of the usual table of values of the probability integral (Thorndike, 'Mental and Social Measurements,' Table 44). We thus have sigma values for each point on the table and the differences between these sigma values give us the sigma intervals shown in Table III. To make the matter still more concrete, we find from our frequency table that at chronological age six there are 36.8 per cent. who test at a mental age of 5.99 or below; 57.9 per cent. test at a mental age of 6.49 or below; 73.7 per cent. test at a mental age of 6.99 or below; 89.0 per cent. test at a mental age of 7.49 or below. These percentages are now turned by means of the integral table into the sigma values (*i.e.*, deviations from the median divided by sigma) − .34, + .20, + .64, + 1.23. For example the 73.7 percentile is 23.7 per cent. above the median and we find from the table that a range from the median to + .64 sigma includes about 23.7 per cent. of the cases, and so with the others. Subtracting these values (*i.e.*, − .34 from + .20, + .20 from + .64, etc.) we get .54, .44, and .59 as standard deviation intervals for the three mental age intervals as shown in Table III. The figures give, for a normal frequency distribution of mental ages of children six years old chronologically, the distance or range on the base line (in terms of sigma) between the points 6.0 and 6.4 mental age, between 6.5 and 6.9 mental age, and so on. In a similar manner the standard deviation intervals for the other chronological ages have been computed. The table shows the general tendency of these standard deviation intervals, representing amounts of intelligence growth, to decrease gradually as we go from the lower to the higher ages.

In the table those standard deviations included within the blocked areas are direct measures of the interval, that is to say they are the chronological age distributions within one year of the mental age considered. Each standard deviation within the blocked areas was arbitrarily given a weight of 3 and those figures of the table outside of the blocked areas were given a weight of 2 each and the average weighted intervals between the mental ages were thereby determined.

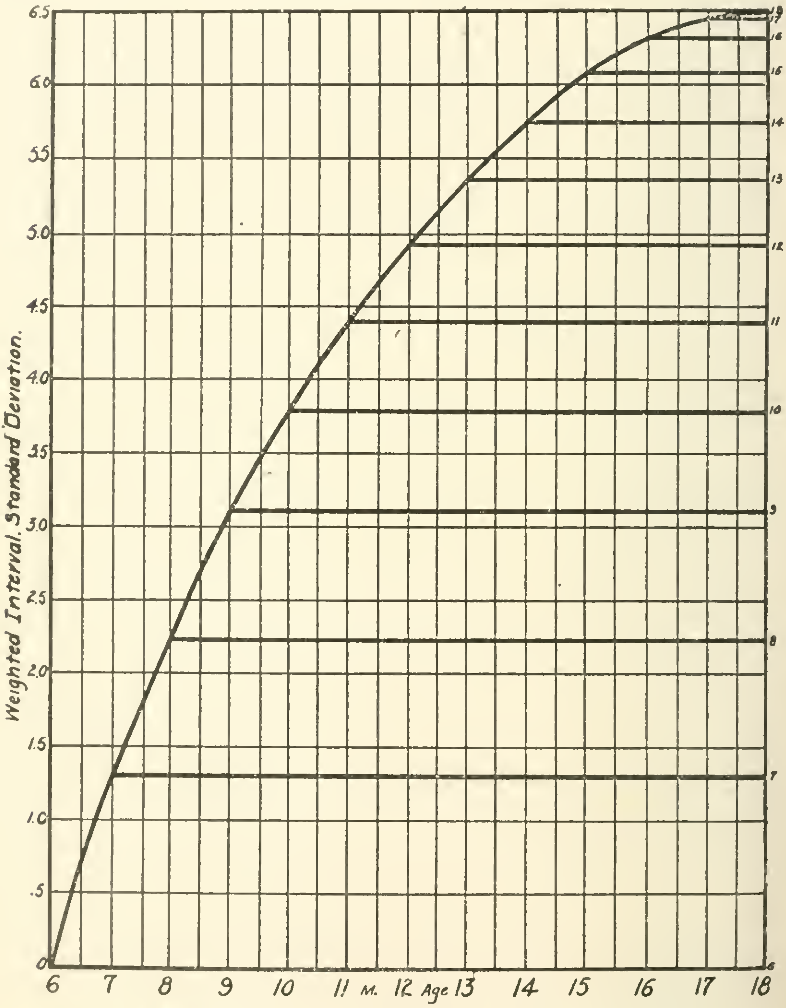

FIG. 3. Method of Determining M.A.-limits in Terms of Amount.

These weighted intervals are shown at the bottom of the table in the row called "Weighted Sigma Intervals." It will be noticed that there is a gradual decrease in the interval with increase in age but that these decreases are not perfectly regular. To eliminate this feature, and secure a probably more accurate determination of the interval as well as having a high utilitarian value in plotting the curves, the mental age

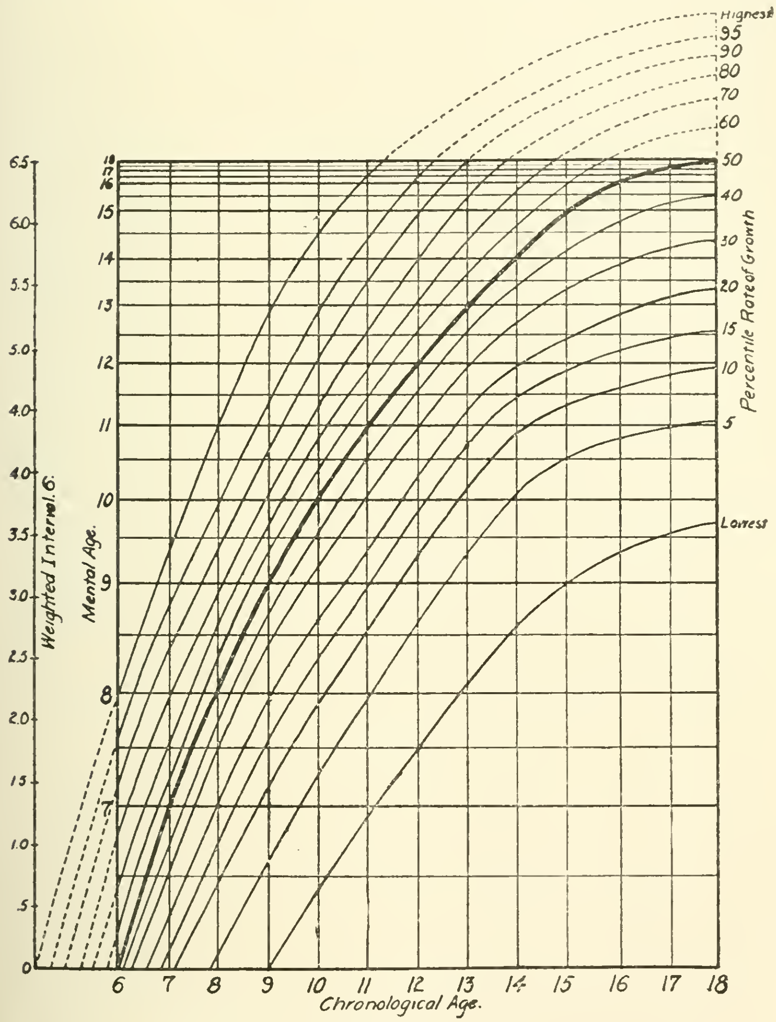

Fig. 4. Curves of Growth of Intelligence. Percentile Rates of Growth.

intervals were cumulated, as shown in the last row of Table II., and plotted as the curve of Fig. 3.

In this curve, very small irregularities were mechanically ironed out in drawing the curve. The intersections of each mental age ordinate with the cumulative curve are projected to the right and form our foreshortened ordinates of Fig. 4.

Using these foreshortened mental age ordinates and the usual equal-interval chronological age abscissae, the data of Table I. were plotted with these coördinates. Due to the method of measurement of mentality in terms of mental age, mentality units above age sixteen are not available. The percentile curves have, however, been prolonged graphically until they come to a point where the rate of growth no longer increases. If we do this, we find that the median of the fifty percentile curve seems to stop at age eighteen. This is probably too high and due to the fact that the sampling of children in the higher ages was very likely more selected than those in the lower ages. As we know from other data, it is more likely that average mental growth ceases about age fourteen or fifteen. There may be some increase after these ages, but such increase is probably so slight as not to be measurable with the rough scales of intelligence that we possess at the present time. Our curves, therefore, based as they are upon the data derived from a specific set of tests, probably show too much increase in the higher ages, but they will serve, nevertheless, to indicate the general trend of mental development for all degrees of intelligence at the various chronological ages.

Part II

We must keep in mind the theoretical assumptions underlying the procedure in Part I. The assumption is made first of all, that intelligence tests of the type used actually do measure intelligence. No intelligence test has ever yet been devised which will give a perfect correlation with any independent social criterion of intelligence. Partial correlation may be used in combining mental tests with the proper theoretical weighting of each test to give the maximum correlation with any given social criterion. It is to be noted, however, that even the social criterion itself will never give a perfect correlation with the presumably true intelligence of the individuals involved.

The second assumption, which has often been used, but which has as yet not been put to experiment, is that the *r*th percentile child at any given chronological age will be the *r*th percentile child at any succeeding chronological age. If this be assumed to be true, then the use of the procedure outlined above is justifiable. If there are any "spurts" in intelligence, these would quite necessarily be covered up by the procedure used. Future measurements of the mentality of large groups of children over a period of years is very desirable in this connection. The assumption here made is purely arbitrary but consistent with the assumption ordinarily made by most experimenters in group testing.

The third assumption that the average weighted standard deviation is the best measure of mental age intervals is also purely arbitrary. A fourth assumption is that rates of growth of intelligence computed from mental tests of *school children* give the desired curves of growth. The statement often made to the effect that a distribution of *school children* is a representative sampling of the population as a whole is manifestly false; very apparently so, when one considers the very high amount of elimination from school in the higher grades. Such elimination in the seventh and eighth grades is largely of two kinds:

(*a*) Elimination of the mentally inferior, and
(*b*) Elimination by graduation of mentally advanced pupils who have also been advanced in school.

In point of numbers, (*a*) and (*b*) most certainly do not counterbalance each other. If we resort to the high school in order to secure a large number of persons of sixteen years or over, we again have a more decidedly selected group of people and not a *random selection* of test subjects at such ages. The difficulty of obtaining a *random selection* of test subjects at any age necessitates resorting to the schools for test subjects and such has been true in all investigations, at least so far as published. This will probably always continue to be the case. Curves of growth of intelligence based upon school children are decidedly not the best for purposes of prognosis.

Thorndike has clearly shown this fact in his investigation of school elimination.[1] Such curves are based upon the 'survivors' and the 'obtainable' and not on a random selection of the whole population of the chronological age limits considered.

[1] Thorndike, E. L., 'The Elimination of Pupils from School,' U. S. Bureau of Education Bulletin, No. 4, 1917, pp. 45-55.

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THE USE OF THE ILLUMINATION SCALE FOR THE DETECTION OF SMALL ERRORS IN REFRACTION AND IN THEIR CORRECTION

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There are doubtless many ways in which sensitivity can be added to the acuity test for the detection of small errors in refraction and in their correction. In connection with the problems which we have undertaken during the past eight years involving modifications and refinements in functional testing, three principles have come to light which can be used very effectively to this end. That is, the eye which suffers from an insufficient resolving power shows the following functional defects. (1) An undue lag or slowness of discrimination and of making the adjustments needed for clear seeing. (2) A marked loss in power to sustain the adjustments needed for clear seeing. And (3) an increase in the amount of light required just to discriminate details in the standard acuity object. The devising of test methods based on the first two of these principles has been treated of in former papers. The third alone will be considered here.

The relation of the illumination scale to the detection of small errors in refraction and in their correction may be stated briefly as follows. Insofar as the test-object is concerned, clearness of seeing depends upon the value of the visual angle subtended and the intensity of the illumination. It follows from this that either the illumination scale or the visual angle scale may be used for the detection of errors in refraction, *i.e.*, in the diagnostic procedure either the illumination may be held constant and the visual angle varied, or the converse. Since the visual angle scale sustains by convention a 1 : 1 relation to acuity while acuity changes slowly with change of illumination for all but very low illuminations, the illumination scale possesses the greater sensitivity for the detection of small errors in refraction—also the greater feasibility of contrivance and manipulation. Used in this way the illumination scale becomes in effect an amplifying scale—somewhat analogous to the use of the tangent scale in detecting small deflections in the magnet system of a galvanometer—and has an advantage in sensitivity in proportion to the amplification. In clinic practice it has been shown to be of particular value in determining the exact amount and placement of the correction of astigmatisms. That is, if the eye has equal resolving power in all meridians, the amount of light required just to discriminate the test-object in all meridians will be the same; if the resolving power is not equal, the amount of light required will be different in the different meridians and different in proportion to the amplification represented by the illumination scale. This gain in sensitivity over the clinic methods is needed in particular to determine the exact amount of the correction in case of high astigmatisms and both the amount and exact placement of the correction in case of low astigmatisms. The checking up of a number of cases shows that the corrections by the clinic methods may be and frequently are off from 0.12–0.25 diopter in the strength of the cylinder and, in case of low astigmatisms, from 5–20 degrees in the placement of the cylinder axis. While errors of this magnitude may or may not be troublesome in the ordinary uses of the eye—sometimes they are very troublesome indeed and, perhaps always tend in time to increase the amount of the defect—they do constitute a much more serious handicap, perhaps an actual disqualification, for work or vocations requiring special ocular proficiencies, *e.g.*, keen acuity, particularly keen acuity at low illuminations; the power to sustain acuity; speed in the use of the eye, especially speed of discrimination and of making the adjustments needed for clear seeing; etc. Moreover, it is safe to say that a considerably greater amount of light is required as a comfortable and efficient working minimum by the poorly than by the well corrected eye. Indeed our experience with the tricornered relation of intensity of light, resolving power and retinal sensitivity to acuity has impressed us with the relative importance of resolving power in explaining the difference in the amount of light that is required by different people as a working minimum.

The relation of the intensity of illumination to acuity may be illustrated by the curve shown in Fig. 1. This curve represents the average results for four observers, tested by Koenig.[1] In this curve acuity is plotted along the ordinate and intensity of illumination along the abscissa. It will be noted, for example, that a change of from 1 to 9 meter-candles, an increase of 800 per cent. in the intensity of illumination, produced an increase of only 74 per cent. in acuity; and a change of 9 to 100 meter-candles, an increase of 1011 per cent. in illumination, produced an increase of only 28 per cent. in acuity. The amplification within the latter range of illumination is doubtless too great for feasibility of application. That is, too wide a range of illumination would have to be used to compensate for the difference between the resolving power in the poorest and best meridians in the ordinary run of astigmatisms. The range from 1–9 meter-candles is, however, quite feasible and the relation between the two scales gives abundant sensitivity. These values fall within the range given by the apparatus described in our former paper, 0.07–9.5 meter-candles. The testing of a large number of astigmatisms with this apparatus showed that in the majority of cases the minimum amount of light required for the discrimination of the opening in the broken circle (visual angle, 1 min.) in the most favorable meridian was of the order of 1–3 meter-candles; in the least favorable meridian, of the order of 6–9.5 meter-candles.

A very convenient apparatus for using the illumination scale for detecting low astigmatisms and small errors in the

[1] "Ueber die Beziehung zwischen der Sehschärfe und der Beleuchtungsintensität," *Verhandl. der Physikal. Ges. in Berlin*, 1885, 16, S. 79–83.

amount and placement of their corrections was described in a former number of this journal, "An Apparatus for Determining Acuity at Low Illuminations, etc.," 1920, Vol. III, No. 1, pp. 59–71. In this apparatus, it will be remembered, uniformity of illumination of the test surface was secured by

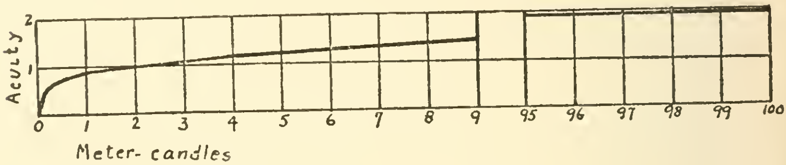

Fig. 1. Showing the relation of intensity of illumination to acuity (Koenig, 4 observers).

projecting upon it the image of an evenly illuminated aperture at the inner end of a projection tube of a lantern or lamp house. In order to secure a uniform illumination of this aperture, the lamp house was lined with opal glass ground on one side, and the aperature itself was covered with a slide of ground glass. The source of light was a round bulb, 100-watt, type C Mazda lamp with its filament well above the aperture to be illuminated, and the changes of illumination were produced by an iris diaphragm placed immediately behind the focusing lens in the projection tube, which reduced the illumination without changing the size or shape of the image. The test-object was a broken circle fastened at the center of a graduated dial the opening of which (visual angle, 1 minute) could be turned into any meridian that was desired. The angle of turning could be read in terms of the divisions on the dial which was graduated to correspond to the readings on the trial frames used in office and clinic work. The results given in this paper were obtained with this type of apparatus. They are fairly representative of the large number that have been obtained.

In the testing and demonstration of the sensitivity and serviceability of the illumination method for determining the exact amount and placement of the correction of an astigmatism the following types of material have been selected: (1) Artificial astigmatisms made with cylinders of low diopter value. In choosing to include artificial astigmatisms in this work it should be understood that we did not consider the artificial astigmatism the precise functional equivalent of the natural astigmatism. We are too strongly impressed with the possibility that the astigmatic eye may progressively acquire power to compensate in part for its defect to be of this opinion. They were selected because we wished to have in one set of cases an exact knowledge of the amount and location of the defect as a check on the determinations made by the test. (2) Natural astigmatisms without a cycloplegic. (3) Office and clinic cases with a cycloplegic. The difference in result between the most and least favorable meridians or between a true and a false correction have thus far been of a considerably greater order of magnitude with than without a cycloplegic either in case of a natural or an artificial astigmatism. (4) Office and clinic cases, submitted to us by experienced refractionists, in which the apparatus has been used merely to check up corrections already made by the clinic methods, objective and subjective. Among these cases it was comparatively rare to find one in which the minimum amount of light required to discriminate the test-object in the corrected meridian was even approximately equal to that required in the other meridians. Indeed in some cases the difference between the most and least favorable meridian exceeded the range of variation obtainable with the apparatus when provided with the 100-watt lamp. And (5) irregular astigmatisms.

For the artificial astigmatisms three cases have been used.

1. *Low Astigmatisms Produced by Weak Cylinders.*—In this case the minimum amount of light required to discriminate the test-object with the opening of the circle turned into the most and least favorable meridians has been determined; also when turned 5, 10 and 45 degrees from the most favorable meridian. In Table I. it will be noted that in case of an astigmatism produced by a 0.25 diopter cylinder the difference in the light required for the discrimination of the test-object in the worst and best meridians amounted to 107.2 per cent. as an average result for five eyes. At 5 degrees from the

TABLE I

SENSITIVITY OF APPARATUS FOR LOCATING MERIDIAN OF ASTIGMATISM

Astigmatisms produced by 0.25 and 0.75 diopter cylinders.

Observer	Value of Cylinder Producing Astigmatism	Minimum Illumination Required for Discrimination of Test-object (Meter-candles)					Difference in Results Between Best and Other Meridians							
							Meter-candles				Per Cent.			
		Best Meridian	5° from Best Meridian	10° from Best Meridian	45° from Best Meridian	Worst Meridian	5°	10°	45°	Worst Meridian	5°	10°	45°	Worst Meridian
A.......	0.25	0.60	0.88	1.49	1.62	1.62	0.28	0.89	1.02	1.02	46.7	148.3	170.0	170.0
B.......	0.25	1.12	1.75	2.405	2.69	2.69	0.63	1.285	1.57	1.57	56.3	114.7	131.5	131.5
C.......	0.25	0.46	0.60	0.76	0.76	0.76	0.14	0.30	0.30	0.30	30.4	65.2	65.2	65.2
D.......	0.25	1.42	2.115	2.49	2.49	2.49	0.695	1.07	1.07	1.07	48.9	75.4	75.4	75.4
E.......	0.25	1.12	1.88	1.95	2.17	2.17	0.76	0.83	1.05	1.05	67.9	74.1	93.8	93.8
Average.	....	....	....	....	....	....	0.501	0.875	1.002	1.002	50.0	95.5	107.2	107.2
A.......	0.75	1.30	2.69	3.05	3.05	3.05	1.39	1.75	1.75	1.75	106.9	134.6	134.6	134.6
B.......	0.75	1.81	4.11	4.39	4.39	4.39	2.30	2.58	2.58	2.58	127.8	142.5	142.5	142.5
C.......	0.75	0.60	1.75	1.95	2.32	2.32	1.15	1.35	1.72	1.72	191.7	225.0	286.7	286.7
D.......	0.75	3.05	6.895	6.895	7.60	7.60	3.845	3.845	4.55	4.55	126.1	126.1	149.2	149.2
Average.	....	....	....	....	....	....	2.171	2.381	2.65	2.65	138.1	157.1	178.3	178.3

best meridian, this difference was 50 per cent.; at 10 degrees, 95.5 per cent.; and at 45 degrees 107.2 per cent. In case of the 0.75 diopter astigmatism, the difference between the worst and best meridians was 178.3 per cent.; at 5 degrees from the best meridian, 138.1 per cent.; at 10 degrees, 157.1 per cent.; and at 45 degrees, 178.3 per cent.

2. *Small Errors in the Placement of the Correction.*—In Table II. it will be noted that in case of an astigmatism pro-

Table II

Sensitivity of Apparatus for Detecting Small Errors in the Placement of the Correction of an Astigmatism

Observer	Value of Cylinder Producing Astigmatism	Minimum Illumination Required for Discrimination of Test-object (Meter-candles)			Difference in Result for Correct and Incorrect Placements					
		Exact Placement of Correction	5° Displacement	10° Displacement	Scale Divisions		Meter-candles		Per Cent.	
					5°	10°	5°	10°	5°	10°
A.......	0.25	0.60	1.33	1.62	13.0	17.0	0.73	1.02	121.7	170.0
B.......	0.25	1.12	2.25	2.69	17.5	22.0	1.13	1.57	100.9	140.2
C.......	0.25	0.46	0.60	0.65	3.0	4.0	0.14	0.19	30.4	41.3
D.......	0.25	1.42	1.55	1.88	2.0	7.0	0.13	0.46	9.2	32.4
E.......	0.25	1.12	1.91	2.17	12.5	20.0	0.79	1.05	70.5	93.8
Average..	....	....	....	....	9.6	14	0.58	0.86	66.5	95.5
A.......	0.75	1.30	2.49	3.05	17.0	22.0	1.19	1.75	91.5	134.6
B.......	0.75	1.81	3.43	4.39	17.0	22.0	1.62	2.58	89.5	142.5
C.......	0.75	0.60	1.42	2.12	14.0	24.0	0.82	1.52	136.7	253.3
D.......	0.75	3.05	6.44	6.44	20.0	20.0	3.39	3.39	111.1	111.1
Average..	....	....	....	....	17.0	22.0	1.75	2.31	107.2	160.4

duced by a 0.25 diopter cylinder a displacement of the correction 5 degrees from the true position required 66.5 per cent. more light for the discrimination of the test-object as an average result for five eyes; a displacement of 10 degrees, 95.5 per cent. more light. In case of a 0.75 diopter astigmatism a displacement of 5 degrees required 107.2 per cent. more light; and a displacement of 10 degrees, 160.4 per cent. more light. In connection with this table note also the large number of scale divisions between the correct and the incorrect placement of the cylinder. In case of a 5 degree displacement for the 0.25 diopter astigmatism, this difference averaged 9.6 for the five eyes. That is, since the diaphragm can be readily set to half divisions, 19 settings of the light control could have been made with precision between the values needed for the true correction and the 5 degree displacement. This shows that the sensitivity of the apparatus far exceeds the present possibilities of the precise manipulation of the correcting cylinders.

3. *Small Errors in the Amount of the Correction.*—In the case of a 0.12 diopter error in the correction of an astigmatism, 54.6 per cent. more light was required for the discrimination of the test-object in the worst meridian; in case of a 0.25 diopter error, 108.9 and in case of a 0.75 diopter error, 178.25 per cent. more light was required. These results are shown in Table III.

Table III

Sensitivity of Apparatus for Detecting Errors in the Amount of Correction of an Astigmatism

Observer	Minimum Illumination Required for Discrimination of Test-object with Different Errors in Amount of Correction (Meter-candles)						Difference in Minimum Illumination to Discriminate Test-object in Most and Least Favorable Meridians					
	0.12 Diopter		0.25 Diopter		0.75 Diopter		0.12 Diopter		0.25 Diopter		0.75 Diopter	
	Best Meridian	Worst Meridian	Best Meridian	Worst Meridian	Best Meridian	Worst Meridian	Meter-candles	Per Cent.	Meter-candles	Per Cent.	Meter-candles	Per Cent.
A......	0.60	0.88	0.60	1.62	1.30	3.05	0.28	46.7	1.02	170.0	1.75	134.6
B......	1.12	2.17	1.12	2.69	1.81	4.39	1.05	93.8	1.57	140.2	2.58	142.5
C......	0.46	0.65	0.46	0.76	0.60	2.32	0.19	41.3	0.30	65.2	1.72	286.7
D......	1.42	1.75	1.42	2.49	3.05	7.60	0.33	23.3	1.07	75.4	4.55	149.2
E......	1.12	1.88	1.12	2.17	——	——	0.76	67.9	1.05	93.8	——	——
Average.	.....	.....	.....	.....	.....	.....	0.52	54.6	1.00	108.9	2.65	178.25

Of the large number of natural astigmatisms tested space will be taken here for the representation of only a few cases.

Astigmatism (Without a Cycloplegic)

Case I. (Age 13 Years)

O.D.: Correction by clinic methods, 0.25 cyl., ax. 70°. (Placement of axis could be varied over a range of about 45° and cylinder could be changed to 0.12 diopter without noticeable change in the results by these methods.) With this correction illumination required with opening of the circle in meridian of cylinder axis, 0.20 m.c.; at 90 degrees from this position, 0.55 m.c.; difference, 0.35 m.c. or 175 per cent.

Correction by illumination method, + 0.12 cyl., ax. 55°. With this correction equal illumination (0.16 m.c.) was required for the discrimination of the test-object in all meridians.

Difference in amount of light required for discrimination of test-object in least favorable meridian for the two corrections, 0.39 m.c. or 244 per cent.

O.S.: Correction by clinic methods, + 0.12 cyl., ax. 180°. (Placement of axis could be varied over a range of about 45 degrees without change in result by these methods.) With this correction illumination required with opening of circle in meridian of cylinder axis, 0.12 m.c.; at 90 degrees from this position, 0.21 m.c.; difference, 0.09 m.c. or 75 per cent.

Correction by illumination method, + 0.12 cyl., ax. 15°. With this correction equal illumination (0.105 m.c.) was required for discrimination of test-object in all meridians.

Difference in amount of light required for discrimination of test-object in least favorable meridian for the two corrections, 0.105 m.c. or 100 per cent.

Case II. (*Age 48 Years*)

O.D.: Illumination required before correction with opening of circle in most favorable meridian, 2.93 m.c.; at 90 degrees from this position, 9.19 m.c.; difference, 6.26 m.c. or 214 per cent.

Correction by illumination method, − 0.50 cyl., ax. 105°. With this correction, equal illumination (2.93 m.c.) was required for the discrimination of the test-object in all meridians.

O.S.: Illumination required before correction with opening of circle in most favorable meridian, 2.35 m.c.; at 90 degrees from this position, 5.25 m.c.; difference, 2.90 m.c. or 123 per cent.

Correction by illumination method, + 0.37 cyl., ax. 137°. With this correction, equal illumination (2.35 m.c.) was required for the discrimination of the test-object in all meridians.

Irregular Astigmatism

Case I. (Age 32 Years)

O.S.: Illumination required with opening of circle turned right, left, and down, 0.97 m.c.; when turned up, 5.25 m.c.; difference for two halves of vertical meridian, 4.28 m.c. or 441 per cent.

Astigmatism (with Cycloplegic)

Case I. (Age 25 Years)

O.D.: Correction by clinic methods, + 0.50 S., + 0.37 cyl., ax. 15°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 2.46 m.c.; at 90 degrees from this position, 9.19 m.c.; difference, 6.73 m.c. or 274 per cent.

Correction by illumination method, + 0.50 S., + 0.37 cyl., ax. 30°. With this correction, equal illumination (1.61 m.c.) was required for the discrimination of the test-object in all meridians.

Difference in amount of light required for discrimination of test-object in least favorable meridian for the two corrections, 7.58 m.c. or 471 per cent.

Case II. (Age 35 Years)

O.D.: Corrections by clinic methods, − 0.62 cyl., ax. 180°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 2.32 m.c.; at 90 degrees from this position, 9.19 m.c.; difference, 6.87 m.c. or 296 per cent.

Correction by illumination method, − 0.75 cyl., ax. 180°. With this correction, equal illumination (2.09 m.c.) was required for the discrimination of the test-object in all meridians.

Difference in amount of light required for discrimination of test-object in least favorable meridian for the two corrections, 7.10 m.c. or 339 per cent.

Astigmatism (Checking Up of Glasses)

Case I. (Age 42 Years)

O.D.: Correction by clinic methods, − .50 S., − .37 cyl. ax. 10°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 2.34 m.c.; at 90 degrees from this position, 7.35 m.c.; difference, 5.01 m.c. or 214 per cent.

Case II. (Age 45 Years)

O.D.: Correction by clinic methods, − 0.25 S., − 0.50 cyl., ax. 125°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 2.02 m.c.; at 90 degrees from this position, 6.67 m.c. in one half of meridian, 7.82 m.c. in other half; difference, 4.65 m.c. (230 per cent.) and 5.80 m.c. (287 per cent.). Astigmatism may be slightly irregular.

O.S.: Correction by clinic methods, − 0.50 cyl., ax. 80°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 0.97 m.c.; at 90 degrees from this position, 5.62 m.c. in one half of meridian, 6.23 m.c. in other half; difference, 4.65 m.c. (479 per cent.) and 5.26 m.c. (542 per cent.). Astigmatism may be slightly irregular.

In the above notes on cases we have, for the sake of brevity, used the term clinic methods, instead of specifying in greater particular the tests employed. Where we have made the comparison ourselves between the illumination method and the methods ordinarily employed in office and clinic work, we have used the acuity method, the astigmatic charts, the point of light test and in some cases the ophthalmometer. We have not made frequent use of the retinoscope because of the need of a cycloplegic. The acuity method was used in different ways. In one, patterned after a procedure much employed by the ophthalmologists, some character difficult of discrimination and taxing the resolving power of the eye in as many meridians as possible, such as the letter B, was selected. It was brought to or near to the threshold of discrimination by fogging, by changing the visual angle, by the use of a graded scale of illumination, etc., in order to make the conditions favorable for a sensitive judgment; and the strength and placement of cylinder was determined which gave the maximum clearness of seeing. In order to decide between doubtful determinations other acuity tasks or tests were imposed. That is, we not only used the acuity test as employed by the practitioner but have endeavored in many ways to add to its sensitivity and precision without sacrificing its distinctive features. However, in collecting the data for the comparison we have preferred to lay the chief stress on the cases in which the clinic testing has been done by practicing ophthalmologists, who have very willingly given us their cooperation, In all cases but one, which have been submitted to us for testing, the physician has himself accompanied the patient, looked after the cycloplegia, and inspected the test procedure at every step, the principle of the apparatus and method having previously been made familiar to him. Due care was taken on both sides that a fair comparison of sensitivities was made.

Doubtless the apparatus can be used in different ways depending upon the experience and preference of the operator. For example, the minimum amount of light required to discriminate the test-object could be determined for one meridian and the setting of the light control be held constant while the test-object is rotated into the different meridians, the observer being required to judge in each case whether the same or more or less light would be required for its discrimination. This would serve as a rough indication of whether or not the eye is astigmatic. The exact meridian of the defect, that is the meridian in which the greatest amount of light is required to discriminate the opening in the circle, could be determined through a series of settings of the test-object and the light control. The placement of the correction having been determined, its amount could be found by the strength of cylinder required to render the minimum illumination needed to discriminate the test-object the same for all meridians, or more roughly speaking for the meridian of the defect and at 90 degrees either way from this position. A quicker and more feasible method, however, is first to make an approximate determination of the amount and placement of the correction by the clinic methods and employ the illumination method only for a more precise determination. In using this method as a refinement on the clinic methods, the procedure we ordinarily employ is as follows: The patient's eye is fitted with a cylinder of the strength and placement indicated by the clinic tests and the minimum amount of light required to discriminate the opening in the circle is determined in four positions, two in the meridian of the cylinder axis and two in the meridian at right angles to this. If the minima are not equal in these four positions, the cylinder axis is shifted and the determinations are made again, the four positions of the opening of the circle always being in the meridian of the cylinder axis and the meridian at 90 degrees from it. If no placement of the cylinder is found which gives equal minima for the four positions, the strength of the cylinder is changed. The strength and placement of cylinder which requires both equal and the smallest amounts of light for the four positions of the test-object is accepted as the final correction.

The apparatus can also be used to advantage with astigmatic charts of the sunburst type, the radial lines of which are no more than 5 degrees apart, in the preliminary approximate determination of the axis of the defect. In this case the procedure would be to reduce the illumination until only one or perhaps two of the lines stand out clearly. This would give a sensitivity roughly speaking of about 5 degrees, and requires little more time than is usually consumed in the use of the astigmatic charts.

In our own work we have found that the apparatus would be very helpful even if it were used only to check up the corrections made by the clinic methods and were not employed further as an aid in finding out the exact amount and placement of the correction. For example, but a very few minutes are required to determine with it whether any given correction equalizes or levels up the resolving power of the eye in the different meridians. The advantage of a checking method which is definite and at the same time feasible, can readily be appreciated by any one who has tried to decide by the clinic methods in any wide range of cases just what should be the exact amount and placement of the correction of an astigmatism. The method has its chief value perhaps in those cases in which it is particularly difficult to make a decision by the clinic methods, that is, in determining the exact amount of the correction in cases of high astigmatism and both the amount and placement of correction in case of low astigmatisms. The simple character of the judgment, namely the mere indication of the direction in which the opening of the circle points instead of the more difficult task of deciding under the comparatively rough conditions of the office and clinic test whether this or that placement or strength of cylinder gives the clearer vision, together with the objective check on the correctness of each judgment, also contribute to make the method especially valuable in case of children, and the subjective, unintelligent and untrained type of adult. A further advantage of the method as worked out in connection with the present apparatus is its great sensitivity for the detection of irregular astigmatisms. The lack of satisfactory tests for this troublesome defect is generally conceded.

THE BACKWARD ELIMINATION OF ERRORS IN MENTAL MAZE LEARNING[1]

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In another paper the writer has shown that an animal through sheer probability will, with a continued series of efforts, reach the food box in a maze; and further, that on the basis of frequency of exercise (if we grant the operation in learning of certain other factors appearing to be indispensable to learning) the errors of entrance to blind alleys should be gradually eliminated in what has been called the backward order, that is, the errors near the food box should first disappear, and the others should be eliminated in order, speaking generally, from this point back toward the entrance place in the maze. In the earlier paper errors were interpreted as entrances to blind alleys; return runs not bringing about such entrances were not counted in the error score. It was shown that the first blind alley is passed, on the basis of probability solely, $3/4$ of the times that it is encountered in forward runs by an animal, and that the animal is turned back by it toward the entrance $1/4$ of the times. The next blind is likewise passed $3/4$ of the times that it is encountered, or $3/4$ of $3/4$ of the number of times that the first blind is reached. Speaking generally, in n trials the mth blind from the entrance would be passed in forward runs $(3/4)^m$ of the number of times that the first blind is reached. The tenth blind, for example, would be passed $(3/4)^{10}$—or a little more than 1/18—of the number of times that the first is encountered.

This matter is, of course, vastly complicated by part-return runs. Disregarding these part-returns for the sake of simplicity, and assuming that each return run will take the

[1] Read in part in Section H of the A. A. A. S., St. Louis, Dec. 30, 1919.

animal back to the starting place in the maze, *i.e.*, to the entrance, we find that when any blind alley is passed n times the one next before it will, on the basis of pure chance, be passed $4n/3$ times. "In general, in a maze with y blind alleys the number of times that the first would be passed in forward runs in n trials is $(4/3)^{y-1}$ *times* the number that the last would be passed, *i.e.*, $(4/3)^{y-1}n$." [1] The last blind would be passed n times, as each trial ends only when this blind alley is passed. In a maze of ten blind alleys, therefore, the last blind would be passed n times in n trials, the next before it $1.33n$ times, and the eighth, seventh, sixth, fifth, fourth, third, second, and first, would be passed $1.78n$, $2.37n$, $3.16n$, $4.22n$, $5.62n$, $7.50n$, $10.00n$, and $13.33n$, respectively.

On this basis it was found, on the above assumption of complete returns, that the ratio of entrances into the respective blind alleys *to* passing them successfully, whether entered or not, would be as follows:

$13.33n - .25 : 13.33n$, for the first blind alley,
$10.00n - .25 : 10.00n$, for the second,
$7.50n - .25 : 7.50n$, for the third,
$5.62n - .25 : 5.62n$, for the fourth,
$4.22n - .25 : 4.22n$, for the fifth,
$3.16n - .25 : 3.16n$, for the sixth,
$2.37n - .25 : 2.37n$, for the seventh,
$1.78n - .25 : 1.78n$, for the eighth,
$1.33n - .25 : 1.33n$, for the ninth, and
$n - .25 : n$, for the tenth.

It will be noticed that this ratio of entrances to passes constantly decreases toward the last *cul de sac*, thus favoring 'exercise in running past' *to* 'exercise in running into' the blind alleys, the advantage being increasingly greater with nearness of the blind alley to the food box. It is important to note also that this advantage of exercise in running past blinds over exercise in running into them also increases as n approaches a small number, as 1, 2, or 3, its value during the early trials when greatest progress is made.

[1] Peterson, Jos., 'Frequency and Recency Factors in Maze Learning by White Rats,' *Jour. Animal Behav.*, 1917, 7, 344.

This quantitative explanation is sufficient to account for the backward elimination of errors in the maze, probably, even if we assume only a general motivating effect of the food, operating equally at all parts of the maze, an assumption, however, not intended here. There is apparently no need of assuming any so-called retroactive association, the existence of which in any case of learning may well be regarded as not established.

In the other paper a question was raised regarding the results of an experiment by Hubbert and Lashley,[1] showing that errors made by entrances into blind alleys numbered with even numbers in Fig. 1, errors of their type I., were eliminated in less than two thirds as many trials as were errors of entrances into blinds numbered in odd numbers. It will be noticed that the errors of type I. are made by the animal's running past the place where it should turn, while the other type of error, type II., is caused by a turn in the wrong direction. The serial backward elimination of errors of type II. was found in the main to agree with the results of Miss Vincent, Carr, the present writer, and others; while no such serial elimination of type II. resulted. This apparent contradiction to the general rule of backward elimination was explained tentatively in our earlier paper as due to the fact that when an animal is just correcting in the last stages the errors of running into blind alleys, it frequently is thrown into confusions (probably by changes in the general kinæsthesis) that result in entrances into later blinds which it has already learned to avoid under normal conditions. Since the animal learns at an early stage in the game, comparatively, to keep the forward direction on emergence from blinds[2] and also to orient itself in general toward the center of the circular maze, as was found by Hubbert and Lashley, it would be expected that a combination of these factors or tendencies would throw it into errors of type I. in parts of the maze nearer the food box than are the sources of the dis-

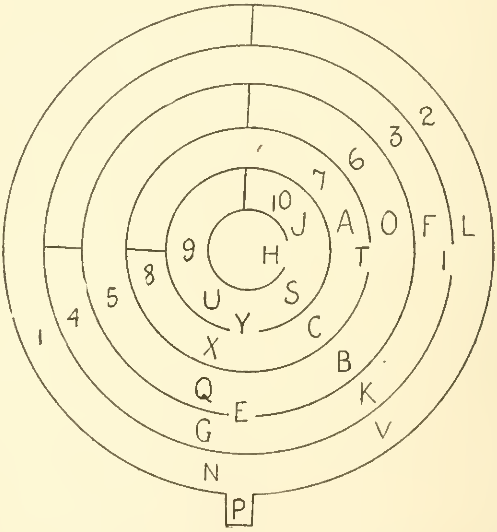
[1] 'Retroactive Association and the Elimination of Errors in the Maze,' *Ibid.*, 1917, 7, 130–138.

[2] Peterson, Jos., 'Behavior Monog.,' Ser. No. *15*, 1917, 24 ff.

turbances. It is our purpose here to present results of an experiment on human subjects with what we have chosen to call a mental maze, a method which eliminates all such confusing spatial factors as those just mentioned.

The Experiment

The present form of procedure was arrived at after considerable experimentation. First, choices of right or left were presented to the subject at the successive stages through



2 4 6 8 10 12 14 16 18 20
w r r r w r w w r w r w w w w r r w r r

Fig. 1. The Mental Maze and Chance Order Schedule.

the maze; it was then found that the different parts of the maze should have a constant designation, so each part was lettered in a meaningless order as shown in Fig. 1. It was desirable to present each pair of letters at any bifurcation simultaneously, but this required an elaboration of apparatus that seemed unadvisable for the practical and wide use of the experiment, though it is still on the program for further experimentation. Tentative experimentation showed that a simpler form of procedure giving reliable results is possible, provided that a pure chance order of presentation of the two alternatives be followed, and that the same emphasis and tone of voice be used throughout. It was therefore decided to follow a chance order schedule or program, exactly the same for all subjects. Accordingly a program was prepared and written just below the drawing of the maze, as shown in the figure. The letter *r* means to call out first the right letter of the two alternatives, and *w* means to call out first the wrong letter. The numbers occurring just above these letters were found to save much time in determining at the beginning of each new trial just where to start, as will be made clear later.

The experimenter, *E*, sits behind a screen with his finger on the schedule of *r*'s and *w*'s. *S* is given the following instructions to read:

> Do you know what a maze is? It is a winding way to some goal, but it has many blind alleys which will lead to error if you enter them. I have drawn a maze which I will show you when I am through with all the subjects that I shall use in this experiment. Wherever there is a choice of two alleys, each alley is designated by a letter. No two parts are lettered the same, and the letters have a constant position in the maze. Beginning at the entrance of the maze, I call out two letters at a time, and you are to choose one of them; then I call out two more; and so on. Whether the right or the wrong letter is called out first is a matter that is determined wholly by flipping a coin, that is, by chance; so you needn't attend to the order. The position of any letter in the maze, moreover, has no relation to its position in the alphabet; letters occur in random order. The problem is to see with how few errors you can get to the goal, and how soon you can learn to follow the true path with no errors at all. You are through when you get to the goal three times in succession without error. You will be told each time when the goal is reached and the number of errors you made in reaching it, but must find out for yourself where the errors are. Remember, accent and order of calling out letters have no significance. Don't attend to them.

After recording *S*'s name and the time of beginning the experiment, *E*, following the program, calls out N–V. If the subject chooses N, the next letters called out are V–P; but if the subject chooses V the first time, I–L are called out; and so on. The letters that the experimenter calls out at any time depend wholly on where *S* has taken him in the maze, but the order of the letters, that is whether the right or the wrong alternative is presented first, is determined by the schedule, which is gone through from beginning to end and repeated in that order till the goal is reached. On *S*'s second trial V is called before N; that is, the experimenter starts with the *second* letter in the schedule. Each successive trial begins with the letter of its number in the chance schedule, the twenty-first trial beginning again at the first letter, and therefore following the same order as the first trial.

The writer conducted all the experiments personally, and he practiced giving the letters in a monotonous tone, always accenting a little the last letter of the pair, whether or not one of the pair had been included in the preceding pair of alternatives, as is the case whenever an error has been made by the subject. When the subject gets to the goal he is told: "That is the goal;" not, "H is the goal;" for that would unduly emphasize H in his mind. The subject is then told that another trial begins, so that he will know exactly what he is expected to do. He knows nothing of the chance program, or the place in it of the start on any new trial, other than he could infer from the instructions.

In the practical operation of the experiment other precautions were developed. It was found that in spite of the explicit instructions regarding a chance-order procedure, some subjects tried to choose the first letter called out, on failure otherwise, or the second, or the first one three times and then the second one three times, etc. Some even "tried to spell words," so it became necessary to throw out the records of all such students, four in number, as they made no progress at all toward a solution without error. To make sure that *S* was not either consciously or unconsciously following any order, the writer decided to put a dot under every choice of the first letter called out. With practice this was found to be easily done, and to leave a record for study that is accurate in all essential details. To guard against errors in following the schedule, *E* moved his finger forward to the next letter each time while recording *S*'s response; that is, it was found best to reduce the whole process of calling out letters and recording responses to a fixed routine.

The experiment from the first proved very interesting, giving *E* an opportunity to study *in actual operation* and in ever varying detail, association processes that we have been prone too much to consider only in tables of averages, and in curves of results. Several subjects require at least two hours to complete the test, while a few good ones do it in a surprisingly short time. The best record to date is twelve minutes for the learning, with a total of only seventeen errors. The experiment seems to have important diagnostic values which we cannot consider in the present paper. The student, for instance, who made the remarkable record just quoted is a young Chinese woman of excellent mental ability and unusual initiative and objectivity of mind, as this attitude has been described elsewhere.[1] Students of poor initiative seem to fall into profitless routine that takes them nowhere.

It will add to clearness to give here the record of one subject in a single trial, that is, till the goal is reached. We shall take the first trial of a young woman who completed the experiment in forty-five trials.

Trial 1. *N** *V* L* V* (P) N* V L* V* (*N*) P* *N** V *L** I F* I* (V) (*P*) N* *P** *V* *I* *K* *G** *E* *Q** *E** (*K*) (*F*) *K* E *Q** B T A* *C* Y *S* *H* (goal). 18 errors.

In the above illustration a letter in italics indicates that the first of the alternatives called out to *S* was chosen; an asterisk after a letter indicates an error (*i.e.*, either an entrance into a blind alley on a forward run, or a return toward the entrance on emergence from a blind); and a letter in parenthesis stands for an uncritical choice, made while going toward the entrance of the maze and at a juncture where no error is possible, as we have defined errors. For each of these meanings the writer has a simple mark that he makes while the experiment is in progress. A complete objective record of all the subject's significant responses is thus easily made possible.

Subjects

The subjects whose records we present in this paper were five men and fourteen women. The women and three of the men were students in the writer's section of general psychology. One of the men is an instructor in psychology, and one is a graduate student. Five of the students reacted to a different lettering of the maze from that shown in Fig. I,

[1] Ruger, H. A., 'The Psychology of Efficiency,' *Archiv. of Psychol.*, No. 15, 1910. Peterson, Jos., 'Experiments in Rational Learning,' *Psychol. Rev.*, 1918, *25*, 463.

the purpose of the change being to make sure that results were not conditioned in an important manner by any particular lettering or letter associations.

Results

Table I. summarizes the results of the nineteen subjects in such a form as to show where and in what trials the 2,956 errors occurred. The totals of the columns, at the base of the table, are arranged to show separately (1) the errors of returning toward the entrance of the maze on emergence from blind alleys, (2) the errors of entrance into blind alleys of odd numbers, Hubbert and Lashley's type II. errors, made by turns in the wrong direction, and (3) the errors of entrance into blinds of even numbers, type I., made by wrongly passing a door.

It will be noticed that our experiment, which eliminates the various disturbing and irrelevant spatial factors peculiar to different mazes, gives results that show practically an equality of errors of type I. and type II., as these errors have already been defined. Moreover, the median of the trials by our several nineteen subjects in which the last error at each part of the maze occurred, shows the same fact of equality in difficulty of these two types of errors, as would, of course, be expected on the basis of probability. This median in each case is found as follows: The last entrance to each blind alley by every subject is tabulated and the trial on which it was made is recorded. We thus have nineteen last entrances for each *cul de sac.* The median of each set of these nineteen numbers is found, and the average of the medians of errors of type I. is obtained and compared with the average corresponding to it for type II. errors. The average of these medians is 6.6 in each case. The results from which these averages were derived will be found in Table II. The median in this case is chosen because it is not affected by certain accidental conditions of extreme cases as is the average.

Studies of the individual records, a matter that we cannot consider here, show that not infrequently when errors of

Table I

Showing the Distribution of the 2,956 Errors of Nineteen Subjects in the Mental Maze Learning

Trials	Parts of the Maze*																				Total Errors	Av. Errors per S
	P	1 N	V	2 L	I	3 F	K	4 G	E	5 Q	B	6 O	T	7 A	C	8 X	Y	9 U	S	10 J	H		
1	59	67	63	74	20	21	45	52	18	15	13	21	11	18	6	10	5	8	5	9		540	30.5
2	20	23	49	56	12	15	24	38	13	18	10	20	11	18	11	17	4	10		3		372	19.6
3	21	26	14	13	12	21	16	22	6	15	6	17	13	13	6	10	13	17	1	5		267	14.1
4	26	29	17	26	13	22	14	19	6	14	7	14	3	6	7	11	2	8		3		247	13.0
5	23	28	15	21	11	18	10	18	15	18	7	17	10	15	6	12	3	9	1	3		260	13.7
6	21	24	17	19	12	13	12	19	6	6	4	10	3	7	2	6	4	8		4		197	10.4
7	7	14	3	14	7	12	1	6	2	8	17	23		7	8	13	3	7		3		155	8.2
8	6	11	3	7	4	9	4	10	6	11	2	7	2	7		4	2	5	1	2		103	5.4
9	9	10	5	11	3	6	1	8	2	4	3	11	3	5	2	4	3	7	1	4		102	5.4
10	9	12	6	9	6	9	3	8	5	4	3	10	2	8		5		2		1		101	5.3
11	7	12	3	11	2	6	2	9	4	4	2	12	2	10	5	9	2	8	1	1		112	5.9
12	2	9	4	11	1	2	1	5	2	4	1	8	1	9		5		5		1		71	3.7
13	1	5	1	7				5		4		6		7		3	1	4				44	2.3
14	2	6		1	1	3		3		3		5		6		4		3				37	2.0
15	3	6	1	3	1	3	2	6	3	6	2	5	1	5		4		2				53	2.9
16	2	5	1	2	6	6	1	3		2		4		4		4		2				42	2.2
17	3	4	2	4	2	3	2	6	1	5	1	6		3	2	3	2	5				54	2.8
18	1	4		1	2	3	1	4		2		4		3	1	5		2				33	1.7
19	1	3	1	1		1		3		2		2		2		2		2				20	1.1
20	1	3		1		2	1	3		1		1		1		2		1				17	.9
21	2	5		1		2		2	1	3		1				1		1				19	1.0
22		3		3		2	1	2	1	1				1				1				15	.8
23	1	2		1				1				1		1				1				8	.4
24		1		2		1		1		1		1				1		1				9	.5
	From here only one subject continues through eighteen more trials with errors distributed in parts of the maze as follows																						
25 to 42	2	19	2	4	1	3		3	2	3	3	9	2	5		4		16				78	
Ret.	229		207		116		141		93		81		64		56		44		10			= 1,041	
Odd Blinds		331				183				153				161				135				= 963 (type 2)	
Even Blinds				303				256				215				139				39		= 952 (type 1)	
																	Grand total					= 2,956	

[1] In this table the order of lettering is that shown in Fig. 1, used for Group 1. Group 2 responded to a different lettering of parts of the maze, but the parts were identical with those here indicated.

entrance to blinds near the goal are fully overcome under normal conditions, the final stages of correction of errors nearer the entrance (errors which persist longer) occasionally throw the subject into enough confusion to cause additional entrances to blinds later encountered. This agrees with the writer's results on twenty-four white rats, published in the monograph already cited.

Table I. shows also a general tendency for the errors near the goal to be eliminated first. The persistence of entrances to some of these blinds by a few of the subjects, who did not quickly get hold of the principle that repetition of letters already called out means that the path can be shortened, tends to obscure this general backward elimination tendency somewhat. A judgment as to the nature of this tendency from the absolute number of errors shown in this table is, however, to be made with some degree of caution; for the number of errors near the goal is smaller than that of errors near the entrance, in conformity with the expectations based on the law of chance. If learning advances at an equal rate, absolutely, in all parts of the maze, which it may or may not do, we should expect the earlier disappearance of errors near the goal. We shall consider in a subsequent paragraph the question of the relative rate of learning per unit of practice in the different parts of the maze.

Table II. gives the medians of the last trials in which the several nineteen subjects entered the different parts of the maze, and shows, therefore, in a manner different from that employed in Table I., that in general the errors nearer the goal are eliminated first. A graphical representation of these data is given in Fig. 2. Table II. and Fig. 2 also show that errors of return toward the entrance on emergence from blind alleys are eliminated earlier in the learning of the maze than are errors of going into the blinds. The unnumbered parts of the maze—the parts lettered P, V, I, K, E, B, T, C, Y, and S, contain all the errors of return. On the average the return errors were eliminated 2.1 trials earlier than were the errors of blind-alley entrance. The ratio of 2.1 to the median of the total number of trials to learn the maze, 11, is an appreciable amount, showing that the early elimination of return runs is a general law supported by our mental maze experiment as well as by our experiments on rats in different forms of mazes.

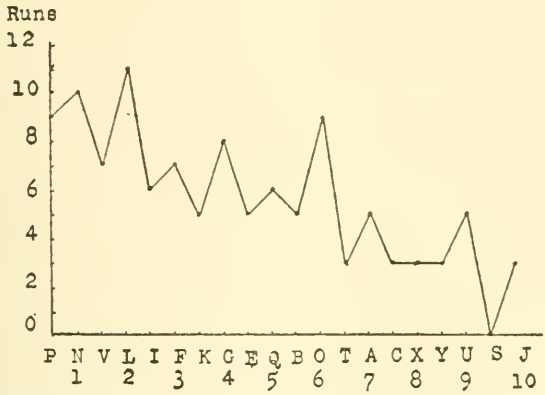


Fig. 2. The letters and numbers under the horizontal line indicate different parts of the maze, and the vertical units indicate the number of the trial on which the last error was made, each number being the median of nineteen subjects. Errors near the goal were eliminated first, and errors of return were eliminated before errors of entrance to blinds.

TABLE II

THE NUMBER UNDER EACH PART OF THE MAZE INDICATES THE MEDIAN OF THE LAST ENTRANCE TO THAT PART BY THE NINETEEN SUBJECTS. NUMBERS ABOVE THE LETTERS STAND FOR BLIND ALLEYS

Parts of Maze

	1		2		3		4		5		6		7		8		9		10	
P	N	V	L	I	F	K	G	E	Q	B	O	T	A	C	X	Y	U	S	J	H
9	10	7	11	6	7	5	8	5	6	5	9	3	5	3	3	3	5	0	3	

There are yet other facts pointing to the law of early elimination of return runs. On the basis of probability, strictly, there should be just half as many return errors as errors of entrance to blinds, since the chance is 1 : 1 that a return run (using the term "run" in the mental maze as we have used it in respect to the animal maze) will be made on the subject's emergence from a *cul de sac;* and if a special habit of keeping the forward direction does not develop, we should expect this ratio of 1 : 2 of the number of return errors *to* the number of blind-alley-entrance errors, to remain constant. Table III., which gives these two kinds of errors separately as well as combined, shows that this ratio, column (4), rapidly decreases from .83, the highest value it has, to 0. This fact, that the forward direction habit develops much

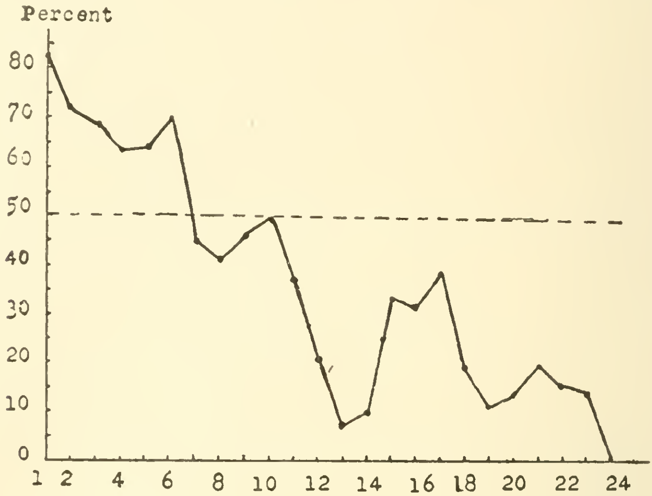


FIG. 3. Units on the base line are trial numbers, and those on the vertical line indicate the ratio of the number of return errors to that of blind-alley-entrance errors. Ratio starts above the expected .50 and rapidly decreases, showing the development of the habit of keeping the forward direction in the maze.

faster in maze learning than do the avoidance-of-blind-alley habits, seems to be of much importance in a proper understanding and analysis of maze learning, though until of late it has been wholly overlooked. This change in the ratios represented in Table III. is shown graphically in Fig. 3.

Table III. and Fig. 3 show clearly, it should also be remarked, another tendency of importance to maze learning, and probably to any kind of actual learning as distinct from mere development of skill in a mechanical performance. These data show that our human subjects had a strong tendency, persisting through the first six trials, to stay by the old and familiar rather than to venture into the novel, a tendency that in this case directly hindered learning and, as the individual records show, differentiated markedly between the good and the poor maze learners. This tendency is shown in the table and graph by the fact that the ratios mentioned

TABLE III

COMPARISON OF THE TWO IMPORTANT KINDS OF ERRORS THROUGH SUCCESSIVE TRIALS IN THE MENTAL MAZE (19 SUBJECTS)

Trial	(1) B. A. Errors	(2) Return Errors	(3) Total Errors	(4) Ratio (2) to (1)	Trial	(1) B. A. Errors	(2) Return Errors	(3) Total Errors	(4) Ratio (2) to (1)
1.....	295	245	540	.83	13.....	41	3	44	.07
2.....	218	154	372	.71	14.....	34	3	37	.09
3.....	159	108	267	.68	15.....	40	13	53	.33
4.....	152	95	247	.63	16.....	32	10	42	.31
5.....	159	101	260	.64	17.....	39	15	54	.38
6.....	116	81	197	.70	18.....	28	5	33	.18
7.....	107	48	155	.45	19.....	18	2	20	.11
8.....	73	30	103	.41	20.....	15	2	17	.13
9.....	70	32	102	.46	21.....	16	3	19	.19
10.....	68	33	101	.49	22.....	13	2	15	.15
11.....	82	30	112	.37	23.....	7	1	8	.14
12.....	59	12	71	.20	24.....	9	0	9	.00

exceed, in the first six trials, the .50 to be expected on the law of probability. The expected ratio is very greatly exceeded in the first trial, in which the effects of the learning were least marked, reaching .83 or 166 per cent. of what is expected by chance. The individual differences with respect to this tendency are so noticeable that the writer feels the tendency will be much more marked in the case of groups of subjects of low mental caliber than in that of subjects of fair ability. Whether this difference among individuals could be made helpful in diagnostic work is a question that we must leave for the present time, one that seems to be worth following out in separate experiments by students having considerable contact with lower grades of mentality. The tendency thus to repeat unprofitable responses in the maze seems not to be very different from the perseverative tendency noted by various experimenters on low grade and on certain types of disorganized minds. Smaller differences in this regard between normal subjects should not, however, be exaggerated as to their significance. What we need just at present is more data.

It seems, then, that the first important step in learning the maze is that of keeping the forward direction, of venturing into ever new choice alternatives. Some of the more clever subjects seemed early to recognize explicitly that return was unprofitable, and therefore to guard against it; but often the avoidance of the return seemed to come with profitless repetition more or less unconsciously. Even when the principle of infallible assistance to keeping the forward direction was not explicitly recognized, as was ascertained by quizzing the subjects after the experiment was over, subjects often *behaved as if they had the principle in mind.* The principle is that of choosing in any subsequent choice the one of the letters that occurred as one of the alternatives—the rejected one—in the previous presentation. For example, if the experimenter calls out N and V and the subject chooses N, then V must be one of the next alternative choices; if then V is chosen, L and I are presented. If I is chosen L is not repeated, but if L is chosen I must be repeated with V for the next choice. The rationale of this is not, of course, evident even to the best subjects, for they do not see the maze; but they often notice the simple *fact* of such repetition of one of the letters after certain choices which they soon learn to be erroneous, and the absence of it with other, or correct, choices, and consciously and rationally govern themselves accordingly. But, and this is the interesting thing after all, subjects who do not have such explicit recognition of the principle, nevertheless show in their behavior that they are governed by it, as they must be to take this first step in mental maze learning. They, at least, show no recognition of the principle in their unsolicited remarks during the experiment, or in answers to questions seeking evidence of such recognition, after the learning process has been completed. As Swift and other experimenters have found regarding the unconscious adoption of better methods and movements in ball-tossing, so we find here, that our subjects were often forced by the general fruitlessness of their reactions to adopt other modes of response resembling principles or rules of action, without any explicit recognition of the principles. The explicit recognition of a principle often comes after the student has unconsciously followed it for some time. Not infrequently the principle considered here is adhered to only in part, and reactions that fail to follow it throw the subject out of keeping the forward direction. Thus a subject given the choice between T and O may choose O, and when the next pair of alternatives, T and B, is presented he may by some accident choose B, after which, following the rule closely, he is taken back entirely to the starting place in the maze. This error occurred occasionally whether or not the subject explicitly recognized the principle of choosing the letter repeated. It may even be doubtful, as a study of rational learning by the writer seems to indicate it is, whether explicit recognition of a necessary principle in any case of learning is really a *sine qua non* of the learning. This question we cannot follow out further in the present paper.

Another point of interest may be considered here. From our statement on the second page, of the relative number of times that each blind alley would be passed in each trial by a subject following strictly the law of probability, we can figure how many errors may be expected at each part of the maze by our total of nineteen subjects, and can then compare this expected number with the actually obtained number. On the basis of chance there should be two thirds as many errors of entrance into the several blinds as there are runs made past them; for the chance of entrance to any blind is 1 : 1, and the chance of passing the blind, whether or not it is entered in 3 : 1. But here we are at once confronted by two difficulties. First, as a result of the learning that each subject shows increasingly toward the end of his practice, the number of runs past the blind alleys will, of course, greatly increase proportionately over the number of errors of entrance into them. But if learning goes on in equal proportion to the amount of exercise in all parts of the maze, we may obviously disregard this fact that the errors at each bifurcation gradually approach zero as the learning approaches perfection. If learning goes on more rapidly, proportionately, at one part of the maze than at another, this fact ought to reveal itself by relatively fewer errors all told at this point. Entrance into the blinds would discontinue earlier, comparatively, or would at any rate occur less frequently in proportion to exercise at the place in question, than at other places in the maze. We thus avoid the first possible difficulty.

The second difficulty that we meet in figuring the expected number of errors at the several parts of the maze is a statistical one. It will be recalled that we found the relative number of runs past each blind alley in the forward direction in n trials to be as follows from the first to the tenth blind, respectively: $13.33n$, $10.00n$, $7.50n$, $5.62n$, $4.22n$, $3.16n$, $2.37n$, $1.78n$, $1.33n$, and n. The determination of these numbers was based, however, on the assumption of complete return runs. In our present experiment, as in most experiments on animal learning in the maze as well, return runs are seldom complete. An animal returning from the tenth blind, for example, is very apt to enter a blind on its return run and to get started again in the forward direction as it emerges from this blind. Our coefficients of n are therefore too great for the blinds near the entrance or beginning place in the maze, and it is no simple matter to correct these coefficients from the effect of the assumption of complete return runs; for an animal in the maze may be sent back and forth many times and in diverse ways, according to the expectations of probability, before reaching the food box. The difficulty is not an insuperable one, however.

On the basis of chance one half of the runs *reaching* any blind will become entrances into it. The entrances to the tenth blind will, therefore, equal one half of $1.33n$, the runs past the ninth blind, or $.67n$. Or otherwise derived, the number of entrances to any given blind is equal to two thirds the number of runs past it, the forward direction runs only being considered, since, as has been shown, the runs past equal three fourths the runs to it ($\frac{1}{2} + \frac{1}{4}$), and the number of runs into it equals one half the runs to it ($\frac{1}{2} : \frac{3}{4} = 2 : 3$). Thus $\frac{2}{3}$ of n, is $.67\ n$, the number of entrances to the tenth blind. It is to be remembered that on the basis of the criteria of errors adopted in the present study—any entrance to a blind on a forward run or a return toward the entrance of the maze on emergence from any blind entered on a run in either direction, being an error—there can be no errors of entrance to blinds on return runs. On the basis of these facts we have worked out Table IV.

Table IV

This Table Shows the Return Runs Past Each Blind Alley from Those Nearer the Goal-end of the Maze, on the assumption of Complete Return Runs from Each Blind. It also Shows the Number of Errors Expected at Each Blind on the Law of Chance, Corrected from the Effects of This Assumption. See Text for Explanation

	The Blind Alleys of the Maze									
	1	2	3	4	5	6	7	8	9	10
(1)	13.33n	10.00n	7.50n	5.62n	4.22n	3.16n	2.37n	1.78n	1.33n	n
(2)	8.89n	6.67n	5.00n	3.75n	2.81n	2.11n	1.58n	1.19n	.89n	.67n
(3)	4.44n	3.33n	2.50n	1.86n	1.41n	1.05n	.79n	.59n	.44n	.33n
(4)	.025n	.03n	.04n	.06n	.08n	.11n	.14n	.19n	.25n	Returns from 10th
(5)	.04n	.06n	.08n	.11n	.14n	.19n	.25n	.33n		" " 9th
(6)	.08n	.11n	.14n	.19n	.25n	.33n	.44n			" " 8th
(7)	.14n	.19n	.25n	.33n	.44n	.59n				" " 7th
(8)	.25n	.33n	.44n	.59n	.79n					" " 6th
(9)	.44n	.59n	.79n	1.05n						" " 5th
(10)	.79n	1.05n	1.41n							" " 4th
(11)	1.41n	1.86n								" " 3d
(12)	2.50n									" " 2d
(13)	5.68n	4.22n	3.15n	2.33n	1.70n	1.22n	.83n	.52n	.25n	Totals
(14, 3)	4.44n	3.33n	2.50n	1.86n	1.41n	1.05n	.79n	.59n	.44n	.33n
(15)	10.12n	7.55n	5.65n	4.19n	3.11n	2.27n	1.62n	1.11n	.69n	.33n
(16)	1.00n	1.00n	1.00n	1.00n	1.00n	1.00n	1.00n	1.00n	1.00n	1.00n
(17)	11.12n	8.55n	6.66n	5.19n	4.11n	3.27n	2.62n	2.11n	1.69n	1.33n
(18)	8.34n	6.42n	5.00n	3.90n	3.08n	2.45n	1.97n	1.58n	1.27n	1.00n
(19)	5.56n	4.28n	3.33n	2.60n	2.05n	1.63n	1.31n	1.05n	.85n	.67n = 23.33n

The first row of the table shows the relative number of runs past each blind in the forward direction, on the assumption of complete returns. The next row gives the expected entrances to the several blinds in these forward runs. The third row shows the number of return runs expected from each blind on emergence of the subject from the blind. It is clear that one half of the choices on emergence from the blinds will be in the wrong direction. We must figure, then, the relative number of returns from the tenth blind that will *pass* the ninth, the eighth, the seventh, the sixth, the fifth, the fourth, the third, the second, and the first blind, respectively. Obviously, of the $.33n$ returns from the tenth blind, three fourths of this number, or $.25n$, will get past the ninth blind; three fourths of these, or $(\frac{3}{4})^2$ of $.33n$, will get past the eighth blind, that is, $.19n$ runs; $(\frac{3}{4})^3$ of $.33n$, or $.14n$ will pass the seventh; and so on, as shown in the table. Only $.025n$ of the $.33n$ returns from the tenth blind will pass the first blind. The same procedure is followed for the return runs from the ninth, the eighth, the seventh, the sixth, the fifth, the fourth, the third, and the second blind. The results of these calculations are clearly indicated in the table. Then summarizing all these returns past each blind in their respective columns, we get row (13), which when added to row (14, 3), the returns from the several blinds, gives us for each blind the total returns past or from it toward the entrance, corrected from the effects of the assumption of complete returns, row (15). Now since each trial must end with one more forward than backward run past each blind, it is clear that to these totals we must add n, to get the *corrected number of forward runs past each blind according to chance only.* This gives us row (17) in the table. But in this result we get $1.33n$ runs past the tenth blind alley on our assumption of n trials, which is impossible. We must therefore reduce the values of row (17) by the ratio $n : 1.33n$, or $3 : 4$, by which process we get row (18). Thus having the realtive number of runs in the forward direction past each blind in the maze, we can easily find the expected number of errors of entrance into each blind according to our present criteria and conditions of experimentation.

Since the ratio of errors at each blind to runs past the blind is ⅔, as we have already seen, we need only take ⅔ of the values given in row (18). In row (19), then, we have the expected number of errors of entrance into each blind from the first to the tenth, corrected from the errors of any assumptions not in agreement with the conditions of the present experiment.

We can now proceed to compare our obtained distribution of errors of entrance into blind alleys with the distribution expected on the basis of chance. The sum of all the expected errors as given in the eighteenth row of Table IV. is $23.33n$. Dividing 23.33 into 1915, the total number of blind-alley-entrance errors made by our nineteen subjects, we get 82.08, the value of n for the present calculation, as is evident from the equation,

$$23.33n \text{ (expected errors)} = 1915 \text{ (obtained errors).}$$

We can then determine how our obtained errors should be distributed by chance purely, by multiplying each of the coefficients of n in row (19) of Table IV. by 82.08. Table V. gives the results thus obtained, the expected errors at each blind, and it also gives the number of errors actually found at each blind by our total of nineteen subjects. The ratios

TABLE V

DISTRIBUTION OF ERRORS EXPECTED ON THE BASIS OF CHANCE, AND OF ERRORS ACTUALLY MADE BY NINETEEN SUBJECTS IN THE TEN BLIND ALLEYS OF THE MAZE

Blind Alleys	Errors in the Respective Blind Alleys										Total
	1	2	3	4	5	6	7	8	9	10	
Errors expected	456	351	273	213	168	134	108	86	70	55	1,914
Errors obtained	331	303	183	256	153	215	161	139	135	39	1,915
Ratio, exp. to obt.	1.38	1.13	1.49	.83	1.10	.62	.67	.62	.52	1.41	

of the expected to the actually obtained errors are also given. The larger the ratio is at any part of the maze, the greater has been the learning rate, proportionately, at that place. It will be seen by an examination of the table that the expected errors exceed the actually obtained errors noticeably at the first three, and the fifth, blinds, and also at the last blind. This we interpret to mean that the subjects had to develop rather specifically the principle of avoidance of blinds—and that roughly the first and the last members of the series of associations to be made were the easiest formed. This is in agreement with results usually obtained in the formation of relatively short series of associations. On the whole, then, transfer effects from one part of the maze to another seem to be slight, despite the fact that a simple rule when once learned in any part of the maze will almost infallibly, if not entirely so, guide the subject through other parts of the maze without return runs. It is true that the rule will not prevent entrances into blinds, but it will prevent return runs, which often increase greatly such entrances. The experimenter believes that the amount of transfer in this sense, and measured as here suggested, will correlate fairly highly with the intelligence of the subject. This, however, is a matter that we can not discuss in the present paper.

It is important to note that this fact of different relative rates of learning at different parts of the maze, has no relation to the backward elimination tendency in maze learning, also found to hold with our subjects in the mental maze, for we are now considering learning in relation to the proportion of actual practice in different parts of the maze; whereas the backward elimination tendency was found to depend on the greater learning coefficients (presently to be defined) of the blinds near the goal-end of the maze. The very low ratio of .52 at the ninth blind is probably due to the many errors of one subject at this point, errors persisting down to the second from the last trial for this subject, that is, far beyond the point of complete learning by all the other subjetcs. Our data, especially those for later trials reached only by a few subjects, need verification on many more subjects, a matter that is not so difficult now that the statistical calculations have been made. The writer, however, believes that the tendency here shown, for the learning to be more rapid at the two ends of the maze, irrespective of the backward elimination tendency, will hold.

With the data shown in Table IV., giving the relative number of errors actually to be expected at the different blind alleys on the law of probability, we are able finally to take still another step of importance to the theory of maze learning: we can improve upon the results shown on our second page, and can find mathematically the relative advantage at each blind of exercise in going *past* over exercise in going *into* the blind. The results of such determinations are given in general terms in the left half of Table VI. The next two columns of the table give the ratios when $n = 1$

Table VI

Learning Coefficients at the Several Blind Alleys of the Maze, also Comparisons of Advantage for Learning at the Tenth Over that at the First Blind Alley

Blind Alleys	Learning Coefficients			Value of n	Advantage for Learning at the 10th Over that of 1st Blind Alley for Different Values of n
	In General Terms	When			
		$n = 1$	$n = 2$		
1st.....	$8.34n : 8.34n - .25$	1.031	1.015	1	1.29[1]
2d......	$6.42n : 6.42n - .25$	1.041	1.019	2	1.13
3d......	$5.00n : 5.00n - .25$	1.053	1.026	3	1.08
4th.....	$3.90n : 3.90n - .25$	1.068	1.033	4	1.06
5th.....	$3.08n : 3.08n - .25$	1.088	1.042	5	1.05
6th.....	$2.45n : 2.45n - .25$	1.114	1.054	—	——
7th.....	$1.97n : 1.97n - .25$	1.145	1.068	10	1.02
8th.....	$1.58n : 1.58n - .25$	1.188	1.086	—	——
9th.....	$1.27n : 1.27n - .25$	1.245	1.109	20	1.01
10th.....	$1.00n : 1.00n - .25$	1.333	1.143	∞	1.00

and 2, respectively. These ratios we have called the *learning coefficients* at the several blind alleys. The learning coefficient at any blind is the ratio of the expected number of runs past that blind in the forward direction *to* the expected number of runs into it (from either direction). It will be noted that the learning coefficients for the two values of n given in the table gradually increase from a small value at the first blind to an appreciably greater value at the tenth blind. A coefficient of 1.00 indicates no learning possibility, so far as frequency effects—the only ones considered here—are concerned; for

[1] This advantage is found by dividing the learning coefficient at the 10th blind alley by that at the 1st; *e.g.*, $1.333 \div 1.031 = 1.29$.

in the case of a blind with such a coefficient, the exercise in running into the blind exactly equals that of passing it.

It will be seen that the learning coefficients not only constantly decrease from the tenth back to the first blind, but that this advantage for the blinds near the goal constantly decreases with increasing values of n. To make this plain we have calculated the advantages at the tenth over that at the first blind alley, when n is 1, 2, 3, 4, 5, 10, 20, and infinity, respectively. This advantage is found by dividing the learning coefficient of the tenth blind by that of the first, for any value of n. Our results, given in the last column of the table, show a constantly decreasing advantage for the tenth blind over the first as the values of n—that is, the number of trials by any subject—increase. When n becomes very great the learning advantage of the near-goal blinds becomes negligible. This is due to the fact that the learning coefficients at all the blinds approach unity as their limit. This general law of decreasing learning advantage of the tenth over the first blind with increasing values of n is important. It suggests one reason why learning in mazes goes on more rapidly in the early trials (when n is small) than in later ones. Another reason has already been pointed out by the writer in the monograph cited above, namely, that the return runs on emergence from blinds disappear relatively early in the learning of the maze, and with them all the probabilities of error due to re-passing blinds nearer the entrance of the maze.

The table in question also makes plain two minor points of interest in maze learning: first, that the learning coefficients reach practically a zero value, a ratio of unity, earlier in the learning process for the near-entrance blinds than for the near-goal blinds, this being really a corollary of what has already been said; second, that the learning advantage for the near-goal blinds over the near-entrance blinds rapidly increases for low values of n—or in the early trials of any subject—with increase in the number of blinds in the maze. From this latter fact we should expect that backward elimination of errors in the maze would be much more marked in mazes with many blinds than in mazes with but few blinds.

We have yet no data from mental mazes of different degrees of difficulty to verify this deduction. It is obvious, moreover, that mazes do not increase in difficulty directly with increase in the number of blinds.

Summary

We have developed an empirical method of studying analytically one of the kinds of learning used in many laboratories, namely maze-learning. Our method applies only to human subjects, but it enables us to study maze-learning independently of the various spatial factors peculiar to individual mazes, and thereby better to determine the general laws involved. Our results from nineteen subjects show convincingly, not only that backward elimination of blind-alley entrances is the rule in maze learning, but also that return runs on emergence from blinds are eliminated earlier in the process than are blind alley entrances. Subjects learn both consciously and unconsciously to follow a simple rule the value of which develops in their random efforts, a rule that leads them to depart from the unprofitable repetitions and to venture into new parts of the maze. There is no qualitative difference between the reactions of subjects who recognize this rule explicitly and those of subjects in whose minds the rule does not come out consciously. In this tendency to depart from the repetition of profitless choices, to go against the valueless effects of frequency, subjects differ materially, the better learners showing greater initiative than the poorer ones.

The experimental results are at every hand compared with expectations based on the law of probability, and mathematical determinations of the learning coefficients at each of the blind alleys are made. The advantages for learning at the tenth blind over those at the first blind are found, and it is shown that this advantage rapidly decreases with successive trials by each subject. This probably is one reason for more rapid learning in the early trials, another being the fact that return runs are eliminated relatively early as compared with errors of entrance to the several blinds.

A method is worked out also of comparing the distribution

of errors in the maze with the expected distribution based on chance, and it is found, in agreement with other experiments on association, that the first and the last members of the series (parts of the maze here) are mastered more readily than are the members between the extremes. Finally it is found that the effects of learning in one part of the maze are not noticeably transferred to other parts. A number of problems for further investigation are suggested.

SOME VOLITIONAL PATTERNS REVEALED BY THE WILL-PROFILE

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I

Descriptive psychology has familiarized us with various classifications of temperament and of character-types. The four traditional temperaments have been interpreted and re-interpreted in attempts to fit them into various structural systems. At times, the predisposition to experience a certain *quality* of emotion has been chosen as the basal factor in making the division; at other times, the *strength* and *persistence* of the emotional reaction has been considered fundamental and its quality subsidiary. Again, readiness and vigor of motor response; or, rapidity and smoothness of thought is made the principle of division. That is, in the discussion of temperamental and character types we find a varying emphasis upon emotional, intellectual, and motor factors, with more or less insistence upon somatic conditions. Probably physiological experimentation will in the future furnish us with material for a basal interpretation from this latter standpoint.

The net outcome from reading upon this topic is a feeling that there exists some psychological justification for broad outlines of characteristic patterns; the refinement of these patterns has, however, proceeded largely on a logical basis, which in such a classification as that of Ribot's[1] leads to so great an overlapping of divisions as to raise the question of the existence of any real types. Ribot himself concludes that the majority of men are amorphous (plastic to excess), a character-brand determined wholly by environment; or else partial characters or types, that is, patterned along some one line of professional interest only.

[1] 'Psychology of the Emotions,' p. 380 f.

My purpose in the present paper is to present an experimental approach to the problem. Specifically, I wish to discuss certain volitional patterns which have been revealed through employment of a series of tests designed for a volitional analysis of the make-up of an individual in supplementation of his intelligence rating. The emphasis is distinctly on degree of readiness, vigor, suppleness, and accuracy of motor reaction, and on smoothness, confidence and rapidity of thought reaction. If the tests reveal emotional predispositions they do so indirectly, in connection with a specific combination of volitional traits.

The volitional patterns are given in the form of graphs plotted in terms of the scores on a percentile basis received on the following traits: Speed of movement (measured by speed of normal writing); freedom from load or inertia (measured by the ratio of time of speeded to time of normal writing); flexibility (measured by expertness in disguise of one's habitual hand and in imitation of another's handwriting); speed of decision (measured by time taken to decide which one of a series of paired character traits one possesses); motor impulsion (measured by amount of increase or decrease in size and speed of writing when attention is distracted); assurance (measured by reaction to contraction); resistance (measured by blocked writing); motor inhibition (measured by the length of time one can retard his writing of a phrase); care for detail (measured by accuracy of imitation and by difference in time between a rapid and a slow imitation); coördination of impulses (measured by success in writing a given phrase on a short line in speeded time); perseverance (measured by length of time one employs in preliminary practice and final writing of a disguised hand); revision (measured by amount of time taken in rechecking one's first set of judgments on character traits). Discussion of the individual tests in the series, directions for giving them, and norms for scoring are presented in another connection.[1] Plate I. gives a sample profile.

[1] 'The Will-Profile,' University of Wyoming, Department of Psychology, Bulletin No. 3, 1919.

The vital question in estimating the value of the graph is this: Does it have any general characterologicol significance? Or is its meaning specific, limited to exhibiting an individual's reaction to a particular material? Of the twelve tests, eight, it will be noticed, utilize handwriting in some form. The series includes speeded, retarded, disguised, blocked, and automatic handwriting, slow and rapid imitation of script and speeded writing in a restricted space. In many cases the reaction from this set of tests is somewhat definitely patterned. A relatively high score on the first four tests indicates a quick flexible reaction; on the second four traits, it suggests an aggressive reaction; on the last four, a deliberate, methodical, careful reaction. Study of actual graphs reveals eight possibilities in the way of pattern; three one-peaked patterns, emphasizing any one of the three possible groups of four traits each; three two-peaked patterns emphasizing any two groups of traits; a balanced reaction maintaining about the same level throughout the graph; and, lastly, a zigzag pattern which rises and falls without any definite plan so far as the adopted arrangement of traits is concerned. A pattern may run its course at several different levels. We may now rephrase our question to read: Do these types of reaction reveal anything more than the subject's organization of graphic (writing) habits?

It may seem that the proper answer to this question should be sought in a checking up of the significance of each test by correlation of the results it gives with those obtained by a different sort of test of the same trait. Or the order of merit method might be utilized; a ranking should be obtained for a group of subjects on each character trait, and this ranking correlated with the rank received on basis of a given test. So far as is possible, I hope eventually to check some of the individual tests by these methods. Something has already been done. For many of the character traits there are, however, no tests yet available for correlation. Nor is the method of relative position wholly satisfactory because the persons to whom I must appeal for judgments are unpracticed in passing such judgments, reluctant in attempting them, and, usually, unequally acquainted with the individuals in the group.

These difficulties in the way of checking the significance of specific tests have induced me to try utilizing the graph itself in a rough determination of the extent to which the tests have general characterological significance or the validity of the pattern as actually a volitional pattern. The results of the investigation now to be recorded are not only positive enough to be highly encouraging but the method has given me material that can be utilized in further revision of the tests, and it has suggested possibilities in the way of working out tests of expertness in character analysis.

At this point I would like to emphasize the statement that both the tests and the scoring of them are still in process of revision. For the normal adult between the ages of eighteen and fifty years, the norms established are fairly satisfactory except for one detail. Where sex and age differences exist to any degree the norms are vitiated by the fact that nearly two thirds of my subjects have been women and in age under thirty-five. It appears from separate tabulation of records that in the case of motor inhibition the central tendency for men is much higher than that for women so that with an equal sex distribution of subjects my norms for this trait would be somewhat different. Furthermore, after a long series of repeated trials on a given group, I have concluded that the third trial of retarded writing is more significant than the first and in the revised scale I shall advise giving three trials of retarded writing, except in cases of excessive retardation. This will necessitate the establishment of new norms. I am also revising instructions so as to rule out, if possible, an automatic reaction to this test.

Other details will be handled in revision. I am checking out, for example, a second method of scoring freedom from inertia. The present scoring does an injustice to very rapid workers who are capable of very intensive spurting.

II

At present, however, I propose to pass over such details and to turn at once to the experiment in question. My procedure was, at first, as follows: I chose three groups of graphs,[1] twelve obtained from college men, twelve from college women, and twelve from university instructors. A group of twelve was submitted to a chosen reagent with a list of names. His task was to fit each profile with the proper name. The instructions given him are reproduced verbatim.

Instructions

You are to identify, if possible, the Will-Profile that fits each person on the list of names given you.

The profiles are graphs which represent the score attained by a given individual on a number of given traits. The method of scoring is as follows:

A score of 10 is the highest given. It would be received by the upper ten per cent. in a group of 100.

1 would be the score received by the lowest ten per cent. in a group of 100.

The other scores grade between 10 and 1 by equal intervals. The traits scored may be defined as follows:

1. Speed of Movement: Speed of movement relative to size of person, and age.
2. Freedom from Inertia or Load: Tendency to work at one's highest speed without external pressure; little tendency to relax speed; quickness in warming up to a task.
3. Flexibility: Ease and success in readjustment; capacity to modify one's routine reactions.
4. Speed of decision: Quickness in reaching a decision or conclusion. A slow reaction here may be due to caution or conservatism in weighing the elements involved in a situation or be caused by one's being side-tracked by irrelevant matters or by a rambling procedure.
5. Motor Impulsion: This trait refers both to impetuosity and energy of reaction. Consider the ease with which brakes or inhibitions are removed and also the tendency to an explosive reaction when the brakes are actually off.
6. Assurance: This refers to the degree of confidence with which one maintains his opinion against contradiction. A 9 or 10 reaction signifies an aggressive reaction —the burden of proof is thrown on the person who does the contradicting; 7 and 8 are confident reactions but reasons are cited for one's confidence and the burden of proof is accepted; 5 and 6 are tactful reactions; below 5 there is a grading down to complete failure to assert one's own opinion.
7. Resistance: The vigor with which one reacts immediately to a blocking of one's purpose. It grades from a strenuous reaction, to complete passivity in the face of opposition.
8. Motor Inhibition: Capacity to keep in mind a set purpose and achieve it *slowly*. It involves power of motor control, imperturbability, and patience.
9. Care for Detail: Attention to details. This trait is not equivalent to accuracy which usually carries an implication of power of keen analysis. One may posses great capacity for detail and yet lack penetration in the selection of details. Care for detail is more evident in execution of a plan than in cleverness in making a plan.

[1] These graphs were plotted for only ten traits. At the time satisfactory norms for Perseverance and Revision were not available.

10. Coördination of Impulses: Capacity to execute a double task without a preliminary trial; capacity to handle a complex situation successfully without forgetting either factor involved. This trait is probably allied to keeping one's head in a confusing situation.

In studying the profiles, give some attention to the general pattern as well as to the scoring on specific traits. In the case of a quick-reacting person the graph runs high at the beginning of the profile; it will run low at the beginning in the case of the leisurely deliberate type of person who carries considerable load. The central part of the profile runs high for the aggressive person. There is an emphasis of the latter part of the curve for the careful deliberate type.

One may find one-peaked patterns, two-peaked patterns, and balanced patterns *at any level of scoring*. Remember 5 and 6 represent a median score. A zigzag pattern demands particular study as it may represent a modification of the natural reaction by training.

Caution: Do not attempt to identify the profile of some one whom you know only by reputation or of whom you have only a general impression. Some specific acquaintance with an individual is necessary.

If you are in doubt as to the meaning of any point in the instructions, question the experimenter about it.

Actual experiment in connection with the attempted identification of profiles showed that the task required considerable effort of attention and power of analysis. Several of the traits measured, freedom from inertia, for example, were new concepts to the judges. In order to give a satisfactory judgment, the reagent felt the need of much more than a casual acquaintance with the persons whose profiles were being exhibited. Not many observers were intimately enough acquainted with each individual in a group of twelve to make satisfactory inter-comparisons. I obtained, however, one series of twelve judgments from each of twelve reagents,—four reagents for each of my three groups of profiles. Correct identification of profiles ran from zero to five, or from total failure to identify any profile (one reagent) to forty-one per cent. of successful identifications (two reagents). The per cent. of successes for the total of one hundred forty-four judgments (twelve judgments by each of twelve reagents) was twenty-two per cent., where chance success would be less than one per cent.

Much more valuable than these summarizing figures was the unmistakable evidence given by the experiment of several important facts:

1. The more intimately an observer knew an individual, the greater chance there was of his correctly identifying the latter's profile. Such a result not only evidences the general value of the profile in character study, but suggests that frequently it might be used by acquaintances in extension of their everyday knowledge of a person.

2. Even in case of incorrect identifications, the judgments, except for one or two very inexpert reagents, were not dictated by chance. There is often considerable agreement as to the *type* of profile chosen for a subject though the specific identification be incorrect. Such confusions are due to general similarity of pattern in the make-up of certain individuals.

3. Individuals vary greatly in their interest in, and capacity to estimate character traits. With a selected group of judges, successes on the will-profile would run fairly high. In this preliminary experiment, the best records were made by the Dean of the College of Agriculture, whose administrative duties enforce a practical interest in character analysis —and by a student of psychology, who undoubtedly has native capacity in this line.

In modification of the method of procedure in such a way as to be able to utilize the judgments of observers who might know intimately only a few individuals whose profiles had been plotted, and yet to insure a simple method of estimating chance versus actual successes, I decided to submit the profiles in groups of three, requesting the observer to choose which profile belonged to a given individual. Chance success of thirty-three and one third per cent. could be anticipated. In order to isolate certain factors for study, I arranged my material in two series. In Series *A*, I presented a given profile with two others like it in general pattern. This series is obviously a difficult one to pass judgment on. In Series *B*, I presented the same profiles but with contrasting patterns. Series *B* is relatively an easy one to handle. It was possible to enlarge my separate groups to any extent I desired, but the combinations in which any particular profile was presented were, of course, kept constant for all observers; furthermore, the profiles of men students were compared only with those of other male students; women students compared with other women students; university instructors with university instructors. This latter condition probably rendered the experiment slightly more difficult than if profiles had been taken indiscriminately from any of the groups.

The instructions used in the preliminary experiment were used again, except that the observer was told that after picking the one profile of the three that seemed most satisfactory, he might criticize it with respect to any score he chose. By this means I obtained some valuable suggestions as to possible defects in the tests. A majority of the observers had themselves taken the test some months before, and, occasionally, tried to think what a given individual would do under specific conditions. They had, however, no way of knowing how I had utilized the tests in getting a majority of the scores, and were, in addition, instructed to pass judgment on the general character of the subject. Some of the best series of judgments were given by persons who were wholly ignorant of the nature of the tests.

TABLE I

PER CENT. SUCCESSFUL IDENTIFICATION OF PROFILES

Profiles	Series *A*				Series *B*			
	Number of Judgments	% R. Whole Group	Highest % R. Any Judge	Lowest % R. Any Judge	Number of Judgments	% R. Whole Group	Highest % R. Any Judge	Lowest % R. Any Judge
Faculty	69	33.3	55.5	0	75	78.7	100	0[2]
Men Students	54	44.4	60.0	0	57	71.9	100	0[2]
Women Students	37	51.3	100.00[1]	0	31	58.6	100	0[2]
Total	160	41.3	100.00[1]	0	163	72.3	100	0[2]

The results of this test are given in Table I. Using the total series of judgments it is evident that identification of a profile occurs slightly more frequently than chance would lead us to expect even when the profile is submitted with

[1] Only one judgment given, next highest 75 per cent., (3 R. judgments out of a total of four).

[2] Only one judgment given.

others like it in general pattern; while when the same profile is compared with contrasting profiles, the probability that the correct profile will be selected is fairly high. Still more convincing of the value of the profile in analysis of one's temperamental pattern are the successes achieved by the best judges. Thus under the definitely difficult conditions of series *A*, a judge has been able to identify correctly in one group 6 of 11 profiles (55 per cent.) or in another group, 3 out of 4 (75 per cent.). In series *B*, there are a number of judges who make 100 per cent. of correct identifications.

Study of the table reveals an interesting possibility which an inspection of the graphs confirms. In the judgments on the profiles of University Instructors, the per cent. of successes is, in Series *A*, the lowest of the three; in Series *B*, highest. Such figures would suggest the possibility of this group of profiles being more highly patterned than those obtained from the younger subjects, and hence more easily confused when submitted for identification with similar patterns but for the same reason more easily identified in connection with contrasting patterns.

There can be no question of the fact that certain individuals present a much more highly patterned reaction to the test than do others, and that these patterned profiles are under certain circumstances more easily identified than zigzag patterns. The profile of the methodical, careful, deliberate type is, apparently, more easily identified than that of the quick flexible type, particularly if the latter combine aggressive traits with his quickness of reaction, in which case there is, I believe, a tendency to overestimate the probable score on all other traits. This type is, on the whole, also overestimated for intelligence when a judgment is based by the observer on general impression.

In Table II., I have tabulated the number of right and wrong judgments on a number of individual profiles, choosing those in which there was a number of right judgments even in Series *A*; and, secondly, those in which there was a number of wrong judgments in Series *B*. One other case is included.

First, a word of comment concerning those Profiles which are correctly chosen even in the difficult series. VI. and *H* are identified by the fact that they maintain a high level throughout the graph; VII. and 15 identified by the low level at which the pattern runs. *F* and 4 represent a very definite type of deliberate, consistent, stable personality, one of the easiest types to identify.

TABLE II

DISTRIBUTION RIGHT AND WRONG JUDGMENTS CHOSEN PROFILES

Profile	Series *A*		Series *B*	
	Right	Wrong	Right	Wrong
VI	8	2	9	1
VII	3	1	4	0
F	5	1	5	0
H	4	1	4	1
4	4	1	3	1
15	3	0	3	0
A	4	3	4	3
D	1	2	2	1
P	1	3	1	3
1	1	3	2	3
5	0	3	0	3
XII	0	9	8	1

The second group of profiles, which are not successfully identified even when presented with contrasting patterns, affords much material for study. Several possibilities demand comment.

1. The general appearance of an individual is sometimes deceptive. Age or physique masks the true nature except for very discriminating observers. Casual observation in class-room or faculty-meetings does not serve for adequate understanding of such a character. Profiles XII., *D*, and *P* may be used in illustration.

Profile XII.—The general impression received from the original is that of vivacity, alertness, quickness, and decisiveness. Subject is small, youthful, buoyant. This exterior masks very great care and deliberateness in forming a judgment and extreme sensitiveness to detail. Close observation reveals some inertia in her make-up.

In Series *A*, her profile is not once chosen correctly. But there is a curious difference in the profile selected respectively by faculty and by student judges. Her faculty colleagues chose a profile emphasizing quickness and flexibility of reaction. They look for a pattern in which the outstanding feature is speed of every sort. Students, on the other hand, look for aggressiveness of reaction and great care for detail. The correct profile runs high on speed of movement, flexibility, assurance, coördination of impulses and care for detail. It dips at speed of decision, freedom from inertia, and resistance to opposition. The original pronounces it an excellent representation, with a reservation concerning the score for coördination of impulses.

An interesting sidelight on this case comes in the choice of profile made by XII.'s housemate who knows her intimately. She overestimates the qualities of deliberateness and detail and chooses a pattern characteristic of a definitely slow methodical individual of much inertia. This reversal of emphasis in choice of pattern is due in part to the bias created by the judge's own very rapidly reacting, inertialess temperament.

Profile P.—This case is a similar one. I do not know the subject personally; the effect he gives to a casual observer is that of a quick, lively, energetic individual. His profile suggests a very stable, careful and methodical person. That he really is of the latter type is asserted by one of his intimate friends. I should suspect it from the fact that his scholastic record runs much higher than his ranking in the freshman tests would lead one to anticipate, a situation which leads one to expect a high record on the traits in the will-profile that emphasize industry and carefulness.

Profile D represents the contrasting case. Here a quick-reacting careless individual is chosen as deliberate and careful. There are two traits that do indeed suggest such a pattern; *D* runs low on motor impulsion and there is some evidence of inertia. Physically, he gives the impression of being slow-moving, phlegmatic and deliberate. As a matter of fact, as I have determined by many timed reactions throughout a year, his speed of decision is excessively rapid and his care for detail distressingly inadequate. He is not slow in movement but there are no excess movements.

His most intimate friend identifies his profile with no difficulty and pronounces it an excellent likeness; as I should, after a year and a half careful observation. But besides the judgments tabulated in the table, there were five failures to identify this profile in the preliminary series with, usually, choice of a highly contrasted pattern.

2. Occasionally, one finds a profile that is unsatisfactory to all or many reagents, but not for the reason just cited. Possibly, we may find a group of reagents of an unstable type whose pattern might fluctuate considerably with repetition of the test. The wavering reactions of observers on profiles *A* and 1 suggest such an explanation, as, in fact, do the personalities in question. They, as personalities, are in definite contrast to *F* and 4, mentioned above; they are much more complex individuals but also much less reliable. Repetition of the test should show much more pronounced shifts in scores for the latter group than for the former.

3. Some errors in identification arise from a strong tendency on the part of many judges to overestimate the possession by others of certain traits. For example, I found one reagent who insisted that every individual whose profile she was seeking to identify should score 10 for assurance. Students were definitely given to overestimation of scores for faculty profiles. One profile, that of an individual of really exceptional volitional make-up, is chosen in Series *A* in almost every instance in which it is submitted as one of a group of three.

4. That the bias of one's own temperament may lead one to overestimate the contrasting traits in others is evident from my records. I have already given an example of this in commenting upon Profile XII., and in mention of the reagent who overestimated the assurance of the individuals whose profiles she handled. I should expect such temperamental bias to operate particularly in estimating the ranking on care for detail. One who would score 10 would probably underestimate the carefulness of others.

5. As might have been anticipated, unfamiliarity with the terms used in analysis caused some difficulty in passing

judgment on a profile. Motor Impulsion, for example, means very little to a reagent. There is also failure to discriminate with reference to traits. The average observer, for example, fails to discriminate between excess movement and rapidity of movement. Smooth, well-coördinated movements are more rapid than those involving effort or excess of nervous activity but they attract much less attention than the latter.

6. That the profile itself needs correction in several details apart from those mentioned earlier in the paper, is also evidenced by the present investigation. The scores most frequently criticized in the detailed inspection were those for Assurance, and for Coördination of Impulses as a test of keeping one's head. It is possible that the test on which Assurance is scored really measures one's susceptibility to suggestion rather than one's self-confidence. The number of double-checks given in passing judgment upon one's own traits may serve as a better indication of the latter trait. This possibility I shall check out.

The significance of the test on Coördination of Impulses would seem open to criticism, not only on the basis of the present returns but also from indications given by the correlational diagrams, to be reported in the third part of the paper.

In a few cases the use of writing as the test material evidently accounts for inaccuracy in scoring on speed of movement and freedom from inertia. Reagents exceptionally practiced in writing may for this reason give an accelerated reaction; while others may show a specialized retardation of speed due to such inhibitions as writing with the right hand instead of the left hand, in the case of a left-handed person.

In concluding this part of the report, I wish to say a word or two concerning the varying value of different judges. A judgment is obviously inadequate when contradicted by the observer's own reaction when conditions are slightly shifted, as in Series *A* and Series *B*. One may also appeal to the judgment of the group or to that of the best qualified judge in checking over the conspicuous successes or failures of individual reagents. A number of the most successful reagents are of the slow deliberate type of reaction which in my experience is more often associated with a visual than with a verbal preoccupation. They had in this test the advantage of dealing with a visual scheme—the graph,—and some of them very definitely reacted to the patterns as patterns. One of my verbal-minded judges complained of the presentation of character traits in a visual form that meant nothing to him.

The judges who run low on motor impulsion appear more accurate in their identifications of character than the explosive type does, although, possibly, the latter set the social reaction more frequently. There is, apparently, an obsession on the part of the former group with character analysis, a beautiful confirmation of the suggestion that individuals with a temperament on the defensive are better judges of character than those who temperamentally take the offensive.

III

The general pattern that results from plotting the graph obtained by means of the volitional tests and the statement of the total score do not exhaust the usefulness of the profile in analysis of temperament. We have already encountered the zigzag or non-patterned profile, which leads to the problem of other possible organizations of traits, or of natural or acquired inconsistencies of reaction. This, in turn, involves the question of intercorrelation between tests employed in the Will-Profile. An intensive treatment of the data would, no doubt, make it possible to determine to how great an extent a given factor, such as speed of movement, operated in the various tests, and it might well suggest lines of possible revision so as to introduce either greater variety in the way of material or greater accuracy in details of the scoring.

As it will be impossible to handle the problem exhaustively in the present paper, I shall limit myself to a brief summary of the conclusions of relationship I have drawn from inspection of diagrams obtained by so plotting the distribution of scores as to show the relationship between any two pairs of measures. The diagrams not only reveal the presence or absence of any gross correlation but also make possible a detailed study of each particular score. When such scores stand for a qualitative reaction, such study may be particularly enlightening. One hundred records, equally distributed in the ten groups scoring from 1 to 10, were utilized for each test. A graph was then made to show the relationship between this test and each of the other eleven. The correlation or lack of it was studied, it will be seen, from two angles, since a selection of one hundred records was made twice for each test.

TABLE III

CORRELATION OF TESTS

	Speed Movement	Freedom Load	Flexibility	Speed Decision	Motor Impulsion	Assurance	Resistance	Motor Inhibition	Care Detail	Coordination Impulses	Perseverance	Revision
Speed of movement..		$+^1$	$+$	$+^2$		$+$			$-$	$+$	$-$	
Freedom from load........	$+^1$			$+$					$-(1, 2)$			
Flexibility	$+$			$+^2$	$+$			$+$	$+^1$	$+$		
Speed of decision.....	$+^2$	$+$	$+^2$				$+$				$-$	$-$
Motor impulsion........			$+$			$+^2$		?		?		
Assurance.....	$+$				$+^2$		$+$	$+$	$+$		$+$	
Resistance....				$+$		$+$		$+$				
Motor inhibition........			$+$		?	$+$	$+$		$+^1$	$+$		
Care for detail.........	$-(9, 10)$	$-(9, 10)$	$+^1$		$-(10)$	$+$		$+^1$		$+$	$+$	$+$
Coördination impulses....	$+$	$+$	$+$		?			$+$	$+$			
Perseverance..	$-$			$-$		$+$			$+$			$+^1$
Revision......				$-$					$+$		$+^1$	

Table III. summarizes my conclusions. Positive correlation is indicated by a plus sign; negative, by a minus; absence of correlation by a blank space. In a few instances correlation, or lack of it, is evident only for certain groups as indicated by figures in parentheses. The question-mark is used where the relationship is obscure. In the case of

[1] High correlation.

[2] Low correlation.

motor impulsion and motor inhibition, this fact will serve to introduce a somewhat detailed discussion of the interrelationship.

Table IV

Own Judgment	Groups					
	I	II	III	IV	V	Total
Impulsive	12(4)	8(5)	15(11)	8(2)	10(4)	53(26)
Deliberate	5(3)	12(0)	5(0)	11(3)	8(3)	41(9)
Qualified	3	0	0	1	2(0)	6
Cheerful	17	18(12)	15(5)	13(6)	19(11)	82
Gloomy	1	2	1	4	0	8
Variable	1	0	4	3	1	9
In Between	1	0	0	0	0	1

A few points in the table deserve particular notice. Where traits in the same group have been scored on reactions differing on the surface completely from one another, the correlation points to a common mental attitude influencing both tests and so far confirms the value of the profile in character-analysis. The striking instances of this sort are as follows: Speed of Movement and Speed of Decision (correlation low); Motor Inhibition and Assurance (low correlation); Assurance and Resistance (high correlation); Motor Inhibition and Assurance; Motor Inhibition and Resistance; Perseverance and Revision (high correlation).

But there are interesting cases of correlation between traits not grouped together in the profile. Thus Flexibility is found to be positively correlated with Motor Impulsion, Motor Inhibition, Care for Detail and Coördination of Impulses. Care for Detail is correlated not only with Flexibility but also with Assurance and with Motor Inhibition. Other interesting examples involve the correlation of Speed of Movement with Assurance and Coördination of Impulses; and the positive relationship between Speed of Decision and Resistance, and between Assurance and Perseverance.

Negative correlations are found for Speed of Movement and Perseverance; Speed of Decision and both Perseverance and Revision. These statements suggest a few comments.

The proper grouping of Flexibility and of Coördination of Impulses is problematic. The relationship found to exist between these two traits was quite unexpected although suggested by certain profiles I had plotted. Both appear to involve somewhat the same combination of motor control and speed of reaction. Whether or not such a combination of traits is akin to capacity to keep one's head is probably very questionable—a conclusion reached on other grounds in the second part of the paper. But that the test for Flexibility does actually measure capacity for general adaptability seems fairly conclusive, although an effort should be made to distinguish in the case of the disguised hand between a dramatic and a studied disguise, at which point my method of scoring is inadequate. That individuals who are notably inflexible fail conspicuously in this test is guaranteed by a number of interesting records.

The positive relationship found to exist between Speed of Decision and Resistance was also unexpected. I do not think, however, that the reason for it is obscure. Obviously, one factor in a quick counter to opposition is a speedy recognition of just what the situation is. I have noted in another connection that members of one of my groups showed very great force in resistance as soon as they had adjusted themselves to the situation. At first they were confused, "fussed"; they are slow-reacting, somewhat inhibited, assured individuals, but just the people whom we know to be excessively obstinate when once they are "set." Just how to revise the scoring so as to do them justice in this particular I do not see as yet.

The positive correlation between Assurance and Perseverance is I think accounted for by the fact that, in part, one's confidence is based on care in noting details with respect either to one's reasons for choice or to objective circumstances.

The most interesting negative correlation is that found to exist between Speed of Decision and Revision. It appears that those individuals who take the longest time to make their decisions in the first place, also spend most time in revising those decisions when given a second chance.

Let us now consider in detail the curious complications of motor impulsion and motor inhibition, since such an analysis may serve to indicate the sort of scrutiny necessary in estimating the significance of different combinations of traits.

The diagrams give no certain indication of either positive or negative correlation of these two traits. But study of individual groups indicates that subjects scoring 9 or 10 for inhibition rarely score over 5 for motor impulsion. The reverse is not true; a low score on inhibition does not suggest a high score on impulsion. There are, actually, four possibilities of combination. A subject may score high on both traits; low on both; or high on one and low on the other. These subsidiary patterns are, I believe, of great significance. A low score on both traits characterizes a rather definitely relaxed and unresistant individual; a high score for impulsion with a low one for inhibition, the explosive or impetuous person; a high score for inhibition and a low one for impulsion, the deliberate, or, even, the obstructed type; a high score on both, the vigorous forceful personality. But in this latter group, I find, again, two divisions, as revealed by the type of writing produced under instructions to retard movement to the greatest possible degree. For some, this writing shows signs of great tension; for others, not.

Signs of tension include decrease in graphic size, increased pressure, falling alignment, pronounced tremor, and increased conventionality. In the groups scoring over 7 for retarded writing there is some tendency to increase the size of the inhibited writing. This is due largely to the fact that increase in size is a natural outcome of any throwing of attention upon the production of individual letters. But in a few cases such increase in amplitude, with a noticeable absence of any signs of tension, is due to the fact that the reagent is having recourse to *automatic* writing. The tendency of some subjects to shift to an automatic control even in production of retarded writing was noticed in my early tests, with a consequent warning against it. Conversation during retarded writing was not permitted and penmen were urged to keep to their normal size. Inspection of the graphic product shows, however, that a small group of subjects executed the instructions with no signs of tension; quite the contrary; they produced a large, light hand characteristically automatic. This group of subjects proved to be those scoring exceptionally high on motor impulsion. A list of them gave me a number of individuals of a colorful dramatic type of personality. By some shift in control—whose mechanism we cannot now go into—it is evident that these subjects solved the problem of retardation by an automatic holding of a mental or motor set so that they achieved success with no signs of the tension so noticeably present in other reagents. Possibly restraint of movement by employment of narrow parallel lines might serve to rule out such a reaction, but I am not confident that it would do so.

I believe that to a very considerable extent it is possible to discover these four types of reaction in characteristics of the normal hand. This possibility gives experimental confirmation to a conjecture of mine,—made elsewhere[1]—that the concepts of motor impulsion and motor inhibition offer us our best tools for graphological analysis of writing.

Plate II. gives us a sample of handwriting characteristic of each of the four classes.[2] I. The hand of the individual high both in control and impulsion is a highly individualized but rapid hand. II. The hand produced by a penman low on impulsion, high on inhibition, gives evidence of tension in its excess of pressure, in its cramped or even crabbed forms, or in its small size. III. The hand high on impulsion but low in inhibition is rapid, light, fluent. IV. A penman deficient in both impulsion and inhibition writes a characterless relaxed hand.

It is instructive in this connection to tabulate from our records an individual's judgment upon himself as impulsive or deliberate. One has a right to expect a certain amount of

[1] 'Graphology and the Psychology of Handwriting,' p. 123f.

[2] Hands I. and II. were chosen for the scale (models for Imitation) as samples of the writing of individuals known to be explosive and deliberate respectively. At the time of choice neither individual had been given the Will-Profile Test. When, later, the record was taken my expectations were confirmed to the highest degree. Hand I. is, written by a penman who maintains an automatic set for retarded writing. Individuals of hyperkinetic make-up whose retarded writing shows tension write a much more highly controlled hand.

agreement between the outcome of the tests and an intelligent individual's rating of himself. One might anticipate that an individual high on impulsion and low on inhibition would check himself as impulsive; one high on inhibition and low on impulsion as deliberate; one's expectation concerning the other two groups (low on both traits, or high on both) would be less definite, so too in case of a balanced reaction.

Table IV. summarizes the judgments of one hundred individuals taken at random, twenty for each of five groups, namely I. High on both impulsion and inhibition; II. High on inhibition, low on impulsion; III. Low on inhibition, high on impulsion; IV. Low on both inhibition and impulsion; V. Medium on both traits. The numbers in parentheses give those who were willing to double-check their judgment because of their confidence in its correctness. I have also included in Table IV., the judgments passed by these individuals upon their cheerfulness or gloominess of outlook.

The table as a whole confirms to some degree the outcome of the tests. Group III., for example, shows a great preponderance of judgments in favor of impulsiveness, with many individuals double-checking this judgment. The case for Group II. is much less pronounced. Turning to my records, I find that of the twelve rating themselves impulsive in Group I., eight were individuals who produced an unmistakably *automatic retardation* of writing, to which reference has already been made; two others were doubtful cases. It is interesting to note that the qualified judgments appear in Groups I., IV., and V.

The most significant fact in the tabulation of Cheerful-Gloomy judgments, is—apart from the large preponderance of Cheerful judgments,—the variable group in III. and the number of those rating themselves as gloomy or variable in IV. Possibly the figures under IV. point to some sense of inadequacy on the part of a number of the group.

The agreement between the reagent's judgment of himself and the outcome of the test might, of course, be much more striking than it actually is. No doubt a certain amount of discrepancy is due to inaccuracy in the scoring for motor im-

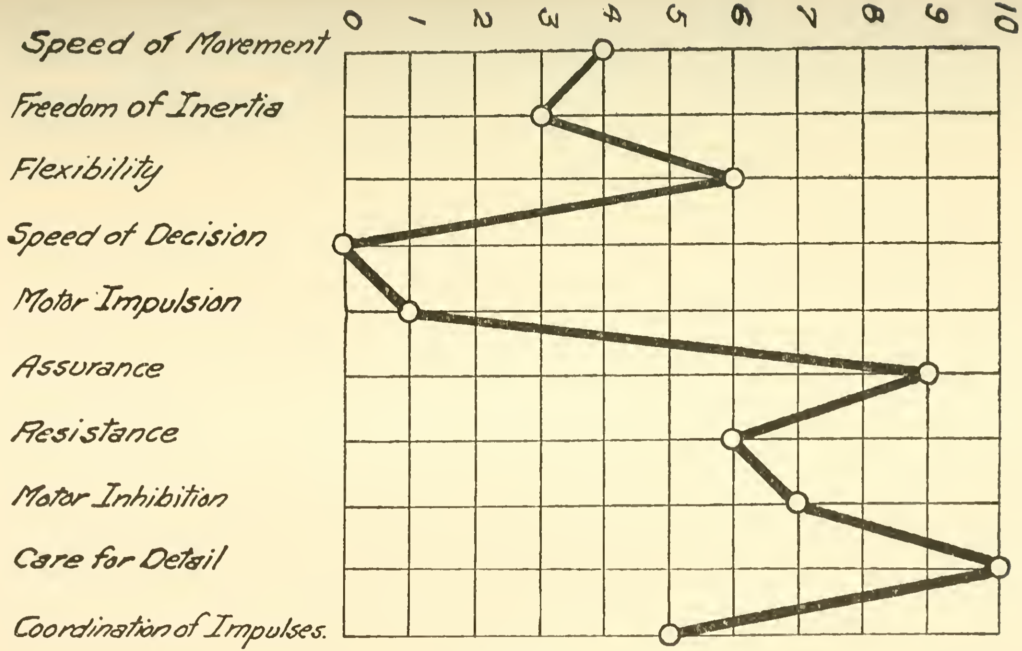

PLATE I.

Very characteristic profile of a scientist, X. Has been picked up from my desk with the exclamation, "That looks like Doctor X!" An intimate associate in the same department makes one comment apropos of his uncertainty concerning significance of score for coördination of impulses. "X," he asserts, "should run high on this trait if handling things; low, if handling people."

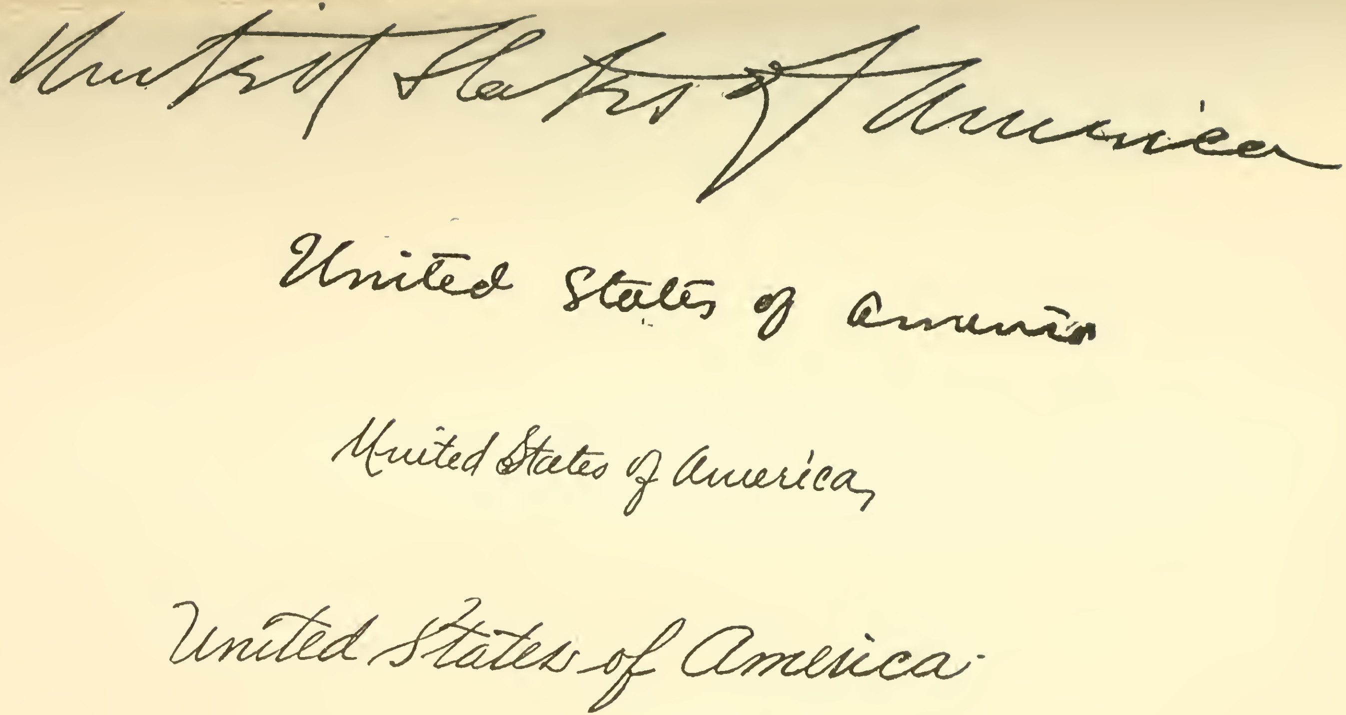

PLATE II

I. Written by an individual scoring high on both Motor Impulsion and MotorInhibition.
II. Written by an individual scoring high on Motor Inhibition, low on Motor Impulsion.
III. The hand of a subject scoring high on Motor Impulsion, low on Motor Inhibition.
IV. The hand of a subject scoring low on both Motor Impulsion and Motor Inhibition.

pulsion, an inaccuracy I hope to remedy by just the sort of analysis here presented, but in large measure the discrepancy is due to individual notions as to the meaning of the terms. One man, certainly far from impulsive in his make-up, who nevertheless so checked himself, explained when questioned that he understood 'impulsive' as equivalent to 'enthusiastic.' He himself never acted without reasons, and in that sense was 'deliberate.' Until character-analysis has developed a technical vocabulary of definitely defined words, we must expect to find ourselves struggling with verbal misunderstandings.

Summary

The outcome of the present investigation indicates that the Will-Profile has considerable general characterological significance and that it can be used to advantage not only in getting the general temperamental pattern of an individual but also in determining the specific combination of traits.

The answer to the question, Does the Will-Profile give you any new information about an individual? is certainly an assured affirmative in the case of a casual acquaintance, and also an affirmative in the case of intimates since it serves the purposes of detailed analysis, just as does an intelligence test. In conjunction with the latter, it certainly affords in many situations, a basis for conservative prophesy.

ON THE 'AFTER-SENSATION' OF PRESSURE[1]

BY R. T. HOLLAND

In 1916 F. L. Dimmick published, from the Cornell Laboratory, an experimental study of cutaneous after-images.[2] The stimulus, a hair of the tension-value of 4.5 gr./mm., was applied to pressure spots localized on the volar surface of the forearm.[3] While the experiments were performed with all due care, it has for various reasons seemed desirable to repeat and extend them. In the first place, Dimmick had but two observers; and, as it happened, the results furnished by these observers differed very considerably. On the whole, the times obtained from the one are eight times as large as those obtained from the other. It is true that the ratios of the times of after-sensation, interval and after-image are approximately constant in both cases, so that the difference is uniform and not casual or variable. Still, it seemed worth while to repeat the experiments with other observers, if only to decide whether or not the extreme types are connected by intermediates. In the second place, there is something paradoxical about a sensory response, to a weak stimulus of 1 sec. duration, which lasts (with the after-image) from 40 to 90 sec. The result contradicts the experiences of everyday life, and suggests the operation of some source of error which the experimental technique failed to control. In the third place, both of Dimmick's observers, though in unequal measure, reported experiences of tickle as accompanying the pressures; and it is not easy to see why that quality should have intervened.[4] We felt, then, that there is sufficient

[1] From the Psychological Laboratory of Cornell University.

[2] *Amer. Jour. Psych.*, XXVII., 1916, 556–569.

[3] The stimulus was so chosen as to affect only the cutaneous and not the subcutaneous organs, a precaution which was apparently neglected by M. H. S. Hayes in her "Study of Cutaneous After-Sensations," *Psych. Rev. Mon. Suppl.*, 60, 1912.

[4] These results are, of course, not peculiar to Dimmick's investigation: see Hayes, *op. cit.* On the other hand, M. von Frey, in his review of the sense of pressure (L. Asher and K. Spiro, 'Ergebnisse der Physiologie,' XIII., 1913, 96 ff.), makes no reference to tickle, even in his discussion of the phenomenon of *Entlastung*.

ground for reopening the problem of the time-relations of the sensation of pressure, and accordingly undertook the experiments now to be described.

It was our intention to work with stimuli of different durations, and also with stimuli of different intensities, some adequate to the cutaneous organs alone, and some involving also the subcutaneous tissues. Circumstances have made it impossible for us to carry out this programme; and the present study is concerned accordingly with the results obtained from a single stimulus applied for a uniform period of time.

Apparatus and Procedure

The stimulus employed was a horse-hair, set in a von Frey hair-æsthesiometer. We wished to use a hair which should affect only the cutaneous organs, should give a definitely supraliminal pressure for all spots and all observers, and should have as nearly as possible the same tension-value as the hair employed by Dimmick. We found such a hair, with the following constants: length from handle, 30.5 mm.; diameter (the hair was sensibly circular in cross-section), 0.2 mm.; force, 0.48 gr.; tension-value, 4.8 gr./mm.[1] Repeated trials with light etherization of the skin showed that this stimulus affected the cutaneous organs only. The stimulus was also positively supraliminal: out of 1,511 applications there were but 27 failures, 19 of which occurred early in the course of the experiment, before the introduction of the reading glass (see below). Since *E*'s uncertainty of manipulation was the reason for recourse to the reading glass, we have little doubt that most at any rate of these 19 blanks are attributable to maladjustment of the stimulus; the remainder of the 27 we can account for only conjecturally, as due to some minimal displacement of *O*'s arm.

The stimulus was applied to the skin by means of the mechanical applicator described by Dimmick. The time of application, 1 sec., was taken by *E* from the swings of a noiseless seconds' pendulum. By way of further control, *E* closed a lip-key at the moment the hair touched the skin, and opened it again as the stimulus was removed. The records of ten series, each one of ten applications, taken at random from the whole number, show the following times:

(1) 0.99 ± 0.04,	(2) 1.09 ± 0.05,	(3) 0.99 ± 0.01,
(4) 1.01 ± 0.06,	(5) 0.96 ± 0.06,	(6) 1.03 ± 0.07,
(7) 1.02 ± 0.07,	(8) 1.06 ± 0.08,	(9) 1.05 ± 0.04,
(10) 1.03 ± 0.05.		

The second was therefore approached with a fairly high degree of accuracy. The procedure is, indeed, less complicated than it might appear; and *E*, after practice, was able to time his applications almost automatically.

The exact localization of the *pressure spots* was secured partly by clipping the surrounding hairs, and partly by entry on a map of transparent celluloid which could

[1] The account of von Frey's stimulus-hairs given by W. H. Howell ('A Text-Book of Physiology,' 1915, 281) is misleading. The pressure-value of the hair is rightly calculated; von Frey, however, calculates, not this pressure-value, but the tension value. See E. B. Titchener, *Exper. Psychol.*, II., ii., 1905, 49.

be fitted to the arm-cast in the manner to be described below. Accuracy of stimulation on *E*'s part was furthered by ink-dots placed just peripherally to the spots and by the use of a large reading glass (magnifying 1.5 diam.). Ten well-marked spots were localized on the dorsal, and ten on the volar surface of the left forearms of our five *O*'s. The rectangular areas within which these spots were included (beginning at a line about 65 mm. from the carpal folds) were of the following approximate dimensions: for D, 30 x 80 mm. (dorsal), 10 x 60 mm. (volar); for H, 25 x 75, 10 x 50; for M, 15 x 50, 10 x 50; for Ta, 20 x 65, 15 x 30; for Tu, 15 x 45, 15 x 45. The smallest distance between spots was 3 mm. (D, M, Tu), the largest was 26 mm. (D); the average distance was 7.5 mm.

The ten spots of the area under experimentation were stimulated once each and once only during an experimental hour. The spots were numbered arbitrarily 1 to 10. The order of stimulation was determined by chance: ten tickets, bearing the serial numbers of the spots, were shaken together, and drawn one by one. This procedure was repeated 10 times, so that *E* obtained a random sequence of 100 stimulations. The sole departure from chance occurred when the numbers of adjacent spots were drawn in immediate succession; in this event the second number was thrown back among the rest, and a new drawing was made. Since there was an average interval of 4 min. between stimulations, there was an interval of at least 8 min. between the stimulations of adjacent pressure-spots.

O's left forearm was held in a carefully molded plaster cast, which included both elbow and hand. The fingers lay in an easy and natural position; and the cast, placed on a low table, allowed the whole arm to rest as comfortably as possible. The edges of the cast were built up to take the celluloid map referred to above; marks were also made upon the plaster, to ensure the precise fitting of the map to the area of stimulation.

O's free (right) hand controlled a key whose pressure and release recorded the appearance and disappearance of sensation or 'after-image.' The two records (*E*'s record of the time of application of stimulus, and *O*'s record of sensation) were made side by side upon ticker tape drawn over a spool by a noiseless motor of practically constant speed and wound upon a kymograph drum; the time-line was given by a Kronecker interrupter marking tenths of a second. The three magnets of the recording apparatus carried eosin writing-points of the type used for recording barometers. *E* threw the motor and interrupter into action by means of a foot-lever. A curtain hanging across the experimental table prevented *O* from seeing arm or instruments.

Five *O*'s served throughout the experiment: Dr. K. M. Dallenbach (D) and Dr. L. B. Hoisington (H), instructors in psychology; Miss M. F. Martin (M), scholar in psychology; and Messrs. S. Takaki (Ta) and S. Tung (Tu), graduate students in the department. Practice was continued for some three weeks before the regular series were begun.

The instructions given to the *O*'s were as follows: "After the usual Ready-Now signals, I shall apply a pressure-stimulus to your forearm. When sensation begins, close the key; when it ends, release the key. Be ready for a recurrence: and if a sensation recurs, close the key again at the moment of its appearance, and release it at the moment of disappearance. Continue until there is no recurrence. Afterwards, give a brief description, in attributive terms, of the pressure or pressures set up by the stimulus." These instructions remained unchanged throughout the experiments, save for a modification introduced in the course of Series II., to which we refer presently.

Experimental

Series I.: Forearm Prone

It is rare that an *O*, in experiments upon the volar surface of the forearm, does not sooner or later complain of the discomfort of his bodily posture. Since we planned a long series of experiments, it occurred to us that it might be better to lay the forearm prone, and to work on its dorsal surface. The suggestion was tried out in a few preliminary series, and was approved by the *O*'s.

As the regular series progressed, it became apparent that the *O*'s fell into two distinct groups. The one, which we shall call the 'regular' group, consisted of D, Ta and Tu. These *O*'s reported times of primary sensation which were of the same order of magnitude as the duration of the stimulus, and noted relatively few and brief recurrences or 'after-images.' The following tables set forth, for these three *O*'s,

Table I

Observer *Tu*

Spot	Sensation	No.	Int.	A.-S.
1	0.56 ± 0.20	(1)	1.5	2.5
2	0.57 ± 0.24	(1)	0.3	0.3
3	0.80 ± 0.40	(2)	0.5 ± 0.1	0.4 ± 0.2
4	0.85 ± 0.39	(1)	1.1	0.2
5	0.81 ± 0.28			
6	0.84 ± 0.50	(2)	2.3 ± 1.0	0.5 ± 0.3
7	0.89 ± 0.36	(2)	1.0 ± 0.3	0.2
8	0.85 ± 0.35	(1)	1.3	0.2
9	0.86 ± 0.33			
10	1.22 ± 0.46			

Table II

Observer *D*

Spot	Sensation	No.	Int.	A.-S.
1	1.3 ± 0.19			
2	1.6 ± 0.43	(1)	1.1	2.5
3	1.8 ± p.38	(1)	1.9	2.5
4	1.6 ± 0.33	(1)	0.5	1.0
5	1.6 ± 0.21			
6	1.8 ± .049			
7	1.2 ± .022			
8	1.4 ± 0.37	(1)	4.6	0.9
9	1.4 ± 0.34			
10	1.7 ± 0.33			

the duration in sec. of the primary sensation (average of 10 applications); then, in parenthesis, the number of times that a given spot showed a recurrence; and then the duration of the blank interval and of the first 'after image.' One table (Ta) is continued to a further stage: it sets forth, again in parenthesis, the number of times that a given spot showed a second recurrence; and then the durations of the second interval and of the second 'after-image.'

TABLE III

OBERSVER *Ta*

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2.
1	1.6 ± 0.15	(7)	2.0 ± 0.32	1.4 ± 0.55	(1)	3.5	6.5
2	1.9 ± 0.40	(4)	1.5 ± 0.72	2.1 ± 0.23			
3	2.1 ± 0.35	(6)	2.1 ± 0.71	1.6 ± 0.40			
4	1.7 ± 0.19	(10)	2.5 ± 0.45	1.9 ± 0.97			
5	2.2 ± 0.44	(7)	2.0 ± 0.58	2.2 ± 0.79	(2)	5.6 ± 3.8	0.9 ± 0.6
6	1.5 ± 0.23	(6)	2.9 ± 0.65	1.6 ± 0.84			
7	2.2 ± 0.64	(4)	2.2 ± 0.90	1.8 ± 0.94			
8	2.0 ± 0.24	(5)	2.4 ± 0.47	1.4 ± 0.57	(1)	3.8	1.0
9	1.9 ± 0.49	(4)	1.7 ± 0.29	2.3 ± 1.35			
10	1.7 ± 0.49	(5)	2.3 ± 0.77	1.4 ± 0.34			

The averages from these results are as follows:

TABLE IV

O	No.	Sensations	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2
Tu.....	(100)	0.82±0.38	(10)	1.18 ± 0.58	0.55 ± 0.45			
D......	(100)	1.56±0.34	(4)	1.02 ± 1.28	1.72 ± 0.82			
Ta.....	(100)	1.89±0.39	(58)	2.21 ± 0.37	1.74 ± 0.44	(4)	4.62 ± 2.38	2.35 ± 2.08

The results themselves are, it seems, what we might have expected. As regards the primary sensation, the *O*'s differ gradually and within narrow limits; the average times of sensation vary from nearly one second to nearly two seconds. The responses of the spots also differ gradually; the extreme ranges give the ratios 1:2 for Tu, 2:3 for D, and 3:4.4 for Ta. The *O* who shows the longest average time for the primary sensation shows the greatest liability to recurrence; but the recurrence rarely (only in 4 cases) goes beyond a single return, and its average value is slightly less than that of the primary sensation. All in all, the tables wear an appearance of uniformity.

Reports.—D's regular[1] report is of a quality of contact or bright, sharp, clear but weak pressure. This quality is sometimes obscured by, or passes into, a thin wiry prick (10 cases) or a more diffuse tickle or itch (4). Usually the intensity decreases steadily throughout the observation; occasionally it remains steady or decreases until the removal of the stimulus, when it suddenly increases (5). Of the 4 recurrent experiences, 2 are of the original quality (one of these may have been erroneously reported, owing to shift of attention), 2 are diffusedly ticklish or itchy.

Tu, a less practised observer, was somewhat disturbed by visual images. His regular report is of a bright, ticklish contact. This quality is sometimes obscured by, or passes into, 'a sort of prick, a little painful' (9). Of the 10 recurrences (2 of which are reported as doubtful), 7 are of the original quality (one of these is localized aside from the spot), and 3 are of a heavier, stingy nature.

Ta, who is also a relatively unpractised *O*, reports regularly for the primary sensation a weak but clear pressure, which gradually fades out, and only rarely brightens or becomes ticklish when the stimulus is removed. Of the 58 first-recurrences, 46 are described as faint, vague, diffuse, tickish; 12 are at first clear, 'concentrated,' fairly intensive, but quickly pass into the other stage. All 4 of the second-recurrences have the quality of vague, unsteady tickle.

TABLE V

OBSERVER H

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3
1	2.4±0.48	(9)	1.5±0.73	3.5±1.14	(1)	3.5	2.0			
2	3.1±1.10	(8)	1.8±0.95	3.8±1.10	(5)	2.5±1.37	4.7±1.83			
3	3.1±0.89	(7)	1.5±0.27	4.5±1.96	(4)	1.7±1.27	4.4±0.76			
4	3.0±0.80	(7)	1.7±0.29	4.0±1.86	(3)	3.1±1.13	2.7±0.55			
5	2.4±0.55	(7)	2.0±1.45	5.6±3.66	(4)	2.4±0.60	6.0±1.71			
6	2.8±0.53	(8)	1.3±0.65	4.1±1.50	(6)	2.1±0.56	4.5±1.34			
7	2.6±0.54	(5)	1.2±0.14	7.4±4.68	(1)	2.7	1.1			
8	2.9±0.82	(7)	2.0±0.82	4.8±1.89	(1)	3.2	2.2			
9	2.1±0.29	(5)	1.5±0.74	3.2±2.04	(1)	2.5	3.5			
10	4.1±0.58	(8)	1.4±0.55	3.8±1.25	(5)	3.8±0.96	4.4±2.00	(1)	0.4	0.8

Our other *O*'s, H and M, belong to what we may call the 'irregular' group.[2] It is, to be sure, possible to make out for these *O*'s tables which show, for the primary sensation, time-values lying on the average under 3 sec. (see Tables V., VI., VII.). But, in the first place, the recurrences are

[1] The reports that we term 'regular' are, of course, summaries of a large number of individual descriptions. All *O*'s, and especially the trained *O*'s, D and H, indicate differences (as between spot and spot) of compactness or sharpness and bluntness or dullness. Since, however, the differences are not pronounced, and since they are normal to groups of pressure spots, we have not thought it worth while to go into further detail.

[2] The group also includes Dr. C. W. Perky, who made 89 observations in this first series of experiments. P's selected average time of primary sensation was (82) 2.38 ± 0.90 sec. The remaining 7 times (nearly 8 per cent.) were long.

Table VI

Observer M

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4	No.	Int.	A.-S. 5
1	3.4±1.33	(5)	6.5±2.44	1.1± 0.56	(3)	7.2± 4.83	3.7± 0.37	(2)	3.2±0.60	3.9 ± 0.6	(1)	13.4	2.8			
2	2.8±1.04	(10)	4.2±2.60	1.4± 1.11	(4)	5.3± 1.37	6.6± 9.34	(2)	4.6±0.30	11.7 ±11.5	(1)	8.5	0.7			
3	3.4±1.15	(6)	4.2±1.80	22.9±33.0	(1)	16.6	2.3	(1)	5.5	0.4	(1)	27.3	3.0			
4	3.1±0.94	(6)	6.0±3.55	5.2± 4.42	(3)	3.3± 1.77	2.0± 2.00	(1)	3.7	0.4	(4)	10.1±2.48	3.0±1.58	(1)	0.15	3.7
5	2.2±1.18	(7)	6.1±1.61	5.5± 3.45	(3)	22.8±18.37	16.1±17.30	(3)	9.0±6.83	16.0 ± 7.9						
6	3.1±1.41	(9)	5.2±4.42	21.3±25.70	(7)	6.5± 4.01	2.8± 1.46	(4)	3.8±1.94	3.3 ± 1.0						
7	2.5±0.96	(4)	4.0±1.35	4.1± 3.11	(2)	2.5± 0.67	1.6± 0.15	(2)	3.3±7.20	1.92± 0.3						
8	2.2±0.49	(8)	6.7±1.29	3.9± 4.92	(4)	4.2± 1.70	4.2± 1.97	(3)	2.4±1.40	27.0 ±18.9	(1)	3.2	0.6	(1)	1.50	3.6
9	2.3±1.10	(9)	2.6±2.14	13.5±17.00	(4)	14.7±11.31	3.9± 2.15	(1)	1.0	5.8	(1)	2.2	5.8			
10	2.0±0.79	(8)	7.0±5.47	9.4± 8.45	(4)	6.6± 5.55	12.1± 6.26	(1)	3.8	3.6	(1)	5.3	3.8			

Table VII

O	No.	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4	No.	Int.	A.-S. 5
H	(100)	2.88±0.78	(71)	1.59±0.66	4.37± 2.01	(31)	2.63±1.04	4.26±1.64	(1)	0.4	0.8						
M	(100)	2.71±1.16	(72)	4.99±3.16	0.21±11.16	(35)	8.30±0.64	5.73±4.80	(20)	2.0±2.8	3.4±7.4	(10)	7.0±4.9	2.0±2.7	(2)	0.8±0.7	3.6

more marked: H gives 71 of the first and 31 of the second order; M gives 72 of the first, 35 of the second, and 20 of the third order. And secondly, the tables do not record (as the earlier tables have done) the results of 100 consecutive experiments; the values are selected. It is characteristic of the members of this group that the ordinary or regular times for the primary sensation are replaced at irregular intervals by times of much greater length, times ranging for H between 10.1 and 55.9, and for M between 10.5 and 204.2 sec. H gives 8 per cent. of these long times, indifferently from the 10 spots, and M gives 27 per cent., principally from 6.[1]

Since H, in particular, is a practised *O*, we thought, on the recurrent appearance of the long times, that something must be at fault with apparatus or procedure. Fatigue could hardly come into play; there were but 10 observations in the hour; and in point of fact the long times, from first to last, were as likely to occur at the beginning as in the middle or at the end of an experimental sitting. H's reports gave us no cue. M, however, sometimes reported with the long times: 'tingling as when the arm is asleep,' 'twitching of muscles under skin like gooseflesh,' 'tickle with pulse,' 'pulse in wrist,' 'pulse in fingers.'[2] Although these remarks were by no means frequent, we nevertheless imagined that they might point to an interference with circulation due to the prone position of the arm. We therefore again worked carefully over the surfaces of the casts, to make them if possible still more comfortable for the *O*'s; and we divided the experiments of an hour into groups of 4, 3, 3 (or 4, 4, 2), between which the arm was taken from the cast and the *O* was free to move. These changes were of no avail; the long times persisted. We therefore determined to continue the observations until series of 100 'regular' times had been completed (Tables V., VI.) and then to repeat the work with the arm supine. It was possible that with this position of the arm the long times would disappear; it was possible also that the reports of the *O*'s would give, by comparison, a clue to their conditioning.

[1] Yet M's spot 8, which responded irregularly to but 2 out of 12 stimulations, jumped in these two instances from its average of 2.2 ± 0.49 sec. to 17.3 and 109.5 sec. respectively!

[2] P also spoke occasionally of 'throbbing' and 'pulse' in the arm.

Reports.—H's regular report is of a weak or moderately intensive neutral pressure, punctiform or of very limited area. The quality sometimes passes into, or is interrupted by, pain (8 cases) or tickle (8). Usually the intensity either decreases steadily throughout the observation or shows a rise followed by a continuous decrease; occasionally it remains steady or decreases until the removal of the stimulus, when it 'flares up' (6). Of the 71 first-recurrences, 41 repeat the original quality, but weaker, and of diffused and indefinite area; 32 (there is a slight overlap) possess a brighter, ticklish quality; and 2 are painful. Almost invariably these experiences fade out gradually. The second-recurrences are still weaker, show a greater tendency to become ticklish, and occasionally are 'very like imagery.' Their disappearance, when recorded, is always gradual.

The reports for the long times are naturally incomplete. There is fluctuation of quality, of intensity, and of area. What is more to the point is that, on several occasions, H refers to these experiences as 'perceptual;' they are 'patterned'; there may be a 'granular core' with 'a fine and fuzzy edge, extended longwise,' and the pattern may shift instably, with change of localization.

M is an observer somewhat more practised than Ta and Tu, but far less practised than D and H. She reports a light, clear contact (68), a tickle (17), a delicate prick (4), or a dual combination of contact with tickle or prick. The quality either fades out gradually, or shows an increase of intensity followed by a steady decline. There is no report of terminal increase of intensity, though in a few cases the disappearance of the primary sensation is said to be sudden or abrupt. The recurrences are almost always described as diffuse tickle, 'spread on the arm,' 'not localized at spot'; the tickle is occasionally blended with contact, and very rarely contact is reported alone.

Primary sensations and recurrences are, one and all, accompanied by visual images, —so M remarks in a summary report; but these images are seldom recorded, and seem not to have distracted attention from the skin. M also reports a strong reflex tendency, when tickle is aroused, to take her right hand from the key and move it to the left arm. The tendency seems to be regarded at least as a secondary criterion of the presence of tickle.

In her reports for the long times, M mentions, as a rule, a temporal fluctuation of qualities, in which two or all three of the familiar members of the group (contact, tickle, prick) appear; sometimes there is a 'third' or a 'fourth' quality which is left unnamed; every now and then warmth or cold flashes in. The attributive report is cut across by references to pulse, arm asleep, tingling, gooseflesh. There are a few cases in which the perceptual nature of the experiences is clearly brought out: M speaks of a wide spread of tickle in which 'points come and go associated with bright spots in [visual] image'; of contact and diffuse tickle accompanied by 'visual image of belt of tickle on arm'; of tickle coming in 'as ring round the original spot touched, as I imagine'; of a blend of tickle and contact 'spreading right and left in visual image, with prick-sensation separate.' Here the visual imagery plays a more active part.

Second and later recurrences, with both regular and long times of primary sensation, are frequently given with a question-mark: 'may have been imaginative,' 'not sure whether real or imaginal.'

Series II.: Forearm Supine

The following Tables VIII.–XI. show the results obtained from the three *O*'s of the previous regular group.

Table VIII

Observer Tu

Spot	Sensation	No.	Int.	A.-S.
1	0.90 ± 0.24	(1)	1.7	0.2
2	0.96 ± 0.20			
3	0.93 ± 0.23	(1)	1.6	1.1
4	0.93 ± 0.14			
5	0.94 ± 0.15			
6	0.98 ± 0.22			
7	0.96 ± 0.20			
8	0.99 ± 0.26			
9	0.97 ± 0.22	(1)	0.7	0.2
10	0.91 ± 0.13			

Table IX

Observer D

Spot	Sensation	No.	Int.	A.-S.
1	1.33 ± .016			
2	1.50 ± 0.27	(1)	3.1	1.0
3	1.35 ± 0.23	(1)	1.5	1.0
4	1.60 ± 0.27	(1)	1.5	1.1
5	1.50 ± 0.23	(1)	3.7	1.4
6	1.53 ± 0.26			
7	1.50 ± 0.20			
8	1.80 ± 0.50			
9	1.42 ± 0.18	(2)	1.5 ± 0.17	0.7 ± 0.5
10	1.62 ± 0.18			

Table X

Observer Ta

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.S.-3
1	2.57±0.70	(7)	6.2±1.8	10.2±7.5	(2)	7.2±2.5	10.2±9.4	(1)	0.4	0.5
2	2.90±0.69	(9)	4.8±2.2	9.9±4.3	(1)	7.5	19.6			
3	3.00±0.90	(8)	6.1±2.4	8.7±8.6	(2)	6.0±1.4	10.0±9.6	(1)	3.1	0.7
4	3.00±1.20	(8)	3.1±1.4	7.2±4.6	(2)	7.1±2.6	1.8±1.8	(1)	3.0	0.5
5	3.00±0.86	(7)	4.6±1.0	13.5±7.2						
6	3.50±0.88	(9)	6.8±2.8	11.7±5.2	(2)	5.9±0.6	1.9±0.5			
7	2.80±0.55	(10)	5.4±1.9	11.0±9.5	(3)	8.8±8.5	2.9±1.3	(1)	2.4	0.5
8	3.40±1.00	(8)	5.8±3.6	12.9±1.0	(1)	3.0	4.6			
9	2.50±0.35	(7)	6.9±2.9	8.2±4.5	(1)	5.2	6.8			
10	3.00±1.00	(2)	5.6±0.6	5.2±3.2						

Table XI

Observer Ta

O	No.	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3
Tu	(100)	0.95±0.19	(3)	1.30±0.4	0.5±0.4						
D	(100)	1.51±0.23	(6)	2.10±0.7	0.6±0.36						
Ta	(100)	3.00±0.85	(75)	6.20±2.5	10.6±6.0	(4)	6.8±3.2	6.3±5.8	(4)	2.2±0.9	0.5±0.6

The tables are very similar to Tables I.–IV. The three *O*'s stand in the same order of rank. Tu has decreased his range to 10: 11; D and Ta have hardly changed in this regard (11:15, 5:7). Ta has increased his average time of total sensation from nearly two to three seconds, and his record of recurrences is fuller, with longer times. On the whole we may say that Tu and D have behaved as they behaved in Series I., and that Ta, while he does not fall obviously out of line with his former record, may perhaps show the influence of some condition tending progressively to lengthen his periods of response.

Reports.—D's regular report is of a contact, or bright, clear pressure, more intensive than in Series I. This quality is sometimes obscured by, or passes into, a prick or wiry pain (13 cases) or a more diffuse tickle (8) or itch (15). Usually the intensity decreases steadily throughout the observation; sometimes it decreases and then increases (11); occasionally it decreases or remains fairly steady until the removal of the stimulus, when it suddenly increases (3). Of the 6 recurrences, one (of which *O* reports that it was probably imaginal) is of the original quality; 4 are diffusedly itchy; one has a tingling character.

Tu's regular report is of a light, delicate, ticklish contact. This quality is sometimes obscured by, or passes into, a bright pain (8). Tu now reports comparatively few visual images; we get, in exchange, reports of intensity. The contact usually disappears gradually; sometimes (9) its intensity decreases and then increases. All three of the recurrences are of the original quality, a ticklish contact.

Ta regularly reports a weak but clear (or very moderately intensive and clear) pressure, which either gradually fades out or (in 22 cases) passes, for the most part abruptly, into something bright and ticklish. Of the 75 first-recurrences, 57 are described as faint, vague, diffuse, unsteady, ticklish; 9 begin in this way, quickly become clearer and more intensive, and relapse into the first stage; 7 begin as dull, heavy and diffuse, and pass into the familiar vague tickle; and 2 only begin as 'concentrated.' Once a sensation of cold is remarked. All of the later recurrences are termed, comparatively, fainter, more obscure, more diffuse, fluctuating; all are of a ticklish quality.

We had hoped that, with change of the position of the arm from prone to supine, the two members of our 'irregular' group would give results comparable with those obtained from D, Ta and Tu. This hope was not fulfilled. In the first series taken under the new conditions with H and M, typical 'long' times (56 and 83 sec. respectively) occurred among the normal short times. Fortunately, one of M's reports in an early series gave us the cue to the situation: it appeared that M was making a voluntary effort to hold the

Table XII

Observer H

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
1	4.4±1.3	(9)	1.7±0.6	5.8±1.0	(4)	2.5±0.4	4.8± 0.7						
2	3.3±0.9	(9)	1.9±0.9	5.3±2.4	(3)	1.9±0.5	14.9±16.5	(1)	1.0	3.5			
3	3.3±1.3	(10)	1.4±0.4	7.4±3.0	(5)	2.9±0.9	6.1± 2.4	(1)	0.5	13.2			
4	4.0±1.4	(8)	2.1±0.8	5.4±1.7	(2)	2.7±0.0	4.0± 1.5						
5	4.5±1.5	(9)	2.2±0.9	7.5±1.7	(5)	4.0±1.6	4.8± 1.3						
6	3.9±0.9	(9)	1.8±0.7	4.9±1.9	(1)	3.8	3.1						
7	4.0±1.2	(8)	1.3±0.4	7.3±3.5	(4)	6.9±6.3	6.0± 2.0	(2)	2.7±2.7	14.9±9.6	(1)	6.5	11.4
8	3.4±1.5	(8)	1.6±0.4	6.2±1.3	(2)	3.6±1.0	4.3± 1.7						
9	4.1±1.2	(7)	3.1±2.0	12.2±8.2	(2)	1.3	5.1± 1.4						
10	3.9±1.4	(8)	1.6±5.8	7.3±2.3	(4)	3.1±0.6	5.0± 1.4						

Table XIII

Observer M

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
1	2.0±0.29	(8)	4.3±3.2	7.0± 9.1	(2)	13.3±1.6	0.7±0.5	(1)	16.0	0.9			
2	1.8±0.34	(8)	3.6±2.1	5.3± 6.4	(6)	3.6±1.3	2.1±2.4	(3)	9.3±7.4	1.8±0.9	(2)	7.3±4.1	0.2±0.03
3	1.7±0.38	(8)	2.9±1.8	1.4± 1.3	(3)	3.2±0.4	0.4±0.2	(3)	27.3±2.3	6.6±6.1	(1)	5.1	0.2
4	1.8±0.54	(8)	3.7±3.0	8.9±10.4	(4)	5.1±5.2	3.6±4.5	(2)	12.2±9.4	1.2±1.7	(2)	2.6±1.2	1.5±0.07
5	2.3±0.77	(9)	5.8±4.5	2.8± 2.5	(4)	3.6±1.1	1.6±0.7	(2)	1.8±3.6	0.7±0.6	(2)	3.8±1.2	0.7±0.05
6	2.4±1.00	(9)	6.4±4.7	2.5± 2.6	(5)	4.6±3.7	4.2±4.6	(2)	6.4±5.2	2.2±2.0			
7	2.1±0.65	(7)	3.4±2.0	1.5± 1.9	(4)	2.4±4.3	0.3±0.1	(2)	8.3±6.4	3.5±0.1	(2)	5.2±2.2	0.3± 0.10
8	1.8±0.36	(8)	4.2±1.2	6.6±10.1	(5)	6.9±2.3	2.6±1.8	(2)	2.8±0.8	1.3±1.2			
9	1.9±0.23	(9)	4.8±3.0	2.1± 2.2	(6)	3.8±1.1	0.9±1.0	(4)	5.2±1.2	1.7±1.5	(1)	5.4	0.5
10	2.0±0.40	(9)	4.2±3.7	13.2±16.0	(5)	4.5±1.4	4.4±5.1	(2)	11.1±7.1	7.3±7.1			

Table XIV

O	No.	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
H...	(100)	3.9±1.3	(85)	1.8±7.6	6.8±3.2	(32)	3.4±1.5	6.0±2.7	(4)	1.7±1.3	16.6±8.9	(1)	6.5	11.4
M...	(100)	1.9±0.5	(83)	4.5±3.0	4.4±6.1	(44)	4.9±0.6	2.4±2.9	(23)	10.0±8.9	1.9±1.9	(10)	4.3±2.6	0.5±0.6

sensation and 'after-image' as long as possible, was seeking by 'effort of attention' to prolong her experiences. We accordingly modified the instruction given to H and M, to the effect that they were to take the stimulus passively, receptively; to be on the alert to signal what should 'come,' by way of sensation and after-experience, but not to try to 'hold' or to 'bring' these experiences. *From the time that this instruction was introduced, not a single 'long' time appeared in the records of the primary sensations.* Tables XII–XIV. show, in the usual form, the results of 100 observations.

M's times of primary sensation have fallen to an average of 1.9 sec., *i.e.*, to a value of the same order as the times of D in both sets of experiments and of Ta in Series I. H's average is higher, nearly 4 sec., Both *O*'s show many recurrences.

Before we discuss the effect of the new instruction, and the light which the consequent regularization of the times throws on the experiment as a whole, we present in summary the reports of H and M in this Series II.

Reports.—H's regular report is of a neutral pressure, of very limited area. The quality often passes into, or is obscured by, tickle (25); sometimes (10) a pain appears, 'quality baffling,' 'quality hard to tell.' Usually the intensity decreases steadily throughout the observation; in 14 cases H notes an increase followed by a decrease. There is in this Series no report of an increased intensity at the moment of removal of stimulus. Most of the 85 first-recurrences appear as a weak, lively, diffuse, indefinite contact, fluctuating in intensity; 15 are reported as actual tickle. There is one instance of 'pain.' The later recurrences are recorded as very weak, light contact; diffuse, fuzzy, fringy, indefinite in area. They seem to be weaker even than weak tickle (which is reported 4 times only), and to be little more than experiences of a 'something light and lively.' Occasionally *O* remarks: 'might have imagined it'; 'may have been illusion.' In three cases he says that the experience is not localized on the skin; it is 'perceptual.'

M regularly reports a light, bright, clear pressure or contact, of punctiform area; there are 19 cases in which a delicate prick, and 9 cases in which a tickle overlaps or replaces the contact. The quality either fades out gradually, or shows an increase of intensity followed by a steady decline. There is no terminal increase, though in a few cases the primary sensation drops out abruptly.

The reports of the recurrences are brief and very often doubtful. Three times only there is a blend of very weak pressure with tickle; once only a blend of very weak pressure with prick. Usually we have the adjectives fainter, weaker, indistinct, not localized, not cognitively clear, diffused, indefinite; not certain, not sure, doubtful; and such phrases as 'perhaps imagined,' 'not sure whether in sensation or imagination,' 'may have been pulse wave,' 'might not have been there.' Occasionally recourse is had to visual imagery, with a perceptual consequence: 'visual image gives meaning of localization;' 'contact, not clear; visual image comes in with eye-movement, to localize'; 'weaker; gradually diffuses; visual image of *R* going down and moving off.'

If in the light of these results we now look back over the course of the entire experiment, we see (1) that D and Tu preserved stably the attitude which we had hoped to induce by instruction. (2) Ta, on the other hand, was impressed by the injunction to 'be ready for a recurrence'; his faith in *E* led him (as he told us later) to believe that if recurrences were mentioned, recurrences would come; he began to observe with an expectant set for recurring after-sensations. Hence in Table IV. we have a normal primary sensation, but an undue number of recurrences. Table XI. is then simply an exaggeration of Table IV.; it appears probable (though we cannot be certain of the point) that Ta's expectancy has extended to the primary sensation, and that the recorded average time (3 sec.) is somewhat too large. (3) H and M, finally, interpreted the instructions to mean that they were to take up an active attitude to their experiences, to secure and to maintain all the cutaneous sensations possible.[1] They give, accordingly, (*a*) highly irregular times for the primary sensation, times which jump from the normal to three or four times the normal's value, and thence to times which are extremely large. They report (*b*), even with their normal times, a high percentage of recurrences. They insist (*c*) on the difficulty of distinguishing between after-experience and imagination. And they have recourse (*d*) to perceptual construction, in order (by the aid of visual images) to anchor the fleeting and doubtful after-experiences. When the instructions are changed, they give (*e*) no more 'long' times for the primary sensation. M's average of 1.9 sec. suggests that her average in Table VII. (like Ta's in Table XI.) is affected by her general attitude. We may guess, though we cannot be sure, that both of H's averages, 2.88 and 3.96 sec., are for the same reason too long. The *O*'s (*f*) also show the continued influence of their original set in the high percentage of recurrences; but their reports have changed. H's recur-

[1] The same thing holds of P, so far as her results go.

rences, after the first, are too weak and faint even to be called ticklish; and M's recurrences are all so indefinite that she hardly ventures to give them a qualitative characterization.

We see, therefore, that the question of attitude or *Einstellung* is here of primary importance. Quantitatively we have a fairly satisfactory accordance of results with conditions. Qualitatively, to be sure, the problem is less simple. We know too little of cutaneous imagery to be able to say definitely wherein the recurrent experiences consist. Some of them, we may suppose, are true after-sensations of pressure. Others may rest upon some faint, ordinarily subliminal sensation, now raised to maximal clearness by *O*'s attentional attitude, and supplemented or embroidered by imagery. Others again may gather about an intercurrent sensation of tingling or warmth or cold. And yet others may be wholly imaginal, the result of autosuggestion. Our experiments do not permit us to decide, or to assign percentages; specially directed observations are required.[1]

We return to Dimmick's experiments. Dimmick's first observer C gives as average time of primary sensation 1.44 sec., interval 0.78, after-image 6.15 . We are not told the precise frequency of the after-images, but they seem to have been rather the rule than the exception. This difference between C and our own D and Tu may have been (like that of Ta) due to C's reading of his instructions; apart from it, there is close agreement.

Dimmick's second observer R gives an average primary sensation of 11.41 sec., interval 6.7, after-image 44.39. It will be recalled that the lowest of H's and M's 'long' times were about 10 sec.; there was a jump from the normal to this abnormal value. We may suppose, perhaps, that R observed more or less consistently (his *MV* was 10.45) at this first level of prolongation, and that he was prevented by some auto-suggestion or suggestion of instruction from proceeding to longer times. It is possible also that his after-image, recorded as a whole, was oftentimes a fluctuating and interrrupted experience.[2] These interpretations are, of course, guesswork, but they are guesswork based upon results gained with longer series and from a larger number of *O*'s.

We have little doubt that the tickle into which the primary sensation may merge is a phenomenon of adaptation; the initial contact brightens and lightens, and the

[1] A. S. Edwards ('An Experimental Study of Sensory Suggestion,' *Amer. Jour. Psych.*, XXVI., 1915, 124 f.) finds anticipatory images of touch which simulate sensation. J. R. Angell (*Psych. Mon.*, 53, 1910, 79, 94 f.) notes individual differences and proposes some simple tests. Hayes (*op. cit.*, 33 f.) makes brief preliminary reference to attention, but in her own work appears not to have regarded 'subjective' conditions.

[2] Our observer M reported only five continuous after-sensations of the 'long' type; they ranged from 40 to 170 sec.

quality becomes ticklish. We may thus account for the frequent appearance of tickle in the reports of Dimmick's short-time observer C. For the rest, there is always the question of practice. Tu's pricks, *e.g.*, may have been true pressure-spot responses; an *O* has to learn by experience the nuances of the pressure-sensation; but the pricks of the more practised *O*'s probably derived from concomitant stimulation of pain-spots.

Series III.: Forearm Supine, Blank Experiments

The care which our *O*'s took to report imaginal characters and resemblances puts it practically beyond doubt that the primary sensations were in fact sensational. We thought, nevertheless, that it would be worth while to add a few series which included blank experiments, in order to determine whether an anticipatory image might arise, clear and strong enough to create illusion. Since the introduction of a single blank in the series of 10 might possibly sharpen *O*'s attention, and thus lead to results not comparable with those of the preceding series, we introduced three or four blanks, placed at haphazard. Three to five series of the regular type (30 to 50 observations; all 10 pressure-spots) were secured from the 5 *O*'s.

D, H, M and Tu reported 'Nothing' every time that the *R* was omitted. Three of these *O*'s made no comment; the fourth remarked—what the others very probably thought—that something must have gone wrong with the apparatus. Ta went successfully through a first series. It happened that the first 'observation' of his second series was a blank; on this occasion Ta pressed his key about a second after the *R*, had there been one, would have been removed, and reported: "Nothing at first; then something very weak and faint." Some 3 sec, later he released the key, with the comment: "Don't know when it went away." Three further series passed without error. Ta's positive report (due, as it seems, to the suggestion of the first observation in a series; we may recall Ta's suggested rendering of the formula of instruction) is valuable; it shows how an after-experience may arise under the influence of expectant attention.[1]

[1] We may remind the reader that there were 8 cases, scattered through the total experiment after the introduction of the reading glass, in which spots failed to respond to stimulation. As it happened, the scattering included every one of the 5 *O*'s.

We conclude, then, that the primary sensations were, as we had supposed, genuinely sensational, and that their somewhat undue length in certain of our tables indicates simply their maximal prolongation by attention.

Conclusions

A. If *O* is in a passive, receptive attitude toward the experiment:

1. A stimulus-hair of 4.8 gr./mm. tension-value, applied to pressure-spots on dorsal or volar forearm, arouses a sensation of contact or light neutral pressure, which under adaptation brightens to a tickle. Sometimes (by stimulation of a near-by pain-spot ?) a wiry pricking pain is also aroused.

2. The normal duration of the primary sensation is from 0.75 to 2.00 sec. The sensation either decreases steadily in intensity throughout its course, or shows an initial increase followed by a decrease. Sometimes there is a sudden intensive increase at the moment of removal of the stimulus.

3. After-experiences are few and brief.

B. If *O* assumes an active attitude, making a voluntary effort to secure as much cutaneous experience and to maintain it as long as possible:

4. The normal primary sensation may last on the average 4 sec.

5. After-experiences are numerous and often of considerable duration.

6. Abnormal primary sensations, lasting as long as 200 sec., appear mixed irregularly with the normal.

7. There is a tendency to resort to perceptual construction.

C. The discrepant results obtained by previous experimenters under similar objective conditions are therefore, in all probability, due to variation in the attitude of their *O*'s.

D. While there is no doubt that true after-sensations ('after-images of pressure') occur, there seems also to be no doubt that free or tied pressure-imagery plays a part in the formation of cutaneous after-experiences. A systematic study of cutaneous imagery is needed.

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THE EFFECT OF FATIGUE ON RETENTION

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Introduction

When it is desired to demonstrate muscular fatigue all that one has to do is to isolate a muscle or a group of muscles, attach a load, provide successive stimuli of sufficient intensity and at sufficiently short intervals and ordinary observation will detect progressive weakening in muscular contraction. The fatigue so produced is caused by the catabolism in the muscle and recovery takes place when the fatigue products of this catabolism are removed. The fact that when one does mental work he feels fatigued and desires rest just as one does when he becomes physically fatigued has led scientists to search for experimental evidence of neural fatigue by methods similar to those used to demonstrate muscular fatigue. The failure of such experiments to demonstrate neural fatigue has led to two diverse results. The first was to stimulate investigators to refine their methods and to invent more delicate instruments to detect catabolism in the nerve. The second was to infer that the neural mechanism does not become fatigued, and to assume that the feeling of fatigue after so-called mental work was due either to fatigue of a peripheral sort—that is, to muscles or sense organs—or to a feeling of monotony.

Experiments have shown beyond a doubt that the nerve fibre is much less susceptible to fatigue than the muscle. If the refractory phase is regarded as a fatigue phenomenon, it must be admitted that neural fatigue is relatively slight and recovery extremely rapid. It is quite possible that this refractory phase forms a resting period and thus gives the nerve the advantage of a much more delicate recovery mechanism than has the muscle. If the nerve is so delicately protected fatigue can not be discovered satisfactorily or studied successfully by the methods used in studying muscular fatigue.

An important element in the neural mechanism that should be considered in connection with the study of neural fatigue is the central part of the nervous arc. If it could be demonstrated that the nerve fiber is not subject to fatigue it would not follow that the formation of bonds does not deteriorate when work is too continuously prolonged. One's ordinary experience teaches him that the time to do some original problem, or to memorize material is when one is fresh and to do routine mental work when one is not at his best. Yet most of the experiments on mental fatigue have been with tasks of a more or less routine character such as multiplication, addition, judgments, reaction time, etc. The investigations so far reported seem to show that the farther one gets from routine material the more likely he is to find some indications of neural fatigue. Memory which is somewhat removed from routine shows the greatest effects of fatigue when memorizing has been continued over long periods.

Oehrn[1] experimented with lists of 12 digits for two hours and nonsense syllables for 1.5 hours. In the latter case the subjects did lose in efficiency as the work progressed. The relations of the number of nonsense syllables learned in the successive 15 minute periods were as 1000, 855, 826, 754, 666 and 751. In criticizing this work Thorndike says: "The meaning of the results obtained in memorizing nonsense-syllables is doubtful; for, as one learns more and more series, there develop interference and confusion. If tib has been connected with (1) pon zet luf (2) nog, biz, ref, and (3) sib, kol, mek, it thereby makes the learning of tib, wif, tek, saw

[1] The investigations here referred to are all reviewed by Thorndike, 'Educational Psychology,' Vol. III.

or tep, lin, tod wak, or deb, nig, ron, puf, and the like harder. The first hour's learning may then reduce later ability by such interference and confusion, as well as by fatigue proper."

In learning lists of 12 digits the scores of the successive 15 minute periods were as; 940, 852, 938, 867, 960, 908, and 1000.

Vogt worked seventy-five minutes daily for twelve days at memorizing series of twelve digits (against additional distractions during the second half-hour). The results for the first and last five minutes of work give an average loss from the beginning to the end of the same day of 7.5 per cent.; and a gain at the beginning of one day over the end of the preceding day, of 9 per cent.

Vogt's results in the case of memorizing nonsense syllables showed a falling off of 27 per cent. in the last five minutes of a seventy-five minute period (of which two fifths was spent in work against distraction) compared with the first five minutes. When the work of the last five minutes of each day is compared with that of the first five minutes of the day following, the drop is 30 per cent. As has been noted, the interference and confusion resulting from the likeness of the different nonsense syllab es may have a part in this.

Woodworth who had some preliminary drill in memorizing numbers worked constantly from 3 to 7 P.M. memorizing sets of numbers. Outside of a slight decrease in efficiency toward the end which was probably due to lighting conditions there was no sign of inability to work.

Tests of the effect of fatigue from school work or other work during the day on ability to memorize have all been negative. Where memory has been looked upon as an index to measure fatigue from other lines of work, little or no loss in efficiency has been discovered. Where memorizing was continued for long periods, and loss in efficiency in memorizing looked for, some effects of fatigue have been discovered; but in these cases the results have been interpreted by Thorndike as confusion and interference due to the use of the same nonsense syllables in different combinations.

In none of this work as far as we have been able to discover were tests made of the permanency of the associative bonds formed in the fatigue experiments. It is possible that a subject could by increased effort overcome fatigue effects and do work nearly as efficiently after long periods of application as before; but, if retention tests were made, inferiority might be apparent in the bonds formed after long periods of work. This possibility was the hypothesis that led the writer to try the experiment to be reported in this paper. *Can a person adapt himself to long periods of memorizing so that no loss in efficiency will appear? If so, is the material learned at the end of a long period retained as well as that learned at the beginning?* Experiments have already indicated the probable truth of the first question. Our main purpose will be to answer the second.

Statement of Experiment

The purpose of the experiment was to determine whether long continued work in memorizing would result in the bonds formed late in the learning period being less well retained than those learned in the beginning of the learning period. Since it might be possible for an individual to adapt himself in the learning so that he would seemingly learn as much at the end as in the beginning of the period it was necessary to test the subjects for the amount learned at the time of learning as well as to test them for the amount retained after a suitable period of time had elapsed. The relative degree of retention could then be measured by determining the percentage the retained material was of that learned. For instance suppose that an individual learned five units of material in the first part of the sitting and also learned five units in the last part. If after twenty-four hours he was able to reproduce two units of the material learned in the first part and only one unit of that learned in the last part of the learning period one would have obtained evidence of the lack of permanency of bonds formed in the latter part of the sitting.

In order to fulfill the conditions of such an experiment it was necessary to get material of sufficiently small and sufficiently equal units to enable us to keep a careful record of the material memorized. It was necessary to have enough material to last for a long period of time. It must be presented uniformly and yet fast enough to cause the subject to exert himself to his utmost in the learning. The material learned at the beginning of the period must be of the same difficulty as that learned at the end so that any difference in learning would be due to the factor of fatigue and not to difference in the difficulty of materials. By proper apparatus for exposure, through careful selection of materials, through the changing of the order in which the material was used with the different subjects and the utmost care in the keeping of constant conditions every attempt was made to eliminate irrelevant factors.

Material

The material used for memorizing was German-English paired associates. A list of such associates compiled by Professor Cattell was procured by the writer. This list contained one thousand pairs. The list was gone over very carefully and all words eliminated which could have any similarity of appearance or sound. After a careful sifting eight hundred and fifty were left and these were used in the experiment. The words were mixed up by having them on slips of paper. They were then selected at random and arranged into lists of ten. The words which happened to be in any list was a matter of chance as well as the order in which the lists were used. To further avoid the possibility that even by chance some lists might be more difficult than others and thus affect the results the order in which the lists were used was changed with the different subjects. This fact, together with the additional fact that seventeen lists are averaged together in the presentation of the results, rules out any possibility of a difference in difficulty in the material accounting for our results. For instance each subject worked with the entire 850 associates at one sitting. The first figure presented in the results indicates the average of the first 170 associates, the second figure the second 170 associates, etc. Furthermore, the 170 associates which were learned first by one subject were learned in another part of the period by another subject.

Apparatus

Since it was necessary to keep the subjects at work and not take any time off to determine how much was learned it was necessary to keep some record of the bonds as they were formed. It was necessary to have an exposure apparatus which would expose the entire 850 paired associates the proper number of times with absolutely no interruption from the beginning of the sitting to the end. Any interruption to change rolls or cards in the apparatus would have given a rest period and would have been as fatal as would any interruption to have the individual undergo a test to determine the amount learned.

In order to meet these rigid conditions a special type of exposure apparatus was designed.[1] The apparatus is repre-

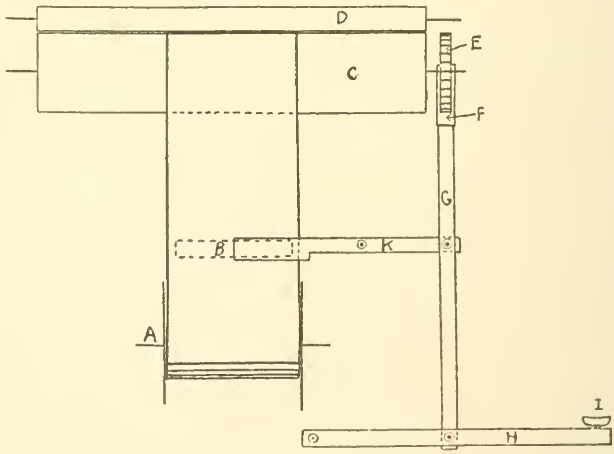

Fig. 1. Paired-associate Exposure Apparatus. Description in text.

sented diagrammatically in Fig. 1. The paired associates were typewritten on a roll of adding machine paper and this roll placed on the bobbin *A*. The end of the roll was then passed behind the exposure opening of the apparatus, which is represented by the dotted lines at *B*, over the larger roller *C* and between roller *C* and the smaller roller *D*. Roller *D* was held tightly against *C* by means of a spring at each end and thus gripped the paper passed between the two. On

[1] The apparatus was constructed in the shops of the Johns Hopkins University.

one end of roller *C* was attached a notched wheel against which played a ratchet, *F*. This ratchet was attached by means of bar *G* to lever *H* so that when pressure was applied to lever *H* at *I* roller *C* turned and pulled the paper the exact distance that a sheet of typewriter paper moves when the double space is used. This movement of the paper changed the material exposed at the apparatus window *B*. Furthermore, it was desired in presenting material to give the subject an opportunity to respond with the proper associate if possible, after the first exposure. This meant that only the first member of the associate be shown until the subject be given an opportunity to respond and then the second member be shown. To accomplish this lever *K* was connected to rod *G* so that when pressure was applied at *I*, half of the exposure opening was covered by *K*. (The apparatus is shown as though *I* were pushed to its lower limit.) When *I* was released the ratchet *F* engaged a new notch in the wheel *E* without turning *C*, and *K* dropped from in front of the opening *B*.

The operation was as follows: The material was all typewritten on the roll of paper in just the order it was to be presented. On the first repetition of a set of associates the experimenter would push the lever *I* and immediately release it. This would bring to the window *B* a complete paired associate—a German word with its English equivalent. After four seconds he would push and again immediately release *I*, thus exposing a second pair. After all ten associates had been shown once they would then appear on the paper in a different order. On the second exposure, however, the experimenter would press *I* down and hold it for two seconds, during which time the subject would try to speak the proper English equivalent of the German word thus exposed. Whether he succeeded or not *I* was released thus showing the English equivalent and two seconds allowed to elapse before it was again pressed and another German word shown. After one set of words had been shown the proper number of times the words "New List" appeared and immediately a new series was presented. Hence, from the very start to the end of the period no rest was given.

The apparatus was equipped with adjustment mechanisms so as to enable the experimenter during the experiment to make fine adjustments and thus insure proper presentation. The light was artificial and was constant throughout. The correct responses of the subject as the experiment progressed were recorded by the experimenter.

Preliminary Experiments

The preliminary experiments are important in that they indicate conditions under which the hypothesis leading to them is not borne out. They also show the steps which led to the formulation of the experiment in its final form. The results from these experiments when considered in connection with later results give our conclusions a form they would not take were these preliminary results omitted.

Table I

First Two Preliminary Experiments

	Divisions of Learning Period			
	1st Quar.	2d Quar.	3d Quar.	4th Quar.
Av. correct res. in learning per..........	7.8	8.0	7.8	7.8
Av. correct res. in relearn per...........	4.5	4.0	4.2	4.5
Percentage...........................	57	50	54	58

Subject *A* was used for four different experiments. In the first two she was presented 17 lists of ten associates each. Each list was presented ten times, the order being varied with each repetition. After twenty-four hours, she was given an opportunity to give the English equivalent of each German word. The order in which the material was presented for this recall differed from that in the learning presentation. In each case whether she made the correct response or not she was shown the correct English associate and then later in the same period given a second opportunity for recall. The second experiment was an exact repetition of the first with different material. Table I. divides the material into four groups, excluding the first set of ten associates, in accordance with the order in which they were presented in the learning period. The first line shows the average number of correct responses made by the subject in learning. The second line gives the average correct responses per list of ten in the recall period. The third line gives the relation of the recall to the learning for each period. It is evident from this table that if fatigue had any effect on retention it was overcome by practice effect, for as a large a percentage was recalled for the last period as for the first.

In the third preliminary experiment the same subject was given a new set of materials in the same way with the exception that only five repetitions were used instead of ten. Two opportunities were again given for recall, the second after a prompting. The results are given in Table II. and indicate that less complete learning did not render the fatigue effect upon retention any more apparent. Sixty-nine per cent. was recalled for the last period as compared with forty-seven for the first period.

Table II

Third Preliminary Experiment

	Divisions of Learning Period			
	1st Quar.	2d Quar.	3d Quar.	4th Quar.
Av. correct res. in learning per.	7.7	7.1	6.6	6.9
Av. correct res. in relearn per.	3.6	2.9	4.2	4.8
Percentage	47	41	64	69

In the fourth preliminary experiment subject *A* was presented 170 nonsense syllable paired associates in the same manner as the German-English associates, ten repetitions were given. After twenty-four hours a recall test was made and not a single associate was recalled. Thirty associates were then selected from the first part of the learning period and thirty from the last part. These sixty associates were mixed up and relearned and the number of errors made in the original learning compared with the errors made in the relearning of the same material. The figures in Table III. indicate that the material learned in the latter part of the first period was not retained as well as that learned in the first part.

TABLE III

FOURTH PRELIMINARY EXPERIMENT

	Errors per List in Learning	Errors per List in Relearning	Percentage
When fresh..........	58	12	21
When fatigued.......	65	20	31

Although this last experiment indicated that the effect sought appeared with nonsense syllables, the work with nonsense syllables was not continued because their use would subject the experiment to the criticism made against them by Thorndike—that they are subject to interference. On the basis of the above experiments it was decided that the subjects probably were not hard pressed enough due to the fact that too many repetitions were given and that probably the work was not long enough continued. It had been first planned to divide the material into sections and try the same subjects with the different sections. Now the material was all arranged so that it could be given to one subject at one sitting and the number of repetitions reduced to four. This meant that the subject would work for nearly four hours and in that time have 850 German-English paired associates presented to him four times. For an individual who had no previous knowledge of German this was no little task.

MAIN EXPERIMENT

Five high-school students were used in this experiment. None of them had any knowledge of German. The learning was conducted on Saturday afternoons beginning at one o'clock. The recall and recognition tests were given the succeeding Monday afternoon. On the first presentation an associate appeared for four seconds and then was followed by another until ten pairs—constituting a list—were presented. Immediately after this first presentation of a list a German word appeared alone for two seconds, and the subject responded with the English equivalent if possible. If he made the correct response the experimenter made the proper record of this fact. Whether he gave the proper equivalent or not the proper word was then shown by the side of the German word for two seconds at the end of which time another German word appeared alone. This process was continued until all the words in a list had appeared four times, including the first appearance. The only respite between lists was for two seconds when the words "New List" appeared at the opening to warn the subject that a new set of associates was to be shown. This was kept up with absolutely no intermission until the entire 850 associates had been treated in the manner described.

Different orders of presentation were used for the different subjects so as to equalize any chance difference in the difficulty of the materials. The material was not presented to a subject in the relearning period in the same order as in the learning but was mixed—some of that used in the first part of the learning period appearing in the latter part of the relearning.

When the subject reappeared on Monday he was first given a number of German words and told to check the ones he recognized as having been used in the experiment two days previously. This list contained about half the German words actually used in the experiment mixed with the same number of German words that had not been used.

After the recognition test the subject was given an opportunity for recall; that is, an opportunity to give the English equivalent for every German word he had studied two days previously. After a prompting he was given a second opportunity for recall. In the tables the recall scores indicate the number of correct responses given in each list of ten associates on the first presentation. The score obtained upon the second presentation—that is, after one prompting—is termed the relearning score.

Table IV. gives the learning score for the five subjects. *A*, *B*, *C*, *D*, and *E* represent five equal consecutive sections of the learning period. Since 850 associates were used in the experiment each of these consecutive periods is the average score for 17 lists or 170 associates. None of these subjects were given any practice in the work but were started directly upon the work. Any practice effect is overshadowed by fatigue and there is a gradual diminution in efficiency until the last section of the learning where there is a slight improvement. This indicates that an individual can adapt himself

Table IV

Learning Score

Subject	Consecutive Sections of Learning Period.				
	A	*B*	*C*	*D*	*E*
E...........	2.10	2.17	1.41	1.49	2.10
F...........	3.20	3.02	2.02	2.78	3.39
G...........	3.67	3.94	2.85	3.18	3.25
H...........	3.55	2.80	2.73	2.06	2.92
I...........	5.57	4.86	5.96	5.33	5.67
Av..........	3.62	3.36	2.99	2.96	3.47

Table V

Recognition Score

Subject	Consecutive Sections of Learning Period				
	A	*B*	*C*	*D*	*E*
E...........	5.33	3.33	3.17	3.17	2.34
F...........	5.25	4.62	4.38	2.88	1.75
G...........	6.00	4.75	2.50	1.38	0.25
H...........	4.38	3.38	2.88	3.00	3.00
I...........	6.36	5.62	5.37	5.37	6.62
Av..........	5.47	4.34	3.66	3.16	2.79

Table VI

Recall Score

Subject	Consecutive Sections of Learning Period				
	A	*B*	*C*	*D*	*E*
E...........	0.71	0.00	0.00	0.12	0.23
F...........	0.41	0.12	0.06	0.30	0.18
G...........	0.24	0.24	0.12	0.18	0.12
H...........	0.12	0.06	0.35	0.18	0.18
I...........	0.53	0.71	0.30	0.30	0.35
Av..........	0.40	0.23	0.17	0.22	0.21

fairly well to the conditions of such an experiment and can persist in spite of any fatigue which might be present. Fatigue in this case cannot be of the same sort as physiological fatigue or such adaptation would have been impossible.

TABLE VII

RELEARNING SCORE

Subject	Consecutive Sections of Learning Period				
	A	*B*	*C*	*D*	*E*
E...........	2.37	1.94	1.12	0.88	1.53
F...........	3.12	2.47	2.06	2.00	2.12
G...........	2.59	2.65	1.71	1.77	1.23
H...........	2.59	1.88	1.94	1.82	2.65
I...........	4.94	4.76	4.94	5.12	4.88
Av..........	3.12	2.74	2.35	2.32	2.48

TABLE VIII

RELEARNING SCORES AS PERCENTAGES OF LEARNING SCORES

Subject	Consecutive Sections of Learning Period				
	A	*B*	*C*	*D*	*E*
E...........	113	90	80	59	72
F...........	98	82	102	72	65
G...........	71	67	60	56	38
H...........	73	67	67	88	91
I...........	89	98	83	96	86
Av..........	89	81	78	74	71

Table VI. gives the number of correct associates which the subjects recalled per list of ten. The number recalled is so small that the results are not significant.

Table VII. gives the score after one prompting. This table indicates that the recall after one prompting is slightly less efficient for the material learned toward the end of the learning period. These results are more significant when compared with the learning scores and are expressed as percentages. Table VIII. gives these percentages and shows a progressive deterioration from the beginning to the end of the learning period. It must be remembered that the recall was carried out with the material in a different sequence from that of the learning period. This table indicates that material learned when one is fatigued is not retained as well as that learned when one is fresh. One subject alone presents an exception to the general result. It must be admitted that the deterioration of ability to retain is relatively slight as can be seen from the graphic representation in Fig. 2.

Table V. and the dotted line in Fig. 2 give evidence of a very significant effect of fatigue. The recognition for the German words studied when the subjects were fresh is vastly superior to that for the German words studied when they were fatigued. This is indication that as work progresses and one becomes loath to go on he tends to narrow himself more and more to the specific thing which he is endeavoring

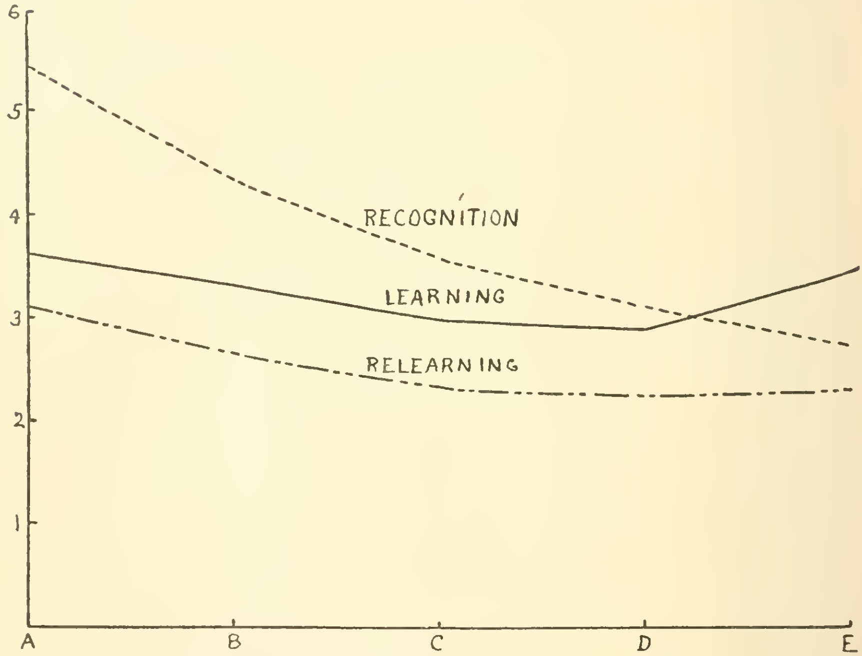

Fig. 2. Relative Effect of Fatigue on Learning, Relearning and Recognition.

to do. When one starts to learn the associates he has some overflow of attention which he can give to details of the elements in the materials with which he is working. As work goes on he excludes more and more of the details which are not strictly in line with the task in hand.

Summary and Conclusions

Experiments have shown that one can memorize for long periods of time with little loss in efficiency, but it is quite possible that the permanency of the bonds formed when one is fresh are more lasting than those formed when one is fatigued. This experiment was designed to ascertain whether material learned at the end of a long period of work is retained as well as that learned at the beginning.

The experiment consisted in having subjects partially learn the English equivalents of 850 German words in one sitting of about four hours. After two days recognition, recall and relearning tests were made to determine the degree of retention for the material learned in the different parts of the learning period.

It was found: 1. There is little loss in efficiency manifested in the learning period. There is a slight drop in the middle followed by a slight spurt toward the end.

2. Of the material learned in the last section of the learning period eighteen per cent. less is retained than that learned in the first section.

3. This result is only apparent when the material is but partially learned, when there is a vast amount of material presented at one sitting and where the work is long continued.

4. Recognition of the material used in the early part of the learning period is much superior to recognition for material used in the latter part. This indicates that adaptation to fatigue involves a narrowing of neural activity to the specific process demanding adaptation.

EFFECTS OF SMOKING ON MENTAL AND MOTOR EFFICIENCY

BY SVEN FROEBERG

Introductory

The question whether or not smoking has any appreciable immediate effect on a person's mental and motor efficiency is one which for several years has been of interest to the writer, both scientifically and personally. This interest was first aroused by certain incidental observations which seemed to indicate a detrimental effect on the higher mental functions.

A series of rough experiments were made on college teachers and students, without, however, obtaining any satisfactory results. Subsequently an opportunity to repeat these on a larger scale and under more favorable conditions was afforded at the psychological laboratory of the University of Michigan. The experiments here reported constitute only a part of the program as originally planned. Change of residence and of work has prevented the completion of the program, and the results are, therefore presented for whatever value they may have for the solution of a problem which just now seems on the point of becoming a social if not a political one.

At the time these experiments were begun there was considerable literature on the subject though little experimental evidence. Such an excellent summary of this literature up to the date of publication was made by Professor Burnham in the *Ped. Seminary* for September, 1917,[1] that it is considered unnecessary to repeat it here.

Such experimental evidence as existed at that time was limited almost entirely to motor functions. The most important was that obtained by Professor Lombard by means of the ergograph. There seemed to be a substantial agree-

[1] Wm. H. Burnham, 'The effect of Tobacco on Mental Efficiency,' *Ped. Sem.*, Sept., 1917, Vol. 34.

ment that the amount of muscular work performed is more or less decreased as a result of smoking.

There is much less agreement with respect to its effect on mental functions. Those who conclude that it is detrimental do so, either because of its detrimental effect on muscular work, or because certain statistical investigations of college students have shown that smokers average somewhat lower in their marks than non-smokers.

It is clear, however, that neither of these reasons is a good one. Mental and motor functions are sufficiently distinct even to a behaviorist, to make it at least conceivable that one function may remain unaffected by conditions which markedly influence the other. And as in many other cases of concomitant phenomena, so also in the case of smoking and low scholarship, it is impossible from the mere figures to determine which is the cause and which the effect. It is quite as conceivable that low scholarship, or the conditions responsible for it, are the cause of smoking as the opposite.

In the absence of satisfactory experimental data the most reasonable conclusions on the subject are perhaps those of Dr. Meylan quoted in the review cited above, to the effect that:

1. The effect of tobacco on adolescents is injurious;
2. On adults the effect is injurious:
   a. In certain individuals suffering from certain nervous affections;
   b. In persons with an idiosyncracy against tobacco;
   c. In all cases where it is used excessively;
3. But that there is no scientific evidence that the moderate use of tobacco by healthy mature men produces any either beneficial or injurious effects that can be measured.

While these experiments were in progress the results of two other investigations in the same field were published. Berry,[1] using himself only as a subject, investigated the effects of smoking on simple addition and found that less time was required and fewer errors made after smoking than when not

[1] Berry, C. S., 'Effects of Smoking on Adding,' *Psy. Bull.*, 1917, Vol. 14, pp. 25–28.

smoking. The fact that every pair of sittings yielded the same general result and that a contrary result was expected by the subject makes these experiments significant as far as this particular subject is concerned.

The other investigation was made by Dr. Bush.[1] He used 17 subjects, 15 who smoked tobacco, 1 who smoked cubebs, and one control, in ten different tests, each test repeated five times after smoking and five times when not smoking. The results show for every test a decrease of efficiency, varying from 3 to 22 per cent., with an average of 10 per cent. for all the subjects taken together. When the subjects are taken separately, as of course they should be, the decrease is from 5 to 18 per cent. when all tests are averaged together, but some of the subjects show no effect, or even a gain after smoking in some of the tests when these are taken separately.

In spite of the apparent care with which this investigation was made there are in it certain defects sufficient to cast serious doubts upon the validity of the results. In the first place the results from the smokers were "corrected" by those from the normal or "control." But the results from only one subject, averaged as they were from only five experiments can scarcely be considered sufficiently "normal" to be used as a standard for the other 15. Secondly, the coefficient of reliability for the individual subjects cannot be calculated, since the variability is not given. Third, the ground upon which certain of the results, which should have been included in the averages, were thrown out as unreliable is not sufficiently explained. To these may be added, the failure to explain how the matter of suggestion was handled, which has proved such a troublesome problem to other investigators.

A third investigation[2] has recently come to the author's attention. The tests were somewhat similar to those used in the present investigation, except that they were fewer and a smaller number of subjects were used. All of the above

[1] Bush, A. D., 'Tobacco Smoking and Mental Efficiency,' *N. Y. Med. Jour.*, 1914, Vol. 99, pp. 519–27.

[2] Effect of Smoking on Mental and Motor Efficiency, *Psychol. Clinic*, June, 1918, and May, 1919. (Oscar J. Johnson.)

criticism apply here also. The superiority of the non-smokers over the smokers amounting to about 1.1 per cent. is too small to be of any significance, and the general conclusion, that "smoking is very detrimental to muscular control and also to the purely mental processes," is not borne out by the figures.

The problem, therefore, still remains to be solved, and the time spent on the experiments reported herewith may not be entirely wasted.

Material and Procedure

The principal difficulty in experiments of this kind, and the one which probably more than any other has served to discourage experiments in this field, is how to eliminate, or at least control, the effect of suggestion. Much of the work on the influence of alcohol and other drugs has been justly criticized for the failure to take this factor into account. Hollingworth in his experiments on the effect of caffeine, and Poffenberger in similar work on strychnine succeeded in eliminating completely the influence of suggestion by the simple expedient of administering the drugs in capsules, and using blank capsules on alternate days. In the case of alcohol similar attempts to disguise the drug have been less successful. Even Dodge and Benedict, who seem to have taken more pains than any previous investigators to neutralize this factor, confess that their mixture of orange juice and other substances succeeded only imperfectly in masking the alcohol. As for tobacco, no attempt whatever seems to have been made in any investigation so far as reported to eliminate or even control suggestion. In one of the investigations mentioned above it is expressly stated, and in the other two it is intimated that a detrimental effect of smoking was expected.

Theoretically there are several methods of eliminating suggestion, but in actual practice serious difficulties were encountered in the case of each one. The first one to suggest itself was to use nicotin-free tobacco on alternate days. It seems that such tobacco has actually been produced, but that there was so little demand for it that it is no longer on the market. There are several ways of extracting nicotin from the tobacco, but since the flavor and aroma is also changed in the process, this method was not considered advisable. A colleague, evidently himself not a smoker, suggested the use of cigars made of dried cabbage leaves on alternate days, but the suggestion was not adopted. It appears, moreover, doubtful if any of the alleged effects of smoking are due to the nicotin, since ordinarily most, if not all of the nicotin is decomposed in smoking and the small amount which may possibly be volatilized, is again condensed and deposited at the bottom of the pipe or in the unconsumed portion of the cigar. The effects, if any, of smoking are held to be due to the various alcaloid decomposition products of the tobacco, the principal of which is pyridin.

But since the characteristic aroma of the smoke seems to be due not to the nicotin nor any of its decomposition products, but to a substance known as nicotinin or tobacco camphor, it was thought a satisfactory method of control might be found by filtering out the former while leaving the latter in the smoke.

Several methods of doing this were tried, among them that of passing the smoke through a succession of bottles of acidulated water. The method finally adopted consisted in passing the smoke through a glass tube filled with absorbent cotton by which the various alcaloid decomposition products were condensed and absorbed. When smoke thus filtered was passed through a solution of silico-tungstic acid, a very delicate indicator of these substances, no sign of their presence could be obtained.

As finally used the smoking apparatus consisted of a sheet metal tube with a cigar holder attached to a rubber stopper, a long rubber tube filled with absorbent cotton, and a narrow rubber tube to the end of which was attached an ordinary cigarette holder. These were then connected in such a way that the air had to pass through the whole apparatus before reaching the smoker. The positions of the sheet metal tube and the glass tube could be interchanged, thus allowing the smoke to be filtered, when they were in one position and allowing it to pass direct to the smoker when in the other. This method was adopted rather than that of dispensing with the filter tube on alternate days, because the difference in "pull" necessary with and without the filter would at once have indicated to the smoker whether the smoke had been filtered or not. There was surprisingly little difference in taste between the smoke thus filtered and ordinary smoke.

In spite of this, however, the method was not entirely satisfactory. The filtered smoke had less visibility than the unfiltered. In some of the subjects this led to more vigorous efforts because they feared the cigar was "going out." In at least one case this was sufficient to indicate to the subject when the smoke was filtered and thus destroyed the value of the apparatus. For this reason only three of the subjects used the apparatus. Such possible methods of overcoming this difficulty as blindfolding the subjects while smoking or smoking in the dark room were not resorted to, since these would have introduced other complicating factors of their own.

In the other subjects counter suggestion was depended upon to eliminate any effects of suggestion. The subject was told that so far there was no experimental evidence to show that smoking had any effect, either favorable or unfavorable, on mental efficiency and were asked to adopt a strictly neutral attitude on the subject. Judging by the statements of the subjects themselves there is every reason to believe that this was also the attitude actually adopted by them. They were, of course, not allowed to see the results while the experiments were in progress.

Cigars rather than cigarettes or pipe were used, because it was desired to eliminate the possible effects of the decomposition products of the paper in the case of the cigarettes, and because a pipe would mean interruption while refilling and relighting. The cigars used were—or at least advertised as—Key West Havana, sold for 5 cents, and varied in weight between 6 and 7 grams. Each cigar as well as the unsmoked part was carefully weighed, and the amount of tobacco actually smoked in each experiment thus determined. The average amount consumed was between 4.5 and 5.5 grams. The nicotin content was analyzed by one of the subjects, who was also taking courses in organic chemistry, and was found to be between 2.29 and 2.33 per cent. dry weight. The smoking period was approximately 30 minutes.

The tests were selected so as to cover as wide a range of mental and motor functions as possible within the 30 minutes alloted to the tests. All were selected from either Whipple's Manual of Mental and Physical Tests or Woodworth and Well's Association Tests, with the exception of the test on attention and the completion test. The motor tests comprise a test of steadiness of motor control, a test of accuracy of voluntary movement, and a test of speed of voluntary movement, listed as Nos. 13, 12 and 10 respectively in Whipple's Manual.

The remaining tests have been classed as Mental. Of these one was intended to be a test of attention and the rest tests of association. The attention test was devised by Prof. J. F. Shepard and used by Dockery in his work on fatigue, but because of imperfections in the apparatus the results of this test have not been included.

The memory test consisted in reading aloud, at the rate of one per ¾ second, ten consonants arranged in such a manner that all combinations likely to suggest meanings were excluded. Two different series were used in each test, and Spearman's foot rule was used in scoring the results.

The association tests comprise, the uncontrolled association, opposites, addition and completion tests. The uncontrolled association test is the same as No. 33 of Whipple's Manual, except for the fact that the stimulus words were taken from the Kent-Rosanoff list.

The addition test was essentially the same as the constant increment test of Woodworth and Wells, except that only the numbers from 11 to 82 not ending in zero were used. The constant increment was 17.

The opposites tests were those of Woodworth and Wells.

The completion test was added because it was desired to include a test of sufficient difficulty to require the use of the most complex functions of intelligence, and because the results of the Trabue Language Scales have shown that this test correlates more highly with general intelligence than any other single test. The test consisted in supplying the missing words in a paragraph of 100 words from which about 15 had been eliminated. The eliminations had been made in such a way that the completion could usually be made by any one of several words but with varying degrees of appropriateness. The subjects were instructed to have regard for both speed and accuracy. The word supplied was graded on a percentage basis by 5 separate judges, except in one set, where one judge made two gradings with an interval of a month between. To combine speed and accuracy into a single score it was assumed that, other things being equal, these vary inversely and in proportion to their coefficients of variability. Both times and grades were then expressed in terms of the Woodworth reduced measures,[1] *i.e.*, the deviation from the average divided by the variability. In the first the standard deviation was used as the coefficient of variability, but since the result was found to be practically the same when the average deviation was used, the latter was adopted in the second set, thereby saving a great deal of time and labor. The measures thus obtained the times were substracted from the corresponding measures for the grades, and the result corrected for unequal difficulty by substracting the similar scores of five control subjects. The reliability of the scores is of course affected not only by the variability of the subjects themselves, but also by the variability of the control subjects and the judges. To make results of this test comparable in reliability with that of the others it would be necessary to increase the number of judges and control subjects as well as the number of test subjects. But as the labor of preparing and scoring the results of this test was already more than twice that of all the test combined, it was not thought that the slight gain in reliability thus obtained would repay the additional work.

[1] R. S. Woodworth, 'Combining the Results of Several Tests,' etc., *Psychol. Review*, Vol. 19, pp. 96 ff.

Since it was necessary to use the same material repeatedly in the memory, opposites, and addition tests, the effect of practice had to be guarded against. To this end the order of the consonants, words, and numbers was different in each experiment.

The 30 minutes preceding the tests were spent in smoking, or in light reading such as newspapers and magazines or in conversation on non-smoking days. The tests usually lasted 3 minutes.

The subjects were all students in the elementary psychology course at the University of Michigan, varying in age from 19 to 24. All of them were what may be termed occasional smokers and had consequently all acquired a tolerance to tobacco, but none were habitual smokers at the time. The original plan provided also for experiments on the latter kind. A certain amount of scholastic credit was given to these subjects for taking part in the experiments, and for this reason they were required to sign an agreement to abstain from smoking or indulging in any alcoholic beverages, tea, or coffee previous to the experiments on the days these were scheduled, to adopt, as far as possible, regularity in the hours or work, sleep, etc., and to observe certain other conditions necessary for the success of the investigation. No experiments were made on days when these conditions did not seem to have been fulfilled.

In order to allow for any possible influence of atmospheric conditions, a complete record was also kept of temperature, pressure, humidity and light conditions.

The pulse rate of each subject before and after smoking was observed at least once during the experiments. In every case except one it was found to be higher after smoking than before.

Results

The results of the first five subjects are given in Table I., the first column of figures being the average record of 8 normal days, the second the difference between this and the score on smoking days, and the third the P.E. of this difference. The sign before the figures in the second column of

TABLE I

Subjects	Steadiness			Coördination			Tapping			Memory			Free Association			Addition			Opposites			Completion	
	Av.	Diff.	P.E.	Av.	Diff.	P.E.	Av.	Diff.	P.E.	Av.	Diff.	P.E.	Av.	Diff.	P.E.	Av.	Diff.	P.E.	Av.	Diff.	P.E.	Diff. Dev./σ	P.E.
Be........	34.0	+10.5	7.8	3.65	−1.08	.27	325.5	− .8	4.0	61.4	− 4.7	3.3	39.4	− .1	.95	74.9	+ .4	3.9	27.9	− .3	1.3	−1.30	.36
Bl.........	64.0	+74.5	7.6	6.63	−2.49	.26	299.6	+ 3.1	3.5	46.9	+ 1.2	2.5	53.5	−1.1	3.4	73.9	−4.6	5.3	40.9	−2.6	1.9	− .11	.38
La........	8.0	− 1.6	2.5	5.84	− .33	.24	300.9	+14.1	5.4	59.8	+ 2.9	3.1	25.8	−2.5	1.3	166.5	−5.0	5.8	34.3	− .4	1.6	+ .74	.36
Ly........	15.4	+23.6	3.2	4.35	−1.06	.28	316.0	− .5	3.3	77.6	− 2.8	3.2	37.1	−1.0	1.8	107.0	+3.9	7.5	32.1	− .5	1.3	− .04	.42
M.........	31.2	+16.8	6.1	4.57	− .92	.30	301.4	+ 9.2	6.2	74.4	−10.7	4.4	45.0	−3.2	1.7	81.9	+2.5	5.6	32.4	+4.0	1.4	− .30	.40
Av........	30.6	+36.8	5.3	5.01	−1.18	.27	308.7	+ 5.0	4.5	64.0	− 2.8	3.3	40.2	−1.4	1.9	100.8	−1.1	5.6	33.5	.0	1.5	− .20	.38

figures indicates whether the score after smoking is higher or lower than the normal.

The score in the steadiness test indicates the number of contacts per minute, in the coördination test the distance at which the first contact was made, in the tapping test the number of taps made during the period, in the memory test the grade according to the Spearman foot rule formula, in the free association test the numbers of words during the ten-second period, in the addition test the time in seconds, in the opposites test also the time in seconds, and in the completion test the combined efficiency score as explained above.

It will be seen from the table that in steadiness all except Be and La and in coördination all except La show a marked decrease in efficiency after smoking. In this respect the results agree with those of Professor Lombard referred to above as well as with a widespread belief among marksmen and training camp instructors that smoking is detrimental to delicate muscular control.

Assuming that a difference in order to be significant should be at least 3 times the P.E. it appears that smoking has no appreciable effect on the speed of movement, at least the sort of movement made in tapping.

The results of the purely mental tests agree neither with those of Bush and Johnson on the one hand nor with those of Berry on the other. In some cases the score is higher after smoking, in other cases lower. But in no case, except that of Be in the completion test, is the difference as much as 3 times the P.E.

It was then suggested that this difference in the results in the two kinds of tests might possibly be due to the fact that the effects of smoking had disappeared by the time the mental tests were given, since these came last on the program. In order to remove all uncertainty on this point, the experiments were repeated on a new set of subjects, using only the mental tests. This made it possible to give the tests twice each day, immediately before and immediately after smoking. Each series of tests occupied 15 minutes and the half-hour intervals between them was spent in smoking or, on alternate days, in conversation or light reading. The method adopted in scoring was that used by Dodge and Benedict in their investigation of the effects of alcohol, viz. $E = (S - Ns) - (N_2 - N_1)$, where E represents general efficiency, S the results after smoking, Ns the results before smoking, N_1 the results of the first series on non-smoking days, and N_2 those of the second on non-smoking days. Twenty-four series in all were given, yielding an average from six differences between smoking and non-smoking days. In all other respects the conditions were the same as before. The results are given in table II.

TABLE II

Subject	Memory		Free Association		Addition		Opposites		Completion	
	Av. Diff.	P. E.	Av. Diff.	P. E.	Av. Diff.	P. E.	Av. Diff.	P. E.	Diff.	P. E.
Bu	59.6 − 14.0	6.1	40.3 − 6.3	2.0	62.7 + 2.6	4.4	17.3 − .6	.6	−1.51	2.54
H	69.8 − 14.0	6.3	31.7 − 3.0	1.1	49.9 − 1.8	1.0	16.8 + 1.9	.4	−.92	1.20
P	64.8 − 24.0	3.8	44.8 − 7.8	2.8	53.9 + 4.2	2.3	20.0 + 1.2	1.2	− .37	1.33
R	51.6 − 13.2	5.1	23.0 − 3.5	1.8	44.8 + 1.8	2.2	13.1 + .3	.8	− .12	3.31
W	50.7 + 16.8	6.4	23.0 − 1.8	.8	34.9 + 6.1	3.8	11.9 + .6	.4	−2.29	1.74
Av.	59.3 − 9.7	5.5	37.6 − 4.5	1.7	49.2 + 2.6	2.7	15.8 + .7	.7	−1.04	2.02

The results differ in no respect from those of the first set. If the two sets of mental tests are taken together it is seen that in only 4 cases out of 50 does the difference exceed 3 times the P.E., in 26 cases it does not exceed the amount of the P.E., and in 12 cases the result gives a higher score after smoking.

There is, on the other hand, no clearly indicated improvement such as the 7.7 per cent. found by Berry, the nearest approach to it being subject La, whose differences, however, in no case meet the requirements of reliability mentioned above.

Two subjects, P and R, show throughout a loss after smoking, but in only one case is this loss greater than three times the P.E. It is possible that if more practiced subjects had been available and the repetitions of the tests increased, the chance variability might have been reduced sufficiently to show significant differences.

It is also conceivable that in such a case individual differences among the subjects might be discovered such as that suggested by subject La on the one hand and subjects P and R on the other. It might be expected that some habitual smokers would show improvement after smoking and that the opposite would be true of non-smokers. Experiments on such subjects were contemplated in the original plan.

As far as the results of these experiments go, and as far as the immediate effect of smoking is concerned, they tend to support the conclusions of Dr. Meylan quoted above: that, except when used in excess, by adolescents, by persons having an individual idiosyncrasy against tobacco, or by persons suffering from certain nervous affections, there is no scientific evidence that the moderate use of tobacco, in smoking produces any either beneficial or injurious mental effect sufficiently great to be measured.

A STUDY OF OCULAR FUNCTIONS WITH SPECIAL REFERENCE TO THE LOOKOUT AND SIGNAL SERVICE OF THE NAVY

BY C. E. FERREE, G. RAND AND D. BUCKLEY

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The incentive for this work was the need for establishing a system of testing for those branches of service in the Navy requiring especially keen scotopic or low illumination acuity. The first step towards the accomplishment of this purpose was the devising of a suitable apparatus and test method. The request for an apparatus came to us from the head of the Eye Division of the United States Naval Hospital at Washington. The apparatus was described in a former paper, "An Apparatus for Determining Acuity at Low Illuminations, etc.," this JOURNAL, 1920, III., pp. 59–71. A further need was to find out what range of difference in scotopic acuity might be expected among eyes graded as fit on the basis of the tests of other functions and capacities. A consideration of this need has led us to make a preliminary survey of eyes graded as normal with regard to photopic acuity and other commonly tested functions in order to determine whether such eyes may be expected to show a significant difference in keenness of functioning at low illuminations.

In a thorough test for vocations requiring keenness of discriminations at low illuminations, the following points should be taken into account: (1) the minimum amount of light required to discriminate the test-object before adaptation; (2) the minimum amount after a properly selected period of adaptation; and (3) the rapidity as well as the amount of gain in acuity in the process of adapting. Determinations covering all of these points have been made in this study.

The Range of Illumination Required by Normal Eyes for the Discrimination of the Standard Test Object

In making these determinations three test-objects were used: the Snellen chart and two single test-objects which could be rotated into different positions—the letter E and the international test-object, the broken circle, each subtending a 5-min. angle. In case of the latter two, the task required of the observer was to indicate roughly the direction in which the opening in the test character was turned, an objective check being had on the correctness of the judgment. The determinations were made at the beginning and end of a 45-min. adaptation period. It is obvious that the results at low illumination should be influenced by the refraction condition of the eye as well as by its light sensitivity and the individual differences in the effect of light sensitivity on acuity. In order that the observers could be chosen so that the results would represent the differences which may occur among eyes having normal or better than normal photopic acuity, each eye was refracted and the acuity was taken under 5 foot-candles (53.8 meter-candles) of light. In the first series of tests 22 observers were used ranging from 18 to 28 years of age. Results were obtained for both eyes and for each eye separately. Of the eyes used, 75.7 per cent. would be rated in the Snellen scale as having 6/4 acuity; 13.5 per cent. as having 6/5 acuity; and 10.8 per cent. as having 6/6 acuity. It was our intention to use throughout only eyes which could be ranked as Grade A with regard to photopic acuity.

The results of these determinations show a greater range of individual difference for the broken circle than for the Snellen chart or the letter E (905 per cent. for the broken circle, 548 per cent. for the letter E and 357 per cent. for the Snellen chart). This superior showing for the broken circle is perhaps in accord with the general finding that the broken circle as a test-object picks up smaller differences in acuity than either of the other two test-objects employed. These differences too, it will be remembered, are amplified in the present case by the fact that they are read on the illumination scale—an amplifying scale—and not on a scale which sustains a 1 : 1 relation to acuity. Inasmuch as the greater sensitivity was shown in these preliminary experiments by the broken circle as a test-object, 15 additional observers (photopic acuity, 6/4) were employed using this test-object alone.

Space will be taken here only for a brief general statement of the results for this latter series of determinations alone. (1) The individual differences in the minimum illumination required for the discrimination of the test-object before the period of dark adaptation fell between 0.70 and 5.29 meter-candles, a range of 657 per cent.; after the period of dark adaptation it fell between 0.32 and 2.2 meter-candles, a range of 593 per cent. A greater range of individual difference, it will be noted, was found for the tests taken before the period of dark adaptation than for those taken after the 45-minute adaptation period. This was doubtless in part due to the lack of careful standardization of the initial sensitivity by a period of preadaptation to light of a fixed intensity and to small individual differences in photopic acuity revealed by the more sensitive method of testing; and in part to individual differences in the amount and rate of adaptation. A careful initial standardization of sensitivity was purposely avoided in this preliminary work with the apparatus in order more closely to approximate the rough conditions of testing which are apt to prevail in the selection of men with reference to vocational fitness. The results of these determinations are shown in Table I.

(2) Thus far without exception the two eyes of the same observer have required a different amount of light just to discriminate the test-object. This difference has ranged after adaptation from 19 to 54 per cent. of the amount of light required for the better eye.

(3) A question is often raised with reference to points of advantage of binocular as compared with monocular seeing. In 6 per cent. of the number of cases tested, the binocular result after adaptation was equal to or approximated the result for the poorer eye; in 88 per cent. of the cases it was better

Table I

Showing the Amount of Light Required just to Discriminate the Test-object at the Beginning of Dark Adaptation and at the End of 15, 30 and 45 Minutes (15 Observers)

In these experiments there was no standardization of the initial sensitivity by a previous adaptation to an illumination of constant intensity.

Observer	Photopic Acuity	Illumination in Meter-candles Required Just to Discriminate the Test-Object				Difference in Illumination Required at Beginning and at End of 45 Minutes	
		Beginning	At End of 15 Min.	At End of 30 Min.	At End of 45 Min.	Meter-candles	Per Cent.
G.......	6/4	0.70	0.55	0.35	0.32	0.38	118.8
M......	6/4	1.00	1.00	0.82	0.82	0.18	21.9
Mc......	6/4	1.24	1.00	1.00	1.00	0.24	24.0
R.......	6/4	1.36	0.60	0.50	0.60	0.76	126.7
L.......	6/4	1.55	1.00	0.88	0.88	0.67	76.1
S.......	6/4	1.75	1.42	0.88	0.88	0.87	98.9
Th......	6/4	2.11	0.82	0.94	0.94	1.17	124.5
Y.......	6/4	2.11	1.55	1.49	1.42	0.69	48.6
St......	6/4	2.40	0.60	0.60	0.60	1.80	300.0
Sw......	6/4	3.43	2.17	2.20	2.20	1.23	55.9
K.......	6/4	3.90	2.81	2.40	2.10	1.80	85.7
T.......	6/4	3.97	1.18	0.82	0.88	3.09	351.1
Sm......	6/4	4.10	3.80	1.40	1.30	2.80	215.4
W.......	6/4	4.20	1.40	0.76	0.76	3.44	452.6
Ba......	6/4	5.29	2.11	2.02	2.11	3.18	150.7

than the result for either eye; and in the remaining 6 per cent. of the cases it was intermediate to the results obtained with the two eyes separately. In none of the cases tested separately was it equal or approximately equal to the result for the better eye. In the 88 per cent. of cases referred to, less light was required for the discrimination of the test-object by the two eyes than by the better eye alone by amounts ranging from 14.5 to 67.3 per cent.

In order to get a rough idea of the grouping of the 15 observers with reference to the minimum amount of light required to meet the standard of acuity imposed by the test, before and after the period of dark adaptation, they have in each case been divided into six equally spaced groups, each group for the work before adaptation covering a range of 1 meter-candle and for the work after adaptation a range of 0.4 meter-candle. For the tests before adaptation 13.3 per cent. fall in the first or best group; 26.7 per cent. in the second group; 20 per cent. in the third group; 20 per cent.

in the fourth group; 13.3 per cent. in the fifth group; and 6.7 per cent. in the sixth group. For the tests after adaptation 6.7 per cent. fall in the first group; 20 per cent. in the second group; 40 per cent. in the third group; 13.3 per cent. in the fourth group; none in the fifth group; and 20 per cent. in the sixth group. A graphic representation of this grouping is shown in Fig. 1.

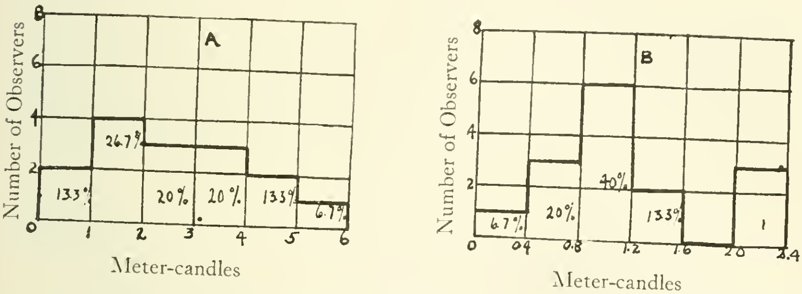


Fig. 1. Representing the relative distribution of 15 observers graded with reference to the minimum illumination required just to discriminate the test-object: *A*, before adaptation; *B*, after a 45-minute period of dark adaptation.

Individual Variations in the Amount and Rate of Adaptation in Terms of Effect on Acuity

The preceding experiments furnish data with regard to the minimum amounts of light required just to discriminate the different test-objects at the beginning and end of the 45-minute adaptation period. In case of the 15 observers tested with the broken circle in the final series of experiments at the beginning of dark adaptation and at the end of 15, 30 and 45 minutes, the minimum difference in this amount of light was 0.18 meter-candle or 22 per cent. of the amount required at the end of the adaptation period; the maximum difference was 3.44 meter-candles or 452.6 per cent. of the amount required at the end of the adaptation period. These results are shown in Table I.

In addition to these experiments a special adaptation series was run in which the minimum illumination required just to discriminate the test-object was determined at the beginning of the adaptation period and at the end of 5, 10, 15, 25, 35 and 45 minutes. In order to standardize the initial sensitivity of the eyes of the observer, a preadaptation period of 20 minutes was given to 80 foot-candles of light (vertical component), the skylight illumination of an optic's room on a medium bright day. A few of the results obtained are represented in Table II. and Fig. 2. These results, it will be remembered, represent adaptation only as it affects acuity, which is the effect of greatest importance to the special work for which the apparatus was devised and the effect with which we are the most concerned for the greater part of our working lives. A comparison of these results with those of similar

Table II

Showing the Amount of Light Required just to Discriminate the Test-object at the Beginning of Dark Adaptation and at the End of 5, 10, 15, 25, 35, and 45 Minutes (6 Observers)

In these experiments the initial sensitivity was standardized by 20 minutes preadaptation to 80 foot-candles of light (vertical component), the illumination of a skylight optics room on a medium bright day.

Observer	Photopic Acuity	Illumination in Meter-candles Required Just to Discriminate the Test-object							Difference in Illumination Required at Beginning and at End of 45 Minutes	
		Beginning	At End of 5 Min.	At End of 10 Min.	At End of 15 Min.	At End of 25 Min.	At End of 35 Min.	At End of 45 Min.	Meter-candles	Per Cent.
I...	6/4	0.55	0.505	0.42	0.35	0.32	0.35	0.35	0.20	57.1
II...	6/4	0.705	0.42	0.42	0.32	0.32	0.32	0.38	0.325	85.5
III...	6/4	1.06	0.76	0.60	0.46	0.35	0.46	0.46	0.60	130.4
IV...	6/4	1.12	0.82	0.52	0.32	0.32	0.38	0.38	0.74	194.7
V...	6/4	1.62	1.12	0.94	0.55	0.60	0.60	0.55	1.07	194.5
VI...	6/4	2.20	1.12	1.14	1.18	1.36	1.24	1.24	0.96	77.4

series in which the object is to measure the increase in light sensitivity as a result of dark adaptation shows that just as acuity increases slowly with increase of illumination (except at very low illuminations) so also does it increase slowly with increase of sensitivity to light. That is, the eye does not gain in acuity by adaptation nearly so fast as it gains in light sensitivity.

In Fig. 2 the actual amounts of illumination required just to discriminate the test-object are plotted against time of adaptation. It thus affords a comparison of the position of the minima of the different observers in the illumination scale and comprehends data from which the following points can be determined: (*a*) their relative ranking with regard to

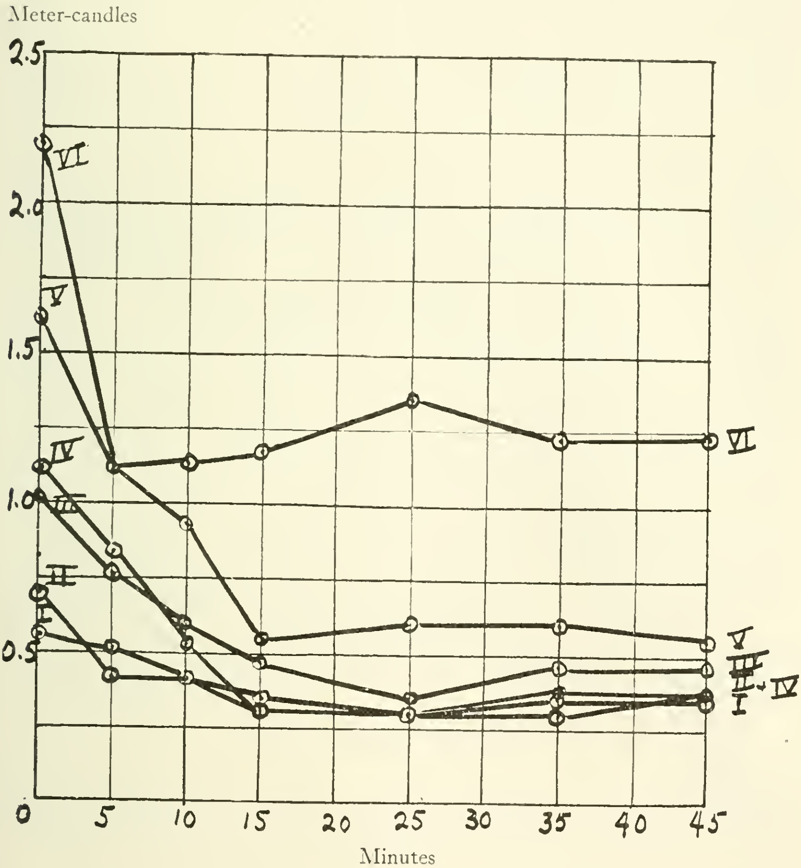


FIG. 2. Curves showing the decrease in the amount of light required just to discriminate the test-object as the result of dark adaptation. In order to standardize the initial sensitivity, the eye was preadapted in each case for 20 minutes to 80 foot-candles of illumination (vertical component).

scotopic acuity before and after adaptation, rated on the illumination scale; (*b*) their light sensitivity before and after adaptation insofar as it affects the minimum amounts of light required to discriminate the test object; and (*c*) their relative amounts of change in sensitivity, measured in terms of effect on acuity, read on the illumination scale, due to adaptation. All of these features are important for vocational and clinic classification. In order to make these results more directly comparable with reference to the last of these points, namely

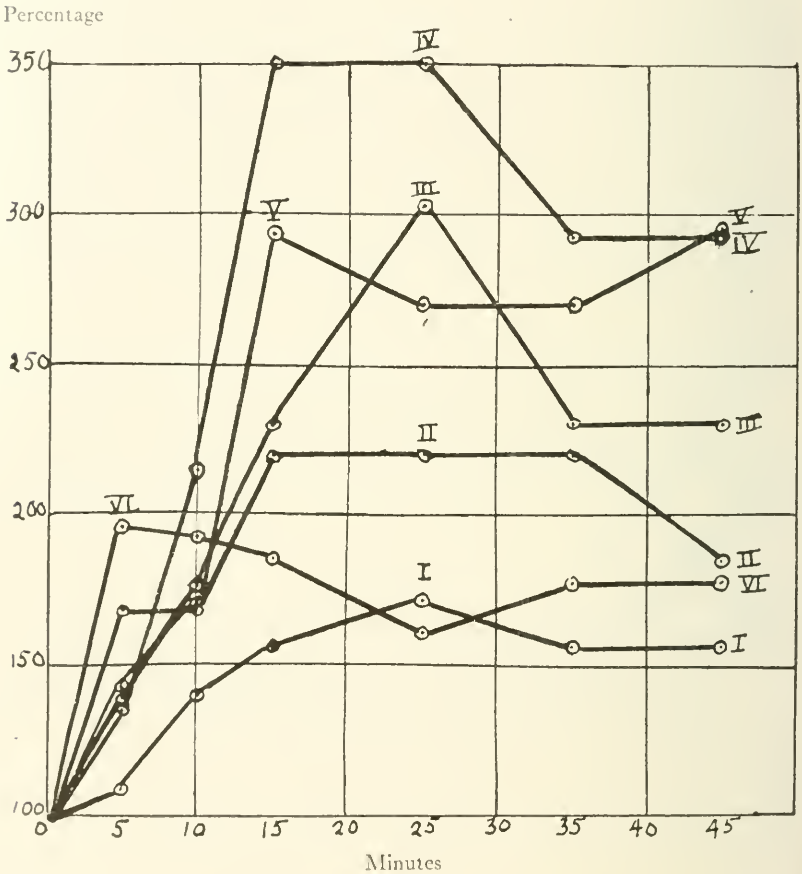


FIG. 3. Curves showing the increase in sensitivity as the result of dark adaptation, the reciprocal of the amount of light required just to discriminate the test-object being taken as the measure of sensitivity. Percentage increase in sensitivity is plotted against time of adaptation. The initial sensitivity of the eye was standardized in each case by 20 minutes of preadaptation to 80 foot-candles of light, vertical component.

relative amounts of change in sensitivity due to adaptation, the ratios or percentages of increase in sensitivity are plotted in Fig. 3, the reciprocals of the minimum amounts of light required just to discriminate the test-object being taken as the measure of sensitivity. That is, the ratio or percentage change in the value of these reciprocals is plotted against time of adaptation, the curves beginning at a common point or unit ratio. The relative ratings with regard to the second point could of course be represented by plotting the reciprocals themselves. Space will not be taken here for this representation.

It will be noted that the greater part of these observers reach their maximum acuity at the end of 15 minutes of adaptation and that some even show a lower acuity if the series is continued beyond this time. The loss in the latter case is doubtless due to fatigue of the muscles of adjustment. That is, in case of the observers more susceptible to muscle fatigue, the loss of acuity due to fatigue more than compensated for the small gain in light sensitivity after the first 15–25 minutes. In this connection it may be noted that the muscle strain imposed by taking the acuity at the minimum illumination is much greater than at the illuminations ordinarily used. Even with a 5–10 minute rest period between determinations and a 2-second interval between the individual observations making up one determination, a very noticeable fatigue was present at the end of the 45-minute series.

In testing fitness for the lookout and signal service work of the Navy, night flying, and for other work and vocations that require the keen discrimination of objects when small amounts of light enter the eye, it was deemed better to make the tests in terms of acuity rather than of the light sense. Retinal sensitivity is only one of the factors in the eye's power to see its objects at low illuminations. For example, we frequently find observers with an excellent light sense whose scotopic acuity or power to discriminate objects at low illuminations is poor. Indeed, as shown in this and former papers, slight differences in resolving power correspond to relatively large differences either in illumination (except for very low illuminations) or retinal sensitivity in their effect on the eye's power to see clearly at low illuminations. Any test therefore for fitness for tasks or work requiring the power to see clearly at low illuminations is far from complete which leaves out of account resolving power and the varying effect of changes in illumination or in light sensitivity on acuity. The acuity test, on the other hand, includes all of the factors involved in seeing and in the exact proportions in which they are contributory to seeing. Moreover, it is much better directly to determine the candidate's power to see clearly at low illuminations than to try to infer this from a light sense test and data on acuity taken at higher illuminations. This was, we may say, also the point of view of the Naval authorities under the auspices of whom we undertook to devise an apparatus suitable for testing acuity at low illuminations.

THE BLOCK-DESIGN TESTS

BY S. C. KOHS[1]

Portland, Oregon

A brief presentation of the Block-design Tests will be attempted in this article. These tests fall in the category of 'performance tests' and have been standardized to measure intelligence. They have been purposely devised to eliminate the factor of language. In this attempt the writer believes he has been especially successful since the instructions themselves may be given entirely through pantomime and imitation.

There has indeed been, and there still is, a great need for tests such as are here presented. In the longer monograph which the writer is preparing for publication, there will be more detailed treatment of many topics, such as the definition of intelligence; an analytic criticism of current methods of standardization; suggested newer statistical procedure; the relation between language ability, performance and intelligence; and other pertinent material.

The content of the present article has been divided into six sections:

(*A*) The Test Material:
  1. The Blocks.
  2. The Designs.

(*B*) The Directions for Applying the Tests:
  1. For Subjects Who Can Understand Spoken Language.
  2. For Subjects Who Do Not Know the Names of the Colors.
  3. For Subjects Who Cannot Understand Spoken Language.

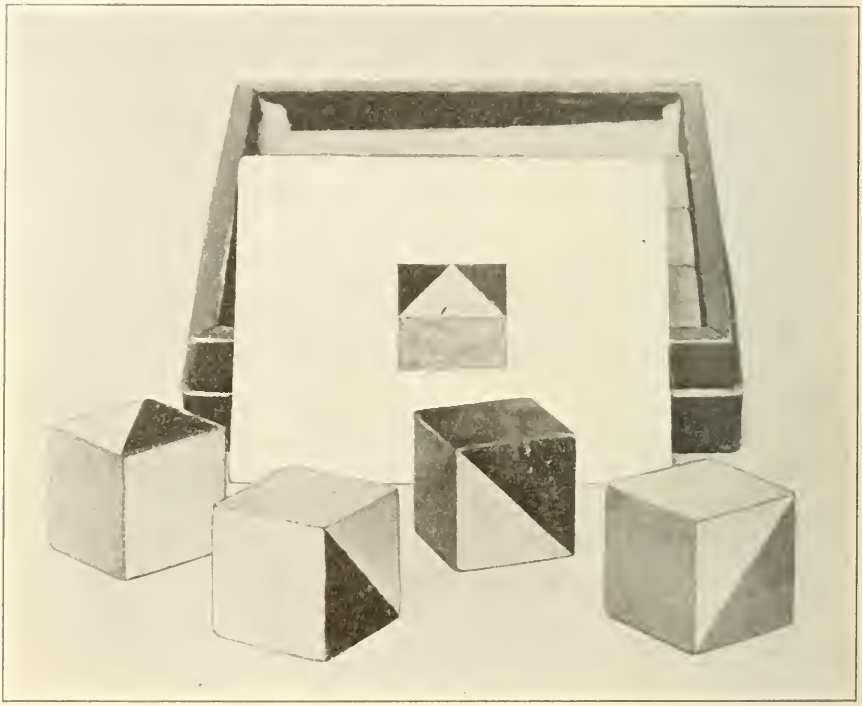
(*C*) The Score Card and Methods of Scoring.

(*D*) The Norms.

[1] Psychologist to the Court of Domestic Relations.

(*E*) The Reliability of the Tests.
(*F*) Serviceability.

In the promised monograph more complete details will be presented which would be out of place in this brief article.



The Block-Design Test.

(*A*) The Test Material

1. *The Blocks*

The Blocks which are used are manufactured by the Embossing Co., and may be secured at any of the large department stores and at various distributing centers of Milton Bradley's. There are sixteen cubes of one inch dimension and all are painted as follows:

One side red
One side blue
One side white
One side yellow

One side blue and yellow (divided on the diagonal)
One side red and white (divided diagonally)

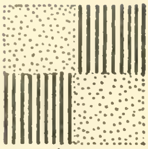
The character of the colors is indicated on the page of designs (pp. 360–1) in this article. A slight difficulty experienced by possibly one or two subjects out of every 100 was a just perceptible but nevertheless disconcerting difference in shade between the blue and yellow on the full faces and the same colors on the diagonal sides. This can be remedied in the later standardization of the test material. One set of the blocks will last through the examination of from four to five hundred children without showing much wear and tear. After that the cubes can be repainted without difficulty.

It is interesting to watch the response of children and even adults when they are given colored cubes to handle. There is no doubt that an appeal exists which touches the roots of some very fundamental original tendencies. Of all the subjects tested, not one has manifested any absence of a desire to *combine* these cubes in some fashion. The experimenter needs only to direct this natural interest toward a specific end and then apply a scientific measuring technique to evaluate the results.

2. *The Designs*

In Chart I. the seventeen designs utilized in this test are represented. The Arabic numerals designate the final numbering of each design. The original number was 35 but fifteen were eliminated in a few of the early preliminary testings. The designs are graded in difficulty which increase by modifying the designs at various stages in the following manner:

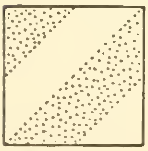
1. By the use of the full colors;
2. By the use of few diagonaled sides;
3. By the use of all diagonaled sides;
4. By turning the design on one of its corners;
5. By eliminating the outside boundary line;
6. By increasing the number of blocks to be used;
7. By increasing dissymmetry in design;
8. By decreasing the number of different colors used in each design.

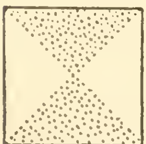

Design 1

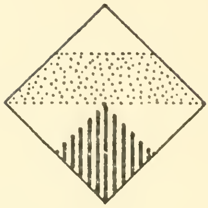

Design 2

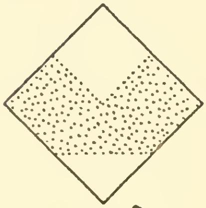

Design 3


Design 4

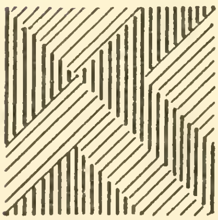

Design 5

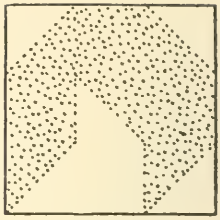

Design 6


Design 7

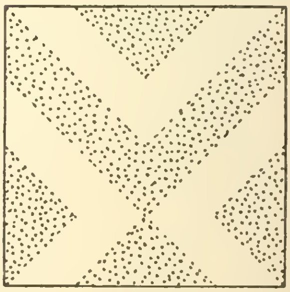

Design 8

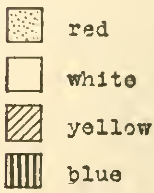

Design 9


Design 10


Design 11


Design 12


Design 13



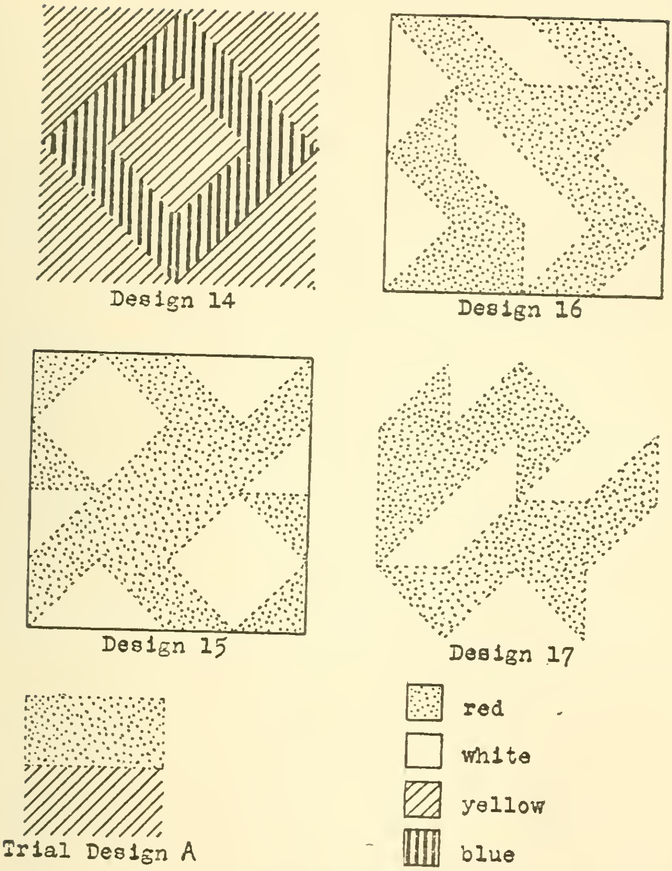


CHART I. Block Designs.

Designs: 1, 2, 3, 4, 5, 6, 7, 8, 9—4 blocks.
Designs: 10, 11—9 blocks.
Designs: 12, 13, 14, 15, 16, 17—16 blocks.
Colors (Windsor & Newton, Ltd.): red = carmine lake; blue = prussian blue; yellow = pale cadmium.

To perform the test, utilizing twenty designs, one averaged about an hour or an hour and a half. In the final revision three designs have been eliminated, leaving seventeen, thus decreasing somewhat the time necessary to apply the tests with no significant decrease of reliability. The criteria for rejection were based on correlations with those arrays of evidence presuming to yield an index of intelligence, such as is obtained through the use of the Binet Scale, as also upon the basis of the diagnostic value of each design determined by the progress of its curve with increasing chronological age. The results at present indicate that the block designs are as good as any single test in the Binet scale (though better in the sense of *diagnostic value*), as good as the Trabue Language Completion Tests, or any other similar single type test, whether involving the use of language or whether mere performance.

The designs, appropriately colored, are printed on medium-thick, white, semi-gloss cardboard. The dimensions of the card are 3 by 4 inches. The printed designs, placed in the center of the card, are *one fourth* the size of the actual designs when the cubes are used. In other words, the face of a cube represented on the designs is only *one half* of an inch on each of its sides. Thus design no. 1 is one inch square, design no. 10 is one and a half inches square, and design no. 14 is two inches square.

The writer has found it of assistance to place in the lower right-hand corner the time limit for each design. These values follow:

TABLE I

TIME LIMITS FOR EACH DESIGN

Design (Number)	Time Limit (Minutes)	Design (Number)	Time Limit (Minutes)
1	$1\frac{1}{2}$	10	3
2	$1\frac{1}{2}$	11	$3\frac{1}{2}$
3	$1\frac{1}{2}$	12	$3\frac{1}{2}$
4	2	13	$3\frac{1}{2}$
5	2	14	$3\frac{1}{2}$
6	2	15	4
7	2	16	4
8	2	17	4
9	2		

The time limit as set for each design is about one minute longer than the time within which a correct response may reasonably be expected.

It may be of interest to remark that if the full limit is allowed on each test the working-time totals only 45 minutes for all of the seventeen designs. With practice an examination should average about thirty to forty minutes. In some cases it may take only fifteen or twenty minutes, in others perhaps an hour.

(*B*) The Directions for Applying the Tests

Preliminaries

Seat the subject comfortably at a table, noting that his visual angle when working with the tests is not less than 45 degrees. Be sure that no designs are visible in your preliminary instructions, nor more than a single design at any one time. The blocks which are not being utilized should be kept in a box, apart, so that they are either invisible to the subject, or if visible, the blocks should be arranged so that the top sides are all of the same color.

Method: Part 1. For Subjects Who Can Understand Spoken Language

(Section *A*) Take a block. (Instructions to subject are placed in quotation marks. For Design 1 four blocks will have been removed from the box.) "Here are some blocks,—give me the name of the color on this side." Sides with the full color are presented first. Place your finger on the side designated. After the subject has responded, turn to another side. "And what is the color on this side?"—"Now the color here?"—"And what is the color here?"—If the subject has succeeded in naming the colors correctly, proceed with the experiment. (If he has failed, further instructions are given below, in Part 2.) Then the experimenter explains: "Now on this side we have blue and yellow, (point), and on this side red and white (point). And all the blocks are painted in the same way."

(Section *B*) "What you are to do is this: Take these blocks," (Shuffle them so that when finally placed before the subject, no more than one fourth of the blocks have topside colors which are present in the design, the separate blocks being placed apart, flat on table, and not piled one on top of another) "pick out the right colors, put them together, and make them look, on top, just like this." (Point to design 1.) Give no further hints nor suggestions if the directions have been understood. CAUTION: Be sure that all the blocks are thoroughly shuffled *before* the design is presented. The purpose is to eliminate the possibility of studying the design before being ready to begin work with the cubes.

(Section *C*) If the subject has not understood what is meant, the experimenter may perform trial design (*A*)[1] slowly, using pantomime freely, the subject watching closely, after which the subject is requested to repeat the operation. This may be repeated any number of times until the subject understands. When he does, proceed with the designs in order, beginning at (Section *B*), and continuing with (Section *D*).

(Section *D*) After the first design has been completed or failed, the blocks are again shuffled, observing the cautions in (Section *B*), and the subject is told again to "Take these blocks, pick out the right colors, put them together, and make them look on top just like this." (Point to design number 2.) The instructions remain the same for all the designs. The subject is not told at any time the number of the blocks he is to use.

Record.—Both time and moves are recorded. A move is counted when a block is given its initial position on the table. Each separate and distinct change in the position of a block is counted a move. Sometimes a child will make three or four changes in the position of a cube, the topside remaining the same color (especially true of diagonal sides, *e.g.*, red-white). But each change in position is counted a separate move. If success is not attained within the time limit, no credit is assigned. The time limits are indicated on the design cards.

The whole test is not regarded as complete unless there are, ordinarily, at least five consecutive failures on designs after the last success, and where doubt exists as to inability in the later designs, give as many designs beyond the last success as is deemed wise.

[1] Trial Design (*A*) is represented on the pages with the other designs and is used only when under the provisions of Section *C* further preliminary explanation is necessary. Trial Design *A* is a four-block design, two full red sides above, two full yellow sides below.

Part 2. For Subjects Who Do Not Know the Names of the Colors

Take all the blocks out of the box and place on the table so that the single-colored faces are all on the top side of the cubes. Have an equal number of reds, yellows, blues and whites. Point to a red-topped block and ask the child to point to all the blocks that have the same color on top. Do the same for the other three colors. If the child can distinguish the colors, proceed with the test at (Section *B*).

Part 3. For Subjects Who Cannot Understand Spoken Language

By means of gestures and pantomime go through the procedure in Part 2. If the subject can distinguish the colors, proceed with (Section *C*), and through the various designs. The method of recording remains the same.

(*C*) THE SCORE CARD AND THE METHOD OF SCORING

In the following table are presented the score values of each of the seventeen designs and the number of score points to be deducted if a design is successfully completed with excess time and with excess moves:

TABLE II

SCORE CARD

Design No.	Score Value	Points to be Subtracted		
		Time		Moves
		1 Point	2 Points	1 Point
1	3	21″ and over	——	6 and over
2	5	31″ and over	——	7 " "
3	6	21″ to 35″	36″ and over	8 " "
4	6	31″ to 1′ 0″	1′ 1″ " "	10 " "
5	7	36″ to 1′ 5″	1′ 6″ " "	11 " "
6	7	36″ to 1′ 0″	1′ 1″ " "	12 " "
7	7	41″ to 1′ 10″	1′ 11″ " "	11 " "
8	8	41″ to 55″	56″ " "	10 " "
9	9	56″ to 1′ 10″	1′ 11″ " "	15 " "
10	9	1′ 56″ to 2′ 10″	2′ 11″ " "	22 " "
11	8	1′ 46″ to 2′ 30″	2′ 31″ " "	19 " "
12	9	2′ 26″ to 2′ 40″	2′ 41″ " "	30 " "
13	9	2′ 21″ to 2′ 33″	2′ 34″ " "	31 " "
14	9	2′ 26″ to 2′ 40″	2′ 41″ " "	32 " "
15	9	2′ 41″ to 3′ 0″	3′ 1″ " "	32 " "
16	10	2′ 41″ to 3′ 5″	3′ 6″ " "	31 " "
17	10	2′ 41″ to 2′ 55″	2′ 56″ " "	30 " "

Maximum score—131 points.

To clarify the table, one or two illustrations will be utilized. For example, design number two has a score value of 5. This full amount is attained if a reagent completes the design successfully in less than 31 seconds and with less than 7 moves. If 31 or more seconds are utilized, one point is deducted from the score, and if 7 or more moves are made an additional point is deducted. Take again design number thirteen which has a score value of 9. This full amount is attained if the subject completes the design successfully in less than 2 minutes and 21 seconds, and with less than 31 moves. If completed between 2 minutes 21 seconds and 2 minutes 33 seconds, one point is deducted, if 2 minutes 34 seconds or more are spent on the problem, two points are deducted. And if 31 or more moves are made an additional point is deducted from the score value of the design.

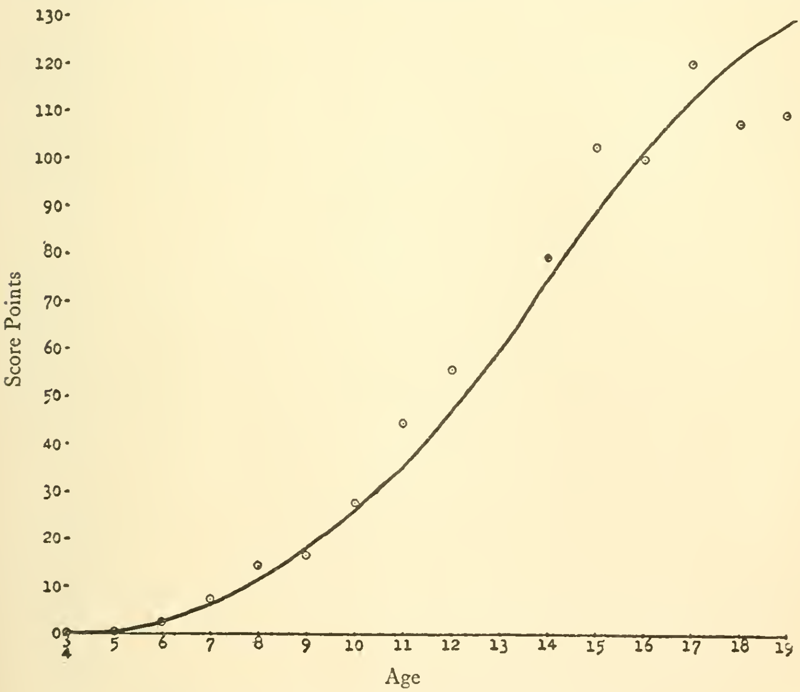
The scoring of a performance is a very simple matter. This will be self-evident from the following examples:

Example one: Design number 7 successfully completed in 1 minute and 23 seconds and at the end of 9 moves. Score 7, for successful completion, less 2 points for excess time. Final score 5. *Example two:* Design number 10 successfully completed in 1 minute 48 seconds, and after 19 moves. Score 9, for successful completion. No deductions for excess time or excess moves. Final score, 9. *Example three:* Design number 16, successfully completed in 3 minutes 27 seconds, and after 48 moves. Score 10, for successful completion. Deduct 2 points for excess time, and one point for excess moves. Final score 7.

It may be worth remarking that successful performance, speed and what may be termed accuracy are all combined in the final score. Successful performance receives greatest weight, speed next and accuracy next. The weight ratio as explained elsewhere in the monograph is roughly 4 : 2 : 1. This ratio has been empirically determined and was not derived by arm-chair philosophizing. The prevalent opinion, which was at one time shared by the writer, that speed and accuracy cannot be combined in one score, does not hold with the Block Design Tests. The writer felt that success, speed and accuracy each had its own diagnostic importance and in order to make the tests most effective all should and must be taken into account in the final score summation. But more of this in the longer monograph.

(*D*) The Norms

The procedure involved in obtaining norms for the different designs was quite a complicated one, requiring a great deal of careful statistical work. In this effort the writer utilized the currently accepted standardization methods, with but slight modification. An explanation of the general pro-



Graph I. Mental Age Equivalents of Score Points.

cedure utilized together with a description of various methods of checking the results has been left for the later monograph. Suffice it to say that the score points mentioned in Table II. are to be interpreted in the same light as those of Buckingham in his standardization of his spelling tests, of Trabue in his standardization of his language-completion tests, and of Woody in his standardization of his arithmetic tests. In this section the final results, merely, will be presented.

Graph I. is the curve indicating the scores to be expected at the various ages from 3 years to 19 years. This curve has been smoothed but slightly within the range of ages below ten, though rather considerably from fifteen to nineteen. This was necessarily the result of a deficiency in the number of cases at the higher ages. The median score at each age is represented by a circlet with a dot enclosed.

Table III

Mental Age Equivalents of Score Values

Score Points	Mental Age	Score Points	Mental Age	Score Points	Mental Age	Score Points	Mental Age
0....	5–3 or below	33......	10– 9	66......	13– 5	99....	15– 9
1....	5– 7	34......	10–10	67......	13– 6	100....	15–10
2....	6– 0	35......	10–11	68......	13– 6	101....	15–11
3....	6– 3	36......	11– 0	69......	13– 7	102....	16– 0
4....	6– 6	37......	11– 1	70......	13– 8	103....	16– 1
5....	6– 9	38......	11– 2	71......	13– 9	104....	16– 2
6....	7– 0	39......	11– 3	72......	13– 9	105....	16– 3
7....	7– 3	40......	11– 4	73......	13–10	106....	16– 4
8....	7– 6	41......	11– 5	74......	13–11	107....	16– 5
9....	7– 8	42......	11– 6	75......	14– 0	108....	16– 7
10....	7–10	43......	11– 7	76......	14– 1	109....	16– 8
11....	8– 0	44......	11– 8	77......	14– 1	110....	16– 9
12....	8– 2	45......	11– 9	78......	14– 2	111....	16–10
13....	8– 4	46......	11–10	79......	14– 3	112....	16–11
14....	8– 5	47......	11–11	80......	14– 4	113....	17– 1
15....	8– 7	48......	12– 0	81......	14– 5	114....	17– 2
16....	8– 9	49......	12– 1	82......	14– 6	115....	17– 4
17....	8–10	50......	12– 2	83......	14– 7	116....	17– 5
18....	9– 0	51......	12– 3	84......	14– 7	117....	17– 6
19....	9– 1	52......	12– 4	85......	14– 8	118....	17– 8
20....	9– 3	53......	12– 5	86......	14– 9	119....	17– 9
21....	9– 4	54......	12– 6	87......	14–10	120....	17–10
22....	9– 6	55......	12– 7	88......	14–11	121....	18– 0
23....	9– 8	56......	12– 8	89......	15– 0	122....	18– 2
24....	9– 9	57......	12– 9	90......	15– 0	123....	18– 3
25....	9–11	58......	12–10	91......	15– 1	124....	18– 5
26....	10– 1	59......	12–10	92......	15– 2	125....	18– 7
27....	10– 2	60......	12–11	93......	15– 3	126....	18– 9
28....	10– 3	61......	13– 0	94......	15– 4	127....	18–11
29....	10– 4	62......	13– 1	95......	15– 5	128....	19– 1
30....	10– 5	63......	13– 2	96......	15– 6	129....	19– 3
31....	10– 7	64......	13– 3	97......	15– 7	130....	19– 7
32....	10– 8	65......	13– 4	98......	15– 8	131....	19–11

In Table III. are presented the mental age equivalents of each score from 1 (mental age 5 years 7 mos.) to 131 (mental age 19 years 11 mos.)

(*E*) Reliability of the Tests

To measure the reliability of any newly devised test of intelligence is not a simple matter. It devolves upon the standardizer to present evidence that the new intelligence scale measures this inadequately defined entity 'intelligence' with approximately the same degree of accuracy as those standards or measuring 'rods' now commonly accepted and in current use.

In this brief article the writer will limit himself to five criteria:

(1) The mental processes employed;
(2) Increase in score from year to year;
(3) Correspondence of median mental ages;
(4) Correlations between mental ages, intelligence quotients and teachers' estimates of intelligence;
(5) Conformance of intelligence-quotient distribution with normal probability.

(1) *Mental Processes Employed*

In devising and standardizing this test the writer did not approach the problem with any bias of 'faculty psychology.' The idea still seems prevalent, though not as much now as in the immediate past, that in order to possess an adequate measuring instrument for intelligence, the device must contain separate tests for each mental 'function': sensation, perception, association, imagination, memory, judgment, reasoning, etc. On the other hand it has been amply demonstrated that the only intelligence scales worth the name draw service freely from all 'functions.' Binet has pointed out that all 'intelligent' operations involve the functioning of three primary activities: first, *attention* to the problem presented; second, a conscious attempt on the part of the subject to consummate an adequate *adaptation* to the situation; and third, the exercise of *auto-criticism* in order to determine how efficiently the specific 'adaptation' has solved the problem. A cursory examination of the demands made upon the mental operations of the person tested with the block-designs will clearly reveal that *attention*, *adaptation* and *auto-criticism* are all involved in the successful accomplishment of each task. That point in the graded series of designs at which a child will begin failing to achieve further success, will be a rough measure of the development of his ability to attend, to adapt and to critically survey his general plan of performance and his ultimate accomplishment. In his discussion of the 'patience test' in the 1908 scale, and these words might as well apply to the block-design tests, Binet states:[1] "It is a game, but at the same time a work of the intelligence. When one analyzes the operation it is found to be composed of the following elements: (1) Consciousness of the end to be attained, that is to say, a figure to be produced; this end must be understood, and kept in mind; (2) the trying of various combinations under the influence of this directing idea, which often unconsciously determines the kind of attempt which should be made; (3) judging the combination formed, comparing it with the model, and deciding if it resembles the other" (p. 198). If 'intelligence' involves the following mental operations; analyzing, combining, comparing, deliberating, completing, discriminating, judging, criticising and deciding, then the block-design tests may, with justice, be said to call upon the functioning of intelligence and to that extent they are a measure of that mental capacity.

(2) *Increase in Score from Year to Year*

As regards the second criterion, reference to Graph I. and to the various tables presented in this article will clearly demonstrate that this requisite is satisfied. The following, however, should be mentioned: At each life age a greater scatter or range in ability is noticeable than is the case with the Binet tests. Whether this phenomenon argues for reliability or not is left for discussion in the later monograph.

(3) *Correspondence of Median Mental Ages*

At each life age do the median mental ages obtained by the block-design tests correspond with the median mental ages obtained by the Binet tests? This question is an important one, and the extent of correspondence or deviation

[1] The Development of Intelligence in Children, Publication No. 11, Vineland, 1916.

should measure very largely the reliability of the newly devised tests.

In the following table this comparison is presented:

TABLE IV

CORRESPONDENCE OF MEDIAN MENTAL AGES

Life Age Yrs.	No. of Cases	Median Binet Age Yrs.-Mos.	Median Block-Design Age Yrs.-Mos.	Difference Between Medians (Mos.)	Average of Two Medians Yrs.-Mos.
6.......	16	6- 1	5- 3	10	5- 8
7.......	27	7- 5	6- 8	9	7- ½
8.......	28	8- 0	8- 2	2	8- 1
9.......	27	9- 0	8-11	1	8-11½
10.......	33	9- 8	10- 3	7	9-11½
11.......	30	10- 6	11- 6	12	11- 0
12.......	29	11-10	12- 5	7	12- 1½
13.......	32	11- 5	12- 6	13	11-11½
14.......	28	13- 9	13- 6	3	13- 7½
15.......	19	13- 3	14- 0	9	13- 7½
16.......	16	15- 6	13-10	20	14- 8
				Average—8.5	

Four important items are worthy of note: In the first place, the average deviation of the median Binet ages from the life ages at each year is 6 months; second, the average deviation of the median block-design ages from the life ages at each year is 8.8 months; third, the average deviation between the two intelligence-test medians is 8½ months, and finally, the arithmetic mean of the two medians for each life age results in a more accurate approximation of what may be the 'true' mental age than either median taken alone. The significance of the last fact will have to be left for more complete discussion in the later monograph. At this point, it may be sufficient to remark that the approximation between the Binet and block-design medians is rather close, especially when we consider that the block-design tests are quite free of the 'language factor.'

(4) *Correlations between Mental Ages, Intelligence Quotients and Teachers' Estimates of Intelligence*

In order to understand and to justly evaluate the relations about to be presented, the Binet results will be mentioned to serve as a standard of comparison.

1. The correlation[1] between Binet age and life age is + .80 (P.E. ± .01) (291 public school cases).

2. The correlation between block-design age and life age is + .66 (P.E. ± .02) (291 public school cases).

3. The correlation between Binet age and block-design age is + .82 (P.E. ± .01) (366 cases). The table is herewith presented:

Table V.

Binet Age and Block Design Age

Block-Design Age \ *Binet Age*	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Total
Below 5	3	7	14	14	3	1		1										43
6				10	10	10	2	3	3	1	1							40
7				2	7	9	7	2	1			1						29
8				4	3	4	10	4	2		2	1						30
9				1	3	14	6	7	1	4	2							38
10					2	6	5	8	5	2	3		2					33
11						6	2	4	9	4		1		1				27
12					1	1	2	6	8	3	4	2	1					28
13							1	3	7	5	3	1	1	1				22
14							2	3	2	7		4		2				20
15								2	3	5	3	4	1					18
16								1	2		1	1	1	2		1	3	12
17									1			2	4	1	3	2	1	14
18											1				2			3
19													1	2			1	4
20														2	2	1		5
Total	3	7	14	31	29	51	37	44	44	31	20	17	11	11	7	4	5	366

(Note: Age 10 means 10–7 to 11–6 etc.)

4. The correlation between Binet age and block-design age is + .81 (P.E. ± .01) (291 public school cases).

5. The correlation between Binet age and block-design age is + .67 (P.E. ± .05) (75 feebleminded cases).

6. The correlation between Binet I. Q. and block-design I. Q. is + .80 (P.E. ± .01) (366 cases). The table is herewith presented:

[1] Pearson's Coefficient (r) used throughout.

TABLE VI

BINET I. Q. AND BLOCK-DESIGN I. Q.

Binet I. Q.

Block-Design I. Q.	20	30	40	50	60	70	80	90	100	110	120	130	140	Total
30		13	7	1	1	1								23
40	1	3	4	6	2	3		1						20
50		1	1	5	7	1	1	2						18
60			2	2	4	8	10	1	2					29
70				1	1	4	6	4	3	2				21
80					1	3	11	12	10	5	2			44
90				1	1	3	9	10	20	2	2			48
100						1	6	20	20	7	1	1		56
110						2	7	10	13	7	2	1		42
120							1	5	8	8	5	1	1	29
130								1	3	4	5	2		15
140								2	3	3	5	1	1	15
150									1	1				2
160										1	1		1	3
170											1			1
Total	1	17	14	16	17	26	51	68	83	40	24	6	3	366

(Note: I. Q. of 50 means 46–55, etc.)

7. The correlation between Binet I. Q. and block-design I. Q. is + .57 (P.E. ± .03) (291 school children).

8. The correlation between Binet I. Q. and block-design I. Q. is + .67 (P.E. ± .05) (75 feebleminded cases).

9. The correlation between teachers' estimates of intelligence and Binet I. Q. is + .47 (P.E. ± .03) (291 school children).

It may be worth remarking that although the correlation between block-design age and Binet age is + .82, teachers' estimates of intelligence correlate only one half as much with the Block-design I. Q.'s as with the Binet I. Q.'s. The reader may recall that one of the original objections to the Binet scale was that it measured school training. Only to a limited extent has this been denied, the explanation having been made that the tests measure intelligence through the medium of knowledge only partly influenced by school training. It has been admitted, true, that practically all children are exposed to these educational influences, but the ultimate difference in achievement is explainable on the basis of differences in endowment. However this may be, the results of the block-design test would perhaps tend to show that there is more to this charge than we have been inclined to admit.

It will, no doubt, be acceded without much question that the block-design tests are less affected by school training than the Binet.

At any rate the total correlational evidence seems to indicate that the block-design tests possess a high degree of reliability.

(5) *Conformity of Intelligence Quotient Distribution with Normal Probability*

A very necessary index in weighing the reliability of any standardized test is to determine the extent to which an actually found distribution conforms to its theoretical distribution.

In the following table are presented the I. Q.-range-distributions for the Binet and the block-design tests. The respective percentage values are compared with what one should theoretically expect.

TABLE VII

INTELLIGENCE QUOTIENT RANGES

	26 to 35	36 to 45	46 to 55	56 to 65	66 to 75	76 to 85	86 to 95	96 to 105	106 to 115	116 to 125	126 to 135	136 to 145	146 to 155	156 to 165	166 to 175
Stanford Binet Obtained	....	....	....	1.7	5.5	16.5	22.7	28.2	13.8	8.3	2.1	1.0	....	....	....
Theoretical Expectation	....	....	....	.16	1.6	8.5	23.42	32.64	23.42	8.5	1.6	.16	....	....	....
Block-Design Obtained	.034	.034	1.4	6.5	6.2	14.4	15.1	18.9	14.4	10.0	5.2	5.2	.07	1.0	.034
Theoretical Expectation (Median at 99)...	.49	1.28	2.78	5.21	8.67	12.05	14.66	15.30	13.84	10.69	7.25	4.11	2.08	.88	.33

The average deviation from theoretical expectation for the Binet I. Q. ranges is 3.3 per cent. per I. Q. group. The average deviation for the block-design tests is only 1.4 per cent. per I. Q. group.

In conclusion, one may state that the evidence presented seems to indicate not only that the tests measure intelligence, but that this is accomplished with a fair degree of accuracy. On the other hand, one should bear in mind Stern's caution:[1] "Psychological tests *must not be overestimated*, as if they were complete and automatically operative measures of mind. At most, they are the psychographic minimum that gives us a first orientation concerning individuals about whom nothing else is known, and they are of service to complement and to render comparable and objectively gradable other observations—psychological, pedagogical, medical—not to replace these."

(*F*) Serviceability

In his 'Stanford Revision of the Binet-Simon Scale' (Warwick and York, 1917) Terman states (p. 150) that "to be widely serviceable a test should demand only the simplest material or apparatus, should require at most but a few minutes of time, and should lend itself well to uniformity of procedure in application and scoring." The writer has attempted to satisfy these demands in standardizing the block-design tests. Those who utilize the tests will find after a little practice that there can be but little variation in the findings of two examiners, and that the only chance for difference is in the recording of the number of moves made.

The special value of the block-design tests lies in the fact that valid results may be obtained independently of the 'language factor.' Neither deafness nor lack of language understanding should be disqualifications in the proper performance of the test. The block-designs may therefore be utilized in the study of racial differences, in determining the mental capacities of the deaf and of those suffering from various other language handicaps.

As regards the borderzone problem, although further investigation of this matter by the writer is now under way, it seems that this test will aid in a better differentiation of the group of cases falling in this category. The writer maintains that feeblemindedness is *not* an arbitrary statistical designation, but is rather a clearly demarked physiological entity

[1] 'The Psychological Methods of Testing Intelligence,' Warwick and York, 1914, p. 12.

quite distinct from normality, statistical-psychologists notwithstanding. Years of experience with this type of defect has fixed the notion in the writer's mind that feeblemindedness is indicative not only of mental mal-functioning, but also of physiological mal-functioning, especially of endocrine character. The results of further research, however, can be the only tests of the truth of one's statements at this time.

The writer regrets the omission of much pertinent material in this brief presentation, but the later monograph will deal with many topics here barely touched upon, if at all.

Addendum

Regarding the Average Mental Age of Adults

Of importance in interpreting the results of this newly devised mental test is the recently raised question regarding the average mental age of adults. In the promised monograph a few pages will be devoted to a psychological and statistical discussion of this important matter. At this point the writer merely wishes to state that the data so far presented does not warrant accepting the suggestion that "The previous notion that the average intelligence of adults is 16 years should be given up."[1] There is a fundamental fallacy underlying the suggested 13 to 14 year criterion, a complete discussion of which must be left for a later time.

[1] E. A. Doll: New Jersey State Prison, Psychologist's Report (1918–1919), p. 72.

RECENT APPARATUS FROM THE PSYCHOLOGICAL LABORATORY OF McLEAN HOSPITAL

F. L. WELLS, Ph.D., AND C. M. KELLEY, M.D.

1. A Galvanometer Control Board

1. The functions of the control board are threefold: to provide through a shunt resistance and a galvanometer (1) a circuit from psychogalvanic electrodes, (2) a current to another circuit such as for psychomotor reaction times, or (3) a known resistance to the current.

2. The units of the board are, with one exception, contained in a small mahogany-stained cabinet, 16″ x 16¼″ x 9¼″, the cover of which is formed by the forward half of the top, hinged to the other half, and the front side attached to the top by right-angle braces. A primary cell rests in a compartment at one rear corner and fixed to the floor are,—an Ayrton shunt; a known resistance, 5,000 ohms; a single-pole, double-throw switch; a single-throw switch; and six Fahnestock clips grouped in an accessible position. A two-pole, double-throw switch has been placed outside the cabinet, but in future constructions should be within it.

3. For identifying the points of contact in the wiring, let the clips be numbered from 1 to 6 in order of position and the binding posts of the shunt from 7 to 10. A wire from the positive pole of the cell leads to clip 6, which is connected with the forward post of the double-throw switch, and also with one side of the 5,000-ohm resistance. A wire from the other side of the resistance is connected to one post of the single-throw switch, the other post being connected with

clip 3, which in turn is connected with post 7 on the shunt. The negative wire from the cell is attached to clip 5, from which a wire leads to post 8 on the shunt. This post is also connected with the rear post of the double-throw switch, the central post being connected with clip 4. The two central posts of the two-pole switch outside the cabinet are wired to clips 3 and 4, respectively. Wires from the rear posts of this switch lead to the psychogalvanic electrodes; and those from the forward posts, to the reaction-time circuit. The galvanometer wires lead from clips 1 and 2, connected with the shunt posts 9 and 10.

4. The circuit from the psychogalvanic electrodes, excluding the cell, is made by closing both double-throw switches rearward, and the reaction-time circuit by closing both switches forward. Closing the single-throw switch throws the current through the 5,000-ohm resistance, independently of the other two circuits. The shunt and galvanometer, however, remain in each of the three circuits. In psychogalvanic work it is ordinarily desired to use body resistance to the cell current. In this case the double-pole double-throw switch is rearward, the single pole double-throw switch forward.

II. A Fall Apparatus for a Galvanometer Chronoscope

5. A homely but satisfactorily efficient model of a fall apparatus for standardizing galvanometric readings has been constructed in the laboratory of readily obtained and inexpensive materials. The suggestion was derived from 'A New Chronoscope and Fall Apparatus,' by Paul E. Klopsteg (*Journal of Experimental Psychology*, 1917, 2, 253–268). The approximate dimensions of the model are 5′ (height) x 1′ x 8″, and its range, up to 5/10 of one second.

6. The base is a block of oak, 12″ x 8″ x 2½″, provided with three levelling bolts. The upright consists of an iron rod 60″ in length, threaded at its lower end; and two boards, each 58″ x 2½″ x 7/8″, grooved to enclose the iron rod snugly when screwed together. The threaded end of the rod, protruding from the assembled upright, is driven into a

hole through the base and bolted, the upright being further secured in the perpendicular by four right-angle braces. Thus, rigidity is obtained and warping prevented. The upright is graduated by machine-ruled millimeter scales, inserted into the front surface and extending from 0, 3″ from the base, to the top. The respective distances of fall for tenths of a second up to .5″ are marked as nearly as possible to the quantities derived by the formula, $s = \frac{1}{2}gt^2$ (g = 980.6 cm.)[1] as follows:

at	122.575	cm. for	.5″
"	78.448	" "	.4″
"	44.127	" "	.3″
"	19.612	" "	.2″
"	4.903	" "	.1″

7. A freely movable wooden frame, 3″ in height, surrounds the upright and is clamped at the desired level by a thumb-screw through the back, which forces a metal plate against the standard. Four Fahnestock clips are secured to the rear surface of the frame. An electromagnet is mounted on the front surface and is connected by wires to two of the clips. Immediately below the core, but not in contact, are two rigid metal strips with slightly curved ends for contact with a 5/8″ soft-iron ball, a wire from each strip passing to a clip at the rear. The magnet and contact points are so adjusted that the lowest portion of the ball is held tangent to a horizontal line from the lower border of the frame, which accordingly serves as an indicator on the scale.

8. A cradle, supporting a horizontal tray 2″ square with vertical borders, is mounted at the foot of the upright. A slight vertical motion, with the tray constantly horizontal, is attained by four uniform, mutually parallel links pivoted, two on either side, on the wooden block supporting the tray

[1] The acceleration value g (980.6 cm.) is for the latitude of Washington, D. C. Variation due to differences in latitude and elevation is shown by the formula $g = 980.6056 - 2.5028 \cos 2\lambda - .000003h$ (Barker, G. F., Physics, 4th. ed., 1892, 105). At the laboratory, latitude (λ) = 42°21′28″, and elevation above mean low tide (h) = 7620.0 cm. g is found to be 980.353282 cm. This introduces an error of .02516 per cent., which is neglected. For the distance and mass involved, error due to air resistance is also negligible.

and on the upright. The tray is held at rest at 0 on the scale by means of a U-shaped lever, pivoted on each side of the upright, the extremities extending forward under the upper links of the cradle, and the bowl of the U in the rear being drawn down by a spring to rest upon a vertical adjustment screw.

9. Any downward movement of the cradle trips a horizontal trigger by means of an adjustable metal plate on the front. The trigger is a rigid rod supported by three bearings on the end of the wooden base of a telegraph key which is secured to the base of the standard. One end of the rod is bent at a right angle to form a short arm extending under the trip plate, and a slightly longer vertical arm with its extremity hooked and beveled is at the other end. When the key lever, without a knob and with the tip beveled to reduce friction, is depressed it is automatically held closed by the trigger actuated by a low tension spring. The trip plate is adjusted to rest on the short arm when the cradle is at 0.

10. A knife-blade switch, a relay, and two Fahnestock clips (by which the apparatus is brought into the galvanometer circuit) are also attached to the base.

11. The wiring provides for three circuits: (1) the electromagnet; (2) the "ball-contact," through the primary of the relay; and (3) part of the galvanometer circuit. In the magnet circuit, the positive wire from a storage battery is connected to the switch and thence to one magnet clip on the magnet carriage, and the negative wire leads from the other magnet clip to the battery. One primary post of the relay is connected with the positive wire from the battery, the other primary post with one "ball-contact" clip, and the remaining clip with the negative magnet clip. In the galvanometer circuit, one clip on the base is wired to one secondary post of the relay, the other post to one side of the telegraph key, and the other side of the key to the second clip. The contact points of the relay are set to close the galvanometer circuit with the armature released.

12. The procedure of operation consists of the following steps: (1) clamping the magnet carriage at the desired level;

(2) with the trigger key open (and the reaction key on the external galvanometer circuit closed), closing the magnet switch; (3) applying the ball to the contact strips (thereby completing the primary relay circuit and breaking the galvanometer circuit at this point); (4) closing the trigger key (making the galvanometer circuit at that point); and (5) opening the magnet switch, which allows the ball to drop and thus closes the galvanometer circuit by releasing the relay armature. Impact of the ball on the tray releases the trigger key, so that the current flows through the galvanometer only during the time of fall.

13. The galvanometer deflection is read by the excursion of the reflected beam of light on a millimeter scale curved with a radius of 2 m., the focal distance of the galvanometer mirror. The suitable range of deflection is obtained by adjustable resistance in the galvanometer circuit. This circuit is made available for experiments by merely opening the reaction key and closing the trigger key.

14. In his original paper (cf. par. 5), Klopsteg recommends not the use of a relay, but of a high resistance in the galvanometer circuit. The present apparatus is equally adaptable to such a technique. The relay was used here as better adapted to other portions of the set-up and has no practical effect on the reliability of the measurements involved.

III. An Attachment for Choice Reaction Experiments

15. It was desired to provide a signal indicating the correctness of a response or any falsity, either by the wrong key alone or by both reaction keys, in case of either correct or incorrect propositions; and further to provide for breaking the (galvanometer) chronoscope circuit on any response. To meet these requirements, a simply constructed switchboard was devised, operated by a single lateral movement of the hand.

16. The board contains a three-pole circular switch, a relay, and a 6-volt signal lamp, two contact screws being near the left border and three near the right. Wires from the storage battery, on one of which an 'operating key' is

placed, lead to the two left-hand screws; and the three right-hand screws are connected respectively with the outer post of the right-hand reaction key, the inner posts of both keys by a common wire, and the outer post of the left-hand key. The subject is instructed to close both keys on the verbal signal 'set' and to release the right-hand key if the proposition exposed is correct or the left-hand key if incorrect, in either case holding down the other key.

17. The fixed element of the switch is composed of six narrow contact strips attached radially at 60°—intervals on a non-conducting board. They are bent and their edges curved upward to permit sliding plates to pass under them to make contact with slight tension. The moving member is a non-conducting disk, 2″ in diameter, to the lower side of which three independent, sector-shaped plates are firmly attached at 120° intervals between centers. The plates project beyond the disk and under the ends of the contact strips, each connecting two strips. A handle, attached to the upper side of the disk, extends horizontally to the front and its range is limited to 60° by stops. For correct propositions, the disk is rotated to the left and for incorrect, to the right.

18. The board is wired as follows: Numbering the contact points in order of position, point 1 is connected with one wire from the battery; point 2 with one primary post of the relay (the secondary of which is in the galvanometer circuit and is broken while the armature is released); point 3 to the second wire from the battery; point 4 with the 'proper' wire from the right-hand reaction key; point 5 with the second primary post of the relay; and point 6 with the 'proper' wire from the left-hand reaction key. The 'common' wire terminal at the board is connected with one side of the signal lamp, the other side of which is wired to point 2 (connected with the relay primary post).

19. The handle is so placed on the disk that in the 'correct' position, one sliding plate connects points 2 and 3; the second plate, points 4 and 5; and the third plate, points 6 and 1. In position for 'incorrect' propositions, the first plate connects points 1 and 2; the second, points 3 and 4; and the third, points 5 and 6.

20. Tracing the circuits established, it is seen that with the reaction keys 'set,' closing the 'operating' key closes the relay and lights the signal lamp; and breaking contact at any of the keys opens the relay circuit. With the switch in the 'correct' position, releasing the right-hand key (the right response to a correct stimulus) permits the lamp to continue lighted—to brighten, in fact, as the relay magnets are cut out. A false reaction, whether by releasing the left-hand or both keys, extinguishes the lamp by breaking the entire circuit. Switching the handle to the right for incorrect propositions reverses the action of the subject's keys so that releasing the left-hand key (the right response) causes the lamp to brighten, but a false reaction or a reaction with both hands extinguishes the light.

IV. A Fall Screen for the Exposure of Choice-reaction Stimuli

21. This piece of apparatus[1] provides for independent, successive exposures of 25 stimuli and, by connections with a chronoscope and attachments or with a multiple-pen chronograph, for recording the time and accuracy of each response.

22. The standard is of wood, its greatest dimensions being approximately 8′ x 22″ x 18″. The pedestal,[2] comprising 16″ of the total height, consists of a top, 12″ x 17″ x 3/8″, supported by four slightly converging legs of 1¾″ stock and braced by cross pieces. The front of the upright is formed by the floor of a vertical trough 64″ long, 10″ wide, 3½″ deep, and is extended upward by a board 14″ long. A horizontal aperture, 2¾″ x ⅞″, with beveled edges and provided with a cover, is cut 52″ from the floor. The sides of the trough are grooved along their inner surfaces to guide a sliding frame. For 42″ of its length, the trough forms the front of a box of the same width and 9½″ deep, containing a shelf 24″ from the base and 1½″ thick. The rear of the upper compart-

[1] Cf. Wells, F. L. and Sturges, H. A., 'Pathology of Choice Reactions,' *Amer. Journ. Insan.*, 1918, 75, 82–85.

[2] In a later model supplied to the Carnegie Institute of Technology, the upright was, for the greater comfort of the subject, hung at the rear of a table, affording room for the knees.

ment is closed by a cover opening upward, and the lower by a door opening laterally. On the shelf is pivoted a metal disk, $6\frac{1}{8}''$ in diameter, from the border of which a radial section $1''$ at the circumference and $\frac{3}{4}''$ deep is cut. A binding post is secured to the disk and a handle is placed diametrically opposite the open section. Thus a hand-operated escapement for the sliding frame is afforded.

23. The sliding frame is a rectangular hollow frame of wood, $30'' \times 5\text{–}\frac{5}{8}'' \times 3''$, with a guide strip on either side fitting into the corresponding groove of the trough, and on the rear is firmly attached a row of 27 hard wooden blocks, $\frac{3}{4}''$ wide, $\frac{7}{8}''$ high, and $\frac{5}{8}''$ thick, arranged alternately on either side of the middle line (as the black squares in two rows of a checker-board). The lower surface of each block is covered with felt and the blocks are placed with these surfaces at 1-inch intervals. Thus alternate movements of the escapement disk permit 1-inch drops of the frame, moved by gravity. Within the shaft of the frame is a $1\frac{1}{2}''$ brass tube, $43''$ long, attached at its base to the pedestal and open at its upper end. A plunger, operating in oil, is connected by an iron rod to a metal arch across the upper end of the sliding frame, whereby the impact of the blocks on the disk is diminished.

24. The stimulus material is printed at 1-inch intervals on interchangeable strips of heavy paper, one of which is slipped down the front of the frame and retained so that one stimulus is exposed at the aperture when the frame is at rest at any station but the first, when blank paper is exposed.

25. A continuous, non-insulated copper wire, arising from a contact clip at the top of the frame, passing over, outside and under each alternate block along shallow grooves, forms contact with the escapement disk except at the instant of change, the break in the circuit being recorded on the chronograph. A flexible wire, on which a weight is suspended by a pulley, is connected between the clip on the frame and another on the shelf. From the latter, a wire leads to one post of a two-pole, single-throw switch on the floor of the box and through the switch, by a 6-conductor Ulesote cable, to the

chronograph. Another wire, attached to the binding post on the disk, passes to the other side of the switch and through the cable to the chronograph circuit.

26. Lateral shelves, 6″ x 10½″, are braced on the sides of the upright 10½″ from the pedestal, a telegraph key being secured to each and concealed, except the operating knob, by a hinged cover, the front of which is open and the border of the top cut away to render the knob accessible without exposing the rest of the key or wiring. The inner posts of both keys are connected by wires to one clip on the pedestal and through the cable to the chronograph circuit. Each outer post, by its individual wire to a corresponding clip and through the cable, is connected with the chronograph electromagnet controlling the pen assigned to the key, so that contact at either key effects its characteristic record.

V. A Multiple-pen Chronograph

27. This is a motor-driven chronograph[1] providing seven simultaneous, independent records, each obtained by a magnetically operated pen striking on moving carbon and paper ribbons against a platen roller. By means of the adjustable speed motor employed and the gearing of the mechanism, any constant rate of motion of the paper between 6″ and 50″ per minute may be obtained. The apparatus is contained in a cabinet, 27″ x 14″ x 13″, the cover of which comprises 10″ of the height and is hinged on the rear wall. Inserted into the top is a glass window, 22½″ x 9¾″.

28. The mechanism is supported by two parallel brass plates, 8″ x 6″, separated by distance rods 2″ long and perforated identically for screws, to provide bearings for the several axles, and by two large openings, to afford accessibility. A rectangular section is cut from one upper corner of each plate to form the seat for a transverse, semicircular frame supporting the magnets, recording points, certain of the rollers, and by a strip of non-conducting composition along the straight border of the upper plate, four binding posts at each end. The frames are mutually attached by small right

[1] This apparatus was built to order by E. S. Lincoln, Inc., of Waltham, Mass.

angle braces, and the two uprights are similarly attached to a base plate, $7\frac{1}{8}''$ x $5''$, screwed to the floor of the cabinet. The stock is $\frac{1}{8}''$ brass plate.

29. A worm-gear box is mounted on the base at the proper level to align the shaft with that of the motor and to enmesh the pinion gear with the main gear wheel in the frame. The ratio of revolutions of the drive shaft and the pinion gear is 40:1. The main gear wheel is $1\frac{7}{8}''$ in diameter and is fixed to the axle of the paper take-up drum by a set screw through a collar. It bears 88 spur teeth and the ratio between the pinion, $7/16''$ in diameter and having 20 teeth, and the wheel is accordingly 4.4:1. A second pinion gear ($11/32''$ in diameter; 15 teeth) is fixed on the same axle and is in mesh with an intermediate wheel, 1 $11/16''$ in diameter with 80 teeth and pivoted on a bar across the upper opening of the supporting plate, the ratio being 5.34:1. On the axle of the carbon ribbon take-up drum, above and to the left of the intermediate wheel, and in mesh with it, is mounted a wheel $3''$ in diameter with 144 teeth, revolving on a collar bound to the axle by a set screw and bearing a 12-tooth ratchet. A pawl mounted on the wheel, and provided with a spring, engages in the ratchet to drive the drum but permits a reverse rotation of the drum in rewinding the ribbon. The ratio between the intermediate wheel and the latter is 1.75:1 and accordingly the rate of rotation of the ribbon drum is approximately only 1/9 that of the paper drum (the ratio between the motion of the carbon and paper ribbons being further increased by the difference in circumferences of the drums).

30. The carbon ribbon, $1\frac{1}{2}''$ wide and 8 yards long, arises from a flanged drum of sufficient capacity, fixed to an axle mounted symmetrically with the corresponding take-up drum and extending through the back plate to present a squared end for rewinding with a clock key. A thin strip of brass, attached to one plate, is bent to afford a slight retaining friction on the drum. The carbon ribbon passes over a roller mounted on the transverse frame, between the recording points (below) and the platen (above), and over a second roller to the take-up drum. The dimensions of the rollers

are the same throughout, 1½″ wide between flanges and ¾″ in diameter, and are brass, except the platen which is of composition.

31. The paper reservoir is a drum 2″ in diameter and 1½″ wide (its flanges having a diameter of 3½″), revolving about a detachable axle held in position by two horizontal, slotted arms extending 2½″ from the supporting plates and provided with straight springs, each perforated near the outer end to receive the axle. A heavy brass plate, pivoted at the top of the frame, rests on the roll to serve as a brake. The paper passes upward over a roller parallel to those described but outside and slightly above, between the carbon ribbon and the platen, over a roller similarly placed at the opposite end of the frame, over a third roller half way down the side of the frame, over its take-up drum, between two guide plates, and leaves the cabinet through an opening in its end. The paper take-up drum, operated by the main geared wheel, is 8 9/32″ in circumference and 1½″ wide, being flanged to guide the ribbon. Eight short, sharp pins are driven radially into the surface of the drum at equal distances, ⅛″ from each flange; and an upper and a lower carriage, each provided with two soft felt rollers and held against the drum by a common spring, press the paper onto the pins.

32. The recording device is contained in the transverse frame. At the center of the straight side is placed a row of seven vertical recording rods, tapered and rounded at their upper ends, and each provided with a collar near the lower end. Each rod is freely movable through holes in the horizontal plates but returns by gravity to rest with its collar on the lower plate. Seven electromagnets are mounted between the plates in an arc; and on the border of the lower plate are pivoted the corresponding, converging armatures extending under their respective magnets and their narrowed ends resting under the corresponding rods on a supporting bar.

33. Each group of binding posts is connected by a cable with binding posts passing through the front wall of the cabinet. From the first post on the frame, a common wire

leads to five magnets, the wires from which lead to their respective posts. The sixth and seventh magnets are in separate circuits, their return wires leading to two additional posts in the cabinet wall. The 6th magnet records the incidence of stimuli. The 7th magnet, in circuit with an electric clock, records the time intervals. The other five magnets are placed on reaction circuits.

34. The motor was constructed by the Holtzer-Cabot Electric Company. It is a direct current, adjustable speed, shunt-wound motor; type HD; size 14; H.P. 1/10; 110-volt; 1.3 amperes; with a speed rating of from 300 to 2,400 r.p.m. The control is placed for the convenience of the experimenter; preferably the recording apparatus itself should be placed in a room other than that of the experiment because of the sounds produced by the action of the recording points.

VI. A Device for Water Recording of Motor Tests

35. The material for the contrivance consists of two wide-necked bottles of 150 c.c. and 500 c.c. capacity, with tightly fitting rubbed stoppers; glass tubing; rubber tubing; a glass funnel; an atomizer bulb; two valves from a second bulb, and a spring clamp.

36. The larger bottle (*A*) is upright and its stopper is perforated by three holes, through one of which the stem of the funnel is inserted. Over the end of the stem is slipped the end of a short piece of rubber tubing containing a valve closing upward. Through the second hole, a glass tube extending to the bottom of the bottle is inserted and its outer end connected with the second bottle by rubber tubing which contains a valve closing downward. A short glass tube is placed in the third hole and its free end connected by rubber tubing with the bulb. A short pin is pressed into the rubber plug of the bulb valve to facilitate releasing the air pressure.

37. The smaller bottle (*B*) is held inverted at a convenient height by a standard, its stopper having four holes for as many glass tubes. The shortest tube (inlet) extends merely through the stopper and to it the tubing from bottle (*A*) is connected. The second glass tube (outlet) extends slightly

higher into the bottle and at its lower end, another tube of small calibre is connected by a short rubber tube provided with a clamp. The last-named glass tube is held vertical by an arm from the standard. The third hole of the stopper contains a glass tube (overflow) of sufficient length to permit adjustment and at its lower end, rubber tubing leads to the funnel into which it empties. The tube through the fourth hole extends the length of the bottle permitting the free passage of air.

38. Bottle (*A*) is nearly filled with water and compressing the bulb, by increasing the air pressure within the bottle, forces water into bottle (*B*), its return being prevented by the valve. The volume desired for the experiment is determined by the level of the overflow tube above the outlet tube, the excess returning to the funnel of bottle (*A*), into which it passes on the release of outward pressure against the valve. The adjustment for volume having been made, the procedure of the experiment consists merely of compressing the bulb, opening the clamp to expel air from the outlet tube, and after reclamping the tube, compressing the bulb until water flows into the funnel. After a moment's pause until the excess is wholly drained off, on the release of the clamp the predetermined volume of water falls from the outlet.

39. A simple application of the device is in tests of motor steadiness or tremor where it is seen how much of the predetermined volume of water, issuing from the small outlet tube, the subject can catch in a vessel with its inlet slightly larger than the outlet tube.

SEX DIFFERENCES IN THE EFFECT OF DISCUSSION

BY HAROLD E. BURTT
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Introduction

The following results were obtained incidentally in connection with a study[1] of the inspiration-expiration breathing ratio as diagnostic of truth and falsehood. In many of the series a 'jury' was present primarily to enhance the emotional state of the subject but a number of interesting problems presented themselves in connection with the judgments made by the jury. The principal one was suggested by Münsterberg's brief experiment[2] in which a group of men discussing an objective situation (number of geometrical figures on cards) profited much more by the discussion than did a corresponding group of women. The writer was a member of the Harvard seminar that furnished the male results and was also familiar with the type of students in the Radcliffe seminar and has felt that the groups were not of the same caliber. The women were for the most part an unselected group of college undergraduates and the men a selected group of graduate students. The present situation afforded an opportunity to check this experiment on a much larger number of people with both sexes of about the same academic status. A minor problem that suggested itself was the general ability to judge a person's veracity by his casually observable behavior (intuition) and especially sex differences in this respect. A further problem related to the ability to judge the veracity of a person of one's own sex as compared with a person of the opposite sex.

Method

The subject was given two papers face down, marked on the backs respectively T and L. If he (or she) wished to

[1] To be published shortly.

[2] Münsterberg, H., 'Psychology and Social Sanity,' 1914, pp. 181–202.

tell a lie on that trial he selected the paper marked L and did not touch the other. The paper recounted in considerable detail an imaginary crime in which the subject was implicated. After four minutes study he was questioned on at least ten crucial points and lied consistently to clear himself. If he selected the paper marked T he was supposed to be charged with the same crime but to be innocent and the paper afforded him the details of a complete alibi. He merely recounted these in response to the same ten or more questions. The L was prepared by the experimenter and the T by an assistant so that during an experiment no one but the subject knew whether he was following the alibi provided or making it up himself. The subject wore a pneumograph and sphygmomanometer. The former recorded the inspiration-expiration ratio before and after the replies to questions and the systolic blood pressure was taken at various points during the examination.

In all the series involved in the present study a number of persons sat in front of the subject. There were usually 'jurors' of both sexes present. The nature of the experiment was explained to them and they were then instructed to "watch him closely and try to decide whether he is using the T or the L paper, *i.e.*, whether he is using the alibi prepared by the assistant or making up his own story."

Immediately after the examination a ballot was taken and the result announced. Then the jury discussed the matter openly for about five minutes. The experimenter did not enter into the discussion except when someone was very palpably judging by illegitimate criteria. The discussion usually centered around the person's behavior, nervousness or calm, position of the eyes, facial expression, rapidity of response and sometimes such things as whether the subject had had sufficient social experience or was clever enough to devise such an alibi. After the discussion a second ballot was taken.

No effort was made to duplicate actual legal conditions. The jury was not weighing evidence to any considerable extent but rather deciding upon the veracity of the witness.

Furthermore the subjects, while not instructed to do so, made every effort to deceive the jury and the experimenter. This 'set' was very pronounced in the experienced subjects and involved simulating guilt while actually telling the truth and vice versa, whereas in real life the witness is practically always simulating a truthful attitude.

Two series of this sort, *i.e.*, two "crimes," were performed during an hour's sitting. The subject was privately instructed to select one T and one L in whichever order he pleased. This was to insure an equal number of series of each sort. The jury did not know this, however, but were told the subject would do whichever he wished. This enabled them to judge each series on its own merits.

The experiments were performed in the psychology laboratory of Ohio State University in the spring of 1920. There were in all 22 series performed, 11 T and 11 L, and the total number of jurors was 244. Of these 88 were women and 156 men. They were present in groups of from 4 to 26. Three subjects—2 women and 1 man—participated in the experiment weekly and had had some experience prior to the series with the jury. Four other subjects—1 woman and 3 men—performed two or more series without previous experience in the experiment.

The Effect of Discussion

There are two ways of evaluating the effect of discussion on accuracy of judgment—by comparing the per cent. of the total number of judgments that are correct before and after discussion or by noting the per cent. of the cases in which the judgment is changed in the right or wrong direction as a result of the discussion. These results are given in Table I.

Table I

Per Cent. Correct Judgments

	Female Judges	Male Judges
Before discussion	48	48
After discussion	52	47

Per Cent. Changes in Judgment

	Female Judges	Male Judges
In right direction	9	5
In wrong direction	5	6
Total	14	11

The table shows that 48 per cent. of the women's initial judgments are correct and 52 per cent. of their final judgments. The corresponding figures for the men's judgments are 48 and 47 per cent. This apparently indicates mere guesswork in both judgments and an absence of any effect of the discussion. The lower part of the table shows however that discussion does play a part but works both ways. The women change an incorrect judgment before discussion into a correct judgment after discussion in 9 per cent. of the cases and change a correct judgment to an incorrect in 5 per cent. The corresponding figures for the men are 5 and 6 per cent. Thus while judgments are *altered* by the discussion they are not on the average *improved.* The sex differences in this respect are negligible. In most cases the jury comprised persons of both sexes. In one jury composed of 6 women the initial and final judgments are the same, and in another composed of 26 men there are two changes in opposite directions thus neutralizing each other in the totals.

Münsterberg (using about 15 judges of each sex) found both initial and final judgments for the women 45 per cent. correct, but for men the initial 51 and the final 78 per cent. correct. The present study with 88 female and 156 male judges fails to find such a result. The experiments seem sufficiently similar to warrant a comparison. In both instances the persons were discussing an objective situation—the comparative number of geometrical figures on two cards or the veracity of a subject as manifested by his directly observable behavior. If anything the present experiment afforded a wider variety of items for discussion and would presumably be more sensitive to the sex variable. It seems doubtful if there is any appreciable sex difference in the ability to profit by discussion.

Accuracy of Judgments of Veracity

A consideration of either the initial or final judgments in Table I. indicates that, in this particular type of situation, estimates of veracity are very unreliable. Further analysis however reveals a rather general tendency. This is shown in Table II.

Table II

Per Cent. Correct Judgments (Both Sexes)

	Subject Telling Truth	Subject Lying
Before discussion	37	58
After discussion	33	62

The initial judgments when the subject is actually telling the truth are 37 per cent. correct and when he is lying 58 per cent. correct. The corresponding figures after discussion are 33 and 62 per cent. There is an obvious tendency to give a correct L judgment more frequently and a correct T judgment less frequently than the chance expectation of 50 per cent. This constant error is increased very slightly by the discussion. A more detailed table shows it to be slightly greater—5 or 6 per cent.—with men than with women. It appears that the situation creates an attitude of suspicion toward the subject and he is thus too frequently judged to be lying.

Ability to Judge Veracity of One's Own Sex

Table III. gives the results bearing upon the ability to judge veracity of persons of one's own sex as compared with persons of the opposite sex.

Table III

Per Cent. Correct Judgments

	Women Judging Women	Women Judging Men	Men Judging Women	Men Judging Men
Before discussion	45	49	52	41
After discussion	52	51	52	38

The successive columns of the table indicate the per cent. of correct judgments made by female jurors with female subjects, female jurors with male subjects etc. The only marked divergence in the table from the chance 50 per cent. correct is in the greater inaccuracy of male jurors in judging men. A more detailed table shows that this is due primarily to the tendency, mentioned above, to give L judgments too frequently. Probably the male subjects in their efforts to appear guilty while telling the truth were more successful in deceiving others of their own sex. It is improbable that this indicates a fundamental sex difference.

Conclusions

In an experiment in which the subject lied or told the truth about an imaginary crime, a group of persons judged his veracity by his observable reactions during examination. After five minutes open discussion a second judgment was made. Approximately 250 judgments of this sort were available.

There was a considerable tendency to change one's decision as a result of the discussion but the change was in the wrong direction about as often as in the right. There was no appreciable sex difference in the ability to profit by discussion.

In the present situation there was a constant tendency to consider too frequently that the subject was lying. There was no clear difference in the ability to judge the veracity of persons of one's own as compared with those of the opposite sex.

PRACTICE EFFECTS IN INTELLIGENCE TESTS[1]

BY KNIGHT DUNLAP AND AGNES SNYDER

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The possible practice effects in intelligence tests are of importance where groups of reactors are to be given relative gradings on tests. If some of the reactors have had experience in the taking of tests; especially tests of the same general sort as that on which the grading is to be done; while others have had less experience, or none at all, with tests of the type used; it is obvious that the ratings will be unfair, unless no considerable practice effects can be demonstrated. The matter is of especial importance in connection with tests for admission to college or school, since the possibility of 'coaching' for these tests must be definitely known, or else the tests are misleading.

For the purpose of a preliminary investigation into practice effects, a college class of 44 men, mostly seniors, was tested four times, with intervals of approximately three weeks between the tests, using four forms of the Army "Alpha" composite test. The Alpha test was selected not because of any assumed superiority of that test over others, but because a number of nearly equivalent forms of that test were available, whereas there were at most only two forms of any other composite test available, with the exception of one standardized test for which the price asked was nearly a dollar each, rendering it prohibitive for experimental work.

The 'practice' in this investigation consisted solely in the taking of the tests: no attempt was made to give further practice between the tests, although this might have been done by using various single tests corresponding in type to the parts of the Alpha composite. No attempt has been made so far to find what effect practice on tests of one type

[1] A paper presented before the Southern Society for Philosophy and Psychology, New Orleans, April 23, 1920.

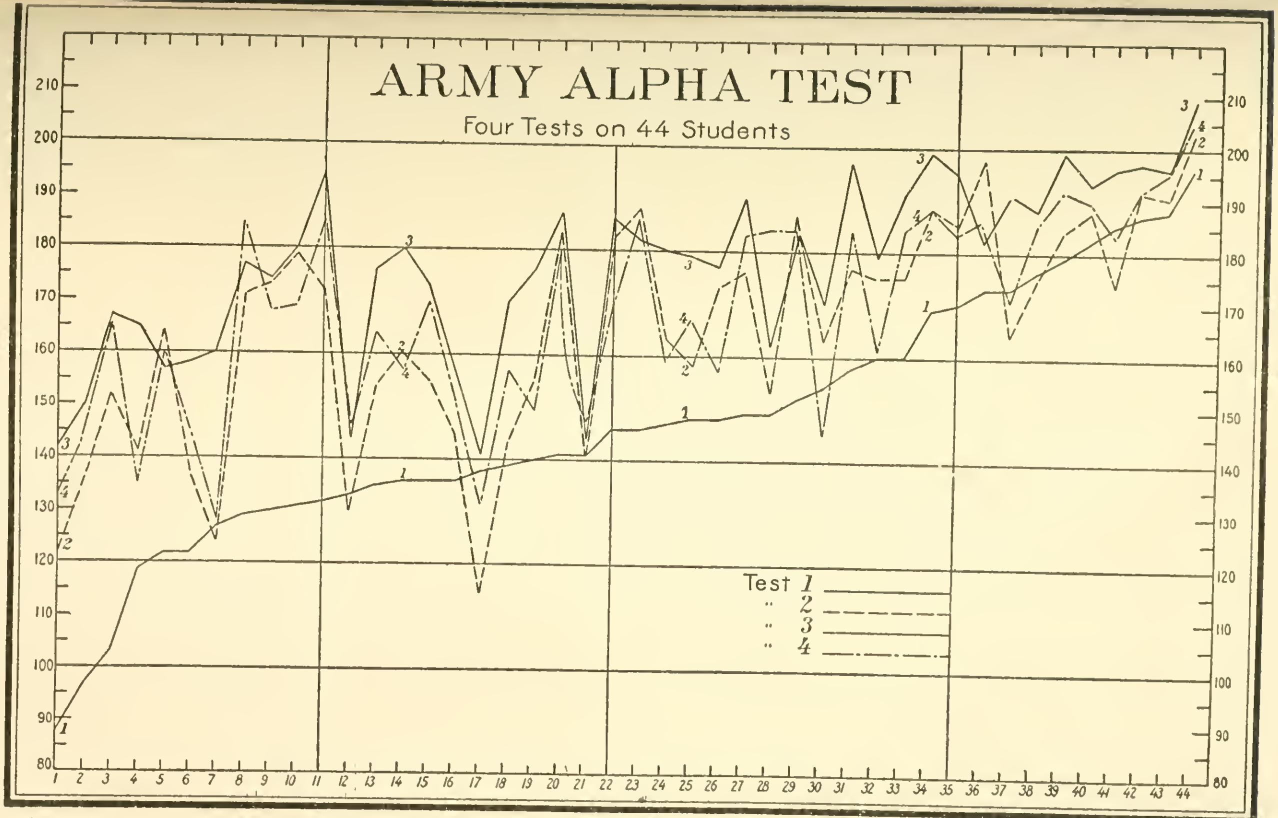

Diagram I. The Abscissæ numbers, from 1 to 44, represent the men, arranged in order of grades attained on the first test, the Graph for which is marked I. The Ordinate numbers, at the left, indicate the scale of markings in absolute scores on the test, not percentages. Graphs 2, 3 and 4 represent the grades received by the men on the second, third and fourth tests respectively.

exercises on tests of a different type. The purpose has been merely to obtain data under conditions which might most easily occur; namely, where persons expecting to submit to a certain sort of group-test might obtain previous forms of that test and study them or work through them for practice.

The results of the four tests are presented in Diagram 1. For this diagram, the members of the class were arranged in the order of their grades on the first test, from low (left) to high (right). The grades for the second, third and fourth tests were then plotted for the same arrangement. The reason for the specific arrangement is that thereby one fairly simple graph is obtained, making comparison with the graphs of the succeeding tests easy.

It will be noticed that there is a general improvement on the second test, and again on the third test, but a general falling back on the fourth. While five men on the second test fell below their marks on the first test, and one other made exactly the same mark on both tests; and five men on the third test fell below their marks on the second; every one made better marks on the third test than on the first. That is: not only was there a striking general improvement from the first to the second, and from the second to the third, but *every man* showed improvement on the third test, over the first.

The improvement on the second and third tests, and the slip-back on the fourth, is shown in another way in Diagram 2, where the ordinates represent the number of men making improvements, or losses, of magnitudes represented by the abscissae.

The main reason for the deterioration on the fourth test was made clear by the reports of the reactors. There was a practically unanimous report that the taking of the first, second and third tests was interesting; but that the fourth was a bore. The fact of lessened interest I inferred also from details of the behavior of the class during the test, before I obtained any reports. Of course, the fourth test should have shown at least as high an average grading as did the third if anything had depended on the test beyond the mere personal ambition to make a good showing, which had been largely satisfied by the preceding tests.

It is probable that the four forms used were not exactly equivalent for this particular group of men. The most likely part in which differences would be found might be assumed to be the part on General Information; and the details of the average gains and losses on the different parts of the test (Diagram 3) indicate that the General Information

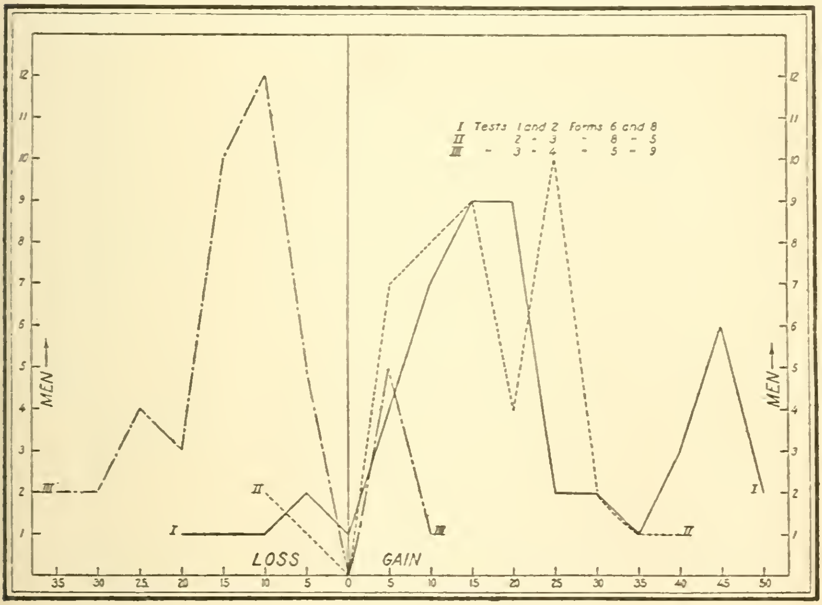

DIAGRAM 2. The abscissæ here represent the number of points of gain or loss between successive tests, the ordinates representing the number of men making a gain or loss not greater that the abscissa at the point of erection of the ordinate, but greater than the next smaller abscissal value marked. Graph I., for example, indicates that two men lost not more than five points on the second test (as compared with the first); one man lost more than five but not over ten; one lost not over fifteen, and one not over twenty.

part of the fourth test was indeed more difficult than the corresponding parts of the other forms. The probability of consequential differences in the other parts of the test is less definitely determinable; but since the excessive loss on the general information part of the fourth form accounts for only a part of the general loss, the apathy or lessened interest of the class may be tentatively assumed as the most plausible major cause for the general decline on the fourth test.

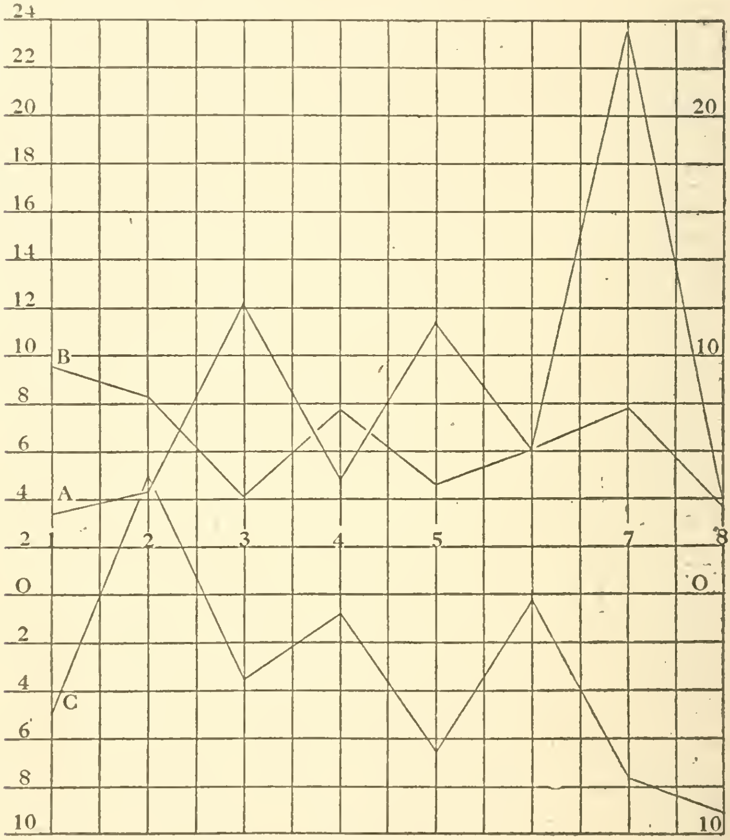

Diagram 3. This represents the gains and losses, in percentages, on the eight parts of the composite test, the percentages being represented by the ordinates, and scaled at the left. The eight points marked on the horizontal axis represent the eight parts of the test. Graph *A* shows the average percentage of gains of the class on the second test as compared with the first: graph *B* represents the average percentage of gains on the third test as compared with the second; and graph *C* represents the gain and losses on the fourth test as compared with the third.

A detailed study of the gains and losses on the several parts of the test has given perplexing results. The close similarity of the corresponding parts of the four forms; especially the second part (problems in arithmetic), in which the same wordings are used in some of the corresponding items of the several forms; and in the fourth part (number series), in which some identical number-series occur; may account for the significantly higher gain in these parts as compared with other parts of the tests. The problems of transfer of training are sharply presented here. The important question is whether repetition-forms of tests can be constructed in such a way as to test the same 'capacities,' and offer the same degrees of difficulty, without permitting a large amount of transfer. It is obvious that a considerable amount of experimental work must be done before tests can be used with assurance as to the absence of important practice effects.

Subject to the foregoing considerations, the following interesting points may be indicated:

In the first place, the relative rating on the third test is somewhat different from that on the first. If the tests had been used to select the best fourth of the group there would have been but little difference in the individuals selected, since 9 of the 11 who were in the first (high) quartile in the first test remain in the first quartile in the third, while the two who go back move back but to the next quartile. The tendency of movement backward or forward to adjacent quartiles is marked in the three lower quartiles. While this has no direct bearing on the present problem, it is a matter of passing interest to note the comparative uniformity in the results of the first and third tests in the first (high) quartile and the contrasting fluctuation of results in the fourth (low) quartile. Were the test given with a view to eliminating the lowest quartile the first and third tests would thus yield quite different results.

In the second place: if the men in the lower half of the class (as indicated by the first test) had had practice in the test equivalent to that actually obtained by the taking of the test twice, and the men who are in the upper half had not had practice, a single test on the class would have given a relative grading which would have differed markedly from either the first or the third of the actual ratings, and which (in all probability) would have been maximally unfair.

The practical conclusions to be drawn from such data are extremely important. The question as to which would be the fairest method of grading a group (of applicants for admission to college, let us suppose): grading them on a test with no preceding practice, or on a test preceded by uniform practice (the same amount for each individual), is of great theoretical interest, but of none at all practically, since groups will not be obtainable in either condition, if the system of testing becomes established and individuals know for months in advance that they are to be tested.

Obviously, if tests susceptible to practice effects are used, the only system which will be at all fair involves the condition that all candidates shall have old forms of the tests in advance, and shall have an opportunity to practice on them; or that in other ways all candidates shall receive an amount of practice which will put them in an equivalent "practiced" condition. The exact amount of practice required for that purpose must be experimentally determined, in order that the necessary minimum may be assured to all the candidates. Since coaching on such intelligence tests is distinctly possible and since the maximal unfairness is obtained where some are coached and others are not, pains must be taken to have all coached effectively.

Whether it is possible to develop types of test which are not susceptible to practice effects, and which yet test the same 'capacities,' and offer equal difficulties, is a question which cannot be answered definitely without prolonged research on that specific problem. The best conjectures we may make on this point are not encouraging. However, if the plan indicated above is followed systematically, we may be able to equalize practice effects and thus render them harmless.

TABLE I

CHANGES IN QUARTILES FROM TEST 1 TO TEST 3

	Fourth Quartile	Third Quartile	Second Quartile	First Quartile
To fourth	6	4	1	0
To third	3	4	4	0
To second	1	3	5	2
To first	1	0	1	9

Table II

AVERAGE PERCENTAGE OF IMPROVEMENT ON THIRD TEST OF MEN IN QUARTILES OF FIRST TEST

Fourth Quartile	Third Quartile	Second Quartile	First Quartile
54	11	40	18
53	41	33	12
64	44	29	11
46	37	31	25
36	21	41	11
35	3	13	8
33	31	31	10
46	36	16	19
44	3	39	19
62	36	19	13
General Average: 47.4	28.1	29.4	16

Table III

AVERAGE PERCENTAGE OF IMPROVEMENT ON THIRD TEST OF MEN IN QUARTILES OF THIRD TEST

Fourth Quartile	Third Quartile	Second Quartile	First Quartile
3	64	49	11
54	31	44	62
3	9	33	25
11	37	36	11
53	44	31	8
35	41	40	10
41	36	46	39
36	46	13	25
33	29	41	19
13	31	31	30
46	19	18	13
General Average: 28	35.2	34.7	23

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PSYCHOLOGICAL TESTS FOR SELECTING AVIATORS

BY

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Part I. Introduction

The research here reported[1] aimed to add to the means already available of eliminating very early those candidates for training in aviation who would later be unable to learn to fly, and of selecting those who possessed unusual aptitude for flying. The research did not aim to assist in the classification of those who were already in the service and who had in practice made clear that they were competent flyers.

An understanding of the conditions under which the experiment was undertaken will be assisted by a brief statement of:

[1] Responsibility for this investigation and its report is as follows: For the general direction of the research, for part of the apparatus and experimental methods, and for writing Part I., Major Stratton; for part of the apparatus and methods and for the administration of the experiments at the aviation fields, for writing the description of methods, a summary of results, and the method of determining the aviation rating, Captain McComas and Lieutenant Bagby; for the entire statistical treatment, for writing the section on the judgment of curves, the detailed results, and suggestion on method, and for assistance in revision, Captain Coover.

Acknowledgment should be made to Captain H. M. Johnson for assistance in preparing the report; to Captain M. Bentley for the form of the Maze Experiment; and to the Commanding Officers, of Taylor Field, Montgomery, Ala., of Souther Field, Americus, Ga. (where the experiments were conducted), and of the Medical Research Laboratory at Mineola, for their hearty coöperation.

1. The means earlier used for the selection of men for training in aviation.
2. An enumeration of the psychophysical tests out of which the present study grew.
3. A table of the psychophysical qualities assumed to be important for aviation, upon which the present research was based.

1. The means earlier used included (*a*) the standard physical and medical examination administered by the Aviation Examining Boards and known as 'Examination 609.' This covered acuity of vision, color vision, the balance of the ocular muscles, the normality of the semicircular canals, normal acuity of hearing, and a large number of other matters. (*b*) There was the professional and mental examination administered by the Aviation Examining Boards, based upon the candidate's written answers to a series of questions covering his parentage, education, business experience, athletic attainments, responsibilities placed upon him by others, military training,—his answers being accompanied by letters from at least three persons who could speak with particular knowledge of his moral character and of his fitness to enter the Air Service. The candidate was also required, as a part of this professional and mental examination, to furnish credentials of his education and to appear for oral examination by the Board.

2. As a result of an earlier study,[1] certain tests had been found to be of aid in detecting ability to fly. The tests which proved of value were those on the perception of gradual tilt; on the power to stand steadily, as judged by the record which a man makes when a writing point attached to his head moves over a smoked surface; on his power quickly to discriminate between a sudden jerk of his body to the right or to the left, particularly when this is combined with his reaction time to a visual signal and to an auditory signal; and on the steadiness of his hand when a pistol shot is fired behind his back.

[1] See Henmon, V. A. C.: 'Air Service Tests of Aptitude for Flying,' *J. of Appl. Psychol.*, 1919, 3, 103. Thorndike, E. L.: 'Scientific Personnel Work in the Army,' *Science*, 1919, 49, 53. Stratton, G. M.: 'Psycho-physical Tests of Aviators,' *The Scientific Monthly*, 1919, 8, p. 421.

The tests which did not scientifically justify themselves (at least with the particular apparatus and methods then employed) were those upon a person's power to learn certain complicated combinations of movement of hand and foot, on the power to continue in imagination a fragment of a curve presented to him in model, and on dexterity. This latter test was disapproved not so much because it arrived at nothing which could be connected statistically with flying ability, as that it could so largely be influenced by practice, and practice would be invited if the test were introduced as a regular part of a Board's examination, when the candidates would soon know beforehand that they would be tested on this feat of dexterity.

The earlier research therefore had its measure of success. Tests were actually found by which it was possible to give a more assured judgment than was possible by credentials and personal interview alone, that the candidate could learn to use with skill the sound and normal body which the medical examination gave assurance that he possessed. These earlier experiments also brought home to one the exceeding complexity of the flyer's ability. His power to attain success in his hazardous and delicate work evidently is not due to any single factor in his psychophysical constitution, nor to any small group of factors, but rather to a happy constellation of many factors, of which each counts only in a limited way.

Upon the basis largely of this earlier research certain tests were approved by the Director of Military Aeronautics, and were concerned with the following: Simple reaction time to visual signal, simple reaction time to auditory signal, discriminatory reaction time to sudden tilt, steadiness of standing, emotional stability under surprise, sensitivity to very gradual tilting of the body, and mental alertness. There was also required with these special tests a detailed report of the amount and direction of the candidate's athletic performance, supported, if need be, by the testimony of others.

3. In continuing the research already mentioned, no attempt was made to attack the particular problems connected with the various specialties of aviation, such as bombing,

pursuit, and artillery control. All of these presuppose ability to fly, and the present research was confined to this ability. In planning the experiments the following analysis served as a guide in the selection of the tests to be studied. With the several qualities here assumed to be of importance, are stated the special—and, one must acknowledge, very insufficient—means for determining whether they were present or absent in a given individual. The new tests studied in the present investigation (indicated by their appearing in italics) will thus be seen in their connection with the tests already officially approved and adopted, as well as with the other means of examination.

Qualities Entering into the Ability to Fly	*Means of Their Detection*
1. Suitable Volitional and Emotional Organization.	Standard mental and professional Examination: "Examination 609."
Strength, constancy, and proper direction of purpose; readiness to meet danger; coolness in emergency; zest for aviation in particular.	Psychiatric examination. *Test of muscular exertion and endurance.* Test of emotional stability; *Test of the rapidity of complex reactions.*
2. General Intelligence and Training. Quickness, discipline of judgment information.	Standard mental and professional examination; Progress in ground school; Special tests of mental alertness.
3. Special Intellectual Equipment.	*Test of judgment of curves.*
Sufficient span and steadiness of attention; power to discriminate and to remember the parts of the airplane; spatial systematization.	*Test of learning and recall of pathways.* *Test of judgment of relative speed.*
4. Sensory and Perceptive Equipment.	
Normal vision with respect to:	
acuity	
'depth' perception	
binocular	'609.'
monocular	
sensitivity to motion in the visual field	
twilight vision	
color vision.	'609.'
Normal hearing with respect to:	
perception, recognition, and discrimination of	
intensities of sound	'609.'
qualities of sound	
localization of sound	
perception of motion by the internal ear.	'609.'

Normal tactile and 'organic' sensitivity.	'609.' Test of discriminatory reaction time to sudden tilt; Test of sensitivity to very gradual tilting of the body; Test of steadiness of standing.
5. Psycho-motor Equipment. Strength, endurance, quickness; skill in learning precise coördination.	'609.' *Test of muscular exertion and endurance.* Test of simple auditory and visual reaction-time. *Test of rapidity of complex reactions.*

Part II. Methods and Results

1. *Judgment of Curves*

The ability of the aviator to estimate the course that his airplane will take as it descends is of great importance, for bad judgment in landing is responsible for many crashes. This ability undoubtedly depends in part upon acuity of vision, balance of the ocular muscles, judgment of spatial relations, all of which are engaged in the work of this test, namely: that of estimating the distance of the point at which a parabolic curve,[1] if continued beyond a given segment, intersects a horizontal plane. The segment of the curve is presented on the upper edge of a metal plate $\frac{1}{8}$ inch in thickness, which is set vertically in the apparatus, so that the OX-axis (at which $y = 0$) lies in the plane of the upper surface of the base of the apparatus. A millimeter scale upon this surface enables the experimenter to read the position of the pointer with which the aviator indicates his estimate. The indicator is a pointer clamped to an endless cord, held taut between pulleys, and is freely manipulated by a knob.

The apparatus is $56\frac{1}{4}$ inches (142.9 cm.) long; the plates are 15 inches long and 15 inches or less in height. The range of excursion of the indicator is from 0, at the end of the plate nearer the observer, to 39 inches (99 cm.) within $2\frac{1}{4}$ inches of the farther end of the base. When the plate is in position, the OY-axis (at which $x = 0$) lies 14 inches back of the origin of the millimeter scale and within $1\frac{1}{3}$ inches of the back end of the base. The headpiece permits a view with the eyes about $23\frac{1}{4}$ inches above the base of the apparatus, or $8\frac{1}{4}$ inches above the highest plates, and approximately in the plane of the OY-axis.

[1] It is of course not to be understood that an airplane in landing follows such a curve.

The general formula for the parabolic curves is $(y - a)^2 = 4p(x - b)$. The following table gives the constants for the equations from which the six curves were derived:

TABLE I

CONSTANTS IN EQUATIONS FOR PARABOLIC CURVES

Curves	1	2	3	4	5	6
a	18.06	20	7.82	12.94	14.94	30
b	5	0	0	0	−3	0
p	2.087	4	0.46	1.92	1.704	11.25
i	44.0	25.0	33.25	21.75	29.76	20.0
i_1	111.8	63.5	84.5	55.2	75.5	50.8
i_2	76.3	28.0	49.0	19.7	40.0	15.3

The values in the table above the line are given in inches; in the lower part, in centimeters. Since the estimates of the point of intersection (i) were read from a millimeter scale originating at the front of the plate which lies 14 inches, or 35.5 cm., from the origin of the curve (where $x = 0$), the distances from the origin of the curves to the points of intersection (i_1) had to be converted into values comparable to the estimates; these are given in the last line of the table (i_2).

The administration of the test provided for one sitting on each of two different days. At each sitting the aviator was given the plates first in 1–2–3 order and then in an amended reverse order; consequently two estimates were made on each plate in a sitting, and they were not consecutive. In making the estimate the aviator, with his eyes approximately in the plane of the OY-axis, glanced down, with binocular vision, upon the curved edge of the plate, and out along the base, and manipulated the indicator until it marked the point at which he judged that the curve if continued would meet the plane of the base. The eye-shield, which resembled that of the common stereoscope, confined the shifting of the point of regard within narrow limits, and the screen into which the eye-shield was set prevented any other view of the curves as related to the base. Consequently the criteria on the judgment were limited to the binocular view of the segment of the parabolic curve on the plate, and of the base, from this point of regard.

Results.—The estimates on each of the parabolic curves were tabulated in frequency-distributions, and two facts at once appeared:

1. There was a general tendency to overestimate the distance; and

2. Dispersion in all of the distributions is large.

The mean amount of overestimation, if we neglect Curve 1, ranges from 25.2 per cent. to 87.5 per cent. of the distance estimated.

The amount of dispersion may be shown by (*a*) the range of estimates, and (*b*) the standard deviation (σ) in Table II.

TABLE II

DISPERSION

(*Values in Centimeters*)

Curve	i	Range		σ		PE of Est.	
		Taylor	Souther	Taylor	Souther	Taylor	Souther
1.......	76.3 cm.	60	80	12.5	15.5	8.4	10.5
2.......	28.0	65	60	11.4	10.9	7.7	7.4
3.......	49.0	80	90	15.9	17.7	10.7	12.0
4.......	19.7	65	55	10.8	9.3	7.3	6.3
5.......	40.0	70	70	16.2	13.7	10.9	9.3
6.......	15.3	45		8.4		5.7	

For the sake of comparison the distance estimated (i) is also shown. The range of estimates, with but one exception, is greater than the distance estimated; and on Curve 4 is three times as great. The standard deviation (σ) is from 1/5 to 1/2 of the distance estimated. Consequently the probable error of an estimate (*PE* of est.) is large: for Curve 1, for example, the least distinguishable distance (a value equal to twice the *PE* of est.) is about 18 cm., or a quarter of the distance estimated; and for Curve 4 it is about 14 cm., or a third of the distance estimated.

The general overestimation indicates the presence of a normal illusion of spatial judgment and suggests the necessity for scoring the observers in terms of *deviation from the mean estimate*, rather than from the point of intersection. And the large probable error of the individual estimates suggests that the correlation of any values based upon them, and aviation rating, would be low.

Although the dispersion of estimates on a curve is large, the four estimates of a single observer commonly vary within a narrower range. For Souther Field, for example, there is a high correlation between the first and second estimates on Curve 1 ($r = .710 \pm .028$) and a fair correlation between the estimates of the two days ($r = .662 \pm .030$). *Consistency of judgment* was rated by MV/M, in which MV is the observer's mean variation and M his own mean.

The coefficients of correlation between aviation rating and the deviation from the mean estimate are shown for the six curves separately in lines 1 to 6 of Table III.: between aviation rating and deviation from the mean divided by the standard deviation (x/σ) for all curves, in line 7; between aviation rating and the mean variation divided by the Mean ($M\ V/M$), in line 8; between aviation rating and deviation from the distance to the point of intersection ($E - i$), in line 9.

TABLE III

CORRELATIONS OF JUDGMENTS OF PARABOLIC CURVES, WITH AVIATION RATING

Line.	Curve	Taylor Field	Souther Field
1......	1	$.119 \pm .047$	$.135 \pm .079$
2......	2	$.142 \pm .046$	$-.025 \pm .080$
3......	3	$.120 \pm .047$	$.192 \pm .077$
4......	4	$.149 \pm .046$	$.204 \pm .080$
5......	5	$-.056 \pm .047$	$.195 \pm .080$
6......	6	$.140 \pm .047$	
7......	Dev. from Mean Estimate (x/σ)	$.109 \pm .038$	$.253 \pm .035$
8......	Consistency of Judgment (MV/M)	$.139 \pm .038$	$-.013 \pm .036$
9......	Dev. from Point of Inter. ($E - i$)	$.037 \pm .020$	$.052 \pm .035$

The results with the two sets of observers are in fair agreement: for the single curves, they agree in rank-order of the coefficients for Curves 4, 3, and 1, while the significance of their disparity for Curves 2 and 5 is largely removed by the appearance of two negative but unreliable coefficients among the set of only moderately reliable, and low, coefficients. Both show a reliable but low correlation between aviation rating and the deviations from the mean estimate when the values from all curves are aggregated (line 7), and show the absence of a reliable correlation with the deviations from the distance to the point of intersection (line 9). A reliable but low correlation between aviation rating and 'consistency of judgment' is found for the observers at Taylor Field, but none for the observers at Souther Field.

On the whole, those aviators whose estimates more closely approximate the 'normal' estimate, and those who are more consistent in their estimates, stand a slightly better chance of a favorable rating in aviation than do their fellows. Since the cadets are a selected, fairly homogeneous class, the low but reliable coefficients of correlation presented above may be taken to indicate that the special abilities tested contribute in some measure to aviational ability. That the correlations with aviation rating are not higher, may depend upon certain disabilities, applicable to the other tests as well, which will be noted in the closing paragraphs of Part III.

2. *Judgment of Relative Speeds*

The aviator is frequently required to estimate the speed of one ship with reference to the speed of another. He must often estimate the speed of his own ship relative to stationary objects also. It is important to learn whether visual judgments of this nature are as greatly influenced by the personal equation as are other types of visual judgment. If large differences in this particular exist, it is important to discover their relation to flying ability in general.

The question was tested by placing before the cadet two moving objects and determining his estimates of their relative speeds.

The apparatus employed may be briefly described as follows.

Two white spots, emerging from behind a screen, travelled along two straight converging lines in a horizontal plane, upon which the observer, standing, looked down. These white spots in due time disappeared behind a second screen which concealed also the common point (*d*) toward which they converged.

Each cadet was given two sittings, each of which consisted of two series, of 10 presentations each. At each presentation the subject was required to judge whether the white spot on the right would pass *d* before, after, or simultaneously with the white spot on his left. The moving spots were visible during their passage through a distance of 55 cm. between the two screens.

In Series *A*, the spot on the subject's left moved at a rate of 3.58 cm. per second, and the one on the right at the rate of 4.46 cm. per second. In Series *B*, the spot on the right travelled 5.34 cm. per second while the one on the left traveled at 3.58 cm. per second. The spots were arranged to pass the point *d* in varying relations to each other: the right-hand spot was set to pass ahead of, or after, or to 'collide' with, the left.

Results.—Errors were made, most frequently when the spots were set to pass a short distance apart, or to 'collide.' There are few errors in the $\pm$ 5 cm. settings, but there are many in the $\pm$ 2.5, the $\pm$ 1.6, and the 0 ('collide') settings.

A comparison of the errors for the different settings of the different days and series shows a typical and persistent error. If the errors are arranged on the basis of overestimation and underestimation of the speed of the right-hand spot with reference to that of the left, we get the following table for the two fields.

Table IV

Number of Errors in Judgment of Speed of Right-hand Spot

Taylor Field

	Series A		Series B	
	Day 1	Day 2	Day 1	Day 2
Overestimated	21	16	14	9
Underestimated	69	42	99	103

Souther Field

	Series A		Series B	
	Day 1	Day 2	Day 1	Day 2
Overestimated	32	42	30	14
Underestimated	69	107	53	143

It appears that there is a strong tendency to underestimate the speed of the right-hand spot, which always moves faster than the left. Possibly this may be correctly described as an overestimation of the speed of the slower with respect to the faster. This tendency increases when the right spot increases its speed, as in Series *B*. The error is persistent, and occurs more frequently in the last sitting of the test than in the first.

It would be interesting to follow this type of illusion in experiments of a different character, and to determine how consistent it is under varying conditions. If the tendency is general, as the uniformity of the results here indicate, the fact is important, as the effect is directly contrary to other effects of 'contrast,' and it should certainly be taken into account in aviational training.

The presence of this illusion complicated the scoring and treatment of the results. If the errors caused by it are frequent, relatively to the errors caused by bad judgment, they, if given the same weight, would tend to obscure the judgmental differences which should be the exclusive basis for scoring.

The likelihood of making any particular error, with each setting of the apparatus, was determined by a tabulation of all of the errors, and each cadet was penalized for an error by a score numerically equal to the number of his fellows who did not make it. Again the graver of two errors, possible in some settings of the apparatus, was more heavily weighted: thus the test-score was freed from the influence of the illusion; and, on the basis of frequency and degree of error, it fairly placed the cadet with respect to normal judgment of speeds.

When the scores are compared with the flying ratings it appears that the tests at both fields yield a positive and moderately reliable correlation; at Taylor Field, $r = .232 \pm .096$; at Souther Field, $r = .224 \pm .085$. These figures to some degree indicate that the better flyer possesses an ability to estimate the relative speed of two moving objects with greater accuracy than does the poorer flyer.

3. *Complex Reaction Time*

The following test was designed at the suggestion of several practical flyers. These men did not agree in their opinions concerning all of the traits the good aviator should possess, but they did agree that he must be quick and accurate in his control of his machine. It seemed advisable, therefore, to test the cadets in reference to the speed and correctness of the movements which they make in obedience to certain signals.

This was done by the determination of a form of 'discrimination time' which was registered by movements somewhat similar to those that the aviator makes in controlling his plane.

The apparatus consisted of a seat, stick, and rudder-bar, taken from a dismantled airplane and assembled in the same relations that obtained in the airplane. The stick and rudder were equipped with electrical contacts which were wired in parallel with each other and in series, with a dry-cell battery, and the magnet actuating the marker of a Foote-Pierson Writing Register. The subject sat before a screen resting on a table. Behind the screen was a shutter, which was thrown open by the operator so as to expose a card through the opening in the screen. The cards were 14 cm. x 11 cm. On each was a letter and an arrow. The direction of the arrow indicated the foot to be thrust forward. The letter indicated the direction the stick should be moved, thus *R*, to the right, *L*, to the left, *B*, backward and *F*, forward.

The shutter was equipped with an electric contact which closed the circuit through the stick and rudder the instant the shutter was opened. This circuit was broken when both the stick and the rudder moved. The current, therefore, ran through the marker from the time of the opening of the shutter until both the stick and rudder were moved. The marker was actuated by a magnet and traced an ink line, while another marker, controlled by a Jaquet clock, registered time in units of 0.2 seconds. To determine the time between the appearance of the signal and the reactions of the cadet, the length of the continuous line was compared with the number of time-divisions registered beside it. The length of the time-marks was great enough to permit of reading the times in units of 0.1 seconds.

At Taylor Field each cadet was given a number of trials to accustom him to the apparatus before the tests began.

When he had learned the procedure, the time was taken for each reaction, and the correctness of his movements on stick and rudder noted. He was required to make 20 reactions at each sitting on two successive days.

The time of each reaction was measured and an average time of reaction for the two series was taken for each cadet; also the number of incorrect reactions. A comparison of these reaction-times and errors shows that the quickest man made his reaction in 0.64 seconds, and the slowest in 1.64 seconds; that the most accurate made no errors on either day, and the least accurate made sixteen errors the first day and thirteen the second, out of the twenty tests for the day. For the correct reactions, in series which contained not more than five per cent. of wrong reactions, the average time was 1.1 second.

The first and most natural comparison is that between the average time of correct reaction, and the percentage of errors in reacting. This comparison shows that the men who make the greatest number of errors have the shortest reaction time ($r = -.415 \pm .06$ for Taylor Field, and $r = -.337 \pm .07$ for Souther Field). The relation between aviation rating and the average time of the correct reactions ($r = .157 \pm .07$) is partially masked by the influence of the errors. By imposing a penalty of 2.3 per cent. in time for 1 per cent. of error in reaction (coefficient of regression of time on error in reaction = .023) we find the relation between the revised scores and aviation rating to be somewhat higher ($r = .170 \pm .067$) and by excluding the disturbing influence of the errors by a more adequate method of 'partial correlation' we find a reliable correlation ($r = .256 \pm .064$).

At Souther Field a different procedure was followed in preparing the cadets for the tests. They were given a preparatory series of but three trials before the actual testing began. In many cases, therefore, the men were not fully accustomed to the test before the times of reaction were taken. Their results show no correlation with aviation rating.

4. *Tests of Learning and Recall of Complicated Pathways*

Individuals differ widely in the ability to remember a route once traversed, when required to retrace their course. As this trait is often serviceable in cross-country flying, a series of measurements was attempted, by the use of a maze to detect variations in the ability and to determine its correlation with aviation rating.

The apparatus consisted of a maze traced on a cardboard 16 x 22 cm., the paths being 1.5 cm. wide. Over this cardboard rested a covering board with a circular opening 2 cm. in diameter. For preliminary practice the cadet was presented with a smaller and far simpler maze, the entrance to which was indicated, and he was instructed to follow the winding paths from the entrançe to the goal, and return. The complete course was to be covered without error in the shortest possible time. The larger maze was then presented to the cadet, who took the covering board, looked through the opening at the entrance and sought to grope his way to the goal by moving the opening over the paths. If he chose a blind alley he returned to the entrance of the alley. He was forbidden to cross a line.

The time was taken in units of 0.2 seconds for the route in to the goal, and again for the return. The test was repeated once on the same day and twice on the following day. The times for ingress and egress for the several trials were compared. In general the memory of the route found in the first tests shortened the times of the later tests. No reliable correlation was found between this and flying ability. Among the possible explanations an obvious one is that the cadets were not ranked for cross-country flying; and, even if the test were of real value in detecting a retentive memory for routes covered, it was not compared with flyers ranked on the basis of such an ability.

5. *Tests of Muscular Exertion and Endurance*

In many situations the aviator is called upon to exercise considerable physical strength and endurance. It was not practicable to test the cadets for strength of all groups of muscles. As the strength of grip is frequently taken by directors of physical culture as a general index of strength, it was deemed advisable to use the grip of hands in a series of tests.

The apparatus used was the Smedley hand dynamometer from which the dial and hands had been removed, and a rotating drum with a pencil marker substituted. The strength of grip was indicated by the pencilled tracing on a ribbon of paper wound upon the drum. The height of the line indicated the strength of the grip. If the cadet held his grip while the drum revolved, the marker traced a temporal record of the relative degree of maintenance of grip.

The maximal momentary grip was taken separately for the right and left hands. The cadet was then directed to grasp the instrument with his right hand, as tightly as he could, and to maintain the grip for three minutes. The test was repeated the next day with the same procedure.

A comparison of these records shows a wide difference in physical strength. At Taylor Field the strongest right-hand grip was 83 kilograms; the weakest 44 kilograms; the strongest left-hand grip was 74 kilograms; and the weakest 37. Several of the men showed equal strength of grip with the two hands.

The curves for the sustained grip were measured by taking the height of the curve at the beginning of the test and after the lapse of each 22.5 seconds thereafter. This gives five measurements within the first $1\frac{1}{2}$ minutes, after which the curves are too erratic for useful comparison.

These five measures show a consistency in decline for men of different initial strengths, the stronger men losing relatively more strength than the weaker men.

There is no significant correlation between the maximum grip of the right-hand and aviation grade ($r = -.034 \pm .09$ for Taylor Field and $r = -.084 \pm .09$ for Souther Field). Nor is there any correlation between aviation ability and the summated strength grips of the two hands; or the difference between right and left, or the endurance shown in sustained grips ($r = -.161 \pm .09$ Taylor Field, and $r = .148 \pm .098$ Souther Field).

It would appear that differences in strength (of the forearm, at least) do not differentiate aviators whose flying abilities were as similar as those of the 48 who took this test at Taylor Field and the 55 at Souther Field.

Part III. Correlation between Test-scores and Aviation Rating

1. *Aviation Rating*

At Taylor Field the instructor of each cadet was requested to give separate estimates on each of the following features of flying:

(*a*)	Dual Flights	(each	day)
(*b*)	" "	("	week)
(*c*)	Solo "	("	day)
(*d*)	" "	("	week)

The following grades were used for the purpose: Fair, Good, Very Good, and Excellent.

From the estimates secured from the instructors, the cadets were divided into eight groups for the purposes of correlation. These are designated by the letters *A* to *H*, *A* being the group of greatest efficiency. In the following table the number of cadets falling into each group is indicated.

Group	No. of Cadets
A	1
B	5
C	7
D	14
E	10
F	11
G	1
H	1

At Souther Field the Gosport system of instruction was in use, and the officer in charge of flying kept rating-cards containing a numerical rating of each cadet on each day's performance in the various features of flying.

For the purpose of correlation with the psychological tests, the various numerical ratings of each cadet were averaged. The range was found to be between 75.00 and 62.80, and the men were placed in fourteen groups as follows (the number of men in each group is indicated):

Group	No. of Cadets
75.00–75.99	1
74.00–74.99	0
73.00–73.99	3
72.00–72.99	9
71.00–71.99	14
70.00–70.99	18
69.00–69.99	11
68.00–68.99	7
67.00–67.99	5
66.00–66.99	2
65.00–65.99	1
64.00–64.99	0
63.00–63.99	0
62.00–62.99	1

2. *Correlations*

The test-scores were correlated with aviation rating by the use of the standard Pearson-Bravais 'product-sum' formula. The coefficients are shown in the following table:

TABLE V

CORRELATIONS BETWEEN TESTS AND AVIATION RATING

	Taylor	Souther
Parabolic Curves: Consistency of Judgment (MV/M)	.139* ± .038	−.013 ± .036
Parabolic Curves: Deviation from Mean Estimate (x/σ)	.109* ± .038	.253* ± .035
Moving Objects: Penalty Score	.232* ± .096	.224* ± .085
Complex Reaction: Time	.170* ± .067	.039 ± .081
Complex Reaction: Errors	.181* ± .066	−.091 ± .078
Complex Reaction: Time Corrected for Error	.256* ± .064	—— ——
The Maze: Time 3d In and Out	.105 ± .09	.124 ± .079
The Maze: Time 4th In and Out	−.008 ± .09	.020 ± .080
The Maze: Immediate Practice-Effect	−.015 ± .09	.006 ± .081
Dynamometer: Endurance of Pull	−.161 ± .09	.148 ± .098
Dynamometer: Strength of Pull	−.034 ± .09	−.084 ± .09

The signs of the coefficients uniformly agree with + for merit in the test: *i.e.*, a positive correlation with "Errors," in Complex reaction, means that merit, with respect to accuracy, has a positive correlation with aviation rating.

All of the coefficients are low, but some of them (indicated by an asterisk) are over or near three times their probable error and constitute evidence for correlation. The coefficients falling below this statistical requirement do not indicate the absence of correlation, since in the nature of the case coefficients of genuine but small correlations cannot meet the statistical requirement. The correlation between some of the tests and aviation rating is about as close as some published correlations between Breadth and Length of Skull in Bavarian peasants ($r = .28 \pm .06$), in French peasants ($r = .13 \pm .09$), or in Aino women ($r = .18 \pm .08$) (Davenport: 'Statistical Methods').

The correlation between the total results of the tests and aviation rating was found by the Grade and Rank methods, in which Pearson's standard '*R*' and '*Rho*' formulæ were used:

TABLE

CORRELATION BETWEEN TOTAL TEST RESULTS AND AVIATION RATING

	Taylor Field	Souther Field
Grade Method..........	$R = .224$; $r = .375 \pm .095$	$R = .124$; $r = .216 \pm .087$
Rank Method...........	$\rho = .355$; $r = .370 \pm .087$	$\rho = .270$; $r = .282 \pm .077$

The probable errors indicate that the coefficients of correlation (r) are fairly reliable. The relationship between the test-results as a whole, and aviation rating, is about as close as that between stature and length of forearm (.37), or between the statures of father and daughter (.360) or mother and daughter (.284).

With respect to the value of these psychological tests for the detection of aviational ability, our results are as clear as the limitations of the research would permit. The tests, as a whole, and some of them singly, are to some extent diagnostic. Their precise value, however, can be known only after trial under more favorable conditions. The special disabilities of this research may be stated as follows:

1. The test-scores are not good measures of the cadet's ability in the test-performances; they include a large amount of adventitious variability associated with changes in method incident to the early period of learning, and with practice-effect of varying rates; and they are based on a small number of trials wholly limited to this unfavorable period of learning.

2. The number of cases was too small. A correlation table of 50 or 70 is incapable of showing a reliable correlation if the coefficient be small. We should have had at least 200 cases from each field. (Test-results were obtained from many flyers for whom no aviation rating was available.)

3. The aviation ratings are not good measures of aviation ability; they are weighted with military, personal, social, and other qualifications. Also, the factors considered and the method of expressing rating differed sufficiently at the two fields to preclude either the aggregation, or the close comparison, of the results from them.

4. No ratings of the more specific aviational abilities were available for evaluating the respective tests as diagnostic of those special abilities.

5. The range of aviational ability was too narrow to furnish a good criterion of diagnostic tests.

These disabilities were unavoidable under the conditions existing at the time the research was carried out, and no criticism is implied. They should, however, serve as guides in further research designed to find the best psychological tests diagnostic of aviational ability.

Conclusion

In view of the disabilities under which this research has been conducted, the positive results are more important than the quantitative statements indicate. The following tests, in the order written, show promise as a means of detecting aviational ability:

1. Judgment of relative speeds of moving objects,
2. Judgment of parabolic curves,
3. Complex reaction time.

They deserve further trial under conditions suggested by the disabilities listed above, in order that their precise individual value may be determined. Their full diagnostic value could then be learned by finding the correlation between the rating in aviation and an aggregate score in which each test is weighted in proportion to the individual coefficients of correlation between the respective tests and aviation rating.

THE RELATIVE VALUE OF GROUPED AND INTERSPERSED RECITATIONS

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The following experiment and discussion is largely an outgrowth of the experiments performed by a number of workers along the line of recitation work in learning. In particular it is an outgrowth of the work done by Witasek[1] and later A. I. Gates.[2] The following work assumes the soundness in general of the conclusions reached by Gates that recitation (attempted recall) is more valuable in learning than mere reading. During the summer of 1919 the writer did a little work, in the foot-steps of Witasek, that showed not only the superiority of the recitation method but indicated that there was an *optimum* combination of readings and recitations. However the work along this line was discontinued.

The purpose of the following work is to throw some light if possible upon this question; granting the superiority of *L*'s plus *R*'s (throughout we will let *L* stand for reading and *R* for recitation) over mere *L*'s, is it better to intersperse *L* and *R* or use some degree of grouping? Put in more practical form we might state the question thus: Is it better for the student to read through his English-German vocabulary, poem or dates or what not, and then try to recall, or should he read two or more times and then try to recall two or more times? In other words shall he intersperse or group? Is there any law indicated? It is upon this question that we have hoped to throw some light—at least we have tried to find indications of some principle or law.

[1] Witasek, S., 'Ueber Lesen und Rezitieren in ihren Beziehungen zum Gedächtnis,' *Zsch. f. Psychol.*, 1906, 44, 161–185, 246–282.

[2] Gates, A. I., 'Recitation as a Factor in Memorizing,' *Arch. of Psychol.*, No. 40, Sept., 1917.

Notwithstanding the good work that has been done by Gates, Witasek and others, the writer wishes to point out a fact that is deserving of some criticism. A recitation (*R*) means not a recitation in a pure sense as used by these investigators, but really means *R* plus *L* (*L* standing for the promptings which, in whatever form given, are after all simply readings). So they have not compared mere *L* with mere *R*, but with a mixed method which complicates the total process a great deal.

Suppose that *S* (subject) takes 6 *L*'s and 6 *R*'s (in the sense used by these workers). In *R* he gives as much as he can and is prompted on the rest. So it goes through the other readings. To take some data from Witasek to illustrate. Keeping the number of original *L*'s constant, that is six in both cases he gives the *S* 5 additional *L*'s and 0 *R*'s. This makes a total of 11. In the next experiment he gives 0 additional *L*'s and then has *S* give 5 *R*'s, again a total of 11. But the two 11's are not strictly comparable—in the first case he has 11 readings. In the second 6 readings plus 5 recitations involving reading in themselves.

It may be replied that while the above procedure complicates the factors involved, nevertheless it lies nearer to actual conditions of learning. The student looks away from his book, reciting all he can, and when stopped, takes a look (a prompt) and so goes on. From this standpoint the use of the above mentioned mixture is probably justifiable. Certainly it complicates matters already sufficiently complicated if we desire to analyze out essential processes. We wish merely to indicate that these workers were not comparing a method of learning without recitation with a method of learning with recitation in any strict sense.

We have tried to eliminate this mixture in Part I. and II. of our experiment, by keeping an *L* an *L* and an *R* an *R* in a strict sense. No promptings of any kind have been given. When it is stated that there were 6 *R*'s grouped, *e.g.*, we mean *S* gave all he could, then again tried to give all he could. And so on for six times. In discussing the imprinting and recall factors we believe that our method permits of more clean-cut comparisons than would have been the case if we had used a more mixed method. However, in Part III. of our experiment we have used 'R' in the mixed sense.

Material.—Part I. of the investigation uses nonsense syllables for the learning material. A series of twelve such syllables constituted a learning series. Part II. and III. use sense material in the form of poetry, eight-line stanzas from Byron's 'Don Juan' furnishing the material.

Part I. Nonsense Material

($L + R$ *Used in Strict Sense of Term*)

Apparatus.—A modified Wirth memory apparatus was employed. A large screen of black cardboard completely hid the experimenter from the subject. A slit was made near the center of the screen so that the window of the machine coincided with it. Thus E could operate the machine, take notes, etc., without S knowing what was happening. Slips of white typewriting paper with the syllables typewritten thereon in capital letters were used. A metronome, in a felt rubber padded box across the room and hidden from the subject, beat the time. The ticking of this instrument was barely audible and was very seldom heard by S.

Method.—Twelve syllables (constituting a series) were shown in succession through the window in the Wirth machine by means of the rotating drum. The tempo selected was one syllable per $1\frac{1}{4}$ second. This time was selected after some exploring for a suitable tempo. We wanted a time that would not "rush" S or be uncomfortable, and on the other hand avoid exposure time long enough to put a premium on anticipation, repetition, and attention relaxations. Finally the above time was selected as suiting our purposes fairly well. As there were always two blank spaces on the rotating drum, in every learning there was a period of two and one half seconds during which nothing was shown and before the second or next reading could begin.

The following instructions were emphasized to each subject:

1st. In learning (reading) never anticipate, *i.e.*, reach out for, try to get before it comes into view, the next syllable. Simply take the syllable given, hold it as attentively as possible, and then take the next one as it appears.

2d. In reading, say the syllable but once. Never repeat to make it stick.

3d. Try to keep the syllables as meaningless as possible. Do not search for some meaning, do not put meaning into the syllable.

4th. Report all distractions and any conditions that would make the results non-comparable with other records.

5th. In recitation, do your best. Do not give up if few syllables come. Keep on trying.

Four methods were used in the course of the experiment:

I. The Interspersed Method (6*L* and 6*R*).

II. *b*. The "Slightly Grouped" Method (2*L*–2*R*–2*L*–2*R*–2*L*–2*R*).

II. *a*. The "Intermediate Method" (3*L*–3*R*–3*L*–3*R*).

II. The Extreme Grouped Method (6*L*–6*R*).

In every method used there were, as a total, 6*L*–6*R*, making in every case twelve. At the end of each experiment there was given an immediate reproduction test. One hour later the subject again recalled all possible. Twenty-four hours later *S* again tested himself or herself to see what could be recalled.

As there are twelve syllables in each series, we worked out averages on this basis. Thus if *S* reproduces, say, 11.5 syllables as his average immediate reproduction, this is with reference to the perfect score, namely 12. In this case, if one hour later the average amount recalled was 7 syllables, *e.g.*, then the per cent. of retention would be 65.86. There is no part credit given—the material is right or wrong! Each syllable is one unit of the perfect score 12.

The data following were obtained from nineteen university students in the elementary psychology course and from five trained subjects—subjects[1] who had been given previous

[1] We wish to express thanks to the trained graduate persons who, as subjects, did their work conscientiously and whose introspections were essential—Miss E. Gordon, Mrs. E. B. Skaggs, Miss M. Gurnsey, and Miss S. Mebai.

training in learning nonsense syllables and four of whom were graduate students in psychology. (The writer is included in this list.) It is chiefly upon the work and introspections of these five trained subjects that we wish to base any finer distinctions, although each is used as a check on the other group.

Table I

Showing Individual Averages and General Averages for Group I. Using the Interspersed Method (Method I, 6L–6R Interspersed)

Subject	Amt. Immed. Reprod.	Amt. Hour Recall.	Amt. 24 Hr. Recall	Per Cent. Retain. 1 Hour	Per Cent. Retain. 24 Hours	Errors Immed.	Hour	Day
1. E.B.C.	11.5	7.0	6.75	65.86	64.51	.75	1.5	1.5
2. M.D.	11.25	10.75	10.75	93.75	93.33	.25	.75	.5
3. D.D.	9.5	7.75	7.30	81.59	74.05	1.75	2.25	1.00
4. R.R.	11.25	11.25	10.00	97.72	87.12	.75	.50	1.50
5. C.W.	10.5	10.50	10.50	100.00	100.00	.50	.75	.75
6. G.F.P.	8.75	5.75	4.50	63.65	54.98	3.00	1.50	1.00
7. C.W.C.	8.00	8.00	6.50	89.10	83.54	1.60	2.25	2.25
8. R.T.D.	12.00	12.00	11.00	100.00	96.16	0.00	0.00	0.00
Totals	82.75	73.00	67.30	691.67	651.69	8.60	9.50	8.50
Average	10.34	9.13	8.41	86.50	81.46	1.08	1.18	1.06
M.V.[1] =	1.21	2.00	2.15					

Table II

Showing Individual Averages and General Averages for Group I. Using the Extreme Grouped Method (Method II, 6L–6R Grouped)

Subject	Amt. Immed. Reprod.	Amt. Hour Recall	Amt. 24 Hr. Recall	Per Cent. Retain. 1 Hour	Per Cent. Retain. 24 Hours	Errors Immed.	Hour	Day
1. E.B.C.	7.00	6.00	5.50	83.93	77.86	1.00	.75	.75
2. M.D.	7.50	7.50	7.50	97.50	96.25	.25	.75	.75
3. D.D.	8.00	6.50	5.50	85.62	77.13	.75	1.50	.25
4. R.R.	9.00	8.50	8.50	94.44	94.10	.75	1.75	1.75
5. C.W.	8.25	8.00	7.25	96.43	87.74	.25	.25	.00
6. G.F.P.	7.75	5.75	4.50	72.29	55.83	2.00	2.00	1.25
7. C.W.C.	7.80	7.33	4.50	89.77	66.66	.60	2.00	3.25
8. R.F.D.	7.00	6.25	5.66	84.92	73.81	1.75	1.75	1.00
Totals	62.30	55.83	48.91	704.90	629.38	7.35	10.75	9.0
Average	7.79	6.95	6.10	88.10	78.67	.93	1.33	1.13
M.V. =	0.48	0.85	1.22					

[1] In all cases our M.V. is taken from the mean.

Table III

Showing Individual and General Averages for Group II. Using the Interspersed Method

Subject	Amt. Immed. Reprod.	Amt. Hour Recall.	Amt. 24 Hr. Recall.	Per Cent. Retain. 1 Hour.	Per Cent. Retain. 24 Hours	Errors Immed.	Hour	Day
9. H.S.	11.37	11.66	11.37	100.00	100.00	.30	.30	.70
10. R.H.M.	9.70	8.70	8.70	89.70	89.70	.00	.70	.70
11. A.S.	11.50	10.50	8.50	90.83	73.74	.50	.25	.25
12. E.G.	11.30	11.00	10.30	97.00	94.67	.00	.30	.70
13. D.S.	11.00	9.70	7.50	87.52	70.90	.70	.30	.30
17. E.S.	8.00	6.70	7.00	82.60	86.80	1.00	1.30	1.30
18. M.S.	9.70	9.00	9.70	93.88	100.00	1.00	1.00	1.00
19. G.B.	10.70	9.00	8.70	85.90	75.90	1.00	1.00	1.00
20. J.L.	11.00	10.30	9.30	91.30	82.30	.70	1.30	1.30
21. F.F.	11.00	8.00	8.00	72.27	71.91	.30	1.70	1.00
22. C.M.	10.00	9.7	9.30	96.30	92.70	1.30	1.70	1.70
Total	115.27	104.26	98.37	987.30	938.62	6.80	9.90	10.0
Average	10.57	9.5	8.95	89.75	85.33	.6	.9	.9
M.V. =	0.79	1.09	.96					

Table IV

Showing Individual and General Averages for Group II. Using Method II. *a* (Intermediate—3*L*-3*R*-3*L*-3*R*)

Subject	Amt. Immed. Reprod.	Amt. Hour Recall.	Amt. 24 Hr. Recall	Per Cent. Retain. 1 Hour	Per Cent. Retain 24 Hours	Errors Immed.	Hour	Day
9. H.S.	12.00	12.00	12.00	100.00	100.00	.00	.00	.00
10. R.H.M.	11.00	10.00	10.00	87.00	87.00	.00	1.00	1.00
12. E.G.	11.00	10.30	11.00	94.00	100.00	1.00	1.30	1.00
13. D.S.	10.30	9.00	7.70	86.52	73.73	.30	1.30	1.30
17. E.S.	8.00	8.50	7.30	100.00	92.00	1.00	.70	1.00
18. M.S.	9.70	8.00	7.30	82.87	72.22	.30	.00	.30
19. G.B.	11.30	9.70	9.70	84.80	85.00	.30	1.70	1.30
20. J.L.	11.70	11.70	10.70	100.00	91.00	.30	.30	.30
21. F.F.	8.00	9.00	7.50	100.00	87.85	.00	.00	.00
22. C.M.	8.00	7.50	7.70	90.00	96.66	1.30	1.30	1.70
Totals	101.00	95.70	90.90	925.20	885.46	4.50	7.60	7.9
Average	10.10	9.57	9.09	92.52	88.55	.45	.76	.79
M.V. =	1.34	1.17	1.59					

Table V

Showing Individual and General Averages for Group II. Using the Extreme Grouped Method (6*L*–6*R*)

Subject	Amt. Immed. Reprod.	Amt. Hour Recall.	Amt. 24 Hr. Recall.	Per Cent. Retain. 1 Hour	Per Cent. Retain. 24 Hours	Errors Immed.	Hour	Day
9. H.S.	9.33	9.66	9.70	100.00	100.00	.30	.30	.30
10. R.H.M.	7.30	8.00	7.50	100.00	93.00	1.30	1.00	1.00
11. A.S.	11.00	10.00	6.25	90.86	74.69	1.00	.75	1.00
12. E.G.	8.60	9.00	——	100.00	——	.70	1.00	——
13. D.S.	10.00	8.50	7.30	88.88	74.50	1.00	1.50	.70
17. E.S.	8.00	7.70	7.30	91.70	88.40	.70	.70	.70
18. M.S.	10.00	8.00	7.00	76.97	67.27	.30	1.00	1.00
19. G.B.	9.70	9.00	8.30	88.30	85.60	.70	.70	1.30
20. J.L.	10.70	11.30	10.70	100.00	94.00	.30	.30	.70
21. F.F.	7.70	7.00	7.00	92.60	92.60	.70	1.00	1.00
Totals	93.00	88.00	71.00	929.31	770.06	7.00	7.75	7.70
Averages	9.30	8.80	7.78	92.93	85.56	.70	.77	.85
M.V. =	1.05	.98	1.07					

Table VI

Comparing Averages for Group II., in Methods I., II., and II. *a*

Method.	Amt. Immed. Reprod.	Amt. Hour Recall.	Amt. 24 Hr. Recall.	Per Cent. Retention 1 Hour	Per Cent. Retention 24 Hours
I. 6*L*–6*R* Intersp.	10.57	9.50	8.95	89.75	85.33
II. *a* 3*L*3*R*3*L*3*R* Intermed.	10.10	9.57	9.09	92.50	88.55
II. 6*L*–6*R* Grouped	9.30	8.80	7.78	92.93	85.56

Table VII

Comparing Results (Untrained Subjects, Group I.) Method I. and II., According to Individual Distribution

Retention Period	Number Better in Method I.	Number Better in Method II.	Number Indifferent	Total
Immediate	8	0	0	8
Hour	7	0	1	8
24 hours	7	0	1	8

Table VIII

Comparing Results (Untrained Subjects, Group II.) Method I. and II., According to Individual Distribution

Retention Period	Number Better in Method I.	Number Better in Method II.	Number Indifferent	Total
Immediate	8	1	1	10
Hour	7	2	1	10
24 hours	7	2	0	9

Table IX

Comparing Results (Untrained Subjects, Group II.) Method I. and II. *a*, According to Individual Distribution

Retention Period	Number Better in Method I.	Number Better in Method II. *a*	Number Indifferent	Total
Immediate	4	4	2	10
Hour	4	6	0	10
24 hours	3	7	0	10

Table X

Comparing Results (Untrained Subjects, Group II.) Method II. *a* and II., According to Individual Distribution

Retention Period	Number Better in Method II. *a*	Number Better in Method II.	Number Indifferent	Total
Immediate	7	1	1	9
Hour	8	0	1	9
24 hours	6	0	2	8

Table XI

Giving Averages for Four Methods, I., II. *b*, II. *a*, and II. for Trained Subject, E.B.S.

Method	Amt. Immed. Reprod.	Amt. Hour Recall	Amt. 24 Hour Recall	Per Cent. Retain. 1 Hour	Per Cent. Retain. 24 Hours	Errors Immed.	Hr.	Day
I.	11.70	11.30	9.50	95.92	81.56	.20	.30	2.20
II. *b*	10.30	9.30	7.80	90.00	75.60	.00	.70	1.20
II. *a*	9.80	9.30	7.70	95.15	78.27	.50	.50	2.50
II.	6.20	6.20	5.20	100.00	84.75	.70	.70	.80

Table XII

Giving Averages for Two Methods, I., Interspersed and II., Grouped for Trained Subject, S.M.

Method	Amt. Immed. Reprod.	Amt. Hour Recall	Amt. 24 Hour Recall	Per Cent. Retain. 1 Hour	Per Cent. Retain. 24 Hours	Errors Immed.	Hr.	Day
I. (6*L*–6*R*) Intersp.	11.70	11.30	9.00	97.10	79.00	.30	.30	1.00
II. (6*L*–6*R*) Grouped	9.50	9.50	7.10	98.60	77.60	.50	.70	1.00

Table XIII

Giving Averages for Four Methods, I., II. *b*, II. *a*, and II. for Trained Subject, M.G.

Method	Amt. Immed. Reprod.	Amt. Hour Recall	Amt. 24 Hour Recall	Per Cent. Retain. 1 Hour	Per Cent. Retain. 24 Hours	Errors Immed.	Hr.	Day
I.	11.3	10.3	7.7	91.13	66.89	1.0	1.3	2.3
II. *b*	11.3	10.0	10.0	87.20	87.20	.3	1.3	1.3
II. *a*	10.3	9.3	8.3	84.54	78.18	1.7	.3	.7
II.	9.0	10.0(?)	7.5	100.00?	75.24	.3	.3	1.7

Table XIV

Giving Averages for Four Methods, I., II. *b*, II. *a*, and II. for Trained Subject, E.I.G.

Method	Amt. Immed. Reprod.	Amt. Hour Recall	Amt. 24 Hour Recall	Per Cent. Retain. 1 Hour	Per Cent. Retain. 24 Hours	Errors Immed.	Hr.	Day
I.	12	11.7	11.3	97.22	85.83	0	0	0
II. *b*	11	11.3	11.0	100.00	95.80	0	0	0
II. *a*	10	9.3	7.5	92.60	72.20	.7	1.3	2.5
II.	8	7.7	6.3	92.19	77.08	1.3	1.7	1.5

Table XV

Giving Averages for Four Methods I., II. *b*, II. *a*, and II. for Trained Subject, I.D.S.

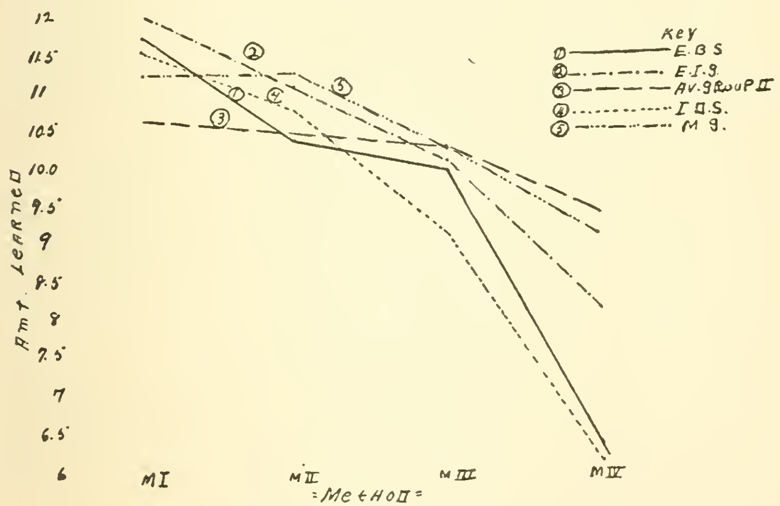
Method	Amt. Immed. Reproduced	Amt. Hour Recall	Amt. 24 Hour Recall	Per Cent. Retain 1 Hour	Per Cent. Retain. 24 Hours
I.	11.50	11.50	10.30	100.00	75.00
II. *b*	10.75	10.25	9.70	93.15	87.27
II. *a*	9.00	8.50	8.00	93.93	90.00
II.	6.00	5.50	4.75	90.84	78.33

Results from Untrained Subjects

Considering first the interspersed and extreme grouped methods, we find our results clean cut. The grouped method is very inferior in comparison. Tables I. and II. (giving the results from group I., untrained subjects) show a marked difference in favor of the interspersed method in the amount learned, the hour and the twenty-four hour retention. Group II. likewise show the same difference though it is slightly less marked (see Tables III., V., and VI., also Graph I.).

In Tables VII. and VIII. we have the same facts shown in a somewhat different way. Here we have taken the individuals who did better by one or the other method and so recorded the fact, regardless of the amount of superiority or inferiority. In group I. no one does better by the grouped method in the amount learned (Table VII.). Of the ten people in group II., only one did better by the grouped method, one being indifferent.

When we turn to the percentage of retention in the two methods, we do not find any significant differences. Though one method may give but few learned, yet is it not possible



Graph I. Curves showing relative amounts learned by various methods. (Four trained subjects and average of Group II., untrained.) Nonsense syllables.

that what little is learned will stick longer? To test this we took as standard the amount learned and taking the amount retained one hour and twenty-four hours later, worked out the percentage. Our question seems to be answered in the negative, there being no regularity in the results. As to errors in the two methods, we again find our results very irregular.

Group II. used, besides the two above mentioned methods, an intermediate method, $3L$, $3R$, $3L$, $3R$. Tables VI., IX., and X. give the comparative results. As we might expect, this method stands intermediate in amount learned. The curve in Graph I. shows this very well. However, in the 24-hour retention the method actually is better than the interspersed. Table IX. indicates that it seems not to matter whether the interspersed or mildly grouped method is used. Four do better in the interspersed method, four better in the intermediate method, and two are indifferent.

Table X., comparing the mildly grouped with the more extreme grouped method shows a marked superiority of the mildly grouped method.

The above data indicate that as one goes from the interspersed method to more and more grouped method, the efficiency of the learning decreases and that a point is quickly reached where the grouped method shows a marked inferiority.

Results from the Five Trained Subjects

It is the results of these five trained subjects that we wish most to emphasize; (1st) because each was given previous preliminary training, (2d) because of their conscientious work and careful following of the conditions of the experiment, and (3d) because of the value of their introspections.

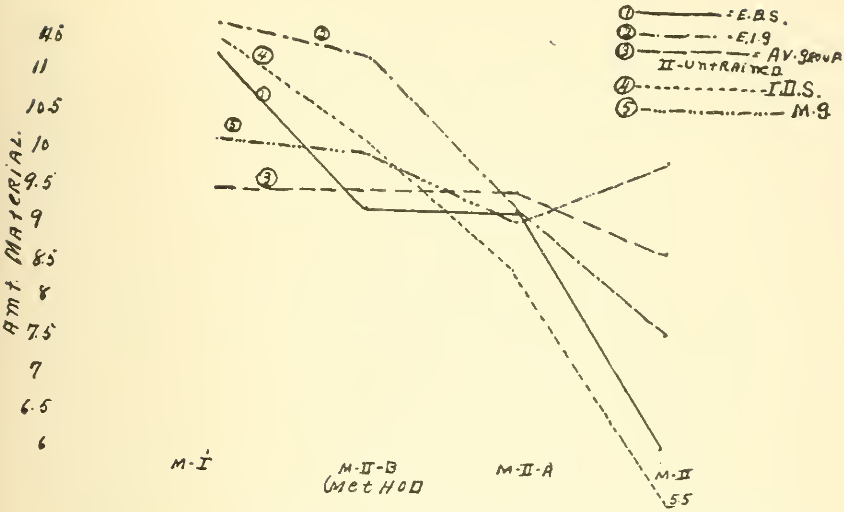
The records of the trained subjects indicate the principle stated above better than do the untrained subjects. Graphs I., II., and III., give curves indicating the principle. (S.M. has no curve plotted because she participated in only two methods.) Tables XI. to XV. give the data tabulated.

A preliminary training was given to all five subjects before any records were kept—the training ranging from ten to fifteen series learned. The averages are based on from four to six learnings in each method, after weeding out learnings which involved abnormal conditions, as fatigue, distractions, etc.

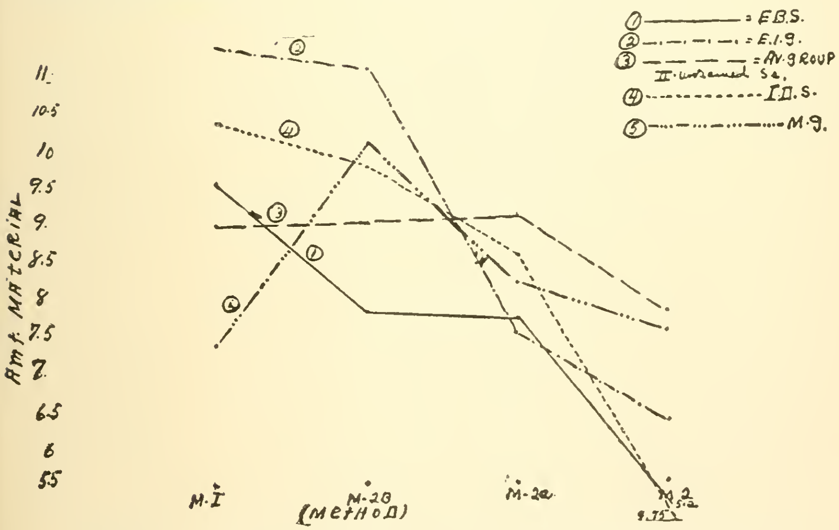
According to the results obtained by the trained subjects the following principle seems strongly indicated: interspersed learning and recitation is better than any form of grouping, the efficiency decreasing as the grouping increases.

Consideration of factors involved which seem to be more effective in the interspersed than in the grouped methods.

Under the conditions of this experiment it is clear that,



GRAPH II. Curve showing relative amount retained (nonsense syllables) after an interval of one hour.



GRAPH III. Curves showing relative amount retained (nonsense syllables) after twenty-four hour interval.

either the interspersed method must involve helping factors not found at all in the grouped methods or else utilize the factors to a greater degree. On the basis of introspective reports we hazard to suggest the following factors as operating in favor of the interspersed method.

1. Time and again our subjects say that in the interspersed method they can find their weaknesses and check up thus on the next learning fixing these eluding syllables. This is doubtless a superficial expression of the more fundamental facts. However, the essential fact seems to be this: Certain syllables are but partly secured, the subject is not certain of them. This furnishes an incentive or attitude whereby, on the next learning, there is a higher degree of concentration and a determination to get the syllables that makes for efficiency. Again, the fragmentary part of doubtful syllables constitutes a mental set favorable to the true syllable when it comes—gives it an extraordinary attention value.

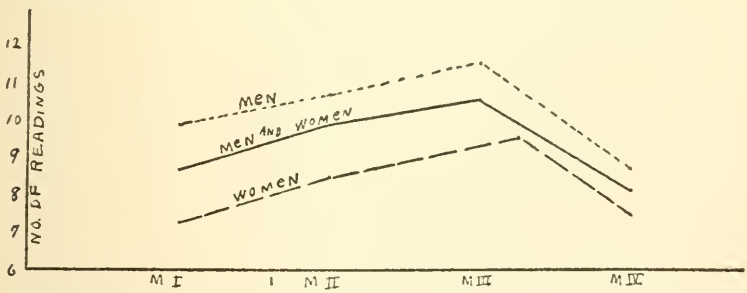
2. Each time the subject recites he repeats (without the stimulus) the syllable. As a mere matter of habit, this tends to fix the syllable, and so *S* does not pay much attention to this or that one next reading but concentrates on the 'new' or 'wrongly recited ones.' This is another condition expressed by practically every subject. It is clear that this method can be little utilized in the grouped methods. You cannot check up on your weaknesses so well here, you cannot have that satisfaction of knowing you have the syllable thoroughly and so gauging your line of attentive attack on new materials. But this repeating of the syllable without the stimulus being before you and at leisure seems to be a very important factor.

Gates finds that his subjects had ill success in forming associations by a mere shifting of attention from one to another contiguous element. This leads Gates to hold as insignificant the importance of attention shift from element to element contiguously. However, there is something else we must consider. We would say that the very core of association forming is the shift of attention from one element to another. But this attention must be of active nature, involving 'anticipation,' a 'reaching out for' the next element, to be effective. Under this condition, when the syllable comes a bond seems to bind the contiguous elements. True, the mere contiguity in consciousness of the two syllables is little effective.

Just what constitutes this anticipation, this reaching out, we are not sure. But all can experience it—all do experience it in memory recall and all can experience it in slow exposures of nonsense syllables. Syllable A comes. If I have seen it before and have time, I definitely employ this anticipation. It seems to be a sort of active attitude, a more or less complete recall into consciousness of the next syllable.

Certainly it involves partially at least the act of recall. The syllable flashes into consciousness before the actual stimulus comes. On the other hand, the anticipation may not thus recall the syllable and one gets something akin to the lowest form of recognition. The association bridge is almost crossed but the experience does not quite come.

Now in recitation, this active attitude, this anticipation, can operate as it cannot in reading nonsense syllables. Reading discourages this process and the more we group our readings the less this factor operates. In so far as this anticipation process enters at all into learning (reading) it is no longer a reading process! The very core of recitation is this anticipation.

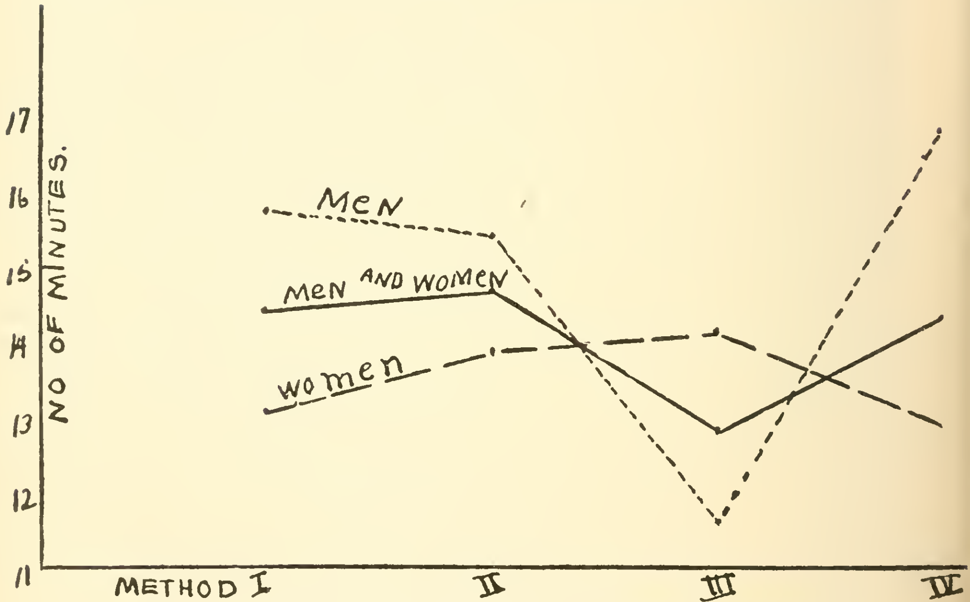


GRAPH IV. Showing comparative efficiency of four methods as to number of L's required to learn.

3. The 'setting time' seems better utilized in recitation and the consequent different types of inhibitory processes are less destructive. Often the subject reported that he had a syllable until the next was shown and then the former was blotted out of existence. It was a help to slide over the syllables and hold longer, in consequence, to the troublesome syllable. The interfering tendencies of retroactive inhibition

were thus decreased. Something of the same factors found in divided rather than concentrated repetitions in memorizing a poem, seems to be effective here.

It is clear that grouping the readings together does not give these above mentioned factors as much opportunity to be effective as the interspersed. The same general principle that makes reading plus recitation better than mere reading alone,



GRAPH V. Showing comparative efficiency of four methods as regards time.

and that makes divided repetitions better than concentrated, makes interspersed reading-recitation better than any form of grouped method.

Incidental Results and Suggestions

1. Using the products moment formula (as there were a number of tie cases we did not think it best to use the rank method), the coefficient of correlation was worked out for the 24 subjects between amount learned and the amount retained after 24 hours. We found r to be .65, probable error as .078.

This is in keeping with the general trend of findings, namely, that those who learn best retain longest.

2. Using our methods of presentation, learning and scoring, our curve of forgetting drops but very little in 24 hours. On the average about 10 per cent. of the syllables were lost during the first hour and then about 5 to 10 per cent. is lost at the end of 24 hours. The main suggestion from our curve is this: we should speak of learning curves rather than any one learning curve. To hold Ebbinghaus' curve as typical seems hardly fair to the facts. The kind of forgetting curve one gets depends on such things as kind of material used, method of learning, method of testing, etc.

3. The fact that some subjects did as well on the extreme grouped readings as on the more interspersed, and the fact that other subjects were very helpless indeed in the extreme grouped methods, seemed to indicate two if not types, then extremes of a distribution curve, as regards learning. One group might be called a 'mechanical sort' the other an 'associative group.' (We do not wish to be charged with calling these 'types.') Subjects A.S., D.S., M.S., J.L., *e.g.*, (Table V.) illustrate that group in which reading alone gives almost as good results as reading plus recitation. On the other hand subjects E.B.S., I.D.S., M.G. seem helpless in the method of grouped readings.

These variations need further investigation and might throw some interesting light on differences in learning.

Part II. Sense Material (Poetry)

(*L and R Used in Strict Sense of Term*)

As the first part of the investigation showed rather clean cut results it was thought well to extend the experiment to include sense material. After all, from the practical standpoint, we wish to know which learning method is best as applied to sense material. Other investigators (Gates, *e.g.*) found that with sense material his principles held less true.

Consequently eight line stanzas of poetry were selected (Byron's 'Don Juan'). As in Part I., the *L*'s and *R*'s were used in the strict sense, no promptings being used whatever in the recitations. An *L* means that the subject read through the stanza from beginning to end with concentrated attention, while an *R* means an attempted recall without any prompting.

Thirty-six persons acted as subjects, all but two undergraduate students taking elementary psychology. Nineteen of the subjects were men and 17 were women.

Procedure.—The stanzas were typewritten upon slips of plain white paper. In *L* the whole method was used. The subjects sat about tables in the room with very little disturbance going on at any time. Each subject took the number of stanzas he thought he would use on that occasion and started work. For the most part an hour's work was the maximum, though in a few cases a longer work period was granted. The subjects were instructed to stop learning at once if they began to feel at all bored, tired, or fatigued. A general supervision was kept over the subjects as they thus worked.

Four methods were used here, each method involving two preliminary but unrecorded readings. The methods were:

1. *L R L R L R*, etc., until learned.

II. *LL RR LL RR*, etc., until learned.

III. *LLL RRR LLL RRR*, etc., until learned.

IV. *LLL RRR LL RR L R L R*, etc., until learned.

As can be seen Method I. is the truly interspersed method, II. more grouped, and III. the most grouped. Method IV. is a mixed method involving III., II., and I. in order and continuing with Method I. as indicated above, until learned.

The subject noted the time by his watch, and started to learn immediately. The two preliminary readings were then done and then he continued on his readings and recitations as outlined in the methods above. When he reached the place where he thought he might reasonably recall the poetry correctly he wrote it out (the previous recitations being mental or whispered). He then checked up his written work by a check reading. If there were no mistakes, he recorded the time and this ended this learning. However, there were usually a few minor mistakes, and in this case, subject counted the check reading in his record and then tried to recite the poem again (but only mentally or whispered as previously), again taking a check reading. This was kept up until a perfect reproduction was made. Then the time was noted. So a record of *L*'s and *R*'s and the time required was kept for each learning.

For the most part four records were secured from each subject for each of the four above mentioned methods (a few, however, did only three). To eliminate practice effects on any method, they were rotated, though at no time was the same method used to begin with (to eliminate the warming up effects hurting any method).

Unfortunately, however, a number did not understand the directions for including method IV. in the rotating plan, waiting until all other methods were finished before doing this method. This, of course, gives Method IV. the advantage of the previous practice. These cases have been indicated in the tables by placing an asterisk at the end of the row. Whenever these have been included in the comparisons, it has been noted.

The average number of *L*'s, *L*'s and *R*'s, and time is given in the following tables for each subject. On the basis of these the general averages are worked out. Due to the fact that the number of records for each method for any one subject is so limited, the general averages probably give truer results than any method of comparing by using absolute individual standings.

An inspection of the tables and graphs shows that, with regard to number of readings required to learn the stanza, Method I. (the interspersed) is best of the three. Two is next best and three takes most readings. If we consider Method IV. (the mixed method) also, then it stands first. The above is true for the averages of the men and women taken separately as well as for their combined averages. (See Graph IV.)

As regards the time required (see Graph V.), Method III., the most grouped method, is slightly the best. However, the differences in time are very small at most and probably insignificant.

Summary.—Using sense material and comparing the three methods used (the interspersed, the slightly grouped, and the more grouped methods) we find that,

Table XVI

Showing Comparative Results for Methods I., II., III., and IV. for 19 Men

Subject No.	Method I.			Method II.			Method III.			Method IV.		
	L's	*LR*'s	Time	*L*'s	*LR*'s	Time	*L*'s	*LR*'s	Time	*L*'s	*LR*'s	Time
1.......	12.75	25.50	16.25	13.00	25.50	11.00	12.00	22.75	13.19			
2.......	18.00	36.25	17.25	26.50	53.00	22.00	25.75	51.50	19.50	24.00	48.00	19.00
3.......	7.50	15.50	14.50	7.75	15.25	16.00	7.50	15.75	16.75	3.70	8.30	18.70
4.......	8.50	17.00	22.80	11.70	22.40	28.30	12.25	22.75	26.75	10.30	20.60	20.30
5.......	10.50	21.00	17.77	13.25	25.25	15.00	13.00	24.00	13.00	9.00	17.50	11.50
6.......	9.00	18.00	—	5.50	11.00	—	6.00	12.00	—	5.00	9.50	—
7.......	6.25	12.25	10.50	7.00	12.75	9.88	5.75	11.00	10.50	—	—	—
8.......	4.00	8.00	7.25	5.50	10.00	10.75	8.50	15.50	12.00	—	—	—
9.......	5.30	10.60	10.00	6.00	11.70	11.00	7.30	14.00	12.00	8.00	16.00	17.00
10.......	5.30	11.30	19.70	4.70	10.40	14.70	5.75	12.75	17.25	6.30	12.60	18.70
11.......	13.50	27.00	14.60	13.50	26.25	14.00	15.00	28.00	13.00	12.50	25.00	25.00
12.......	13.75	27.50	15.25	11.50	22.00	14.00	12.00	22.00	11.50	11.50	22.50	11.25
13.......	6.75	13.50	7.25	10.25	19.75	7.33	15.00	27.75	12.25	10.70	20.30	8.00
14.......	11.75	23.50	12.62	13.00	25.50	11.38	12.25	23.50	12.88	9.30	18.70	10.70
15.......	15.75	31.50	18.50	13.75	27.50	17.50	13.50	27.00	16.25	16.30	29.30	14.00
16.......	12.00	24.00	27.00	13.00	25.70	22.00	15.70	31.40	22.00	13.70	27.40	21.30
17.......	8.00	16.00	16.30	9.00	16.70	17.70	10.00	18.30	19.00	10.30	20.30	21.00
18.......	6.70	13.40	14.70	6.70	13.40	14.70	7.00	13.70	15.30	6.70	13.70	16.00
19.......	11.70	23.40	20.00	12.00	23.70	19.30	15.00	29.30	17.70	11.70	23.40	19.30
Total....	187.00	375.20	282.24	203.60	397.75	276.54	219.25	422.95	208.82	149.00	333.10	251.75
Av.......	9.84	19.76	15.78	10.71	20.93	15.40	11.53	22.26	11.60	8.76	18.51	16.78
M.V.....	3.27	6.52	3.82	3.58	7.10	3.93	3.58	6.88	4.17	3.50	7.62	3.90

TABLE XVII

SHOWING COMPARATIVE RESULTS IN METHODS I., II., III., AND IV., FOR 17 WOMEN

Subject No.	Method I.			Method II.			Method III.			Method IV.		
	L's	*L*'s and *R*'s	Time	*L*'s	*L*'s and *R*'s	Time	*L*'s	*L*'s and *R*'s	Time	*L*'s	*L*'s and *R*'s	Time
21.......	7.00	14.00	11.00	8.25	15.50	14.25	8.75	16.00	11.00	7.00	14.50	10.25*
22.......	10.50	21.00	13.25	14.25	28.25	14.25	17.25	34.00	15.25	8.00	16.00	11.70*
23.......	4.75	9.50	10.50	6.25	11.75	9.75	5.50	10.00	10.00	5.50	11.00	6.50*
24.......	6.75	13.50	12.25	6.25	12.50	10.00	6.25	12.50	11.00	6.30	12.30	12.70
25.......	7.00	14.00	10.30	10.00	19.70	14.00	10.70	20.40	16.00	9.30	18.60	12.30
26.......	5.10	10.00	8.50	5.30	10.00	8.00	4.50	9.00	7.95	4.50	8.75	8.25
27.......	9.30	18.60	14.00	10.00	20.00	13.70	13.00	25.30	11.30	8.00	16.00	14.50
28.......	8.30	16.60	16.30	9.00	17.75	16.70	14.30	28.60	18.30	10.00	20.00	8.30
29.......	6.75	13.50	10.75	7.25	14.00	10.25	7.00	14.00	9.90	6.67	13.67	9.70
30.......	10.30	20.60	15.30	10.30	20.60	16.20	11.30	22.00	17.70	7.30	14.60	11.50
31.......	6.00	12.00	15.00	7.30	14.30	19.70	9.00	18.00	22.00	7.00	14.00	17.70
32.......	9.00	18.00	15.70	10.70	21.40	15.70	10.00	19.30	11.30	9.70	19.40	11.30
33.......	8.00	16.00	17.50	11.70	23.00	25.70	11.30	22.00	26.00	9.00	18.00	20.80
34.......	4.75	9.50	10.25	7.00	13.25	10.75	7.00	12.70	13.30	7.00	13.30	11.00
35.......	5.25	10.25	9.25	7.00	13.00	8.50	10.50	19.00	11.00	7.00	13.30	9.30
36.......	5.30	10.60	9.70	5.87	11.40	8.90	6.50	12.75	9.20	7.00	14.00	11.03
37.......	9.30	18.60	22.70	7.30	14.60	19.70	8.30	16.60	19.30	8.00	16.00	22.00
Total....	123.35	246.25	222.25	143.72	281.00	236.05	161.15	312.15	240.30	127.27	253.40	218.80
Av.......	7.25	14.48	13.09	8.45	16.52	13.88	9.48	18.36	14.13	7.49	14.90	12.87
M.V.....	1.63	3.23	2.96	1.97	4.12	3.67	2.66	5.15	4.18	1.13	2.31	3.46

(1) as regards the number of readings and recitations, the interspersed method is the best, the slightly grouped second best, and the more grouped method least efficient;

(2) as regards the time required to learn, while there is very little difference, the grouped method is best, on the whole.

Considering all four methods, we find that as regards readings and recitations the mixed method is best. As regards time however, it is not so good as the Method III.

Explanation.—The "mixed method" probably derives one advantage from the fact that there are more readings to start with. However, as it utilizes all three other methods, but Method I. most, and inasmuch as Method I. is most efficient in comparison to Method II. and III., it seems reasonable to think that Method IV. gets its second advantage through involving the interspersed method.

Sex differences.—A glance at the tables (XVI. and XVII.) and graphs IV. and V. shows that the women do much better than the men in:

(1) the number of readings required,

(2) the number of readings and recitations combined, and,

(3) the relative amount of time taken to learn.

Also, the men are far more variable than the women, as shown by the mean variations. In a word, the women surpass the men in memorizing poetry and the men are more variable.

Part III. *R* Used in a Mixed Sense

For comparative purposes, and to test our methods as they would be involved in a practical learning (*e.g.*, by the student), we now used *R* in the mixed sense, that is, an *R* was an attempted reconstruction plus a prompting (visual) when the subject was 'stuck.' The *L* remained the same as previously used. The subject would, *e.g.*, read through according to the method and then try to recall all possible, taking a look to help himself along. Obviously the first *R*'s were largely readings but became progressively more truly *R* as the process continued. Table XIX. gives the results, averages and mean variations.

Table XVIII

Showing Combined Results for All Methods for 19 Men and 17 Women

	Method I.			Method II.			Method III.			Method IV.		
	L's	*LR*'s	Time	*L*'s	*LR*'s	Time	*L*'s	*LR*'s	Time	*L*'s	*LR*'s	Time
Total....	310.35	621.45	504.49	349.32	678.75	582.59	380.40	735.10	449.12	276.27	586.52	470.58
Av.......	8.62	16.98	14.41	9.92	19.39	14.64	10.56	20.41	12.83	8.12	17.25	14.26
M.V.....	2.65	2.45	3.61	3.05	6.09	3.83	3.28	6.38	3.71	2.51	4.94	4.27

Table XIX

Showing Comparative Results in I., II., III., and IV., for 11 Men

Subject No.	Method I.			Method II.			Method III.			Method IV.		
	L's	*LR*'s.	Time.	*L*'s	*LR*'s.	Time.	*L*'s	*LR*'s.	Time.	*L*'s	*LR*'s.	Time.
41.......	8.70	17.40	17.70	7.30	15.00	13.70	7.00	14.30	13.00	6.30	13.00	11.30
42.......	6.50	13.00	12.25	6.50	13.00	12.75	8.25	16.50	17.00	7.25	14.50	12.75
43.......	6.50	13.00	18.00	8.00	16.00	18.30	7.30	14.60	17.30	6.75	13.50	18.00
44.......	5.00	10.00	9.75	6.70	13.00	12.00	6.75	16.25	12.10	6.25	12.00	12.50
45.......	4.00	8.00	9.10	4.50	8.75	9.90	5.25	9.50	8.80	4.50	8.75	10.20
46.......	7.50	15.00	15.25	8.70	16.70	15.70	7.50	14.75	15.00	7.50	14.50	12.25
47.......	5.00	10.30	9.70	5.50	10.00	8.00	3.00	6.00	8.00	—	—	—
48.......	7.70	15.40	15.00	7.30	14.30	13.75	7.70	15.40	16.00	9.30	18.30	14.30
49.......	4.70	9.40	10.00	6.30	12.30	8.80	6.70	12.00	7.20	5.00	10.00	6.70
50.......	6.30	12.60	13.70	4.00	8.00	10.70	6.00	10.70	11.70	5.30	10.30	10.70
51.......	5.25	10.50	11.90	6.50	12.50	11.50	4.25	8.25	8.75	5.50	9.50	7.00*
Total....	67.15	134.60	142.40	71.30	139.60	135.10	69.70	138.30	134.90	63.65	124.40	115.70
Av.......	6.10	11.23	12.94	6.48	12.68	12.28	6.33	12.57	12.26	6.35	12.43	11.57
M.V.....	1.18	2.45	2.78	1.23	2.16	2.32	1.25	3.07	3.12	1.06	2.32	2.49

The results are not very satisfactory, because the differences are not large. However, of all four methods, Method I., interspersed, is the best as regards the number of readings and readings-recitations combined. As regards time, the order of efficiency is IV., III., II., I.

General Conclusion

Using nonsense material and sense material, the interspersed method of reading recitation seems to be better than methods of grouping. The results are more striking for nonsense material than for sense material.[1]

The question still unsettled is as to how many preliminary readings one should make before introducing the interspersed method of reading recitation. Gates has answered this question probably better than any one else so far, but the optimum preliminary readings will vary according to the length of the material, its difficulty and organization. Certainly introduction of the recitations too early does not favor the interspersed method of reading-recitation.

[1] The experiments reported in this paper were performed in the Psychological Laboratory at the University of Michigan.

RESULTS OF SOME EXPERIMENTS ON AFFECTION, DISTRIBUTION OF ASSOCIATIONS, AND RECALL

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The data presented in this report represents the result of some preliminary spade-work which has for its goal a means of measuring individual differences in cheerfulness. The immediate stimulus for the first experiment was the appearance in 1917 of the report by Baxter, Yamada, and Washburn,[1] which not only suggested a possible means of measuring temperaments, but also served to re-open the question of the relation between affection and recall. Later, the same year, the present writer gave a free, discrete association test three times to the same subjects, who designated as '*P*,' '*U*,' or '*I*' the nature of the association in each case, hoping to obtain information of the following points: (*a*) the relation between affection and recall, (*b*) the relation between scattering or concentration of associations for the three presentations, and recall, (*c*) the relation between this distribution of associations and the kind of affective response, and (*d*), the existence or non-existence of a general affective sensitivity. Nothing more was done until the last school year, when other similar experiments were given, partly to check up on the results of the first experiment, and partly to determine the results of correlating the scores of these later tests with self-estimates in cheerfulness. These experiments will be described later.

Baxter, Yamada, and Washburn, in the article just mentioned, reported the results of an investigation in which they used two lists of stimulus-words, to one of which the subjects were required to get pleasant associations, to the other, unpleasant associations. The ratios of the average reaction

[1] 'Directed Recall of Pleasant and Unpleasant Experiences,' *Amer. J. of Psychol.*, 1917, 28, 155–157.

times of the two lists furnished an index which showed some correlation with the estimates of friends who judged the subjects in cheerfulness.

In 1919, Morgan, Mull, and Washburn[1] used a different method, which is best described by the instructions to their subjects. "When I pronounce a word to you, observe what idea that word first calls to your mind, and report whether it is a pleasant or unpleasant idea. If it is neither pleasant nor unpleasant, but indifferent, continue thinking until either a pleasant or unpleasant idea is suggested, and report what it is." No times were recorded, the scores being the number of pleasant and unpleasant words suggested. These scores showed some correspondence with the estimates of friends.

The results of these experiments make possible another explanation of most of the data which have been presented in favor of the theory that unpleasant experiences are repressed, a theory of considerable importance in connection with tests of cheerfulness and depression. In general, three sorts of evidence have been advanced in favor of this theory, one of which is represented by the work of Henderson[2] and of Myers,[3] who asked subjects to recall a number of experiences, or of objects of a certain class, and to indicate which were pleasant or were liked, and which were not. The results agree in showing a greater number of pleasant than of unpleasant objects. But these results may be the result of the operation of any one or more of at least three factors: first, the repression of the unpleasant as such; second, a greater number of pleasant than of unpleasant experiences in the previous life of the subject, and third, the mood of the subjects, most of whom are likely to be rather more cheerful than depressed.

Another line of evidence has been secured by testing the

[1] 'An Attempt to Test Moods or Temperaments of Cheerfulness and Depression by Directed Recall of Emotionally Toned Experiences,' *Amer. J. of Psychol.*, 1919, 30, 302–304.

[2] 'Do We Forget the Disagreeable?' *J. of Phil., Psychol., & Sci. Methods*, 1911, 8, 432–438.

[3] 'Affective Factors in Recall,' *J. of Phil., Psychol., & Sci. Methods*, 1915, 12, 85–92.

ability to memorize lists of pleasant and of unpleasant words. Tait[1] found that ordinarily the 'pleasant' lists are more readily memorized than either 'unpleasant' or 'indifferent' lists. In most cases the memorability of the 'unpleasant' lists is higher than that of the 'indifferent' lists. The results, therefore are not regarded as conclusive evidence that the unpleasant is repressed. They might conceivably be due either to the greater number of pleasant experiences, or to the mood of the subject. That these may be factors is suggested also by the fact that for some subjects the 'unpleasant' lists are remembered best.

Tolman,[2] using a similar method, found that an average of 11.5 repetitions were required for the 'pleasant' lists, 12.7 for the 'indifferent,' and 13.5 for the 'unpleasant.' His results differ from Tait's in placing the memorability of the indifferent words above that of the unpleasant words.

Somewhat different is the evidence submitted by Tolman and Johnson,[3] who determined the average association reaction times to '*P*,' '*U*,' and '*I*' words. They found that the times were longer when the '*U*' words were given. Here we may be dealing not so much with a repression of the unpleasant as with a disturbance due to the affective or emotional reaction. The same result might also be expected if we assume that there are more pleasant than unpleasant experiences in the lives of the students who generally act as subjects in these experiments. Tolman and Johnson found their most positive results in the words referring to simple sensory qualities, and it seems to the writer that more of the pleasant than of the unpleasant words used by them would call up experiences which the average college student has actually had. If so, a difference in the reaction times of the two lists would naturally be the result.

Peters[4] used a discrete association method, requiring his

[1] 'The Effect of Psycho-physical Attitudes on Memory,' *Studies in Abnorm. Psychol.*, Series 4, 10–38.

[2] 'Retroactive Inhibition as Affected by Conditions of Learning,' *Psychol. Monog.*, 1917, **25**, No. 107, pp. 22 ff.

[3] 'A Note on Association-time and Feeling,' *Amer. J. of Psychol.*, 1918, **29**, 187–196.

[4] 'Gefühl und Erinnerung,' *Psychol. Arb.* (Kraepelin), 1911, **6**, 197–260.

subjects to respond to the stimulus words by recalling some previous experience, followed by the description of the feeling tone of the original experience as well as of the memory itself. He found more pleasant than unpleasant associations. Perhaps there were more to be recalled.

In all the above experiments,[1] a difficulty arises from the fact that we cannot always control the nature of the affective reaction by our choice of stimulus-words. The result of this, however, would be rather to obscure results than to give spurious results. In the experiments about to be described an effort was made to get around this particular difficulty.

Kate Gordon[2] showed a series of figures to her subjects and later tested their ability to recognize the ones previously shown. She concludes that "Dás Wiedererkennen der früher gesehenen Figuren ist gleich gut für die gefälligen, missfälligen und gleichgültigen."

In the light of the above experimental data and of the various possible interpretations in most cases, no definite answer can as yet be given to the question regarding the repression of the unpleasant.

Method

Experiment I.—A list of fifty words, half adjectives and half nouns was prepared, about equally divided between those which might be expected to call up pleasant associations and those which might be expected to call up unpleasant associations. The instructions to the subject were as follows: "I have a list of fifty words, which will be pronounced one at a time at intervals of thirty seconds. When a word is given, you are to wait until it suggests some previous experience when you are to write in the proper space some word connected with the associated idea. You will also record, in the third column, whether the association called up was pleasant, unpleasant or indifferent." The nouns were given first, fol-

[1] No attempt has been made to cover the whole field of literature bearing on this subject: representatives only of the different methods have been selected. Tait gives a rather extensive bibliography.

[2] 'Ueber das Gedächtnis f. affectiv bestimmte Eindrücke,' *Arch. f. d. ges. Psych.*, 1905, 4, 437–459.

lowed immediately by the adjectives. The whole set was given three times, in immediate succession, in different order. Three separate responses were therefore obtained for each word. Then, after a series of group mental tests had been given, requiring about an hour, the subjects were asked to recall as many as possible of the stimulus words used in this test. This was wholly unexpected by the subjects. There were thirty-six subjects, all of whom were beginning students in psychology.

A separate blank had been prepared for each presentation, with a numbering system to facilitate the tabulation of the nature of each of the three responses to each stimulus-word. It may readily be seen that any one word might evoke three '*P*,' '*U*,' or '*I*' responses, two of any one response and one of either of the other two, or one each of the three. There were therefore ten possibilities to consider in tabulating the nature of the affective responses.

It is also evident that the reaction-word (if such it may be called) might be the same in each of the three responses; that there might be but two different reaction-words, one of these being repeated; or that there might be three different associations. Since the distribution of associations might affect the recall of the stimulus word, and because it seemed advisable to determine the relation between this distribution of associations and the nature of the affective responses, three tables were prepared, headed *D*1 (the same reaction-word each of the three times); *D*2 (two associations, one twice, the other once); and *D*3 (three separate associations, each appearing once). Under each of these three main headings, there are ten columns, one for each of the possibilities noted in the preceding paragraph. In tabulation, the results for each subject were kept separate, by using a different row in the tables for each subject. This row was divided by a fine line. If the stimulus word were recalled, the mark in the table indicating the particular combination of affective and associative (ideational) responses was placed above this line; if the stimulus word were not recalled, this mark was placed below the line. This large table, when completed, was totalled both by columns and by rows. The totals by rows indicated the number recalled and forgotten by each subject, for each of the three possible distributions of associations. The totals of the columns showed the number remembered and forgotten for each of the possibilities with regard to affective responses; that is, the totals showed the number of marks 'above the line,' and the number of marks 'below the line.' The totals of the affective responses were determined for each subject by a system of points. A mark in the $P3$ (each response pleasant) column counted three points for P; a mark in the $P2$–$U1$ column counted two points for P and one for U, since this meant that two of the three responses were pleasant and the other unpleasant. This was done separately for the marks 'above the line' and 'below the line' (stimulus word recalled, and stimulus word not recalled). Three columns of totals were the result, one each for P, U, and I responses. Three additional columns showed the ratio of the points above the line to the points below the line for each subject, and for the P, U, and I columns respectively. These ratios were used as a measure of the effect on recall of each of the three possible affective responses, although separate results will be given where the $P3$, $U3$, and $I3$ columns only were considered.

It may be objected that the actual distribution of experiences recalled can not be accurately determined in the manner described above, and the justice of the criticism must be admitted. Yet the experimenter believes that, on the whole, the results are approximately correct. The errors which doubtless crept in would not be likely to produce fictitious correlations.

Another criticism may be that no consideration was given in the instructions to the subject to the rather important distinction to be made between the affective tone of the original experience which was recalled and the affective coloring of the recall. Nothing was said about this distinction to the subjects for fear that the net result would be to confuse more than anything else, since most of the subjects were just beginning the study of psychology. It is probable that in the majority of cases the judgment was based more on the nature of the original experience than upon the feeling tone accompanying the recall. Yet in the majority of cases this affective tone was probably the same, though of less intensity, in the recall as in the original experience. For convenience, in the discussion to follow, I shall speak of the 'affective response' or the 'affective reaction' to the stimulus-word.

The pronounciation of three of the one-syllable words was misunderstood by several of the subjects and were eliminated in the tabulation of the results.

Experiment II.—This experiment was similar to the preceding, differing in that 100 words were used, in that the words were presented visually, and in that the list was given but once. The subjects were later asked to recall the stimulus-words, as in the first experiment. The tabulation of the results was the same except for the simplification arising from the fact that there was but one presentation.

Experiment III.—This was a duplication of Experiment II., save for the fact that a different list of words was used. There were only thirteen subjects in this group.

Experiment IV.—Continuous association test. The subjects were first given a free, continuous association test; then a three-minute test in which they were to write as many as possible of words with pleasant meanings or associations; and another similar period during which 'unpleasant' words were to be written. The number of words written in each of the three parts of the test were tabulated, and the different ratios between the scores computed for each subject.

V. Questionnaire.—The subjects were asked to rate themselves in cheerfulness, as Very Low, Low, Average, High, or Very High. The value of such estimates is obviously limited. Yet this method was used in the hope that any very close correlations would show up to a degree in such results. Most of the subjects were strangers to each other. This was a part only of a more extensive questionnaire, which included self-estimates in twenty-five traits. The rest of this questionnaire was given for other purposes and need not concern us here.

Experiments II. and IV. and the Questionnaire were given to the same group of sixty subjects, all beginning students in psychology. Coefficients of correlation were computed between the results of these tests.

Results

The results will be presented and discussed by topics rather than by experiments, since this will save duplication which would otherwise arise from the similarity of most of the experiments. Those results for which we must look to the first experiment alone are discussed first.

1. The relation between the nature of the affective responses and the distribution of associations (Experiment I.). The score for each subject for the distribution of associations was the total number of different associations called out by the stimulus words. This was obtained by multiplying the number of the *D*1 cases by one, the *D*2 cases by 2, the *D*3 cases by three, and by adding the three products. The total number of points for *P*, *U*, and *I* responses respectively, also the ratio of pleasant to unpleasant responses were then correlated with the scores for distribution of associations with the following results:[1]

Distr. of Associations, and *P* responses	−.20
Distr. of Associations, and *U* responses	.53
Distr. of Associations, and *I* responses	−.17
Distr. of Associations and *U*/*P* responses	.39

These figures indicate a fairly close relationship between the number of unpleasant responses and the number of different associations. This is also clearly evident in the data in Table I.

Yet if we turn to Table II., showing the tabulation of the *P*3, *U*3, and *I*3 responses only, we find no evidence of any such relationship. If, in the '*P*3' column, we multiply the number in the *D*1 row by one, the number in the *D*2 row by 2, and the number in the *D*3 row by three, add the three products

[1] No attempt was made to correct for attenuation any of the coefficients presented in this report. The short-cut method proposed by Adams (*Psychol. Bull.*, 1918, 15, 456–460) was used. This makes use of a formula derived by Thurstone (*Psychol. Bull.*, 1917, 14, 28–32) from the familiar 'products-moments' formula.

and divide by the total number of $P3$ responses, and then do the same with the $U3$ and $I3$ columns, we get the following values: $P3$, 1.85; $U3$, 1.86; $I3$, 1.73. This means that those words with three pleasant responses called up, on the average, 1.85 different associations, etc., there being no difference in the number of different associations called up by the $P3$ and the $U3$ stimulus-words. However, we should not expect to find the same difference in the results shown in Table II. that we find in the results shown in Table I. where there was a change in the nature of the affective responses to many of the stimulus words. In this connection it should be remembered that the data presented in Table I. include the data shown in Table II.

TABLE I

SUMMARY OF RESULTS OF EXPERIMENT I., BASED ON THE TOTAL NUMBER OF POINTS IN EACH CASE

	P	*U*	*I*	Total
*D*1, Number	511	371	489	1,371
% recalled	64.3	59.8	50.5	58.2
*D*2, Number	735	638	655	2,028
% recalled	62.0	58.2	56.8	58.8
*D*3, Number	593	506	578	1,677
% recalled	64.2	64.2	62.6	63.6
Total, Number	1,839	1,515	1,722	5,076
% recalled	63.3	60.5	56.6	60.2

TABLE II

SUMMARY OF RESULTS OF EXPERIMENT I., BASED ON TOTALS OF POINTS, BUT LIMITED TO $P3$, $U3$, AND $I3$ CASES

	*P*3	*U*3	*I*3	Total
*D*1, Number	142	104	126	372
% recalled	66.2	60.5	50.0	59.2
*D*2, Number	146	121	111	378
% recalled	62.4	62.8	52.3	59.5
*D*3, Number	86	63	49	198
% recalled	71.0	65.0	59.2	66.2
Total, Number	374	288	286	948
% recalled	65.7	62.5	52.4	60.8

2. The distribution of associations and the recall of the stimulus-word (Experiment I.). Direct evidence of a positive correlation may be found in Tables I. and II. The '*I*' column in Table I. and the '*I*3' column in Table II. contain

data which are not affected by the nature of the affective response to the stimulus-words, and in these columns there seems to be clear evidence of a direct positive relationship between the number of different associations called out by a stimulus-word and the probability that that word will later be recalled.

In addition, the results obtained by the use of adjectives and those obtained by using nouns were tabulated separately, to show the per cent. recalled for each of the three distributions ($D1$, $D2$, and $D3$) with the following results:

Adjectives.......................$D1$, 58.6; $D2$, 64.4; $D3$, 72.5.
Nouns..........................$D1$, 56.7; $D2$, 53.4; $D3$, 56.5.

The coefficient of correlation between the number of different associations and the total number of all stimulus words recalled is only .18. The method of partial correlation, taking the affective response into account raises this to .38. Yet the small size of these coefficients is not to be regarded as evidence against the existence of a real correlation.

3. Distribution of affective responses. On this point the results are in accord with those reported by other investigators. In Experiments I., II., and III. (Tables I., II., and III.), the results agree in showing a greater proportion

TABLE III

DISTRIBUTION OF AFFECTIVE RESPONSES AND THE PER CENT. OF STIMULUS-WORDS RECALLED IN EACH CASE. BASED ON 'POINTS' IN THE ORIGINAL TABULATION OF DATA

Response	Experiment II		Experiment III	
	Av. Number	Per Cent. Recalled	Av. Number	Per Cent., Recalled
P...........	41.1	30.0	27.3	25.2
U...........	31.0	24.6	17.6	23.1
I............	27.8	27.4	10.1	23.0
U/P.........	78.8	84.6	57.5	71.0

of pleasant than of unpleasant associations. Since different sets of words were used in each of the three experiments I believe it improbable that the results are due to an unfortunate choice of words. Yet, as has been pointed out earlier in this report, such results offer no evidence that unpleasant experiences are repressed.

The results of Experiment IV., shown in Table IV., show that more 'pleasant' than 'unpleasant' words may be written in three minutes.

TABLE IV

(EXPERIMENT IV)

Average Number of Words per Subject Written in Three Minutes

	Free	*P*	*U*	*U/P*
Mean........	62.8	36.5	31.7	.88
S.D..........	14.7	12.6	11.8	.23

4. The effect of the affective response upon the recall of the stimulus-word. The most direct evidence on this point is to be found in Tables I., II., and III., showing the per cent. recalled under different conditions. In every case a greater per cent. of *P* than of *U* stimulus-words were recalled, and, generally, a greater per cent. of *U* than of *I* words. However, these results are not necessarily to be regarded as evidence that unpleasant experiences and the things associated with unpleasant experiences are repressed. They indicate rather that an affective reaction of any kind is favorable to the recall of the stimulus-word. The fact that a greater per cent. of words calling up pleasant associations than of those calling up unpleasant associations were recalled may indicate either that a pleasant reaction is more favorable to recall than an unpleasant reaction, or else, that the responses designated as pleasant were, on the whole, stronger than those designated as unpleasant.

For some reason the nature of the affective response seems to be more effective upon recall when adjectives are used as stimulus-words than when nouns are used (Experiment I.). The per cents. of *P*, *U*, and *I* stimulus-words respectively, which were recalled in each case were as follows: For adjectives, 74, 64, and 54; for nouns, 55, 56, 54.

Coefficients of correlation were computed between the numbers of *P*, *U*, and *I* responses, respectively, and the number (not per cent.) of *P*, *U*, and *I* stimulus-words recalled. The resulting coefficients were; *P*, .68; *U*, .83; *I*, .86. These would be quite as large if not larger in the absence of any effect upon the recall of the stimulus-word by the affective reaction.

I have assumed that ordinarily there was some affective reaction accompanying those associations designated by the subjects as '*P*' or '*U*.' Physiological reactions observed during similar experiments make such an assumption reasonably safe. If one should make the opposite assumption, the results would indicate at least that those words which call up experiences which were affectively toned are more likely to be recalled than other words.

5. Another question of considerable practical importance in the devising of tests of temperament has to do with the relation of the percentage of *P*, *U*, or *I* responses and the percentages of the stimulus-words recalled for each of the three kinds of response. For example, will the factors which cause one person to have more pleasant reactions than the average of the group, also cause him to rank high in the group with regard to the ratio of the percentage of *P* stimulus-

Table V[1]

Intra-Test Correlations

Association	Per Cent. Recalled	Experiments		
		I	II	III
P	*P*	.06	.08	−.10
U	*U*	−.19	.14	.30
I	*I*	.30	−.13	.00
U/P	*U/P*	−.16	−.18	−.32
U/I	*U/I*	.39	.26	.20
P/I	*P/I*	.07	.10	.13

words recalled to the percentage of *U* stimulus-words recalled? If the kind of affective reaction is affected by the mood of the subject (association by congruity of emotional tone), or by a greater number of pleasant or of unpleasant experiences, or by a repression of the unpleasant, it would seem reasonable

[1] The method of tabulation 'by points' has been explained under 'Method.' In tabulation the results from each stimulus-word for each subject, the mark in the table representing the particular combination of affective responses and 'distribution of associations' was placed above a line if the word were later recalled, below the line if it were not recalled. The 'Per Cent. Recalled' represents the ratio of the number above the line to the whole number.

that the same factors would affect the memorability of the stimulus-words for the different kinds of affective response. Yet the coefficients of correlation shown in Table V. offer little positive evidence either way.

When there were a very few affective reactions of a certain kind, ordinarily a greater percentage of these than of the other stimulus-words were recalled. This may be because they were something of a novelty. If so, this novelty would be a factor operating against any effect of the mood of the subject in determining recall. It may also be true that there is something of a negative reaction away from those words calling up unpleasant memories. But if so why should there not be more evidence of a repression of the stimulus-words evoking unpleasant responses? The answer may be that this tendency, if it exists, is overcome by the operation of other factors, particularly the favorable effect upon recall of any affective response. The fact that the recall value of the *P* stimulus-words is higher than that of the *U* stimulus-words, and that it seems to be higher for both than for the *I* stimulus-words would fit in with this explanation. It remains for further work to isolate these factors more completely and determine which are operative and when.

6. Is there a general affective sensitivity? Is a high threshold for one kind of affective reaction associated with a high or low threshold for the other kind of reaction? Some writers seem to assume an affirmative answer, others a negative. While the question is fundamental, it is one which is rather difficult to answer.

If, in association experiments, there were always some affective reaction, it is evident that the coefficient of correlation of the number of pleasant responses with the number of unpleasant responses would be minus one. Yet if we could measure the strength of the reactions we might find a fairly large positive correlation. In any event, the negative correlation between the numbers of the different reactions could not be regarded as evidence against the existence of a general affective sensitivity. In my experiments, however, there were many cases regarded as 'indifferent' by the subjects.

This makes it possible to obtain some information on this point by correlating the scores (frequency) for the different affective reactions with each other, and each with the number of '*I*' responses. This was done with the data from Experiments I. and II. It should be remembered that there were twice as many subjects and twice as many words used in the second experiment, and that each word was given but once instead of three times as in the first.

Table VI

Intra-Test Correlations

	A. Experiment I				*B*. Experiment II		
	P	*U*	*I*		*P*	*U*	*I*
P.......	—	−.02	−.66	*P*.......	—	.26	−.75
U.......		—	−.75	*U*.......		—	−.78
I.......			—	*I*.......			—

C. Experiment I

U/I and *P/I*................	.76	*U/P* and *I*..................	−.09
U/I and *U/P*................	.49	*U/I* and *P*..................	.39
P/I and *P/U*................	.14	*P/I* and *U*..................	.45

In considering these correlations, shown in Table VI., it must also be remembered that we are still moving in a closed circle, so to speak, since the sum of the reactions is always the same. This being true, an increase in the number of *P* responses must be associated with a decrease in the sum of the *U* and *I* responses. If, therefore, the number of *U* responses should remain the same under such conditions, it would indicate an actual lowering of the threshold for these reactions. Therefore, even a zero correlation between the frequencies of *P* and *U* responses, with a large negative correlation between the *P* and *I*, and between the *U* and *I* responses, would point to an actual positive correlation between the positions of the two thresholds. The writer therefore believes that the coefficients given in Parts *A* and *B* of Table VI. may be regarded as evidence of the existence of a general affective sensitivity.

The coefficients shown in Part *C* probably furnish further evidence along the same line. One reason for showing these coefficients is their significance with regard to methods of scoring the results of such experiments. Tolman and Johnson used the ratios *P/I* and *U/I* in working up their results. But since the results may be affected by the mood of the subject (association by congruity of emotional tone), by a general affective sensitivity, and possibly by a repression of the unpleasant, care must be taken in the selection of the ratios to be used. To bring out the relative positions of the *P* and *U* thresholds the ratios *P/U* or *U/P* will be found valuable. The use of any such ratios will be made safer by reducing them to logarithms.

I have been speaking of two thresholds, one for *P* and one for *U* reactions. It may be that there is but one.

I had hoped that further information might be secured by correlating the results of the three parts of Experiment IV. (continuous association tests, three minutes for each part). The following coefficients were obtained: Between the Free and the 'Pleasant' tests, .59; between the Free and the 'Unpleasant' tests, .42; between the 'Pleasant' and the 'Unpleasant' tests, .61. Since the last one mentioned is so nearly the same as the other two the only safe conclusion is merely that some constant factor or group of factors was operating.

Other evidence of the existence of a general affective sensitivity is to be found in the data presented in Table VII., showing the distribution of affective responses for each of the three presentations of the stimulus-words in Experiment I.

On the whole, the writer believes that the data just discussed indicates the existence of a general affective sensitivity. In other words, there seems to be a positive and not a negative relation between the positions of the two thresholds, the one for pleasant reactions, the other for unpleasant reactions. Common experience tells us that the person who is most likely to show pleasure in one situation is most likely to show displeasure in another situation; the stolid, phlegmatic person is not so likely to show any evidence of an affective reaction in either. Yet, on the other hand, the mood of the individual does play a part, and the same thing may at one time cause pleasure and at another displeasure. In one mood one threshold seems to be much higher than the other; in the opposite mood, the other threshold seems to be the higher. In abnormal cases the apparent opposition between mania and melancholia would seem to point to a negative relation.

7. The effect of repetition upon affective sensitivity.[1] The number of *P*, *U*, and *I* responses for each of the three presentations in Experiment I. is shown in Part *A* of Table VII.

Part *B* of this same table shows the results of treating the data in another way. For each subject the order-of-merit method was used in ranking each of the three presentations with regard to the number, first of *P*, then of *U*, and finally of *I*, responses. These were then totalled, with the results shown in the table (the larger the number the lower the score).

TABLE VII

A. DISTRIBUTION OF AFFECTIVE RESPONSES
(1,692 Cases, Each Presentation)

	First Presentation	Second Presentation	Third Presentation
	%	%	%
P.........	35.9	37.6	36.4
U.........	29.4	29.9	30.1
I.........	35.4	33.0	33.4
B. SAME; ORDER-OF-MERIT METHOD			
P.........	79.0	70.0	67.0
U.........	75.5	75.0	65.5
I.........	63.5	80.0	72.5

These results are not very conclusive, but suggest a greater affective sensitivity for the second presentation, while that for the third is less than that for the second but more than that for the first. It may be noticed that the number of *P* responses is greater the second presentation, but that there is a steady increase in the number of *U* associations. The experimenter felt that some of the subjects were becoming 'bored' toward the close of the experiment.

8. As stated earlier in the paper, the purpose in giving Experiment IV. was to determine the correlation, if any, between the results of this test and the results of Experiment II.

[1] Professor H. F. Adams first suggested to the writer the desirability of re-tabulating the data to obtain information on this point, because of its importance in the psychology of advertising.

The value of this correlation is affected by the fact that it was not possible to give the two experiments the same day but on days a week apart. The following coefficients were obtained between the results of the two tests: for P, .18; for U, .09, for U/P, .30. The last may be of some significance. These results are not very encouraging, although the writer intends to include this in a new series of experiments along the same line the coming school year.

9. Questionnaire. Correlations were computed between the self-estimates in cheerfulness and the different scores in Experiments II. and IV., the only experiments in which the subjects were asked to rate themselves. The correlations were generally positive with the scores based on the number of pleasant associations or on the number of 'pleasant' words written in three minutes, and ordinarily negative with the scores based on the number of unpleasant associations or on the number of 'unpleasant' words written in three minutes. However, the only correlation large enough to be of much significance was one of .47, P.E., .06, between self-estimates in cheerfulness and the ratio of pleasant to unpleasant responses in the discrete association test (Experiment II.).

Summary of Results

1. When the same stimulus-words are presented three times in a free, discrete association test there is a correlation of .41 between the number of unpleasant associations and the distribution of associations (number of different associations).

2. There seems to be a correlation between the number of different associations, when the stimulus words are given three times each, and the probability that the stimulus-word will later be recalled. This correlation is fairly large when adjectives are used as stimulus-words, but is practically zero when nouns are used.

3. In every experiment there were more pleasant than unpleasant associations.

4. There is but little, if any evidence that stimulus-words calling up unpleasant associations are repressed. To the contrary, the evidence indicates that an affective reaction of

any kind is favorable to the recall of the stimulus. A greater per cent. of the words arousing pleasant associations are recalled than of those arousing unpleasant associations. The memorability is least for those with indifferent associations.

5. The coefficients show very little, if any correlation between the numbers of *P*, *U*, and *I* responses and the percentages of the corresponding stimulus-words which were recalled. A subject with more pleasant than unpleasant responses may or may not recall a greater per cent. of the stimulus-words for the pleasant associations than of the stimulus-words with unpleasant associations.

6. There is evidence of the existence of a general affective sensitivity. In other words, that there is a positive and not a negative correlation between the threshold for pleasant reactions and the threshold for unpleasant reactions.

7. In the first experiment the stimulus-words were presented three times. The affective sensitivity, in so far as it is measured by the sum of the pleasant and unpleasant associations, was greatest for the second presentation, and was higher for the third than for the first.

8. Three minutes were devoted to the writing of as many 'pleasant' words (names of pleasant objects, etc.) as possible, and another three minutes to the writing of 'unpleasant' words. There is some correlation, .31, P.E., .06, between the ratio of the number of words in the 'pleasant' list to the number in the 'unpleasant' list, and the ratio of pleasant to unpleasant associations in a free discrete association test taken by the same group of subjects.

9. The ratio of pleasant to unpleasant associations in a free discrete association test showed a correlation of .47, P.E., .06 with self-estimates of cheerfulness.

TWO NEW TIME CONTROL INSTRUMENTS

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The two instruments described below were designed to meet the requirements of a research problem which were not satisfied by any existing forms of time control apparatus. Inasmuch as accessory apparatus for measuring out and controlling time intervals is of a considerable utility in a psychological laboratory in regulating the duration and sequence of stimuli and operations, in actuating certain types of exposure apparatus, in operating a series of instruments as is necessary in the finer measurements of reaction times and in numerous other well-arranged measurements, and in checking the accuracy of other instruments; and since these to be described possess several unique and desirable features over other forms it is well that they be placed at the disposal of laboratory psychologists in general.

I. An Acceleration Time Control Apparatus

Time control apparatus based upon the action of gravity on falling bodies has been in use almost since the beginning of laboratory psychology. In the present device, however, the physical phenomena of acceleration is utilized. The essential construction of the apparatus consists of an inclined U-shaped groove upon which a steel sphere rolls. The inner edges of this groove, which form the track for the sphere, are constructed of alternating strips of insulating and conducting material placed in such a manner that the sphere in its passage along the inclined track completes the circuit through the conducting strips arranged opposite each other.

Knowing the angle of inclination of the groove (ϕ), the distance the sphere travels before coming into contact with the conductors (d^1), the distance the ball travels over the conducting strips (d^2), the radius of the sphere (r), the distance separating the edges of the strips (s), and the acceleration of gravity (g) it is possible to compute the time required for the passage of the sphere over the conductors by the following formula:[1]

$$T = \sqrt{\frac{1}{g \sin \phi}\left[2 + \frac{4}{5\left(1 - \left[\frac{s}{2r}\right]^2\right)}\right]}\left(\sqrt{d^1 + d^2} - \sqrt{d^1}\right).$$

Theoretically the time consumed by the ball in passing over the conductors could be varied in several ways. The distance over which the ball passes before coming into contact with the conductors could be varied, the actual length of the conductors altered, the angle of inclination increased or decreased, the size of the sphere varied, or the distance separating the edges of the groove changed. In practical experience it was found that the instrument operated with the greatest reliability, *i.e.*, made the most secure contacts, when the inclination of the groove was about 3 degrees. Some of the other possible sources of alteration of the time intervals being measured out are obviously out of the question with a firmly constructed instrument, as, for example, changes in the distance separating the strips. Theoretically, also, if a V-shaped groove, the sides of which formed a constant and known angle, were used the acceleration would be independent of the size of the sphere; but again, as a matter of experience, it was soon found that a much better contact and a more uniform track was secured when the strips were placed on edge.

The point at which the sphere is started on its downward journey is kept constant and the initial velocity kept at zero by means of a release attachment, consisting of an electromagnet arranged so that it can be securely fastened at any point along the track. To start the sphere it is only necessary to break the circuit through the magnet. The best results were obtained when a laminated core was used and a thin mica sheet attached to the end with which the ball comes into contact. This precaution avoids the undesirable in-

[1] This formula was arrived at in collaboration with Mr. Victor Hoersch, of the Department of Physics of the University of Oklahoma.

fluence which any appreciable residual magnetism might have.[1] Certain other precautions should also be noted:

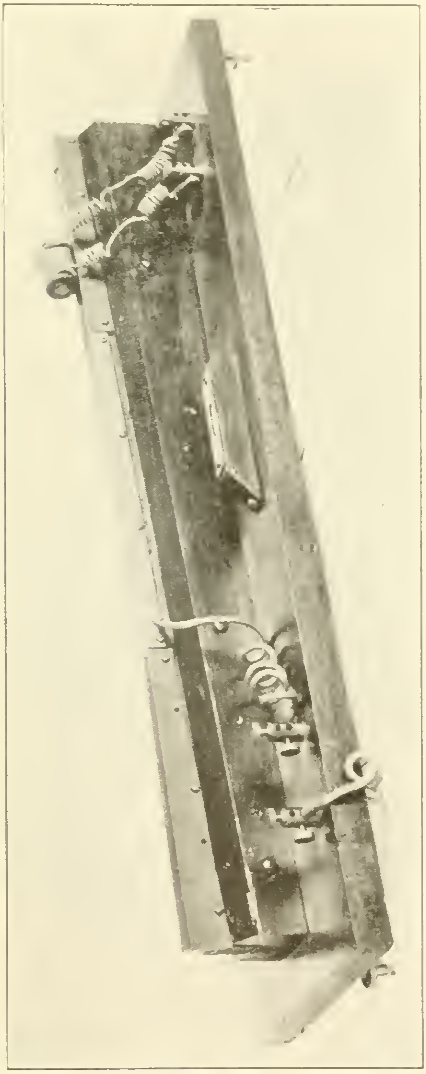

Fig. 1. Acceleration apparatus.

(1) The current must not vary, otherwise the arcing at the bottom of the conductors will produce variations in the

[1] This residual lag could be entirely eliminated if a simple, coreless, magnetic coil were used.

contact length of the strips, (2) with the size and material of the sphere used in the present arrangement a secure contact is not obtained when less than three dry cells are used, (3) the track itself must be kept free from dust, grease, and all foreign matter.

The original instrument as illustrated in Fig. 1 is made of wood with the inclined track of brass and fiber and at an angle of 3 degrees with the base. A one-inch steel sphere is used and the inner edges of the track separated by 12.0 mm. The base is adjustable by means of the three leveling screws and constancy of position is assured by a small spirit level attached to the upper surface. No attempt was made to provide for an adjustment in the inclination of the track as preliminaries had demonstrated the limits of the arrangement. The instrument was planned so that one second would be consumed while the sphere is traveling over the conductors and regardless of the inaccuracies attendant upon amateurish construction it consistently measured intervals of 1.01 second. With a slight adjustment of the position of the release magnet this interval was quickly changed to 1.00 second. The instrument was checked only to the hundredth but it successfully measured intervals between 0.5 and 1.0 seconds accurately within the limits of the checking set up.

The merits of this instrument are:

1. A wide range of times in comparison with other forms of time control devices.
2. A greater flexibility. This instrument is not limited to a make and a break but a variety of electrical combinations is possible in sequence.
3. Accuracy to at least the hundredth.
4. Few adjustments and none to get out of order.
5. Certainty of operation within the limits mentioned.[1]
6. Cheapness and ease of construction. The model illustrated was constructed in six hours. The materials are available around even a modestly equipped laboratory.

[1] Increasing the mass of the rolling body would increase the working limits.

II. A Disc Commutator Time Control Instrument

This instrument utilizes the principle of the staggered commutator combined with the magnetic friction clutch as applied by Dunlap. The chief claim to originality is in the

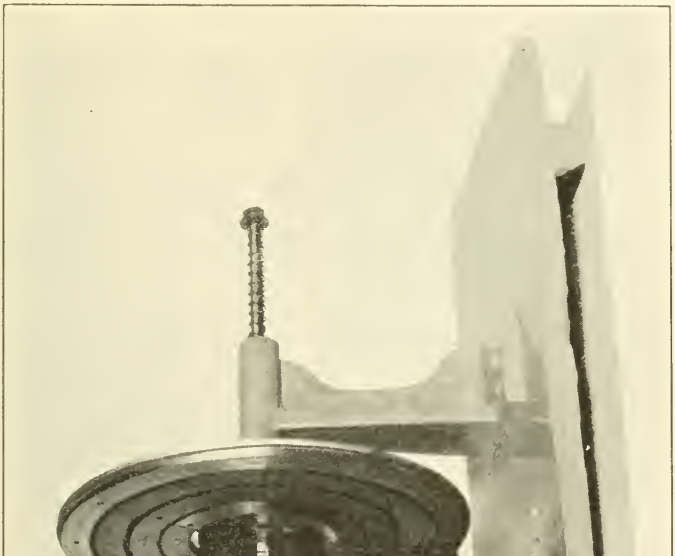

Fig. 2. Disc commutator apparatus.

use of a disc rather than a drum for the commutator and in the matter of the electrical system of the clutch control. The use of a disc makes possible a more compact and a lighter instrument requiring less power and attention in addition to simplifying the calculations necessary for adjusting the collector sectors for varying time intervals. By the system of electrical connections the instrument can by synchronized with the observer or experimenter, it not being necessary to get the human element into step with a steadily revolving drum or constantly swinging pendulum. This feature is especially desirable in all work with children. The electrical control is such that the apparatus requires no attention and the noise incident to its operation can be removed, with the instrument, into a distant room and all auditory disturbance will be removed from the vicinity of experimentation.

Two shafts placed with their centers opposite each other are mounted on a base (Fig. 2). End thrust is eliminated from the drive shaft by shoulders placed each side of the bushings. One end of the drive shaft is fitted to carry the follower of a set of 45-degree spiral gears which meshes with the driver on the motor shaft. The other end of the drive shaft carries two small electromagnets.[1] A small double ring commutator on the drive shaft carries the current from the brushes to the magnets. The disc shaft is held in place by a shoulder on the end nearest the clutch magnets and by a light compression spring on the other end. The tension of this spring is adjustable by the nut at the end of the shaft. The disc commutator is normally about 1/32 inch from the magnets, so that upon the energization the disc shaft springs laterally 1/32 inch and is firmly gripped by the magnets before beginning to revolve with them. When released by the clutch the tension of the spring brings the disc back into normal position and stops it almost instantly. The two outer overlapping conductors on the disc complete the circuit for the clutch magnets through the two lower pair of brushes. All the pairs of brushes are of four receding leaves of spring brush brass and are always in firm contact with the commutator. The brushes are fully adjustable so that the take off is on a line with the radius of the disc perpendicular to the

[1] In order that eddy currents may be avoided both shafts are supported by aluminum castings.

base. Compensation for wear is provided for in the case of the time brushes (two upper pair) by having the end bent at an angle of 90 degrees.

The disc itself is made of aluminum and is attached to the disc shaft by means of machine screws. Being constructed of a light material it gains less momentum than if it were heavier and consequently less tension is required to bring the disc to a full stop than would otherwise be the case. In

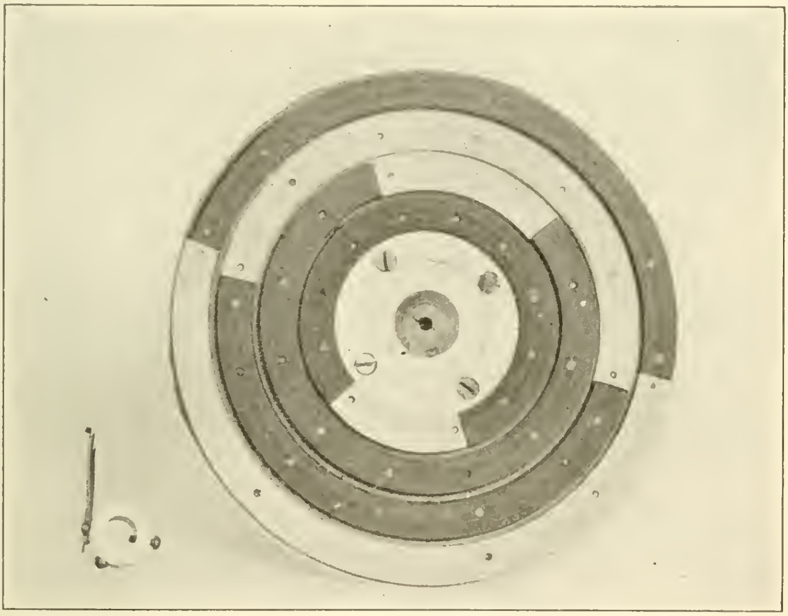

FIG. 3. Face of disc commutator; brush showing holder and adjusting screws.

other terms, less power is used and a smoother operation is attained. The collector sectors are insulated from the disc proper by a sheet of fiber on the face of the disc and fiber washers and bushings are used on the attaching screws.

As arranged in the present instrument the shafts make one complete revolution in 3.6 seconds. At this rate one degree passes the brushes in 0.01 second. Since the time collectors are 70 degrees, seven tenths of a second is measured out in each phase or half revolution.

A wide range of flexibility and a convenient method of change is provided by having holes drilled and tapped in the disc in a symmetrical arrangement on the line of each con-

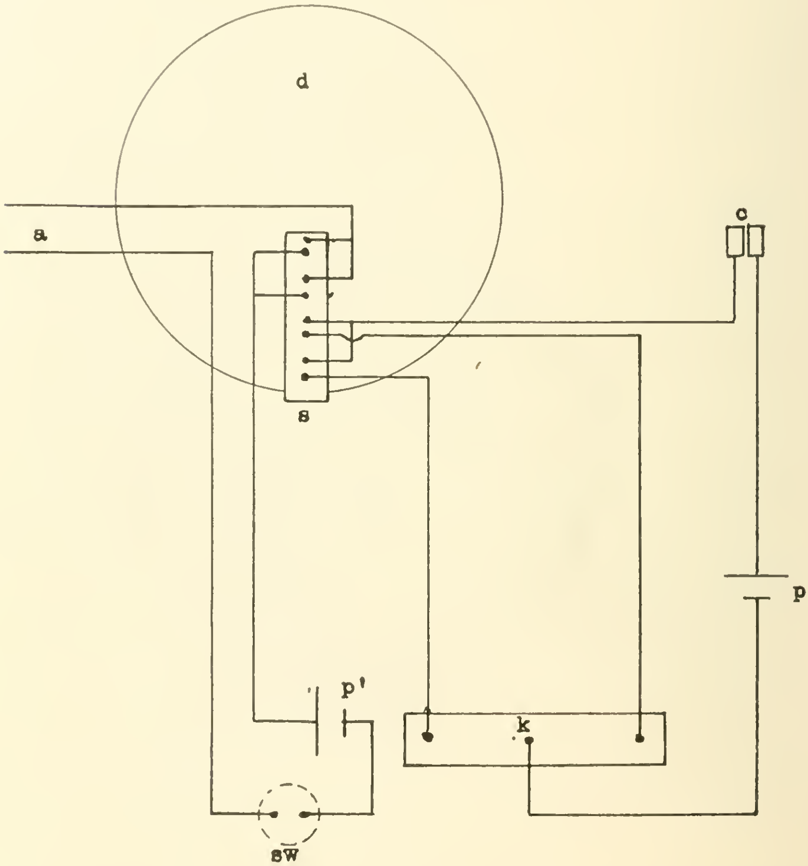

FIG. 4. Diagram of the electrical connections of the disc commutator apparatus. *p* is the source of the primary circuit which passes through the electromagnets by means of the two lower pair of brushes on the brush support, *s*, and the magnet commutator, *c*. *k* is the foot key which shifts the current from the lower to the next above pair of brushes to start the exposure between the phases of revolution of the disc commutator, *d*. *p'* the source of the secondary circuit which passes to the timed apparatus, *a*, through the two inner pair of brushes. *sw*, snap switch for breaking secondary circuit when the control apparatus is not in use.

centric ring. Supplementary sets of collector and insulating rings are threaded all ready to be cut and matched and can be fastened at any point and for any length on the disc, provided that they are on a line with the circle of openings corresponding to their curvature.

By means of a double throw switch the current can be made to pass through either of the primary (overlapping) collector sectors, as shown in Fig. 4. When the switch is once thrown the disc begins its revolution and continues until its first phase is completed when the circuit is broken by the one primary collector passing the brushes. When the switch is again thrown the other primary conductor is brought into the circuit and the disc again revolves for another phase, completing one revolution. Due to the overlapping of the primary sectors a start is always assured. By this arrangement any initial lag has no effect upon the interval being measured, for the disc travels several degrees before the timing collectors come into contact with their brushes.

Any desired range of speed intervals can be produced by altering the speed of the drive shaft or by changing the sector degrees of the collectors, or by a combination of both. A series of intervals can be produced by providing a series of conducting strips in line with one pair of brushes if it is desired to use the same line, or more than one collector circle can be used.

As a source of power for the motor a 1/6 h.p. motor of the constant speed, repulsion-induction type is recommended. A synchronous motor of the same power could also be used, but the portability and sureness of operation of the former in addition to its reliability and simplicity of operation lead me to prefer the former. No accessory apparatus is needed for the operation of a constant speed motor and alternating current is always available. When operated on alternating current the synchronous motor reacts to the fluctuations in pulsation rate which are constantly occurring and if the motor is delicately adjusted a slight decrease in potential will cause the motor to stop and 'sing.' The higher types of constant speed motors on the other hand, do not react to fluctuations of less than 10 per cent. and very rarely is the electrical service such that this limit is approached and on ordinary city current a repulsion-induction motor of good construction will operate with a maximum error of only 1/4 of one per cent., even when influenced by the peak load. On an interval of one second this would lead to a possible maximum error of 0.0025 second. It is well known that unless a thermostat is used with a synchronous motor when operated in tandem a gradual but persistent decrease occurs in the rate of rotation.

This disc commutator instrument is more difficult to construct but on the whole is more desirable than the acceleration apparatus for laboratories that can afford the more expensive construction. Its chief points of superiority over the acceleration apparatus and other forms of apparatus for measuring out time intervals are:

1. A range of intervals with a lower limit of about 0.01 second and practically no upper limit.

2. Any combination of connections desired is possible within the time circuits either simultaneously or in sequence.

3. Accuracy. The instrument is as accurate as the source of power.

4. Operation is certain with any currents within the range of laboratory use.

5. Freedom from distraction in the sphere of experimentation.

6. Simplicity of operation. There is nothing to do but throw a switch or press a key when the interval is desired.

7. The intervals can be given at any moment desired and in rapid succession.

The designer of the instrument has the patterns from which the original was constructed and will coöperate with other laboratories in the construction of similar instruments or of adaptations.